Kjeld Schmidt

Cooperative Work and Coordinative Practices

Contributions to the Conceptual Foundations of Computer-Supported Cooperative Work (CSCW)

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For Irene

Preface

This book is about cooperative work and the coordinative practices through which order in cooperative work is accomplished.

The development of computing technologies have from the very beginning been tightly interwoven with the development of cooperative work. Indeed, in important respects the challenges facing cooperative work in different domains have at various points been decisive in motivating and shaping crucial computing technologies such as interactive computing and networking. Over the last couple of decades computing technologies are also and increasingly being developed and used for *coordinative* purposes, as means of regulating complex activities involving multiple professional actors, in factories and hospitals, in pharmaceutical laboratories and architectural offices, and so on. The economic importance of the applications of these coordination technologies is enormous but their design often inadequate. The problem is that our understanding of the coordinative practices, for which these coordination technologies are being developed, is quite deficient, leaving systems designers and software engineers to base their system designs on rudimentary technologies. The result is that these vitally important systems, though technically sound, typically are experienced as cumbersome, inefficient, rigid, crude.

The research reflected in this book addresses these very practical problems and is concerned with trying to establish—in the *intermundia* between the social sciences and computer science—a conceptual foundation for the research area of Computer-Supported Cooperative Work (CSCW). What is cooperative work in the first place? Is it something of which we can talk and reason sensibly? Is it a category of practice that can be observed, described, and analyzed in anything like a rigorous manner? How do the many actors engaged in this kind of practice accomplish their tasks in an orderly fashion, without succumbing to chaos? Can we distinguish classes of practices, coordinative practices, by means of which they do so? Can they be observed, described, and analyzed? How are these coordinative practices organized? How do they evolve? How do actors manage to organize routine cooperative activities? What difficulties do they face and how do they cope with them? By means of which conventions, procedures, techniques, etc. do they regulate their joint work? How are these practices facilitated by traditional technologies, from paper and pencil, forms and binders, to time tables and archives? What are the costs and benefits of such technologies? Which issues arise when such practices are computerized, when control and execution of routines and schemes are transferred to computational artifacts? Can we devise computational facilities by means of which ordinary actors themselves can develop their coordinative practices, devise methods and tools for improved coordination?

Those are the kinds of questions I have been trying to answer in the course of the last twenty–five years. They are not questions for which sociology has answers, because they are not questions sociology has raised. They are questions raised by the diffusion of computing technologies in cooperative work settings.

The book comprises three rather different bodies of text. The bulk of the book—Part II—consists of articles written from 1991 to 2004 in which I have addressed and explored the issues and problems of cooperative work and coordinative practices in different directions. What unites these studies is a conception of cooperative work that makes it a researchable phenomenon, amenable to a technological research program. Instead of the ideological notion of 'cooperation' as an ethical imperative or the sociological notion of 'cooperative work' as coextensive with the notion of social nature of human conduct, these studies are based on a conception of cooperative work as observable relations of interdependence that are formed in response to practical exigencies but which then in turn require the development a family of equally observable coordinative practices.

The purpose of assembling these articles, of which some have reached a large audience and some not, is to place them together, back to back, and thereby highlight their connectedness. In other words, the aim is to present a set of contributions to the conceptual foundations of the field of CSCW that, although it is unfinished business as far as a unified conceptual framework is concerned, is nevertheless sufficiently elaborated and tested as an ensemble to be taken on: applied, extended, amended, challenged. . .

The articles are reprinted without substantive changes. What changes I have made are these. I have deleted 'abstracts' from articles that had any: they are useful in journals or conference proceedings, as announcements to the busy reader, but would here be more of a distraction. I have also removed the usual but often terse acknowledgment statements. Typographical and other minor technical faults have been corrected without notice. Similarly, incomplete or faulty citations and references have been corrected, and citations and references have been reformatted to a common standard.

However, although based on a common approach, these studies were not in a strong sense conducted in a planned and goal-directed manner and the resulting ensemble of articles evidently exhibits inconsistencies, false starts, in addition to the inevitable repetitions. To provide the reader with an initial overview of the meandering argument, Part I contains an introduction in the form of a 'progress report'. It gives a sketch of the development of the conception of cooperative work and coordinative practices and, by making the underlying research strategy explicit, serves to show how the different contributions are somehow connected.

Finally, since the research represented by these articles has had, in part at least, a distinctly programmatic character—the subtitle is intended to indicate just that—the book is also an occasion to revert to where the journey started, to the issue of what CSCW is all about. Not for the sake of whipping a dead horse, but simply because the discussion is as topical as ever. In fact, as a research area CSCW is in disarray, and it is time to reconsider CSCW's research program. This is the aim of Part III.

Copenhagen, Denmark Kjeld Schmidt
1 May 2010

Acknowledgments

The history of my research, as reflected in this book, is a clear demonstration of how tricky the concept of cooperative work can be. Some articles were obviously written in close collaboration with colleagues, while others, the majority, were written by myself. But even these could not have been written had I not been collaborating, in different ways, with a large number of colleagues. In fact, irrespective of the formal authorship of the individual articles, the general framework developed in the articles collected here has evolved over many years in more or less continual debates with colleagues and students with whom I have discussed my work and who have offered opposition, often staunch but always stimulating opposition, to my notions and contentions. I should mention, at the very least, Hans Andersen, Liam Bannon, Jørgen Bansler, Susanne Bødker, John Bowers, Geof Bowker, Giorgio De Michelis, Peter Carstensen, Eli Gerson, Christine Halverson, Christian Heath, Erling Havn, Betty Hewitt, Thomas Hildebrandt, John Hughes, Rachel Israël, Bjarne Kaavé, Finn Kensing, Kristian Kreiner, Jacques Leplat, Paul Luff, Gloria Mark, Morten Nielsen, Irene Odgaard, Wolfgang Prinz, Dave Randall, Jens Rasmussen, Mike Robinson, Tom Rodden, Yvonne Rogers, Pascal Salembier, Dan Shapiro, Wes Sharrock, Carla Simone, Susan Leigh Star, Lucy Suchman, Carsten Sørensen, Halina Tomaszewska, and Ina Wagner.

As is typical of research work these days, my work has been carried out in collaboration with countless partners and coworkers in a number of European and Danish research projects such as, to name but the most important, FAOR, TIA, CoTech, MOHAWC, COMIC, COTCOS, DMM, DIWA, FASIT, IDAK, HIT, CITH, Cosmobiz... Those who were involved too will recognize the acronyms and will know my debt. It also so happens that the research reflected in this volume has been carried out while I was working for a string of institutions: Dansk Datamatik Center, the research center of the Danish Trade Union Federation (LO), Risø National Laboratory, the Technical University of Denmark, the IT University of Copenhagen, the University of Siegen, and Copenhagen Business School. Without the support of these institutions, none of this could have been accomplished.

An early version of the present book, carrying the same title, was submitted to the IT University of Copenhagen for the *dr.scient.soc.* degree. The two official opponents, Wes Sharrock and Yrjö Engeström, were gracious and the degree was awarded me in June 2007.

The present book differs from the dissertation in many respects. Most importantly, it contains new chapters in which I, at the instigation of the anonymous reviewers, undertake a critical discussion of CSCW. The new chapters are the three that make up Part III. On the other hand, in order to prevent the book from becoming excessively large, two articles have been omitted from Part II.

A couple of sojourns as visiting professor with Volker Wulf's group at the University of Siegen, Germany, in 2007 and 2008 gave me the welcome opportunity to begin drafting the new chapters. A version of the account of the formation and fragmentation of the CSCW (in Chapter 11) was used as a basis for an article published as a discussion paper ('Divided by a common acronym') at ECSCW 2009. A planned chapter on 'The concept of work in CSCW' was taken out and used as a basis for another article and submitted for COOP 2010. Readers who want to inspect those aspects of my critique of the state of CSCW are referred to these articles.

I was fortunate that a number of colleagues, among them Liam Bannon, Susanne Bødker, Lars Rune Christensen, Lise Justensen, Dave Randall, Satu Reijonen, Signe Vikkelsø, and Volker Wulf, have commented on versions of Part III (or, rather, fragments thereof), directing my attention to all kinds of shortcomings, not least points where the argument was acutely in need of clarification. I thank them all, hasting to add that the responsibility for remaining shortcomings in terms of style, grammar, logic, clarity, judgment, and plain good sense is mine alone.

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Progress Report

So here I am, in the middle way, having had twenty years [. . .]
Trying to learn to use words, and every attempt
Is a wholly new start, and a different kind of failure [. . .]
And so each venture
Is a new beginning, a raid on the inarticulate
With shabby equipment always deteriorating
In the general mess of imprecision of feeling,
Undisciplined squads of emotion. And what there is to conquer
By strength and submission, has already been discovered
Once or twice, or several times, by men whom one cannot hope
To emulate—but there is no competition—
There is only the fight to recover what has been lost
And found and lost again and again: and now, under conditions
That seem unpropitious.

T. S. Eliott: Four Quartets

Chapter 1
Cooperative Work and Coordinative Practices

Over the last few decades, the interests and concerns of researchers from areas or disciplines that are otherwise rather disparate have been converging on at set of issues that are closely related, in practical terms as well as conceptually, and which all somehow center upon cooperative work practices.

We have by now a rather overwhelming body of literature that, in different ways, is concerned with issues of cooperative work practice, although the issues, as is always the case, are named and framed differently by different research traditions: 'articulation work' (Strauss, 1985; Strauss et al., 1985; Gerson and Star, 1986), 'situated action' (Suchman, 1987), 'due process' (Gerson and Star, 1986), 'working division of labor' (Anderson et al., 1987), 'actor networks' (Latour, 1987; Law and Hassard, 1999), 'horizontal coordination' (Aoki, 1988), 'boundary objects' (Star and Griesemer, 1989; Star, 1989), 'distributed cognition' (Hutchins, 1991, 1995), 'socially shared cognition' (Resnick et al., 1991), 'distributed decision-making' (Rasmussen et al., 1991), 'communities of practice' (Lave, 1991), 'coordination theory' (Malone and Crowston, 1990, 1992), 'cooperative work' (Bannon and Schmidt, 1989; Schmidt and Bannon, 1992), 'heedful interrelating' (Weick and Roberts, 1993), 'contextualized integration of human activities' (Harris, 1995, 2000), 'team work' (Grudin, 1999), 'team situational awareness' (Endsley and Garland, 2000; McNeese et al., 2001), 'embodied interaction' (Dourish, 2001), etc.

Whatever the name and irrespective of the frame, the various research undertakings listed above focus on problems such as: How is concerted action of multiple individuals actually accomplished? Through which practices is such action coordinated and integrated? How do actors manage to act in a sufficiently concerted way, under conditions of partial knowledge and uncertainty, and how do they routinely manage to do their joint work in an orderly fashion in spite of local troubles and the heterogeneity of interests, approaches, and perspectives? What is the role of formal constructs such as checklists, plans, blueprints, standard operating procedures, classification schemes, coding schemes, notations, etc.? How are they constructed, appropriated, applied, amended? What is the role of material artifacts and practices of writing in this context? How do actors interact through inscribed artifacts and how do they coordinate and integrate their individual activities by means of such devices? How do the different material characteristics of infrastructures, settings, and artifacts impact on cooperative practices? How do transformations of these material artifacts,

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media, and modalities affect the practices and the organization of cooperative work?

These questions have also defined the research work reflected in this book, but the research strategy that has been developed and pursued in the course of this work differs in important respects from some of the other approaches. Outlining this strategy and how it has developed is the topic of this chapter. A brief account of how it all began in my individual case is an appropriate place to start.

1 The Road to CSCW

The problem of cooperative work—understanding the changing forms cooperative work takes under different economical and technological conditions as well as the skills involved in cooperative work—has, it seems, been with me for ages. Since I, as a young man in the late 1960s, immersed myself in Marx' *Grundrisse* (1857–1858b) and *Das Kapital* (1867b), the phenomenon of cooperative work has played a central role in my understanding of working class organization, that is, cooperative work conceived of as the material source of working class autonomy and assertiveness and as the source of a progressive constitution of modern industrial society based on the 'association of free producers'. In fact, I became a sociologist in an attempt to understand these issues and my very first publications addressed those very issues (e.g., Schmidt, 1970).

Then came the so-called 'microprocessor revolution' of the early 1980s. With the microprocessor the computer became a commodity. It became economically feasible not only to incorporate computers in plastic boxes with keyboards and screens that could be sold to individuals as 'personal' appliances, but also to incorporate computers in virtually any part of the production facilities in industrial settings. The world of industrial production was in for radical transformation. In industry, apart from 'process industries' such as chemical industries and power plants where key processes are automatic by nature, manual control had been prevalent up to this point in time. It is true that the overall flow of materials and parts had been mechanized in mass-production industries. The assembly lines of the automobile industries were of course famous but of marginal economic importance; the typical manufacturing enterprise was more like an engineering shop than a mass-production plant. And in any case, in engineering shops and large-scale manufacturing alike, production control was still 'manual': the control of the production processes (cutting, grinding, welding, etc.) were 'in the hands' of workers. Now, with the advent of the microprocessor, this began to change on a giant scale. In rapid succession an extended family of new technologies were devised and began to be rolled out: computer-controlled machining centers, industrial robots, automated materials-handling and transportation systems, flexible manufacturing systems (FMS), computer-aided design and manufacturing (CAD/CAM), production-planning and control systems (MRP), and so on (Schmidt et al., 1984). It was also evident that similar upheavals were underway outside of production proper, in design and engineering, in the administrative domain, etc.

I was thrilled. The chance of witnessing, in one's life time, a technological, organizational, occupational, social, and economical transformation of such magnitude and scope was not lost on me. In that mood, it did not take long for me to decide to enter the fray. My move was, more specifically, motivated by my realization of a number of deep-set methodological problems with the program I had been pursuing until then.

(1) A sociological study that tries to determine the social and organizational impact of a specific technological change is faced with a methodological nightmare. The reason is that, in order to do so, one must first of all be able to characterize the technology in question with respect to actual practices. How can a sociologist, of all people, adequately characterize technologies? Worse, how does he do that with novel, perhaps not yet fully developed technologies? How does he do it without understanding the specific roles of different technologies in actual working practices? What happens is of course that sociologists stick to second-guessing, that is, to produce *post-festum* 'predictions', or that they produce forecasts on an aggregate level so elevated as to be meaningless.

(2) As pointed out by none other than Marx (1877, 1881), there is no 'superhistorical' 'master key' that allows us to anticipate societal developments in any specificity. Processes of social change may exhibit striking regularities but they always play out in a particular 'historical milieu', as a result of which they may have widely different effects in different local settings. The industrial revolution in Britain, for example, produced an entirely different 'historical milieu' for pre-industrial branches of production in Britain as well as for other countries. Understanding technological change and its impact is no exception. Even when a specific technology is understood and has been adequately characterized, its 'effects' may differ widely according to the socio-economic milieu: for example, the national and regional rate of employment, the quality and coverage of the educational system, migration patterns, social security systems, labor protection, etc.

(3) Moreover, it quickly dawned on me that central to the transformation process generated by 'the microprocessor revolution' were some very complex research issues such as, for example, the famous one of 'allocation of functionality between human and machine'. Enlightened engineers were beginning to realize that the default strategy of *automating whatever can be automated* leads to all kinds of dysfunctional socio-technical systems and, especially in the case of safety-critical systems, possibly to accidents and disasters. They discovered that the 'allocation of functionality' is a design problem in its own right. At first, it was hoped that cognitive psychology could help out by offering something close to a check list of 'tasks' that are optimally best allocated to machines and humans, respectively (Jordan, 1963). It did not work out that way, of course (Kantowitz and Sorkin, 1987). The problem is a wicked one (Rittel and Webber, 1973).

The design of complex technical systems presumes certain 'job designs' which in turn presume technical systems of certain shapes and forms. Coping with the circularity of the problem requires an understanding of the 'dynamics' of technical system and job design, which in turn requires an understanding of the technical, organizational, and socio-economic environment. And since modern work is cooperative work, this means that the design of complex technical systems and the shape and form of the organization of cooperative work are inexorably intertwined. As I saw it, CAD/CAM systems, FMS systems, MRP systems, office information systems, etc. were all systems by means of which workers would cooperate and also coordinate their cooperative activities.

My realizing these methodological problems prompted me to make my move. Twenty years earlier, in 1965, I had dropped out of university, where I was initially studying philosophy, to become a computer programmer. My motive was partly to make a living, of course, but also partly fascination with the new computing technology. In 1985, I made a parallel move. I left academia to join a private research laboratory (Dansk Datamatik Center) where I became responsible for the lab's research in office information systems. That move took me directly to the research area of CSCW, which was then just being formed.

In the few years I spent at DDC, I became increasingly involved in doing field work. After some initial 'quick and dirty' workplace studies in various administrative organizations (e.g., a regional planning office, a standardization organization), I did fieldwork in domains as diverse as engineering design (e.g., design of cement factories), mathematical research, and portfolio management. In the course of these studies, the problem of cooperative work became far more concrete to me than it had been before 1985. In a report on *Integrated Engineering Workstations* from 1987, I summarized the observations I had made in my studies in a few theses on 'forms of cooperative work' (Schmidt, 1987), which were first turned into a short paper that I had the opportunity to present at the first European workshop on CSCW in Athens (1988b), and then into a rather long paper that was presented at a workshop on 'technology and distributed decision-making' in Germany (1988c).

The strategy that I, in effect, pursued in this early work exploited the rather unusual experience I had gained from doing a series of workplace studies in very different settings in quick succession. It provided me with the opportunity of observing patterns of cooperative work in a large number of settings and hence of subjecting the work in these settings to a comparative analysis. Although I was doing field work 'for money' and did not have much time to do a systematic comparative analysis, my understanding of cooperative work, as it emerged from these studies, differed radically from the then prevailing notions of cooperative work. I already knew from the classic study by Heinrich Popitz and his colleagues (1957) that 'group work' is a rare occurrence in industrial settings, the 'group fetishism' of especially American sociology notwithstanding. In my own studies, I did not see much 'team work' either, nor did I observe actors solemnly decide to 'collaborate' to reach a 'shared goal'. Instead, the cooperative work arrangements, which were easily observable, came across, vividly and massively, as an entirely practical matter, motivated by highly pragmatic (but contradictory) concerns such as external requirements, operational constraints, and limited resources. What I saw were patterns of cooperative work emerging, changing, and dissolving again in response to recurring or changing requirements, constraints, and resources.

This insight—that cooperative work is a ubiquitous occurrence in industrial and similar work settings, that it is not something invented by sociologists, social-psychologists, or management consultants but is a routine practical measure to meet practical needs—was the platform from which I engaged in CSCW. It first of all provided me with a basis for defining cooperative work, and hence the scope of CSCW, and was then, in turn, instrumental in pointing to some absolutely key issues for CSCW.

Instead of giving a summary of the various arguments and positions that are generally spelled out well enough, and more than often enough, in this collection, I will here concentrate on the *strategy* rather than the particular campaigns. However, in order to give readers who are unfamiliar with the research reported in this book a chance for following the remainder of this introduction, let me introduce a very simple example of cooperative work in everyday life.

2 The Concept of Cooperative Work: The Mundane Case of Moving

Consider two men moving a dining table set consisting of a table and six chairs from one end of a living room to the other.

They may do this for all sorts of reasons. Perhaps they agree to the very idea of moving the table and even that it should be moved to that particular location. They may have discussed different possibilities and only then negotiated a solution. In this case they may be said to have a 'shared goal' in the sense that a certain future state of affairs, a new location of the table set, has been stated as desirable and explicitly agreed to. 'OK', one of them may have said, eventually, 'I think you're right, let's move it to the window.' 'Yes, let's try,' the other one said.

Perhaps the two men do not agree on the desirability of moving the table set at all. Perhaps one of them is merely assisting the other person, for a fee perhaps, or to return a favor, or out of sheer generosity. Whatever the reason, he does not really care very much about the location of the table set. He does not 'share' the other man's 'goal', he's just helping out. 'Now, where do you want it?', he asks. 'Over there, by the window.' 'All right.'

Now, whatever the social arrangement—who wants to move the table and who acquiesces and who merely lends a helping hand to someone else's project—actually moving the table involves a specific category of interaction.

Moving the chairs is straightforward. Each of the men just picks up one chair at the time, carries it to the other end, and puts it down, and then repeats the operation until they are finished. To do this, in fact, two men are not needed. It might even be easier for one man to do it, since being two requires certain coordinative measures: they must take care not to be in each other's way or to bump into the other, and they have to make sure that they do not put down the chairs in a location where these will become an obstacle to the other or pose an obstruction when moving the table.

Carrying the table is a different kind of activity. Let us say that the table is large and heavy. It just might be possible for one of them to move it to the new location by dragging or pushing it across the floor, but that would surely damage the beautiful hardwood floor and perhaps also put excessive stress on the joints of the table. That is, by being more than one to do the job, it is feasible for them to move the table without causing damage to the table, to other things, or to themselves.

Now, to move the table, each of them grabs it, lifts it, and they then carry it to its new location. But apart from these individual-sounding actions (grabbing the table, lifting it, carrying it, and putting it down again), how do they do it as a *joint* effort? Which interactions occur between the two men to make it happen?

First of all, and very much as in the case of the chairs, they must somehow agree about what to do with the table. They also need to make initial arrangements such as, who takes which end of the table, and perhaps also the exact destination. Having sorted that out, they need to synchronize their respective actions: when to pick it up, when to start walking, the general direction in which to walk, the pace of walking, and when to put it down and at which exact spot by the window. If they do not handle this coordination well, the trivial task of moving the table may turn out to be demanding and may cause some broken furniture and even an injured back. These coordinative actions can happen in myriad ways. But basically, by holding the table in their hands, they are both immediately 'aware' of the state of the table: its location in space (altitude, pitch, and roll), its velocity, its weight. As soon as one of them walks slightly more briskly or slows down just a little, tilts the table to this or that side, lowers it or raises it, the changed state of the table is instantly conveyed to the other man who then has to act accordingly, by doing likewise or by counter-acting. In the act of carrying the table, the two men are causally interrelated.

The two men are also able to interact in other ways. They may hold the table in such a way that they can see each other's face; each may then be able to gather from the other man's expression if he is having problems or what he intends to do next and may adjust his own actions to that. Each of them can talk, groan, and nod to make the partner understand his problems or intentions, but also to acknowledge that the other man's problems or intentions have been understood, and so on. If these coordinative actions—the nodding, grunting, talking, swearing, shouting—are not sufficiently effective, any one of the two men can deliberately change the state of the table (force it in a certain direction, stop abruptly, shake it, etc.) to make a point that did not come across all that well verbally or through gestures. They are thus likely to succeed.

Indeed, moving a table horizontally a few meters across the floor of a room does not pose extraordinary challenges to the two men's coordinative competencies. But taking the table through a door opening might. The narrow space of a doorway, compared to that of a room, will typically impose strict constraints on the operation. The men cannot move the table horizontally through the door opening but will have to tilt it. Perhaps the width of the doorway is less than the height of the table and perhaps the next room is a narrow corridor. In this case they may have to carry the table vertically while simultaneously turning it around its vertical axis to get the legs through. In order to do this, then, they have to take the table through a carefully choreographed sequence of spatial positions while moving it forward, through the opening, into the corridor. The more severe constraints of this task, compared to moving a table to another position in the same room, change the nature of the cooperative task considerably. Since the degrees of freedom are much fewer, the activities of two men become more 'tightly coupled'. There is, literally and metaphorically, less leeway for each of them. They need to coordinate their individual actions much more closely.

Things would be somewhat different if they are, say, moving a rolled-up carpet. When taking the carpet through the doorway they can bend it fairly easily, which may practically neutralize the constraints otherwise posed by a doorway leading into a narrow corridor. Their individual actions are therefore less tightly coupled, and they need not strive to coordinate their individual actions as closely and carefully. On the other hand, however, due to the carpet's floppiness, its state is not as immediately apperceptible to the two men as that of the table. It may not be immediately obvious, from the state of the carpet as experienced locally, by each of the two, what the other is doing to the carpet. That is, while moving the carpet does not pose strong demands on their coordinative skills, when close coordination for some reason is called for they may have to be more verbally explicit than when moving a table.

Imagine, finally, an effort of moving on an entirely different scale such as, for instance, when a family is moving to a new house or a firm is relocating to a new building. In such cases, more than two persons will be involved for the simple reason that the number of items to be moved is much larger. If only two men were to do the job, the exercise could easily last for weeks or months. In the case of the family's moving to a new home, the effort may involve friends and family or professional movers; in the case of the relocation of the firm, the effort will undoubtedly require dozens of professional movers. In any event, we would observe exactly the same practices as the ones we have just described, the lifting and carrying of chairs, tables, boxes, etc. The important difference is the scale: these actions will be happening in parallel and will be repeated multiple times. However, since many more items and many more actors are involved, what decision analysts call the 'space of possibilities' is vastly larger than in the case of simply moving one table and six chairs from one end of a room to the other. There simply are so many things to move and so many places things can be moved to. And, to confound the problem, there are dozens of actors who are simultaneously picking up items and moving them to other locations. In such cases we will observe specialized professional practices. Furniture, lamps, carpets, etc. will carry labels telling where they are to be put ('Kitchen' or 'Room K4.55'). Boxes will have similar labels indicating not only their destination but also what they contain ('Porcelain', 'Unfinished manuscripts'). The movers may also have floor plans of the building, and doors may be similarly marked with labels with inscriptions that correspond to the inscriptions on the floor plan ('Kitchen' or 'Room K4.55') and on the items that are to be taken there. If the relocation operation is a large one, one may also observe a pre-specified workflow of sorts, for example a list indicating the sequence in which items belonging to particular building sections are to be relocated ('Monday: Ground floor; Tuesday: First floor', etc.), in order to avoid congestion, confusion, chaos.

3 Strategic Distinctions

Our mundane example has already induced us to make a series of important and interlaced distinctions.

(1) The first distinction is of course that of *individual work* and *cooperative work*. Whereas each of the men could move the chairs individually, they could not do so, for whatever reason, when it came to moving the table. For this task the joint effort of two men was required; when approaching and then grabbing the table, the two men entered a *cooperative work arrangement*.

One should notice, however, that there is an important issue of scope or granularity in making this distinction in an actual case. Instead of focusing on their moving the chairs and then moving the table as separate tasks, we could just as well look at the moving of the whole dining table set *as a set*. Looked at this way, it would still be a cooperative effort, since two men would be required for at least part of the effort, but it would still be composed of a range of actions, of which some (moving the chairs) were 'loosely coupled' and others (moving the table) 'tightly coupled' cooperative activities. In fact, the example we are looking at here could itself be merely a small part of a much larger cooperative relocation effort. The level of scope or granularity at which we describe it depends on the purpose of our investigation.

We also distinguish the work itself, the work of moving the table set, from the secondary interactions required to coordinate and integrate the contributions of multiple individuals, for which I have adopted the term used by Anselm Strauss and his colleagues: *articulation work* (Strauss, 1985; Gerson and Star, 1986).

(2) When we describe the cooperative activities of moving the furniture, we are applying a distinct analytical perspective. We look at the cooperative effort and the practices involved in that effort without knowing very much about the socio-economic setting in which it takes place. In fact, we *do not need* to know the socio-economic roles of the two men: if either or both of them are wage earners and do this for a salary, or if they live there and do it for their own benefit, or if one of them is providing neighborly help. Nor do we need to know anything about their 'state of mind', that is, if they are happy or not happy about the whole ordeal. In short, we can focus on and investigate cooperative work and coordinative practices as a distinct domain of practice, while leaving the socio-economic and organizational setting in the background.

To be more precise, a number of distinctions are involved here (cf. Schmidt, 1994c, 2002a). There is the unfolding pattern of cooperative interdependencies and interactions, as the two men engage in the task and perform their work: as they approach the table set, pick up the chairs and carry them, one at a time, to the end of the room, and then return to the table, pick it up, and carry that too. These shifting patterns of actually enacted relationships is what I call the *cooperative work arrangements*. Other categories of relationship can also be distinguished, in particular the relatively stable configuration of actors for which the term *work organization* is normally used. The distinction is that of 'mobilization' *versus* 'deployment', that is, between the *contingency* arrangement (e.g., the particular configuration of workers with a range of skills deemed adequate to handle the tasks expected on a particular shift) and the *enacted* arrangement. In our example of moving, two men are enlisted in the contingency arrangement because the work to be done includes the moving of a large and heavy table; the enacted arrangements coalesce and dissipate again, as the two men first move the chairs and then, jointly, the table.

Both of these perspectives are essential when looking at cooperative work, not only the enacted arrangements but also the contingency arrangement, because the shifting cooperative work arrangements play out among the members of the work organization. They combine and deploy as the situation unfolds, on the basis of what is to be done, what it requires, who is ready, etc.

In contrast to these perspectives, in which cooperative work is conceived of as material relationships and which are central to CSCW, there are of course other perspectives, in which cooperative work is conceived of from the point of view of the socio-economic relationships that are also played out in cooperative activities, in and through the material relationships. There is the unit of appropriation through which resources are committed and pooled and the results of the effort are allocated to the participants—the economic unit, if you will. In our case, different units may be involved: the family whose furniture it is and who will hopefully benefit from the moving about, and possibly the neighbor who may be helping, or the professional movers who in turn may be wage earners or members of a cooperative. And there are, finally, the contractual arrangements through which members of the unit regulate their diverse, partially incongruent, sometimes conflicting interests and concerns: the pizza and beer that the neighbor is due, or the contract specifying the 'transaction' between the family and the movers.

In making these distinctions, or rather, in recognizing the different domains of discourse in which we talk differently about practical organization of work, I was (implicitly) influenced by the Marxian distinction between 'material' and 'social' relationships of human sociality (for an excellent reconstruction, cf. Cohen, 1978). However, I was also strongly influenced by neo-classical institutional economics (Williamson, 1979, 1981). This is not as unprincipled as it may seem. Williamson makes the exact same distinction as I do here: between the *relationship of interdependence* in work and its immediate organization (the cooperative work arrangement and the work organization) on one hand, and on the other the *contractual governance arrangements* regulating 'transactions', the relationships of ownership and appropriation. The small but important difference is that he focuses on the socio-economic relationships of transfer of possession across 'technologically separable' activities ('transactions') and considers the cooperative work arrangements as a singularity, whereas I have shifted 'figure and ground' and focus on the cooperative work arrangement, pushing the socio-economic relationships to the back.

This distinction—between cooperative work and the contractual settings in which it is situated—is useful for defining the 'boundary' between CSCW and Information Systems research and other areas of organizational IT. The distinction is not, as it is sometimes posited (e.g., Grudin, 1994, 1999), one of size ('small groups' versus 'organizations') but one of perspective. The CSCW perspective addresses IT *sub specie* cooperative work practices, irrespective of the institutional economics (not to mention *the size*) of the arrangement, whereas IS research addresses IT *sub* *specie* the socio-economic interests and motives of the actors (business models and the concomitant performance measurement and remuneration arrangements).

The distinction between cooperative work and the institutional and contractual arrangement is fundamental to my strategy. It allows us to single out 'cooperative work' as a distinct category of practice that can be conceived of independently of actors' motives and interests and thus to talk fairly unambiguously about 'interdependence'.

(3) In our scenario we begin to discriminate different 'kinds' of relationships of interdependence. We noticed, for example, that moving chairs and moving tables and moving carpets involve interdependencies with rather different characteristics. We also noticed that moving the table across a room involves interdependencies that are distinctly different from moving the same table from one room to another. These different characteristics can be conceived of a so many kinds of complexity of cooperative work.

Being *interdependent in work* is categorially different from being 'interdependent' by virtue of sharing a scarce resource, such as the road system in the morning rush-hour, or being 'interdependent' by virtue of sharing a budget, as one does when employed with the same company. Different rules apply and hence different practices are involved. Without the distinction, the term 'interdependence' is analytically useless.

Thus defined, the concept of 'interdependence' itself plays a strategic role. First of all, it has provided a firm ground for defining cooperative work in a way that does not subscribe to notions of occult alignment of minds such as 'shared goal' or 'shared understanding'.[1] Such mentalist definitions invariably end up in tautologies: cooperative work is defined by a shared goal, and the criterion for ascribing a shared goal to actors is that they—well, act in concert. If cooperative work is conceived of this way, we are not really, i.e., accountably, able to speak of a cooperative effort that is *not* carried out successfully. By contrast, the concept of interdependence in work enables us to conceive of cooperative work in terms of actual observable conduct.

In addition it has served a heuristic or methodological function. When we conceive of cooperative work in terms of observable interdependencies, the obvious next step is to investigate the different characteristics of different relations of interdependence, such as, for instance, the 'degree of coupling', the direction of dependence, as well as irreversibility, uncertainty, and various temporal characteristics, etc. This has proved analytically quite productive in the context of workplace studies, by offering a useful path towards a systematic conceptual framework for workplace studies and for comparative analysis.

The concept of interdependence expresses the particular material, dynamic, and environmental characteristics of a particular cooperative effort. It therefore also implies that different relations of interdependence may have different

[1] I am not exaggerating. Endsley and Jones, for example, explicitly conceive of 'common goal' and 'shared understanding' as 'overlapping' 'sets' and even go as far as to talk about 'shared situation awareness' and 'shared mental models' (Endsley and Jones, 2001).

characteristics. The relationships of interdependence that actors enter when forming a specific cooperative work arrangement to undertake a particular task pose specific 'complexities' for the cooperating actors to cope with that differ from those posed by a similar task in another setting or by another task. Carrying a table across a floor poses coordinative complexities of a more manageable kind than carrying the same table through a narrow door frame, whereas the relocation of an entire firm from one address to another in turn poses quite different and far less tractable complexities.

Although the concept of *complexity* was at first introduced rather informally in my thinking about cooperative work and was not discussed at all until my 'Remarks on the complexity of cooperative work' (2002a), it served well as an intuitive and cogent way of expressing what motivated the widespread use of specialized coordinative practices involving coordinative artifacts. The concept of the complexity of cooperative work was also useful by implicitly highlighting those settings and practices for which CSCW technology might be most relevant and have the highest potential.

(4) We have, in our fictional case, observed a variety of coordinative practices, ranging from the trivial one of moving the chairs prior to moving the table and avoiding collisions, to carrying the table in synchrony in the right direction, to the entirely different specialized practices of using standardized inscriptions to identify or categorize items and of developing work schedules.

The concept of interdependence also, and this has been its most important strategic function, offers a framework for carefully and deliberately embracing the entire spectrum of coordinative practices, from actors' effortlessly and unnoticeably aligning their own activities with those of others (in CSCW often referred to under the label 'mutual awareness'), to actors' methodically regulating their interdependent activities through pre-established schemes expressed in a set of rules (conventions, operational procedures) and concomitant, appropriately formatted textual artifacts (forms, taxonomies, schedules, etc.). So, instead of creating a categorical gulf between, say, 'awareness' and 'workflows', the concept of interdependencies enables us to conceive of coordinative practices as an open-ended repertoire of practices, some inconspicuously quotidian and ubiquitous, others exceedingly specialized and sophisticated.

The idea that different cooperative work arrangements have to cope with interdependencies of different complexity and that they develop different coordinative practices to accomplish exactly that, gave me a handle on a vast and heterogeneous class of coordinative practices that all rely on coordinative artifacts and, behind that, practices of writing. I dubbed them 'coordination mechanisms'.[2] They are massively present in modern cooperative work settings and one would have to be blind (or ideologically blinded) not to notice them. They are ubiquitous because economically vital.

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[2] In fact, these practices were at first called 'mechanisms of interaction' (1994b), but having realized that the term 'interaction' covers about everything in social life and that the term 'mechanism of interaction' thus were far too sweeping, I later adopted the more modest term 'coordination mechanisms'.

4 Coordinative Practices: From 'Coordination Mechanisms' to 'Ordering Systems'

The concept of coordination mechanisms was developed in opposition to the then prevailing opinion in CSCW according to which IT systems cannot or should not regulate interaction. While the observation that formal organizational constructs are widely used in cooperative work was far from controversial, apprehension had grown and become widespread in the CSCW community with respect to the idea that computer systems could be successfully designed to regulate cooperative interaction by means of computational procedures, workflows, process models, etc. These misgivings were not at all groundless. Early attempts in CSCW to build systems that somehow imposed rules on cooperative interaction such as The Coordinator (Flores et al., 1988), DOMINO (Victor and Sommer, 1989), etc. were generally perceived as failures, sometimes even by the designers themselves (Kreifelts et al., 1991a), and a number of critical sociological studies by Lucy Suchman and others argued that such constructs, instead of determining action in a 'strong sense', by specifying step by step how work is actually performed, serve as 'maps' that responsible and competent actors may or may not consult to accomplish their work (Suchman, 1987).

From the very beginning I found these interpretations of the experiences and of the field work data problematic and found the conclusions drawn from them unduly pessimistic. Fearing that the thinking that was already rapidly becoming the CSCW canon (Agre, 1990) would condemn CSCW research to a program that, devoid of sociological realism and practical relevance, posited that computational systems should simply provide a space of sorts for unregulated interaction ('media spaces', 'workspaces', 'collaborative virtual environments'), I suggested an alternative research program (Schmidt, 1991a).[3] Using Suchman's dictum that 'plans are resources for situated action' as my shield, I very cautiously sketched my alternative approach:

> models of cooperative work in CSCW systems (whether procedures, schemes of allocation of tasks and responsibilities, or taxonomies and thesauri, etc.) should be conceived of as *resources* for competent and responsible workers. That is, the system should make the underlying model accessible to users and, indeed, support users in interpreting the model, evaluate its rationale and implications. It should support users in applying and adapting the model to the situation at hand; i.e., it should allow users to tamper with the way it is instantiated in the current situation, execute it or circumvent it, etc. The system should even support users in modifying the underlying model and creating new models in accordance with the changing organizational realities and needs. The system should support the documentation and communication of decisions to adapt, circumvent, execute, modify etc. the underlying model. In all this, the system should support the process of negotiating the interpretation of the underlying model, annotate the model or aspects of it etc. (Schmidt, 1991a)

[3] My critique of Suchman's analysis of the role of formal organizational constructs was unfolded a few years later in my article entitled 'Of maps and scripts' (1997). (Cf. also the discussion in Chapter 12).

4.1 Coordination Mechanisms in Practice

I had originally begun to concern myself with work practices that somehow depend on formal constructs when I was doing my early work on office information systems for administrative work domains in the 1980s, but from about 1990, when I joined Jens Rasmussen's group at Risø, my colleagues and I started on a systematic investigation of the phenomenon. Bjarne Kaavé was already engaged in his fascinating study of production planning and control practices in a Danish manufacturing plant (Kaavé, 1990). Our discussions and analyses of his observations played an important role in developing my understanding of interdependence and of the role of coordination mechanisms. A little later, Peter Carstensen and Carsten Sørensen did a study of a large industrial design project and were able to observe, virtually first hand, how a group of ordinary engineers developed and adopted a set of procedures and forms (e.g., a bug report form, a binder, a spreadsheet with a project schedule) in an attempt to cope with a cooperative effort that had become chaotic (Carstensen, 1994; Carstensen et al., 1995a; Carstensen, 1996; Carstensen and Sørensen, 1996). To us this was a demonstration that coordination mechanisms cannot be reduced (Braverman style) to mere control instruments in the service of capital. They are, in some important respects at least, indispensable practical means for maintaining order under conditions of division of labor. Hans Andersen's study of 'change notes' in another design organization complemented these findings (Andersen, 1994b).

This view of coordination mechanisms—that they are essential means that members of cooperative work arrangements devise, adopt, and adapt in order to be able to manage their complex interdependencies—was later substantiated by a series of studies of 'self-governing production groups' in Danish industry that were carried out from 1998 onwards. Such groups are a key element in a strategy that aims at increasing the competitive power of manufacturing in high-cost Western countries by increasing operational flexibility and product quality. However, as it had been pointed out by Irene Odgaard in a study of production groups at a large Danish manufacturing company (1994), the groups were largely unable to accomplish the coordination tasks that had been delegated to them because they did not have the requisite tools to do it properly.

Inspired by this, my colleagues and I embarked on a series of studies of shop-floor production planning and control in Danish industrial enterprises that lasted from 1998 to 2002. In the first of these studies, of shop-floor planning and control in a manufacturing enterprise that was then switching from forecast-driven to order-driven production, we were able to show that the standard MRP system was far to crude to offer the required coordination support on the shop floor. On the other hand, it was evident that the models underlying the MRP system (e.g., bill of materials, routing schemes, processing schemes) were as indispensable as in the case studied by Kaavé. Our study resulted in a demonstrator prototype of a system that would exploit the models underlying the MRP system but give operators extensive power to overrule the plans generated by the MRP system, whenever they decided that the generated plans were inadequate, and our prototype would then, again on the basis of known interdependencies, try to anticipate the effects of the new plans enforced by the operators. One could say that the kind of system we sketched was an interactive MRP system (Carstensen et al., 1999; Odgaard et al., 1999).

After that, in a subsequent and much larger research project, we launched a series of concurrent field studies in five Danish manufacturing enterprises: a shipyard, a maritime propulsion manufacturing plant, a cable manufacturer, a manufacturer of steel cabinets, and a manufacturer of electronic instruments (Carstensen et al., 2001; Carstensen and Schmidt, 2002). This series of studies, in which members of production groups played an active role, further substantiated the line of thinking that was orienting our research: that the kinds of construct we have called 'coordination mechanisms' are of critical importance to actors in complex cooperative work settings; that actors build, adopt, manipulate, adapt such schemes when it is deemed useful to do so; that their ability to do so is critical to the productivity, effectiveness, and quality of their work and essential to their collective control of their daily working life; and that there may be potentially vast benefits to be gained from developing information technologies that support ordinary workers in those practices. In short, the studies served as 'proof of concept' for the concept of 'coordination mechanism' as well as a powerful reminder of the practical importance of the problem.

4.2 Understanding Computational Coordination Mechanisms

We knew of course, from the outset, that for coordination mechanisms to be truly viable the existing technological platforms were insufficient. Therefore, at the same time as these analytical and design studies were pursued, but tightly interlaced with them, another long-term research program was launched in close collaboration with Carla Simone and her colleagues (at the Departments of Computer Science at the Universities of Milano and Torino).

In this work, we understood coordination mechanisms as consisting of two basic and closely related elements: (a) a *coordinative protocol*: a set of rules pertaining to interaction (taken-for-granted ways of proceeding, established conventions, official policies, standard operating procedures); and (b) a *coordinative artifact*: a stable data structure expressed in a standardized graphical format.

From our field work we concluded that a computational coordination mechanism should meet a set of requirements which we expressed as follows: Computational coordination mechanisms should be 'malleable'. This has several implications. A CSCW system of this kind should enable actors to define the protocol of a new coordination mechanism and also to later redefine it, in order to be able to meet changing conditions, by making lasting modification to it. Furthermore, actors should be able to control the execution of the protocol and make local and temporary modifications to its behavior, for example to cope with unforeseen contingencies. In order for actors to be able to define, specify, and control the execution of the mechanism, the protocol should be 'visible' to actors at 'the semantic level of articulation work', i.e., it should be expressed in terms that are meaningful to competent members of the cooperative work arrangement. Moreover, to allow for incomplete initial specification of the protocol, it should be possible for actors to specify the behavior of the computational coordination mechanism incrementally, while it is being executed. And finally, we had observed that coordination mechanisms, even though they were typically developed for handling specific coordination issues, so to speak enter relationships with other mechanisms. More precisely, a particular coordination mechanism will typically be part of a wider complex of interdependent mechanisms. A change to the state of one mechanism may thus have implications for the state of another, and the propagation of state changes from one mechanism to another will therefore have to be taken care of somehow, manually or automatically (Schmidt et al., 1995). Consequently, a computational coordination mechanism should be constructed in such a way that it can be linked to other coordination mechanisms in the wider setting (for the consolidated formulation of this conception, cf. Schmidt and Simone, 1996).

In a systematic attempt to fully understand and, ultimately, meet these requirements an experimental notation was developed, i.e., a set of categories and predicates of articulation work and the rules of their combination. Ariadne, as the notation was called, was designed to enable actors to express, construct, maintain, execute, and link computational coordination mechanisms. (The work involved was extensive and resulted in a large number of publications. For a brief summary of this work, cf. Simone and Schmidt, 1998). In the development of the Ariadne notation, a considered decision was made to postpone the implementation and concentrate on developing a formal specification of its elements and on evaluating it against the requirements and scenarios derived from our field studies. This strategy was adopted, deliberately and explicitly, in order to avoid having the notation influenced, in an implicit and uncontrollable manner, by the inevitable limitations of currently available implementation platforms. The formal specification showed that it was feasible to construct malleable coordination mechanisms by means of the notation. Subsequently, a ‘concept demonstration’ of the formal specification of the notation was implemented in an environment, ABACO, which is particularly suitable to managing relational structures and their behavior. This partial implementation established that the internal architecture of the Ariadne notation is workable (Simone et al., 1995b).

In the context of the overall strategy I have been pursuing, the concept of ‘coordination mechanisms’ has done a good job. It has provided a workable approach for CSCW research to address the realities of complex cooperative work settings and has thus offered an alternative to programs that in my view could cut no ice and which have subsequently been abandoned as viable programs. In addition, we were able to show that the recommended approach was theoretically feasible.

4.3 Coordination Mechanisms Reconsidered

As noted above, we defined a coordination mechanism as consisting of a coordinative protocol as well as a concomitant coordinative artifact. This duality of coordination mechanisms was important for our work, for a number of reasons.

It was crucial that the concept of coordination mechanism was not, as so often happens, instantly dissolved in idle metaphorical talk. It is almost a defining characteristic of present-day intellectual life that any new and interesting concept that arrives on the scene is immediately appropriated, stretched, transformed, abused, and eventually rendered practically useless. And it was obvious that there was a significant temptation to use the term to denote any type and form of convention, from dinner party etiquette to the grammar of ordinary speech. We therefore found it of critical importance to restrict the use of the term to the historically specific class of practices that have developed as an integral aspect of complex cooperative work practices, coordinative practices that are sufficiently standardized and specialized that they are complemented by standardized and specialized coordinative artifacts. Similarly, my colleagues and I did not want the vast array of artifacts that populate cooperative work settings, bug report forms as well as screw drivers, time tables as well as machining stations, to be included under the concept of coordination mechanisms. That would instantly render the concept meaningless. Artifacts 'as such' have nothing other than abstract materiality in common and is thus an empty notion. We wanted to address a specific class of artifacts, namely, *specialized* artifacts, *coordinative artifacts*, that have been devised to serve in a regulatory capacity in cooperative work arrangements and that, thus, are used in accordance with specific sets of rules, namely, *coordinative protocols*. In view of this, and for the sake of intellectual economy, it was assumed—or rather stipulated—that a 'coordination mechanism' is defined by having *one and only one* 'artifact'.

This stipulation had an additional advantage. We had observed that coordination mechanisms, although devised for handling specific coordination issues, are regularly used in conjunction with other mechanisms. The 'one artifact, one protocol' stipulation seemed to make it relatively straightforward to identify and delimit individual coordination mechanisms and thereby to conceive of and construct well-bounded computational (models of) coordination mechanisms that then, again in a relatively straightforward way, could be combined to form complex coordination mechanisms.

Now, let us then look at the costs of that strategy. For analytical purposes the concept of coordination mechanisms has serious shortcomings. Some of the shortcomings were known and had been identified from the beginning or early in the process, namely the ways in which coordinative issues of time and space were dealt with. As far as issues of time were concerned, when the Ariadne notation was devised, we were well aware that we only had a superficial understanding of the issue of time in coordinative practices. As a result, the temporal aspects of coordination could only be expressed as pre- and post-conditions, and as points in time, of course. The problem was duly noted and left for later work. As for spatial issues in coordinative practices, when we began to investigate production control systems such as MRP systems more in depth, it became clear that the coordinative protocols incorporated in these systems could not express spatial aspects of coordination, such as, for instance, limited storage space on the shop floor or in the shipping department. Again the problem was duly noted and left for later work.

More seriously, the deliberate rigor was obtained at a high price. Firstly, the concept of coordination mechanisms was developed on the paradigm of workflows and thus does not support analysts in understanding, describing, or even noticing other kinds of coordinative protocols. Secondly, the ‘one artifact, one protocol’ stipulation is unduly restrictive when used analytically and may lead analysts to engage in futile analytical exercises. I will address the problems in that order.

(1) The concept of coordination mechanisms was developed on the paradigm of pre-established workflows: an MRP system, a *kanban* system, a bug report, a project schedule, a change note. The kind of protocol we took as the exemplar has a fundamentally temporal structure: *when* A has done x, the task (in state x') is to be transferred to B who *then* has to do y.

The fact that we granted privileged status to protocols of this procedural kind was not the result of an oversight. It was obvious already then that other coordinative techniques play a crucial role in cooperative work, most importantly classification schemes. In fact, the Ariadne notation has a slot for ‘conceptual structures’, but these were only treated in a rudimentary manner, subordinate to procedural protocols. Our problem was not one of principle but one of practical expediency: we had not as yet had the opportunity to do proper studies of practices of classification that would have enabled us to address them thoroughly, and so we simply left the problem for later.

Anyway, this makes the concept of ‘coordination mechanism’ overly exclusive when used analytically. It excludes *ab initio* important coordinative practices, not by definition, but due to the framework’s lack of a rich and differentiated set of distinctions. It notoriously makes analysts overlook coordinative artifacts and protocols of crucial importance, for the simple reason that they are not, or apparently not, part of a coordination mechanism.

In sum, then, workflow specifications (schedules, time tables, routing schemes) should be demoted from the status of paradigm to the one of a special case of coordinative artifacts and protocols. Although it may, in some cases, be relevant and appropriate to single out coordination mechanisms from the wider cluster of coordinative practices, the analyst should not be unaware that workflows cannot work if actors are not also using, for example, maps, templates, location designators, etc., as well as classification schemes, nomenclatures, ranking schemes, verification and validation procedures, coding schemes, notations, etc.

(2) As noted above, the ‘one artifact, one protocol’ stipulation turns out to be unduly restrictive too.

On one hand, there evidently are coordinative protocols to which no coordinative artifacts are attached, at least not directly, and protocols evidently exist at multiple levels of abstraction. That this was also realized by us when the framework was developed is manifest from the fact that it was felt necessary to include ‘policies’ (e.g., Simone et al., 1995b). ‘Policies’ were taken to be exactly that: global protocols, with no associated artifacts, that constrain the local specification and application of protocols.

On the other hand, there evidently are coordinative protocols to which multiple artifacts are associated. It would be excessively pedantic or directly meaningless to divide such protocols into a range of discrete sub-protocols to correspond to the various distinct artifacts. To take but a simple example: Organizing a meeting in an organization, a design meeting, say, requires not only that a call or an agenda is issued, perhaps with an attached set of minutes of the preceding meeting, a list of participants, etc., but also a myriad of artifacts such as clocks and calendars, codes for rooms (names or numbers) and often inscriptions of these codes on doors, floor plans, etc. Although all of these artifacts of course presume and imply historically developed skills and conventions,[4] it makes little analytical sense to insist, for each artifact, that the associated protocol must be discrete, nor does it for that matter make much sense to insist, in each and every case, on conceiving of the associated skills and conventions as coordinative protocols. The problem here is, on one hand, that artifacts such as clocks and calendars, besides their use for coordinative purposes in work settings, are used generally in modern civilization, for an infinite variety of purposes, and, on the other hand, that it leaves large white spots on the map of coordinative practices if they are left out.

The coordination mechanism framework may thus engender a rather doctrinarian approach to analysis, and in fact, experience has shown that scholastic debates easily erupt as to whether a particular artifact is or is not part of a particular coordination mechanism. An alarm on the bridge of a ship, for instance? City maps in fire engines? Deployment plans? Access instructions? This conclusion is reinforced when we take into account not just clocks and calendars and maps and floor plans. Let us therefore, in accordance with the strategic aim of not granting privileged status to particular types of coordinative practice, try a fresh look at what is actually there.

(3) What an analyst observes when entering a modern workplace is a plethora of coordinative artifacts. He or she will see bulletin boards, shift staffing plans, vacation plans, phone lists, shelves with dozens and dozens binders, often subdivided by markers, stacks of files on desks and shelves, in- and outboxes, archive boxes, production plans at work stations, production orders, part drawings, product specifications, etc. The analyst should not dogmatically exclude any of these from consideration. We have to understand how they are used, as a heterogeneous totality, in coordinative practices.

For me the occasion for unpacking the concept of coordination mechanisms and resume the original program of embracing coordinative practices in their endlessly rich multiplicity arose when I seriously began to address the issues of classification schemes.

Classification schemes, I knew from the very beginning, is a vitally important phenomenon, but also one for which I did not have anything like proper empirical foundation. I had observed its importance in my various studies, for instance, of the distribution of research papers within the mathematical community, of the handling of labor protection and tariff contract cases in trade unions, etc. In fact, it was to a large degree the realization that classification practices are hugely important in

[4]For excellent accounts of the development of these ‘generic’ script-based coordinative practices, cf. the classic work by Jack Goody (1977, 1987), David Olson (1994), and Alfred Crosby (1997).

cooperative work, which motivated Liam Bannon and myself to highlight 'common information spaces' as a central problem for CSCW (Bannon and Schmidt, 1989; Schmidt and Bannon, 1992).

A serious attempt to get a handle on the issue of classification was undertaken in the middle of the 1990s, in collaboration with Hans Andersen. The study of the design organization in a large Danish manufacturer of water pumps identified some, to us, very interesting coding practices, in particular a 'product key', a coding scheme that, by generating a unique, predictable, and reproducible designation for each of the about 25,000 product variants produced by the company, also and at the same time, generated a rigorous classification of the same items and of the hundreds of thousands of associated documents. The scheme had the additional advantage of being open-ended: changes to the design of a particular product variant, e.g., the introduction of new materials, say Teflon, for sealing a shaft, would be reflected in the coding scheme and thereby in the name and classification of the variant (Schmidt et al., 1995; Andersen, 1997).

But it was not until Ina Wagner and I, from 2001 onwards, jointly undertook a systematic analysis of coordinative artifacts based on the rich set of data she had gathered in the course of her long-term ethnographic study of architectural work practices, that substantial progress was finally made.

4.4 Ordering Systems

Investigating the large and heterogeneous collection of coordinative artifacts Ina Wagner had gathered in her ethnography of an architectural office, we began to understand that a wide variety of ordering principles were at play in the coordinative practices of architectural work. There were, of course, the temporal ordering of workflows (sequential order, stipulated phases, deadlines, versions). But there were also, intertwined with the workflows, a divine multiplicity of ordering principles, such as practices of validating documents (creator, status), as well as practices of identifying and naming documents, practices of association, aggregation, and classification of documents, coding schemes, notations, etc.

Accordingly, we embarked on a meticulous analysis of each of the different coordinative artifacts used by the members of the architectural office, in an effort to reconstruct the practical logic and principles of ordering embodied in their different graphical formats and their interrelationships.

We quickly abandoned all hope of coming back with a finite set of ordering principles. The task we set out to accomplish was, rather, to understand the logic of these practices, without forsaking the observed multiplicity of coordinative artifacts and practices. The strategy we adopted can be said to involve two moves:

(1) *The practical logic of multiplicity*. The multiplicity of coordinative artifacts and protocols we observe in modern cooperative work settings is not accidental; nor is it an artifact of superficial analysis. The multiplicity is constitutive. We have long ago learned from Gerson and Star that 'No representation of the world is either complete or permanent' (Gerson and Star, 1986, p. 257). Coordinative protocols and the principles of ordering they incorporate are constructed to handle issues that are

'local' in the sense that they are limited to a specific activity, process, project, etc. or to a specific coordinative issue. They are specialized constructs.

Rationality is always *local* rationality. This is not because the world is absurd or irrational, as if such a proposition would make sense. Nor is it because we mere mortals, constrained by 'bounded rationality', cannot grasp the rationality of the world, as if it would make sense to conceive of an agent with infinite rationality. Rationality is always local simply because the production of any kind of insight, knowledge, conceptualization, theory, representation, formulae, model, principle, protocol, procedure, scheme, and so on requires time and effort and because time and effort *in practice* are limited resources. Rationality is local for reasons of practical epistemology. The fragmented rationality that we seem to observe in the multiplicity of protocols and artifacts is the result of what Bourdieu has called the economy of practical logic: 'The *economy of logic* [. . .] dictates that no more logic is mobilized than is required by the needs of practice' (Bourdieu, 1980, p. 145). Practitioners are not in a position to indulge in unnecessary 'logic'; to get the job done and in general 'move on', they have to economize on logic, consistency, and coherence.

(2) *The principle of historical specification.*[5] In making sense of the wealth of coordinative practices and their tricky interdependencies and in developing the required rather delicate distinctions, it was of paramount importance to steer well clear of what Gilbert Ryle has called the 'intellectualist legend' (Ryle, 1949). This legend is deeply entrenched in our thinking, it is a cornerstone of our modern mythology and constitutes the key strategic asset of cognitivism. In order to account for intelligent conduct, the intellectualist imputes occult operations of inference of a specifically intellectual nature to any kind of intelligent conduct. With respect to classification in particular, the intellectualist confounds specialized practices such as classification, which have been developed historically and rely on complex literate practices, with an organism's ability to tacitly and immediately discriminate ordinary features of the world such as edible and non-edible things or sad and happy faces. The problem we were facing was the same problem that a decade previously had made us make the *one artifact, one protocol* stipulation, namely, the urge to use concepts indiscriminately, to blur or ignore distinctions, which has gained so much impetus and become so prevalent in the course of the cognitivist movement. But instead of issuing a new stipulation, we were now able to express the criteria much more succinctly. What makes a coordinative protocol what it is (in addition to its specific coordinative function, of course), is not that it has a specific bond to an artifact, but that it is a specific kind of literate practice (that serves a specific coordinative function). In other words, coordinative practices are specialized practices that in turn presume an entire range of other, equally historically specific, literate practices.

In our effort to get a grip on the specifics of the practices we were trying to understand, the work of Ludwig Wittgenstein and Gilbert Ryle again and again helped us to stave off imminent confusion. Furthermore, in our attempt to disentangle the web

[5]The term 'principle of historical specification' was introduced by Karl Korsch (1938).

of interlaced semiological practices as (elements of) coordinative practices and to do so meticulously and accountably, the work of the British 'integrational linguist' Roy Harris has proved to be immensely valuable (cf., e.g., Harris, 1986, 1995, 2000).

As this work progressed and matured, Ina Wagner and I, in our effort to be able to embrace the multifarious nature of coordinative practices in contemporary workplaces as exemplified in the work of architects, developed an approach in which coordinative artifacts and protocols in their infinite variety are taken as the point of departure, without any presumption that they bond or have to bond in specific ways. However, in going beyond the concept of coordination mechanisms, the concepts of coordinative artifacts and protocols were not abandoned at all. They are applied, not as mere subordinate elements of coordination mechanisms, but in an open-ended way, as observable and reportable phenomena. Similarly, the concept of the interlinkage of coordinative protocols and artifacts is not abandoned either but is rather, again, applied in an open-ended way. The emphasis is on how myriads of coordinative protocols and artifacts are related and connected *in different ways* and in an *intricately recursive* manner, and how they form more or less well-defined and more or less tightly coupled clusters. We call such clusters 'ordering systems' (Schmidt and Wagner, 2004).[6] The concept is related to the concept of interlinked coordination mechanisms but does not grant privileged status to a certain kind of coordinative protocol and artifact, nor does it stipulate a strict pairing of the elements. The purpose is to support the analyst in embracing the motley of coordinative practices required in highly complex cooperative work settings.

5 CSCW's Radical Program

As this progress report comes to an end, a few words on the rationale of the entire program are called for. Why would a conceptual foundation for CSCW be needed in the first place? Why the systematically conceptual approach that has been pursued so doggedly? Why not simply build computational artifacts that can be put to good use here and now?

Firstly, has the strategy led me the wrong way? Does the complexity of the research program I have been developing, as evidenced by its still unfinished state, the openness of the whole thing at this stage, indicate that the strategy is somehow deeply flawed, perhaps even mistaken? Might it not, after all, be wiser, to reconsider the alternative strategy, the one of developing technologically advanced 'spaces' of whatever kind for unregulated interaction? Well, that would amount to not answering the question that was asked and answering another one instead. However useful such 'spaces' might be for many purposes, they do not offer anything like a strategy for the complex cooperative work settings, the factories and

[6] We began using the term 'ordering systems' at the same time as the Nestor of systematic zoology, the late Ernst Mayr, began using it in exactly the same way (Mayr and Bock, 2002).

hospitals, pharmaceutical laboratories and architectural offices, for which specialized coordinative protocols and artifacts, ordering systems, and galaxies of ordering systems, are vitally indispensable. Practitioners simply develop these practices in order to somehow master the complexities of their interdependent work.

Furthermore, the apparent simplicity of the 'awareness space' strategy, if one can call it that, is deceptive. The problem with the notion of 'awareness', as it is used in CSCW, is not just that it is poorly understood or that it barely has been defined. The problem is, I have slowly come to realize, that it grows out of an effort to give an explanation where none is needed.[7] The notion of 'awareness' is used as a proxy for a mental state of some kind ('awareness information', or what have you) that the individual produces and that then somehow prompts the individual to adjust his or her conduct accordingly.

As the word 'awareness' has been used in CSCW, by myself and many others, it is what Gilbert Ryle calls a 'heed concept' (1949, pp. 135 ff.). It does not explain a performance by reference to some occult preceding state; it *characterizes* it. Just as the term 'intelligence' cannot be used to explain smart conduct, but rather is used to characterize the conduct in question as inventive, ingenious, clever, diligent, adroit, imaginative, cunning, or whatever is meant in the context, the term 'mutual awareness' does not explain anything, nor does it stand proxy for an explanation, but is rather used for describing that a particular cooperative activity is successfully aligned and meshed, *and* that this was accomplished effortlessly and inconspicuously, without conversations, queries, commands, gesturing, emails, or other kinds of interruptive interaction.

However, the term that was picked for this job, the term 'awareness', is not a 'heed concept' at all. One can be aware of things that one does not take into account (or *heed*) in one's actions. The fact that I did not heed some good advice, *could*, but does not necessarily, imply that I was not aware of it: I could have ignored the advice for all sorts of reasons. 'Awareness' is an 'attention concept'. It was probably picked to do the job for which it has been used because 'being aware' is close to 'realizing', 'conscious of', 'noticing' but still is used quite differently. Alan White, one of Ryle's colleagues, has elaborated the difference well:

> What one is conscious of or what one realises, one must be aware of. But one can become aware of things otherwise than by realising them and one may be aware of them even when one is not conscious of them. [...] Being aware of something is entirely different from noticing something. We may become, remain and cease to be aware whereas we either notice or fail to notice. We can be continuously aware but we cannot continuously notice. "Noticing", but not "being aware", signifies an occurrence. (White, 1964, pp. 41f.)

This is not the place to elaborate these distinctions.[8] The point I am trying to make is that 'awareness' in CSCW has been used as a heed concept and that it, at

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[7] I have sketched a critique of the notion of 'awareness' in CSCW in the article 'The trouble with "awareness"' (2002b) but that critique is merely an outline of what in my view needs to be done in this matter (cf. also, Schmidt and Simone, 2000).

[8] Alan White's work is both an excellent introduction to Ryle and an essential corrective (cf., e.g., White, 1964, 1967, 1982).

the very same time, has had all the usual connotations of 'awareness': that one can 'be aware' without necessarily noticing whatever it is one is or becomes aware of and without realizing it. That is, 'awareness' has been doing not just one but two jobs in CSCW, the job of a heed concept and the job of an attention concept. Hence, I submit, all the confusion. 'Awareness' has been used in two ways: *officially*, to describe that somebody is acting in accordance with the situation, and, *unofficially*, to imply that this is accomplished because of some un- or subconscious processes or mechanisms or some particular mental state. As a result, it has made us look in the wrong direction. It made us search for a mental intermediary where none normally is.

Cooperating actors mutually heed what each other is doing and do so effortlessly and without interrupting ongoing work because they (normally) know the work and hence know what the others are doing, could be doing, should be doing, would not be doing, and so on. They know the drill. Heeding what goes on is part of competent conduct. Their heeding is *also* effortless and seamless because work (normally) takes place in a material setting that is replete with indications of states and processes and activities that, for competent actors (normally) makes it straightforward to assess the state of affairs and know what adjustments might be called for.

Now, if this argumentation holds, this means that we, instead of searching for putative intermediate mental states, should try to identify the strategies competent cooperating actors employ to heed what colleagues are doing etc. How do they discriminate significant states, possible states, problematic states, etc.? What do they monitor *for* in the setting? What is ignored as irrelevant, what is taken into account? And so on. As the next step, this then leads to constructive explorations in an entirely different direction than that of 'space' technologies (no pun intended), namely, in the direction of finding ways in which these very specific monitoring strategies can be supported (by sensor technology, for instance). The conclusion is, then, that in order to support 'mutual awareness' adequately, the strategy would be to develop novel kinds of protocols, based on in-depth analyses of the perceptual and similar coordinative strategies of cooperating actors in complex settings, that among the actors convey selected indications about, for example, significant patterns of states. Ironically, then, instead of abandoning our program of developing technologies to support the coordinative protocols that have been developed in complex cooperative work settings, we should rather explore ways of complementing 'natural protocols' with 'artificial' coordinative protocols.

The program of CSCW, when taken seriously, is indeed an ambitious one. As argued repeatedly over the years, and perhaps most explicitly in my remarks on 'The critical role of workplace studies in CSCW' (2000), the problem for CSCW is a radical one. If CSCW is to deliver on its promise and develop technologies that can support cooperative work as it exists out there, in laboratories, factories, and hospitals, etc., the field must be able to offer technologies that enable ordinary workers to do, in computational environments, what they do now: express coordinative protocols and construct coordinative artifacts (For related approaches to this set of issues, cf., e.g., Klein et al., 2000; Pipek et al., 2009). In contrast to the coordination technologies that do exist and the systems that are being rolled out, the problem for CSCW is that of developing technologies that enable ordinary actors to construct computational devices by means of which they can (a) *regulate* their own complex cooperative activities more efficiently, safely, dependably, etc. and (b) at the same time *control the regulation* of their activities.

More than that. If such means are not developed, the ongoing diffusion of information technologies in working life will undoubtedly cause all sorts of impediments and disruptions, as the ability of workers to manage their everyday coordinative concerns is eroded or lost. These prospects are already becoming reality. The proliferation of network protocols (ftp, email, http, chat, instant messaging, etc.) already creates parallel and mutually exclusive communication channels, which invariably impedes mutual awareness in ways that are strikingly analogous to classical multi-user database systems that were carefully designed to shield actors from experiencing the presence of others and thus create a setting in which actors can only monitor the activities of colleagues with great difficulty and outside of the system (Rodden et al., 1992). This problem is not alleviated but rather compounded by rigid coordinative protocols that are incorporated in groupware systems and in group calendar systems, scheduling systems, booking systems, document management systems, CAD systems, electronic patient records, etc. The various protocols are of course specialized constructs that cannot simply be unified for *all* purposes. The problem is that, *as it is*, actors are left with little or no means of practical integration of the various protocols to get the job done, locally and temporarily—other than doing it manually. Thus, without a rigorous conceptual foundation, collaboration technologies are in risk of becoming part of the problem rather than the solution.

6 For Lack of a Conclusion

The research represented by this book never had the character of a research project. It was never that well delimited. It was rather a research program, or even more to the point: the research primarily aimed at developing a research program. This is reflected in the subtitle of the book: *Contributions to the conceptual foundations of CSCW*.

A conclusion in the standard sense is thus not appropriate. Anyway, this much *has*, after all, been achieved:

(1) Cooperative work has been identified as a phenomenon we can study systematically, as a category of work practice, distinct from its organizational and socio-economic form, and irrespective of what mutual feelings of companionship actors may or may not have. That is, cooperative work practices have been made a researchable phenomenon.
(2) This in turn has cleared a path for making coordinative practices, their methods and techniques, a researchable phenomenon as well.
(3) The research is grounded in investigations of specialized coordinative practices in different settings and has identified key features of these practices: the central role of coordinative artifacts and their associated protocols. It has shown

that coordinative practices often–if not generally–involve entire clusters of such coordinative artifacts, ‘ordering systems’, and that there is what could be called a higher-order logic to this clustering, namely, that the same general schemes and notations are reused and recombined endlessly.

(4) These investigations may also serve, in a loose way, as examples of how investigations of coordinative practices might be performed. Perhaps the main contribution of the research lies here, in offering, certainly not a paradigm, but some examples that other researchers may want to emulate, extend, develop.

(5) It has finally been demonstrated that it is–in principle, at least–technically feasible to create computational environments by means of which ordinary workers, not programmers, can define and execute ‘coordination mechanisms’ in a fully distributed and flexible manner. For this purpose a notation for defining ‘computational coordination mechanisms’ was specified. However, the technical aspects of this research of course lies outside of the scope of the present book.

The research program represented by the articles collected in Part II of this book has made substantial progress but is far from finished. It has only just begun. Many issues are still open, even wide open

Part II
Surveying the Connections

One difficulty with philosophy is that we lack a synoptic view. We encounter the kind of difficulty we should have with the geography of a country for which we had no map, or else a map of isolated bits. We can walk about the country quite well, but when we are forced to make a map we go wrong. A map will show different roads through the same country, any one of which we can take, though not two, just as in philosophy we must take up problems one by one though in fact each problem leads to a multitude of others. We must wait until we come round to the starting point before we can proceed to another section, that is, before we can either treat of the problem we first attacked or proceed to another. In philosophy matters are not simple enough for us to say "Let's get a rough idea", for we do not know the country except by knowing the connections between the roads. So I suggest repetition as a means of surveying the connections.

Wittgenstein, The Yellow Book (1934–1935, p. 45)

Chapter 2
Riding a Tiger, or Computer-Supported Cooperative Work (1991)

The idea of supporting cooperative work by means of computer systems—the very idea!—can be compared with riding a tiger. Cooperative work may seem familiar and tame. And in fact, a plethora of languages and schemes has been furnished that confidently claim to provide reliable models of organizational roles and patterns of communication.

The innocence and familiarity of cooperative work is deceptive, however. Cooperative work is difficult to bridle and coerce into a dependable model. And anyone trying to incorporate a model of a social world in a computer system as an infrastructure for that world is as reckless as a daredevil mounting a Bengal tiger.

The apparent stability of organizational roles and patterns of communication is a superficial hide beneath which a capricious beast is hidden. Cooperative work arrangements should rather be conceived as emerging formations that change dynamically in accordance with the requirements of the situation, and cooperative work involves, inescapably, the vicissitudes of distributed decision making. These characteristics have important implications for CSCW systems design.

1 The Emergent Nature of Cooperative Work

In his concise way, Montesquieu stated that 'Man is born in society and there he remains'. In the same vein, Marx (1857) posited that

> Individuals producing in society — hence socially determined individual production — is, of course, the point of departure. The individual and isolated hunter and fisherman, with whom Smith and Ricardo begin, belong among the unimaginative conceits of the eighteenth-century Robinsonades.

Kjeld Schmidt: 'Riding a tiger, or computer supported cooperative work,' in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference on Computer-Supported Cooperative Work, 24–27 September 1991*, Amsterdam, Kluwer Academic Publishers, Dordrecht, 1991, pp. 1–16.

K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1_2,

Marx' critique of the Robinson Crusoe metaphor is rooted in a conception of work as an intrinsically social phenomenon:

> Production by an isolated individual outside society — a rare exception which may well occur when a civilized person in whom the social forces are already dynamically present is cast by accident into the wilderness — is as much of an absurdity as is the development of language without individuals living *together* and talking to each other. (Marx, 1857)

Society, that is, is ubiquitous. Work is always immediately social in that the object and the subject, the end and the means, the motives and the needs, the implements and the competencies, are socially mediated. The social character of work is not a static property, however; it develops historically. With the ever deeper and increasingly comprehensive social division of labor, the subject and object of work, etc. become increasingly social in character. Hunter-gatherers, for instance, work in an environment that is appropriated socially and yet to a large extent naturally given, whereas, in the case of operators in modern chemical plants, every aspect of work is socially mediated—to the extent that it is conducted in an 'artificial reality'.

While work is always socially organized, the very work process does not always involve multiple people that are mutually dependent in their work and therefore required to cooperate in order to get the work done. We are social animals, but we are not *all* of us *always* and in *every* respect mutually dependent in our work. Thus, in spite of its intrinsically social nature, work is not intrinsically cooperative in the sense that workers are mutually dependent in their work.

The essence of the notion of mutual dependence *in work* is not the negative interdependence among workers using the same resource. They certainly have to coordinate their activities but to each of them existence of the others is a mere nuisance and the less their own work is affected by the others the better. The time-sharing facilities of operating systems for host computers cater for just that by making the presence of other users imperceptible. Being mutual dependent *in work* means that 'A' relies positively on the quality and timeliness of 'B's work and vice versa. 'B' may be 'down stream' in relation to 'A' but in that case 'A' nonetheless will depend on 'B' for feedback on requirements, possibilities, quality problems, schedules etc. In short, mutual dependence in work should primarily be conceived of as a positive, though by no means necessarily harmonious, interdependence.

Due to their being interdependent in conducting their work, cooperating workers have to articulate (divide, allocate, coordinate, schedule, mesh, interrelate, etc.) their respective activities. Thus, by entering into cooperative work relations, the participants must engage in activities that are, in a sense, extraneous to the activities that contribute directly to fashioning the product or service and meeting the need. The obvious justification of incurring this overhead cost and thus the reason for the emergence of cooperative work formations is, of course, that workers could not accomplish the task in question if they were to do it individually, at least not as well, as fast, as timely, as safely, as reliably, as efficiently, etc. (Schmidt, 1990). For example, in a study of the impact of technology on cooperative work among the Orokaiva in New Guinea, Newton (1985) observes that technological innovations for hunting and fishing such as shotguns, iron, torches, rubber-propelled spears, and goggles have made individual hunting and fishing more successful compared to cooperative arrangements. As a result, large-scale cooperative hunting and fishing ventures are no longer more economical or more efficient and they are therefore vanishing. Likewise, the traditional cooperative work arrangements in horticulture for purposes such as land clearing and establishment of gardens have been reduced in scope or obliterated by the influence of the steel axe. A similar shift from cooperative to individual work can be observed wherever and whenever new technologies augment the capabilities of individual workers to accomplish the task individually: harvesters, bulldozers, pocket calculators, word processors, etc.

Cooperative work relations emerge in response to the requirements and constraints of the transformation process and the social environment on one hand and the limitations of the technical and human resources available on the other. Accordingly, cooperative work arrangements adapt dynamically to the requirements of the work domain and the characteristics and capabilities of the technical and human resources at hand. Different requirements and constraints and different technical and human resources engender different cooperative work arrangements.

As befits an emergent phenomenon, cooperative work develops historically. For example, agricultural work and craft work of pre-industrial society was only sporadically cooperative. Due to the low level of division of labor at the point of production, the bulk of human labor was exerted individually or within very loosely coupled arrangements. There were, of course, notable exceptions to this picture such as harvest and large building projects (e.g., pyramids, irrigation systems, roads, cathedrals), but these examples should not be mistaken for the overall picture.

Cooperative work as a systematic arrangement of the bulk of work at the point of production emerges in response to the radical division of labor in manufactories that inaugurated the Industrial Revolution. In fact, systematic cooperation in production can be seen as the 'base line' of the capitalist mode of production. However, cooperative work based on the division of labor in manufactories is essentially amputated: the interdependencies between the specialized operators in their work are mediated and coordinated by means of a hierarchical systems of social control (foremen, planners etc.) and by the constraints embodied in the layout and mode of operation of the technical system (conveyer belt etc.). In Marx' words:

> To the workers themselves, no combination of activities occurs. Rather, the combination is a combination of narrow functions to which every worker or set of workers as a group is subordinated. His function is narrow, abstracted, partial. The totality emerging from this is based on this partial existence and isolation in the particular function. Thus, it is a combination of which he constitutes a part, based on the his work not being combined. *The workers are the building blocks of this combination.* The combination is not their relationship and it is not subordinated to them as an association. (Marx, 1861–1863, p. 253)

The societal precondition for the prevalence of this 'fetishistic' form of cooperative work is that manufacturing and administrative organizations are in control of their environment to the extent that they can curtail its complex and dynamic character. By severely limiting the range of products and services offered and by imposing strict schedules and procedures on their customers and clientele, organizations in branches of mass production and mass-transactions processing were able to contrive synthetic work settings where activities, for all practical purposes, could be assumed to be subsumed under preconceived plans.

In view of the fundamental trends in the political economy of contemporary industrial society, the 'fetishistic' form of cooperative work is probably merely a transient form in the history of work. Comprehensive changes of the societal environment permeate the realm of work with a whole new regime of demands and constraints. The business environment of modern manufacturing, for instance, is becoming rigorously demanding as enterprises are faced with shorter product life cycles, roaring product diversification, minimal inventories and buffer stocks, extremely short lead times, shrinking batch sizes, concurrent processing of multiple different products and orders, etc. (cf. Gunn, 1987). The turbulent character of modern business environments and the demands of an educated and critical populace, compel industrial enterprises, administrative agencies, health and service organizations, etc. to drastically improve their innovative capability, operational flexibility, and product quality. To meet these demands, work organizations must be able to adapt rapidly and diligently and to coordinate their distributed activities in a comprehensive and integrated way. And this requires horizontal and direct cooperation across functions and professional boundaries within the organization or within a network of organizations.

In short, the full resources of cooperative work must be unleashed: horizontal coordination, local control, mutual adjustment, critique and debate, self-organization. Enter CSCW.

In order to support and facilitate the articulation of distributed and dispersed work activities, modern work organizations need support in the form of advanced information systems. This is illustrated by the efforts in the area of Computer Integrated Manufacturing to integrate formerly separated functions such as design and process planning, marketing and production planning, etc., and by the efforts in the area of Office Information Systems to facilitate and enhance the exchange of information across geographical distance and organizational and professional boundaries. Common to the efforts in these very different areas are the issues explored by CSCW: How can computer systems assist cooperating ensembles in developing and exercising horizontal coordination, local control, mutual adjustment, critique and debate, self-organization?

These issues all revolve around the problem of the distributed character of cooperative work.

2 The Dialectics of Cooperative Work

Cooperative work is, in principle, distributed in the sense that decision making agents are semi-autonomous in their work.

Situated action. Reality is inexhaustible. The contingencies encountered in any human action—'in the fog of war,' as Clausewitz aptly put it—invariably defeat the very best plans and designs. As pointed out by Suchman (1987), 'the relation of the intent to accomplish some goal to the actual course of situated action is enormously contingent.' Plans may of course be conceived by actors prior to action but they are not simply executed in the actions. Action is infinitely rich compared to the plan and cannot be exhausted by a plan.

Since the circumstances encountered in human action defeat the very best plans and designs, each individual encounters contingencies that may not have been predicted by his or her colleagues and that, perhaps, will remain unknown to them. Each participant in the cooperative effort is faced with a—to some extent—unique local situation that is, in principle, 'opaque' to the others and have to deal with this local situation individually. For example: misplaced documents, shortage of materials, delays, faulty parts, erroneous data, variations in component properties, design ambiguities and inconsistencies, design changes, changes in orders, cancellation of orders, rush orders, defective tools, software incompatibility and bugs, machinery breakdown, changes in personnel, illness, etc.

No goal or criterion applies to all contingencies. In order to handle local contingencies effectively, actors may have to apply criteria that violate even putatively global criteria such as corporate policy. In fact, on closer examination the putative global goals and criteria are also local in the sense that they are formulated in specific contexts as answers to specific questions.

Thus, due to the 'situated' nature of human action, cooperative work arrangements take on an indelible distributed character. No agent in the cooperative ensemble is omniscient

Incommensurate perspectives. Reality is inexhaustible in another sense too. The world defies unitary and monolithic conceptualizations. As pointed out by Gerson and Star (1986), 'no representation of the world is either complete or permanent.' A representation is a 'local and temporary closure.' Accordingly, a multiplicity of distinct perspectives is required to match the multiplicity of the field of work. A perspective, in this context, is a particular—local and temporary—conceptualization of the field of work, that is, a conceptual reproduction of a limited set of salient structural and functional properties of the object, such as, for instance, generative mechanisms, causal laws, and taxonomies, and a concomitant body of representations, e.g., models, notations, etc. Thus, to grasp of the diverse and contradictory aspects of the field of work as a whole, the multifarious ontological structure of the field of work must be matched by a concomitant multiplicity of perspectives on the part of the decision-making ensemble (Schmidt, 1990). Accordingly, the cooperative ensemble reproduces the multiplicity of its environment in the form of the multiplicity of 'small worlds' of professions and specialties.

There are two aspects to the multiplicity of perspectives.

First, as demonstrated by Rasmussen in a number of studies (e.g., Rasmussen, 1979, 1985), a stratified structure of conceptualizations is characteristic of a number of work domains. In technical domains, for example, Rasmussen has identified five levels of abstraction in a *means-end hierarchy.*

Second, perspectives are not always related to conceptual *levels* in the sense of a stratified order, however (Rasmussen, 1988). In addition to conceptualizations as different levels of generative mechanisms or means-end relationships, conceptualizations may reflect *different functional requirements* that are contradictory in the sense that efforts directed at solving one functional problem interfere with efforts directed toward the others. That is, contradictory ends divide the field of work into distinct object domains, orthogonal to the levels of abstraction of the means-end hierarchy.

An omniscient and omnipotent agent to match the multifarious environment of modern work does not exist. The application of multiple perspectives—whether stratified conceptualizations such as means-end relationships or the orthogonal conceptualizations of distinct object domains—will typically require the joint effort of multiple agents, each attending to one particular perspective and therefore engulfed in a particular and parochial small world. So, in addition to the distributed character of cooperative work stemming from the contingent nature of work, cooperative work in complex settings is distributed in the profound sense that the cooperative ensemble is divided into myriads of small worlds with their own particular views of the world.

This dissolution must be overcome, however. The cooperative ensemble must interrelate and compile the partial and parochial perspectives by transforming and translating information from one level of conceptualization to another and from one object domain to another (Schmidt, 1990). Again there is no omniscient and omnipotent agent to perform these transformations and translations. Rather, the transformations and translations are performed in the context of specific situations, to solve particular problems. The generalizations by means of which the partial perspectives are integrated are not globally valid; they are merely satisfactory to solve the problem at hand. They are local and temporary closures.

Bucciarelli (1984) has provided an excellent example of this aspect of cooperative work. In a study of cooperative work in engineering design he observed that

> different participants in the design process have different perceptions of the design, the intended artefact, in process. What an engineer in the Systems Group calls an interconnection scheme, another in Production calls a junction box. To the former, unit cost and ease of interconnection weigh most heavily; to the latter, appearance and geometric compatibility with the module frame, as well as unit cost, are critical.
>
> The task of design is then as much a matter of getting different people to share a common perspective, to agree on the most significant issues, and to shape consensus on what must be done next, as it is a matter for concept formation, evaluation of alternative, costing and sizing — all the things we teach.

This also applies to the propagation of goals and criteria from one level of conceptualization to another. Propagation of goals and criteria within a cooperative ensemble is not a simple ‘decomposition’ or a syllogistic inheritance operation but involves a conceptual translation and a transformation of representations (Rasmussen, 1988). Again, there is no omniscient and omnipotent agent to perform these transformations and translations.

An interesting issue, raised by Charles Savage in a ‘round table discussion’ on Computer Integrated Manufacturing, illustrates this issue quite well:

> In the traditional manual manufacturing approach, human translation takes place at each step of the way. As information is passed from one function to the next, it is often changed and adapted. For example, Manufacturing Engineering takes engineering drawings and

> red-pencils them, knowing they can never be produced as drawn. The experience and collective wisdom of each functional group, usually undocumented, is an invisible yet extremely valuable company resource. (Savage, 1987)

This fact is ignored by the prevailing approach to CIM, however:

> Part of the problem is that each functional department has its own set of meanings for key terms. It is not uncommon to find companies with four different parts lists and nine bills of material. Key terms such as *part*, *project*, *subassembly*, *tolerance* are understood differently in different parts of the company.

The problem is not merely terminological. It is the problem of multiple incommensurate perspectives. The effort to 'design for assembly,' for example, requires an 'iterative dialogue' involving guardians of incommensurate perspectives: Assembly, Subassembly, Parts Processing, Process Planning, Design, Marketing, etc. The issue raised by Savage is rooted in the multiplicity of the domain and the contradictory functional requirements. In Savage's words: 'Most business challenges require the insights and experience of a multitude of resources which need to work together in both temporary and permanent teams to get the job done'.

In sum, in complex work settings the multiplicity of the field of work is matched by multiple 'small worlds', each specialized in applying a particular perspective. There is no omniscient and omnipotent agent to match the multifarious environment or to integrate the specialized and local knowledge.

Incongruent heuristics. In complex environments, decision making is performed under conditions of excessive complexity and incomplete, missing, erroneous, misrepresented, misunderstandable, incomprehensible, etc. information and will thus require decision makers to exercise discretion. In discretionary decision making, however, different individual decision makers will typically have preferences for different heuristics (approaches, strategies, stop rules, etc.). Phrased negatively, they will exhibit different characteristic 'biases'. By involving different individuals, cooperative work arrangements in complex environments are arenas for different decision making strategies and propensities (Schmidt, 1990). Thus, the decision making process of the cooperating ensemble as a whole is distributed in the sense that the agents involved are semi-autonomous in selecting their heuristics.

However, in order to ensure a satisfactory degree of consistency and objectivity in the performance of the ensemble as a whole and thus to meet the requirements of the environment in terms of product quality, reliability, safety etc., the different heuristics must be integrated. To ensure this integration of heuristics, the different decision makers subject the reliability and trustworthiness of the contributions of their colleagues to critical evaluation. This way they are able, as an ensemble, to arrive at more robust and balanced decisions.

For example, take the case of an 'experienced and skeptical oncologist,' cited by Strauss and associates (Strauss et al., 1985, pp. 21 f.):

> I think you just learn to know who you can trust. Who overreads, who underreads. I have got X rays all over town, so I've the chance to do it. I know that when Schmidt at Palm Hospital says, "There's a suspicion of a tumor in this chest," it doesn't mean much because she, like I, sees tumors everywhere. She looks under her bed at night to make sure there's not some

> cancer there. When Jones at the same institution reads it and says, "There's a suspicion of a tumor there," I take it damn seriously because if he thinks it's there, by God it probably is. And you do this all over town. Who do you have confidence in and who none.

This process of mutual critical evaluation was described by Cyert and March (1963) who aptly dubbed it 'bias discount.' Even though dubious assessments and erroneous decisions due to characteristic biases are transmitted to other decision makers, this does not necessarily entail a diffusion or accumulation of mistakes, misrepresentations, and misconceptions within the decision-making ensemble. The cooperating ensemble establishes a negotiated order.

Incongruent interests. Any cooperative work arrangement is a tricky—or, in the terminology of 'dialectical logic', 'contradictory'—phenomenon in so far as it is a phenomenon of *individuals working together*. On one hand, since the individuals are mutually dependent in their work, the work of the individual is a particular functional element of the concerted effort of the cooperating ensemble as a totality. But on the other hand, work is an individual phenomenon in so far as labor power happens to be tied to individuals and cannot be separated from the individuals. That is, a cooperative work process, is performed by individuals with individual interests and motives. Because of that, cooperative ensembles are coalitions of diverging and even conflicting interests rather than perfectly collaborative systems. Thus, in the words of Ciborra (1985), the use of information for 'misrepresentation purposes' is a daily occurrence in organizational settings. The Russian proverb saying that 'Man was given the ability of speech so that he could conceal his thoughts' applies perfectly to the use of information in organizations.

In sum, then, cooperative work in complex settings is, in principle, distributed in the sense that decision making agents are semi-autonomous in their work in terms of: goals, criteria, perspectives, heuristics, and interests and motives. There is no omniscient and omnipotent agent.

The design of CSCW systems is therefore faced with the challenging problem of supporting the exchange and integration of information within a self-organizing cooperative ensemble of decision makers that have a high degree of autonomy in their cognitive strategies and conceptualizations.

This makes the question of modelling cooperative work and the incorporation of such models in computer systems come to the fore.

3 The Precarious Use of Models in CSCW

A computer system embodies a model of another system in the 'real world', e.g., in the simple case of a payroll system, a model of the wage calculation system (tariffs etc.) and the staff of the company (names, positions, account numbers etc.).

Models, however, are limited abstractions; they are only valid within a limited area of application. Thus, a computer system will inevitably encounter situations in which the underlying model of the world is no longer valid. With simple systems the user is normally able to know immediately if and when the system's world model does not apply and to take the necessary corrective measures. However, the more complex the system, the more obscure the validity of the system's performance. Thus, as pointed out by Roth and Woods (1989), a 'critical element for effective intelligent systems is that they provide some mechanisms to facilitate the detection and resolution of cases that fall outside their bounds.' This facility is rarely provided, however: 'One of the major failure modes that we have observed in AI systems is to not provide support for the human problem-solver to handle cases where the AI system is beyond its bounds.'

Like any other computer system, a CSCW system is based on a model of an aspect of the world, in this case a model of a social world. And like any other model, a model of a social world has an application area within which it is a valid—abstract and limited—representation of the world. That is, there is a boundary beyond which the model is invalid. Thus a CSCW system is inevitably placed in a situation beyond the bounds of the underlying model. The critical question is what happens to the cooperating ensemble using this system when the underlying model of cooperative relations is beyond its bounds? Unlike a typical expert system, a CSCW system is not controlled by a single agent in a position to switch the machine off if its performance is blatantly unsatisfactory. A CSCW system is part and parcel of the infrastructure of the cooperating ensemble it supports. Thus, with the conventional 'automation' paradigm, CSCW systems are disasters to come. Therefore, CSCW systems should not be designed on the assumption that the system will automate the functions of articulation work. To the contrary, users should be in full control of the system so that they are able to know and maintain control when the system is beyond its bounds.

Let us therefore look into the problems of modelling cooperative work in CSCW design.

Different aspects of the social world is modelled in the different approaches to CSCW systems design. For example, even a CSCW facility as 'generic' as a shared view system, must provide a floor-control protocol for managing turn-taking. Of the more elaborate approaches to modelling cooperative work, two categories are of particular relevance here: models of organizational structures and models of conceptual structures.

Models of organizational structures. In the Office Automation tradition, systems incorporated a model of a canonical allocation of tasks and responsibilities or prescribed patterns of communication (e.g., Zisman, 1977; Ellis, 1979; Ellis and Nutt, 1980; Hammer and Kunin, 1980; Hammer and Sirbu, 1980; Ellis, 1983). Although this approach has been stubbornly perpetuated under the CSCW label (e.g., Sluizer and Cashman, 1984; Smith et al., 1991; Victor and Sommer, 1991), it was critiqued accurately in 1983 by Barber, de Jong, and Hewitt:

> In all these systems information is treated as something on which office actions operate producing information that is passed on for further actions or is stored in repositories for later retrieval. These types of systems are suitable for describing office work that is structured around actions (e.g. sending a message, approving, filing); where the sequence of activities is the same except for minor variations and few exceptions. [. . .] These systems do not deal well with unanticipated conditions. (Barber et al., 1983, p. 562)

In the dynamic environments characteristic of modern work settings, work articulation by means of execution of pre-established schemes of task allocation, procedures, plans, and schedules is no longer adequate. Rather, the radical transformation of work and its organization calls for an 'open systems' approach. In the words of Gerson and Star (1986):

> Every real-world system is an open system: It is impossible, both in practice and in theory, to anticipate and provide for every contingency which might arise in carrying out a series of tasks. No formal description of a system (or plan for its work) can thus be complete. Moreover, there is no way of guaranteeing that some contingency arising in the world will not be inconsistent with a formal description or plan for the system. [. . .] *Every real-world system thus requires articulation* to deal with the unanticipated contingencies that arise. Articulation resolves these inconsistencies by packaging a compromise that 'gets the job done,' that is, closes the system locally and temporarily so that work can go on.

In the analysis of conventional mass-production and mass-transaction processing organizations a cautious and guarded abstraction from the 'open' nature of the system is legitimate and provides valuable insight. The current transformation of work, makes a complete inversion of perspective mandatory. Instead of conceiving of the work organization as a closed and stable system, subject to local and temporary disturbances, a work organization under contemporary conditions should be conceived of as an open system that reduces complexity and uncertainty by local and temporary closures. Thus, in view of the dynamic nature of the environment facing modern work organizations, patterns of cooperative work relations should be conceived as being, in principle, ephemeral.

An alternative approach to the OA tradition, suggested and explored by Barber and Hewitt, posited that systems should embody an explicit representation of the goal structure of the organization: 'This builds a teleological structure of the office work within the computer' (Barber and Hewitt, 1982; Barber, 1983). Thus the system provides a resource to handle unexpected contingencies. However, as pointed out by Woo and Lochovsky (1986), while such systems (for instance, Barber's OMEGA) may be useful for office applications that are *logically centralized* and involve only a single user in performing the work, they do not support the *distributed nature of cooperative work*: 'Supporting distributed, yet cooperative, office activities by providing a logically centralized office system (i.e., gathering the knowledge of all office workers involved in performing a task into a global and consistent knowledge base) creates a number of problems.' First, cooperative work in complex environments involves integration of specialized conceptualizations, and 'converting specialized, yet cooperative, office procedures to fit an integrated environment will not be easy since it requires the integrator to have knowledge of all the different kinds of specialization.' And second, 'In a logically centralized office system, inconsistent office procedures, specified by different office workers, are not allowed.' In spite of intentions, the approach suggested by Barber and Hewitt assumes the intervention of an omniscient and omnipotent agent.

Models of conceptual structures. Even in systems that do not prescribe procedures for human interaction but, rather, provide facilities for a community to cooperate via a common information space (Bannon and Schmidt, 1989; Schmidt and

Bannon, August 1991, 'CSCW, or what's in a name?', Unpublished Manuscript), the conceptual structure of that space is in itself a model of aspects of a social world. A taxonomy, for instance, is a negotiated order.

Engelbart and Lehtman (1988) have outlined an ambitious vision of a 'system designed to support collaboration in a community of knowledge workers.' Such a system should support the creation, modification, transmission etc. of messages, as well as cross-referencing, cataloging and indexing of the accumulating stock of messages. With services such as these, they claim, 'a community can maintain a dynamic and highly useful "intelligence" database.' And they propose extending this facility toward

> the coordinated handling of a very large and complex body of documentation and its associated external references. This material, when integrated into a monolithic whole, may be considered a "superdocument." Tools for the responsive development and evolution of such a superdocument by many (distributed) individuals within a discipline- or project-oriented community could lead to the maintenance of a "community handbook," a uniform, complete, consistent, up-to-date integration of the special knowledge representing the current status of the community.
>
> The handbook would include principles, working hypotheses, practices, glossaries of special terms, standards, goals, goal status, supportive arguments, techniques, observations, how-to-do-it items, and so forth. An active community would be constantly involved in dialogue concerning the contents of its handbook. Constant updating would provide a "certified community position structure" about which the real evolutionary work would swarm.

While this 'community handbook' effectively addresses the issue of supporting cooperation via a common information space, there is no omniscient and omnipotent agent to ensure that the special and local knowledge of the different semi-autonomous agents is integrated in 'a uniform, complete, consistent, up-to-date' way. A 'uniform, complete, consistent, up-to-date' community handbook is simply a chimera.

First, the data incorporated in the community handbook will be incomplete. It is simply a question of the benefit versus the cost of entering or capturing 'all' data, whatever that may mean. In fact, the community handbook will be a coarse representation of the diversified and multifarious reality of the community.

Second, the data incorporated in the community handbook will not be indexed consistently. The system would of course provide a global classification scheme to support the distributed indexing of information items to be included in the database, for example, taxonomies and thesauri. Such a classification scheme is itself an partial and temporary conceptualization, however. In order to include an information item in the database, an agent needs to *interpret* the conceptual structure of the classification scheme, *relate* it to the specialized conceptualizations of his or her particular perspective, and *translate* it to local circumstances. That is, the scheme will not be applied uniformly, and the database will over time become inconsistent.

And third, the conceptual structure of the community handbook as embodied in the classification scheme is itself of local and temporary validity. The semantics of categories will change and new categories will emerge. In order not to deteriorate, the scheme must evolve with the conceptual evolution of the community it is a reflection of. Integration of the diversified work activities of modern organizations requires that actors from the different sub-domains and specialties involved negotiate a shared understanding. Because of the incommensurate perspectives involved, a shared understanding is a local and temporary closure destined to break down in face of a diversified and dynamic environment. To support the ongoing integration work, then, the taxonomies and classification schemes embodied in and supporting company-wide databases and other integrated business systems must be maintained, reinterpreted, adapted, etc. by means of an ongoing cooperative effort. That is, the conceptual structure of the 'community handbook' is itself subject to the vicissitudes of distributed decision making and it will thus itself be incomplete and inconsistent.

In short, irrespective of the approach taken to modelling cooperative work for CSCW systems design, it is a precarious undertaking.

We do not have to despair, though.

The problem with incorporating models of plans (established procedures, organizational structures, or conceptual schemes) in computer systems is not that plans are fictitious. Rather, plans serve a heuristic function in action by identifying constraints, pitfalls and strategic positions in the field of work. As observed by Suchman (1987), in order to serve this heuristic function 'plans are inherently vague'. Thus, in Suchman's conception,

> plans are resources for situated action, but do not in any strong sense determine its course. While plans presuppose the embodied practices and changing circumstances of situated action, the efficiency of plans as representations comes precisely from the fact that they do not represent those practices and circumstances in all of their concrete detail.

In fact, 'plans' may serve different functions. Consider organizational procedures, for example: Procedures may of course codify 'good practice,' recipes, proven methods, efficient ways of doing things, work routines. In flexible work organizations such procedures are of little value and may actually impede flexibility. However, a procedure may also convey information on the functional requirements to be met by the process and the product; it may highlight decisional criteria of crucial import; it may suggest a strategy for dealing with a specific type of problems (e.g., which questions to address first?); it may indicate pitfalls to avoid; or it may simply provide an *aide memoir* (such as a start procedure for a power plant or an airplane). And third, a procedure may express some statutory constraints in which case disregard of the procedure may evoke severe organizational sanctions. More often than not, a particular procedure will express, in some way, all of these different functions. Whatever the function, however, organizational procedures are not executable code but rather heuristic and vague statements to be interpreted and instantiated, maybe even by means of intelligent improvisation

Therefore, instead of pursuing the elusive aim of devising models that are not limited abstractions and thus in principle brittle when confronted with the inexhaustible multiplicity of reality, models of cooperative work in CSCW systems (whether procedures, schemes of allocation of tasks and responsibilities, or taxonomies and thesauri, etc.) should be conceived of as *resources* for competent and responsible workers. That is, the system should make the underlying model accessible to users and, indeed, support users in interpreting the model, evaluate its rationale and implications. It should support users in applying and adapting the model to the situation at hand; i.e., it should allow users tamper with the way it is instantiated in the current situation, execute it or circumvent it, etc. The system should even support users in modifying the underlying model and creating new models in accordance with the changing organizational realities and needs. The system should support the documentation and communication of decisions to adapt, circumvent, execute, modify etc. the underlying model. In all this, the system should support the process of negotiating the interpretation of the underlying model, annotate the model or aspects of it etc.

An approach similar to this has been explored in some 'shared view' systems. Cooperative work in real world settings is characterized by immense flexibility because people proficiently utilize the rich resources of everyday conversation to handle contingencies. It has therefore been argued (Greenberg, 1990) that 'shared view' systems should provide support for a broad variety of modes of interaction (turn–taking protocols etc.) and, most importantly, provide support for users to control the choice of mode of interaction.

Likewise, in the case of models of organizational structures, CSCW systems to support flexible work organizations should not impose prescribed or pre-established patterns of cooperative work relations. Rather, CSCW systems should provide facilities allowing users to interpret and explore prescribed procedures and formal structures as well as conventional patterns of communication, and leave it to the users to abide by or deviate from norm and practice according to their professional judgment of the contingencies of the current and local situation. That is, in CSCW systems, models of organizational structures should be presented as heuristic information that users can appropriate, explore, modify, negotiate, reject, circumvent, or execute according to the contingencies of the situation.

Similarly, in the case of models of conceptual structures, a CSCW system should provide facilities supporting users in appropriating, exploring, modifying, negotiating etc.—cooperatively and yet distributed—'community handbooks' that are openly incomplete and inconsistent.

Providing support for distributed cooperative appropriation, circumvention, modification of the system is, perhaps, the toughest challenge in designing computer systems for cooperative work. Is it possible to formulate general principles of the design of the functional allocation between humans actors and a CSCW artifact so that the cooperating ensemble can maintain control of the situation when the underlying model is beyond its bounds? Which aspects of social systems are suitable for being modelled in CSCW systems? Roles, procedures, rules of conduct, patterns of communication, conceptual structures? What are the specific problems and limitations of different kinds of models? How can users be supported in designing models of their world for incorporation in CSCW systems? How should the underlying model of the system be made visible to users? How should different users perceive the model? How and to which extent can it be made malleable? Should all users really be allowed to circumvent all constraints of the system? Is it possible to support users in anticipating the consequences of a circumvention or modification under consideration? Should a circumvention affect other users? How should a circumvention of the model be logged, reported, and presented to other users? And so forth. Questions such as these are still open issues in research and development of computer systems for cooperative work in complex and dynamic settings.

Chapter 3
Taking CSCW Seriously: Supporting Articulation Work (1992)

As a research field, CSCW is distinct from any of the fields on which it draws.

Irene Greif *(1988b)*

While the area of Computer Supported Cooperative Work, or CSCW, appears to have established itself as a research field in its own right over the last few years, judging from the wealth of conferences and papers devoted to the topic, confusions concerning the very nature of the field continue to surface. Differences abound, for example, as to the centrality of the 'group' in CSCW, as to what is or is not regarded as a CSCW system, the relation between CSCW and what has been termed Groupware, etc. Indeed, at times, it appears that there is no coherent conception of the area at all!

In this paper, we put forward our own conceptualization of CSCW, firmly anchored in a framework that pays attention to the computer support requirements of cooperative work, and briefly discuss how this view relates to other popular conceptions of the field. However, it is important to note that the paper is not intended as an introduction or overview of the field per se. Rather it is aimed at readers who already are interested or involved in the area, yet concerned about its focus and direction. The paper is thus programmatic, in that it lays out a set of arguments as to how to conceive of the CSCW field, and the consequences of taking this conception seriously, in terms of a research agenda. Our intent is not to contribute to

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Kjeld Schmidt and Liam J. Bannon: 'Taking CSCW seriously: Supporting articulation work,' *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 1, no. 1–2, 1992, pp. 7–40.

[Footnote in original version:] This paper has had a lengthy gestation period, and aspects of the arguments have appeared elsewhere. Some of the points made here first surfaced in a short polemic by the authors entitled "CSCW: Four Characters in Search of a Context," appearing in the *Proceedings of the First European Conference on Computer Supported Cooperative Work*, September 1989, Gatwick, UK. (Subsequently published in Bowers and Benford (1991)). Major revisions of this paper, under the title "CSCW, or What's in a Name?" were made in 1990, and again in 1991, and copies of these manuscripts were distributed and discussed widely. The present paper is completely re-written with substantively new argumentation and material.

K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1_3,

sterile definitional debates prescribing what is or is not CSCW, but to construct a conception of the field that allows us, i.e. those active within CSCW, to have some common reference point, and some understanding of what are important questions for the field.[1] We then pursue the consequences of our approach in the remainder of the paper, discussing a number of important research issues for the field of CSCW which emerge from the particular framework outlined here.

The paper is organized as follows. As context for our discussion, some background information on the CSCW field is presented in Section 1. This is not intended as a comprehensive account of the field, and readers interested in more descriptive accounts should look elsewhere.[2] We then proceed to formulate our conceptualization of the field in Section 2, focusing on the nature of cooperative work, the need for computer support, and how the field or arena of CSCW could be viewed as a timely response to these concerns. Following this, we investigate aspects of an important issue—support for articulation work—that emerge as a core concern for the field as we have conceptualized it.

1 Some Background

The term 'Computer-Supported Cooperative Work' was first used by two researchers (Irene Greif and Paul Cashman) back in 1984, to describe the topic of an interdisciplinary workshop they were organizing on how to support people in their work arrangements with computers (Greif, 1988a). The meaning of the individual words in the term was not especially highlighted. Subsequently the term was abbreviated to CSCW, and discussion on the meaning of this somewhat unwieldy label describing the field was expected to disappear. This has not occurred. This may be in part due to the fact that both the boundaries of the field and its focus are still somewhat unclear, despite numerous attempts to clarify the nature and breadth of the field (Bannon et al., 1988; Greif, 1988a; Bannon and Schmidt, 1989; Ellis et al., 1991; Grudin, 1991). Indeed, at one point it appeared as if the label CSCW was simply being used as a umbrella term under which a variety of people from different disciplinary perspectives could discuss aspects of computer system design and use by people (Bannon et al., 1988). It is hard to see how anything in the form of a coherent research area could emerge from such a loose description. At the same time, it is not surprising that in the early stages of the emergence of a new field, which is actively being constituted by a variety of groups with quite different disciplinary

[1] On occasion, readers of the draft ms. have complained of an overly prescriptive tone to the paper. It is true that at times we take the liberty of saying that 'the field of CSCW should'—focus on certain issues, or take certain directions. However, our intent with these remarks is simply to highlight issues in the field that we believe are important, indeed, which we are currently investigating, and arise out of the underlying conceptualization of CSCW that we argue for here. Our intent is not to police the field, but to provide some rationale for why a certain agenda for the area makes sense. We welcome debate with those who hold other conceptualizations of the field that lead them to formulate alternative research programmes to that presented here.

[2] See, for example, Bannon and Schmidt (1989), Grudin (1991), and Wilson (1991).

perspectives, there is some difficulty in agreeing on what are the core issues for the new field, and how they should be handled. Indeed, any such development must also take into account the politics of the situation, as the emerging field becomes an arena where different conceptualizations of the area are fought over.

Attempts at a more coherent definition of the field have also appeared over the years. In the introduction to an important collection of readings in CSCW, Greif (1988b, p. 5) defines CSCW as '. . . an identifiable research field focused on the role of the computer in group work'. This focus on the 'group' as the unit of analysis for CSCW will be subjected to some scrutiny later in the paper (Section 2.2.2). An alternative formulation of the field, that has since been adopted by many researchers, is that proposed in Bannon and Schmidt (1989): 'CSCW should be conceived as an endeavor to understand the nature and characteristics of cooperative work with the objective of designing adequate computer-based technologies'. This definition focuses on understanding the nature of cooperative work as a foundation to designing information systems to support the work. Suchman (1989, p. 1) provides a somewhat related but weaker definition of CSCW: '. . . the design of computer-based technologies with explicit concern for the socially organized practices of their intended users.' The focus here is more loosely on the sociality of work, rather than on specific characteristics of cooperative work as a distinct category of work (see Section 2.2 for an elaboration).[3]

Many in the field of CSCW simply refer to the area by the term Groupware. This term is often used by people who focus on the design of software that supports group work.[4] For a period in the development of the field, the wisdom of substituting one term for the other was debated, with concern being expressed that the Groupware label connoted too narrow a view of the field of CSCW, with the former's emphasis solely on developing software for 'group' processes (Bannon and Schmidt, 1989). The issue no longer stirs much controversy, with both terms still popular, and most proponents of Groupware quite aware of the multiplicity of cooperative work arrangements besides that of the 'group'. While many commercial reports still refer to the Groupware field, the term CSCW has come to be preferred in the research community due to its more comprehensive remit, despite its admitted awkwardness (Greenberg, 1991).

2 A Conceptualization of CSCW

According to the British sociologist of science, Richard Whitley, a research area is constituted by a *problem situation:* 'A research area can be said to exist when scientists concur on the nature of the uncertainty common to a set of problem situations' (Whitley, 1974). Applying this criterion to our topic, we may ask what

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[3]Given this work, Kling's claim that CSCW embodies 'a "worldview" that emphasizes convivial work relations' is surprisingly off the mark (Kling, 1991, p. 83).

[4]The original usage of the term (Johnson-Lenz and Johnson-Lenz, 1982) differs somewhat from its prototypical use.

are the problem situations addressed by researchers working under the CSCW label? Are the problem situations in fact related? Do scientists in the area actually concur on the uncertainty common to this set of problem situations? Are they exploring the same basic issues? This is questionable when one notes that the CSCW label seems to be applied to just about any application, such as: face-to-face meeting facilitation, desk-top presentation, project management, multi-user applications, text-filtering software, electronic mail, computer conferencing, hypertext, etc. Also studies formerly appearing under the rubric of Office Information Systems or Computer Mediated Communication now appear regularly under the CSCW banner.

2.1 The Approach of CSCW: Computer Support

By virtue of the first part of its name, the 'CS' part, the professed objective of CSCW is to *support via computers* a specific category of work—cooperative work. Thus the term *computer support* seems to convey a commitment to focus on the actual needs and requirements of people engaged in cooperative work. Of course, new technologies of communication and interaction necessarily transform the way people cooperate and CSCW systems are likely to have tremendous impact on existing cooperative work practices. Nonetheless, *cooperative work* can be conceived as a specific category or aspect of human work with certain fundamental characteristics common to all cooperative work arrangements, irrespective of the technical facilities available now or in the future (see Schmidt, 1990, for an initial elaboration).

By virtue of its commitment to *support* cooperative work, CSCW should not be defined in terms of the *techniques* being applied. CSCW is a research area aimed at the design of application systems for a specific category of work—cooperative work, in all its forms. Like any other application area, CSCW, in its search for applicable techniques, potentially draws upon the whole field of computer science and information technology. Accordingly, a technology-driven approach to CSCW will inevitably dilute the field. To some extent, the current lack of unity of the CSCW field bears witness to that.

CSCW should be conceived of as *an endeavor to understand the nature and requirements of cooperative work with the objective of designing computer-based technologies for cooperative work arrangements.* The fact that multiple individuals, situated in different work settings and situations, with different responsibilities, perspectives and propensities, interact and are mutually dependent in the conduct of their work has important implications for the design of computer systems intended to support them in this effort.

Thus, CSCW is a research and development area addressing questions such as the following: What are the characteristics and hence the general support requirements of cooperative work as opposed to work performed solely by individuals? Why do people enter into cooperative work arrangements and how can computer-based technologies be applied to enhance their ability to do whatever it is they strive to do by cooperating? How can the coordination requirements of cooperative work arrangements be accomplished more easily, rapidly, flexibly, comprehensively, etc. with information technology? What are the implications of these requirements for the architectures of the underlying systems and services? All in all, the thrust of these questions is to *understand,* so as to better *support,* cooperative work.

As a research effort that involves a large number of established disciplines, research areas, and communities, CSCW is an arena of discordant views, incommensurate perspectives, and incompatible agendas. However, in the conception of CSCW proposed here—as a research area devoted to exploring and meeting the support requirements of cooperative work arrangements, CSCW is basically a *design oriented research area*. This is the common ground. Enter, and you must change.

Thus, the objective of social science contributions to CSCW should not be to cash in on the new wave and do what they have always done but rather to explore exactly how insights springing from studies of cooperative work relations might be applied and exploited in the design of useful CSCW systems. This demand not only raises the issue of how to utilize insights already achieved in related fields to influence the design process. It raises more fundamental issues such as: Which are the pertinent questions being pursued in field studies and evaluations for the findings to be of utility to designers? And how are the findings to be conceptualized? If CSCW is to be taken seriously, the basic approach of CSCW research should not be descriptive but constructive.

On the other hand, as a research area devoted to exploring and meeting the support requirements of real world cooperative work arrangements, CSCW requires that technologists extend out from a strict technical focus and investigate how their artifacts are, or could be, used and appropriated in actual settings.

Likewise, designing systems for specific cooperative work settings raises new issues for requirements analysis: How can designers unravel the essential functions of the cooperative work relations to be supported as opposed to ephemeral or accidental cooperative work practices that may be observed? What are the reasons for this particular task allocation or practice? Can it be attributed to customary privileges or prejudices? Is it imposed by labor market agreements? Is it required by law? Or is it required by the customer, e.g., to ensure specific quality requirements? Can it be attributed to the technical resources at hand in the given case? Can it be attributed to the available facilities for information retrieval or communication, for instance? How should designers approach the complex and delicate problem of designing systems that will inevitably change existing cooperative work patterns?

In short, the drive of CSCW should be directed towards designing systems embodying an ever deepening understanding of the nature of cooperative work forms and practices.

While this conceptualization of the general approach does recommend the CSCW field to focus on understanding the nature of cooperative work so as to better support people in their cooperative efforts, it does not prescribe a particular research strategy. Of course, field studies of cooperative work in diverse domains with the objective of identifying the research requirements of various kinds and aspects of cooperative work is much needed, but the design and application of experimental CSCW systems may also yield deep and valid insights into the nature and requirements of cooperative work. We thus concur with the drive of Groupware enthusiasts to actually *construct* working artifacts to support cooperative work processes.

2.2 The Scope of CSCW: Cooperative Work

Turning now to the second pair of characters in CSCW—'CW' or 'Cooperative Work', the first thing we notice is rampant confusion. There are many forms of cooperative work, and distinctions between such terms as cooperative work, collaborative work, collective work, and group work are not well established in the CSCW community. For instance, Hughes, Randall, and Shapiro (1991) contest the validity of the very concept of 'cooperative work' as a distinct category on the grounds that 'all work is [...] socially organized'. While this is assuredly the case at some level, we would hope that a more specific definition of cooperative work can help us in understanding more clearly different forms of work activity. At another extreme, Sørgaard (1987) has enumerated a very specific set of criteria for what would count as cooperative work, for instance, that it is non-hierarchical, relatively autonomous, etc. From yet another perspective, e.g. that of Howard (1987), the term 'cooperative work' is inappropriate because of the ideology he believes is inherent in the term. For Howard, and many others, there is a connotation to the term 'cooperative' that assumes compliance, shared sentiments, etc., which he regards as inappropriate for the realities of everyday work situations. He prefers the allegedly more open term, 'collective work,' which he sees as being induced in a variety of ways through the use of computers in general. Concurring in this criticism of the allegedly strong positive connotations of the 'happy terms' 'cooperation' and 'collaboration', Kling (1991) prefers the term 'coordination' in stead. Given this confusion, a closer examination of the concept of cooperative work is required.

2.2.1 The Nature of Cooperative Work

'Cooperative work,' the term chosen by Greif and Cashman to designate the area to be addressed by the new field, happens to be a term with a long history in the social sciences and one which is quite appropriate to the current context of CSCW. It was used as early as the first half of the nineteenth century by economists as the general and neutral designation of work involving multiple actors (e.g., Ure, 1835; Wakefield, 1849) and developed by Marx (1867a) who defined it as 'multiple individuals working together in a conscious way in the same production process or in different but connected production processes.' In this century, the term has been used extensively with the same general meaning by various authors, especially in the German tradition of the sociology of work (e.g., Popitz et al., 1957; Bahrdt, 1958; Dahrendorf, 1959; Kern and Schumann, 1970; Mickler et al., 1976). At the core of this conception of cooperative work is the notion of *interdependence in work*.

Work is, of course, always immediately *social* in the sense that the object and the subject, the ends and the means, the motives and the needs, the implements and the competencies, are socially mediated. However, people engage in *cooperative work* when they are *mutually dependent* in their work and therefore are required to cooperate in order to get the work done (Schmidt, 1991a). The notion of mutual dependence *in work* does not refer to the interdependence that arises by simply having to share the same resource. In this case people certainly have to coordinate their activities but to each of them the existence of the others is a mere nuisance and the less their own work is affected by others the better. For example, time-sharing systems cater for just that by making the presence of other users imperceptible. Being mutually dependent *in work* means that 'A' relies positively on the quality and timeliness of 'B's work and vice versa and should primarily be conceived of as a positive, though by no means necessarily harmonious, interdependence.[5]

Because of this interdependence, any cooperative effort thus involves a number of secondary activities of mediating and controlling these cooperative relationships. Tasks have to be allocated to different members of the cooperative work arrangement: which worker is to do what, where, when? And in assigning a task to a worker, that worker is then rendered accountable for accomplishing that task according to certain criteria: when, where, how, how soon, what level of quality, etc.? Furthermore, the cooperating workers have to *articulate* (divide, allocate, coordinate, schedule, mesh, interrelate, etc.) their distributed individual activities (Strauss et al., 1985; Gerson and Star, 1986; Strauss, 1988). Thus, by entering into cooperative work relations, the participants must engage in activities that are, in a sense, extraneous to the activities that contribute directly to fashioning the product or service and meeting requirements. That is, compared with individual work, cooperative work implies an overhead cost in terms of labor, resources, time, etc. The obvious justification for incurring this overhead cost and thus the reason for the emergence of cooperative work formations is, of course, that workers could not accomplish the task in question if they were to do it individually (Schmidt, 1990).

True, all work is complexly social. We are indeed social animals, but we are not *all* of us *always* and in *every* respect mutually dependent in our work. Thus, in spite of its intrinsically social nature, work is not intrinsically *cooperative* in the sense that workers are mutually dependent in their work. Cooperative work is thus distinct from individual work, in theory as well as in practice. In so far as cooperating workers have to articulate their distributed individual activities and, thus, must engage in activities that are extraneous to the activities that contribute directly to fashioning the product or service and meeting requirements, cooperative work has characteristics distinctly different from individual work.

In our view, this conception of cooperative work is quite appropriate for CSCW for several reasons.

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[5]This conception of interdependence in work as constitutive of cooperative work is related to Thompson's concept of 'reciprocal interdependence' (Thompson, 1967, pp. 54–55).

2.2.2 The Rich Diversity of Cooperative Work

It is crucial that the CSCW field does not artificially and inadvertently exclude specific forms of cooperative work from the scope of research. Rather, the conceptualization of cooperative work in CSCW should allow us to embrace the rich diversity of forms of cooperative work.

(1) In a large part of the literature on CSCW, cooperative work is simply defined as group work. Greif (1988b), for instance, defines CSCW as 'an identifiable research field focused on the role of the computer in group work.' However, replacing the term 'cooperative work' with that of 'group work' or defining the former by the latter does not bring clarity to the scope of the field. To the contrary, it entails a host of problems of its own.

The term 'group' is quite blurred. On the one hand it is often used to designate almost any kind of social interaction between individuals. For instance, in his book on *Groupware*, Johansen (1988) mentions 'teams, projects, meetings, committees, task forces, and so on' as examples of 'groups' and even includes interaction among workers, supervisors and management in manufacturing operations, 'often across both distances and work shifts,' under the same notion (Johansen, 1988, p. 1). On the other hand, the term 'group' is quite often used to designate a relatively closed and fixed aggregation of people sharing the same 'goal' and engaged in continual and direct communication. The very notion of a 'shared goal' is itself murky and dubious, however (Bannon and Schmidt, 1989). The process of decision making in a group is a very differentiated process involving the interaction of multiple goals of different scope and nature as well as different heuristics, conceptual frameworks, motives, etc.

It has—implicitly or explicitly—been the underlying assumption in most of the CSCW oriented research thus far that the cooperative work arrangement to be supported by a computer artifact is a small, stable, egalitarian, homogeneous, and harmonious ensemble[6] of people,—a 'group'. However, in general cooperative work in real world settings has a number of characteristics that must be taken into account if CSCW systems are to be acceptable to users and, hence, commercially viable:

- Cooperative ensembles are either large, or they are embedded within larger ensembles.
- Cooperative ensembles are often transient formations, emerging to handle a particular situation after which they dissolve again.
- Membership of cooperative ensembles is not stable and often even non-determinable. Cooperative ensembles typically intersect.
- The pattern of interaction in cooperative work changes dynamically with the requirements and constraints of the situation.
- Cooperative work is distributed physically, in time and space.

[6]The terms 'ensemble' is used by Sartre (1960) to denote an, as yet, unstructured aggregation of people. It is used here to denote a *cooperative work arrangement* irrespective of its specific organizational form—whether it is a group, a team, a network, a department, or a company.

- Cooperative work is distributed logically, in terms of control, in the sense that agents are semi-autonomous in their partial work. Cooperative work involves incommensurate perspectives (professions, specialties, work functions, responsibilities) as well as incongruent strategies and discordant motives.
- There are no omniscient agents in cooperative work in natural settings.

(2) The term 'cooperative work' should be taken as the general and neutral designation of multiple persons working together to produce a product or service. The term 'cooperation', of course, has multiple connotations in everyday usage. Apart from the neutral and general meaning adopted in the economic and sociological literature, 'cooperation' may designate a process of give-and-take in a spirit of compromise (Bowers, 1991). In political science, the term is naturally used in the latter meaning. In CSCW, however, we should stick to the neutral and general meaning. What we are arguing for here is an interpretation of 'cooperative work' that goes beyond its everyday meaning, yet which we earlier showed has an excellent sociological pedigree. The objectives of CSCW are quite ambitious as it is, so there is no reason to claim that computer-based systems will be instrumental in eliminating social, ideological, ethnic etc. enmity or animosity. Thus, in the context of CSCW, the concept of cooperative work should not be taken to imply a particular degree of participation or self-determination on the part of the workers, nor a particularly democratic management style. Actually, the concept has historically been developed and used in analyses of the harsh realities of industrial life (e.g., Ure, 1835; Marx, 1867a; Popitz et al., 1957). Nor are we saying, 'Thou shalt cooperate!' Cooperative work is not necessarily preferable to individual work; nor is it inferior to individual work. In our context, cooperative work relations are seen as emerging in response to technical necessities or economic requirements in certain work environments.

(3) More generally, the conception of cooperative work suggested here does not assume or entail specific forms of interaction such as mode and frequency of communication, comradely feelings, equality of status, formation of a distinct group identity, etc. or even specific organizational settings. Indeed, we do not want to restrict the scope of CSCW to cooperative work relations that are defined and bounded in legal terms, i.e. in terms of formal organizational structures. Cooperative work is constituted by interdependence in work, that is, by work activities that are related as to content in the sense that they pertain to the production of a specific product or service. Thus, the boundaries of cooperative work networks are defined by actual cooperative behavior and are not necessarily congruent with the boundaries of formal organizations. A business corporation may have multiple cooperative work processes with no mutual interaction, and a cooperative work process may cross corporate boundaries and may involve partners in different companies at different sites, each of the partners producing but a component of the finished product. For example, in response to the emerging dynamic business environment manufacturing enterprises are establishing stable cooperative networks incorporating multiple companies by involving supplier companies in 'just-in-time' and 'total quality control' arrangements.

Actually, a major thrust of current practical efforts to apply computer based technologies to cooperative work settings is directed at the problems of supporting indirect and distributed cooperative work relationships,—for instance in administrative work, engineering design, and scientific research, where actors often cooperate at 'arm's length,' without direct communication and without necessarily knowing each other. Let us briefly mention some examples from specific domains.

In advanced manufacturing enterprises cooperative work relations are not limited to the group or team responsible for a particular shop. Cooperative work relations span the entire enterprise, from marketing to shipping, from design to final assembly. For a manufacturing enterprise to be able to adapt diligently and dynamically to emerging dynamic markets, the entire enterprise must react 'simultaneously and cooperatively' (Harrington, 1979). In fact, this is the very essence of advanced manufacturing systems. For example, the Just-In-Time principle of production control should be conceived as a semi-horizontal coordination mechanism for rapid adaptation of manufacturing operations in complex environments, thus embracing all functions from marketing to shipping (Aoki, 1988). Likewise, the objective of the concept of Company Wide Quality Control is to make the 'voice of the customer' audible throughout the company so as to ensure that distributed decision making (e.g., to handle local disturbances) is guided by pertinent knowledge of customers' needs and requirements. The ambition of the efforts of the Computer Integrated Manufacturing (CIM) field is to link and fuse the diverse information processing activities of the various manufacturing functions such as design and process engineering, production planning and control, process planning and control, purchasing, sales, distribution, accounting, etc. into a unitary information system (Harrington, 1979, 1984; Gunn, 1987). A CIM system embracing these information processing activities on a company-wide scale should be seen as a unified database system facilitating and supporting the horizontal and hierarchical, indirect and direct, distributed and collective cooperation of a heterogeneous ensemble of distributed decision makers throughout all functions of manufacturing. CIM is thus faced with issues that are crucial to CSCW (Schmidt, 1991b). However, despite the large amount of work on CIM, and its obvious pertinence to the CSCW field, this domain is almost totally absent in the work of the CSCW community. In our view, this is a loss to the field.

Likewise, the Office Information Systems (OIS) field aims at meeting the need of organizations for exploiting their information assets more effectively by designing information systems incorporating the mass of documents handled by the organization. By recording, indexing and providing access to the multitude of information objects in a large organization, office information systems should be seen as computer based systems that also support the indirect and distributed cooperation of an ensemble of workers.

In sum, we certainly want CSCW to address the aspects of computer support for cooperative work *wherever* they occur. In this sense, established research and development fields such as, for example, Computer Integrated Manufacturing (CIM), Office Information Systems (OIS), Computer-Aided Design (CAD), and Computer-Aided Software Engineering (CASE) are all legitimate and indeed necessary fields for CSCW as domains of inquiry. Also, we do not want to restrict the scope of CSCW to those special settings where the responsibility of accomplishing a task has been allocated to or assumed by a relatively closed and stable collective. The concepts of 'group' and 'group work,' however, invariably connote special types of cooperative relations characterized by shared responsibilities. This conceptualization of CSCW will tend to ignore or even dismiss the major challenges posed by the design of systems that support cooperative work arrangements that are characterized by a large and maybe indeterminate number of participants, incommensurate conceptualizations, incompatible strategies, conflicting goals and motives, etc. These challenges are not merely academic. They are the challenges posed by ongoing practical efforts in CIM, OIS, CAD, CASE etc.

2.2.3 Articulation Work

The conception of cooperative work as outlined above points to a set of key issues for CSCW that can be subsumed under the notion of supporting the articulation of distributed activities.

A cooperative work arrangement arises simply because there is no omniscient and omnipotent agent. Specifically, a cooperative work arrangement may emerge in response to different requirements (Schmidt, 1990):

- A cooperative work arrangement may simply *augment* the mechanical and information processing capacities of human individuals and thus enable the cooperating ensemble to accomplish a task that would have been infeasible for the workers individually.
- A cooperative work arrangement may *combine the specialized activities* of multiple workers devoted to the operation of different specialized tools, techniques, or routines.
- A cooperative work arrangement may facilitate the application of multiple problem solving *strategies and heuristics* to a given problem and may thus serve the function of balancing the individual biases ('bias discount', in the words of Cyert and March, 1963).
- A cooperative work arrangement may facilitate the application of multiple *perspectives and conceptions* on a given problem so as to match the multifarious nature of the work environment.

Therefore, cooperative work is, in principle, distributed in the sense that decision making agents are semi-autonomous in their work in terms of the unique situations and contingencies they are faced with locally as well as in terms of goals, criteria, perspectives, heuristics, and interests and motives.

However, due to the very interdependence in work that gave rise to the cooperative work arrangement in the first place, the distributed nature of the arrangement must be kept in check, managed. The distributed activities must be articulated. Articulation work arises as a integral part of cooperative work as a set of activities required to manage the distributed nature of cooperative work.[7] In the words of Strauss (1985, p. 8), articulation work is 'a kind of supra-type of work in any division of labor, done by the various actors':

> Articulation work amounts to the following: First, the meshing of the often numerous tasks, clusters of tasks, and segments of the total arc. Second, the meshing of efforts of various unit-workers (individuals, departments, etc.). Third, the meshing of actors with their various types of work and implicated tasks.

In order to be able to articulate the distributed activities of a cooperative work arrangement, the participants need access to appropriate means of communication. By providing communication facilities in the form of file sharing, shared view, email, computer conferencing, and video conferencing that increase the bandwidth and reduce the turnaround time of communication, CSCW systems can enable small and relatively stable cooperating ensembles to exploit the powerful repertoire of everyday social interaction in spite of distance and thus augment the capacity of the ensembles in articulating their distributed activities.

However, in 'real world' cooperative work settings—characterized by dispersed, distributed, and dynamic cooperative work arrangements and involving a large, varying, or indeterminate number of participants,—the various forms of everyday social interaction are quite insufficient. Hence, articulation work becomes extremely complex and demanding. In these settings, people apply various *mechanisms of interaction* so as to reduce the complexity and, hence, the overhead cost of articulation work, e.g.:

- Organizational structures in the form of formal (explicit, statutory, legally enforceable) and less formal (implicit, traditional, customary) allocation of resources, rights, and responsibilities within the cooperating ensemble.
- Plans, schedules, e.g. master schedules and kanban systems in manufacturing enterprises (Schmidt, 1991b).
- Standard operating procedures (Suchman, 1983; Suchman and Wynn, 1984).
- Conceptual schemes (e.g., thesauruses, taxonomies) for indexation or classification of information objects so as to organize distributed inclusion and retrieval of objects in 'public' repositories, archives, libraries, databases etc. maintained by multiple persons (Star and Griesemer, 1989; Bowker and Star, 1991).

These protocols, formal structures, plans, procedures, and schemes can be conceived of as *mechanisms* in the sense that they (1) are objectified in some way (explicitly stated, represented in material form), and (2) are deterministic or at least give reasonably predictable results if applied properly. And they are *mechanisms of interaction* in the sense that they reduce the complexity of articulating cooperative work.

[7] The concept of 'working division of labour' developed by Anderson et al. (1987) is related to the concept of 'articulation work' as developed by Strauss, Gerson, Star, and others.

However, as observed by Gerson and Star (1986, p. 266), being nothing but local and temporary closures, these mechanisms themselves require articulation work:

> Reconciling incommensurate assumptions and procedures in the absence of enforceable standards is the essence of articulation. Articulation consists of all the tasks involved in assembling, scheduling, monitoring, and coordinating all of the steps necessary to complete a production task. This means carrying trough a course of action despite local contingencies, unanticipated glitches, incommensurate opinions and beliefs, or inadequate knowledge of local circumstances.
>
> Every real world system is an open system: It is impossible, both in practice and in theory, to anticipate and provide for every contingency which might arise in carrying out a series of tasks. No formal description of a system (or plan for its work) can thus be complete. Moreover, there is no way of guaranteeing that some contingency arising in the world will not be inconsistent with a formal description or plan for the system. [. . .] Every real world system thus requires articulation to deal with the unanticipated contingencies that arise. Articulation resolves these inconsistencies by packaging a compromise that 'gets the job done,' that is closes the system locally and temporarily so that work can go on.

Major research efforts in CSCW have been directed at incorporating such mechanisms of interaction in CSCW applications, e.g. AMIGO and COSMOS (Danielsen et al., 1986; Benford, 1988; Bowers et al., 1988; COSMOS, 1989), THE COORDINATOR (Flores et al., 1988), DOMINO (Kreifelts et al., 1991a), and the Community Handbook proposed by Engelbart and Lehtman (Engelbart and Lehtman, 1988).

However, in these CSCW research activities, a set of related issues are encountered recurrently, namely the problem of how to support the ongoing dynamic articulation of distributed activities and the cooperative management of the mechanisms of interaction themselves. This issue, we posit, is the key issue in CSCW.

2.3 *Why CSCW Now?*

Why CSCW now? There are, of course, a multitude of answers to that question (Bannon et al., 1988; Grudin, 1991; Hughes et al., 1991). In a cynical interpretation, for example, CSCW could be seen as the answer to the silent prayers of researchers from HCI who feel that their field has done its duty by developing the graphical user interface, researchers from AI who feel that their field no longer offers the exciting opportunities it used to, or researchers from social science who feel that it is now their turn to get funded! All of these stories are probably relevant to some extent. But there are other—more material—developments involved.

First, however, we need to establish what is so new about CSCW. For many people working in the information systems field, it is difficult to comprehend the recent surge of interest in CSCW issues as if these issues were totally new and deserving of a distinct field within information systems research. Even from the earliest days, the cooperative nature of human work has been taken into account in the design of information systems. For example, one could certainly argue that the emergence of CSCW as a field was anticipated by many commercial transaction-oriented applications for cooperative work settings, e.g., airline reservation systems, that could, in a primitive sense, be viewed as CSCW applications. Airline reservation systems actually do support a rudimentary form of cooperative work in the broad sense posited above. The multi-user database of an airline reservation system can be viewed as a 'shared object' by means of which people interact in an indirect fashion. However, the limited ability to track changes and communicate between those involved make the range of support provided rudimentary. So, although people using these systems may engage in cooperative problem solving and discretionary decision-making, the technology itself provides little or no support for these cooperative processes—these aspects of their cooperative efforts take place outside or around rather than through the system. In other words, just as, for example, a simple accounting system could be viewed as a simple and special case of office information systems, an airline reservation system could be viewed as a simple and special case of CSCW applications.

Now, a major development that gives impetus to the rise to CSCW *as a field* is the current transformation of the organization of work. Comprehensive changes in the societal environment permeate the realm of work with a whole new regime of demands and constraints. The turbulent character of modern business environments and the demands of an educated and critical populace, compel industrial enterprises, administrative agencies, health and service organizations, etc. to drastically improve their innovative skills, operational flexibility, and product quality (Gunn, 1987). To meet these demands, work organizations must be able to adapt rapidly and to coordinate, in a comprehensive and integrated way, their distributed activities across functions and professional boundaries within the organization or within a network of organizations. In short, the full resources of cooperative work must be unleashed: horizontal coordination, local control, mutual adjustment, critique and debate, self-organization. Work organizations thus require support from advanced information systems that can facilitate the coordination of distributed decision making. Simultaneously, the proliferation of powerful workstations in cooperative work settings and their interconnection in comprehensive high-capacity networks provide the technological foundation to meet this need.

Such developments are illustrated in the area of Computer Integrated Manufacturing by the efforts to integrate formerly separated functions such as design and process planning, marketing and production planning, etc., and by the similar efforts in the area of Office Information Systems to facilitate and enhance the exchange of information across organizational and professional boundaries.

In emerging complex work environments, cooperative work exhibits specific characteristics, e.g., dynamic patterns of interaction, multiple decision making strategies, incompatible conceptualizations, etc. that are of little import in conventional transaction-oriented applications. Thus, while the dynamic and distributed nature of cooperative work arrangements could be ignored (for all practical purposes) in the design of conventional information systems for cooperative work settings, these characteristics pose challenging problems to the development of computer-based systems to support cooperative work in the newly emerging organization of work.

These developments have often been encountered by unsuspecting systems designers who thus become aware of the complexity of the cooperative work processes the hard way. All too often computer-based systems are introduced in cooperative work settings with disruptive effects due to an insufficient appreciation of this complexity (Grudin, 1989; Harper et al., 1989a).

3 Supporting Articulation Work

If we take our conceptualization of CSCW seriously, one issue that looms large is how to support the ‘articulation work’ that people must engage in, in order to make the cooperative mechanisms developed to support different aspects of work in complex environments fit together and fit to local circumstances.

In this section of the paper we broach two aspects of this articulation issue, one focusing on the management of workflow, the other on the construction and management of what we term a ‘common information space’. The former concept has been the subject of discussion for some time, in the guise of such terms as office automation and more recently, workflow automation. The latter concept has, in our view, been somewhat neglected, despite its critical importance for the accomplishment of many distributed work activities. Here the focus is on how people in a distributed setting can work cooperatively in a common information space—i.e. by maintaining a central archive of organizational information with some level of ‘shared’ agreement as to the meaning of this information (locally constructed), despite the marked differences concerning the origins and context of these information items. The space is constituted and maintained by different actors employing different conceptualizations and multiple decision making strategies, supported by technology.

3.1 Supporting the Management of Workflows

According to the traditional ‘bureaucratic’ conception of organizational work, people perform a number of tasks according to a set of well-specified ‘procedures’ that have been developed by management as efficient and effective means to certain ends. The traditional formal organization chart is presumed to show the actual lines of authority and the correct pattern of information flow and communication. However, the conception has been proved highly idealized and grossly inadequate for analyzing and modelling the articulation of real world cooperative work arrangements.

Due to the dynamic and contradictory demands posed on a social system of work by the environment, task allocation and articulation are renegotiated more or less continuously. This has been documented thoroughly in the domain of ‘office work’ and many other arenas. For example, a number of studies of office work, conducted by anthropologists and sociologists, have emphasized the rich nature of many allegedly ‘routine’ activities and the complex pattern of cooperative decision-making and negotiation engaged in by co-workers, even at relatively ‘low’ positions within the organization (Wynn, 1979; Suchman, 1983; Gerson and Star, 1986). Suchman gives a concise account of this discrepancy between the office procedures that supposedly govern office work and the practical action carried out by office workers. She notes: 'the procedural structure of organizational activities is the *product* of the orderly work of the office, rather than the reflection of some enduring structure that stands behind that work' (Suchman, 1983, p. 321). It is not that office procedures are irrelevant, it is just that these procedures require problem solving activities and negotiation with co-workers, the result of which can be interpreted as performance according to procedures. The 'informal' interactions that take place in the office thus not only serve important psychological functions in terms of acting as a human support network for people, for example, providing companionship and emotional support, but are crucial to the actual conduct of the work process itself. Evidence for this is apparent when workers 'work-to-rule,' i.e. perform exactly as specified by the office procedures, no more and no less. The result is usually that the office grinds to a halt very quickly.

Still, the early computer systems developed to 'automate the office' were built by designers who implicitly assumed much of the traditional procedural conception of office work (Zisman, 1977; Ellis and Nutt, 1980; Hammer and Kunin, 1980; Hammer and Sirbu, 1980; Ellis and Bernal, 1982). Designers were 'automating a fiction' as Beau Sheil (1983) so aptly put it. Such systems have now been admitted as failures (Barber, 1983, p. 562):

> In all these systems [i.e., *inter alia*, Zisman's "Office Procedure Specification Language", Ellis' "Information Control Net" and Hammer and Kunin's "Office Specification Language"] information is treated as something on which office actions operate producing information that is passed on for further actions or is stored in repositories for later retrieval. These types of systems are suitable for describing office work that is structured around actions (e.g. sending a message, approving, filing); where the sequence of activities is the same except for minor variations and few exceptions. [. . .] These systems do not deal well with unanticipated conditions.

So, what does this imply for the design of CSCW systems? Building computer systems where work is seen as simply being concerned with 'information flow,' and neglecting the articulation work needed to make the 'flow' possible, can lead to serious problems. Computer support of cooperative work should aim at supporting self-organization of cooperative ensembles as opposed to disrupting cooperative work by computerizing formal procedures. A number of researchers within the office information systems field have accepted the rich view of office work provided by social scientists and attempt to develop systems that support office workers in their activities (Fikes and Henderson, 1980; Barber et al., 1983; Ellis, 1983; Croft and Lefkowitz, 1984; Hewitt, 1986).[8] For example, Woo and Lochovsky (1986) critique the rush to formalize and the treatment of office systems as closed systems, noting that in many real situations allowance should be made for *inconsistent* office procedures, as 'these inconsistencies represent different opinions on common tasks.'

[8]Much of this new work can also be critiqued with respect to their concept of 'goals' and how they are represented, but this critique will not be pursued here.

Some early CSCW systems did not seem to take note of these issues. For instance, take the early CSCW project management support tool XCP. In the words of its designers (Sluizer and Cashman, 1984):

> XCP is an experimental coordinator tool which assists an organization in implementing and maintaining its procedures. Its goal is to reduce the costs of communicating, coordinating and deciding by carrying out formal plans of cooperative activity in partnership with its users. It tracks, prods, and manages the relational complexity as captured in the formal plan, so that human resources are available for more productive tasks. [. . .] An important effect is that XCP encourages an organization to clearly define formal procedural obligations and relationships.

It would appear that XCP assumes that what people do in many work settings is to follow procedures. No wonder the authors note the difficulty involved in developing and 'debugging' the formal protocol. The generalization of such an approach to a wide range of office situations seems unrealistic. It appears to exclude the dimension of task articulation.

A similar, rather rigid, view of office procedures as a series of directly executable steps can be found in the initial work on the office procedure system DOMINO. It was meant to incorporate a model of a procedure in the form of 'a formalized exchange of messages' serving as 'a fixed regulation in an organization' (Victor and Sommer, 1991, pp. 119–120). However, after initial testing of a prototype, designers have noted the need for a more flexible interpretation of such 'procedures' and have begun work on a more open-ended version of the system (Kreifelts et al., 1991a).

Thus it can be argued that, in addition to the naked functionality of the specific CSCW application, the system should also have facilities that allow users to freely negotiate task allocation and articulation. Robinson (1991) has argued for such a position forcibly, stating that a CSCW application should support at least two interacting 'levels of language' as a general design principle, as 'unless these two levels interact, fruitful co-operation will not happen.' (Robinson, 1991, p. 48). He cites the COORDINATOR system developed by Flores et al. (1988) as an example of a system that is problematic not because it requires people to be explicit about their communication but because, in his view, it has 'excluded, marginalised, and even illegitimised' alternative channels of conversation to facilitate the negotiation of task allocation and articulation.[9] As an example of a system providing a simple, yet effective, alternative channel for cooperative task articulation, Robinson cites the GROVE system (Ellis et al., 1991). GROVE is a multi-user outline processor, allowing multiple users to cooperate on drafting a common text. In addition to the interactions visible through the ongoing online textual modifications, users can talk to each other about what was going on, and why, by means of a voice link. In the terminology suggested by Robinson, the voice link provided 'the second level of language.' Robinson's insightful remarks are worth quoting here:

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[9] There is a lot of controversy surrounding the COORDINATOR system, both in terms of its design philosophy and its success or failure in practice. While this debate is important, our purpose here is not to take sides, but simply to note one of the claims made about the system.

> In general it can be said that any non-trivial collective activity requires effective communication that allows both ambiguity and clarity. These ideas of ambiguity and clarity can be developed as the "formal" and "cultural" aspects of language as used by participants in projects and organizations. "Computer support" is valuable insofar as it facilitates the separation and interaction between the "formal" and the "cultural." Applications and restrictions that support one level at the expense of the other tend to fail.
>
> The formal level is essential as it provides a common reference point for participants. A sort of "external world" that can be pointed at, and whose behaviour is rule-governed and predictable.
>
> The "cultural" level is a different type of world. It is an interweaving of subjectivities in which the possible and the counterfactual [. . .] are as significant as the "given." [. . .] The formal level is meaningless without interpretation, and the cultural level is vacuous without being grounded. (Robinson, 1991, p. 43)

While we have a number of reservations with Robinson's use of the terms 'formal' and 'cultural'—for example, the 'formal' level can quite legitimately also be viewed as a 'cultural' construct—his distinction between the level of the primary work content and the level of task articulation and his contention that any CSCW system should provide alternative channels of conversation seems fruitful.

Taking the point even further, we would argue that the problem with incorporating models of organizational procedures or structures in computer systems is not that organizational procedures and structures are fictitious. Rather, they serve a heuristic function in action by identifying constraints, pitfalls and strategic positions in the field of work. As observed by Suchman (1987) in her analysis of the role of plans in situated action, in order to serve this heuristic function 'plans are inherently vague'. Thus, in Suchman's conception,

> plans are resources for situated action, but do not in any strong sense determine its course. While plans presuppose the embodied practices and changing circumstances of situated action, the efficiency of plans as representations comes precisely from the fact that they do not represent those practices and circumstances in all of their concrete detail. (Suchman, 1987, p. 52)

The observation that *plans are resources* is generally valid. Organizational procedures may, of course, codify 'good practice,' recipes, proven methods, efficient ways of doing things, work routines. In flexible work organizations required to be able to cope with unanticipated contingencies procedures of this kind are of little value and may actually impede flexibility. However, a procedure may also convey information on the functional requirements to be met by the process as well as the product; it may highlight decisional criteria of crucial import; it may suggest a strategy for dealing with a specific type of problem (e.g., which questions to address first?); it may indicate pitfalls to avoid; or it may simply provide an *aide memoir* (such as a start procedure for a power plant or an airplane). Also, a procedure may express some statutory constraints where non-compliance may evoke severe organizational sanctions. More often than not, a particular procedure will express, in some way, all of these different functions. However, whatever the function, organizational procedures are not executable code but rather heuristic and vague statements to be interpreted, instantiated, and implemented, maybe even by means of intelligent improvisation.

Therefore, instead of pursuing the elusive aim of devising organizational models that are not limited abstractions and thus in principle brittle when confronted with the inexhaustible multiplicity of reality, organizational models in CSCW applications should be conceived of as *resources* for competent and responsible workers. That is, the system should make the underlying model accessible to users and, indeed, support users in interpreting the procedure, evaluate its rationale and implications. It should support users in applying and adapting the model to the situation at hand. It should allow users to tamper with the way it is instantiated in the current situation, execute it or circumvent it, etc. The system should even support users in modifying the underlying model and creating new models in accordance with the changing organizational realities and needs. The system should support the documentation and communication of decisions to adapt, circumvent, execute, modify etc. the underlying model. In all this, the system should support the process of negotiating the interpretation of the underlying model, annotate the model or aspects of it, etc.

This set of issues is of vital importance for the future direction of CSCW research and development and raises many crucial questions which need to be investigated in future research programmes.

3.2 Supporting the Management of a Common Information Space

Another approach to the design of CSCW applications, other than the procedural or workflow approach described above, is to allow the members of a cooperating ensemble to interact freely, i.e. without being constrained by prescribed procedures or established conversational conventions, through the provision of facilities enabling them to cooperate via the joint construction of a *common information space*. Here, they perceive, access, and manipulate the same set of information, for example in a shared database, but further work is then required by the actors in order to arrive at an agreed interpretation concerning the meaning of these objects. Cooperative work is not facilitated simply by the provision of a shared database, but requires the active construction by the participants of a common information space where the meanings of the shared objects are debated and resolved, at least locally and temporarily. Objects must thus be interpreted and assigned meaning, meanings that are achieved by specific actors on specific occasions of use. Computer support for this aspect of cooperative work raises a host of interesting and difficult issues. In this section we discuss the difficulties involved in achieving a shared understanding of the meaning of objects among a group of people who are working in a distributed fashion, and what the implications are for computer support of such activities. Let us begin with problems of interpreting objects in a 'shared' system.

3.2.1 The Role of Interpretation Work

A common database is not a common information space. Objects in a database are perceived and manipulated at different semantic levels. They can be manipulated *qua* objects, but are more usually perceived and manipulated as *carriers* of representations. Their importance lies in the interpretation human actors place on the meaning of the representational object. The distinction between the material carrier of information—the object—and its meaning is crucial. The material representation of information in the common space (e.g., a letter, memo, drawing, file) exists as an objective phenomenon and can be manipulated as an artifact. The semantics of the information carried by the artifact, however, is, put crudely, 'in the mind' of the beholder, and the acquisition of information conveyed by the artifacts requires an interpretive activity on the part of the recipient. Thus, a common information space encompasses the artifacts that are accessible to a cooperative ensemble *as well as* the meaning attributed to these artifacts by the actors.

In the—unlikely but simple—case of an individual working totally on his or her own, the recipient of an information object is identical with the originator. Since the actor has produced, organized, indexed etc. the potentially relevant information himself, the interpretation of the information does not pose much of a problem (although even here there can be problems, faulty memory, etc.). Likewise, the boundary of the information space—that is, the extension of the set of information relevant to the particular problem situation at hand—is constituted in the particular situation by the agent.

Now, what happens if the information object accessed by one actor is produced by another and vice versa, that is, if the set of information objects are produced and accessed by multiple actors? At the level of the objects themselves, shareability may not be a problem, but in terms of their interpretation, the actors must attempt to jointly construct a common information space which goes beyond their individual personal information spaces. A nice example of how this is a problem has been given by Savage (1987, p. 6): 'each functional department has its own set of meanings for key terms. [...] Key terms such as *part, project, subassembly, tolerance* are understood differently in different parts of the company.'

Of course, in order for work to be accomplished, these personal, or local information spaces must cohere, at least temporarily. But the important point is to realize that one cannot just produce a common information space, that it does not automatically appear as the result of developing a common dictionary of terms and objects, as the meanings of these terms and objects must still be determined locally and temporally. The common information space is negotiated and established by the actors involved.[10]

As an example, imagine a situation where a cooperating ensemble is working together in a meeting room, using a whiteboard, for instance. The material objects carrying the information are the inscriptions on the whiteboard and, fleetingly, the

[10] In this context, the following comment is *apropos*: 'There are many actors; they each have different perspectives (often, multiple incommensurate ones); the points at which they come together are typically only a small cross-section of the activities of each. I think we need models which represent multiple "information spaces" and then concern themselves with the specifics of cooperation among actors who don't necessarily agree on anything, or whose cooperation is strictly bounded in time, location, and scope.' (Gerson, Personal Communication, August 1989).

sound waves permeating the air. Again, the meaning of it all is 'in the minds' of the participants. Each of the participants contributes to the common information space by drawing and writing on the board or changing what has been drawn and written, by defining or questioning the meaning of a particular object, etc. Being together in a room, they are able to mobilize all the communicative resources of face-to-face interaction (cf. Heath and Luff, 1991a; Tang, 1991) to negotiate a shared understanding of what is said and written and of the boundary of the common information space. A person entering the room after the meeting, however, is able to perceive the remnants of the cooperative construction of a common information space in the form of more or less legible inscriptions, erasures etc., and may be able to infer—more or less cursorily—on the evidence of the remainders what has been going on at the meeting. But with the actors absent, the latecomer is unable to negotiate and thus corroborate or modify this interpretation.

Shared view. The concept of a 'common information space,' as used here, should be distinguished from the 'shared view' approach in CSCW.[11] The core of the notion of a 'shared view' is that multiple actors perceive the same object—text, drawing, etc.—in the same state and perceive any changes in the state of the object concurrently. Any changes to the object effected by one actor will be immediately perceptible to the other actors. This approach has been implemented in a number of systems. For example, Engelbart, in his early NLS/AUGMENT system, implemented a 'shared view' concept where two participants could perceive the same (code or graphic items) on their respective displays and could alternate control of the objects between them at will (Engelbart and English, 1968; Engelbart, 1984). In the work on Xerox PARC's CoLab, this approach was developed further to provide different participants with a WYSIWIS or 'What You See Is What I See' facility (Stefik et al., 1987).

What is 'shared' in the perspective of this approach is the object as such, as opposed to its meaning to the actors. The latter requires an interpretive activity on the part of the recipients, as discussed earlier. In these systems, this happens either via face-to-face discussion or over an audio channel while the actors are jointly viewing the 'shared view'. Thus, this work is primarily addressing the needs of small teams solving a common task of limited scope and duration by supplementing media such as the whiteboard as a means of communicating and recording or by supporting face-to-face type interaction among geographically distributed actors: for example, a couple of authors writing a joint paper by means of a multi-user document processor (note the reference to the GROVE system earlier) or a group of engineers observing and examining the behavior of a computer simulation of some phenomenon.

While such work is of interest to CSCW, it occupies a relatively small niche in the space of cooperative work activities. Certainly, the work of small groups involves multiple decision makers, in so far as all cooperative work involves distributed decision making (that is, decision making conducted by multiple, semi-autonomous

[11] See Greenberg (1990), for a concise and articulate overview of issues in this area.

actors). However, a—sufficiently—shared understanding of the organization and the boundary of the information space of the group can be *negotiated on the spot* due to the limited scope and duration of the task and the intensity of face-to-face interaction in small groups (and the emulation of face-to-face interaction by means of audio-visual media). In terms of design of support systems for this kind of cooperative work (meeting rooms, multi-user applications, etc.), the *distributed nature of cooperative decision making* is of minor import, yet it is precisely the latter that accounts for the bulk of cooperative work activities. We now move to consider the additional problems which this causes in the construction of a common information space.

Cooperation at arm's length. In real world settings, semi-autonomous knowledge workers typically cooperate 'at arm's length' by adding to, modifying, linking, searching, and retrieving items from a common set of information objects, centralized or decentralized, but accessible to some or all members of the given community.

An example would be the common information space of a particular research community, that is, the 'content' of the body of literature (reports, books, journals, preprints, proceedings, etc.), the concomitant verbal contributions (presentations, objections, gossip etc.), and the conceptual frameworks and assumptions applied by the participating scientists in interpretive work. Another example would be the common information space of an organization, that is, the 'content' of the mass of memos, letters, forms, documents, files, agendas, minutes, drawings, photos, etc., the verbal arguments of organizational life, and the beliefs and semantic structures of the staff involved.

Computer systems meant to support cooperative work in real world settings must support cooperation through the joint construction of a common information space in such settings. In our view, this constitutes one of the core problems for the CSCW field.

This problem has been recognized by several authors. For example, in contrast to the 'group work' oriented paradigms, Engelbart and Lehtman (1988) discuss a set of facilities necessary for a 'system designed to support collaboration in a community of knowledge workers.' First, in addition to services facilitating the creation, modification, transmission etc. of messages such a system should provide services supporting the cross-referencing, cataloging and indexing of the accumulating stock of messages. Second, the services for cataloging and indexing items generated internally, i.e. by means of the system, 'should also support managing externally generated items.' Having identified the basic technical requirements of such a system, Engelbart and Lehtman chalk out a grandiose vision of an information system supporting collective as well as distributed cooperative work in a community: 'With centrally supplied (and hence uniformly available) services such as these, a community can maintain a dynamic and highly useful "intelligence" database...' And they propose extending this facility toward...

> the coordinated handling of a very large and complex body of documentation and its associated external references. This material, when integrated into a monolithic whole, may be

> considered a “superdocument.” Tools for the responsive development and evolution of such a superdocument by many (distributed) individuals within a discipline- or project-oriented community could lead to the maintenance of a “community handbook,” a uniform, complete, consistent, up-to-date integration of the special knowledge representing the current status of the community.
>
> The handbook would include principles, working hypotheses, practices, glossaries of special terms, standards, goals, goal status, supportive arguments, techniques, observations, how-to-do-it items, and so forth. An active community would be constantly involved in dialogue concerning the contents of its handbook. Constant updating would provide a “certified community position structure” about which the real evolutionary work would swarm. (Engelbart and Lehtman, 1988, p. 249)

While this magnificent scheme effectively addresses the need of supporting cooperation via a common information space, in that it provides levels of interpretation and context around the information objects in the database, the notion of ‘a uniform, complete, consistent, up-to-date integration’ of the knowledge in a community handbook is hardly realistic. As we shall show, interpretive work remains to be done by actors accessing the community handbook. It too can be a valuable *resource* for developing a common information space with other actors, but due to the distributed nature of cooperative work the handbook will be necessarily incomplete and partial.

3.2.2 The Distributed Nature of Cooperative Work

If the decision making process (1) involves a large and indefinite number of people, (2) requires the integration of a number of different perspectives or domains, and (3) continues for a protracted period of time or even indefinitely, the interpretation of the objects in a common database and hence the construction of a common information space is hampered by the fact that the other originators and recipients are not co-present. Here, a shared understanding cannot be negotiated on the spot and ‘on the fly’ and the distributed nature of cooperative work is thus of paramount importance.

In this case, A—as a relatively autonomous decision maker—applies a particular strategy in a local context to a particular problem. The resultant documents are then, at least to some extent, transferred to the ‘public domain’ in the sense that it may be found, retrieved, used, trusted, neglected, rejected and so forth by B, C, D, etc. in other local contexts, working on other problems with different strategies. The information objects are produced and accessed in a distributed manner, by multiple, semi-autonomous decision makers. The meaning of the various information objects, their interrelationship, and their potential relevance have to be re-established by multiple agents whose shared understanding is incomplete. There is no central omniscient agent to ensure a consistent and comprehensive organization of the content of the information space and any negotiated understanding that may be established is a ‘local and temporary closure’ (Gerson and Star, 1986).

Thus, while requirements of ‘uniformity’ and ‘consistency’ and even ‘up-to-date-ness’ of a community handbook to support the construction of a common information space in cooperative work at 'arm's length' can be relaxed, the concept of a community handbook raises a plethora of new issues to be addressed if the practical utility of the community handbook is to materialize.

(1) *Identifying the Originator of the Information*. Different people prefer different problem solving strategies or heuristics. Accordingly, information bears the stamp of the strategy applied in generating it. It is the result of biased reasoning. In cooperative decision making, then, which we regard as the norm in even supposedly 'routine' office work, people discount for the biases of their colleagues. This point was brought home by Cyert and March in their classic study (1963, p. 77). Thus cooperative decision making involves a continuous process of assessing, and re-assessing, the validity of the information produced by the participants. For example, take the case of an 'experienced and skeptical oncologist,' cited by Strauss and associates (1985):

> I think you just learn to know who you can trust. Who overreads, who underreads. I have got X rays all over town, so I've the chance to do it. I know that when Schmidt at Palm Hospital says, "There's a suspicion of a tumor in this chest," it doesn't mean much because she, like I, sees tumors everywhere. She looks under her bed at night to make sure there's not some cancer there. When Jones at the same institution reads it and says, "There's a suspicion of a tumor there," I take it damn seriously because if he thinks it's there, by God it probably is. And you do this all over town. Who do you have confidence in and who none. (Strauss et al., 1985, pp. 21 f.)

As observed by Cicourel (1990, p. 222), the point is that 'the source of a medical opinion remains a powerful determinant of its influence.' That is, 'physicians typically assess the adequacy of medical information on the basis of the perceived credibility of the source, whether the source is the patient or another physician.' Thus 'advice from physicians who are perceived as "good doctors" is highly valued, whereas advice from sources perceived as less credible may be discounted.'

In cooperative work settings involving discretionary decision making, the exercise of mutual critique of the decisions arrived at by colleagues is required for all participants. Therefore, in order to be able to assess information generated by discretionary decision making, each participant must be able to access the identity of the originator of a given unit of information.

The fact that information produced by discretionary decision making cannot be conveyed anonymously has important implications for CSCW systems design. Naturally, such information must be accompanied by the identity of the source. But how to represent and present the identity of the source? It probably depends on the nature of the information and the context, but how? Name, picture, position? Which identity properties are pertinent in which situation? If the source is unknown to the recipient, the name or the picture may be of no use. To which kind of information does this apply? On closer inspection, we will probably come across a spectrum of categories of information—from factual to discretionary. Which categories of information can be disseminated with different kinds of identifiers? Is it possible to circumvent the identifier problem by providing 'depersonalized' contextual information (see below) that would provide a basis for critical assessment of anonymous information? Is it possible to record and convey the heuristics applied by a decision maker so as to enable the recipient of information to assess the validity etc. of the information? Can a computer system elicit or acquire the relevant background information, the decisional criteria applied, etc.? Alternatively, how can a computer system support the originator in expressing pertinent background information? Etc.

(2) *Identifying the Context of the Information*. Information is always generated within a specific conceptual framework, as answers to specific questions. Thus knowledge of the perspective applied by the person in reaching a decision and producing information is indispensable to colleagues supposed to act intelligently on information conveyed to them. Accordingly, in addition to the task-related information being conveyed (the message itself, so to speak), a CSCW system supporting the construction of a common information space must provide contextual knowledge of the conceptual frame of reference of the originator. Thus, a computer-based system supporting cooperative work involving decision making should enhance the ability of cooperating workers to interrelate their partial and parochial domain knowledge and facilitate the expression and communication of alternative perspectives on a given problem. This requires a representation of the problem domain as a whole as well as a representation, in some form, of the mappings between perspectives on that problem domain. Again, we are not very far along in understanding how to build in such properties into our systems, despite the converging evidence that these kinds of supports are required by people. However, some encouraging experimental systems that provide features of this kind are coming forward.

For example, Storrs (1989) describes a system—The Policy Application—which provides computer support for cooperative work in a policy making agency. Describing the characteristics of policy making on the basis of 4 years of field studies of cooperative work patterns in the target agency, Storrs notes: 'The particularly odd thing about it is that the "group" is widely dispersed in space and time. Yet it is a problem which calls for the bringing together of a great many people with a wide range of expertise and with widely differing perspectives' (Storrs, 1989, p. 118). In order to cater for these characteristics, the system provides a 'logical model' of the domain, in this case the social security legislation, in order to facilitate inquiries about the effects of legislation, to model changes to the legislation so as to assess their efficacy as solutions to policy problems, and to check for unexpected interactions with other parts of the legislation. In conjunction with this decision support facility, the system provides a hypertext-like argumentation structure for policy documents that allows policy makers to retrieve the 'hidden' argumentation substructure behind policy documents. The Policy Application is an interesting example of a new brand of CSCW systems that allows participants, old or new, to be able to assess some of the notions and opinions of the different parties involved in producing a piece of information. A similar approach has been advocated by Conklin (1989) in order to record the design decisions and assumptions that occur during the process of system design. Normally, the careful deliberation and much of the domain learning that went into resolving key design issues are not documented and therefore wasted, thus increasing overall system cost, especially the cost of system maintenance later on in the system's lifetime. In order to preserve the design rationale Conklin proposes a hypertext network that integrates all of the documents, artifacts, notes, ideas, decisions, etc. of the design process. One can see some overlap here with the ideas in Engelbart's Community Handbook described earlier. To summarize, then, data-bases for cooperative work must make visible the identity of the originator of information and the strategies and perspectives applied in producing the information.

(3) *Identifying the Politics of the Information*. Yet a third problem, albeit one that has had some public discussion, has been the presupposition among many designers of information systems that information is something innocent and neutral. This view implies that to design an information system for a company one need only to consider the data flows and files existing in that company. Consequently, a common data base containing all the relevant data from different parts of the organization, providing managers with a unified data model of the organization, is seen as attainable. In the words of Ciborra (1985), hard reality has condemned this idea to the reign of utopia. In fact, the conventional notion of organizations as being monolithic entities is quite naive. Organizations are not perfectly collaborative systems. Rather, the perspective on organizations that views them as a mixture of collaboration and conflict, overt and covert, appears to be more illuminating and have greater explanatory potential than the traditional 'rationalistic' account (see Kling's (1980) classic survey paper). We view organizations as a coalition of individuals motivated by individual interests and aspirations and pursuing individual goals (Cyert and March, 1963). Accordingly, in organizational settings information is used daily for misrepresentation purposes. Most of the information generated and processed in organizations is subject to misrepresentation because it has been generated, gathered, and communicated in a context of goal incongruence and discord of interests and motives.

On the one hand, the visibility requirement is amplified by this divergence. That is, knowledge of the identity of the originator and the situational context motivating the production and dissemination of the information is required so as to enable any user of the information to interpret the likely motives of the originator. On the other hand, however, the visibility requirement is moderated by the divergence of interests and motives. A certain degree of opaqueness is required for discretionary decision making to be conducted in an environment charged with colliding interests. Hence, *visibility must be bounded*. The idea of a comprehensive, fully exposed and accessible database is not realistic. A worker engaged in cooperative decision making must be able to control the dissemination of information pertaining to his or her work: what is to be revealed, when, to whom, in which form? Deprive workers of that capability, and they will exercise it covertly.

That is, a common information space must be 'peopled' by actors who are responsible for the information in the system. Problems of information-ownership and the responsibility for its upkeep and dissemination to others, have been neglected in much of the information systems literature (see Nurminen, 1988, though).

These realities of organizational life must be investigated seriously if CSCW is to be turned from a laboratory research activity into an activity producing useful real world systems. By ignoring the diversity and discord of the 'goals' of the participants involved, the differentiation of strategies, and the incongruence of the conceptual frames of reference within a cooperating ensemble, much of the current CSCW research evades the problem of how to provide computer support for people cooperating through the establishment of a common information space.

4 Conclusion

This paper has outlined a number of important issues in the field of CSCW which come to the fore as a result of taking seriously the concept of cooperative work and its computer support. Emphasis has been placed on the distributed nature of cooperative work arrangements, and the additional requirements this places on the design of technological support. Our approach has thus broadened the scope of CSCW beyond support of small groups or teams, as this has been shown to be but one form of work arrangement, and indeed one that has special characteristics which do not generalize to the majority of work settings. The paper has attempted to turn the spotlight onto the nature of these different cooperative work forms, and discussed crucial aspects of how work is managed and effected. This discussion hinged on the concept of 'articulation work', a pervasive and ubiquitous aspect of work practices that is essential in the accomplishment of work. We have attempted to outline the problems and opportunities this re-orientation of the field provides for on-going CSCW work.

Chapter 4
The Organization of Cooperative Work (1994)

Beyond the 'Leviathan' Conception of the Organization of Cooperative Work

1 The Problem

The current comprehensive transformation of the political economy of modern industrial society is engendering a new regime of demands and constraints on the realm of work. The business environment of modern manufacturing, for instance, is becoming rigorously demanding as enterprises are faced with increasingly global competition, contracting product life cycles, radical product diversification, and the need to pamper customers—with the concomitant transformation of the organization of production towards order-driven production bordering on custom-tailoring, insignificant or completely eradicated inventories and buffer stocks, shortened lead times, dwindling batch sizes approximating batches of one, concurrent processing of multiple different products and orders, and so forth (Ohmae, 1985; Gunn, 1987; Best, 1990; Womack et al., 1990). The transformation does not merely affect manufacturing and directly related industries. The demands of an educated and critical populace (and the needs of manufacturing and other industries), compel administrative agencies, health and service organizations, and so forth to drastically improve their innovative capability, operational flexibility, and product quality. Accordingly, modern work organizations must be able to adapt rapidly and diligently to changes in environmental conditions and demands and the same time be able to coordinate and integrate their distributed activities in an efficient and effective way. Altogether, this requires horizontal and direct coordination and integration of activities within and across functions and professional boundaries within the organization or among a network of organizations. The permanent managerial campaigns bear witness to that: Flexible Manufacturing, Concurrent Engineering, Design for Manufacturability, Total Quality Control, Business Process Reengineering, and so on.

Kjeld Schmidt: 'The organization of cooperative work: Beyond the 'Leviathan' conception of the organization of cooperative work,' in Thomas W. Malone (ed.): *CSCW'94: Proceedings of the Conference on Computer-Supported Cooperative Work, 24–26 October 1994, Chapel Hill, North Carolina*, ACM Press, New York, 1994, pp. 101–112.

Accordingly, modern work organizations require support from advanced information systems that can facilitate the horizontal coordination of distributed decision making. Simultaneously, the proliferation of powerful workstations in cooperative work settings and their interconnection in comprehensive high-capacity networks provide the technological foundation to meet this need. Such developments are illustrated in the area of Computer Integrated Manufacturing (CIM) by the efforts to apply computer-based technologies to integrate formerly separated functions such as design and process planning, marketing and production control, etc., and by similar efforts in areas such as Office Information Systems (OIS), Computer Aided Design (CAD), Computer Aided Software Engineering (CASE) to facilitate and enhance the exchange of information across organizational and professional boundaries.

These developments inaugurate a fundamental shift in the approach to the design of computer systems. In the design of conventional computer-based systems for work settings the core issues have been to develop effective computational models of pertinent structures and processes in the field of work (data flows, conceptual schemes, knowledge representations) and adequate modes of presenting and accessing these structures and processes as represented in computer systems (user interface, functionality). Surely, normally computer systems were used in organizational settings and were even often used by multiple users as in the case of systems that are part of the organizational infrastructure (e.g., database systems). Nevertheless, the issue of how multiple users work together and articulate (coordinate, schedule, interrelate, integrate, mesh, fuse) their individual activities—'through' the system or 'around' it—was not addressed directly and systematically, as a design issue in its own right. So far as the underlying model of the structures and processes in the field of work was 'valid', it was assumed that the articulation of the distributed activities was of no import or that it was managed somehow by whoever it might concern. It was certainly not a problem for the designer or the analyst. With CSCW, however, the very issue of how multiple users work together and articulate their individual activities has become the focal issue (Schmidt and Bannon, 1992).

Now, cooperative work is not a separate work domain. Rather, CSCW addresses a set of crucial design issues that are common to different application domains such as MIS, OIS, CIM, CASE, etc., and CSCW systems should thus be conceived of as a distinct category of facilities incorporated in the various domain specific applications.[1] For example, in the work domain of mechanical design, CSCW mechanisms supporting the articulation of distributed activities may be incorporated in project management tools, CAD tools, and process planning tools as well as in generic 'groupware' tools such as departmental calendar systems and collaborative writing tools. Thus, as CSCW facilities are introduced in different applications and their functionality is enhanced, users will be inundated with overhead activities of articulating the different mechanisms: updating each mechanism with respect to changes

[1] We have discussed the issues concerning such facilities ('computational mechanisms of interaction') at length elsewhere (Simone and Schmidt, 1993; Schmidt, 1994a; Schmidt and Rodden, 1996).

to other mechanisms. In order to prevent CSCW systems from thereby introducing an impedance between the multitude of interlaced—individual and cooperative—activities, a CSCW environment must provide means for establishing links between the different mechanisms incorporated in the different applications and systems, at least locally and temporarily. Furthermore, since cooperative work is articulated with respect to and in terms of domain-specific objects and structures, CSCW mechanisms incorporated in applications must provide access to these objects and structures, e.g., via information systems providing access to common repositories (previous designs, components, work in progress, drawings, patents), to other available human and technical resources (skills, machinery), to statutory constraints, and so on.

Thus, a central problem in the design of CSCW facilities that are actually able to support real-world cooperative ensembles in handling the increasing complexities of their work, is to provide an appropriate 'interface' to the wider *organizational context* as represented by other CSCW facilities incorporated in other applications as well as domain-specific information systems such as MIS, OIS, CIM, and CASE systems (De Michelis and Grasso, 1993; Fuchs and Prinz, 1993; Prinz, 1993). The challenge is, as Ellis and Keddara aptly put it, to make groupware 'organizationally aware' (Ellis et al., 1995).

With this in mind, the objective of the present paper is to examine the issue of the relationship between cooperative work and the wider organizational context. In doing so, the paper will concentrate on a critique of the Transaction Cost approach, not because of its weaknesses but because of its potential strengths as a conceptual foundation for the design of information systems and other computer-based infrastructures in organizational settings: (1) it identifies the relative costs of information processing under different degrees of 'uncertainty' as the underlying generative mechanism of the emergence of organizations; (2) it offers a healthy dose of crass realism by conceiving of conflicting goals and motives as constituent of organizational formations; and (3), by combining the two, it posits a generative mechanism for the emergence and development of organizational formations. Taken together, these features of Transaction Cost theory makes it potentially amenable to concurrent design of information systems and business organizations (Ciborra, 1981, 1985; Malone et al., 1987; Ciborra and Olson, 1988). Thus, in examining the relationship between cooperative work and the wider organizational context, we should examine the applicability of the Transaction Cost approach.

The purpose of the discussion is not to contribute to organizational theory in general, nor is it to critique Transaction Cost theory as a theory of institutional economics, but to critique the Transaction Cost approach *from the point of view of cooperative work*: Does it provide a theoretical foundation for a conception of the relationship between cooperative work and the wider organizational context? Accordingly, the following discussion does not pretend to review the vast body of literature constituting the Transaction Cost movement. Rather, in order unravel the underlying assumptions and discuss their pertinence with respect to CSCW issues, the discussion focuses on the classics of the movement.

2 The 'Leviathan' Approach to Organizational Theory

While most contributions to Transaction Cost theory address important aspects of cooperative work and the organization of cooperative work, they all investigate cooperative work and organizational interactions *from the point of view of the firm*—not from the point of view of cooperative work. The reason being that the central issue for the Transaction Cost movement is the *relationship between organization and market*.[2]

The problem that haunts the different authors in this movement is how to reconcile the radical presuppositions of Neo-Classical Economics with the manifest existence of firms and corporations encompassing multiple actors engaged in various activities. Why do some economic interactions occur within firms and other corporate entities while other interactions do not? Or, to use the classic metaphor of Adam Smith and Wright Mills, why is the Invisible Hand of market-mediated coordination replaced by the Visible Hand of direct administrative coordination (Smith, 1776a; Mills, 1951)?

2.1 Commons

The Transaction Cost school may be said to originate with the work of Commons on the economic origins of 'collective action' (Williamson, 1975, p. 3, 1981, p. 550). The analysis by Commons is based on the proposition that 'conflict of interest' is universal and fundamental to political economy. He is in this respect building on Hume and Malthus who, according to Commons, 'made scarcity the basis of coöperation, fellow feeling, justice, and property' (Commons, 1934, p. 6).

According to Commons, then, economic activity involves not only ubiquitous and rampant conflict of interest but also—given the mutual dependence of the conflicting interests—the endeavor to bring 'order out of the conflict of interests' through the collective action of various institutions (Commons, 1934, p. 4). Consequently, Commons' conceives of organization as a 'collective action' established through 'coöperation', that is, through the institutional subjection of individual self-interests to the putative common good. The firm and other forms of 'collective action' is thus a governance structure emerging to curb the centrifugal forces of individual self-interests. Commons' notion of organization is thus closely related to the Hobbesian Leviathan that is called for to prevent 'that miserable

[2]Ouchi has created much confusion in the transaction cost school by defining an organization 'as any stable pattern of transactions' (Ouchi, 1980, p. 132)—only to contradict this definition on the very same page by stating: 'In this definition, a market is as much an organization as is a bureaucracy or a clan.' (ibid.) Typically, however, the patterns of transactions in markets are not particularly stable; to the contrary, they are typically volatile and transient. And if the pattern of transactions is not stable then, according to Ouchi's own definition, there is no organization, market or no market. What he intends to say is that organization and market are two alternative governance structures.

condition of Warre, which is necessarily consequent [. . .] to the natural Passions of men, when there is no visible Power to keep them in awe, and tye them by feare of Punishment to the performance of their Covenants’ (Hobbes, 1651, Chap. XVII, p. 223). Thus, in a language strongly reminiscent of Hobbes’, Commons expounds his concept of ‘coöperation’:

> coöperation does not arise from a *presupposed* harmony of interests, as the older economists believed. It arises from the necessity of *creating a new harmony* of interests—or at least order, if harmony is impossible—out of the conflict of interests among the hoped-for coöperators. It is the negotiational psychology of persuasion, coercion, or duress. The greatest American piece of actual coöperation, latterly under ill repute [anno 1934], is the holding companies which suppress conflicts, if persuasion proves inadequate. A more universal coöperation, suppressing conflict in behalf of order, is proposed by Communism, Fascism, or Nazism. These have found their own ways of submerging conflicts of interest. Hence, harmony is not a presupposition of economists—it is a consequence of collective action designed to maintain rules that shall govern the conflicts. (Commons, 1934, pp. 6 f.)

The most influential contribution by Commons, however, is his introduction of *the transaction* as the ‘unit of economic activity’: ‘I made the transaction the ultimate unit of economic investigation, a unit of transfer of legal control.’ (Commons, 1934, pp. 4, 55). For Commons, transactions occur between actors defined in terms of ownership. That is, transactions occurs when goods are transferred across boundaries of private property; ‘transactions [. . .] are the transfer of ownership’ (Commons, 1934, p. 58):

> Transactions [. . .] are the alienation and acquisition, between individuals, of the right of future ownership of physical things, as determined by collective working rules of society. The transfer of these rights must therefore be negotiated between the parties concerned, according to the working rules of society, before labor can produce, or consumers can consume, or commodities be physically delivered to other persons. (Commons, 1934, p. 58)

That is, a transaction can be seen as an interface between conflicting interests. Consequently, the notion of transaction is imbued with this sentiment: ‘I make conflict of interest predominant in transactions. But I conclude that this cannot be allowed to be the only principle, because there are also mutual dependence and the maintenance of order by collective action’ (Commons, 1934, p. 6). In these two sentences, Commons stated the core of the program of the subsequent Transaction Cost movement: Conflict of interest is predominant in transactions; because of the mutual dependence of the warring parties, however, order must be established and maintained by collective action in the form of organization.

2.2 Coase

What Commons did not explain or even address was the obvious question: Why are some transactions carried out within organizations while others are carried out beyond the auspices of organization?

This question was addressed and—to some extent—solved by Coase, another pioneer of the transaction cost approach to the theory of the firm. Coase retains the dichotomy of market and firm. Thus, quoting D. H. Robertson's colorful description, he conceives of firms as 'islands of conscious power in this ocean of unconscious cooperation like lumps of butter coagulating in a pail of buttermilk' (Coase, 1937, p. 388). Or in his own words:

> Outside the firm, price movements direct production, which is coordinated through a series of exchange transactions on the market. Within a firm, these market transactions are eliminated and in place of the complicated market structure with exchange transactions is substituted the entrepreneur-coordinator, who directs production. It is clear that these are alternative methods for co-ordinating production. (Coase, 1937, p. 388)

Coase's innovation was to explicitly conceive of markets and firms as alternative 'governance structures' and to explain the proportions of market and firm coordination by measuring the administrative costs of each in the same unit, namely transaction costs. Both modes of economic coordination carry costs of administering a transaction: the costs of discovering relevant prices and of negotiating, implementing, and enforcing a contract in the market, versus the managerial costs of organizing transactions in the firm. 'The main reason why it is profitable to establish a firm would seem to be that there is a cost of using the price mechanism' (Coase, 1937, p. 390). Firms exist where the cost of conducting a transaction within the firm is less than the cost of conducting the same transaction in the market: 'a firm will tend to expand until the costs of organising an extra transaction within the firm become equal to the costs of carrying out the same transaction by means of an exchange on the open market' (Coase, 1937, p. 395). Taking the analysis a step further, Coase identifies the source of the cost of using the price mechanism by highlighting the 'uncertainty' facing economic actors: 'It seems improbable that a firm would emerge without the existence of uncertainty' (Coase, 1937, p. 392).

According to Coase, all economic activity requires transactions defined as an act whereby 'resources are allocated' between the different 'factors of production' (Coase, 1937, pp. 389, 391). In redefining transactions this way, and thus conceiving of 'transaction[s] within the firm' (Coase, 1937, p. 395), Coase enables us to conceive of markets and hierarchies as alternative governance structures, that is, alternative 'coordination instruments' for the 'allocation of resources' (Coase, 1937, p. 389), and he makes the transaction concept far more powerful. However, his solution raises another problem, namely: What constitutes a transaction, then? Coase did not address this problem explicitly, he merely seemed to assume (Coase, 1937, p. 388) that all work could, in principle, be carried out by individuals who then interact and coordinate on the open market (at a higher cost, of course).[3]

This, however, is not a realistic assumption, to put it mildly. As already pointed out by Hodgskin (1825) at the dawn of the industrial era:

> Wherever division of labour exists, and the further it is carried the more evident does this truth become, scarcely any individual completes of himself any species of produce. Almost any product of art and skill is the result of joint and combined labour. So dependent is man

[3]That claim is made explicitly by Ouchi: 'The 10,000 individuals who comprise the workforce of a steel mill could be individual entrepreneurs whose interpersonal transactions are mediated entirely through a network of market and contractual relationships.' (Ouchi, 1980, p. 134).

> on man, and so much does this dependence increase as society advances, that hardly any labour of any single individual, however much it may contribute to the whole produce of society, is of the least value but as forming a part of the great social task. In the manufacture of a piece of cloth, the spinner, the weaver, the bleacher and the dyer are all different persons. All of them except the first is dependent for his supply of materials on him, and of what use would his thread be unless the others took it from him, and each performed that part of the task which is necessary to complete the cloth? [. . .] Each *labourer produces only some part of a whole, and each part having no value or utility of itself, there is nothing on which the labourer can seize, and say: "This is my product, this will I keep to myself."* (Emphasis added.)

That is, if the performance of an individual cannot be measured, the notion of a transaction does not make any sense. Thus, when confronted with the realities of cooperative work, transaction as a 'unit of analysis' is not as simple as assumed by Coase (not to mention Commons). What is the exact relationship between the realities of cooperative work and transactions? This problem was addressed innovatively by Williamson.

2.3 Williamson

Following his predecessors, Williamson makes transactions 'the basic unit of analysis' (Williamson, 1981, p. 549). However, in Williamson's analysis, economic interactions do not necessarily take the form of transactions. Rather, he posits, transactions take place between—at the 'interface' between—different cooperative work arrangements: 'A transaction occurs when a good or service is transferred across a technologically separable interface. One stage of activity terminates and another begins.' (Williamson, 1981, p. 552).

Williamson makes is perfectly clear that cooperative work activities that are brought together under the same scheme of common ownership in order to share resources are technologically separable: 'the joining of separable stations—for example, blast furnace and rolling mill, thereby to realize thermal economies—under common ownership is not technologically determined but instead reflects transaction-cost-economizing judgments' (Williamson, 1981, p. 556). That is, according to Williamson, a cooperative work arrangement (the cooperative activities taking place at a 'station' such as a steel furnace or a hot rolling mill) is not 'technologically separable'.[4] In other words, according to Williamson cooperative work at 'stations' has a unitary and indivisible character that is 'technologically determined'. Thus, in Williamson's conception, *cooperative work arrangements and transactions are complementary units of analysis*. The one starts when the other terminates, and vice versa.[5]

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[4]Ouchi's analysis (1980), while pretending to follow Williamson's, is actually an emulation of Coase's. Like Coase, Ouchi assumes that all economic activity could be carried out by individuals on the market.

[5]It is worth noticing that this definition of transaction is radically different from the one offered by Commons and the one offered Ouchi.

By defining the unit of analysis, the transaction, with reference to the interface between technologically separable cooperative entities, and by thus taking cooperative work into account, at least nominally and marginally, Williamson supersedes the implicit individualism of Neo-Classical Economics and of his predecessors and relates his reasoning to the realities of cooperative work that massively characterize the realm of work in modern industrial society. Thus, according to Williamson, firms are *aggregations of cooperative work arrangements*, not of atomic individuals. Nonetheless, for Williamson, like Coase, the reason for the emergence of firms is 'market failure'. The cooperative work arrangements are joined under 'common ownership', even though they are technologically separable entities, in order to counter market imperfections caused by bounded rationality and opportunism. Williamson thus retains the dichotomy of market and hierarchy.

Accordingly, Commons' Hobbesian notion of the organization as a common power to keep the centrifugal forces of self-interest in awe is echoed in Williamson's notion of contracting as a means to curb 'opportunism':

> if agents [. . .] were fully trustworthy, comprehensive contracting would still be feasible (and presumably would be observed). Principals would simply extract promises from agents that they would behave in the manner of steward when unanticipated events occurred, while agents would reciprocally ask principals to behave in good faith. Such devices will not work, however, if some economic actors (either principals or agents) are dishonest (or more generally, disguise attributes or preferences, distort data, obfuscate issues, and otherwise confuse transactions), as it is very costly to distinguish opportunistic from non-opportunistic types ex ante. (Williamson, 1981, p. 554)

The general conception of organization developed by the Transaction Cost approach can be summarized as follows: Organizations are entities of *common ownership*: 'islands of conscious power in this ocean of unconscious cooperation like lumps of butter coagulating in a pail of buttermilk'. They arise so a to provide a governance structure to certain types of transactions for which market exchanges are inadequate (less cost-efficient) as a governance structure. Outside the organizational boundary is the market, 'inside management exercise authority and curb opportunistic behavior' (Powell, 1989).

3 Critique of the 'Leviathan' Approach

As far as design of CSCW systems (as well as the design of work organizations) is concerned, the Transaction Cost approach suffers from the following fundamental problems:

1. *The myth of the primordial market.* The basic methodological presupposition of the Transaction Cost approach is to conceive of the market as the default governance structure. The market is taken to be the Natural State, as it were: 'The technique is to contend that all transactions can be mediated entirely by market relations' (Ouchi, 1980, p. 133). Hence the persistent preoccupation with the question why market forces have failed wherever an organization can be observed. However, this methodological contention is empirically unfounded—as any student

of archaeological, historical, and ethnographic evidence will know. Or, to be quite candid, it is a fiction (Powell, 1989).

Along with the presupposition that the market is the default governance structure, the Transaction Cost approach has inherited the implicit radical individualism underlying Neo-Classical Economics. In the Transaction Cost world—that is, the world as seen from a Transaction Cost perspective—individuals only interact as opportunistic actors trying to maximize their own individual gains. Of course, opportunistic behavior is part and parcel of economic life, under the auspices of 'common ownership' as well as on the 'open market', and in designing CSCW system this fact of life must certainly be taken into account (Kling, 1980; Grudin, 1989; Orlikowski, 1992). But when promoted to the status of a general generative mechanism of organization, the conception of organization as a (cost-efficient) governance structure for curbing opportunistic behavior among economic actors becomes a gross exaggeration. When this notion is applied as the dominant or exclusive conception of cooperative work in organizational settings, essential aspects the multi-faceted phenomenon of cooperative work is marginalized or simply lost: the cooperative work itself: operating the hot rolling mill or the blast furnace and interacting through changing the state of the hot rolling mill or the blast furnace; the many technical and social skills required; the effort of maintaining a mutual awareness by monitoring what others are doing and making one's own work publicly visible; the mutual help (Popitz et al., 1957; Heath and Luff, 1992a; Harper and Hughes, 1993).

2. *The dichotomy market versus hierarchy.* The organization is conceived of as a Leviathan, 'a common Power to keep them all in awe' (Hobbes, 1651, Chap. XIII, p. 185). It is a monolith constituted by 'common ownership' and controlled from one center in the sense that there is, somewhere, a set of consistent interests and goals to which the opportunistic behavior can be subjected by a single and unitary will that presumably also exists somewhere.

It is difficult, if not impossible, to relate the highly abstract notion of 'common ownership' to the infinitely differentiated relations of ownership and possession and rights and obligations that characterize the realm of work: Employees will for instance successfully treat the pencils, desks, computers, etc. they use in their work as *their* possession.

Also, as pointed out by, *inter alia*, Powell (1989), Best (1990), and Stinchcombe (1990) the rich variety of organizational forms adopted by contenders in the market is ignored by the market/hierarchy dichotomy: firms, corporations, multi-divisional corporations, joint-ventures, strategic alliances, equity partnerships, collaborative consortia for large-scale research, supplier networks (e.g., Toyota and its association of 35,000 suppliers), co-operatives, 'quasi-firms' in construction, tacit networks of recurrent contractors in publishing, and regional networks and industrial districts (e.g., Modena in Emilia-Romagna, the textile industry in Baden-Wurttemberg, the Route 128 in the US). 'Many firms are no longer structured like medieval kingdoms, walled off and protected from hostile forces. Instead, we find companies involved in an intricate latticework of collaborative ventures with other firms, most of whom are ostensibly competitors.' (Powell, 1989, p. 301)

The dichotomy of market and hierarchy underlying Transaction Cost theory does not help us here. In the words of Powell: 'By sticking to the twin pillars of markets and hierarchies, our attention is deflected from a diversity of organizational designs that are neither fish for fowl, nor some mongrel hybrid, but a distinctly different form.' (Powell, 1989, p. 299). In fact, on closer inspection, categories such as 'market exchange' and 'common ownership' tend to loose their superficial clarity:

> When the items exchanged between buyers and sellers possess qualities that are not easily measured, and the relations are so long-term and recurrent that it is difficult to speak of the parties as separate entities, can we still regard this as a market exchange? When the entangling of obligation and reputation reaches a point that the actions of the parties are interdependent, but there is no common ownership or legal framework, do we not need a new conceptual tool kit to describe and analyze this relationship? Surely, this patterned exchange looks more like a marriage than a one-night stand, but there is no marriage license, no common household, no pooling of assets. (Powell, 1989, p. 301)

3. *The dichotomy of cooperative work and organization.* Williamson's attempt to define transactions as the interface between technologically separable units, 'stations'—which are, in their turn, non-separable cooperative work arrangements—suffers from a dichotomy quite similar to the market/hierarchy dichotomy. Williamson's concept of transaction does not take into account that cooperative work arrangements are constituted by interdependencies of different nature and intensity. In his analysis, the units are either 'technologically separable'—or not. Thus, while he attempts to accommodate for the realities of cooperative work, the notion of a singularity (as the subject of transactions), is still at work. As a result, since the formation of organization, in Williamson's analysis, begins at the boundary of the cooperative work arrangement, cooperative work and organization are of different worlds. Organization is not organization of cooperative work but of transactions between otherwise unrelated singularities—like potatoes in a sack (Marx, 1852, p. 180).

The Transaction Cost approach does not enable us to grasp the rich multiplicity of interdependency and reciprocity among actors in cooperative work arrangements. The Transaction Cost world is populated by singularities (individuals in the case of Commons, Coase, and Ouchi, and 'stations' of cooperative work in the case of Williamson) who are partially conflicting and mutually repellent and whose only interactions take the abstract form of allocations of resources. What else might occur in organizational life is beyond Transaction Cost.

The Transaction Cost notion of organization is that of a system of decontextualized and dematerialized administrative regulation of transactions between singularities. That is, firms are only conceived of as *administrative governance structures*. In the words of Best:

> It is an advance to envisage the firm as a governance structure as opposed to a production function, but Williamson's concept of governance structure is not given much force. The production activities themselves are independent of the governance structure. Governance refers simply to administrative coordination. But Big Business is about more than coordination of, in Williamson's words, "technologically separable entities" [. . .]; it is also about interrelations amongst mutually interdependent units. [. . .] A theory of the firm must move beyond considerations of coordination with concepts that allow for links between governance structure and production performance. . . (Best, 1990, p. 115)

The Transaction Cost approach has proved to be quite useful in requirements analysis in administrative work domains (e.g., public administration, corporate administration, banking, insurance) (Ciborra, 1985; Schmidt, 1986, 1988a). Beyond these domains, however, the abstract notion of organization as an administrative governance structure is fundamentally inadequate. More importantly, in the context of CSCW, the facets of organizational life that the Transaction Cost approach can grasp are marginal to the rich variety of interactions of cooperative work and its articulation.

4 Beyond the 'Leviathan' Approach

The weaknesses of Transaction Cost approach as conceptual basis for the design of CSCW systems for organizational settings (and for the design of work organizations), can be attributed to its intellectual roots in Neo-Classical Economics and contract theory. These roots reveal themselves in the market mythology, the radical individualism, the dichotomy of market and hierarchy, the purely administrative notion of coordination, and so on.

However, by combining the seminal contribution of the Carnegie-Mellon school of organizational theory, especially the concepts of task uncertainty and bounded rationality (March and Simon, 1958; Cyert and March, 1963; Thompson, 1967), with the economic concept of the relative costs of handling these complexities and uncertainties under different arrangements, Transaction Cost theory (in particular Williamson, 1975, 1981; Ciborra, 1981, 1985) has sketched a sound materialistic approach to analyzing organizational formations that—when released from the fetters of market mythology and radical individualism as well as the dichotomy of market and hierarchy and the purely administrative notion of coordination—provides us with something that can serve as Ariadne's thread in the labyrinth of the organization of cooperative work.

4.1 Perspectives of a 'Cooperative Work' Approach to Organizational Theory

First, however, we need to establish that 'organization' is such an enormously complex and infinitely facetted phenomenon that the mere thought of developing an overarching theory of the organization phenomenon seems unrealistic (for want of stronger words). Given that, we must learn to live with multiple perspectives—each serving and defined by a specific purpose—and distinguish multiple superimposed organizational formations.[6]

[6]The justification of, and need for, a 'multi-layer' approach has been demonstrated quite convincingly in the work of LaPorte, Rochlin and others (Rochlin et al., 1987; Rochlin, 1989; La Porte and Consolini, 1991).

From the point of view of cooperative work, the following four perspectives on organization are particularly relevant:

(1) the *cooperative work arrangement* as an emergent formation;
(2) the *work organization* as a relatively persistent pattern of cooperative work arrangements;
(3) the *formal organization* as a governance structure regulating the diverse, partially incongruent interests within the cooperating ensemble;
(4) the *firm*, the *network*, etc. as different forms of market-oriented organization.

In the following sub-sections we will discuss these four perspectives on cooperative work and its organization.

4.1.1 Cooperative Work Arrangement

The point of departure is to conceive of cooperative work in terms of actual interdependence in work, as opposed to the notion of 'cooperation' as institutional subjection of individual self-interests to a putative common good that stems from the notion of the primordial market and the concomitant radical individualism. In fact, the concept of 'cooperative work' has a long tradition in economic and sociological investigation. It was used as early as the first half of the 19th century by economists such as Ure (1835) and Wakefield (1849) as the general and neutral designation of work involving multiple actors and was further developed by Marx (1867a) who defined it as 'multiple individuals working together in a conscious way [planmässig] in the same production process or in different but connected production processes.' In this century, the term has been used extensively with the same general meaning by various authors, especially in the German tradition of the sociology of work (e.g., Popitz et al., 1957; Bahrdt, 1958, 1959; Kern and Schumann, 1970; Mickler et al., 1976).

A cooperative work relationship is constituted by the fact that multiple actors are transforming or controlling a complex of *mutually interacting* objects and processes. They are, so to speak, working on the same 'field of work' (Schmidt, 1994b). They are therefore mutually dependent *in their work* in the sense that one actor depends on the quality and timeliness of the work of the others and vice versa (Schmidt and Bannon, 1992).

Because of the underlying and constitutive interdependence, individual actors must articulate (divide, allocate, coordinate, schedule, mesh, interrelate, etc.) their respective activities. Thus, by entering into cooperative work relations, the participants must engage in activities that are, in a sense, extraneous to the activities that contribute directly to fashioning the product or service and meeting the need. The obvious justification of incurring this 'overhead cost' and thus the reason for the emergence of cooperative work formations is, of course, that actors could not accomplish the task in question if they were to do it individually, at least not as well, as fast, as timely, as safely, as reliably, as efficiently, etc. (Schmidt, 1990). Thus, a shift from cooperative to individual work can be observed wherever and whenever new technologies augment the capabilities of individual actors so as to accomplish the given task individually: combine harvesters, bulldozers, pocket calculators, word processors, etc.

Generally speaking, cooperative work relations are formed because of the limited capabilities of single human individuals faced with the complexity and uncertainty of the task, that is, because the work could not be accomplished otherwise, or at least could not be accomplished as quickly, as efficiently, as well, etc., if it was to be done on an individual basis. In the words of Barnard: 'If we eliminate from consideration personal satisfaction [. . .], their coöperation has no reason for being except as it can do what the individual cannot do. Coöperation justifies itself, then, as a means of overcoming the limitations restricting what individuals can do' (Barnard, 1938, p. 23). More specifically, a cooperative work arrangement may emerge in response to different requirements and may thus serve different generic functions (Schmidt, 1990):

Augmentation of capacity: A cooperative work arrangement may simply augment the mechanical and information processing capacities of human individuals and thus enable a cooperating ensemble to accomplish a task that would have been infeasible for the actors individually. As an ensemble they may, for instance, be able to remove a stone that one individual could not move one iota. This is cooperative work in its most simple form. By cooperating, they simply augment their capacity: 'With simple cooperation it is only the mass of human power that has an effect. A monster with multiple eyes, multiple arms etc. replaces one with two eyes etc.' (Marx, 1861–1863, p. 233).

Differentiation and combination of specialties: A cooperative work arrangement may combine multiple *technique-based specialties*. In augmentative cooperation the allocation of different tasks to different actors is incidental and temporary; the participants may change the differential allocation at will. By contrast, technique-based specialization requires an 'exclusive devotion' to a set of techniques. That is, as opposed to the contingent and reversible differentiation of tasks that may accompany augmentative cooperation, the *technique-based specialization is based on an exclusive devotion to a repertoire of techniques*. In the words of the eulogist of technique-based specialization, Adam Smith: 'the division of labour, by reducing every man's business to some one simple operation, and by making this operation the sole employment of his life, necessarily increases very much the dexterity of the workman' (Smith, 1776a, pp. 9–10). The different techniques must be combined, however, and the higher the degree of technique-based specialization, the larger the network of cooperative relations required to combine the specialties (Babbage, 1832, §§ 263–268, pp. 211–216). That is, *technique-based specialization requires combinative cooperation*. This combinative cooperation is defined by Marx as 'cooperation in the division of labor that no longer appears as an aggregation or a temporary distribution of the same functions, but as a decomposition of a totality of functions in its component parts and unification of these different components' (Marx, 1861–1863, p. 253).

Mutual critical assessment: A cooperative work arrangement may facilitate the application of multiple problem-solving *strategies and heuristics* to a given problem and may thus ensure relatively balanced and objective decisions in complex environments. Under conditions of uncertainty decision making will require the exercise of discretion. In discretionary decision making, however, different individual decision makers will typically have preferences for different heuristics (approaches, strategies, stop rules, etc.). Phrased negatively, they will exhibit different characteristic 'biases'. By involving different individuals, cooperative work arrangements in complex environments become arenas for different decision making strategies and propensities where different decision makers subject the reliability and trustworthiness of the contributions of their colleagues to critical evaluation (Schmidt, 1990). This process of mutual critical evaluation was described by Cyert and March (1963) who aptly dubbed it 'bias discount.' Even though dubious assessments and erroneous decisions due to characteristic individual biases are transmitted to other decision makers, this does not necessarily entail a diffusion or accumulation of mistakes, misrepresentations, and misconceptions within the decision-making ensemble. The cooperating ensemble establishes a negotiated order.

Confrontation and combination of perspectives: A cooperative work arrangement may finally facilitate the application of multiple *perspectives* on a given problem so as to match the multifarious nature of the field of work. A perspective, in this context, is a particular conceptualization of the field of work, that is, a conceptual reproduction of a limited set of salient structural and functional properties of the field of work, such as, for instance, interdependencies, generative mechanisms, causal laws, and a concomitant body of representations (taxonomies, models, notations, etc.).

To grasp of the diverse and contradictory aspects of the field of work as a whole, the multifarious nature of the field of work must be matched by a concomitant multiplicity of perspectives on the part of the cooperating ensemble. The application of multiple perspectives will typically require the joint effort of multiple agents, each attending to one particular perspective and therefore engulfed in a particular and parochial small world. The cooperative ensemble must articulate (interrelate and compile) the partial and parochial perspectives by transforming and translating information from one level of conceptualization to another and from one object domain to another (Schmidt, 1990).

In sum, a cooperative work arrangement arises simply because there is no omniscient and omnipotent agent.

Cooperative work arrangements are thus conceived of as transient formations, emerging contingently to handle specific requirements—in response to the requirements of the current situation and the technical and human resources at hand—merely to dissolve again when there is no need for multiple actors and their coordinated effort to handle situations. Different requirements and constraints and different technical and human resources engender cooperative work arrangements of different size and shape.

By conceiving of cooperative work arrangements in terms of actually interdependent activities (as opposed to legal criteria), a business firm may encompass multiple cooperative work arrangements with no mutual interaction and, conversely, a cooperative work arrangement may cut across corporate boundaries and may involve partners in different firms at different sites.

Whereas Williamson assumes that cooperative work arrangements are permanent singularities (. . . as far as the theory is concerned), cooperative work arrangements are here taken to be dynamic and emergent formations. Furthermore, the relations of mutual dependence that constitute the cooperative work arrangement are by no means uniform. To the contrary, the interdependencies of the activities of the cooperating actors differ with respect to complexity, coupling, and uncertainty (La Porte, 1975; Perrow, 1984; Woods, 1988; Schmidt, 1994b). That is, in a cooperative work arrangement different activities may depend on other activities in different ways and with different intensity. A cooperative work arrangement is therefore not a uniform network that can be taken as a singularity, an atomic element solely interacting with other atomic elements via market-mediated transactions or via the administratively mediated transactions of a firm but should rather be conceived of as a complex of interdependent activities whose interdependencies differ in kind and intensity. Thus, cooperative work arrangements are not discrete entities, singularities whose internal composition and topology can be ignored, but rather dynamic networks of varying intensity and density.

4.1.2 Work Organization

Now, establishing a cooperative work arrangement entails an *initial* overhead cost—not in terms of transaction costs, i.e., the cost of handling allocation of resources and performance measurement and remuneration—but in terms of (a) the need for identifying likely and appropriately skilled partners and negotiating the allocation of tasks and responsibilities, and (b) the need for new partners to acquire particular skills and become acquainted with local settings and practices. Therefore, in order to reduce this initial overhead, cooperative work is normally *organized*—in the sense that the articulation of cooperative activities assumes a specific and relatively stable *organizational form*.

In other words, under conditions of recurrent tasks, cooperative work arrangements exhibit persistence in terms of the composition of the cooperative ensemble (as a cohort) and the allocation of tasks and responsibilities within it. In accordance with the general usage in the literature, we will suggest the term ‘work organization’ to denote the relatively stable composition and structure of the cooperative work arrangement as determined by the demands and constraints of the work environment, that is, the decomposition of the work into tasks, the allocation of tasks within the ensemble, and the combination of tasks into jobs (Mintzberg, 1979; Child, 1987; Cummings and Blumberg, 1987).

That is, in order for cooperative work to be an economically viable way of working, the arrangements cannot—as the rule—be established in an *ad hoc* manner. A certain orderliness is required in terms of the stability and reliability of the composition and structure of the ensemble. The degree and form of orderliness required depends, of course, on the specific demands and constraints posed by the specific work environment such as, for instance, adaptability and safety of the operation, reliability of product quality, reliability and timeliness of delivery, and so forth.

For example, having observed that advanced manufacturing systems are tightly coupled to vendors and customers and that this may place severe demands on the adaptive capacity of the system, Cummings and Blumberg conclude that for advanced manufacturing systems the 'appropriate work designs should be oriented to groups of employees rather than individual jobs, and to employee self-control rather than external forms of control, such as supervision. This calls for self-regulating work groups' (Cummings and Blumberg, 1987). In the same vein, Aoki observes that the semi-horizontal mode of coordination required in modern flexible manufacturing operations 'crucially depends on the skills, judgment, and cooperation of [a] versatile and autonomous work force on the shop floor', and 'a certain degree of blurring of job territoriality between workers on the one hand and foremen, engineers, programmers, etc., on the other' (Aoki, 1988).

Likewise, a cooperative work arrangement operating in a safety-critical environment, for example a governmental policy making body, will need to devote resources to prevent decisions that may jeopardize the system itself or its environment. The classical method applied by civil services faced with political risks and a high degree of task complexity is to involve multiple officers to decision making tasks so as to ensure that any decision is assessed critically by multiple actors. In general, the higher the risk and the higher the degree of discretion in decision making, the higher the degree of (apparent) redundancy in the work organization is likely to be (Rochlin et al., 1987; La Porte and Consolini, 1991).

By thus defining the work organization as a constellation of deployable resources configured so at to meet the needs of a, more or less broadly defined, repertoire of recurring cooperative activities, the emergence and configuration of the work organization is conceived of in terms the costs of searching for and acquiring the required resources on as the need arises versus the costs of maintaining resources in a stand by mode.

Furthermore, since cooperative work arrangements may cut across boundaries of ownership, the work organization may as well cut across such boundaries.

4.1.3 Formal Organization

A cooperative work arrangement invariably involves multiple individuals with partially diverging interests and motives; its organization is thus, in a sense, a 'coalition' of individuals motivated by individual interests and aspirations and pursuing individual goals (Cyert and March, 1963, p. 27). That is, cooperative work activities will hardly take place if these discordant interests are not mediated and regulated in some way. Thus, as observed by Barnard:

> 'In coöperation the objective of action is necessarily removed from the individual, requiring a new form of activities, those of distribution.' (Barnard, 1938, p. 36). 'Personal purposes cannot be satisfied through coöperative action except as there comes into the action an intermediate process. This process is distributive.' (Barnard, 1938, p. 32.)

In other words, participants in cooperative work activities will ask, overtly or tacitly: What's in it for me? And if they are not satisfied, or convinced, that their contribution to the joint effort is worthwhile they may withdraw their contribution (again overtly or tacitly). Thus, *a governance structure of contractual arrangements* administrating the allocation of resources and the measurement and remuneration of performance within the cooperative ensemble is required which we, again in accordance with the literature, can call the *formal organization* in the sense that the arrangement is explicitly defined (e.g., in the form of statutes) and can be enforced through legal or administrative means.

Superimposed on the shifting patterns of cooperative work arrangements and the provision of adequate skills in the form of the work organization, we then enter the world analyzed by Transaction Cost theory. However, the analysis outlined here does not take 'market failure' as the point of departure but, to the contrary, the fact that work is done cooperatively in the first place, and conceives of the formal organization as a required governance structure of the cooperative ensemble. The formal organization of cooperative work is required independently of whether the members of the cooperating ensemble are independent operators, employees of the same firm or of several firms. This analysis is corroborated by Stinchcombe's analysis of contractual arrangements:

> A structure with legitimate authority; with a manipulable incentive system; with a method for adjusting costs, quantities, and prices; with a structure for dispute resolution; and with a set of standard operating procedures looks very much like a hierarchy, very little like a competitive market. Yet all these features of hierarchy are routinely obtained by contracts between firms in some sector of the economy. (Stinchcombe, 1990, p. 198)

From a cooperative-work perspective, then, the formal organization is, essentially, a contractual governance structure influencing the behavior of individuals (and collectives) in accordance with the interests of the other members of the ensemble as well as the interests of external stakeholders to which the ensemble may be accountable such as customers, shareholders, creditors, government agencies, industrial federations, trade unions, consumer groups, standardization agencies, etc.

4.1.4 Firm, Network...

Finally and obviously, cooperative ensembles often take the form of a *firm*, in the specific legal sense of an entity of ownership within which resources are, in principle, common property, such as a corporation or a company. Sometimes, however, also cooperative ensembles that are only loosely or intermittently interdependent, or not interdependent at all, are subjected under the same ownership scheme. And conversely, cooperative work arrangements may involve partners belonging to different ownership schemes.

For the purpose of this discussion, it is sufficient to observe that, when facing the market, cooperative ensembles and clusters of cooperative ensembles adopt certain organizational forms that allow them to amass resources in order to obtain the competitive impact that is deemed necessary, sometimes in the form of informal networks of mutual obligations, sometimes in the form of contractual arrangements, sometimes in the form of firms, joint-ventures, etc. The important point in the context of this discussion is that the organizational forms adopted to meet the challenges of the market do not necessarily match the cooperative work arrangements, the work organization, not even the formal organization. That is, the different, superimposed organizational formations are bounded differently and develop according to different demands and constraints.

5 Conclusions

In sum, *the* organization does not exist. The organization phenomenon is, rather, a complex of superimposed and interacting organizational formations (of which we, for our purpose, have discussed four). Further, there is no overarching conceptual scheme for the analysis of organization—and none is required. No single generative mechanism—not even a strong contender as Transaction Cost—can explain the formation of organizations in general nor the formation of firms.

However, conceiving of organization from the point of view of cooperative work provides a foundation for overcoming some of the weaknesses of the Transaction Cost approach—without abandoning the undeniable gains made by that school, in particular the dynamic approach to an understanding of the emergence of organizations and the specific gains such as opportunistic decision making and the relative costs of administrating the allocation of resources and the measurement and remuneration of performance. Conceiving of organization from the point of view of cooperative work also provides a foundation for distinguishing and relating multiple perspectives, which is needed in order to deal with phenomena of this order of complexity.

Some of the specific implications of conceiving of organization from the point of view of cooperative work are:

(1) There is no 'common power to keep them all in awe'. The distributed activities and interactions of cooperative work are not and can not be controlled by a single will. The formal organization is merely a governance structure of certain aspects of the multifaceted realities of cooperative work.
(2) The organization of cooperative work is 'open ended'. Irrespective of the perspective applied to organization, *the organizational boundary is contingent and porous*.

Having established this, let us then revert to the issue of computational representations of organizational context in CSCW systems.

A particular cooperative work arrangement is always situated in an organizational context. From the above analysis, it is evident that the organizational context of a cooperative work arrangement is a multi-facetted and open-ended phenomenon. That is, the organizational context can not be defined and bounded in any absolute sense, only relatively, with respect to a particular cooperative work arrangement.

The organizational context of a cooperative work arrangement can thus be seen as a ‘field’ of dynamically varying intensity and density, thinning at the edges but seemingly without end: other intersecting arrangements, the enterprise, the division, the corporation, the network of suppliers and other collaborative partners, and customers, unions, banks, shareholders, and so on. This has radical implications for the design of systems supporting the representation of organizational context. We cannot assume that ‘the organization’ is a well bounded, closed, finite structure. What is within or beyond the organizational context is contingent, negotiable. We therefore cannot assume that the content of the representation of organizational context or the classification schemes required to construct, maintain, and provide access to it are managed by a single center. They are, most likely, themselves constructed and maintained cooperatively (Schmidt, 1991b). Accordingly, in the design of CSCW systems supporting awareness of and access to representations of organizational context we need to address the issue of how to make the classification schemes for the management of common repositories amenable to be managed cooperatively in a ongoing process of distributed decision making.

Chapter 5
Coordination Mechanisms (1996)

Towards a Conceptual Foundation of CSCW Systems Design

A major research issue in CSCW is to understand how computer systems can be instrumental in reducing the complexity of coordinating cooperative activities, individually conducted and yet interdependent.

In fact, the issue was identified and defined with admirable precision quite early in the history of CSCW, by Anatol Holt: 'The new capabilities at which coordination technology aims depend on finding and installing appropriate conceptual and structural units with which to express tasks, their diverse relations to each other and to the people who ultimately bear responsibility for them.' 'To be useful, this must be done in a flexible yet well-integrated manner, with plenty of leeway for the unpredictability of real life.' (Holt, 1985, p. 281).

Since then, this issue has been investigated by a range of eminent CSCW researchers. The initial results were not encouraging, however, in that coordination facilities were experienced as excessively rigid, either because the underlying protocol was not accessible and could not be modified (e.g., the COORDINATOR, cf. Winograd, 1986; Winograd and Flores, 1986; Flores et al., 1988), or because the facilities for changing the protocol did not support actors in modifying the protocol (e.g., DOMINO, cf. Kreifelts et al., 1991a, b).

In response to these initial experiences, a number of research projects have attempted to make coordination facilities flexible to actors, e.g., Egret (Johnson, 1992), ConversationBuilder (Kaplan et al., 1992; Bogia et al., 1993, 1996), and OVAL (Malone et al., 1992, 1995).

While closely related to these and other CSCW research efforts, our research has taken a somewhat different approach in that we have aimed at developing a computational notation which, on one hand, is sufficiently general to facilitate the construction of computer-based coordination mechanisms for any cooperative work arrangement and which, on the other hand, supports the cooperating actors themselves in constructing mechanisms which are both malleable and linkable. This

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notation—named Ariadne[1]—exists in the form of an abstract formal specification which has been partially implemented as a 'concept demonstrator' (Simone and Schmidt, 1994; Simone et al., 1995b).

The development of the general notation for constructing coordination mechanisms involves a range of research activities. Firstly, we need to understand how cooperating actors devise and use coordinative constructs such as coordinative protocols and workflows and how such constructs are supported by artifacts. A series of focused, in-depth field studies of coordinative practices in real-world cooperative settings have therefore been undertaken with the specific objective of investigating how the distributed activities of cooperative work arrangements are articulated and, in particular, how prescribed procedures and artifacts are devised, appropriated, and used for these purposes. On the basis of these and other investigations, we have developed a general conceptual framework which provides a set of categories and models which, in turn, form the basis of Ariadne. The whole process has been highly iterative, of course, in that the development of Ariadne has raised questions to the underlying conceptual framework which, again, has generated issues to be investigated further in the field studies. On the other hand, the findings from the field studies have be used to put the conceptual framework and the notation to test; as a result the framework and the notation have been modified and refined repeatedly.

While the research reported in this paper has been underway for several years and has been reflected in a number of papers and reports, the purpose of the present paper is to present a systematic exposition of the conceptual framework that has been developed in the course of this research and which serves as the foundation for the Ariadne notation.

The argumentation underlying this exposition is, unfortunately, rather intricate. Thus, in order to help the reader to navigate the labyrinthine exposition, its flow is punctuated by a number of propositions which highlight and offer a condensed expression of the crucial points of the argumentation.

1 The Issue of Articulation Work

In the design of conventional computer-based systems for work settings the core issues have been to develop effective computational models of pertinent structures and processes in the field of work (data flows, conceptual schemes, knowledge representations) and adequate modes of presenting and accessing these structures and processes (user interface, functionality). While these systems typically were used in cooperative work settings and even, as in the case of systems that are part of the organizational infrastructure, were used by multiple users (e.g., administrative database systems), the issue of *supporting the articulation of cooperative work by*

[1] In Greek mythology, Ariadne was the daughter of Minos, the king of Crete. She fell in love with Theseus and helped him slay the Minotaur, the monster of the Labyrinth. She did that by giving Theseus a thread that would help him to find his way in and out of the Labyrinth.

means of such systems was not addressed directly and systematically, as an issue in its own right. If the underlying model of the structures and processes in the field of work was 'valid,' it was assumed that the articulation of the distributed activities was managed 'somehow.' It was certainly not a problem for the designer or the analyst.[2]

Consider, for example, the booking system of an airline. It is a computer-based system for the cooperative task of handling reservations. The database of the booking system embodies a model of the seating arrangements of the different flights. Taken together, the seating arrangements and the database model constitute what we call *the common field of work* of the booking agents. Thus, the operators of the booking agencies cooperate by changing the state of the field of work, *in casu*, by reserving seats. Apart from providing a rudimentary access control facility, the booking system does not in any way support the coordination and integration of the interdependent activities of the operators. In this case, however, the field of work can be treated as a system of discrete and extremely simple (binary) state changes. A seat can only be assigned to one person at a time, there are no interactions between processes, and the state of any given seat can be ascertained unambiguously. Accordingly, even though a booking system does not support articulation work, it is seemingly quite sufficient for the job.

However, some if not most cooperative work arrangements in modern industrial society are faced with a far more complex field of work. The field of work may have a multitude of possible states, the state of the field of work may be ambiguous, and state changes may be interdependent in numerous ways, may occur intermittently and concurrently, and may be dynamic and unpredictable. Since members of such ensembles therefore are faced with complex interdependencies between individual activities, they cannot rely on accomplishing their individual and yet interdependent tasks merely by changing the state of the field of work. They must articulate their distributed activities in other ways. With CSCW, these issues become crucial. In fact, CSCW can be conceived of as a field devoted to exploring how computer-based systems can enhance the ability of cooperating actors in articulating their activities (cf., e.g., Schmidt and Bannon, 1992; Fitzpatrick et al., 1995).

In order to be able to conceptualize and specify the support requirements of cooperative work, we make an analytical distinction between 'cooperative work' and 'articulation work.' This distinction is fundamental to the approach presented here—and, in our opinion, to CSCW in general (Schmidt, 1994b). *Cooperative work* is constituted by the interdependence of multiple actors who, in their individual activities, in changing the state of their individual field of work, also change the state of the field of work of others and who thus interact through changing the state of a common field of work. Since it involves multiple actors, cooperative work is inherently distributed, not only in the usual sense that activities are distributed in

[2] A similar point was made very early in CSCW by Anatol Holt: 'Whatever has to do with task inter-dependence—*coordination*—is left to the users to manage as best they can, by means of shared databases, telephone calls, electronic mail, files to which multiple users have access, or whatever ad hoc means will serve.' (Holt, 1985).

time and space, but also—and more importantly—in the sense that actors are semi-autonomous in terms of the different circumstances they are faced with in their work as well as in terms of their strategies, heuristics, perspectives, goals, motives, etc. (Schmidt, 1991a, c). To deal with this source of confusion and disorder, individual and yet interdependent activities must be coordinated, scheduled, aligned, meshed, integrated, etc.—in short: articulated.[3] That is, the orderly accomplishment of cooperative work requires what has been termed *articulation work* (Strauss et al., 1985; Gerson and Star, 1986; Strauss, 1988, 1994).

Proposition 1. Cooperative work is constituted by the interdependence of multiple actors who interact through changing the state of a common field of work, whereas *articulation work* is constituted by the need to restrain the distributed nature of complexly interdependent activities.

The distinction between cooperative work and articulation work is *recursive*; that is, an established arrangement of articulating a cooperative effort may itself be subjected to a cooperative effort of re-arrangement which in turn also may need to be articulated, and so forth. To take a simple, and perhaps simplistic, example: At some point during a design meeting one of the participants interrupts the design discourse to change the agenda for the meeting. Following that, the participants discuss the proposal for some time, adopt it in an amended form, and resume the design discourse where they broke off. In this case, the established arrangement (the agenda) is treated as the field of work of another cooperative effort, namely that of rearranging the agenda. This recursion is, in principle, infinite. For instance, during the discussion about the proposed change to the agenda, someone may raise the issue of floor control by, say, proposing that nobody should be allowed to speak about the proposal more than once, which may in turn ignite yet another round of exchanges at another level of recursion. While this could go on forever, the infinite recursion is made finite and closed, to get the job done. The severe constraints under which work takes place in the real world dictate that such recursions are normally terminated well before they become frivolous.

Proposition 2. Articulation work is a *recursive* phenomenon in that the management of an established arrangement of articulating a cooperative effort may itself be conducted as a cooperative effort which, in turn, may also need to be articulated.

2 The Complexity of Articulation Work

Cooperative work is, as noted above, inherently *distributed.* This distributed character of cooperative work varies, however, according to the complexity of the interdependence, that is, depending on factors such the distribution of activities in time and space, the number of participants in the cooperative ensemble, the structural complexity posed by the field of work (interactions, heterogeneity), the degree

[3] The word 'articulate' is used in the sense of 'to put together by joints'.

and scope of specialization among participants, the apperceptive uncertainties posed by the field of work and hence the variety of heuristics involved, and so on. The more distributed the activities of a given cooperative work arrangement, the more complex the articulation of the activities of that arrangement is likely to be.

With low degrees of complexity, the articulation of cooperative work can be achieved by means of the modes of interaction of everyday social life. In fact, under such conditions, the required articulation of individual activities in cooperative work is managed *so* effectively and efficiently by our repertoire of intuitive interactional modalities that the *distributed* nature of cooperative work is not manifest. As demonstrated by the body of rich empirical studies of cooperative work within CSCW, actors tacitly monitor each other; they perform their activities in ways that support coworkers' awareness and understanding of their work; they take each others' past, present, and prospective activities into account in planning and conducting their own work; they gesture, talk, write to each other, and so on, and they mesh these interactional modalities dynamically and seamlessly (Harper et al., 1989b; Heath and Luff, 1992a; Harper and Hughes, 1993; Heath et al., 1995a).

However, in the complex work settings that characterize modern industrial, service, and administrative organizations (hundreds or thousands of actors engaged in myriads of complexly interdependent activities), the task of articulating the interdependent and yet distributed activities is of an order of complexity where our everyday social and communication skills are far from sufficient.

Faced with a high degree of complexity of articulation work, cooperating actors typically use a special category of artifacts which, in the context of a set of conventions and procedures, stipulate and mediate articulation work and thereby are instrumental in reducing its complexity and in alleviating the need for ad hoc communication. Consider, for example, the case of the S4000 project.

The S4000 project. Foss Electric is a Danish manufacturing company that produces advanced equipment for analytical measurement of quality parameters of agricultural products, e.g., the compositional quality of milk in terms of fat content and the count of protein, lactose, somatic cells, bacteria, etc. At the time of the field study,[4] the company was engaged in a large design project called S4000 which aimed at building a new instrument for analytical testing of raw milk. The S4000 project was the first project aiming at building an integrated instrument that would offer a range of functionalities that previously had been offered by a number of specialized instruments. In addition, as an innovation compared to previous models, the S4000 system would introduce measurements of new parameters in milk (e.g., urea and citric acid), and the performance was to be radically increased. The instrument would consist of approximately 8,000 components grouped into a number of functional units, such as cabinet, pipette unit, conveyer, flow-system, and measurement unit. Finally, the S4000 was the first Foss instrument to incorporate a personal computer (an Intel-based 486 PC) by means of which the configuration and operation

[4]The field research of the S4000 project was done by Henrik Borstrøm, Peter Carstensen, and Carsten Sørensen.

of the instrument were to be controlled (through a Windows interface). Eventually, the first version of the software consisted of approximately 200,000 lines of source code. Altogether more than 50 people were involved in the S4000 project, which lasted approximately 30 months (for the first version).

The design team was faced with quite a challenge:

(1) The different subsystems, e.g., the software control system and the mechanical and chemical processes in the flow and measurement system, were intricately interdependent and might interact in unforeseen ways.
(2) The S4000 project introduced measurement of new parameters in raw milk for which new technologies had to be developed and mastered.
(3) The different subsystems were developed concurrently and the requirements to be satisfied by each subsystem would therefore change as other subsystems were developed.
(4) Production facilities at the manufacturing plant were constantly changing as the use of existing machines was optimized and new machines and processes were introduced. Hence, the repertoire of manufacturing processes that the production function could offer to designers and that designers thus had to take into account in their decisions was continually changing.
(5) Because of its technological heterogeneity, the S4000 project involved a number of specialisms. The core design team consisted of designers from mechanical engineering, electronics, software, and chemistry. In addition, a handful of draught-persons and several persons from organizational entities such as production, model shop, marketing, quality assurance, quality control, service, and top management were involved to varying degrees at different stages in the course of the project.

All in all, the project was significantly more complex than previous projects at Foss.

To survive these challenges, the participants took a number of measures to reduce the complexity of managing the project:

As always at Foss, all project participants from the different technical departments were moved to the same office area to create a shared physical space by means of which participants could develop and maintain shared awareness of the state of the project. Furthermore, of course, a sequence of meetings was scheduled at different intervals and, as the project took its course, a large number of ad-hoc meetings were arranged as well.

However, the amount of detailed information that had to be communicated, aligned, negotiated, etc., required more robust measures. A number of procedures and artifacts were introduced to keep track of the state of affairs and to manage relations and dependencies among actors, tasks, and resources: an ‘augmented’ bill of materials that identified actors responsible for parts in order to support the coordination of mechanical design, process planning, and production in the construction of prototypes (Sørensen, 1994a); a CEDAC board (Cause and Effect Diagram with the Addition of Cards) for coordinating the diagnosis of faults between mechanical design and process planning (Sørensen, 1994b); and a product classification scheme supporting the distributed classification and retrieval of CAD models (Sørensen, 1994c). Some of these procedures and artifacts were invented for this project, some were redesigns of existing artifacts, and others were merely adopted.

The most dramatic measures were taken with respect to the software design process. In the early phases of the software strand of the S4000 project, the software designers felt that their overview of the state of the project was quite defective and that they needed much greater coordination. As one of the software designers put it:

> It has really been problematic that we did not have any guidelines and descriptions for how to produce and integrate our things. The individual designers are used to work on their own and have all the required information in their heads, and to organize the work as they wish to [. . .] When we started, we were only a few software designers. And suddenly — problems! And, oops, we were quite a few software designers and external consultants involved.

At the height of the crisis the software design goals were almost abandoned. To overcome the crisis, the software designers developed a repertoire of procedures and artifacts to ensure the monitoring and control of the integration of software components and modules.

An important component in this repertoire, was the 'software platform' institution. Initially, the 'software platform' was just a point in time at which all software designers would stop coding in order to integrate their bits and pieces. For each platform integration period, one of the designers was appointed as 'platform master', which implied that he or she would be responsible for collecting information on changes made to the software and for ensuring that the software was tested and corrected before it was released. Before the software was released as a 'platform' for further development, the project schedule was updated with revised plans and tasks for the next 3–6 weeks. The establishment of the software platform institution was considered absolutely necessary for the S4000 project.

Moreover, the software design team devised and introduced other procedures and artifacts. Most importantly, a 'bug report form' with corresponding procedures for reporting, classifying, and correcting faults were introduced to ensure that bugs were properly registered, that corrected bugs were duly reported, and to make the allocation of responsibilities clear and visible to all members. As a complementary measure, copies of bug forms were collected in a publicly available repository in the form of a simple binder. (For further details, cf. Carstensen et al., 1995a).

The software designers experienced the hard way that it was practically impossible to handle the distributed testing and bug registration activities of some twenty testers and designers without, *inter alia*, a bug report form and its associated procedures. By devising and introducing these constructs, they managed to alleviate the coordination crisis in the project.

The case of the S4000 project is particularly valuable because we here witness the introduction of specialized artifacts for coordination purposes in response to overwhelming problems encountered in coping with the complexities of articulating the distributed and interdependent activities of cooperative design under conditions that are typical for contemporary industry. However, while daunting to the participants, the complexity of the S4000 project is not exceptional. Such complexities are an everyday occurrence in modern industrial, service, and administrative settings.

Proposition 3. In cooperative work settings characterized by complex task interdependencies, the articulation of the distributed activities requires specialized artifacts which, in the context of a set of conventions and procedures, are instrumental in *reducing the complexity of articulation work* and in alleviating the need for *ad hoc* deliberation and negotiation.

Artifacts have been in use for coordination purposes in cooperative settings for centuries, of course—in the form of time tables, checklists, routing schemes, catalogues, classification schemes for large repositories, and so on. Now, given the infinite versatility of computer systems, it is our contention that such artifacts in the form of computational coordination mechanisms can provide a degree of perspicuity and flexibility to artifactually supported articulation work that was unthinkable with previous technologies, typically based on inscriptions on paper or cardboard. This opens up new prospects for moving the boundary of allocation of functionality between human and artifact with respect to articulation work so that much of the drudgery of articulation work (boring operations that have so far relied on human effort and vigilance) can be delegated to the artifact, but also, and more importantly, so that cooperative ensembles can articulate their distributed activities more effectively and with a higher degree of flexibility and so that they can tackle an even higher degree of complexity in the articulation of their distributed activities!

As a generalization, we call these artifacts and the concomitant procedures and conventions 'coordination mechanisms.'[5] In the following sections of this paper, we will expound this concept at length.

3 Coordination Mechanisms: Evidence and Concept

The concept of coordination mechanisms has been developed as a generalization of phenomena described in different ways in numerous empirical investigations of the use of artifacts for coordination purposes in different work domains:

- standard operating procedures in administrative work (Zimmerman, 1966, 1969a, b; Wynn, 1979; Suchman, 1983; Suchman and Wynn, 1984; Wynn, 1991);
- classification schemes for large repositories (Bowker and Star, 1991; Andersen, 1994a; Sørensen, 1994c);
- time tables in urban transport (Heath and Luff, 1992a);
- flight progress strips in air traffic control (Harper et al., 1989b; Harper and Hughes, 1993);

[5] In earlier papers we used the term 'mechanism of interaction'. The change of terminology does not imply any conceptual changes and is merely motivated by our experience that the term 'mechanism of interaction' can have unintended connotations.

- production control systems in manufacturing (Schmidt, 1994b);
- schedules in hospital work (Zerubavel, 1979; Egger and Wagner, 1993);
- planning tools for manufacturing design (Bucciarelli, 1988b; Sørensen, 1994a; Carstensen et al., 1995a);
- fault correction procedures in engineering and software design (Carstensen, 1994; Pycock and Sharrock, 1994; Pycock, 1994; Sørensen, 1994b).

Consider, for example, the bug form as described in the study of the Foss Electric S4000 project:

The bug report form. The bug report form (see Fig. 5.1) was a two page form (both sides of one sheet of paper). A new bug report was initiated and filled-in by anyone involved in testing the software, which could be software designers, other designers, quality assurance staff, or marketing people. The originator of the

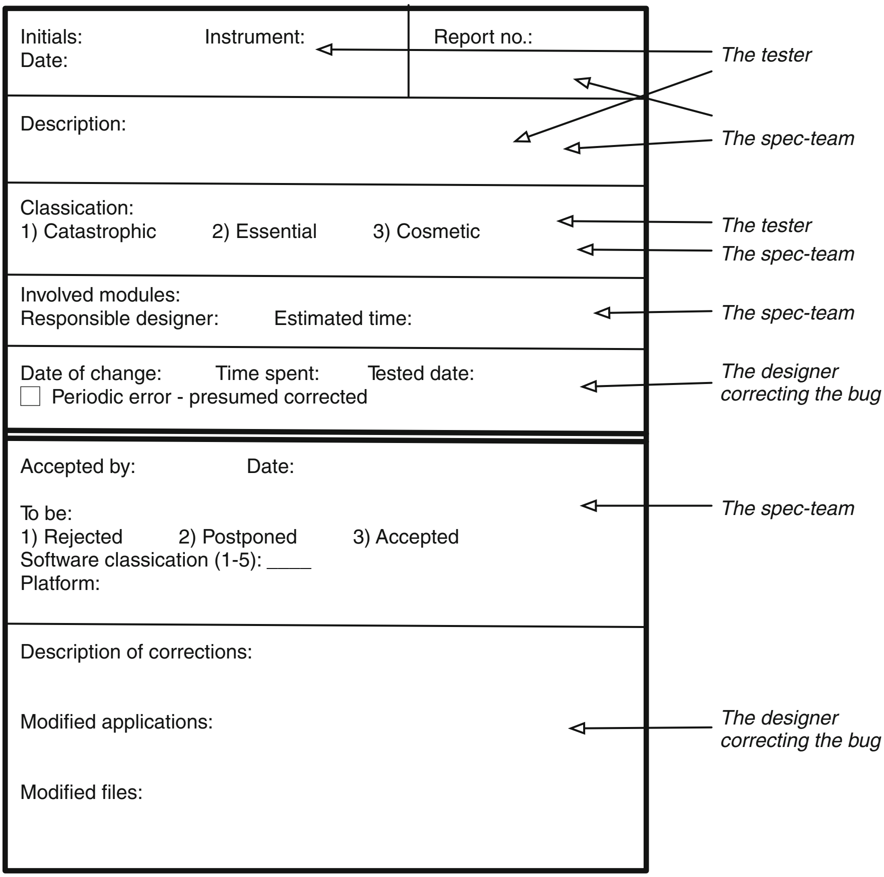

Fig. 5.1 The bug report form (translated from Danish), with indications of which actor (role) is supposed to fill in which fields (Carstensen et al., 1995b)

bug report also provided a preliminary description and diagnosis of the problem. A so-called ‘spec-team’, that is, a group of three software designers responsible for diagnosing and classifying all reported bugs, then determined the module which presumably was responsible for the fault and, by implication, identified the designer responsible for that module and therefore for correcting the bug. The ‘spec-team’ also specified the platform integration period by which the bug should be corrected, and classified the bug according to its perceived severity (as seen from a software reliability perspective). Finally, each designer was responsible for fixing the problems (handling ‘his’ or ‘her’ bugs) and reporting back to the Platform Master, i.e., the designer responsible for the next software module integration and verification period.

As noted above, all bug forms were filed in a public repository (‘the binder’) which was organized according to the following categories: (1) non-corrected ‘catastrophes’, (2) non-corrected ‘semi-serious’ bugs, (3) non-corrected ‘cosmetic’ bugs, (4) postponed, (5) rejected, (6) corrected but not yet tested, and (7) corrected bugs. The forms collected in the binder were successively re-classified and re-filed according to decisions made by the spec-team, messages concerning specific bugs from the designers, results from the verification of reported corrections, etc.

Finally, a list of not-yet-fixed bugs (category 1, 2, and 3) was produced continually and accessed by the software designers. It gave them an indication of the state of the software system as a whole and supported them in being aware of design activities concerning modules for which they were not responsible but with which their own modules might interact.

Basically, five different roles were involved in coordinating the debugging activities in the S4000 project: the testers testing the software, the ‘spec-team’ diagnosing the reported bugs, the software designers correcting the diagnosed bugs, the ‘platform master’ verifying the corrected bugs, and the designer maintaining the bug form repository to keep track of the distributed debugging activities (see Fig. 5.2).

As illustrated by the case of the bug report form, a coordination mechanism can be conceived of as constituted by two devices: On one hand we have a *coordinative protocol* in the form of a set of agreed-to procedures and conventions which, to competent members of the ensemble, stipulates the responsibilities of the different roles, the possible classifications of bugs, the intricate flow of forms, acknowledgments, reports of bugs corrected, etc. On the other hand we have the bug report form as *an artifact*, i.e., as a distinct and persistent symbolic construct, in which the protocol is imprinted and objectified.

Proposition 4. A coordination mechanism is a construct consisting of *a coordinative protocol* (an integrated set of procedures and conventions stipulating the articulation of interdependent distributed activities) on the one hand and on the other hand *an artifact* (a permanent symbolic construct) in which the protocol is objectified.

Coordination mechanisms are characterized by a specific and crucial relationship between protocol and artifact. In the next sections we will analyze this relationship by investigating the constituent parts in turn.

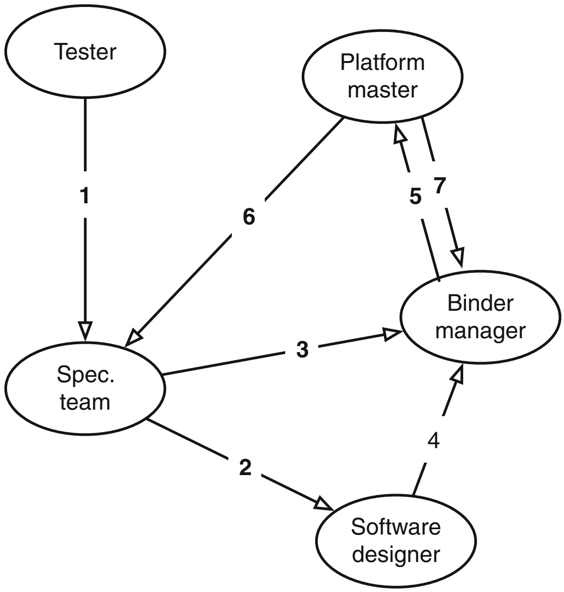

Fig. 5.2 A schematic illustration of the roles and information flows in software testing in the S4000 project. The flows in the diagram indicate the intended flow according to the bug handling protocol. The protocol follows seven major steps: (1) a tester sends a form, describing a bug, to the 'spec-team;' (2) the 'spec-team' diagnoses the bug and sends the annotated form to a software designer; (3) a copy is send to the manager of the 'binder', i.e., the repository of bug forms; if a reported bug is rejected, the original is sent to the 'binder' manager; (4) the software designer reports on corrected bugs and sends the form to the 'binder' manager; (5) the 'binder' manager sends a stack of forms of bugs to be verified to the 'platform master'; (6) forms of bugs that cannot be verified are sent to the 'spec-team'; (7) verified forms are sent to the 'binder' manager. (Carstensen et al., 1995b)

3.1 Coordination Mechanisms: The Protocol

While the notion of protocols that stipulate the articulation of cooperative work is crucial to the concept of coordination mechanisms, it is also contested. In a large body of sociological literature, the notion of pre-defined organizational constructs in general (formal structures, procedures, methods, plans) as determinants of action has been subjected to critical examination. Study after study have demonstrated, unambiguously and beyond any doubt, that the status of these formal organizational constructs in the actual course of work is problematic in that these constructs are impoverished idealizations when taken as representations of actually unfolding activities. In the words of Philip Selznick's classic summary of this line of sociological investigation:

> The formal administrative design can never adequately or fully reflect the concrete organization to which it refers, for the obvious reasons that no abstract plan or pattern can — or may, if it is to be useful — exhaustively describe an empirical totality. At the same time,

> that which is not included in the abstract design (as reflected, for example, in a staff-and-line organization chart) is vitally relevant to the maintenance and development of the formal system itself. (Selznick, 1948, p. 25)

This conception of the status of formal constructs has been highly influential in that it, as observed by Egon Bittner in a now classic paper, has 'furnished the necessary theoretical argument for an entire field of sociological investigations by directing attention to a sphere of adaptive and cooperative manipulations, and to the tensions typically found in it.' (Bittner, 1965, p. 240)

The issue—for Bittner and for us—is: what is the status of these formal organizational constructs? The problem with the received tradition of critical studies of formal organizational constructs, however, is the almost ceremonial status it implicitly ascribes to these formal constructs and the ensuing dichotomy of the 'formal' and the 'informal,' the notional and the corporeal. The argument implies that members of the given organizational settings are somehow supposed to take formal constructs literally—as if constructs such as procedural formulations are *supposed* to be exhaustive specifications of how the work gets done.

In addressing this problem, Bittner makes some very cogent observations:

> While Selznick quite clearly assigns the formal schemes to the domain of sociological data, he does not explore the full range of consequences out of this decision. By retaining Weber's conception of them as normative idealizations, *Selznick avoids having to consider what the constructions of rational conduct mean to, and how they are used by, persons who have to live with them from day to day. It could be, however, that the rational schemes appear as unrealistic normative idealizations only when one considers them literally, i.e., without considering some tacit background assumptions that bureaucrats take for granted*. (Bittner, 1965, p. 242—emphasis added)

Bittner's methodological recommendation is quite pertinent to the issue of analyzing and designing coordination mechanisms. To be able to contribute constructively to the design of computational coordination mechanisms, we need to understand not only 'the tacit background assumptions' that members take for granted and without which any formal construct would be merely a rhetorical statement but also 'what the constructions of rational conduct mean to, and how they are used by, persons who have to live with them from day to day.'

In the course of the following exposition and discussion, it is important to keep in mind that we are not trying to address or solve the general problems of general sociological theory. We are not investigating human action in general, merely the means of articulating distributed and interdependent activities *in work settings*, that is, under conditions of severe constraints. Nor are we investigating the nature of tacit and implicit plans, rules, routines, habits, and so on in human social conduct in general but, more specifically and modestly, the role of *artifactually imprinted protocols* in the articulation of cooperative work.

However, since it is not merely the status of coordinative protocols that is contested but the status of protocols in general, and since much of the evidence produced for this discussion accordingly does not specifically refer to protocols designed and used to support the articulation of cooperative work but to formal organizational constructs in general, we cannot, in our investigation, avoid to refer to and draw upon evidence of artifactually imprinted protocols that are not used in the *articulation* of cooperative work. In doing that, our objective remains, however, to understand the more specific problem of the role of artifactually imprinted protocols in the articulation of cooperative work. To make this explicit we will use the term ‘artifactually imprinted protocol’ to denote any protocol, coordinative or otherwise, which is objectified in an artifact.

During the last 15 years or so, our understanding of how procedures and artifactually imprinted protocols are used by actors in everyday work activities has been greatly enriched by a number of outstanding studies (e.g., Wynn, 1979; Suchman, 1983; Suchman and Wynn, 1984; Bucciarelli, 1988b; Wynn, 1991). The general conclusion of these studies is that such procedures and artifacts serve as ‘maps’ (Suchman, 1987, p. 188 f.; Bucciarelli, 1988b, p. 114). Consider, for instance, Suchman’s study of the accounting office (Suchman, 1983):

The accounting office. This office was responsible for the orderly payment of money due to outside organizations supplying goods and services to the organizational units in its charge. Orderly payment was documented through record-keeping, and accuracy was monitored by the auditing of invoices against records of requisition and receipt. According to the standard procedure, items on a given purchase order could be received and billed in separate installments over an extended period. Again, if all went smoothly, the items marked off on the receiving report from Shipping or Receiving would correspond to those on the invoice from the vendor. The purchase order, receiver, and invoice would be matched and audited. The payment for the items received would be recorded by margin notes on the purchase order, which would then be returned to the temporary file to wait for the next shipment and billing. Only after all bills had been received and paid was the completed purchase order filed permanently in the paid file.

In the case presented and analyzed by Suchman, however, the record of what had happened was incomplete: The original purchase order was missing. A completed receiving document was found with eight items listed on it, all of which had been marked as received. The two invoices found in the paid file showed only two items as paid, however; there was no invoice or record of payment for the other items, yet the vendor reported that the transaction would be completed with payment of the past due invoice for only two of those items that seemingly had not yet been paid. The study then shows how the two actors, the accounting clerk and the auditing clerk, step by step solved the ‘mystery’: Of the invoice for one of the items, only page two was on file; page one was missing. It thus transpired that four other items were invoiced with this item and had already been paid.

This case shows convincingly that orderly records are not necessarily the result of some prescribed sequence of steps and that it may involve the practice of completing a record or pieces of it after the fact of actions taken: ‘once the legitimate history of the past due invoice is established, payment is made by acting as though the record were complete and then filling in the documentation where necessary. The practice of completing a record or pieces of it after the fact of actions taken is central to the work of record-keeping’ (Suchman, 1983, p. 326). Thus, precisely because it is a case of recovery from error, the case gives a vivid impression of the massive heuristic use of standard procedures even in a seemingly abnormal situation. The two actors are able to solve the abnormal problem because of their 'knowledge of the accounts payable procedure' (Suchman, 1983, p. 322). Standard procedures can thus be said to have a heuristic function in the sense that they 'are formulated in the interest of what things should come to, and not necessarily how they should arrive there' (Suchman, 1983, p. 327).

Taking this interpretation further, Suchman posits that a standard procedure serves as an extraneous and subservient referent for situated action:

> It is the assembly of orderly records out of the practical contingencies of actual cases that produces evidence of action in accordance with routine procedure. This is not to say that workers "fake" the appearance of orderliness in the records. Rather, it is the orderliness that they construct in the record that constitutes accountability to the office procedures. (Suchman, 1983, p. 327)

This interpretation is generalized in Suchman's seminal book on *Plans and Situated Action* (1987):

> plans are resources for situated action, but do not in any strong sense determine its course. While plans presuppose the embodied practices and changing circumstances of situated action, the efficiency of plans as representations comes precisely from the fact that they do not represent those practices and circumstances in all of their concrete detail. (Suchman, 1987, p. 52)

Suchman's thesis that 'plans are resources for situated action' is of fundamental importance to CSCW systems design and has served us as a guiding principle in the development of the concept of coordination mechanisms, but it also leaves a number of unsettling questions unanswered: What is it that makes plans such as production schedules, office procedures, classification schemes, etc., useful in the first place? What makes them 'resources'? Furthermore, is it merely the fact that plans are underspecified in comparison with the rich multiplicity of actual action that makes them 'resources'? Is that really all there is to it?[6] What, then, makes one procedure or form or schedule more useful than another for a certain purpose in a specific setting?

Later in the book, Suchman returns to these issues and suggests a rather apt metaphor for the role of artifactually imprinted protocols, namely that of a 'map':

> Just as it would seem absurd to claim that a map in some strong sense controlled the traveler's movements through the world, it is wrong to imagine plans as controlling actions. On the other hand, the question of how a map is produced for specific purposes, how in any actual instance it is interpreted *vis-à-vis* the world, and how its use is a resource for traversing the world, is a reasonable and productive one. (Suchman, 1987, pp. 188 f.)

While the same irksome questions arise here as well, the 'map' analogy is a fitting condensation of the role of artifactually imprinted protocols that have been described in a number of studies. In Suchman's study of the accounting office, for

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[6]Is it indeed '*precisely* [. . .] the fact that they *do not* represent those practices and circumstances in all of their concrete detail' that makes plans efficient and effective? Does that mean that the less specific the better? Suchman probably does not intend to imply that.

example, the standard operating procedures were found to be 'formulated in the interest of what things should come to, and not necessarily how they should arrive there.' They were used as a general reference for orientation purposes, not as a prescribed sequence of actions to be taken.

However, other studies lead to quite different conclusions as to how artifactually imprinted protocols are used by actors in everyday work activities.

Checklists. Firstly, consider the relatively simple case of the 'normal checklist.' The checklist is an artifactually imprinted protocol that has been deliberately and carefully designed to *reduce* local control in safety-critical environments. More specifically, a checklist is used to organize tasks whenever it is essential that a set of actions *all* be performed, typically where it is essential that the actions of the performance also be taken in a particular order, to ensure a high level of operational safety. For example, the normal aircraft flight-deck checklist indicates a set of different tasks the pilot must perform or verify during all flight segments in order to configure the aircraft and prepare the flight crew for certain 'macro-tasks' such as ENGINE START, TAXI, TAKEOFF, APPROACH, LANDING, etc. For each one of these macro-tasks there are several 'items' to be accomplished and verified by the crew (Degani and Wiener, 1990).

In his analysis of the checklist, Don Norman observes that 'The fact that the preparation of the list is done prior to the action has an important impact upon performance because it allows the cognitive effort to be distributed across time and people' (1991, p. 21). This preparatory task—which Hutchins aptly has dubbed 'precomputation'—can be done when more convenient, e.g., when there is no time pressure and no safety and security risk, and by another actor, e.g., by a specialist. 'In fact,' Norman observes, 'precomputation can take place years before the actual event and one precomputation can serve many applications' (1991, p. 21). The concept of a precomputation of essential aspects of a task is crucial to understanding the role of artifactually imprinted protocols: The flight-deck checklist, for instance, provides a precomputed selection of safety-critical tasks, which all need to be performed at the particular flight segment as well as a precomputed sequence for their execution.

Now, the protocol of the flight-deck checklist does not stipulate the *articulation* of cooperative activities but the activities themselves and it is therefore not a coordinative protocol. For a case of the use of artifactually imprinted protocols that stipulate the articulation of cooperative activities, consider the *kanban* system.

The kanban system. In 1990, Bjarne Kaavé conducted a study of cooperative production control in a manufacturing company we can call Repro Equipment.[7] The company manufactured specialized optical appliances and covered about 50% of the world market for this category of equipment. At the time of the study, the company produced about 6,000 units a year in 15 models, each in seven variants.

[7] This analysis is based on Bjarne Kaavé's findings as reported in his thesis (Kaavé, 1990) as well as in several joint analysis sessions with one of the present authors.

A manufacturing operation, like the one at Repro Equipment, involves multitude discrete parts and processes that are complexly interdependent: Each product consists of many component parts, in some cases tens or hundreds of thousands of components, and their production may require a number of different processes in a specific sequence. Different processes, such as cutting, bending, welding, etc., typically require specialized tools and skills which are distributed at different workstations and require hugely different set-up times. This is compounded by the fact that, at any given time, a large number of products and their components coexist in the production process at different stages of completion, which means that different parts for the same or for different products compete for the same workstations. Thus, in the words of Harrington (1984, p. 4), manufacturing can be conceived of as 'an indivisible, monolithic activity, incredibly diverse and complex in its fine detail. The many parts are inextricably interdependent and interconnected.' Accordingly, for a manufacturing enterprise to be able to adapt to changing conditions, the entire enterprise must react 'simultaneously and cooperatively' (Harrington, 1979, p. 35).

To deal with this complexity, Repro Equipment had introduced a *kanban* system to coordinate processes in the production of cabinets. *Kanban* is a Japanese word for 'card' or more literally 'visible record' (Schonberger, 1982, p. 219) and it is now in widespread use in manufacturing to denote a just-in-time production control system where a set of cards acts as the carrier of information about the state of affairs *as well as* production orders conveying instructions to initiate certain activities. The basic idea is that loosely interdependent production processes can be coordinated by exchanging cards between processes. When a new batch of parts or sub-assemblies has been produced and the batch is to be transported 'down-stream' from the present work station to the station where it is to be used, for instance, as components for a sub-assembly, a specific card is attached to the container used for the transportation. When the operator at the work station down-stream has processed this batch of parts, the accompanying card is sent back to the operator who produces these parts. To the operator, receiving the card means that he or she has now been issued a production order.

The basic set of rules of a *kanban* protocol is as follows (Schonberger, 1982, p. 224):

(1) No part may be made unless there is a *kanban* authorizing it.
(2) There is precisely one card for each container.
(3) The number of containers per part number in the system is carefully calculated.
(4) Only standard containers may be used.
(5) Containers are always filled with the prescribed quantity — no more, no less.

Setting up a *kanban* system requires a careful configuration of the number of containers per part number and the quantity per container. This configuration, in effect, amounts to a precomputation of tasks in terms of batch size per part number, task allocation in terms of work stations for different part numbers, and task sequences.

However, a *kanban* system is not adequate for coordinating manufacturing operations faced with severe demands on flexibility of volume: a *kanban* system can only handle small deviations in the demand for the end product (Schonberger, 1982, p. 227; Monden, 1983). Accordingly, since Repro Equipment was faced with extreme differences and fluctuations in demand for different models and variants, operators recurrently experienced that the configuration of the *kanban* system (the number of containers per part number and the quantity per container) was inadequate. For instance, in a situation where a particular part number that was only used for a special product variant had all been used, the protocol would automatically generate a production order for this part number, although the part number in question probably would not be needed in months, and would thereby absorb production facilities that would be needed for other, more pressing orders.

In such situations, where the *kanban* system is 'beyond its bounds' (Roth and Woods, 1989), operators at Repro Equipment would tamper with the *kanban* protocol. For example, having heard of a new rush order from the girl in the order office, the fork lift operator might put the card for a rarely used part for another model in his back pocket or leave it on the fork-lift truck for a while. Similarly, in order to rush an order, operators would occasionally order a new batch of parts for this order *before* the container had actually been emptied and the card had been released, or they would deviate from batch sizes as specified on the card, etc.

It is crucial to notice that instead of abandoning the *kanban* system altogether, or at least temporarily, the operators *changed the configuration* of the system. That is, when an operator pocketed a *kanban*, he or she was *modifying* the protocol, not switching it off, and when the card was put back in circulation (or released belatedly), the default configuration was in force again. The reason for this is that the *kanban* system incorporates (implicitly, in the configuration of the system) a precomputed model of crucial interdependencies of the manufacturing process (routing scheme, set-up-times, etc.). Thus, even though the *kanban* system at Repro was often used in situations where it was 'beyond its bounds,' it was not discarded but merely modified locally and temporarily according to the requirements of the situation.

In order to be usable in a setting like Repro Equipment, the *kanban* system had to be managed (monitored, adapted, modified) continually. This was facilitated by the formation of a network of clerks, planners, operators, fork-lift drivers, and foremen in various functions such as purchasing, sales, production, shipping, etc., who kept each other informed about the state of affairs to be able to control the flow of parts. A member of this network would for example explore the state of affairs 'upstream' to be able to anticipate contingencies and, in case of disturbances that might have repercussions 'down-stream,' issue warnings. That is, the indirect, dumb, and formal *kanban* mechanism was subsumed under a very direct, intelligent, and informal cooperative arrangement. The cooperative ensemble 'appropriated' the *kanban* system in order to increase its flexibility. They took over control of the system and controlled production far more closely and effectively than warranted by the design of the *kanban* system.

Coming back to the issue of the status of formal organizational constructs in cooperative work, the *kanban* system illuminates several important points.

Suchman's contention that the function of abstract representations such as plans 'is not to serve as specifications for the local interactions, but rather to orient or position us in a way that will allow us, through local interactions, to exploit some contingencies of our environment, and to avoid others' (Suchman, 1987, p. 188) is not correct as far as the *kanban* system is concerned. When an operator receives a card, he or she will produce the batch as specified by the card, in accordance with the general rules of the protocol, *without actively searching for reasons not to do so* and without deliberating or negotiating whether to do so or not.

In their individual activities, actors rely on the *kanban* system to issue valid and sensible production orders, unless they have strong reasons to believe that its unmitigated execution in the particular situation at hand will have undesirable results. Even then, they do not discard the system but alter its behavior by reconfiguring it, after which the system is allowed to 'switch back' to the default configuration. That is, in the case of the *kanban* system:

- actors coordinate their distributed activities by *executing* the *kanban* protocol—*unless* they have strong reasons to act otherwise;
- when actors have reasons to doubt the rationality of executing a production order issued by the system, they temporarily reconfigure the system, i.e., respecify the protocol, by withholding cards or introducing false cards;
- by reconfiguring the system, actors do not discard the system but alter its behavior temporarily, upon which the system is allowed to 'switch back' to its default configuration.

The *kanban* system thus determines action in a far stronger sense than the map of a traveller determines the traveller's movements (Suchman, 1987, p. 188 f.; Bucciarelli, 1988b, p. 114). In the *kanban* case the protocol conveys a *specific* stipulation in the form of a production order to the particular actor instructing the actor, under the conditions of social accountability, to take the particular actions specified by the card according to the general rules of interpretation laid down in the protocol. It is thus more like a *script* than a *map*. In fact, the *kanban* system works well even though it does not provide a 'map' in the form of an overview of interdependencies among processes.

The point is that the *kanban* protocol under normal conditions of operation relieves actors of the otherwise forbidding task of computing myriad—partly interdependent, partly competing—production orders and negotiating their priority. They can, for all practical purposes, rely on the precomputed protocol to issue valid production orders; they take it for granted. Thus, for an actor in Repro Equipment to question the rationality of the protocol at every step in every situation would be an utter waste of effort, and it does not happen.

As a generalization, we find that a protocol stipulates the articulation of distributed activities by conveying affordances and constraints to the individual actor which the actor, as a competent member of the particular ensemble, can apply without further contemplation and deliberation unless he or she, again as a competent member, has accountable reasons not to do so. That is, actors deviate from the stipulations of the protocol if and when they have compelling reasons to do so, and only then.[8]

Proposition 5. A coordinative protocol is a resource for situated action in that it reduces the complexity of articulating cooperative work by providing *a precomputation of task interdependencies* which actors, for all practical purposes, can rely on to *reduce the space of possibilities* by identifying a valid and yet limited set of options for coordinative action in any given situation.

As demonstrated by the conflicting findings from different cases, artifactually imprinted protocols, such as plans, conventions, procedures, and so forth, play different roles in cooperative work. They may, on one hand, play the 'weak' role of the 'map' of the traveller by providing a codified set of functional requirements which provides a general heuristic framework for distributed decision making. On the other hand, they may play the 'strong' role of a 'script' that offers a 'precomputation' of interdependencies among activities (options, sequential constraints, temporal constraints, etc.) which, for each step, provides instructions to actors of possible or required next steps. Which role is appropriate naturally depends on the extent to which it is possible to identify, analyze, and model interdependencies in advance.

Moreover, the role of a particular protocol may vary according to the situation. Thus, in a situation where a standard operating procedure does not apply, the procedure may merely serve in its weak default capacity as a vehicle of conveying heuristics (as, for instance, in the accounting office). In other cases, however, such as the *kanban* case, the role of the protocol does not vary in the face of contingencies; rather, because of the complexity of the interdependencies of discrete parts production, the *kanban* protocol was not discarded, suspended, or 'weakened' but temporarily respecified (reconfigured) by operators to accommodate the passing disturbance.

Proposition 6. The role of coordinative protocols varies from case to case and from situation to situation, according to the fitness and expressive power of the precomputation of interdependencies as represented by the protocol, from weak stipulations, as exemplified by 'a map,' to strong stipulations, exemplified by 'a script.'

However, whether weak or strong, a protocol only conveys stipulations within a certain social context, within a certain community, in which it has a (more or less) certain and agreed-to meaning and it only does so under conditions of social accountability.

Moreover, as pointed out by Suchman with regard to office procedures, protocols are characterized by 'the inherent and necessary under-specification of procedures

[8] Even the simple checklist reduces the semantic distance for its users. Lacking the checklist, the novice must discover the steps that need to be done and an order in which they can be applied. With the checklist, the task is transformed: reading and following instructions take the place of procedural reasoning. (Norman and Hutchins, 1988, p. 15)

with respect to the circumstances of particular cases' (Suchman, 1982, p. 411). Furthermore, Suchman observes, 'the vagueness of plans is not a fault, but is ideally suited to the fact that the detail of intent and action must be contingent on the circumstantial and interactional particulars of actual situations' (Suchman, 1987, pp. 185 f.). However, the degree of vagueness of specific plans is itself contingent:

> While plans *can* be elaborated indefinitely, they elaborate actions just to the level that elaboration is useful; they are vague with respect to the details of action precisely at the level at which it makes sense to forego abstract representation, and rely on the availability of a particular embodied response. (Suchman, 1987, p. 188)

Thus, it is not only that a protocol, as a linguistic construct (Suchman, 1987, p. 186), is inherently vague compared to the rich details of the actually unfolding activities of the cooperative work arrangement in which it is applied, nor is it only that a protocol is inherently decontextualized, but a protocol is deliberately under-specified with respect to (a) factors that are immaterial for the purpose of the given protocol or (b) factors that can more efficiently and effectively be left unspecified, typically until a later stage. The protocol must, to use the apt phrase of Bowker and Star, be defined at 'an appropriate level of ambiguity' (Bowker and Star, 1991, p. 77).

Proposition 7. As a preconceived plan for the articulation of the distributed activities of a specific cooperative work arrangement, *a coordinative protocol is inexorably under-specified* in the sense that the *nominal* preconception cannot encompass and denote the infinite multiplicity of *actual* circumstances and occurrences unfolding during its situated enactment.

Thus, whether a protocol is weak or strong, its execution involves an unavoidable aspect of situated interpretation and improvisation—which, nonetheless, as in the case of protocols used as scripts, may be inconsequential to competent members.

On the other hand, whether weak or strong, the protocol will, inevitably, encounter situations where it is beyond its bounds, its inherent vagueness and appropriate ambiguity notwithstanding. This is eloquently illustrated by the case of the *kanban* system. Similarly, the software designers intermittently experienced situations where the bug form protocol they had devised and adopted did not seem to provide adequate stipulations and where the execution of the bug form protocol was modified accordingly. For example, testers would occasionally inform a software designer directly of a detected bug, without filling-in a bug report form and initiating a new instance of the protocol.

Proposition 8. Whether weak or strong, *a coordinative protocol will, inevitably, encounter situations where it is beyond its bounds* and where actors therefore must deviate from or circumvent the execution of the protocol.

Let us now turn to the role of the artifact in coordination mechanisms.

3.2 Coordination Mechanisms: The Artifact

The role of the artifact in coordination mechanisms is, fundamentally, to objectify and give permanence to the protocol for which it stands proxy. The artifact conveys the stipulations of the protocol in a situation-independent manner. So far, the artifact is conceived of merely as a written record of the protocol, e.g., as a standard operating procedure.

While written language, as observed by Jack Goody, 'is partly cut off from the context that face-to-face communication gives to speech, a context that uses multiple channels, not only the purely linguistic one, and which is therefore more contextualized, less abstract, less formal, in content as in form.' (Goody, 1987, p. 287), written records (log books, recordings, minutes, memos, etc.) provide persistence to decisions and commitments made in the course of articulation work: 'The written language [reaches] back in time' (Goody, 1987, p. 280). Written records are, in principle, accessible to any member of the ensemble, whatever its size and distribution in time and space. In the words of Stinchcombe, 'Written systems can provide a larger number of people with the same information at one time' and written messages are 'portable, allowing interaction without spatial constraints.' On the other hand, written systems 'are much less dependent on physical arrangements' and 'less time-dependent than oral systems.' (Stinchcombe, 1974, pp. 50 f.). Written artifacts can at any time be mobilized as a referential for clarifying ambiguities and settling disputes: 'while interpretations vary, the word itself remains as it always was' (Goody, 1986, p. 6). They are, for all practical purposes, unceasingly publicly accessible.

Proposition 9. The role of the artifact in a coordination mechanism is fundamentally *to objectify and give permanence to the coordinative protocol* so that its stipulations are unceasingly publicly accessible.

Consider, for example, a standard operating procedure or a checklist. The state of the artifact is completely static irrespective of the state of the execution of the protocol it prescribes. Even when the artifactually imprinted protocol is used as a script (actors are following the instructions of the procedure or the items of the checklist step by step), it is entirely up to the actor to produce and maintain the required dynamic representation of the state of the protocol with respect to the unfolding cooperative activities.

In the case of the bug report, however, *the state of the artifact changes* according to the changing state of the protocol. Firstly, the form is transferred from one actor to another and this *change of location* of the artifact in itself conveys, to the recipient, the stipulations of the protocol in a specified form, that is, the change of location transfers to the particular actor the specific responsibility of taking such actions on this particular bug that are appropriate according to the agreed-to protocol and other taken-for-granted conventions. Secondly, at each step in the execution of the protocol, *the form is annotated* and the thereby updated form retains and conveys this change to the state of the protocol to the other actors—the state of each reported bug is thus reflected in the successive inscriptions on the form made by different actors. That is, a change to the state of the protocol induced by one actor (a tester reporting a bug, for example) is conveyed to other actors by means of a visible and durable change to the artifact. In so far, the artifact can be said to provide a 'shared space,' a space with a particular structure that reflects salient features of the protocol. Furthermore, this change is propagated within the ensemble according to the stipulations of the protocol, and the state of the total population of reported bugs is publicly visible in the public repository of bug forms ('the binder').

Similarly, in the case of the *kanban* mechanism, the artifact mediates articulation work in the sense that the change of location of a card, that is, the fact that it is transferred 'up-stream' from one actor to another, is equivalent to the arrival of a production order at that work station. However, as opposed to the bug report, the inscription on the *kanban* card is not changed and the state of the *kanban* protocol is thus not reflected in any particular card. Hence, state changes to the protocol under execution can not be inferred from the inscription on the cards, only from their location.

In these cases, the artifact not only stipulates articulation work (like a standard operating procedure) but *mediates* articulation work as well in the sense that the artifact acts as an intermediary between actors that conveys information about state changes to the protocol under execution.[9] By serving the dual function of stipulating and mediating articulation work, the artifact is instrumental in reducing the complexity of articulating a vast number of interdependent and yet distributed and perhaps concurrently performed activities.

Proposition 10. The artifact of a coordination mechanism may, in some form and at a particular level of granularity, *dynamically represent the state of the execution of the protocol* and may thereby, among actors, mediate information about state changes to the protocol as it is being executed.

By virtue of the artifact's mediation of the changing state of the protocol between actors, the coordination mechanism not only conveys the general stipulations of the protocol but *specifies the stipulations* in the sense that the individual actor is instructed that it is he or she that has to take this or that specific action at this particular point in time. In other words, by representing and conveying the changing state of the protocol, the artifact also mediates the transition from 'nominal' to 'actual' in the enactment of the protocol.

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[9] Edwin Hutchins has a related but not identical analysis of artifactually imprinted protocols. In his discussion, Hutchins suggests the term 'mediating structures' for artifacts that are not part of the field of work (as tools are) and yet are instrumental in reducing the complexity of work by providing some kinds of constraints to the conduct of the actor (Hutchins, 1986, p. 47). For Hutchins, the artifact or structure serves as an intermediary between an actor planning or defining the protocol for an activity and the actor performing the activity, whereas we, for our purposes, reserve the term 'mediate' to denote an artifact serving as an intermediary of *horizontal* propagation of state changes to the protocol, i.e., *within* the cooperative work arrangement at hand. In other words, coordination mechanisms can be conceived of as a special case of 'mediating structures', namely artifactually embodied 'mediating structures' that are used to *constrain the articulation of distributed activities* in cooperative work settings.

Proposition 11. By mediating the changing state of the protocol, the artifact of a coordination mechanism *specifies* the general stipulations of the protocol.

To provide permanence to the coordinative protocol and serve as a mediator, the artifact upon which the protocol is imprinted must be *distinct from* the field of work (Proposition 1). An artifact may, of course, be subjected to all sorts of unforeseen use and an artifact that is involved in the transformation processes of the work may at the same time be used for coordinative purposes, perhaps to support a coordinative protocol. For example, actors writing a joint report may have adopted a convention according to which their coordinative interactions (comments to the evolving text as well as records of responsibilities, for example) be conveyed in and through the text of the report. That is perfectly feasible, and may indeed be very effective, but what happens, for instance, to the records of responsibilities and schedules when the text of the report is changed, for instance reorganized? They may have vanished as 'the snows of yesteryear,' to abuse the words of François Villon. That is to say, artifacts that are part of the field of work or coupled to the state of the field of work may be unreliable and treacherous as a material carrier of a coordination mechanism.

Proposition 12. The artifact of a coordination mechanism is *distinct from* the field of work in the sense that changes to the state of the field of work are not automatically reflected in the state of the artifact and, conversely, changes to the state of the artifact are not automatically reflected in the state of the field of work.

Moreover, due to its mediating role with respect to the state of the execution of the protocol, the artifact may support the development and maintenance of mutual awareness among the actors within the cooperating ensemble. By reflecting the state of the execution of the protocol, the artifact may convey information about occurrences within the ensemble from which actors can make inferences about likely or possible problems and develop an overview of the state of the protocol in its totality. This potential is clearly illustrated by the case of software testing, especially due to the successive inscriptions on the bug forms and the systematic assembly of (copies of) bug forms in 'the binder.' The *kanban* system, on the other hand, does not provide a facility for obtaining such an overview. In fact, the information conveyed by the transfer of cards up-stream is drastically filtered and distorted by the successive translations from card to card. The only interface to the state of the protocol across the total population of cards in circulation is the (ever changing) *location* of the myriad cards in the distributed manufacturing system. That is, the *kanban* system does not provide facilities allowing actors to develop and maintain a mutual awareness so as to, for instance, anticipate disturbances and obtain an overview of the situation within the cooperative ensemble at large; they are, so to speak, enveloped by an overwhelming and inscrutable quasi-automatic coordination mechanism.—In fact, in the *kanban* system, changes to the state of the artifact are strongly coupled to state changes in the field of work. Information only propagates 'up-stream' as parts are used down-stream: the speed and pattern of propagation of information are thus restricted by the rate and pattern of changes to the field of work at large. The *kanban* system does allow operators to control the execution of the protocol, however, since that control is ultimately in the hands of the operators: it is the operator who has used the parts in a particular container who takes the card and sends it up-stream; it is the truck driver who delivers it; it is the operator further up-stream who receives the card and decides to act on it. That is, due to the operators' control of the execution of the *kanban* protocol, the direct coupling of the *kanban* system to the field of work can be severed whenever they deem it appropriate to exercise that control.

An artifact is, of course, more than a permanent symbolic construct; it has a specific material format which, in itself, is of importance to its use. For example, consider the simple checklist again. The checklist can be conceived of as an artifactually imprinted protocol that has been deliberately and carefully designed to reduce local control, typically in safety-critical environments. The use of the checklist requires the actor to employ a strategy for sequential execution which permits him or her to ensure that the steps are done in the correct order and that each step is done once and only once. The material format of the checklist as an artifact may be of assistance to the actor in ensuring this:

> The fixed linear structure of the checklist permits the user to accomplish this by simply keeping track of an index that indicates the first unexecuted (or last executed) item. Real checklists often provide additional features to aid in the maintenance of this index: boxes to tick when steps are completed, a window that moves across the checklist, etc. (Hutchins, 1986, pp. 47 f.; cf. also Norman and Hutchins, 1988, p. 9)

In a similar vein, Jack Goody, in a discussion of the specific affordances provided by the *material format* of written text, observes that writing introduces certain spatio-graphic devices such as lists, tables, matrices by means of which linguistic items can be organized in abstraction from the context of the sentence (Goody, 1987) and points out that the spatio-graphic format of an artifact can stipulate behavior by reminding an actor of items to do and directing attention to missing items: 'The table abhors a vacuum' (Goody, 1987, p. 276). This is, again, eloquently illustrated in the case of the bug form where the bug report form provides a set of fields which match crucial points of the bug handling protocol and which are to be filled in by the different actors in the course of the bug's life (cf. Fig. 5.1).

Proposition 13. By reflecting salient features of the protocol, the *material format* of the artifact conveys coordinative stipulations and may provide a 'shared space,' structured accordingly, for mediating changes to the state of the protocol in compliance with the protocol.

By way of concluding this discussion, it is important to keep in mind that an artifact only conveys stipulations within a certain social context, within a certain community, in which the protocol and any change to the state of the protocol have a (more or less) certain and agreed-to meaning and that it only does so under conditions of social accountability. The point we want to make here, however, is that the specific structural and behavioral properties of the artifact (its material format as well as its protocol, if such have been incorporated in the artifact) are formed to serve the purpose of conveying specific stipulations within this particular context by constraining and forcing the actors' behavior.

We can summarize our analysis of the constituent parts of the protocol-cum-artifact dyad, by attempting a formal definition of coordination mechanisms:

Proposition 14. A *coordination mechanism* is a specific organizational construct, consisting of a *coordinative protocol* imprinted upon a *distinct artifact*, which, in the context of a certain cooperative work arrangement, *stipulates* and *mediates* the articulation of cooperative work so as to *reduce the complexity of articulation work* of that arrangement.

3.3 Coordination Mechanisms: Alignment

Consider, again, the case of software testing in the S4000 project. As observed previously, in addition to the bug report mechanism, the software designers introduced and used a variety of protocol-cum-artifact dyads to handle the complexity of coordinating the distributed activities of software testing, each of them devised to serve specific purposes in the setting. However, the different protocols intersect and must therefore be aligned somehow by the actors.

For example, a project schedule in the form of a spreadsheet was used to capture and display the relationships between actors, responsibilities, tasks, and schedules. In handling bug reports, participants would consult the project schedule to obtain information about who would be responsible, as 'platform master,' for verifying the corrected bug; this was indicated in the bug report form by the number of the platform period. Similarly, an integration period number inscribed in the bug report form also indicated a deadline for the correction task to be finished. Neither the name of the platform master nor the deadline were explicitly stated but could be derived from the project schedule, a spreadsheet where the name of the platform master and the date for that integration period number would be inscribed, at some point. That is to say, from the point of view of the involved coordination mechanisms, one coordination mechanism (the bug report mechanism) subscribed to the specification of a role and a date to be provided by another mechanism (the project schedule). Thus, an array of multiple protocols-cum-artifacts that intersect at various points makes it possible to instantiate a particular protocol (e.g., a particular bug report) while it is still not completely specified; the missing specifications can be 'filled-in' later by consulting another artifact. Thus actors do not need to specify explicitly what can be inferred from other mechanisms at some point in time.

The case also demonstrates a more active form of alignment between protocols, namely in the form of one protocol inciting the execution of another when a certain condition occurs (see Fig. 5.3). For example, when a reported bug was accepted as a bug, a new task was announced and inscribed in the project schedule. That is, the change of the state of one mechanism (the bug report) instructed actors to make certain inscriptions on the artifact of another mechanism (the project schedule). Similarly, when a bug was reported to have been corrected, yet another task were to be announced, namely the task of verifying the correction.

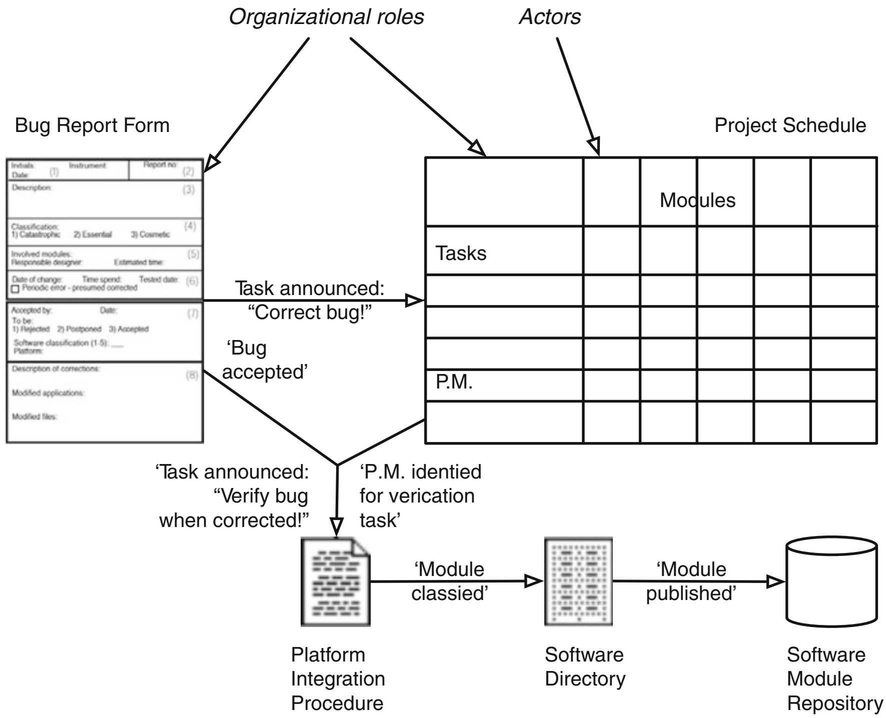

Fig. 5.3 The interacting coordination mechanisms of the software testing case. (‘P.M.’ denotes the Platform Master, i.e., the actor in charge of the integration of modules and verification of corrections at the end of the current platform period)

In the case of software testing, the continual alignment of multiple, specialized mechanisms seemed to be seamless and achieved effortlessly. This can largely be attributed to the fact that all designers would assume all roles, simultaneously or in turn. They could thereby develop and maintain a high degree of awareness of the intersections of the many mechanisms involved. In a larger or more complex arrangement this would not be as easily achieved and the alignment of the involved coordination mechanisms may thus require more effort and be less seamless.

Finally, while the array of protocol-cum-artifact dyads intersect at various points and therefore need to be aligned by the actors in the course of their work, they are only loosely coupled. Each mechanism addresses a very narrow set of coordinative activities. Because of that, each coordination mechanism can be constructed relatively independently of the others. Thus, in designing a mechanism for a specific purpose, one does not need to have an overview of the totality of work processes in the setting at large. In fact, because coordination mechanisms can be designed and introduced to serve limited purposes, the array of coordination mechanisms of the work arrangement at large can be designed and maintained in a distributed and bottom-up manner. The concept of coordination mechanisms thus suggests an approach to the design of workflow systems which, in line with Davenport's suggestions (Davenport, 1993), may result in composite workflow systems that are far less brittle in the face of the vicissitudes of contemporary business environments than workflow systems often seem to be.

Hence,

Proposition 15. Coordination mechanisms are *specialized constructs* that are devised to support certain aspects of the articulation of a specific category of distributed activities within a particular cooperative work arrangement and the use of a coordination mechanism may therefore require that it is *aligned with other mechanisms* devoted to different aspects of the articulation of those activities or to related activities.

4 Computational Coordination Mechanisms

As noted above, coordination mechanisms based on paper artifacts (e.g., forms, catalogues, time tables) have been around for ages and are used on a massive scale in modern work settings. While mundane and unassuming, they have crucial affordances: (a) the artifact can represent and convey stipulations among actors in a permanent and publicly accessible form; (b) the protocol and the artifact can be defined and specified by the actors themselves—operators, clerks, managers, auditors, etc.—by means of the ordinary skills of their professions; (c) actors have total control of the interpretation and execution of the protocol and can, under conditions of social accountability, modify or deviate from the protocol; (d) the artifact can dynamically represent state changes to the protocol and mediate these among actors, and (e) multiple coordination mechanisms can be aligned, seamlessly and smoothly, by actors.

Nonetheless, such mechanisms have serious inherent limitations: (a) state changes to the protocol are conveyed by paper and similar unwieldy artifacts and the speed and pattern of propagation of changes to the state of the protocol are thus severely limited; (b) the protocol is only immediately visible to actors to the extent that the protocol is mapped onto the symbolic construct of the artifact that serves as an intermediate; (c) modifications to the protocol only take effect when or if actors become aware of them through other channels; (d) maintaining a conventional, paper-based coordination mechanism involves a plethora of mind-numbing operations; (e) it involves massive housekeeping efforts and it may thus be practically impossible for actors to obtain an overview of the state of the protocol, and (f) the seamless and smooth alignment of multiple intersecting coordination mechanisms is only feasible insofar as the same actors are involved with the coordination mechanisms in question.

These limitations with conventional coordination mechanisms become increasingly problematic as modern industrial, service, and administrative organizations need to be able to operate in a radically flexible and adaptive and yet highly coordinated fashion. In view of these issues, it seems obvious to explore whether it is possible to construct *computational coordination mechanisms* in which the allocation of functionality between actor and artifact is changed in such a way that the coordination mechanism, as a software device, incorporates the artifact in a computational form *as well as* aspects of the protocol which, again in a computational form, operates on the artifact.[10]

Proposition 16. A *computational coordination mechanism* can be defined as a software device in which *the artifact* (in the sense of a permanent symbolic construct) *as well as aspects of the protocol* are incorporated in such a way that changes to the state of the protocol induced by one actor are conveyed, in accordance with the protocol, by the computational artifact to other actors.

Notice that, as far as the computational protocol is concerned, only *aspects* of the protocol are incorporated. As observed above, a protocol only has its (more or less) certain and agreed-to meaning within a certain social context, and it is inescapably under-specified. A computational coordination mechanism is invariably embedded within a social context of conventions and routines which competent members take for granted but which are not amenable to incorporation in a computational protocol. Hence, a computational protocol cannot simply replace members' more or less tacit conventions and social competencies. For every computational coordination mechanism, there will exist facets of the protocol which are not incorporated in the computational protocol. That is, in the construction of a computational coordination mechanism, the protocol is split into a computational protocol and a residual non-computational or 'social' protocol. Furthermore, the precise allocation of functionality between human actors and computational coordination mechanism is a non-trivial analysis and design task. A computational coordination mechanism is not an immaculate reincarnation of a coordination mechanism.

Proposition 17. The specific *allocation of functionality* between human actors and a given computational coordination mechanism reflects the extent to which it is feasible to incorporate the various aspects of the conventions and routines of the social context into a computational protocol.

Now, 'no representation of the world is either complete or permanent' and coordination mechanisms are thus 'local and temporary closures' (Gerson and Star, 1986). That is, no computational coordination mechanism will be able to handle all aspects of articulation work in all work domains. Particular computational coordination mechanisms will be designed to support cooperating actors in specific aspects of their articulation work which are particularly complex and which, most likely, are specific to the given work domain. A computational coordination mechanism should

[10] The term artifact can create misunderstandings since it can refer to two different phenomena here: On one hand we have a computational artifact in the previously defined narrow sense of a permanent symbolic construct. But on the other hand one can, of course, conceive of the computational coordination mechanism *as a software artifact* which then would incorporate the computational protocol. For the sake of clarity, we will in this context restrict the use of the term 'artifact' to denote the permanent symbolic construct.

thus be conceived of as a specialized software device that, while it is distinct from the field of work, interacts with a particular software application (e.g., a CASE tool, an office information system, a CAD system, a production control system, etc.) so as to support the articulation of the distributed activities of multiple actors with respect to that application.

Proposition 18. A computational coordination mechanism should be conceived of as *a specialized software device which interacts with a specific software application* so as to support articulation work with respect to the field of work as represented by the data structures and functionalities of that application.

On the basis of the analysis of the empirical studies of artifactually imprinted protocols in general and in particular coordinative protocol-cum-artifact dyads, a set of requirements for computational coordination mechanisms can be identified. The requirements can be organized into two categories: 'malleability' and 'linkability.'

4.1 Malleability

Since coordination mechanisms are 'resources for situated action' (Suchman, 1987), a computational coordination mechanism must be *malleable* in the sense that users are supported in defining its behavior.

Organizational demands and constraints change, and procedures and conventions change accordingly. In the case of the bug form mechanism, for example, the entire mechanism—the artifact as well as the procedures and conventions—was designed from scratch by the actors themselves and the actors were later on discussing various modifications to the protocol, for example the introduction of the role of a project manager in the protocol. It should thus be possible for actors to design and develop new computational coordination mechanisms and to make lasting modifications to existing ones. Accordingly:

Proposition 19. Actors should be able to *define the protocol* of a new coordination mechanism as well as to *redefine it* by making lasting modification to it, so as to be able to meet changing organizational requirements.

On the other hand, in view of the inexorably contingent nature of work, actors must be able to control the execution of the protocol, for instance by suspending a step, and to make local and temporary changes to the protocol, for instance by bypassing a step, by 'rewinding' a procedure, by escaping from a situation, or even by restarting the protocol from another point. For example, the bug form protocol was deviated from during its execution as erroneous classifications of bugs were discovered, as designers rejected the responsibility ascribed to them, etc. In other words, actors must be able to exercise *local control* over the execution of the protocol.

Proposition 20. A computational coordination mechanism must be constructed in such a way that actors are able to control its execution and make *local and temporary modifications* to its behavior to cope with unforeseen contingencies.

More generally stated, the specification (or instantiation) of an already defined protocol should not be conceived of as a singular act of creation. As stated in Proposition 7, a protocol is in principle under-specified. Thus, a protocol will typically be specified incrementally, at least to some extent, while it is executed in the course of the work. Furthermore, a protocol can be invoked implicitly, without any explicit announcements, for instance by certain actors taking certain actions (Strauss, 1985; Schäl, 1996). Thus, in order to allow for implicit understanding of certain aspects of articulation work as well as incomplete and not-yet complete specification, and also in order not to force actors to have to specify a coordination mechanism more explicitly than deemed necessary, a computational mechanism must be constructed in such a way that a *partial specification* of the protocol is possible. That is, it should be feasible for attributes of the protocol specification to be left un-specified and for the missing specification to be provided, at a later stage, perhaps by another mechanism or by inference from actions taken by actors (cf. Proposition 15). For example, if actor A starts performing task *a*, it may be assumed that he or she is committed to accomplish task *a* and it may also be inferred that he or she has assumed the role *x* defined as responsible for task *a*.

Hence,

Proposition 21. A computational coordination mechanism must be constructed in such a way that *its behavior can be specified while it is being executed*, at least partially, so as to allow for incomplete initial specification of the protocol.

From these requirements (Propositions 19–21) follows that the definition and specification of the protocol must be 'visible' to actors, not only in the sense that it is accessible but also, and especially, that it *makes sense* to actors in terms of their articulation work:

Proposition 22. In order for actors to be able to define, specify, and control the execution of the mechanism, the protocol must be perspicuous, i.e., accessible and intelligible to actors *at the semantic level of articulation work*.

We are not here addressing the issue of which modality of presentation is most appropriate: graphs, trees, nets, matrices, or standardized prose. The point is that the protocol must be perceptible at a semantic level, at a level of granularity, and in a modality which is appropriate for the specific work domain at hand. That is, the objects and functional primitives available to actors for defining or specifying the protocol must be expressed in terms of categories of articulation work such as roles, actors, tasks, activities, conceptual structures, resources, and so on that are meaningful to the participants involved in terms of their everyday work activities.

Moreover, as a specialized software device supporting the articulation of distributed activities with respect to a particular field of work, as represented by the data structures and functionalities of a particular application, a computational coordination mechanism must be distinct from the other software components of that application (Proposition 12) in order for the mechanism to be malleable to actors at the semantic level of articulation work. If the coordination mechanism cannot be defined and specified independently of the other components of the system, malleability cannot be bounded and actors will thus be confronted with a vast space of possibilities at innumerable semantic levels, which will lead to utter confusion.

On the other hand, articulation work is always fundamentally conceived of with respect to the common field of work and in terms of the specific ordering of objects and processes constituting this field of work. The bug report protocol, for example, refers to entities of the field of work such as 'module name,' whereas the *kanban* protocol refers to such entities as 'part name' and 'number of parts,' etc. Accordingly, a computational coordination mechanism must be constructed in such a way that its stipulations can be related to and expressed in terms of the objects and processes of the field of work. For example, a computational coordination mechanism interacting with a collaborative-writing application to support the coordination of the flow of distributed activities of writing, editing, evaluating, reviewing, proofreading, and accepting contributions to a technical report would need to be able to relate to the usual data structures of the word processor application: text strings, formatting instructions, document components (paragraphs, sections, headings, tables, headers, footnotes, etc.). The same, of course, applies to the data structures and functionalities of the various domain-specific information systems such as MIS, OIS, CIM, and CASE systems which are part of the (wider) field of work of the cooperative work arrangement in question.

Proposition 23. A computational coordination mechanism must be constructed in such a way that actors, in defining and specifying the mechanism, can establish *relationships between components of the mechanism and the field of work* as represented by the data structures and functionalities of the target applications.

Since articulation work, as we noted earlier (Proposition 2), is a recursive function, changing a coordination mechanism may itself be done cooperatively, as part and parcel of the cooperative effort. That is, changing a coordination mechanism (permanently or temporarily) may itself be a cooperative activity which may need to be supported. Accordingly, it should be possible for actors to control the propagation of changes in terms of factors such as: When should a given change take effect? Which instances of the (previous) protocol should be affected, and how? Which actors should be notified, and how? Which complementary actions should be taken pursuant to the change, by whom? And so forth.

Proposition 24. Since a computational coordination mechanism must be malleable, it must be constructed in such a way that actors are supported in *controlling the propagation of changes to the protocol* within the cooperative work arrangement.

4.2 Linkability

Coordination mechanisms are local and temporary closures, as we have frequently noted. A given cooperative work arrangement will—in all but the most extreme circumstances—be working with multiple CSCW applications and they will need to articulate their distributed activities with respect to these different applications (as well as to many other aspects of their environment, of course). For example, in the domain of engineering design, the cooperative ensemble may be using applications such as project management tools, CAD tools, and process planning tools as well as generic 'groupware' tools such as departmental calendar systems and collaborative writing tools. To regulate articulation work with respect to these applications an array of specialized coordination mechanisms may be devised. And, to confound matters, multiple mechanisms may be required to address specific aspects of articulation work with respect to each application. This may pose problems.

In the case of paper-based coordination mechanisms, the artifact is completely inert and any changes to the state of the protocol or the artifact are exclusively the result of actions by human actors (even when, as in the *kanban* case, it may appear the result of a monstrous, distributed machinery). Because actors are totally involved in the execution of paper-based mechanisms—they are completely 'in the loop' —, they are also relatively well placed to align the different coordination mechanisms with respect to each other, at least in so far as the different actors generally are equally involved in the use of the different mechanisms. When the allocation of functionality between artifact and actor changes, however, as a result of the introduction of computational coordination mechanisms, the ability of actors to align protocols in the former intuitive way may deteriorate.

Thus, as multiple coordination mechanisms are introduced to regulate articulation work with respect to multiple applications users will be inundated with overhead activities of aligning the different mechanisms: aligning each mechanism with changes to other mechanisms. In order not to create an impedance between the multitude of interlaced—individual and cooperative—coordinative activities, it should be possible for actors to link different coordination mechanisms addressing the different applications to facilitate a seamless alignment of articulation work with respect to these applications.

Proposition 25. A computational coordination mechanism should be constructed in such a way that it *can be linked to other coordination mechanisms* in its organizational context.

The requirement of linkability is not limited to links to other computational coordination mechanisms in the strict sense but applies to the relationship of a computational coordination mechanism to computational representations of the organizational context in which the given cooperative work arrangement is embedded. In constructing a coordination mechanism it may for instance be appropriate to provide links to indices to common repositories (previous designs, components, work in progress, drawings, patents, etc.), indices to technical resources (processes, tools, machinery), indices to available personnel (skills, competencies, schedules), indices to statutory constraints, and so on (De Michelis and Grasso, 1993; Fuchs and Prinz, 1993; Prinz, 1993). The challenge is, as Ellis and Keddara aptly put it, to make groupware 'organizationally aware' (Ellis et al., 1995).

Since computational coordination mechanisms must be able to interact in a concerted fashion, they must be constructed by means of the same set of elements, at the same semantic level. To ensure that, a *general notation* for constructing computational coordination mechanism is required.

Proposition 26. To ensure comprehensive linkability of computational coordination mechanisms, *a general notation* for constructing computational coordination mechanism is required.

Now, malleability and linkability are evidently contradictory requirements, since the former hinges upon the possibility of *changing* the behavior of a mechanism while the latter hinges upon the *stability* of the behavior of other mechanisms. This conflict is unavoidable since no representation of the world is either complete or permanent. Nevertheless, the conflict can be alleviated in different ways: Basically, coordination mechanisms can be made tolerant of limited and relatively trivial modifications of other mechanisms by means of interface agents. However, when modifications are too radical to be handled by such interfaces, and the problem therefore recurses (Bowker and Star, 1991), the affected cooperative ensembles of course have to sort out the mess and negotiate a new arrangement. In line with the recursive nature of articulation work, such negotiations may themselves be governed by a suitable coordination mechanism. In that case, one coordination mechanism would take another cooperative work arrangement and perhaps also its coordination mechanisms as its field of work. This suggestion is not the fruit of idle speculation on our part but is grounded in the field study evidence. For example, in a study of the cooperative production of technical documentation in a Danish manufacturing company we have observed cases where one coordination mechanism, a 'construction note' which was normally used for governing the distributed process of negotiating proposed changes to designs, was now used for *governing the process of negotiating proposed changes to another coordination mechanism*, namely a 'classification scheme' that was used to govern the distributed production and dissemination of the technical documentation within the company (cf. Schmidt et al., 1995). In other words, the problem recurses, but so does the solution.

5 In lieu of a Conclusion: The Ariadne Notation

On the basis of the conceptual framework outlined in the preceding sections of this paper, especially the general requirements for computational coordination mechanisms (Propositions 19–25), a general notation for constructing computational coordination mechanisms has been implemented under the name Ariadne. As noted in the introduction, a proper description of the Ariadne notation is beyond the scope of this paper and has been published elsewhere (Simone and Schmidt, 1994; Simone et al., 1995b). Nonetheless, it seems appropriate to conclude our exposition by sketching very briefly the general shape of the Ariadne notation as derived from the empirical investigations and theoretical analyses.

The crucial point in developing a notation for constructing computational coordination mechanisms is to determine a repertory of elemental categories, at the semantic levels of articulation work (Proposition 22), by means of which coordinative protocols can be expressed. Taking Anselm Strauss' quite informal lexicon of articulation work (who, what, where, when, how, how soon, for how long, etc. (Strauss, 1985)) as our baseline, a set of elemental categories was derived from the studies of how artifactually imprinted protocols are designed and used by actors in everyday work activities, as shown in the table of Fig. 5.4.[11] These categories represent the minimal set of categories required to express the protocols examined in these field studies.

Two qualifications are required here. Firstly, for the purpose of developing the Ariadne notation it was assumed that the set of elemental categories of articulation work identified in Fig. 5.4 is complete and definite. Nevertheless, this set of categories has been derived empirically through an iterative process of induction, and the repertory is undoubtedly neither complete and nor definite. For the construction of a computational notation the assumption of completeness and definiteness is necessary, however, but this does not preclude the lexicon from evolving over time, as long as the set of categories at any given point in time can be taken to be finite. Secondly, this lexicon is not intended to be a particularly useful or comprehensive, terminology for ethnographic field work or for other kinds of empirical investigations of cooperative work (and, in fact, it is far too crude for such purposes). The set of categories of Fig. 5.4 has been derived solely for the purpose of defining coordinative protocols with a view to constructing computational protocols.

In the table of elemental categories of articulation work, the categories and predicates are ordered along two dimensions: vertically, categories of articulation work with respect to the *cooperative work arrangement* versus *the field of work* (Proposition 1); horizontally, categories of *nominal* versus *actual* articulation work (Proposition 7). Two aspects require elaboration, albeit very briefly:

(1) The category termed 'conceptual structures' denotes the various constructs needed to express the conceptualizations of the field of work (definitions, classifications, etc.) that the members of the cooperative ensemble have adopted to be able to refer to the multifarious objects and processes of their common field of work in an orderly fashion.
(2) The distinction between nominal and actual (Proposition 7) identifies categories pertaining to the *definition* and *specification* of the computational coordination mechanism, respectively (Propositions 19 and 20–21). The point of this distinction is that the transition from nominal to actual status is not merely *a refinement*, since the categories are qualitatively different. An *activity*, for example, denotes a work process as an unfolding course of action in terms of those aspects of a work process that are relevant to *doing* the work with the currently available resources. By contrast, a *task* denotes an operational intent, irrespective of how it is implemented (Andersen et al., 1990). In other words, a *task* is expressed in terms of *what*, an *activity* in terms of *how*.

[11] A similar idea of selecting objects and related operations has been suggested by Malone and Crowston (1990) as an initial foundation for an interdisciplinary 'coordination theory'. This effort has evolved into the current attempt to define 'tools for inventing organizations' (Malone et al., 1993).

Nominal		**Actual**	
Elemental categories of articulation work	**Elemental predicates**	**Elemental categories of articulation work**	**Elemental predicates**
Articulation work with respect to the cooperative work arrangement			
Role	assign to [Committed actor]; responsible for [Task, Resource]	Committed-actor	assume , accept, reject [Role]; initiate [Activity];
Task	point out, express; divide, relate; allocate, volunteer; accept, reject; order, countermand; accomplish, assess; approve, disapprove; realized by [Activity]; to be aligned with [Task]	Activity/Action	[Committed actor] initiate; [Actor-in-action] undertake, do, accomplish; realize [Task]; [Actor-in-action] makes publicly perceptible, monitors, is aware of, explains, questions; aligned with [Activity]
Personnel	locate, allocate, reserve;	Actor-in-action	does [Activity];
Articulation work with respect to the field of work			
Conceptual structures	categorize: define, relate, exemplify relations between categories pertaining to [Field of Work];	State of field of work	classify aspect of [State of field of work]; monitor, direct attention to, make sense of, act on aspect of [State of field of work];
Informational resource	locate, obtain access to, block access to;	Informational resources-in-use	show, hide content of; publicize, conceal existence of;
Material resource	locate, procure; allocate, reserve to [Task];	Material resources-in-use	deploy, consume; transform;
Technical resource	locate, procure; allocate, reserve to [Task];	Technical resources-in-use	deploy; use;
Infrastructural resource	reserve;	Infrastructural resources-in-use	use;

Fig. 5.4 Elemental categories of articulation work model: The table identifies the elemental categories of articulation work and their predicates

The categories of articulation work are the basic building blocks made available by Ariadne whereas the elemental predicates are used to identify, and to define the meaning of, the attributes which characterize the categories and relations among them within the Ariadne notation.

The provision of a notation at the semantic level of articulation work distinguishes Ariadne from many proposed environments for the development of CSCW applications. These environments are typically based on partial repertoires of categories of articulation work. In some case, though, CSCW environments offer languages at the semantic level of the manipulation of general-purpose objects (e.g. Malone et al., 1992). To use such an environment for constructing a coordination mechanism, actors have to specialize these general purpose objects, that is, define a set of objects at the level of articulation work. This transformation involves the effort of defining an *ad hoc* model of articulation work and often leads to the definition of partial articulation work models, since the effort normally is undertaken in the context of developing a specific application. The *ad hoc* and partial character of these models becomes problematic when the resulting computational coordination mechanisms need to be modified and linked during the distributed and evolutionary construction process of their life cycles. By contrast, Ariadne provides a finite and expressive framework for developing a shared understanding across the activities of the distributed and evolutionary construction process. In this framework, the lexicon of articulation work plays the fundamental role of governing the design of computational coordination mechanisms, of allowing for their malleability and linkability, of governing the impact of changes, of supporting the handling of partial specifications and, finally, of making the notation perceptible and hence usable to all categories of actors.

Finally, to meet the requirements of malleability and linkability the Ariadne notation has been given an internal structure organized into three levels that are called α, β, and γ, as illustrated in the central part of Fig. 5.5.

The three levels are not hierarchical, and going from one level to the lower level is not identical to a refinement, since the information handled at each level is of a different nature. Rather, each level defines the 'space of possibility' for the lower one: the γ-level provides the grammars that can be applied at the β-level to define protocols, whereas the protocols defined at the β-level can be specified at the α-level. More specifically:

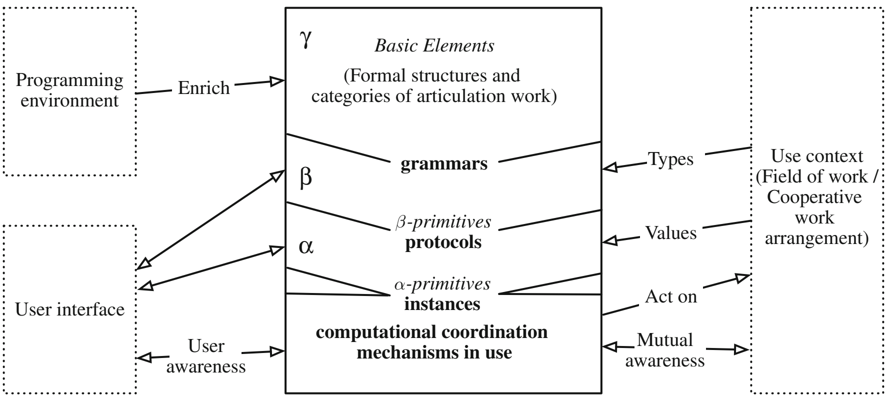

Fig. 5.5 The architecture of the Ariadne notation

At the γ-level, it is possible to define or modify a grammar which can then be used to construct an infinite variety of protocols at the β-level. Building a grammar means determining the expressive power of a language for defining a class of computational coordination mechanisms, that is, the components that will constitute the computational coordination mechanism together with their structural interrelationships (for instance, to construct workflows or classification schemes) as well as the operational semantics associated to the elements of the grammar. The 'space of possibility' within which grammars can be defined at the γ-level is determined by the set of categories of articulation work underlying Ariadne at any point in time, and by the available set of formal structures for representing various relations (causal, hierarchical, instrumental, etc.) among the categories (the Basic Elements of Fig. 5.5).

At the β-level, it is possible to define or modify the protocol itself according to the chosen grammar. For instance, a bug report protocol can be specified with more or less emphasis on its distributed features by choosing an appropriate grammar. In this context, the user can determine the allocation of functionality between human actor and protocol, select methods for handling partial specifications, and make permanent changes to an existing protocol as part of its evolutionary design.

At the α-level, finally, it is possible to instantiate and activate the protocol in a particular situation and to do so in an incremental fashion (Proposition 21). The same protocol, e.g., the bug form protocol, can be executed repeatedly and concurrently by different actors. Moreover, the primitives at this level allow for the management of local changes (Proposition 20).

The different levels of Ariadne will typically be accessed by users with different skills. At the α- and β-level, the use of the notation does not require specialized skills, at least not skills more specialized than those required to use a spreadsheet application to construct a model, for example a household budget. That is, at these levels the use of the notation will merely require the ability of selecting and combining predefined items according to the rules of a relevant grammar and the associated semantics. The α- and β-levels are typically needed by end-users who, as part of their everyday work activities, use and design coordination mechanisms. The γ-level, on the other hand, is typically the realm of the 'application designer' or, in our framework, actors who define grammars needed by a particular community of end-users for defining their protocols.[12]

While the distinction between the β-level and the α-level of the notation can be recognized in almost all recent CSCW applications, the γ-level is unique. There are two reasons for the introduction of the γ-level. First of all, the γ-level makes it possible to define an appropriate language for constructing a family of computational coordination mechanisms, for instance for a particular work domain. The aim is to overcome a certain limitation of workflow systems, and of CSCW applications

[12]Notice that while the construction of grammars could be performed by end-users with a specific aptitude and competence for modelling, the design of new elements for the notation is a pure programming activity that requires specialized technical skills.

in general, namely that they impose a particular modelling approach. For example, the dynamic aspects of the protocols are in most applications and environments described through partial models of articulation work, of which the most common focus on the flow of objects across organizational units (individuals, tasks, etc.), on the flow of control across actions, on some communication patterns among roles (negotiation), or on some predefined combination of these (e.g., Ellis, 1979; Cook, 1980; Shepherd et al., 1990; Medina-Mora et al., 1992; Swenson et al., 1994). Since the adequacy of the language strongly depends on which aspects of articulation work are to be supported and on the organizational environment in which the planned coordination mechanisms are intended to operate, Ariadne does not impose a preconceived modeling approach. Rather, Ariadne allows designers to adopt a particular interpretation of the underlying repertory of categories of articulation work.

The second reason for introducing a γ-level of the notation is the requirement of linkability of computational coordination mechanisms. In fact, the γ-level of Ariadne offers an Interoperability Language that the designer can make an integrated part of the language for constructing computational coordination mechanisms (Divitini et al., 1995; Simone et al., 1995b).

Referring again to Fig. 5.5, the arrows on the left-hand side connect the basic elements at the γ-level with the framework in which they are developed. The connection to the Programming Environment is intended to stress that the notation is in a dynamic but disciplined relationship with its development environment, in the sense that any increase of the expressive power of Ariadne is realized by enriching the sets of basic elements of the notation while otherwise preserving the properties of the notation.

The notation also has an obvious connection with the user interface service. The design of Ariadne is concerned with the definition of the information necessary to design computational coordination mechanisms (that is, the appropriate expressive power of the language) and not with how this information is requested by or presented to the users. By this choice we are not denying the crucial role of the 'material format of the artifact,' as it is represented at the user interface. To the contrary, we realize and acknowledge that the design of the material format of computational coordination mechanisms requires a specialized and demanding research effort that is beyond the scope of Ariadne. However, some requirements of the user interface service are obvious. Firstly, of course, appropriate multi-modal representations must be devised for the various types of elements of the notation, the syntactic rules of their combination as well as their behavior. These representations should exhibit the same properties that characterize the notation, namely compositionality, malleability, linkability. Secondly, in accordance with the layered structure of the notation, the representations should be tailorable to different organizational roles in different application domains.

The arrows on the right-hand side of Fig. 5.5 connect the computational coordination mechanism to the field of work and to the cooperative work arrangement, that is, to its context of use (Proposition 23). At the γ-level there are no connections to the context of use. In fact, the grammars are independent of the particular contexts of use in that they simply define the expressive power of the grammars to be used to define specific protocols. By contrast, the definition and specification of a protocol are related to a given (class of) field of work and work arrangement. The objects of the field of work and of the work arrangement are related to the categories of articulation work: as ‘types’ at the β level and as ‘instances’ at the α level. In protocols, ‘types’ are imported together with the related ‘methods’ which are then conveyed to the ‘instances’ in the standard way. Thus, Ariadne explicitly requires a clear interface between the computational coordination mechanisms and their context of use and provides facilities for defining such an interface.

We conclude this section with some comments on the realization of Ariadne. In the initial development of the Ariadne notation, a considered decision was made to postpone the implementation and concentrate on developing a formal specification of its elements and on evaluating it against the requirements and scenarios derived from field studies. This strategy was adopted, consciously and explicitly, in order to avoid having the notation influenced, in an implicit and uncontrollable manner, by the inevitable limitations of currently available implementation platforms. The formal specification showed that it was feasible to construct malleable coordination mechanisms by means of the notation. Subsequently, a ‘concept demonstration’ of the formal specification of the notation was implemented in an environment which is particularly suitable to managing relational structures and their behavior. This partial implementation has established that the layered structure and compositionality of the Ariadne notation are workable (Simone et al., 1995b) and has demonstrated that an agent-based architecture is most suitable for Ariadne (Divitini et al., 1995; Simone et al., 1995a). A new implementation of Ariadne, based on such an architecture, is envisioned.

Chapter 6
Of Maps and Scripts (1997)

The Status of Formal Constructs in Cooperative Work

Thanks to impressive CSCW systems such as TeamWorkStation (Ishii, 1990), GroupDesk (Fuchs et al., 1995), wOrlds (Fitzpatrick et al., 1996), and TeamRoom (Roseman and Greenberg, 1996), to name but a few, it is by now widely accepted that computer artifacts can provide effective support for cooperative work by offering a 'shared space' through which actors can interact directly, i.e., by means of generic competencies such as talking, gesturing, pointing, monitoring etc., without other restraints than the constraints of limited bandwidth and so on.

There is considerably less certainty and consensus, however, as to whether computer systems can be successfully designed to support cooperative work by providing representations of formal organizational constructs—procedures, workflows, process models, etc.—so as to regulate the routine coordinative activities and thereby enable cooperative ensembles to perform more reliably and efficiently. In fact, there are strong concerns about the status of such formal constructs.

In a way CSCW can be said to have been born with these concerns. The office automation movement had already given way to disillusionment, and artificial intelligence was increasingly being confronted with unfulfilled promises. At the same time, a number of critical studies had demonstrated that the problems were deep rooted: office procedures were of a different nature than presumed by the protagonists of office automation (e.g., Wynn, 1979; Suchman, 1983; Suchman and Wynn, 1984). The general conclusion of these studies were that such constructs, instead of determining action causally, serve as 'maps' which responsible and competent actors may consult to accomplish their work (Suchman, 1987, p. 188 f.; Bucciarelli, 1988b, p. 114). Thus, Lucy Suchman's radical critique of cognitive science (1987) and the 'situated action' perspective she proposed has played a significant role in defining the CSCW agenda and has become a shared frame of reference to many,

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Kjeld Schmidt: 'Of maps and scripts: The status of formal constructs in cooperative work,' in S. C. Hayne and W. Prinz (eds.): *GROUP'97: Proceedings of the ACM SIGGROUP Conference on Supporting Group Work, 16–19 November 1997, Phoenix, Arizona*, ACM Press, New York, 1997, pp. 138–147.

perhaps most, of us. For good reasons, then, designers of CSCW systems have been advised to treat them with great caution (cf., e.g., Robinson and Bannon, 1991).

Thus, the issue of how computer systems can be successfully used to regulate or govern routine coordinative activities has been surprisingly marginal on the CSCW agenda and one can detect a pronounced reluctance to design systems which regulate coordinative activities in the work of many eminent CSCW researchers who instead pursue different 'minimalist' design strategies (e.g., Kreifelts et al., 1993; Trevor et al., 1993; Bentley and Dourish, 1995; Dourish et al., 1996). This is quite legitimate and reflects serious and well-founded concerns about the status of formal organizational constructs.

However, the role of formal constructs in cooperative work is still far from understood. Although Suchman explicitly submits the 'situated action' perspective as a research approach (Suchman, 1987, pp. 178 f.), it often seems as if the issue is presumed to have been settled. Only few CSCW researchers have attempted to address this issue (Bowers, 1992; Bowers et al., 1995).

It is perhaps not surprising, then, that CSCW has had little influence on the development of workflow technologies (Abbott and Sarin, 1994). These technologies have not benefited from CSCW insights (e.g., the crucial role of mutual awareness and shared artifacts) while CSCW has become similarly marginalized with respect to the needs of large-scale cooperative work arrangements where 'shared spaces' typically are of only marginal relevance.

In other words, it's time to face the specter of 'formal constructs' again. Firstly, I will argue that determining whether particular observable work activities are in accord with standard procedures or not raises fundamental methodological problems. Next, I will revisit some of the empirical evidence of the status of formal constructs such as office procedures and will argue that these studies did not investigate the use of procedures under routine conditions and, hence, that the way in which the findings from these studies have been generalized is problematic. As a contrast, I will offer contradictory evidence which suggests that formal constructs sometimes serve more like scripts than maps and that the role of formal constructs is more differentiated than generally taken for granted in CSCW. And finally, I will argue that in order to understand the role of formal constructs in cooperative work we need to take into account the fact that such constructs—to be effective—are inscribed upon textual artifacts and that we therefore must investigate thoroughly how such objectified formal constructs are used in the coordination of cooperative work.

The purpose of this discussion is not to suggest that the 'situated action' perspective as such is problematic. The paper is a critique in the Kantian sense, i.e., an attempt to determine the proper domain of this approach, so as to unburden it of some popular misunderstandings and unwarranted generalizations and suggest some nuanced conceptualizations for further research. Thus, if the following discussion again and again takes issue with some of Lucy Suchman's propositions, it is merely an indication of the significance of her contribution to CSCW and of my own intellectual debt to her work.

1 Determining the Meaning of Formal Constructs

In a large body of sociological literature, the commonsense presumption that predefined organizational constructs (formal structures, procedures, methods, plans) somehow determine action has been subjected to critical examination. For years, study after study have demonstrated, unambiguously and beyond any doubt, that the status of these formal organizational constructs in the actual course of work is problematic in that these constructs are abstract idealizations when taken as representations of actually unfolding activities. In the words of Philip Selznick's classic summary of this line of sociological investigation: 'The formal administrative design can never adequately or fully reflect the concrete organization to which it refers, for the obvious reasons that no abstract plan or pattern can—or may, if it is to be useful—exhaustively describe an empirical totality. At the same time, that which is not included in the abstract design (as reflected, for example, in a staff-and-line organization chart) is vitally relevant to the maintenance and development of the formal system itself.' (Selznick, 1948, p. 25).

Years later, in the context of examining the notion of office procedures underlying office automation, Suchman and Wynn raised the same question: 'how adequately do these accounts describe how office work gets done?' and made a quite similar point: 'The problems involved in accomplishing office tasks, while central to work practices, are ignored in procedural formulations of how the work gets done. The point of this observation is not to critique procedural formulations, but to indicate *another domain of the work*, in which those formulations are brought to bear on the practical contingencies of actual tasks' (Suchman and Wynn, 1984, p. 139).

This conception of the status of formal constructs has been highly influential in that it, as observed by Egon Bittner in a classic paper, has 'furnished the necessary theoretical argument for an entire field of sociological investigations by directing attention to a sphere of adaptive and cooperative manipulations, and to the tensions typically found in it.' (Bittner, 1965, p. 240).

The conception is also a methodological nightmare, however, that systematically confounds analyses of the use of formal constructs in working life. The tradition of critical studies of formal constructs implicitly ascribes an almost ceremonial status to these constructs and it thus falls victim to a dichotomy of the 'formal' and the 'informal,' 'procedure' and 'practice'. The argument implies that members of the organizational settings in question are somehow supposed to take formal constructs literally—as if constructs such as procedural formulations are *supposed* to be exhaustive specifications of how the work gets done.

In addressing this problem from the perspective of ethnomethodology, Bittner makes some very cogent observations: 'While Selznick quite clearly assigns the formal schemes to the domain of sociological data, he does not explore the full range of consequences out of this decision. By retaining Weber's conception of them as normative idealizations, *Selznick avoids having to consider what the constructions of rational conduct mean to, and how they are used by, persons who have to live with them from day to day. It could be, however, that the rational schemes appear* *as unrealistic normative idealizations only when one considers them literally, i.e., without considering some tacit background assumptions that bureaucrats take for granted*.' (Bittner, 1965, p. 242, emphasis added).

Bittner's methodological recommendation is quite pertinent to the issue at hand: The meaning of formal constructs cannot be taken to be immediately obvious to the investigator. To the contrary! Or in Zimmerman's accurate restatement of Bittner's injunction, 'the investigator should not provide his own (essentially commonsense) interpretation of what such rational constructions mean for those charged with their use' (1966, p. 12). Hence, 'the import or meaning of rational constructions for action is a matter of empirical determination' (ibid.).

When, for instance, Suchman says 'that situated action turns on local interactions between actor and contingencies that, while they are made accountable to a plan, remain *essentially outside* the plan's scope' (Suchman, 1987, pp. 188 f., emphasis added), a dichotomy between plan and action is introduced which is methodologically problematic. By contrast and in the words of Zimmerman, 'the observed practices' should be viewed as 'the "governing sense" of the existing rules' (Zimmerman, 1966).[1]

Consider, for example, Louis Bucciarelli's excellent study of design work (1988b) which is based on several years of ethnographic investigation of design at an engineering firm engaged in making photovoltaic modules for the production of electrical power from sun light. In his analysis of the findings, Bucciarelli questions the status of the organizational constructs and corresponding artifacts used in that particular setting: 'The milestone chart [. . .] can be viewed as a snapshot of a month in the life of a participant in design, a picture of how his or her time is to be "spent" over the next one or several months. [. . .] The chart suggests that there exists clear and distinct beginnings and ends to design tasks. What can be surer than a "deadline"?' (Bucciarelli, 1988b, p. 98).

In general, Bucciarelli argues, charts such as the milestone chart and the critical path chart 'suggest that tasks are all of a finite duration and bounded by well-defined starting dates and deadlines' (Bucciarelli, 1988b, p. 104). In Bucciarelli's analysis, it thus offers an 'illusion of definiteness': 'While it suggests the continuous chinking away of a finite number of days to come, from the perspective of the individual whose milestone chart it is, the exercise of its construction has an element of fantasy about it, asking for too high a degree of precision in pacing future, uncertain events. To account for one's future in the terms of the chart engenders an uneasiness, a sense that its format is too confining and disallows any adequate explanation of what it will take to get the job done.' (Bucciarelli, 1988b, p. 107).

The core of Bucciarelli's interpretation is the contention that the chart 'suggests' that there exists clear and distinct beginnings and ends to design tasks and that it thereby offers an 'illusion of definiteness'. While there is no reason to doubt the

[1] This does not mean that the distinction between following a rule and breaking a rule is obliterated. 'To do so would be to violate an essential feature of a rule; i.e., that it be possible to determine whether or not the rule was correctly applied. [This] determination is left to persons whose task it is to decide such matters.' (Zimmerman, 1966, p. 155).

veracity and accuracy of Bucciarelli's observations, this interpretation is methodologically dubious: To whom does the chart make such illusionary suggestions? Are the constructs indeed 'misleading' (p. 106) to competent members? Has anybody been mislead by them? How? Is the 'fantasy' being enforced? Or, to put it the other way round, has any of the engineers been admonished for not subscribing to the notion that 'tasks are all of a finite duration and bounded by well-defined starting dates and deadlines'? Bucciarelli does not present any evidence to that effect and one is thus led to surmise that the putative contradiction between the formal constructs and the actual practice of the engineers may be the investigator's own construction and that the design of these constructs presumes the observed practice.

2 The Problem of Generalization

On the basis of which kind of evidence can we make well-founded statements about the status and use of formal constructs in cooperative work settings? Or rather, what can be learned from which kind of evidence?

First of all, there is reason to assume that formal constructs are used in radically different ways in small ensembles and in large-scale cooperative settings. That is, we need to be cautious as to how and to which extent we generalize from studies of the use of formal constructs in small groups, perhaps co-located, where activities can be articulated seamlessly, as opposed large-scale cooperative work arrangements. This issue has for instance been raised by Dubinskas in an interesting comment to Bucciarelli's study of the solar energy panel project (Dubinskas, 1988). Comparing Bucciarelli's findings to his own observations from the automobile industry, Dubinskas notes that the temporal and design flexibility observed by Bucciarelli is not to be expected in automobile design settings: '[In the solar energy panel project, the] number of people and components was lower, the range of expertise was much narrower in scope, and the design process took place in a largely face-to-face environment. Schedule building was intimately tied to the progressive emergence of the artifact—the panel—and the project direction was perhaps less clearly defined (or constrained) technically than new car development is. One result was that schedule formation became a regularized forum for negotiations about the order of work and the character of the artifact' (Dubinskas, 1988, p. 18). That is, due to the special conditions of the solar energy panel project the participants were presumably able to articulate their various activities without, for instance, unceasingly relying on the stipulations of the milestone chart which, thus, could remain in the desk drawer or on the bulletin board for reference.

Dubinskas' point can be illustrated by the case of the S4000 project which shows in detail how formal constructs are invented and introduced to handle the increased complexity experienced when the scale of cooperative work is increased (Carstensen et al., 1995b; Carstensen, 1996; Carstensen and Sørensen, 1996). Foss Electric is a Danish manufacturing company that produces advanced equipment for analytical measurement of quality parameters of agricultural products, e.g., the compositional quality of milk in terms of fat content and the count of protein, lactose, somatic cells, bacteria, etc. At the time of the field study, the company was engaged in a large design project called S4000 which aimed at building a new instrument for analytical testing of raw milk. It was the first attempt to build an integrated instrument which would offer a range of functionalities that previously had been offered by a number of specialized instruments and the S4000 would be the first Foss instrument to incorporate a personal computer to control the instrument. On the whole, the project was significantly more complex than previous projects at Foss.

The project posed the most dramatic challenge to the software designers who had, until then, been working individually or, occasionally, in teams of two. In the S4000 project, however, eleven programmers were involved in the design of an integrated software system which ultimately amounted to some 200,000 lines of code.

Traditional measures such as shared office spaces to support mutual awareness and *ad hoc* interaction as well as the usual design meetings were soon experienced as insufficient. The software designers felt that were pretty much in the dark with respect to the state of the project and that much more effective coordination was required. At the height of the crisis the software design goals were almost abandoned. To overcome the crisis, the software designers developed a repertoire of procedures and artifacts to ensure the monitoring and control of the integration of software components and modules. Thus, a 'bug report form' with corresponding procedures for reporting, classifying, and correcting faults were introduced to ensure that bugs were properly registered, that corrected bugs were duly reported, and to make the allocation of responsibilities clear and visible to all members. As a complementary measure, copies of bug forms were collected in a publicly available repository in the form of a simple binder (For details, cf. Carstensen, 1996; Carstensen and Sørensen, 1996).

The software designers thus realized that it was impossible to handle the distributed testing and bug registration activities of some twenty testers and designers without, *inter alia*, a bug report form and its associated procedures. By devising and introducing these constructs they managed to alleviate the coordination crisis in the project.

The case is particularly valuable because we here witness the introduction of formal constructs for coordination purposes in response to overwhelming problems encountered in coping with the complexities of articulating cooperative work under conditions that are typical for contemporary industry. However, while daunting to the participants, the complexity of the S4000 project is not exceptional. Such complexities are an everyday occurrence in modern industrial, service, and administrative settings.

In our effort to understand the use of formal constructs in cooperative work we should thus bear in mind that it may be problematic to generalize from cooperative work on a small scale where activities can be articulated largely or entirely by means of direct interaction.

Another major problem of generalization is the extent to which we can learn of the use of formal constructs from studies of work in non-routine situations.

Consider, for instance, Suchman's study of the accounting office (1983): This office was responsible for the orderly payment to outside organizations which supplied goods and services to the company. Orderly payment was documented through record-keeping, and accuracy was monitored by the auditing of invoices against records of requisition and receipt. According to the standard procedure, items on a given purchase order could be received and billed in separate installments over an extended period. Again, if all went smoothly, the items marked off on the receiving report from Shipping or Receiving would correspond to those on the invoice from the vendor. The purchase order, receiver, and invoice would be matched and audited. The payment for the items received would be recorded by margin notes on the purchase order, which would then be returned to the temporary file to wait for the next shipment and billing. Only after all bills had been received and paid would the completed purchase order be filed permanently in the paid file.

In the episode described by Suchman, however, the record of what had happened was incomplete: The original purchase order was missing. A completed receiving document was found with eight items listed on it, all of which had been marked as received. The two invoices found in the paid file showed only two items as paid, however; there was no invoice or record of payment for the other items, yet the vendor reported that the transaction would be completed with payment of the past due invoice for only two of those items that seemingly had not yet been paid. The study then shows how the two actors, the accounting clerk and the auditing clerk, step by step solved the 'mystery': Of the invoice for one of the items, only page two was on file; page one was missing. It thus transpired that four other items were invoiced with this item and had already been paid.

Suchman's interpretation of the case is cogent and succinct: 'Standard procedure is constituted by the generation of orderly records. This does not necessarily mean, however, that orderly records are the result, or outcome, of some prescribed sequence of steps. [...] In this case, once the legitimate history of the past due invoice is established, payment is made by acting as though the record[s] were complete and then filling in the documentation where necessary. The practice of completing a record or pieces of it after the fact of actions taken is central to the work of record-keeping.' (p. 326). The case thus provides a graphic impression of the massive heuristic use of standard procedures even in a seemingly abnormal situation. The two actors were able to solve the problem because of their 'knowledge of the accounts payable procedure' (p. 322). Standard procedures have a heuristic function in the sense that they 'are formulated in the interest of what things should come to, and not necessarily how they should arrive there' (p. 326). The case thus gives us an insight into the crucial role of prescribed procedures even in the handling of contingencies; it shows that prescribed procedures convey heuristic information for the handling of errors as well as for routine tasks.

However, in several places Suchman suggests more general and radical interpretations of the case. She introduces the study by stating, without reservation, that 'the case suggests that the procedural structure of organizational activities is the *product* of the orderly work of the office, rather than the reflection of some enduring structure that stands behind that work' (p. 321). And she concludes with similar general formulations: 'It is the assembly of orderly records out of the practical contingencies of actual cases that produces evidence of action in accordance with routine procedure' (p. 327). Because the argumentation in other passages is carefully guarded, the reader is left with the impression that the general interpretations are deliberate, that is, that office procedures are the *product* of the orderly work of the office and that they do not in some way or at some level determine the course of action. And this reading is how the study has been generally understood. For instance, in his review of Suchman's *Plans and Situated Action*, Phil Agre summarized the study of the accounting office as follows: 'She discovered [. . .] that the actual role of the prescribed office procedures was *not* to specify how their work should proceed. Instead, the procedures provided a criterion for judging how their work should turn out at the end of the day. The office workers used the office procedures as resources in figuring out what their work should come to, but they based their decisions about how to achieve this end on the particulars of each next case that came along' (1990, p. 375).

This interpretation of the case is not supported by the published data. The study presents an analysis of a recovery from breakdown. It does not attempt to demonstrate that prescribed procedures do not—in some form and to some extent—determine the handling of routine cases; it does not even attempt to give an analysis of how prescribed procedures are used in routine cases. The study thus provides little insight into how standard procedures, defined as pre-defined written stipulations, are applied in routine daily work.[2]

While this and other studies have contributed substantially to our understanding of the articulation of cooperative activities and have been highly influential in dissipating the simplistic notion of the 'office automation' movement, they are problematic in that the evidence does not warrant the general conclusions the authors seem to draw. In their analyses of the status of formal constructs such as procedures, the authors do not take into account the fact that the situations studied are beyond the 'jurisdiction' of these constructs, that is, beyond the operational conditions for which they had presumably been designed. The point I'm trying to make is that, contrary to what seems to be claimed by the general conclusions of these studies and how their have been interpreted and received, *the use of procedures under everyday routine conditions* for which such procedures are designed, *is not investigated* in these studies.

Instead of merely observing in case study after case study that procedures are impoverished abstractions when confronted with the multifarious and contingent nature of practical action, it is necessary to investigate precisely *how* they stipulate the articulation of cooperative work, *how* they are interpreted and used, designed and adapted by competent actors 'who have to live with them from day to day'.

[2]Standard operating procedures are, of course, instrumental in defining what constitutes an 'error' in a particular setting and how to detect whether or not there is an error and what kind of error it might be. The point I want to make here, however, is that a procedure may work quite differently under routine conditions and under breakdowns.

3 Maps and Scripts

Suchman's analysis of office procedures as the *product* of the orderly work of the office, rather than the reflection of some enduring structure that stands behind that work, has been generalized in her book on *Plans and Situated Action*: 'plans are resources for situated action, but do not in any strong sense determine its course. While plans presuppose the embodied practices and changing circumstances of situated action, the efficiency of plans as representations comes precisely from the fact that they do not represent those practices and circumstances in all of their concrete detail' (1987, p. 52).

Suchman's proposition that 'plans are resources for situated action' is of fundamental importance to CSCW systems design and has served me and my colleagues as a guiding principle in the development of the concept of malleable 'coordination mechanisms' (e.g., Schmidt, 1991a; Schmidt and Simone, 1996), but it also leaves a number of questions unanswered: What is it that makes plans such as production schedules, office procedures, classification schemes, etc., useful in the first place? What makes them 'resources'? Furthermore, is it merely the fact that plans are underspecified compared to the rich multiplicity of actual action that makes them 'resources'? Is that really all there is to it? What, then, makes one procedure or form or schedule more useful than another for a certain purpose in a specific setting?

Later in the book, Suchman returns to these issues and suggests a rather apt metaphor for the role of plans, namely that of a 'map': 'Just as it would seem absurd to claim that a map in some strong sense controlled the traveler's movements through the world, it is wrong to imagine plans as controlling actions. On the other hand, the question of how a map is produced for specific purposes, how in any actual instance it is interpreted *vis-à-vis* the world, and how its use is a resource for traversing the world, is a reasonable and productive one' (Suchman, 1987, pp. 188 f.). While the same irksome questions arise here as well, the 'map' analogy is a fitting condensation of the role of procedures as understood in Suchman's study of the accounting office: procedures were found to be 'formulated in the interest of what things should come to, and not necessarily how they should arrive there' and were used as a general reference for orientation purposes, not as a prescribed sequence of actions.

Other studies, however, lead to quite different conclusions as to how formal constructs are used by actors in everyday work activities.

Consider the relatively simple case of the normal checklist. A checklist is basically a list used to organize tasks whenever it is essential that a set of actions *all* be performed, typically where it is essential that the actions of the performance also be taken in a particular order, to ensure a high level of operational safety. For example, the normal aircraft flight-deck checklist indicates a set of different tasks the pilot must perform or verify during all flight segments in order to configure the aircraft and prepare the flight crew for certain 'macro-tasks' such as ENGINE START, TAXI, TAKEOFF, APPROACH, LANDING, etc. For each one of these macro-tasks there are several 'items' to be accomplished and verified by the crew (for a study of the design and use of flight-deck ckecklists, cf. Degani and Wiener, 1990).

Like any other formal construct, the checklist does not describe the prescribed action exhaustively. Indeed, no linguistic construct can describe any action exhaustively (Garfinkel and Bittner, 1967). That said, it's clear that the flight-deck checklist does not serve in as weak a role as that of the traveller's map. The use of such checklists requires the actor to employ a strategy for sequential execution which permits him or her to ensure that the steps are done in the correct order and that each step is done once and only once. In fact, the checklist can be conceived of as an construct that has been deliberately and carefully designed to reduce local control, typically in safety-critical environments. The flight-deck checklist thus provides a 'precomputed' selection of safety-critical tasks, which all need to be performed at the particular flight segment as well as a 'precomputed' sequence for their execution (Norman, 1991, p. 21).

Consider another example which is more complex than the checklist but which is also far more pertinent to the use of formal constructs for coordinative purposes under routine as well as non-routine conditions, namely the study of the *kanban* system at Repro Equipment. The study has been described and discussed at length elsewhere (Schmidt and Simone, 1996) but a brief recapitulation is unavoidable.

A manufacturing operation involves myriad discrete parts and processes that are complexly interdependent: Each product consists of many component parts, in some cases thousands of components, and their production may require a number of different processes in a specific sequence. The different processes require specialized tools and skills which are allocated to different workstations and require hugely different set-up times. This is compounded by the fact that, at any given time, a large number of products and their components coexist in the production process at different stages of completion, which means that different parts for the same or for different products compete for the same workstations.

To deal with this complexity, Repro Equipment had introduced a *kanban* system to coordinate processes in the production of cabinets. *Kanban* is a Japanese word for 'card' which it is widely used to denote a just-in-time production control protocol[3] where a set of cards acts as the carrier of information about the state of affairs *as well as* production orders conveying instructions to initiate certain activities. The basic idea is that loosely interdependent activities can be coordinated by exchanging cards between actors. When a new batch of parts or sub-assemblies has been produced and the batch is to be transported 'down-stream' from the present work station to the station where it is to be used, e.g., as components for various sub-assemblies, a specific card is attached to the container used for the transportation. The card specifies the part number, the number of parts to be produced per batch and other relevant information. When the operator at the work station down-stream has processed this batch of parts, the accompanying card is sent back to the operator who produces these parts. To the operator, receiving the card means that he or she has now been issued a production order.

[3] The term 'protocol' is used here to denote a formal organizational construct which regulates the *coordination* of cooperative work.

The basic rules of a *kanban* protocol are as follows: (1) No part may be made unless there is a *kanban* authorizing it; (2) there is precisely one card for each container; (3) only standard containers may be used; and (4) containers should only be filled with the prescribed quantity (Schonberger, 1982, p. 224).

Setting up a *kanban* system requires a careful configuration of the number of containers per part number and the quantity per container. This configuration, in effect, amounts to a precomputation of task interdependencies in terms of batch size per part number, task allocation in terms of work stations for different part numbers, and task sequences.

However, Repro Equipment was faced with extreme differences and fluctuations in demand for different models and variants, but a *kanban* system is not adequate for coordinating manufacturing operations when faced with such fluctuations; it can only handle small deviations in the demand (Schonberger, 1982, p. 227). Accordingly, operators recurrently experienced that the configuration of the system was inadequate. For instance, in a situation where all parts of a particular part number which was only used for a special product variant had all been used, the protocol would automatically generate a production order for this part number, although the part in question probably would not be needed for several months. Unmitigated execution of the protocol in this situation would thus absorb production facilities that would be requested for other, more pressing orders.

In such situations, where the *kanban* system was 'beyond its bounds,' operators would tamper with the *kanban* protocol. For example, having heard of a new rush order from the girl in the order office, the fork lift operator might put the card for a rarely used part for another model in his back pocket or leave it on the fork-lift truck for a while. Similarly, in order to rush an order, operators would occasionally order a new batch of parts for this order *before* the container had actually been emptied and the card had been released, or they would deviate from batch sizes as specified on the card, etc. In doing that they, of course, deviated from the lexical statement of the rules but when management later was informed of these practices the reaction was an enthusiastic endorsement. In breaking the literal rules of the protocol they acted in accordance with management's reasons for adopting the *kanban* system in the first place. In the word of the CEO, 'if it weren't for these guys we'd gone bust long time ago.'

It is crucial to notice that instead of abandoning the *kanban* system altogether, or at least suspend it temporarily, the operators *changed the configuration* of the system. That is, when an operator pocketed a card, he or she was *modifying* the protocol, not switching it off, and when the card was put back in circulation again, the default configuration was in force again. The reason for this is that the *kanban* system incorporates (implicitly, in the configuration of the system) a precomputed model of crucial interdependencies of the manufacturing process (routing schemes for different parts, set-up-times for different processes, etc.). Thus, even though Repro often experienced situations where the *kanban* system was 'beyond its bounds,' it was neither discarded, nor suspended, but merely modified locally and temporarily according to the requirements of the situation.

The *kanban* system illuminates several important points.

Suchman's contention that the function of abstract representations such as plans 'is not to serve as specifications for the local interactions, but rather to orient or position us in a way that will allow us, through local interactions, to exploit some contingencies of our environment, and to avoid others' (1987, p. 188) is not accurate as far as the *kanban* system is concerned. When an operator receives a card, he or she will produce the batch as specified by the card, in accordance with the general rules of the protocol, without actively searching for reasons not to do so and without deliberating or negotiating whether to do so or not. The *kanban* protocol does not exhaustively describe and prescribe action—no linguistic construct does—but it nonetheless generates specifications for the local interactions. Workers at Repro Equipment rely on the *kanban* protocol to issue valid and sensible production orders, unless they have strong reasons to believe that its unmitigated execution in the particular situation will have undesirable results. Even then, they do not discard or suspend the system but alter its behavior by reconfiguring it, after which the system is allowed to 'switch back' to the default configuration.

The *kanban* system thus determines action in a far stronger sense than the map of a traveller determines the traveller's movements (Suchman, 1987, p. 188 f.; Bucciarelli, 1988b, p. 114). In the *kanban* case the protocol conveys a *specific* stipulation in the form of a production order to the particular actor instructing the actor, under the conditions of social accountability, to take the particular actions specified by the card according to the general rules laid down in the protocol. It is more like a *script* than a *map*. In fact, the *kanban* system works well even though it does not provide a 'map' in the form of a visible overview of interdependencies among processes.

The point is that the *kanban* protocol under normal conditions of operation relieves actors of the otherwise forbidding task of computing myriad—partly interdependent, partly competing—production orders and negotiating their priority. They can as competent members, for all practical purposes, rely on the precomputed protocol to issue valid production orders; they take it for granted. Though the relation of plan to action, according to Suchman, can be construed as 'enormously contingent' (1987, p. 38), then this is not necessarily so to competent members. Thus, for a worker at Repro Equipment to contemplate the meaning and rationality of the protocol at every step in every situation would be an utter waste of effort, and it does not happen. In Wittgenstein's words: 'When I obey a rule, I do not choose. I obey the rule *blindly*.' (Wittgenstein, 1945–1949, § 219).[4]

But the relationship between the formal construct serving as a script (in this case, the general *kanban* protocol as it was instantiated in a particular configuration and distribution of cards) and actual observable practice is not *causal*: the operators deviated from the lexical statement of the rules when they found that to be appropriate and such putatively illicit practices were actually deemed to be competent and responsible. So, while contradicting Suchman's contention that the function of

[4]For discussions of Wittgenstein's analysis of the use of rules, cf. Lynch (1993) and Shanker (1987a)

plans is merely 'to orient or position us in a way that will allow us, through local interactions, to exploit some contingencies of our environment, and to avoid others', the case does not contradict Suchman's more general observation that formal organizational constructs such as procedures are of a different nature than computational procedures and algorithms (Suchman, 1983, p. 322).

Formal organizational constructs in the form of scripts are not *causal schemes* but should rather be thought of as *normative constructs* based on a precomputation of interdependencies. A script offers a limited selection of safe, secure, legal, valid, advisable, efficient or otherwise prescribed 'moves' while excluding 'moves' that generally would be considered unsafe, etc. Whether or not a particular option is actually deemed feasible or infeasible in a particular situation under certain practical circumstances is a discretionary matter. But such discretion is not exercised in each and every situation. Actors are not endlessly reflexive (Heritage, 1984, p. 118). Under conditions of limited resources, practical exigencies, and social accountability they rely on the stipulations of the script, if one is at hand, in order to get the job done, unless they have good reasons not to do so.

The contradictory findings thus indicate that protocols play very different roles in cooperative work. They may, on one hand, as suggested by Suchman and others, play the weak role of the 'map' of the traveller that offers a codified representation of salient features of past and future actions which actors may consult as a referent. On the other hand, however, they may play the strong role of a 'script' that offers a precomputation of interdependencies among activities (options, required actions, sequential and temporal constraints, etc.) which, at critical points, provides instructions to actors of possible or required next steps. The characteristics of formal constructs, be it maps or scripts, can be summarized as follows:

(1) Both maps and scripts represent constraints and affordances. As pointed out by Suchman, the 'emergent properties of action' do not mean that action is 'random' In order to understand the observable regularities in action, she argues, one should investigate the 'relationship between structures of action and the resources and constraints afforded by physical and social circumstances' (1987, p. 179). We can here add formal organizational constructs as representations of 'the resources and constraints afforded by physical and social circumstances'. That is, formal constructs—maps as well as scripts—stand proxy for the affordances and constraints of the physical and social environment. For all practical purposes, they thereby circumscribe action in the same way as the physical and social circumstances. More specifically, as far as coordinative protocols are concerned, such protocols convey a precomputation of task interdependencies which assists actors in reducing the complexity of coordinating their activities.

(2) Whether weak or strong, a protocol only conveys stipulations within a certain social context, within a certain community, in which it has a satisfactorily certain and agreed-to meaning and it only does so under conditions of social accountability.

(3) Whether a formal construct serves as a map or a script depends on the extent to which it is possible to identify, analyze, and model interdependencies in

advance. Moreover, the role of a particular protocol may vary according to the situation. Thus, in a situation where a standard operating procedure does not apply, the procedure may merely serve in its weak default capacity as a vehicle of conveying heuristics (as, for instance, in the recovery from error in the accounting office). In other cases, however, such as the *kanban* case, the role of the protocol does not vary in the face of contingencies; rather, because of the complexity of the interdependencies of discrete parts production, the *kanban* protocol is not discarded, suspended, or 'weakened' but merely temporarily reconfigured by the operators to accommodate the passing disturbance.

(4) Protocols and other formal constructs cannot exhaustively describe action. As pointed out by Suchman, procedures are characterized by 'inherent and necessary under-specification [. . .] with respect to the circumstances of particular cases' (1982, p. 411). Furthermore, Suchman observes, 'the vagueness of plans' is 'ideally suited to the fact that the detail of intent and action must be contingent on the circumstantial and interactional particulars of actual situations' (1987, pp. 185 f.). In fact, the degree of vagueness of specific plans is itself contingent (1987, p. 188). Thus, not only is a protocol, as a linguistic construction, inherently vague compared to the infinitely rich details of actually unfolding activities, and not only is it inherently decontextualized, but it is deliberately under-specified with respect to (a) factors that are immaterial for the purpose of the given protocol or (b) factors that can more efficiently and effectively be left unspecified, typically until a later stage. The protocol must be defined at 'an appropriate level of ambiguity' (Bowker and Star, 1991, p. 77).

(5) 'No representation of the world is either complete or permanent' (Gerson and Star, 1986, pp. 257–258). That is, weak or strong, the protocol will, inevitably, encounter situations where it is beyond its bounds, its inherent vagueness and appropriate ambiguity notwithstanding.

4 The Crucial Role of Artifacts

Formal constructs would be of only marginal utility if they were not *inscribed upon artifacts*. In the coordination of cooperative work (to stick to my main concern here) the role of the artifact is, fundamentally, to give permanence to the protocol for which it stands proxy in the sense that it conveys the stipulations of the protocol in a situation-independent manner. As observed by Jack Goody, 'The written language [reaches] back in time' (Goody, 1987, p. 280). Written artifacts can at any time be mobilized as a referential for clarifying ambiguities and settling disputes: 'while interpretations vary, the word itself remains as it always was' (Goody, 1986, p. 6). They are, for all practical purposes, unceasingly publicly accessible.

Due to these characteristics of the written language, symbolic artifacts play a crucial role as mediators of the coordination of cooperative work. In the case of a standard operating procedure or a checklist, the state of the artifact is completely static, irrespective of the state of the execution of the protocol it prescribes. Even when the protocol is used as a script (actors are following the instructions of the procedure or the items of the checklist step by step), it is entirely up to the actor to produce and maintain the required dynamic representation of the state of the protocol with respect to the unfolding cooperative activities.

In other cases, however, the state of the artifact changes according to the changing state of the protocol. Consider the case of the bug report form mentioned briefly above (Carstensen, 1996; Carstensen and Sørensen, 1996). As an artifact, it is a simple paper form. The agreed-to protocol dictates that when a new bug is detected by anyone involved in testing the software, a new bug report is initiated and filled-in. The originator of the bug report also provides a preliminary description and diagnosis of the problem. Three designers acting as a so-called 'spec-team' then determine which module might be culpable; they specify the date when the bug should be corrected, and classify the bug according to its perceived severity. The bug report is then passed on to the relevant designer who is then responsible for the correcting 'his' or 'her' bugs and for reporting back to the designer who will be responsible verifying and integrating the many software modules.

This mundane example shows how a protocol and the corresponding artifact supplement each other: Firstly, the form is transferred from one actor to another and this *change of location* of the artifact in itself conveys, to the recipient, the stipulations of the protocol in a specified form, that is, the change of location transfers to the particular actor the specific responsibility of taking such actions on this particular bug that are appropriate according to the agreed-to protocol and other taken-for-granted conventions. Secondly, at each step in the execution of the protocol, *the form is annotated* and the updated form retains and conveys this change to the state of the protocol to the other actors—the state of each reported bug is thus reflected in the successive inscriptions on the form made by different actors. That is, a change to the state of the protocol induced by one actor (a tester reporting a bug, for example) is conveyed to other actors by means of a visible and durable change to the artifact. The artifact can thus be said to provide a 'shared space,' albeit a space with a structure that reflects salient features of the protocol. Furthermore, the change is propagated within the ensemble according to the stipulations of the protocol, and the state of the total population of reported bugs is publicly visible in a public repository of bug forms.

In such cases, the artifact not only stipulates articulation work (like a standard operating procedure) but *mediates* articulation work as well in the sense that the artifact acts as an intermediary between actors which conveys information about state changes to the protocol. Because the artifact mediates the changing state of the protocol among the actors, it not only conveys the general stipulations of the protocol but *specifies the stipulations* in the sense that the individual actor is instructed that it is he or she that has to take this or that specific action at this particular point in time.

An artifact is, of course, not 'just an empty space,' as the song goes. It has a specific *material format* which is formed to serve the purpose of conveying the specific stipulations of the protocol within a particular context. For example, consider the simple checklist again. As noted, the use of the checklist requires the actor to employ a strategy for sequential execution which permits him or her to ensure that the steps are done in the correct order and that each step is done once and only once. The material format of the checklist may be of assistance to the actor in ensuring this: 'The fixed linear structure of the checklist permits the user to accomplish this by simply keeping track of an index that indicates the first unexecuted (or last executed) item. Real checklists often provide additional features to aid in the maintenance of this index: boxes to tick when steps are completed, a window that moves across the checklist, etc.' (Hutchins, 1986, pp. 47 f.; cf. also Norman and Hutchins, 1988, p. 9). In a similar vein, Goody, in a discussion of the specific affordances provided by the material format of written text, observes that writing introduces certain spatio-graphic devices such as lists, tables, matrices by means of which linguistic items can be organized in abstraction from the context of the sentence and points out that the spatio-graphic format of an artifact can stipulate behavior by reminding an actor of items to do and directing attention to missing items: 'The table abhors a vacuum' (Goody, 1987, p. 276; cf. also Suchman and Trigg, 1991; Suchman, 1993b).

These comments are far from conclusive, they are not intended to either. They are rather meant to indicate a crucial and fertile area of CSCW research. There have been several attempts at investigating the use of artifacts for coordinative purposes in CSCW (e.g., Harper et al., 1989b; Suchman and Trigg, 1991; Rogers, 1993; Schneider and Wagner, 1993; Suchman, 1993b; Carstensen and Sørensen, 1996; Grinter, 1996a; Potts and Catledge, 1996; Schmidt and Simone, 1996; Symon et al., 1996). But we are far from a grounded understanding of the role of formal constructs in cooperative work that can serve as a conceptual foundation for the design of CSCW systems that support the regulation of cooperative work. To get there, it is essential to investigate how artifacts are used as objectifications of coordinative protocols and how the material format of such artifacts support that role.

5 Conclusions

I have tried to demonstrate that the prevalent understanding in CSCW of the status of formal constructs in cooperative work is problematic. The empirical evidence for the received understanding is not as robust as we may have believed and there is evidence from other studies that indicates that formal constructs are not always as feeble and ephemeral as we may have taken for granted. There are good reasons to believe that formal constructs incorporated in computer artifacts may be quite helpful in reducing the complexity of coordinating cooperative activities and thus serve as genuine 'resources for situated action.' We are thus in a situation in which a reconsideration of the premises of much of the research in CSCW is called for. Most importantly, we need to investigate—thoroughly, systematically, and critically—the actual use of formal constructs and the artifacts in which they are objectified. In short, we've got a lot of work to do.

Chapter 7
The Critical Role of Workplace Studies in CSCW (2000)

While there is no question that workplace studies play a prominent role in computer-supported cooperative work or CSCW, the exact nature of this role has been a subject of much reflection and debate over the years. So far, the deliberation has been inconclusive, and, moreover, in the last few years a certain sense of disillusionment and even skepticism has arisen concerning the ways in which and the extent to which such studies in fact contribute to CSCW systems design.

Plowman et al. (1995), for example, have raised the question 'what are workplace studies for?' To investigate this issue they undertook a survey of a large part of the workplace studies published in the area of CSCW—altogether 75 papers—and found what they called a 'paucity of papers detailing specific design guidelines' (p. 313). While they hesitated to conclude that 'workplace studies do not produce specific design guidelines', they did feel confident that the observed paucity 'can be attributed to the lack of reported research which has developed to the stage of a system prototype' (ibid.). Discussing these observations, Plowman et al. surmised that the reason for the apparent failure to bridge the gap is 'a big discrepancy between accounts of sociality generated by field studies and the way information can be of practical use to system developers' (p. 321).

While agreeing with the characterization of the state of affairs advanced by Plowman et al., Bob Anderson has challenged their tentative explanation, arguing that the issue of how ethnographic findings are formatted is a distraction; ethnography can be highly formal when *that* is appropriate for the research program at hand (Anderson, 1997). Instead, Anderson argued that the problem has deeper roots. Observing that not all kinds of qualitative studies of social life in the 'real world' are ethnographies and that the idea of ethnography 'as a method for the specification of end-user requirements for systems' is 'predicated in a misunderstanding of ethnography's role in social science', he stated flatly that 'designers do not need ethnography to do what they wish to do' (Anderson, 1994, p. 153):

Kjeld Schmidt: 'The critical role of workplace studies in CSCW,' in P. Luff, J. Hindmarsh, and C. C. Heath (eds.): *Workplace Studies: Recovering Work Practice and Informing System Design*, Cambridge University Press, Cambridge, 2000, pp. 141–149.

> designers may well work closely with users, engage in fieldwork among the end-user organizations for whom they are designing; and focus on the intersection of the technological, the organizational, and the social dimensions of the working environments within which their designed systems will find a place, all without ever engaging in the kind of analytic ethnography [. . .] found in the social sciences. In fact, doing ethnography may prove a barrier to achieving the goals that designers want to set themselves. (Anderson, 1994, p. 155, Emphasis deleted)

While Anderson's observations that not all kinds of qualitative studies of social life in the 'real world' are ethnographies and that ethnography cannot serve as a requirements analysis methodology are topical and appropriate, he did not get to what I consider the root of much of the confusion, namely the mix-up of two distinct questions: (a) the role of workplace studies of particular settings with a view to the design of specific CSCW systems for the same or similar settings, that is, the role of workplace studies as a requirements analysis method; and (b) the role of workplace studies of particular settings as contributions to the development of the conceptual foundation for CSCW and, thereby, to the development of CSCW technologies. While workplace studies in both roles might be said to contribute to 'systems design', albeit in very different senses and through quite different mechanisms, the latter role is critical whereas the former is highly problematical.

Firstly, let me address the role of workplace studies in the development of the conceptual and technological foundation of CSCW.

Cooperative work is a tricky phenomenon. We are all engaged in cooperative activities of various sorts in our everyday lives and routinely observe others working together around us. We are all experts from our everyday experience. And yet this quotidian insight can be utterly misleading when applied to the design of systems to support cooperative work.

As participants of a cooperative effort we routinely take its orderly accomplishment for granted. We have to do that, in order to get the job done. In depending on the activities of others, we are 'not interested' in the enormous contingencies and infinitely faceted practices of colleagues, unless these may impact on the our own work (cf. Schütz, 1943, 1953, 1967). An actor will thus routinely expect not to be exposed to the myriad detailed activities by means of which his or her colleagues deal with the contingencies they are facing so as to ensure that their individual contributions are seamlessly articulated with the other contributions. Conversely, an actor will routinely avoid to publicize those contingent practices which colleagues do not 'need to know', not only in order to appear competent in the eyes of colleagues and managers, but also and more importantly in order to not to add to the complexity of the work of his or her colleagues. The individual activities of cooperating actors are *made to appear as if* they are seamlessly integrated and meshed. Only disclosing those aspects of the work required to articulate the distributed and yet interdependent activities which are relevant to the concerns of colleagues—that is, knowing what to make publicly visible and what *not* to make publicly visible in a given situation—is a crucial aspect of competent conduct in any cooperative work setting. Just like illusionists and acrobats strive to make their acts appear as if performed effortlessly, cooperating actors strive to 'dampen the noise' from the contingencies of their own work and from the concomitant efforts of articulating their own activities with the other contributions to the joint endeavor by skillfully modulating which aspects of their work are made visible, and how, and which aspects are performed such that they are inconspicuous to colleagues.

The notion of orderliness which cooperating actors take for granted and have to take for granted and which they, in turn, convey to colleagues through the way they make publicly relevant aspects of their own local affairs publicly accessible and visible, is not an illusion or some kind of 'false consciousness'. The mutual projection of order is rarely deceptive to competent members. It reflects the fact that myriads of cooperative activities usually are accomplished, integrated, meshed, articulated successfully, day in and day out, and it reflects this fact perfectly adequately by 'escamotating'[1] the detailed practices by means of which this orderliness is achieved. It is rather a necessary simplification, indispensable for us to be able to cope with the routine complexities of our daily work.

The problem arises when the categories in which these notions are generalized as common-sense constructs (e.g., 'task,' 'goal,' 'shared,' 'context,' 'role,' 'procedure,' 'team,' 'organization') are used uncritically beyond the realm of everyday work. It may for example make a lot of sense to refer to a 'shared goal' in a particular setting, for instance if one actor has asked the other participants in a meeting 'Do we all agree this is what we want to do?' and they have nodded their consent. While the category of a 'shared goal' can be seen to escamotate the ways in which the members arrange the multiple, partially dissonant, motives and interests into a workable compromise and handle the unavoidable indications of continual discord and diverging interpretations of the compromise, competent members of the particular setting know the extent to which and the sense in which the 'goal' is 'shared'. But if a joint effort—for other purposes, e.g., for the purpose of sociological theory or for the design of organizational information systems—is conceived of as constituted by a 'shared goal,' the notion of a 'shared goal' becomes utterly misleading.[2] Thus, in his studies of the engineering design process as it unfolds within design projects, Louis Bucciarelli found that

> different participants in the design process have different perceptions of the design, the intended artifact, in process. [...] The task of design is then as much a matter of getting different people to share a common perspective, to agree on the most significant issues, and to shape consensus on what must be done next, as it is a matter of concept formation,

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[1]From the French 'escamoter', to remove something diligently and surreptitiously, normally used to denote the skilled practices of illusionists and conjurers.

[2]For a brilliant example, cf. Sabbagh: 'Each person working on Worldwide Plaza had a different goal: for a bricklayer, during 1987, to see the gleaming, soft-beige-and-rose expanse of crisply laid brick reach up to 600 feet; for a steel fabricator in Houston, to see 19,000 tons of steel erected into a soaring framework of complex ellipses and sturdy rectangles; and for the developers, to seen an investment that would transform the West Side of New York, and bring profits for decades to come.' 'Linked to any major construction project are men and women with every type of personality, intellect, and qualification. Scientists and engineers, welders and electricians, artists and writers, salesmen and real-estate brokers, accountants and bankers, canteen managers and dynamite experts, seismologists and calligraphers—all feeling entitled to think of a building as "their" building in the same way as the architect or the principal developer. This possessiveness can be a driving force behind each craftsman and his task. It can lead to the excitement of competition, as the mason, the waterproofer, and the window installer will the steel erector to complete *his* stage in the building to make *their* work possible.' (Sabbagh, 1989, pp. 1–3).

> evaluation of alternatives, costing and sizing—all the things we teach (Bucciarelli, 1984, p. 187)

That is, the 'shared goal' is not there in advance; it is constructed by the members in the course of the project, and it is in the process of agreeing to a 'shared goal' that the designers arrive at an agreed-to design. When the designers have a 'shared goal', they have—for all practical purposes—finished the design task. In fact, they may not even agree on anything but the design when they finish; agreeing on a 'shared goal' may require additional effort and participants may simply decide, tacitly, that it is not worthwhile: 'Design decision in this instance is best seen as an overlay of interests rather than their synthesis within some flat, cognitive domain' (Bucciarelli, 1988a).

Similarly, the notion of 'shared knowledge,' which spontaneously crops up in CSCW contexts, ignores the work required to make knowledge 'shared': determining the adequate level of abstraction for a given purpose, eliminating aspects of less relevance to the intended audience and formatting according to the expected use situation, providing indexation, etc. (cf., e.g., Bowker and Star, 1991). Even such ubiquitous and seemingly innocuous categories as 'task' and 'collaboration' are problematic, in that they introduce a conceptual separation of 'individual' and 'collective' which, at closer inspection, turns out to be misleading since 'seemingly individual and specialized work tasks are produced with respect to the actions of colleagues' (Heath and Luff, 1996, p. 97).

In order to develop computer-based technologies which can enhance the ability of actors to accomplish their cooperative endeavors we cannot take the orderliness of cooperative work for granted. On the contrary, we need to go beyond the common-sense notions of everyday working life. We need to understand *how* orderliness is accomplished in cooperative endeavors; we need to uncover the practices through which the myriad distributed and yet interdependent activities are meshed, aligned, integrated, because *it is the very practices through which such orderliness is accomplished that must be supported*. The primary role of workplace studies in CSCW is thus to dismantle the common-sense conceptions of cooperative work, take them apart, unpack and disclose the hidden practices of articulation work, and thus give us access—analytically and conceptually—to the intricate ways and means of the production of social order in cooperative activities. This role is critical in the sense that it is crucial, but it is also critical in the Marxian sense of uncovering the social practices through which categories that are otherwise taken for granted are produced as necessary 'thought forms' and thereby determining the boundaries of the validity of these categories.[3]

And indeed, those workplace studies that have had the strongest influence on CSCW research have been studies which did not aim at arriving at specific design recommendations for specific systems but instead tried to uncover, in minute detail, the ways in which social order is produced in cooperative work settings, whatever the design implications of the findings might be.

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[3] Cf. the subtitle of Marx' *Capital: Critique of political economy*.

In this respect the studies of office work conducted by Suchman and Wynn almost two decades ago are exemplary. They undertook to demonstrate empirically that the conception of 'office work' then prevailing among managerial ideologists, designers of 'office automation' systems, office equipment vendors, etc. were misleading. In particular, they subjected the common-sense presuppositions about the status of office procedures vis-à-vis the actual course of action to a critical analysis and demonstrated that office procedures do not determine action causally; they could thereby show that the design visions of the office automation movement were misguided (Wynn, 1979; Suchman, 1982, 1983; Suchman and Wynn, 1984). In doing so, they were highly influential in shaping the agenda of the research program which a few years later became CSCW.

Since then, workplace studies have had and continue to have profound impact on the development of CSCW technologies. Not in the form of a direct relationship of 'requirements specification' with respect to the design of specific systems, but by contributing to the conceptual foundation of CSCW. Most significantly, a series of studies such as the Lancaster study of air traffic control (e.g., Hughes et al., 1988; Harper et al., 1989b, 1991; Harper and Hughes, 1993) and the study of the London Underground control room (Heath and Luff, 1992a, 1996) have made the CSCW community understand the delicate interplay of individual and cooperative activities and appreciate the crucial role of 'awareness' in ensuring that individual activities are seamlessly integrated. This have incited and inspired computer scientists to explore ways in which the production of awareness in cooperative ensembles can be supported in CSCW systems through 'shared object servers' (e.g., Rodden and Blair, 1991; Rodden et al., 1992; Trevor et al., 1995), awareness models (e.g., Rodden, 1996; Benford and Greenhalgh, 1997; Sandor et al., 1997; Simone and Bandini, 1997), and so forth. Other areas of CSCW research can tell similar stories of how workplace studies have informed the development of CSCW technologies. For instance, ethnographic and other in-depth workplace studies have played a crucial role in the development of the concept of 'computational coordination mechanisms' and of the corresponding software environment (Simone et al., 1995b; Schmidt and Simone, 1996; Simone and Schmidt, 1998).

That is, the observed 'paucity of papers detailing specific design guidelines' (Plowman et al., 1995, p. 313) does not reflect on the *relevance* to CSCW of ethnographic or other in-depth workplace studies informed by sociological programs such as ethnomethodology or symbolic interactionism. Nor does it, in fact, reflect on the actual impact of workplace studies on the development of CSCW technologies.[4] That is, 'designers' of novel CSCW technologies—as opposed to applications of existing technologies to the requirements of specific settings—indeed do need

[4]Notice that the trails of this impact—the histories of how workplace studies inform the development of CSCW technologies—is not always readily visible in papers reporting on findings from workplace studies. The transfer of findings and insights typically happens in the course of discussions within cross-disciplinary research teams and are often only documented in design-oriented papers.

ethnography and other sociologically informed kinds of workplace studies 'to do what they wish to do'.

Instead, I will suggest that the paucity of specific design guidelines reflects (1) on the state of CSCW technology and (2) on a lack of appreciation of how radical the CSCW program really is.

(1) Conducting a requirements analysis presumes a mature and reasonably understood technology. The analyst investigates a particular work setting or a set of settings in a particular work domain in order to determine if a given family of technologies might be usefully deployed, to determine which aspects of the work activities in the domain would benefit most from computerization, and to sketch a design. Without knowing the general characteristics of the potential technologies, the analyst would be faced with an infinite space of possibilities and would in fact, in order to give specific guidelines or recommendation, be expected to develop the new technologies more or less from scratch.

In terms of technology, CSCW has a long way to go. Discussing the state of CSCW technologies in any kind of detail is, of course, completely beyond the scope of a brief set of comments on the role of workplace studies. Allow me to mention one point, however, just to illustrate the situation: As pointed out by foundational CSCW workplace studies such as the ATC study and the London Underground study, cooperative and individual activities are inextricably interwoven in daily work practice, and a CSCW system should thus support a fluent and seamless meshing of individual work and cooperative work. However, current operating systems are basically designed to support work conceived of as individual work. They do not provide facilities for supporting the articulation of cooperative activities with respect to the shared data structures and functionalities as represented by applications. Thus, although CSCW facilities supporting mutual awareness and adaptation (monitoring the activities of colleagues, making one's work appropriately visible to colleagues, directing attention to anomalies, etc.) are orthogonal to applications such as word processors, spreadsheets, and drawing tools, CSCW designers attempting to build shared work spaces are forced to incorporate such facilities in the domain-specific data-structures and functionalities, i.e., in applications. As a result, users are suddenly faced with 'individual' as well as 'cooperative' word processors, spreadsheets, drawing tools, etc. and an impedance is consequently created between individual and cooperative activities. CSCW facilities providing 'shared work spaces' should not be conceived of as applications or be implemented as part and parcel of applications but as extended operating system functions that can be accessed from and combined with, in principle, any application. Otherwise the delicate and dynamic relationship between cooperative and individual work breaks down. (For an attempt to outline the implications of workplace studies for the architecture of a CSCW software environment, cf. Schmidt and Rodden, 1996).

In the absence of appropriate computing environments—and I have indicated only one example of many equally fatal deficiencies—it is no wonder if workplace studies do not result in *specific* design recommendations or CSCW prototypes for specific settings. We are still in the murky prehistory of CSCW, and there is a long way to travel until environments that support articulation work fairly adequately become available. Until then, there will remain a big discrepancy between accounts of sociality generated by field studies and the way information can be of immediately practical use to system developers.

However, while CSCW technology is still far from mature, important practical steps in the development of CSCW technologies are of course being taken in the form of experimental systems, sometimes developed as attempts to explore possibilities of supporting certain modes of interaction (Ishii, 1990; Ishii et al., 1992; Fuchs et al., 1995; Fitzpatrick et al., 1996; Roseman and Greenberg, 1996), sometimes to explore the feasibility and limitations of certain existing technologies for CSCW purposes (e.g., media spaces, workflow technology, hypermedia, etc.) in particular work settings (e.g., Shepherd et al., 1990; Grønbæk and Mogensen, 1997) or more generally (e.g., Heath and Luff, 1991b, 1992b; Heath et al., 1995b), and sometimes even to solve very practical problems in particular work settings (e.g., Pougès et al., 1994). In any case, these experimental systems inevitably support only certain modes of interaction and thus provide quite limited support for articulation work. These unavoidable limitations notwithstanding, the experiments provide indispensable insights, not only in the advantages and problems with applying those technologies for CSCW purposes, but also often—when the experience is carefully documented—in the (perhaps unforeseen) problems that can arise when such technologies are introduced in the social organization of work.

In the development of experimental CSCW systems, designers often—as pointed out by Anderson—work closely with users and engage in fieldwork in the settings for which they are designing; they may even invite sociologists and psychologists to assist in investigating the setting and evaluating the system and its impact. In these cases, however, the objectives of the experiment are clearly defined and the technological options identified and bounded in advance.

Thus, while requirements analysis—in line with other ways of developing requirements such as user participation in design—plays an important role in the development of experimental CSCW systems that investigate the applicability of specific technologies for specific aspects of articulation work, the impact of this kind of requirements engineering is limited by the fundamental inadequacies of existing software environments for CSCW purposes.

(2) Ironically, however, when the new technology eventually matures and the adequate software environments become available, to a large extent due to the long-term impact of sociologically inspired workplace studies, it may very well turn out that this technology does not leave much room for requirements analysis as a distinct kind of activity which requires specialized qualifications. In the 1980s much attention was paid to developing a methodology for requirements analysis of 'office work'. Most of that effort was made redundant with the development of modern graphical user interfaces and inexpensive 'shrink wrapped' software. As a result, contemporary users do not need to hire experts to conduct a requirements analysis and devise a requirements specification to configure, for instance, a Macintosh.

In fact, a radical conception of CSCW and CSCW systems argues that a CSCW system should provide an environment that supports users in designing and manipulating the coordination mechanisms that are appropriate for the particular setting (Schmidt, 1991a; Kaplan et al., 1992; Malone et al., 1992; Ellis et al., 1995; Schmidt and Simone, 1996). In a similar vein, Bentley and Dourish (1995) have suggested that a CSCW system should be seen 'as one whose behavior can be adapted through high-level customization to meet the needs of its users' (p. 134). From this perspective, they argue, in-depth requirements analyses will no longer be necessary in order to design effective systems to support cooperative work.

That is, if the radical program in CSCW proves realistic, and I for one is convinced it will, the conventional notion of the product life cycle as constituted by distinct stages defined by the involvement of different professionals—'requirements analysis', 'design', 'use', 'evaluation', 'maintenance', and 'redesign' etc.—will not be adequate for the design of CSCW systems.

In sum, then, the role of workplace studies in CSCW is crucial and critical: to dismantle prevalent common-sense notions of cooperative work by uncovering how orderly cooperative work is routinely and inconspicuously accomplished. On the other hand, there does not seem to be much room for workplace studies—e.g., ethnographies—in the design of specific CSCW systems, in part because the technology is not mature yet and requirements analysis therefore as yet is a problematic undertaking, and in part because CSCW represents a radical technology in which requirements analysis may eventually turn out to be gratuitous anyway

Chapter 8
The Problem with 'Awareness' (2002)

At a very early stage in the course of CSCW, it became evident that categories such as 'conversation' or 'workflow' were quite insufficient for characterizing and understanding the ways in which cooperative work is coordinated and integrated. It quickly became obvious that cooperating actors somehow, while doing their individual bits, take heed of the context of their joint effort. More specifically, the early harvest of ethnographic field studies in CSCW (e.g., Harper et al., 1989a, b; Heath and Luff, 1991a) indicated that cooperating actors align and integrate their activities with those of their colleagues in a seemingly 'seamless' manner, that is, without interrupting each other, for instance by asking, suggesting, requesting, ordering, reminding, etc. others of this or that. As a placeholder for these elusive practices of taking heed of what is going on in the setting which seem to play a key role in cooperative work, the term 'awareness' was soon adopted.

Not surprisingly then, the concept of 'awareness' has come to play a central role in CSCW, and from the very beginning CSCW researchers have been exploring how computer-based technologies might facilitate some kind of 'awareness' among and between cooperating actors.

For many years, a significant effort was devoted to exploring the potential benefits of 'media space' technologies for these purposes, i.e., an array of continually open computer-integrated audio-video links between dispersed actors (e.g., Mantei et al., 1991; Gaver et al., 1992; Bly et al., 1993). It was presumed that audio and video would offer a 'rich' medium through which actors could interact in ways that were 'virtually' as unrestricted and fluently as ordinary face-to-face encounters and that the mere provision of a 'media space' between actors in different locations would enable them to cooperate approximately as if they were in the same physical space. Unfortunately, however, the expected benefits from these technologies never materialized. Something was obviously amiss in the understanding of 'awareness' underlying this line of research (cf., e.g., Gaver, 1992; Heath and Luff, 1993).

Kjeld Schmidt: 'The problem with "awareness": Introductory remarks on "Awareness in CSCW",' *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002, pp. 285–298.

K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1_8,

In response to these experiences as well as to findings emerging from the growing body of ethnographic studies of cooperative work, the research focus has shifted away from face-to-face interaction as the presumed paradigm of human interaction.

Abandoning the assumption that the 'high fidelity' presumably offered by video is necessarily crucial to 'awareness', and arguing that it may even be problematic due to the discontinuous nature of media spaces, other researchers began to explore whether and how 'collaborative virtual environments' could facilitate interactions in artificial but continuous environments (e.g., Benford et al., 1994a). While impressive technical progress has been made, it is still not clear if and how these technologies could be used productively in cooperative work settings.

A related important body of work takes the situated and distributed character of cooperative work as the point of departure and abandons the communicational approach altogether. Instead this research attempts to develop *computational environments* based on 'event propagation mechanisms' or similar for collecting, disseminating, and integrating information concerning cooperative activities (e.g., Fuchs et al., 1995; Syri, 1997; Prinz, 1999) or more comprehensive 'awareness models' based on a spatial metaphor (e.g., Rodden, 1996; Sandor et al., 1997) or the reaction-diffusion metaphor (e.g., Simone and Bandini, 1997).

Whether or not these approaches will ultimately succeed is of course uncertain, but for serious progress in these directions to be made, however, major conceptual issues need to be addressed. Which aspects of the world of work and interaction should feature in these computational environments? Which objects and events and at which level of abstraction and aggregation? How should computational environments and material environments interface and interact? How can computational environments facilitate embodied action and interaction?

The papers collected in this special issue on 'Awareness in CSCW' all contribute to the investigation of these issues in a variety ways, by 'unpacking' and identifying the variegated practices that have been thrown together under the label 'awareness' (Chalmers, 2002; Heath et al., 2002; Mark, 2002; Robertson, 2002); by systematic conceptual analysis of the 'elements of knowledge' at play in 'workspace awareness' (Gutwin and Greenberg, 2002); by exploring new 'sensation and interaction possibilities' offered by for instance wearable computational devices (Gaver, 2002); by studying novel mechanisms to 'produce, gather and redistribute information from everyday activities' (Fitzpatrick et al., 2002); and by exploring a radically distributed computational model of awareness based on the 'reaction-diffusion metaphor' (Simone and Bandini, 2002).

At the very same time, however, the role of the term 'awareness' as a placeholder is coming under increasing strain as the term is being used in increasingly diverse ways (Robertson, 2002). The signs of the tensions are clear.

The term 'awareness' is obviously found ambiguous and unsatisfactory. The term 'awareness' of course refers to actors' taking heed of the context of their joint effort. But this is hardly a concise concept by any standard. CSCW researchers are obviously far from confident with using the term and thus often use the term in combination with different adjectives, e.g., 'general awareness' (Gaver, 1991; Bly et al., 1993, p. 29), 'collaboration awareness' (Lauwers and Lantz, 1990), 'peripheral awareness' (Gaver, 1992; Bly et al., 1993, p. 34; Benford et al., 1994a), 'background awareness' (Bly et al., 1993, p. 34), 'passive awareness' (Dourish and Bellotti, 1992, p. 107; Dourish and Bly, 1992, p. 541), 'reciprocal awareness' (Fish et al., 1990; Schmidt, 1994b; Robertson, 1997, pp. 19–21, 155–158), 'mutual awareness' (Benford et al., 1994a; Schmidt, 1994b; Rønby Pedersen and Sokoler, 1997), 'workspace awareness' (Gutwin, 1997; Gutwin and Greenberg, 1999, 2002), etc. The proliferation of adjectives is a clear indication that the term 'awareness' is found to be equivocal, that researchers are aware that the term is being used in significantly different ways, and that it is in need of some qualification to be useful.

In fact, the term 'awareness' is being used in increasingly contradictory ways. For example, while 'awareness' initially was adopted to denote those practices through which cooperative activities are somehow tacitly and unobtrusively aligned and integrated, some researchers are now using the very same term to conceptualize even the use of instant messaging systems where an actor's deliberately typing and sending a message that then interrupts the flow of activities of other actors is taken as instances of 'awareness' (Nardi et al., 2000).

In short, it is becoming increasingly clear that the term 'awareness' does not denote a set of related practices. In fact, it is hardly a concept any longer.

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The very word 'awareness' is one of those highly elastic English words that can be used to mean a host of different things. Depending on the context it may mean anything from consciousness or knowledge to attention or sentience, and from sensitivity or apperception to acquaintance or recollection.[1] One will for instance come across expressions such as 'alcohol awareness', 'cancer awareness', 'cult awareness', 'spiritual awareness', 'pagan awareness', and so forth. It goes without saying that these different uses of 'awareness' refer to quite different social and ideological phenomena. For instance, one would think that 'awareness' as in 'cult awareness' implies a radically different stance and practice than 'awareness' as in 'pagan awareness', and that 'awareness' as in 'spiritual awareness' has little in common with 'awareness' as in 'alcohol awareness' and that the latter is not the same as one's 'awareness' of acute thirst.

The first step towards some kind of conceptual clarity is to realize with the philosophers, from Husserl and Schutz to Wittgenstein and Ryle, that it does not make sense to conceive of 'awareness' as such, i.e., as a distinct (mental) entity. That is, the term 'awareness' is only meaningful if it refers to a person's awareness *of* something.

The noun form is misleading. In that respect 'awareness' is akin to 'intelligence', 'efficiency,' 'carefulness', etc. Awareness is an attribute of action. Doing one thing

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[1] According to Webster, the noun 'awareness' generally has two broad meanings: (1) *consciousness, cognizance, knowingness: having knowledge of*: 'he had no awareness of his mistakes'; 'his sudden consciousness of the problem he faced'; 'their intelligence and general knowingness was impressive' and (2) *sentience*: state of elementary or undifferentiated consciousness; 'the crash intruded on his awareness'. (There is no direct counterpart in German, French, or the Scandinavian languages.)

while taking heed of other relevant occurrences are not two parallel lines of action but a specific way of pursuing a line of action, namely to do it heedfully, competently, mindfully, accountably. In a CSCW context 'awareness' does not refer to some special category of mental state existing independently of action but to a person's being or becoming aware of something. 'Awareness' is an integrated aspect of practice and must be investigated as such.

Since we cannot talk of awareness as an separate entity but only as somebody's being aware of some particular occurrence and thus only with reference to certain practices of which being aware of something is an aspect, we cannot even assume that the term 'awareness' denotes the *same* practices. 'Awareness' of *x* may or may not entail the same practices as 'awareness' of *y*. The first question to be addressed thus is, *of what* are actors supposedly aware when we in CSCW use the term 'awareness': *awareness of what?*

As soon as we address that question, it is evident that what in CSCW is innocuously labeled 'awareness' has little in common, besides the fact that it somehow is an aspect of human interaction.

In the 'media space' research, for which—to cite the canonical paper on the influential media space experiment at Xerox PARC—the 'motivating problem' has been to 'recreate in a working group separated geographically the sense of embeddedness that we had found working together locally' (Bly et al., 1993, p. 33), 'awareness' is typically conceived of in very general terms as relating to various aspects of members' taking heed of the social context of action and interaction:

> Coworkers sitting together over lunch discussing everything from the latest Super Bowl game to the knotty problems they encountered that day in their work is not particular unusual—unless the workers are separated by 800 miles. The smooth integration of casual and task-specific interactions, combined with the ability to meet informally as well as formally, is a critical aspect of productive group work. [. . .] When groups are geographically distributed, it is particularly important not to neglect the need for informal interactions, spontaneous conversations, and even general awareness of people and events at other sites. (Bly et al., 1993, p. 29)

What is immediately noticeable in this fictional situation is that the actors are only engaged in cooperative work in a very loose and broad sense, *if at all*. We meet them when they are not engaged in their work. They are having lunch. Like most people involved in the same profession or employed with the same company will do during conversations over a meal or a drink, the people in this situation chat about sports, the weather, absent colleagues, stupid bureaucrats, or unusual experiences in their work such as 'knotty problems'. As is well known, such face-to-face encounters can be quite helpful as a vehicle for 'social bonding' in general, for creating alliances, and for eliciting advice and help among colleagues. The problem with this conception of awareness, however, is that it is only peripherally and indirectly related to any *cooperative effort*. The situation above may involve cooperating workers but it may just as well involve people who do not work together at all, but who are merely employed with the same company, working in the same line of business, or living in the same neighborhood.

It is clear that awareness, as conceived of in this fictional situation, is awareness of the social context of work, not of the ongoing activities and artifacts of a joint cooperative effort:

> ***Awareness.*** Although seemingly the most invisible, the use of the media space for *peripheral awareness* was perhaps the most powerful use. The [video link] view frequently found in peoples' offices was the Commons at the other site. This view, at first glance, appeared to be nothing more than a view of an empty public space. On closer examination, however, there was rarely more than a minute or two in which there were not at least sounds from the other location giving clues about the ongoing activities there. People walked through and were in and out of offices; conversations took place. Being aware of such activities required no response; it provided an overview of who was around and what was happening (and afforded the possibility of joining in). Of course, this background awareness was not constrained to the common areas. Lab members who were working closely together often had a colleague's office on the monitor as a background view. We have seen people casually sharing jokes, show a group of friends a new toy, or move cameras to unusual positions for interesting visual effects.' (Bly et al., 1993, p. 34)

In a paper on a related research project, the Portholes experiment at Xerox EuroPARC, Dourish and Bly conceive of 'awareness' quite similarly as an aspect of so-called 'informal interaction':

> Awareness involves knowing who is "around", what activities are occurring, who is talking with whom; it provides a view of one another in the daily work environments. Awareness may lead to informal interactions, spontaneous connections, and the development of shared cultures—all important aspects of maintaining working relationships which are denied to groups distributed across multiple sites. (Dourish and Bly, 1992, p. 541)

Awareness is thus conceived of as awareness *of the social context* and is seen as something that engenders 'informal interactions' and 'a shared culture' (or even the formation of collaborative alliances (Kraut et al., 1990)). There are, of course, domains in which awareness of the general social context is an important aspect of articulation work, especially in domains such as politics and management where the formation of coalitions is of paramount importance, or domains such as teaching where socialization is crucial; but in the wide and multifarious world of cooperative work such settings and situations are exceptional. Of course, awareness of the general social context ('Who is around today?' 'Why is he befriending *him*, of all people?') *may* be of import for cooperative work in any domain, but *in the coordination and integration of interdependent activities other and more urgent concerns are pivotal* ('Will he be able to do it? Can I continue as planned?' 'Is she on time, or should I reschedule my work?' 'Oh, he's ahead of schedule, I'd better hurry up now' 'What is he up to? Hasn't he noticed the problem? Well, seems like I better take care of it').

The term 'awareness' is of course also used within CSCW in precisely this sense, i.e., as a label designating various, more or less specified, practices through which cooperating actors, while engaged in their respective individual activities and dealing with their own local urgencies and troubles, manage to pick up what their colleagues are doing (or not doing) and to adjust their own individual activities accordingly (Gutwin and Greenberg, 2002). That is, in this line of research the term ‘awareness’ is being used to denote those practices through which actors tacitly and seamlessly align and integrate their distributed and yet interdependent activities.

It is clear that the phenomena of which actors are supposedly aware when the term ‘awareness’ is used in this line of research, are not merely tangential or external to the ongoing activities that constitute a cooperative effort. The term ‘awareness’ here denotes taking heed of unfolding events and of possibly unfolding events; of things being done, of things done, and of things in need of being done; of developments within the joint effort that may be advantageous, detrimental, hazardous, etc. for one’s own work; of occurrences that makes one’s own work more urgent or less so, that require action or inaction, that necessitate changes to the intended course of action, etc.—all of it directly motivated by the actors’ being interdependent in their work and hence by the unavoidable requirements of coordinating and integrating their various actions.

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As soon as we turn to the issue of ‘awareness’ as the tacit and seamless integration of ongoing cooperative activities, however, problems abound.

The problem can be seen in the uncertainty with respect to the relationship between the concept of awareness and the concept of attention. Whereas awareness, in some quarters of constructive CSCW research, is conceived of and defined in terms of ‘focus’ (e.g., Benford and Fahlén, 1993; Rodden, 1996), other CSCW researchers emphatically distinguish the phenomenon of ‘awareness’ from ‘attention’ or ‘focus’ by defining awareness as ‘information’ that is ‘being gathered passively, while other workplace activities progress’ (Dourish and Bly, 1992, p. 541). A later paper by Dourish is even more emphatic in this respect: ‘The passive nature of [awareness] information is important. Information arises directly out of each person’s activity, rather than having to be managed explicitly; awareness information does not need to be sought out’ (Dourish, 1997). Dourish’s point is well taken. When we are talking about ‘awareness’ we are talking about the phenomenon that actors align and integrate their activities with the activities of others without interrupting the current line of action and in a seemingly effortless way.

But the notion of ‘passive awareness’ (Dourish and Bellotti, 1992, p. 107; Dourish and Bly, 1992, p. 541) is problematic in its own right, in that it mystifies what we need to understand: the practices through which actors align and integrate their distributed but interdependent activities. As if an actor’s ‘passive awareness’ of the state of the cooperative effort is the inscrutable effect of merely ‘being there’, the result of some kind of mental osmosis. . .

The problem with ‘awareness’ in cooperative work is to understand how actors so effortlessly pick up what is going on around them and make practical sense of it. In this regard, significant progress has been achieved through a range of seminal ethnographic studies such as the study of the London Underground control room by Heath and Luff (1992a, 1996) and the study of air traffic control work by the Lancaster group (Hughes et al., 1988; Harper et al., 1989a; Harper and Hughes, 1993) as well as a number of other in-depth workplace studies. As a result, various practices that are crucial for actors’ ongoing articulation of their activities with respect to the activities of others have been identified and described. The central findings of these studies can be briefly summarized as follows.

Actors obviously somehow *monitor* the activities of their colleagues in the setting—by observing or listening—so as to ascertain the state, progress, direction, etc. of these activities, that is, to ascertain whether they are being done and progressing as expected, to determine exactly how one's own activities need to be adjusted to mesh with the unfolding work of the colleagues, and so forth. Actors typically do so inconspicuously or unobtrusively or even 'surreptitiously' (Heath and Luff, 1992a). That is to say, they do it in such a way that their monitoring the work of others does not require or elicit a response from them.

On the other hand, actors make their own activities appropriately 'publicly visible' (Heath and Luff, 1992a) or, with a more general term, actors *display* those aspects of their activities that may be of relevance to their colleagues. That is to say, in doing their individual part of the joint effort, actors will typically and if possible 'design' or modulate their own activities in such a way that their colleagues are provided with cues and other kinds of resources pertinent to *their* monitoring these activities: 'we begin to discern how the design of particular activities may be simultaneously sensitive to the potential demands of different "recipients" both within and beyond the local physical environment' (Heath and Luff, 1992a, p. 82). That is, actors conduct their own activities in such a way that colleagues can perceive *that* they is being done, *how* they are being carried out, *that they will* meet constraints in terms of time and quality, that obvious contingencies *will not* affect the work of colleagues (as if to say, 'Don't worry, I can handle it'), or that they *will* affect the work of colleagues and when and how and to which extent, so that *they* can adjust *their* part of the effort accordingly.

Displaying and monitoring are thus *complementary aspects* of the same coordinative practices. My monitoring the activities of others is facilitated by their displaying those aspects that are relevant for me and my displaying aspects of my work to others presupposes that I am monitoring their activities and thereby am aware of their concerns, expectations, and intentions.

Displaying and monitoring are subtly attuned. While individual activities *sometimes* may be 'systematically, yet unobtrusively, coordinated with the actions of colleagues' (Heath et al., 1995a, p. 156), this is not *always* the case. Actors regulate their monitoring quite delicately so as to *adjust the degree of obtrusiveness* to the requirements of the situation, and they similarly display their own work in a form and at a level of granularity which is attuned to the situation facing their colleagues. For example, as observed by Heath and Luff, one operator, while talking to a remote colleague over a telephone line, not only coordinates his talk with his remote co-conversationalist, but simultaneously emphasizes details to colleagues sitting next to him 'by volume and repetition of certain elements' (Heath and Luff, 1992a, p. 82). Indeed, one of the most striking findings emerging from these studies is what we may call 'appropriate obtrusiveness'.[2] In monitoring the work of others

[2]This phrase is, of course, a play on the term 'appropriate ambiguity' aptly suggested by Bowker and Star (1991) in their analysis of fundamental characteristics of coordinative artifacts such as classification schemes.

and in displaying aspects of their own work, actors exhibit great care and much skill in choosing an interactional modality that is obtrusive or unobtrusive to a degree and in a manner that is appropriate to the situation at hand. For instance, at a critical point in the joint effort, e.g., in a hand-over situation, an actor may monitor the activity of the colleague rather conspicuously, for instance by gazing, so as to make the other aware that he or she is aware of what the other is doing, as if to say, ‘Don’t worry, I see what you are doing and I’m ready.’ Similarly, actors may display their work in ways that are designed to attract the attention of colleagues to the activity or certain features of it, by gazing at certain objects, humming, thinking aloud, placing artifacts in certain locations or orientations, leaving traces in the setting, etc. They may even, by pointing or tapping at an item or talking to colleagues, impose an obligation on the others to notice and react accordingly and therefore disrupt current activities (which may or may not be appropriate). The repertoire is infinite and is applied deftly.

More than that, because of the fine-graded repertoire of modalities of monitoring and displaying, ranging from something quite inconspicuous to something dramatically obtrusive, no clear distinction exists between, on the one hand, the coordinative practices of monitoring and displaying, normally referred to under the labels ‘mutual awareness’ or ‘peripheral awareness’, and, on the other hand, the practices of directing attention or interfering for other purposes. In fact, by somehow displaying his or her actions, the actor is always, in some way and to some degree, intending some effect on the activities of colleagues. The distinction is not categorical but merely one of degrees and modes of obtrusiveness.

If correct, this short analysis has important implications. ‘Awareness’ is not the product of passively acquired ‘information’ but is a characterization of some highly active and highly skilled practices. Competent practitioners are able to align and integrate their activities because they know the setting, they are not acting in abstract space but in an material environment which is infinitely rich in cues (Heath et al., 2002; Robertson, 2002). They understand the processes and the issues, they know how activities intersect, they know what probably will happen and what might happen, they expect things to happen and other things not to happen, they anticipate what will happen next, they are in the rhythm, they monitor for indicators of what is expected to happen, and so on. They know the procedures, the rules, the naming conventions, the schedule (Mark, 2002). In short, they are not struggling to make sense but in the middle of things, doing what they do every day; they know the drill. Occurrences beyond their immediate line of action is seen, made sense of, and understood against this background.

The confusion which notions like ‘passive awareness’ are meant to dispel arises from dichotomies such as ‘explicit’ *versus* ‘implicit’, ‘deliberate’ *versus* ‘automatic’, ‘conscious’ *versus* ‘unconscious’, ‘focused’ *versus* ‘unfocused’, or ‘obtrusive’ *versus* ‘unobtrusive’. These dichotomies are false.

At another level, the source of the conceptual troubles that has been pestering this line of research is perhaps to be found in the very formulation of the problem. The fact that actors take heed of occurrences beyond their immediate task was and is seen as something of a paradox. The paradox reflects an underlying assumption, namely that focus or attention is by definition exclusive, like some kind of mental tunnel vision.

From a cognitivist point of view, the very notion that an actor is able to pick up and relate to occurrences beyond the scope of his or her line of action and without interrupting that line of action, is difficult if not outright impossible to fathom. For example, in his 'closing plenary talk' at CSCW 2000 Warren Thorngate claimed that 'It is impossible to get information in or out of our head without paying attention. Yet attention, as Herbert Simon has noted, is a limited resource' (Thorngate, 2000). Actors' seemingly effortless taking heed of the work of others runs counter to the cognitivist presumption that the mind is single-channel information processor, perennially struggling with 'mental overload'.

On closer inspection, then, in order to understand the phenomenon of 'awareness' in cooperative work we have to address the fact that *the world in which cooperating workers act and interact is given to them as a meaningful world*.

In addressing the problem of meaning, we are of course up against the Cartesian presumptions underlying cognitivism, in so far as cognitivism tries to account for human cognition in terms that not only ignore but *deliberately dismiss the problem of meaning*. The Cartesian trick is a simple but effective one. It consists in adopting a skepticist stance that, by conceiving of the mind as an internal realm, mystifies cognition beyond recognition, so to speak. In his *Philosophical Meditations* Descartes describes how he is sitting by the window overlooking a square somewhere in Northern Holland around 1,640:

> if I look out of the window and see men crossing the square, which I just happen to have done, I normally say that I see the men themselves [...]. Yet do I see more than hats and coats which could conceal automatons? I *judge* that they are men. And so something which I thought I was seeing with my eyes is in fact grasped solely by the faculty of judgment which is in my mind.' (Descartes, 1641, p. 21)

One might object that what Descartes saw through that window in Holland was not really 'hats and coats' either. What he saw, one might suggest, was merely colors and shapes. To which one might object, in turn, that he did not see colors and shapes either but merely different frequencies of light in different patterns. And so forth, *ad absurdum*. What the trick boils down is a straightforward abuse of the concept 'to see'. Whatever happens in the retina and the visual cortex, *the spectator* does not 'see' mere colors and shapes, nor does he or she 'see' mere configurations of 'hats and coats' that are somehow propelled horizontally across a space. What the spectator 'sees' is 'men crossing the square'. In the words of Gibson, 'Phenomenal objects are not built up by qualities; it is the other way around. [...] The meaning is observed before the substance and surface, the color and form, are seen as such.' (Gibson, 1979, p. 134).

In their 'natural attitude' actors are not ignorant or disinterested spectators, they are actively engaged in and with the world. As such they have interests, things to do, things to achieve. Things are meaningful to them. When an actor perceives a colleague doing something in the shared setting, he or she observes something that (typically) is immediately meaningful to him or her. To a competent member, making sense is thus (typically) effortless.

To competent members relevant occurrences stand out, impose themselves. An event 'leaps to the eye' because it is expected or is a deviation from that which one would expect. It does not require special attention.

There is nothing paradoxical in being engrossed in one line of action and simultaneously making sense of and taking heed of what goes on beyond one's immediate line of action in what appears as other lines of action.

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These very brief methodological remarks do not constitute anything like a solution to the problem of 'awareness', of course. But they do indicate an approach that makes it a researchable problem.

Liberated from the skepticist mystification of intersubjectivity, we no longer have to marvel at the apparent miracle that actors effortlessly make sense of the actions of coworkers. Nor are we caught in a dichotomy between attention and peripheral awareness, active and passive, explicit and tacit etc. Instead we can form and pursue researchable questions such as:

(i) Upon which evidence does an actor rely when heeding the activities of others? What data (signals, cues) are available to the actor? What is the actor able to perceive of the actions of others? At which point does the actor seem to change course of action? Which specific situations or constellations of events seem to make actors change their course of action?
(ii) By virtue of which competencies are cooperating actors able to make sense of what others are doing? Which 'taken-for-granted knowledge' is invoked by the actor in making sense of the evidence available to him or her? Which 'indicators' or 'typifications' do actors primarily rely on? What do they monitor for and what is ignored? What is displayed and what is not? Which events make a difference and which are of no consequence?
(iii) How do actors exploit the material and conventional environment in monitoring unfolding events? Which indicators play a key role in determining the state of affairs? What is the relationship between the materiality of artifacts and their representational role as vehicles of signs? How is this duality exploited in monitoring and displaying?
(iv) How does the actor determine what is relevant to his or her own effort? How does the actor manage to sort out and pick up what is relevant? How does an actor, in modulating his or her activities so as to make relevant aspects thereof accessible to colleagues, determine what is relevant for the others? On the basis of which insight? How does an actor know when and how to 'attune' the 'obtrusiveness' of his or her monitoring or displaying?

Based on such insights we may be able to move the research on computational environments to support awareness in cooperative work significantly forward.

Chapter 9
Remarks on the Complexity of Cooperative Work (2002)

1 The Puzzle of 'Cooperative Work'

1.1. Cooperative work, as an aspect of human ecology, seems to have existed in human societies for hundreds of thousands of years, but until recently only as a marginal phenomenon. Work has, of course, been socially situated and socially organized in all human societies, and 'cooperation' as the sharing of the fruits of our toil is arguably constitutive of human sociality (Reynolds, 1981). Cooperative work in a more specific sense, however, i.e., conceived of as constituted by interdependencies among actors in productive activities, is far from universal. In fact, in the modes of production of hunter-gatherers, horticulturalists, ancient and medieval agriculture, craft work, etc. the bulk of work activities such as gathering, hunting, fishing, butchering, skinning, collecting wood, cooking, weaving, sewing, pottery, weeding, harvesting, trashing, etc. were almost always carried out individually; cooperative work only occurs sporadically, for example for the hunting of large game or for clearing land (Johnson and Earle, 1987).

Naturally, mobilization of labor on a large scale for political, infrastructural, military, or religious purposes, such as warfare or the building of dams, canals, roads, fortifications, monuments, temples, etc., looms large in the historical record, but is nonetheless economically marginal (Moore Jr., 1966). It is not until the establishment of the world market and the concomitant industrial revolution, that is, it is not until the emergence of the capitalist mode of production, that production generally begins to acquire the character of systematic cooperative work in the form of commercial farming, manufactories, industrial factories, etc. (Marx, 1867a).

1.2. Why is that?

First of all, until the emergence of the capitalist mode of production, the scale of the political economy did not necessitate or even warrant systematic production on a large scale (Johnson and Earle, 1987). Furthermore, and irrespective of the scope of the political economy, cooperative work arrangements are only entered into

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K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1_9,

reluctantly, due to the mutual dependencies that define these arrangements. On the one hand, cooperative work relations imply definite social tensions—the precarious reconciliation of divergent motives and interests, the potential conflicts of sharing the burden and the product, the loss of individual and local autonomy—which are all to be addressed and dealt with. On the other hand, coping with the mutual dependence in cooperative work entails an added effort of aligning, meshing, coordinating, and integrating the different individual contributions to the joint endeavor and requires specific organizational skills in terms of punctuality, accountability, planning, logistics, etc.

We can, accordingly, observe that cooperative work arrangements emerge in response to certain requirements—the fact that a necessary or desirable task cannot be performed at all (or as well, as timely, as safely) on an individual basis—only to dissolve again when the situation requiring individuals to join forces no longer exists, or due to technological innovations which enable individuals to achieve the same results without joining forces. This pattern not only emerges strongly from the ethnographic and historical record but also from studies of cooperative work in contemporary work settings and, indeed, from everyday experience.

That is, a cooperative effort seems to entail such social and coordinative intricacies and troubles that it is not something undertaken lightly. It is apparently only embraced when absolutely unavoidable or at least when working together offers obvious and substantial benefits. In fact, it seems as if the more intricate the interdependencies and the more distributed the control, the more demanding and difficult the alignment, coordination, integration of the cooperative effort and the stronger the reluctance to collaborate. There are, indeed, strong indications that contemporary work organizations typically are faced with coordinative problems which are at the limit of what they can manage—and not rarely beyond that limit, with all kinds of turmoil and breakdowns as a result. If this description is just reasonably correct, the development of computer-based systems that can assist work organizations in coping with the complications, intricacies, and entanglements of the coordinative and integrative aspects of their work can be seen as critically important.

1.3. In the 1990s I was engaged in an extensive research effort in which we were trying to understand a range of coordinative practices which we gave the collective label 'mechanisms of interaction' and later 'coordination mechanisms' (Schmidt, 1994e; Simone and Schmidt, 1994). These 'mechanisms' are characterized by the fact that interactions among cooperating actors are somehow regulated by means of pre-established coordinative protocols coupled with mediating artifacts. It was of course obvious from the CSCW literature and from our own observations that cooperative work in many settings is coordinated and integrated effectively without such 'mechanisms' and, accordingly, that they were only required and adequate under certain conditions of working. We could moreover observe situations where cooperative ensembles invented and adopted 'coordination mechanisms' so as to somehow cope with a task of coordinating and integrating their work that had become overwhelming (Carstensen, 1996; Carstensen and Sørensen, 1996). That is, it was obvious that 'coordination mechanisms' were not universally valid practices. Given that, how could we characterize the conditions under which such practices become required and adequate? We also observed that 'coordination mechanisms', when they were used in a setting, were merely complementary to other coordinative practices and that they thus were instrumental in handling only certain aspects of the coordinative and integrative task. Again, for which aspects of the work of coordinating and integrating cooperative activities are 'coordination mechanisms' required and appropriate?

These questions are of course critical to the design of 'computational coordination mechanisms' in particular and of CSCW technologies in general. Thus, in a first attempt to sketch a possible conceptual basis for the design of computational coordination mechanisms, we tried to account for our findings by suggesting the concept of *complexity*. That is, we interpreted what we had observed as so many examples of coordinative tasks that were complex in ways that made it inefficient or impossible for the actors to cope without pre-established and artifactually mediated regulation of their interactions. As to a further analysis of the complexity of cooperative work, our observations indicated that the complexity of a cooperative effort depended upon the number of actors involved combined with the differentiation of the roles, skills, heuristics, conceptualizations etc. represented in the cooperative work arrangement. Apart from such initial and intuitive ideas, however, our use of the concept of complexity was never systematically worked out or critically discussed.

This then leads to the issues addressed in the following remarks: How can we systematically conceptualize the multifarious coordinative and integrative challenges of cooperative work? Can we methodically identify and compare these challenges? Can the coordinative practices of cooperative work be understood as practices of managing complexity in some sense? Can we analytically identify aspects or areas of cooperative work in which digital technologies might assist cooperative ensembles in managing the complexities of the cooperative effort? Can we in our analyses identify salient or typical sources and dimensions of complexity? How can we understand the complexity or complexities of cooperative work? And, more fundamentally, does it make sense at all to talk about the complexity of cooperative work?

2 Taking Serious Work Seriously

2.1. First of all, of course, we have to clarify the term 'complexity' as it seems to be used in confusingly different ways.

In the everyday use of the term it is often being used rather loosely as a synonym of 'composite', i.e., to denote that something is made up of many different parts. Normally, however, the term is being used in the sense of *intricacy*, i.e., to denote that something is made up by many *interacting* elements. This is, for instance, the sense in which the term 'complexity' has been generally used in organization theory (Barnard, 1938; Simon, 1962; Thompson, 1967; La Porte, 1975; Demchak, 1987). In this usage, the concept of complexity is a systemic concept. In the words of Condillac, 'A system is nothing but the disposition of the various parts [. . .] that they mutually sustain one another'(Condillac, 1749, p. 1). The concept of complexity expresses the *systemic* qualities of a system, i.e., the intensity of interactions among its elements. A system is complex in that and to the extent that its various parts 'mutually sustain one another'. More precisely, but in exactly the same sense, the term is being used to express features of systems that result from non-linear interactions among the parts. This is for instance the general usage of the term in software engineering. In this conception, the complexity of a system is conceived of as an expression of the number of possible states the system may have (Brooks, 1987).

However, the term 'complexity' is also being used in various research programs striving to overcome the perplexities of the reductionist paradigm when tasked with accounting for the evolution of complex and yet stable systems such as biological species and eco-systems. However, because of the particular perspective of these research programs, namely their critique of reductionism, 'complexity' is here often used in a derived sense, to denote *emergent order* (as in 'organized complexity', 'self-organization', 'anti-chaos', 'collapse of chaos', 'complex adaptive systems': Bertalanffy, 1968; Prigogine and Stengers, 1979; Nicolis and Prigogine, 1989; Cohen and Stewart, 1994; Holland, 1995; Kauffman, 1995). This particular usage can of course be made out to be in conformance with the above conception of complexity as a feature of non-linear interactions, but the jargon is confusing nonetheless, in that the term 'complexity' is being used both to denote the internal *intricacy* of the system as well as the higher-level *order* in which this intricacy is hidden or collapsed. Anyway, the objective of this paper is not to attempt to conceive of cooperative work as emerging complex order or in other ways apply an evolutionary model to cooperative work, but to investigate the coordinative and integrative problems facing members of cooperative ensembles. More specifically, the task at hand is to investigate if we can usefully conceive of coordinative practices as practices of managing complexity in the sense of a space of (possible) interactions.

2.2. Before we move on to explore how it might make sense to conceive of cooperative work in terms of complexity, we also need to address the fundamental question: Does it make any sense to conceive of work in terms of complexity and thus, by implication, in systemic terms? Is it at all warranted to talk about work in systemic terms? And, if we do that, if we conceive of work in systemic terms, what kind of analyses are we then formulating?

2.2.1. When we conceive of human affairs in terms of 'systems', the perspective adopted is one in which social relations and social processes are not accounted for in terms of the intentions and experiences of individuals but in term of interactions beyond the scope of the concerns of individual actors—that is, systemic interactions. This does not imply, however, that it is not a sensible thing to do. One can obviously obtain valid and significant insights by conceiving of national and international economies, technological revolutions, human populations and demographic transitions, and large-scale socio-technical systems such as the development of cities, traffic patterns, the Internet, etc. in systemic terms, that is, as formations whose emergence, dynamics, and characteristics cannot be understood (entirely or primarily) on the basis of an understanding of the dispositions and notions of the individual actors.

This perspective was succinctly expressed by Marx in his famous proposition that 'society does not consist of individuals, but expresses the sum of reciprocal relations between these individuals' (Marx, 1857–1858a, p. 188) and was given a striking manifestation in his analysis of the circulation of commodities and money: ‘Although this movement [i.e. the circulation of commodities and money] as a whole appears as a social process, and although the individual moments of this movement appear to arise from the conscious will and particular purposes of individuals—the totality of the process appears as an objective connection that arises spontaneously; it arises, it is true, from the reciprocal actions of conscious individuals, but it is neither located in their consciousness, nor is it as a whole subsumed under them. Their own mutual collisions with one another produce an *alien* societal power standing above them; their reciprocal action as a process and power independent of them.’ (Marx, 1857–1858a, p. 126).

2.2.2. By adopting a systemic perspective, however, one does not obtain an insight into the actions of members as these themselves conceive of and account for them, that is, into their concepts and routines, their concerns, aspirations, motives, etc., and thus into how actors make sense of what they and others do, have to do, could have done, etc—in short, into what it means to be a practitioner. Without such insights, it is very difficult if not impossible to design computer-based systems that facilitate or enhance the coordinative practices of cooperating actors.

What we need to understand is, what does it mean to be a practitioner? To answer that question, in the words of Bourdieu, ‘one has to situate oneself *within* “real activity as such,” that is, in the practical relation to the world, the preoccupied, active presence in the world through which the world imposes its presence, with its urgencies, its things to be done and said, things made to be said [. . .]’ (Bourdieu, 1990, p. 52). What are the concerns of the practitioners? What do they strive to achieve and to avoid? Which pressures and impediments do they have to deal with?

More than that, we are not trying to understand what it is to be practitioner of human activity in all generality: hiking, reading poetry, praying, besieging a beloved, daydreaming, finding a proper spouse, etc. We are after something far more specific, namely what it is to be involved in *cooperative work*.

Working, in turn, can be conceived of in infinite ways and from innumerable perspectives: as the source of income and identity and new jokes; as the location of the exercise of skill and as the site of the acquisition of skill; as scenes where the passions of human life play out, ambition and disenchantment, power and submission, mutual aid and deceit, friendship and competition, hate and love, and so on and so forth. None of these threads of working life can be dismissed in advance and in general as irrelevant to understanding coordinative practices, but they do not constitute what working is about.

2.2.3. Work is activity that is highly constrained technically. As argued by Marx, work is ‘always a realm of necessity’ (Marx, 1863–1867, p. 838). By contrast to Fourier who envisaged that working in a future ideal organization of social life becomes ‘mere fun, mere amusement’, Marx insisted that even ‘really free working, e.g., composing, is at the same time the most damned seriousness, the most intense exertion’ (Marx, 1857–1858a, p. 499). In general terms work facilitates humanity’s ‘metabolism’ with (external) nature, directly or indirectly. Human kind must work in order to survive, as a species and as formations of social division of labor. As a constituent part of this wider system, a given cooperative ensemble must see to it that the product of its labor is of some utility to its members or, as a functional part of a system of social division of labor, to its customers or clients or other stakeholders. The wider social, political, and organizational environment may also pose severe demands as to how the work processes are to be conducted, e.g., in terms of efficiency, safety, and security. And finally, work is conducted under more or less severe limitations in terms of resources: financial, personnel, space, time, etc. That is, work is conducted under severe demands in terms of the functionality, quality, and cost of the product, as well as in terms of the safety, reliability, and timeliness required of the process. The sum total of all this is that the activities of work are characterized by (typically unremitting) technical constraints in terms of cause/effect, dynamics, means/ends, spatial order (e.g., part/whole, separation/containment, relative location, relative volume), temporal order (e.g., sequentiality, concurrency, synchronicity, reversibility), and so forth.

If such constraints and interrelationships are not considered systematically in a study of the phenomenon of work, the study will make the 'phenomenon disappear', as Sharrock and Anderson put it. Thus, in a critique of sociologists who generally 'assumed that sociological inquiry must avoid the technical issues involved in them, and that the purpose of sociological inquiry is to show that considerations of a social, rather than a technical, kind enter into practice', Sharrock and Anderson argue that the opposite obtains: in the study of domains of work '*the technical aspects of activity*, far from being an irrelevance, *must be central to all consideration*, for it is in their technicalities that [different kinds of work] consist'. Thus, for the purpose of understanding work practices, our inquiries must be *directed at* finding out what the specific activities consist in 'as course of action': 'what does someone *really have to do*' to do that particular kind of work (Sharrock and Anderson, 1986, pp. 86–89, Emphasis added.).

A serious investigation of cooperative work must take the technical aspects of activity seriously and make them a central concern of the study, because work is fundamentally about mastering technicalities: 'Surely we are free to chose what we are interested in, but this interest, once established, determines the system of relevances intrinsic to the chosen interest. We have to put up with the relevances thus set, to accept the situation determined by their internal structure, to comply with their requirements. [. . .] We are, however, not only centers of spontaneity, gearing into the world and creating changes within it, but also the mere passive recipients of events beyond our control which occur without our interference. Imposed upon us as relevant are situations and events which are not connected with interests chosen by us, which do not originate in acts of our discretion, and which we have to take just as they are [. . .]' (Schütz, 1946, p. 126 f.).

In their work activities, actors have to accept the situation determined by 'the internal structure' of the 'system of relevances' and 'comply with their requirements'; more than that, actors have to deal effectively with the 'imposed relevances' of 'events beyond [their] control' which 'occur without [their] interference' and of which they are 'the mere passive recipients'. *In doing that, actors are conceiving of and dealing with the world in systemic terms*, i.e., in terms of causality, temporal and spatial order, means and ends relations, etc.. And to the extent that actors are conceiving of and dealing with the world in a systemic terms, we as analysts (whether ethnomethodologists or not) are not only perfectly entitled to conceive of their work in the same terms: *to the same extent any serious study of cooperative work must adopt a systemic perspective to be able to understand what it is like to be a member*.

3 The Problematic Concept of Complexity

3.1. In addition to the use of the concept of complexity in parts of organization theory, the concept of complexity has been object of particular attention in research areas devoted to the study of work. It was here adopted along with concepts such as 'system', 'control', and 'feedback' as part of the 'systems approach' that had become required in the design and management of large-scale 'man-machine systems' or 'socio-technical' complexes such as ballistic missile systems and nuclear power plants (La Porte, 1975; Rouse and Rouse, 1979; Perrow, 1983, 1984, 1986; Von Glinow and Mohrman, 1990). It was hoped that conceiving of work in terms of complexity would enable analysts to identify and formulate 'objective' characteristics of 'human-machine systems' in different work domains (Rouse and Rouse, 1979; Woods, 1987). These hopes were soon abandoned, however, as researchers realized that it was exceedingly difficult if not simply nonsensical to compare the complexities facing workers in different domains. After all, which is most complex, a psychiatric ward or a nuclear power plant?

3.1.1. Thus, in a critical discussion of the concept of complexity, Rasmussen and Lind argued that 'complexity' is a fundamentally problematic concept: 'The complexity observed depends upon the resolution applied during information search. A simple object becomes complex if observed through a microscope. Objective complexity can only be defined for a given representation of a system, not for the system itself' (Rasmussen and Lind, 1981, p. 8).

The point is well taken. A cow can for instance be described as just another cow in the herd; as an average unit with standard component parts (shank, round, rump, loins, flank, rib, tail, etc.); as an individual unit with certain characteristics (milk yield per week, etc.); as a physiological system with interacting subsystems (blood circulation system, digestive system, neural system, hormonal system, reproductive system, etc.); as a system of interacting cells, hormones, proteins, etc.; as a self-reproducing system of nucleic acids; as a system of molecules and atoms; as a quantum mechanical wave function, etc. The cow that, from the farmer's point of view, is just another brown cow, will be conceived of by a biologist as a complex of systems, from tissues and organs, over the various control systems such as the hormone and immune systems, to the elemental processes of cellular respiration, excretion, and reproduction.

However, while Rasmussen and Lind put the finger on a very problematic issue in the common-sense and often naïve use of the concept of complexity, the solution proposed by them is not satisfactory. Firstly, they do not address the obvious issue that a system may be more or less complex 'for a given representation' of the system. More fundamentally, however, their argument hinges on a common-sense notion of 'object' or 'system'. They argue as if what constitutes an 'object' or a 'system' is self-evident, or more to the point: as if it is the same 'object' or 'system' we see with our eyes and under the microscope, as if the difference is only one of 'resolution'. What we look at when we use a microscope is not necessarily the same system at a different resolution but may be a different system with properties that cannot be directly applied to the macroscopic system. It is surely not possible and does not make sense to conceive of the brown cow in terms of its quantum mechanical wave function. The 'underlying' system cannot be conceived of in the same terms as the target system and the difference is not necessarily one of data compression. *A system is always defined under a certain perspective and hence in terms of certain principles of abstraction, distinction, aggregation, and representation.* Hence it leads to confusion to conceive of the relationship between the 'representation of a system' and 'the system itself' as an *external* relationship. Different principles of abstraction, distinction, aggregation, and representation refer to different 'systems' and vice versa. By implying certain principles of abstraction, distinction, aggregation, and representation, the definition of a particular system implies a certain *metric of complexity*. This has important implications. A given system (as defined in terms of certain principles of abstraction etc.) *may be more or less complex* according to the implied metric of complexity.

3.1.2. Suchman makes a point rather similar to Rasmussen and Lind's, by stating that 'the complexity or simplicity of situations is a distinction that inheres not in situations but in our characterization of them. All situations are complex under some views, simple under others' (Suchman, 1993a). We *can* certainly describe *any* situation, or any action, in such a way that it, from a certain point of view, can been seen as highly or even infinitely complex. Consider, for example, an encounter between two persons in a room. From the point of view of the actors themselves or of an unsuspecting observer it may be quite ordinary and unremarkable. From the point of view of other observers, however, the same situation may appear rather complex. Where the neutral observer may only see a couple talking about a job, a jealous lover to one of the persons, for example, may see lingering glances, stolen touches, shining eyes. Likewise, where the participants will only see each other, the carpet, the chair, the table, etc., a forensic investigator will look for and see foreign fibers on the carpet, human hairs on the back of one of the chairs, fingerprints on the table top, and so forth. Which of the many potential points of view is *relevant* depends, of course, upon the interests of the observer (Schütz, 1947–1951). If no crime has been committed, for example, the forensic perspective is hardly relevant.

One can only talk about complexity with reference to a specific system, that is, from a certain perspective and thereby in terms of certain principles of conceptualization (categorization, distinction, abstraction, aggregation, representation, etc.). Which perspective is relevant depends on the purpose of the analysis.

3.1.3. In this context Alfred Schutz' concept of the 'natural attitude' of the actor is of crucial importance. In 'the natural attitude' characteristic of everyday practice, the practitioner will not take the infinite number of possible perspectives, points of view, or principles of conceptualization into consideration, before acting. According to Schutz 'this world is to our natural attitude in the first place not an object of our thought but a field of domination. We have an eminently practical interest in it, caused by the necessity of complying with the basic requirements of our life. But we are not equally interested in all the strata of the world of working. The selective function of our interest organizes the world in both respects—as to space and time—in strata of major or minor relevance' (Schütz, 1945, p. 227). Unless an actor has practical reasons for considering the situation from a different perspective, he or she will stick to those perspectives that are presumed *relevant* to his or her project. Thus the observer's perspective should not be taken for members' perspective. While members are faced with things to be done, with constraints to observe, and with urgencies to be dealt with, the observer is in the privileged position of considering the different points of view of different actors and of contemplating any number of alternative perspectives that might also be applied.

Egon Bittner argues this point forcefully.[1] 'The paramount fact about the reality bounded by an ethnographic held work project is that it is not the field worker's own, actual life situation.' The urgencies with which members have to deal are not urgencies to the observer who has deliberately undertaken to view the world 'as the world of others': 'Since the field worker, as field worker of course, always sees things from a freely chosen vantage point [. . .] he tends to experience reality as being of subjective origin to a far greater extent than is typical in the natural attitude. Slipping in and out of points of view, he cannot avoid appreciating meanings of objects as more or less freely conjured. [. . .] Hence, without it ever becoming entirely clear, the accent of the field worker's interest shifts from the object to the subject. [. . .] Moreover, since he finds the perceived features of social reality to be perceived as they are because of certain psychological dispositions people acquire as members of their cultures, he renders them in ways that far from being realistic are actually heavily intellectualized constructions that partake more of the character of theoretical formulation than of realistic description.' (Bittner, 1973, pp. 121 ff.).

3.1.4. For the purposes of designing usable and useful computer systems for cooperative work settings we do not need to know how cooperative work *could* be construed as complex by a disinterested observer but what makes it complex to competent members and how computer systems may be of assistance in reducing or otherwise coping with this complexity. The relevant perspective from which to analyze the complexity of cooperative work is not something which can only be determined arbitrarily or subjectively, as Rasmussen and Lind as well as Suchman seem to imply. It is a *researchable* issue: what is the relevant perspective, the relevant level of abstraction, etc. to a competent actor 'in the natural attitude' of a given line of action has to be determined empirically.

Modern architects, for example, are faced with and have to deal with issues and interdependencies which previous generations of architects did not have to handle. In the course of the industrial revolution, the needs of industry led to the introduction of facilities for heating, ventilation and sanitation, which began to be applied to domestic architecture as well. Central heating in the form of steam-heating systems

[1] Bittner's analysis of the observer's perspective is a development of Schutz' analysis of 'commonsense' and scientific perspectives (Schütz, 1953).

appeared in the early nineteenth century; cold- and hot-water systems and sanitary plumbing developed rapidly in the second half of the century. Gas lighting came to London in 1809 and by the 1880s electric light was 'available to those who could afford it and were prepared to take the risk of using it.' Elevators, telephones and mechanical ventilation were introduced in the last decades of the century. 'However much people may have regretted or been frightened by the scale and rapidity of the changes, what had been produced in a 100 years was a whole new range of possibilities, and therefore a new aesthetic and a new challenge to the designer. How was he to cope with change and express architectural qualities in such a revolutionary milieu?' (Nuttgens, 1997, p. 245). The challenge to architects is not only an aesthetic one, but an immensely practical one, as the vastly increased 'range of possibilities' implies a vastly increased number of structural and functional elements of the building to be integrated in the design and in the construction plans. In the course of the last century, the range of possibilities and thus structural and functional elements to be integrated has been further extended by the introduction of air-conditioning systems, facilities for tele-communications, security, safety, fire-fighting, etc. (Sabbagh, 1989). As a result the work of modern architects is intensely collaborative. In a typical large building project various people work on different sections of the building and they may be responsible for particular design tasks. Thousands of documents are created in this process. Moreover, a contemporary building project engages many external actors, in addition to the client and perhaps prospective users: technical consultants (for structure, insulation, heating and ventilation, electricity, the lighting concept, the facade, etc.), various authorities, the general contractor, specialized building companies, and craftspeople. The architects may have to coordinate the effort and consent of between 30 and 50 different people from different institutions and companies, each with their own professional competences and perspectives (Schmidt and Wagner, 2002b).

Although fundamentally different in important ways, the medical domain exhibits the same increase in 'range of possibilities'. Witness, for instance, this description of developments in contemporary medical work by 'a veteran of emergency rooms, post-operative wards, and intensive-care units': 'The remarkable advances of ultramodern biotechnology have brought with them complexities of such magnitude that medicine sometimes seems in danger of being overwhelmed by forces of increasing intricacy and incomprehension. In certain situations, only the small number of superspecialists who deal in a particular aspect of diagnosis or therapy are equipped to interpret a finding or observation. What conclusions are to be drawn from a hard-to-interpret test of liver function? In what situation is it better to recommend angioplasty rather than coronary bypass? Which of three possible antibiotics is best for a particular resistant bacterium? The opinions of the highly specialized consultants called to address such problems sometimes conflict. The responsible attending physician who must actually make the major decisions may not fully comprehend every assumption and each piece of medical evidence that should enter into them. And once having chosen a course of action, he must then trust to an unknown number of others that it will be properly carried out. These range from the surgeon doing an operation to the maintenance man servicing a piece of equipment or the orderly cleaning it. We are caught in a spiral of uncertainty that is only magnified by the increasing range of our capacity.' (Nuland, 2002, pp. 10–11).[2]

The same goes for manufacturing (Schmidt, 1991b; Carstensen et al., 1999), software engineering (Carstensen, 1996; Carstensen and Sørensen, 1996; Grinter, 1996a, b), etc. Practitioners are faced with complexities in the form of issues and constraints that interact and intersect. An observer may think of them as subjectively constructed. There is, of course, no 'objective' reason why the farmer should not look at a brown cow with foot-and-mouth infection as a quantum-mechanical wave function, just as there is no 'objective' reason why an architect should take for granted that buildings need plumbing, heating, electrical wiring, air-conditioning, etc. To practitioners, however, these issues are given, imposed, taken for granted,—not construed. Architects have to deal with the specific technical and organizational complexities stemming from the requirements that buildings must have plumbing, heating, electrical wiring, air-conditioning, and so on. Medical workers have to deal with the specific technical and organizational complexities stemming from the vast array of technologies and techniques of modern medicine. Software engineers have to deal with the specific technical and organizational complexities stemming from the astronomical number of possible interactions in large software systems.

Deprived of the concept of complexity as an objective feature of a system we would be unable to take seriously the specific challenges and urgencies contemporary workers are face with and thus understand their specific work practices.

3.2. The concept of complexity as used here implies a notion of limited resources on the part of the actor in question.

3.2.1. The notion of resource limitations is even more problematic than the concept of complexity, however; it is ripe with confusion. The source of this confusion is the cognitivist postulate that human action and cognition can and should be conceived of in terms 'information processing'. In the words of one of the key proponents of the 'information processing' paradigm, Herbert Simon, 'The point of departure is the observation that human thinking powers are very modest when compared with the complexities of the environments in which human beings live. If computational powers were unlimited, a person would simply consult his or her preferences (utility functions) and choose the course of action that would yield maximum utility under the given circumstances. That is, of course, just what the "rational man" of classical economic theory does. But real human beings, of bounded rationality, cannot follow this procedure. Faced with complexity and uncertainty, lacking the wits to optimize, they must be content to satisfice—to find "good enough" solutions to their problems and "good enough" courses of action.' (Simon, 1979, p. 3).

Simon's way of reasoning is quite bizarre. For the proposition of 'bounded rationality' to make sense, the obverse notion of 'unbounded rationality' would have to make sense too, but theological discourse aside, the notion of 'unbounded rationality' is nonsensical. The 'boundedness' of rationality is not a function of some

[2]For identical observations about the complexity of hospital work, cf. (Strauss et al., 1985).

fixed limit on human cognitive capacity (whatever that might mean and however it might be measured) but of a trivial epistemological condition: the world cannot be exhaustively described; there is always another possible perspective, another level of abstraction or granularity, etc. under which things can be seen and described differently. But instead of realizing that the premises of neo-classical economics are epistemologically false, Simon presumes that the problem is psychological.

This 'psychologism'—false epistemology dressed up as an empirical proposition—goes a long way towards explaining the utter futility of the endeavors to investigate the putative mechanisms underlying the fixed limits of human cognitive capacity. It is of course evident that there are limitations to our individual and collective cognitive capabilities at any point in time. We make mistakes, we oversee important things, we forget. But to explain those facts of life by reference to a specific determined capacity limitation is unwarranted. In fact, when such claims are thoroughly investigated they are just as thoroughly dissolved.

3.2.2. For example, when summarizing an expert conference on 'Mental Workload' (Moray, 1979a), Neville Moray observes in a quite disillusioned manner 'that perhaps there is no value which can be assigned to the [mental] load imposed on a man. The extent to which a man is loaded is a function not merely of the man, but of the task-specific situation in which he finds himself. The implications would be that there are as many measures as there are types of task, and the hope for a unified theory of load is a false hope' (Moray, 1979b, p. 13). Moreover, referring to a series of experiments 'in which apparently infinite transmission rates were obtained', Moray comments dryly: 'Since no system can have an unlimited channel capacity, the theory's performance is not as good as that of man' (Moray, 1979b, p. 14). And finally, in a report from a working group on 'mental workload', Johannsen notes that 'It is at least certain that man is not a "single channel" system, and when sufficiently well practiced may not even be of "limited capacity". But on occasions and in certain tasks he may behave like either [...]. [W]ith practice performance improves and effort declines. This seems to be linked to the fact that prolonged practice in highly motivated human subjects makes the single channel limited capacity model of the human operator less and less appropriate.' (Johannsen et al., 1979, pp. 103, 108).

In a quite similar vein, Ulric Neisser and his colleagues undertook a series of experiments in which the claim that human attention capacity is limited was put to test and failed miserably (Neisser, 1976, pp. 79 ff.). Neisser concludes that 'there is no physiologically or mathematically established limit on how much information we can pick up' [p. 99]. It is all a matter of acquired skills: 'Practiced subjects can do what seems impossible to the novice as well as to the theorist [...]. The more skilled the perceiver, the more he can perceive.' [pp. 92 f.].[3] More fundamentally, Neisser points out that the very notion of a single object of attention is confused:

[3]One may object that the phenomenon of selective attention has been subjected to extensive research since 1976. However, in an extensive review of the literature on this issue from 1986 Johnston and Dark wonder whether 'understanding the nature of selective attention is ultimately futile' (Johnston and Dark, 1986, p. 70). Again, in a sweeping review of the literature from 1993

'The very notion of "a single thing" is far from clear: how many things am I aware of when I listen to an orchestra, watch a ballet, drive a car, make love?' (Neisser, 1976, p. 104).

3.2.3. The notion of bounded rationality is evidently[4] based upon the concept of 'information' in the 'mathematical theory of communication' proposed by Shannon for the purpose of being able to measure the transportation capacities of communication channels, from which the notion of the *meaning* of 'information' was explicitly and deliberately eradicated: 'The fundamental problem of communication is that of reproducing at one point either exactly or approximately a message selected at another point. Frequently the messages have meaning; that is they refer to or are correlated according to some system with certain physical or conceptual entities. *These semantic aspects of communication are irrelevant to the engineering problem.* The significant aspect is that the actual message is one selected from a set of possible messages' (Shannon, 1948, p. 379, Emphasis added.). In his summary of Shannon's theory, Warren Weaver was even more clear and adamant in distinguishing this telecommunications usage of 'information' from its everyday usage: 'The word *information*, in this theory, is used in a special sense that must not be confused with its ordinary usage. In particular, *information* must not be confused with meaning' (Shannon and Weaver, 1949, p. 8). Realizing, however, that a theory of human cognition from which the notion of *meaning* has been erased fails immediately and utterly when given the task of accounting for skillful everyday human activity, the 'information processing theory' was surreptitiously tampered with, by the introduction of the obscure idea of 'chunking' (Miller, 1953, 1956; Simon, 1974). Miller, for example, describes 'chunking' as 'a process of organizing or grouping the input into familiar units or chunks, and a great deal of learning has gone into the formation of these familiar units. [...] Since the memory span is a fixed number of chunks, we can increase the number of bits of information that it contains simply by building larger and larger chunks, each chunk containing more information than before.'[5] Miller and Simon and other fathers of this idea never bothered to notice that they thereby had abandoned their claim of having a *computational* model of human cognition. They just swapped paradigm whenever convenient.

In fact, this confusion runs deep. The 'information processing' paradigm is based on using the concept 'information' in the sense of an arrangement of *signs* (a spoken utterance, a written text) and at the same time in the sense of the *meaning* of these signs. The equivocation has been carefully and deftly crafted and maintained. The term 'information' is being used to denote the arrangement of a set of physical objects (a sequence of sounds, a graphical configuration of ink marks) *as well as* the meaning that the particular arrangement of signs has in a particular community in a

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Allport questions whether attention is a coherent field of study and whether any theoretical progress has been made in 25 years of research (Allport, 1993).

[4]George Miller is quite explicit about this (Miller, 1953, 1956).

[5]Notice how diligently Miller makes 'the number of bits of information' the measure of 'familiarity' and 'learning'.

certain situation. When cognitivists speak of the processing of ‘physical symbols’ it is thus never quite clear whether they mean, say, sheets of paper with ink marks or the conventional usage of those objects by members of a community. It is by means of this equivocation that they seem to be able to reduce human understanding, reasoning, doubting, etc. to the manipulation of physical objects by agencies to whom these objects do not have any meaning as signs. The fact that ‘physical symbols’ only are ‘symbols’ if they have meaning to some community has been officially eradicated, only to be invoked again surreptitiously.

This goes a long way towards explaining why the notion of ‘limited processing capacity’ in its many guises (‘bounded rationality’, ‘mental workload’, ‘cognitive overflow’, and sometimes ‘selective attention’) has been so persistent in spite of overwhelming evidence to the contrary. The point is that the handling of ‘physical symbols’ (speaking and listening, writing and reading, as well as the arranging, indexing, sorting, storing etc. of objects that serve as signs) are *genuine activities*; they take time and effort. An actor’s capacity for handling ‘information’ in this sense (as *encoded*) is of course bounded, constrained by his or her skills in handling different coding schemes and in organizing the code, as well as by the technologies at hand. But as Wittgenstein points out, ‘don’t think of understanding as a “mental process” at all’ (Wittgenstein, 1945–1948, § 446). Seeing a house, understanding a gesture, intending some action, having an insight, etc., by contrast, are not activities in this sense; they are not limited by some fixed ‘processing capacity’ but by the conceptual constructs of the community and by the competences of the actor.

3.2.4. Complexity is a systemic category and it makes *no sense* to say that *p* is too complex to be meaningful. The concept of complexity does not apply to the experience of meaning. The experience of meaning does not involve work. The confusion created by such a conflation of categorically different concepts is truly boundless.

The trivially evident limitations on our individual and collective cognitive capabilities are not the expression of the limited capacity of some innate mechanism. Nor can the various limitations be conceived of as expressions of the *same* underlying phenomenon. In the world of situated real-world action, the actor is not a disinterested subject trying to perform absurd tasks such as remembering meaningless syllables. In their everyday activities, actors are not *struggling to cope* with, say, a barrage of ‘sense data’ or an infinite array of possible interpretations and avenues of action. They are not fighting to make sense; their everyday world is overwhelmingly meaningful. The infinite range of possibilities facing the actor is delimited, not by some inherent ‘processing capacity,’ but by the fact that the actor has needs and desires, interests and motives, ambitions and intentions, fears and concerns. The actor is faced with a ‘system of relevances’. There are things to do and things to avoid, immensely practical issues.

However, as pointed out by Charles Babbage (Babbage, 1832, §217), *learning* takes time and effort. Thus, at any point in time, the skills and competences of a particular individual are finite. There are things, many things, that are difficult or even impossible for a particular actor to do at a particular point in time. The experimentalist will for instance easily find that a given subject cannot read a text while simultaneously counting backwards from one hundred. But as demonstrated by Neisser and his colleagues, with time and effort even such perverse tasks can be learned and mastered. More than that, we also acquire techniques that enable us to acquire other competences more efficiently and effectively: actors learn to speak and to read and write, to calculate and to use mathematical notations, to use dictionaries and libraries, and so forth.

In fact, it is absurd to abstract from historically developed and culturally mediated practices. In the words of Geertz, 'Men without culture [. . .] would be unworkable monstrocities with very few useful instincts, fewer recognizable sentiments, and no intellect: mental basket cases' (Geertz, 1973, p. 49). The cognitivist theory of delimited cognitive capacity is a theory of such monstrocities, not a theory of culturally situated, competent actors engaged in perpetual development and refinement of their practices.

That is, in addition to the issue of the 'system of relevances', the trivially evident limitations of our collective and individual cognitive capabilities are expressions of the frailty and fallibility of the vast array of techniques, procedures, representations, notations, etc. we routinely rely on, collectively and individually.

3.2.5. What must be investigated is the 'system of relevances' and the practices that competent practitioners have developed and employ in their daily work. What the concept of complexity helps us to do in this context is to handle important systemic aspects of cooperative work, namely causal relationships, means/ends relationships, part/whole relationships, temporal order, etc., in short, what Schutz call 'imposed relevances'.

Identifying the urgencies and constraints under which actors act and coordinate and integrate their activities only provides the groundwork of the analysis. What is then required is to investigate, in detail, the strategies and techniques, the procedures and routines, the categories and classifications, the criteria and indicators, the artifacts and notations actors use in their actual routine coordinative practices.

4 A Systemic Conception of Cooperative Work

Now, how do we define 'the system' when analyzing cooperative work?

4.1. Cooperative work is often defined by actors' having a 'shared goal'. Although widely popular, especially with social psychologists, the definition is confused. We can certainly talk about 'shared goals', but a 'shared goal' is not a state, mental or otherwise (and even less so is it, in Garfinkel's words, 'a common intersection of overlapping sets' of mental states (Garfinkel, 1967)); it is a practice, the very practice we need to understand. Why this is so is not difficult to see. What is the *criterion* for determining that A and B have a 'shared goal'? Well, that is their obviously concerted effort. Defining cooperative work in terms of a 'shared goal' thus presumes what it purports to define.

Cooperative work arrangements arise because individual actors cannot do the work individually, or cannot on their own do it as well, as timely, as safely, etc. In this sense, cooperative work can be said to be constituted by interdependence of activities. That is, the cooperating actors in a given cooperative work arrangement are interdependent in their work in that one actor's actions will change the state of affairs in some part of the world upon which other actors depend and this change of state, in turn, has implications, directly or indirectly, for the work of the other members of the ensemble, and so forth (Schmidt, 1991c, 1994a). To simplify language, let us call that part of the world the state of which actors affect through their actions, and through which they thereby interact, *the common field of work* of this cooperative work arrangement.

This should not be taken to mean that the field of work is something simple. In the same way as other cases of reciprocal concepts (such as 'figure and ground', 'means and ends', 'text and context', etc.), the concepts 'cooperative work arrangement' and 'field of work' reciprocally define each other: a cooperative work arrangement is defined in terms of the particular part of the world in virtue of which their actions are interdependent, whereas the field of work hand is defined with respect to a particular cooperative work arrangement.

The reciprocity or circularity of the definition does not mean that the concept is arbitrary. The concept of field of work is an analytical construct that denotes certain systemic aspects of working together. It is legitimate as an analytical construct in so far as cooperating actors in their practices *take and have to take* their *interdependencies* as objectively given and *take for granted and have to take for granted* that their colleagues do so and must do so as well. Take, for example, the work of architects (Schmidt and Wagner, 2002b). The not yet existing building for which architects are developing and specifying the plans may be a notional construct in that the building can not be seen in the street and entered, but architects have every reason to treat the notional building as an objective construction; it is, if you will, a 'social fact'. If somebody removes the representation of a load-bearing wall from the CAD drawing, the work of the other architects is immediately and substantially affected, and they will observably act accordingly. Although not yet materially realized, the objectivity of the planned building is taken for granted as a fact and has to be taken for granted as such by the practitioners.

Conceiving of the interdependencies as a field of work does not imply that it exists as a clearly delimited and tangible object or set of objects. It may not, for example, be possible for the cooperating actors themselves, or by an observer, in advance to delimit and define the set of objects and processes the state of which is being affected by the actors, or it may be more or less difficult or costly to do so. If that is the case, as it obviously is in domains of criminal or scientific investigation for instance, the notion of a field of work is not useless, however, but rather allows us to characterize such settings as settings in which actors, as an integrated and crucial aspect of their cooperative effort, cannot take 'the system of relevances' for granted. To manage the quite particular circumstances for orderly interaction posed by such settings, members develop and establish rigorous procedures for assessing and accepting the relevance and validity of whatever is submitted as belonging to the field of work. In other domains of work, crucial aspects of the field of work may be ephemeral, invisible, and intangible, as in the cases of the energy transformations of a nuclear power plant, the economic transactions of the clients of a bank, or the behavior of a software system, or the field of work simply does not exist yet, as in the case of design work. Again, such a description is an illuminative and useful characterization of the conditions of cooperative work in these domains; and in order to establish conditions for orderly interaction in such settings, members develop and use a vast variety of representational artifacts, such as the mass-flow and energy-flow representations provided by the control room, the records of the bank clerks, the drawings of the engineers, the class diagrams and flow charts of the programmers, etc., by means of which the state of the otherwise invisible and intangible crucial aspects of the field of work are made visible and tangible. In short, in order to be able to cooperate in an orderly fashion under such conditions, members so to speak compensate for the indeterminacy, ephemerality, or virtuality of the field of work by objectifying it in the form of representations that are visible at a glance, persistent, and tangible.

Finally, however, what to members is *something to be done*, is an inferred construction to the observer. Constructivism is the observer's privileged stance. To competent members, the field of work is objectively given for all practical purposes. For the analyst, however, it is less straightforward. The interdependencies that constitute the field of work of a given cooperative work arrangement only have hypothetical status until the analyst empirically and systematically has uncovered the interdependencies in members practices and accounts, until the interdependencies have been identified and characterized, etc.

4.2. By involving multiple actors, cooperative work is inherently and inexorably *distributed*, not only in terms of time and space but also, and more importantly, *in terms of 'control'*, i.e., in terms of contingencies, individual heuristics and biases, incongruent specialisms and incompatible perspectives, divergent or conflicting motives and interests, etc. This usage of the term 'distributed' accords with the definition of distributedness suggested by Gasser and Bond: 'Elements of an intelligent system are distributed if there is some *distance* between them,' i.e., 'a *conceptual distance*, with respect to some conceptual frame, such as time, space, semantics, etc.', 'and if some significant cost and/or some intermediary process is entailed in connecting them.' (Gasser and Bond, 1988, p. 8). In other words, as long as we do not master telepathy and clairvoyance, there is a cost to any coordinative act: a cost of effort, a cost of time, etc. Shared goals, shared knowledge, shared understanding, etc. are not defining features or preconditions of cooperative work but rather local and temporary closures, obtained and maintained at a cost. Thus, no actor is all-knowing and all-powerful; actors must act and interact on the basis of partial knowledge and they are, accordingly, partially autonomous in their work (Schmidt, 1991a; Simone and Schmidt, 1998).

4.3. However, from the point of view of systems analysis, a system of processes that are *interdependent* but which are nonetheless controlled in a *distributed* manner will become 'as densely tangled as a plate of spaghetti', so that every action is contingent on multiple other actions and vice versa, and it is thus on the verge of 'trashing around chaotically', to use Waldrop's apt metaphor (Waldrop, 1992, p. 109). I hasten to add, however, that this kind of reasoning is not, strictly speaking, theoretical, as it has no explanatory force. It is simply an elaboration of the concept of system. Anyway, to prevent interdependent and yet distributed activities of a cooperative effort from degenerating into chaos requires, in sociological terms, the additional or secondary activities of alignment, coordination, integration,—in short, what Anselm Strauss and his colleagues have called ‘articulation work’ (Strauss, 1985; Gerson and Star, 1986).

4.4. The distinction between cooperative work and articulation work is a deviously slippery one. Articulation work is not another *kind* of action. One cannot create a taxonomy in which actions are neatly categorized as either cooperative work or articulation work. Cooperative work and articulation work are of the same category. The activities involved in articulating a cooperative effort can themselves be the object of articulation work. Activities undertaken to ensure the articulation of activities within the arrangement (somebody observing another, somebody directing another’s attention to something, somebody asking somebody else about something, somebody requesting or ordering some action, or somebody negotiating actions to be taken) may themselves be observed, negotiated, and so forth. In other words, cooperative work is reflexive. Articulation work is work to make work work. Or to be exact, articulation work is cooperative work to make cooperative work work. The articulation of cooperative work is itself a cooperative effort; its field of work the given cooperative work arrangement.

What is taken to be cooperative work and articulation work respectively is determined by the given perspective; what from one perspective is considered articulation work can be considered cooperative work from another perspective. The distinction is crucial, however, as articulation work poses specific complexities in its own right.

The distinction between cooperative work and articulation work is reflexive. This reflexivity is a fundamental and ubiquitous aspect of human action in general. As pointed out by Talbot Taylor, it is found in everyday metadiscursive forms of expression such as ‘I told you so!’, ‘That’s what she said’, ‘What did he mean by that, anyway?’, ‘Sorry, could you say that again?’, ‘Did she understand what you said?’, ‘That’s not true!’, ‘He said he was sorry’, ‘She ordered me to leave’, ‘Yes, that’s right’, ‘Will you explain that?’, ‘What’s that called?’, ‘I believe you’, ‘Would you ask him to shut up?’ And so on (Taylor, 1997, 2000). Reflexive practices are not confined to linguistic reflexivity or to the practices of sign systems in general (mathematical notations, sheet music, written dance lessons). They are aspects of showing somebody how to strip paint from an old commode, of forming a line at a bus stop, and so on.

Finally, the distinction between cooperative work and articulation work is not merely an analytic distinction; it is a distinction members make, for instance when actors interrupt a meeting to discuss the agenda or interrupt the discussion of the agenda to discuss the protocol of the discussion, or when members of a design project interrupt their design work to have a meeting about, say, the project schedule. In fact, members will typically institutionalize the distinction, for example when the responsibility of certain aspects of articulation work (e.g., chairing meetings, taking minutes, planning and scheduling tasks, allocating resources, monitoring progress) is assigned to or assumed by certain actors (e.g., chair persons, secretaries, coordinators, foremen, project leaders). More than that, actors performing such specific coordinative activities may have acquired specialized competences for such coordinative responsibilities (conductors of symphony orchestras, logistics specialists, production planners).

4.5. In order to understand the complexity of cooperative work we must thus systematically distinguish different 'systems':

(1) The *common field of work:* the constellation of interlaced processes and interlinked objects, actual or anticipated, physical or social, which constitute the part of the world upon which the given cooperative work arrangement is operating as well as the interfaces to these processes and objects, such as sensors, effectors, tools, and representations, e.g., the power plant with its fuels and energy transformation processes, its vast array of cables and steam pipes, as well as the sensors, effectors, and representations of its control system; the steel plant with the scrap and melted steel being processed, the alloying elements and the control system; the ship, its cargo, and its immediate environment; the economic transactions of the bank's clients and the vast repositories of representations of these transactions; the emerging design as represented in drawings, specifications, models, prototypes, test results, minutes, etc.—Also, since the characteristics of the field of work only make sense in view of the operational constraints that are and have to be observed by workers (e.g., safety, quality, timeliness), the identification of these constraints is a necessary aspect of the definition and characterization of the field of work.

(2) The *cooperative work arrangement,* the ensemble of interdependent actors as constituted by a system of interdependent activities: the operators of a power plant or a steel plant; the crew of a ship; the controllers of a train line; the clerks of a bank branch; the engineers involved in the design of an artifact, etc.—as well as the formation of the cooperative work arrangement, that is, the 'first order' articulation work through which the cooperative work arrangement is actually constituted and organized: the mobilization and deployment of actors with respect to activities and resources, the differentiation and configuration of skills, etc.

(3) *Articulation work*: the reflexive system of 'second-order' activities (or aspects of activities) through which the interdependent and yet distributed activities of the cooperative work arrangement, as deployed and configured, are continually coordinated and integrated.

The different 'systems' pose different complexities, and the complexity of one system does not necessarily express itself in another.

5 The Complexities of the Common Field of Work

5.1. In his seminal comparative study of high-risk and high-tech work settings, Charles Perrow (1984) suggests a two-dimensional distinction: on one hand 'complex' *versus* 'linear interactions' and on the other hand 'tight' *versus* 'loose coupling'. This distinction allows him to compare (the systemic risk-potentials of) different 'systems'. Perrow's distinctions are demonstrably quite suitable for identifying systemic risk potentials—he argues convincingly that a 'system' that is both 'complex' and 'tightly coupled' is a catastrophe waiting to occur—and they have been highly influential in the area of human factors research. The same orientation also makes Perrow's distinctions somewhat problematic for our purposes, however.

Firstly, the concept of 'the system' is ambiguous. This becomes evident when, for example, characteristics of the work organization are treated as characteristics of 'the system'. From the point of view of the issue of safety one can definitely justify to consider the socio-technical system in its totality, without discrimination, as the potentially problematic 'system'. But from the point of view of designing computational artifacts for cooperative work, exactly that kind of distinction is critical.

Secondly, the concept 'tightly coupled' is ambiguous as well, in so far as it collapses (a) *causal coupling,* in which sub-processes have no (or few) degrees of freedom and changes propagate without intervention and without modifications, and (b) *intense interdependence,* in which different processes are (largely) causally separate and hence (relatively) decoupled but in which their coupling has to be *ensured*. The various processes of steel production, for example (viz. melting, refining, and casting) are not coupled in any physical sense but they are intensely interdependent nonetheless; it is for the actors to ensure the coupling. By contrast, a mechanically coupled system is characterized by predictable and non-ambiguous interdependencies. A causally coupled system may thus pose interdependencies that are easily managed by a cooperating ensemble. For example, in their classical study of the cooperative operations of a hot rolling mill Popitz et al. noted that the four operators were able to cope with their task—not in spite of but *because of* the rigorous temporal regime its operation imposed on the actors (Popitz et al., 1957).

5.2. In an approach that continues Perrow's work for the design of decision support systems for safety-critical work domains, David Woods (Woods, 1988) distinguishes different complexity factors for problem solving with respect to three basic elements ('the agent', 'the representation', and 'the world'). As far as the complexities posed by 'the world' are concerned, Woods divides the two dimensions suggested by Perrow into four, namely 'dynamism', 'many highly interacting parts', 'uncertainty', and 'risk'.[6] In suggesting these distinctions, Woods gives an important contribution to our ability to understand and analyze work. Again, however, the preoccupation with safety-critical systems makes the categories problematic for the purposes of focusing on the cooperative aspects of work. Firstly, it is not evident why the issue of 'risk' in the relation to the environment should be given a unique status, as other socio-economic issues might be equally relevant, such as, for instance, cost, reliability, flexibility, product quality, quality of working life. Furthermore, and more critically for our purposes, the categories proposed by Woods are not orthogonal: If the complexity of the system as a result of 'many highly interacting parts'

[6] Wood's distinctions hark back to Thompson's (1967).

is intractable (a non-computable problem space, for instance), actors are faced with massive 'uncertainty'. Conversely, 'uncertainty' concerning the current state of the field of work, e.g., due to distortions in the available representations of the state of the system, arguably increases the number of *possible* system states to be taken into account at any point in time; that is, it adds to the complexity of the task.

Be that as it may, the distinctions suggested by Perrow and by Woods provide a fertile ground for developing a conceptualization of the 'system of relevances' of the common field of work.

5.3. Building on the dimensions developed by Perrow and Woods, I submit that we can, as a starting point, assume that the complexity of the common field of work of a given cooperative ensemble will depend on, e.g.:

(i) Structural sources of complexity, such as the number of interacting elements; the heterogeneity of the elements (different kinds of properties, behavior); the number and intensity of interdependencies among elements, the heterogeneity of interdependencies among elements; the degree of freedom of elements (i.e., the number of possible states of each element); the stability, operability, and consistency of constraints, etc.

(ii) Intensity of interdependencies (cf. 'tight versus loose coupling'), such as the rate of spontaneous changes in field of work (i.e., changes not induced by actors); the rate of propagation of state changes among elements of the system; the irreversibility of changes; the system's reactivity to induced changes; the frequency of interactions among elements, etc.[7]

(iii) Apperceptive sources of complexity (cf. 'uncertainty'), such as inadequacy of indicators or sensors (and their representations) with respect to the state of (parts of) the field of work (latency, invisibility, distortions, granularity); inadequacy of effectors with respect to the state of (parts of) the system (sluggishness, distortions, granularity); inadequacy of available information (in terms of, e.g., level of abstraction, granularity, organization), ambiguity, missing information, dubious information, misinformation, etc.

6 The Complexities of the Cooperative Work Arrangement

6.1. Conceived of as constituted by a set of interdependent activities, the cooperative work arrangement is (in principle, conceptually) a transient formation of interdependent actors, emerging contingently to meet specific requirements, only to dissolve again if and when the need for multiple actors and their concerted effort is no longer present. However, the interdependencies of the actors do not in any simple and straightforward way reflect the structural and behavioral characteristics of the

[7] The more intense the interdependencies of activities are, the larger the space of possible states the actor will need to take into account before taking action, especially with respect to the state of the field of work of colleagues, etc.

field of work; they reflect the field of work *as mediated* by a *particular configuration* of the cooperative work arrangement.

The cooperative work arrangement is ongoingly produced in processes of reciprocal 'mapping' of interdependent activities onto interdependent actors, and vice versa, and involves processes of mobilization (who?) and of deployment (what, where, when?).[8] The specific complexity of the cooperative work arrangement consists in the fact that these processes of deployment and mobilization involve multidimensional tradeoffs.

6.2. There is a sense in which actors must be *mobilized*, that is, be identified and at hand as potential actors, before they can deploy.

The formation of a cooperative work arrangement entails an initial cost in terms of (a) the need for identifying likely and appropriately skilled partners and negotiating the allocation of tasks and responsibilities and (b) the need for new partners to acquire particular skills and become acquainted with local settings and practices.

This is complicated by the fact that the constellation of available or potentially available actors with requisite skills and competences is open-ended in that it is contextually determined (i.e., facilitated and constrained) by the socio-economic environment: the historically, culturally, and geographically given levels of education and training; the typical configurations of skills and competences in individuals, the geographical and industrial distribution of actors with different skills and competences, the institutional provisioning of skilled actors (guilds, trade unions, professional associations, etc.), and so on.

However, in order to deal with these issues under the conditions of continual emergence and dissipation of cooperative work arrangements, that is, in order to deal with the costs of mobilization, the formation of such arrangements is typically facilitated by relatively stable configurations of human resources, aimed at a certain range of tasks and situations, which in organizational theory and sociology of work is often termed *the work organization*. The work organization is a contingency plan; more than that, it is, so to speak, an *embodied* contingency plan: it is a plan in the form of a cohort of potential actors.

The work organization should be distinguished from the *system of appropriation* through which resources are committed and pooled and results are distributed, e.g., household and band, farm and manor, village and commune, firm and corporation, department and institution, consortium and alliance. A crucial aspect of

[8] The terms 'mobilization' and 'deployment' are here used as the neutral terms to denote the processes of (a) identifying potential actors and (b) specifying the relationship between actors and activities, irrespective of the particular social form of these processes. Mobilization may thus involve *conscription* as well as *volunteering*. Likewise, 'deployment' may mean somebody's *assigning* somebody else to a particular task as well as somebody's *assuming* a particular task. The point is that the styles and forms of mobilization and deployment vary immensely. When deploying, actors may be commanded to do something, they may be specifically paid to do it, they may be expected to do it, they may believe they might be expected to do it, they may do it because they believe it may be in their interest to do it, they may offer to do it as a helpful gesture, they may do it because it's fun, and so on. Very often all of these styles and forms of governance coexist and complement each other in an infinitely variegated pattern.

the system of appropriation is what in organizational theory is often called the *'formal organization'*, i.e., the institutional practices of contractual governance through which stakeholders (workers, customers, investors, trade unions, professional organizations, authorities, etc.) regulate their diverse, partially incongruent, sometimes conflicting interests and concerns, such as informal conventions for the allocation of responsibility and resources or formalized schemas such as tariff agreements; certifications; budgeting, accounting and auditing procedures; safety regulations; etc.

These distinctions are required and warranted for several reasons. Firstly, they enable us to avoid the gross simplifications of much of sociology of work and organization theory where the organization of cooperative work is typically thought of in terms of reified notions such as 'team work' or 'Taylorism' or 'bureaucracy'. These are rather to be taken as prototypical regimes, specific configurations of specific institutionalized forms of work organization and systems of appropriation. Secondly, and more importantly for our purposes, the distinctions between cooperative work arrangement, work organization, and system of appropriation are essential so as to avoid confusing cooperative work with for instance collocation (working at the same location) or mere employment (working for the same company).

That is, the distinctions help us not to be blind to the dynamics and complexity of the formation of the cooperative work arrangement. The point is, on one hand, that a particular work organization at a certain point in time may encompass multiple, operationally autonomous cooperative work arrangements The night shift of the emergency ward at a hospital, for instance, may at one point in time exhibit multiple cooperative work arrangements working in parallel in caring for different patients, whereas at some other time all staff members may be involved in one and the same cooperative effort, or some may be working while others are on stand-by. On the other hand, a cooperative work arrangement may intersect multiple work organizations, as in the case of emergency management when actors from fire departments, hospitals, police forces, toxic waste disposal agencies, and so on converge on the site and cooperate to deal with the situation. Similarly, a given unit of appropriation such as a firm or a department may incorporate multiple work organizations (shifts, projects, etc.), whereas a given work organization may involve actors from separate companies.

The conventional and institutional practices of the work organization and the system of appropriation are instrumental in reducing the complexity of mobilization, in that they provide an ensemble of potential cooperating actors, as a cohort, with requisite skills and competences, at the appropriate time and location, with the issues of interests and motives contractually regulated. The infinite space of possible constellations of actors is drastically reduced; closure is provided.

6.3. As far as *deployment* is concerned, it is first of all crucial to keep in mind that the cooperative work arrangement and the field of work reciprocally constitute each other; not only in the sense that they define each other, but in a very practical sense. On one hand, the characteristics and changing state of the field of work pose requirements to the cooperative ensemble; but on the other hand it is the 'project' of the cooperative ensemble, the 'things to be done', the 'system of relevances', which identify the field of work. Alternative paths of action may exist that imply interdependencies of perhaps radically different intensity and scope. When exploring and considering alternative paths of action, cooperating actors may opt for strategies that will pose less complex interdependencies. They may acquire technologies that augment the capacity of individuals with respect to certain tasks (chain saws, bulldozers, combine harvesters, calculators) and thus reduce or eliminate the need for multiple actors to be involved in the effort in the first place. Alternatively, they may even abandon the effort as being overly demanding or unacceptably uncertain or unsafe. In the context of working, however, to actors in the 'natural attitude', under the typical conditions of 'imposed relevances' and massive constraints, the field of work is given. It poses practical requirements.

Actors deploy according to vastly different criteria. Actors may, for example, deploy to take charge of particular time periods, or of a particular position, station, desk, machine, room, etc.; or they deploy to take responsibility for specific types or groups of task, product or product family, case, client, customer, etc.

Just as activities that are intensely interdependent may be distributed over multiple actors, activities that otherwise are completely unrelated may be assigned to a particular actor, for the simple reason that he or she happens to be at the relevant location at the pertinent time to do them, but as a consequence of this particular deployment the activities become interdependent, like two universes connected by a wormhole.

Actors may deploy on the basis of incidental individual preferences or propensities; or they may deploy systematically, by virtue of having—by experience, training, or education—acquired specialized skills and competences. Working with a range of different materials or processes, for example, may involve radically different procedures and strategies and hence a range of differentiated skills and competences. Systematically differentiated deployment may also arise to meet requirements in terms of temporal orientation (maintenance versus operation, design versus production), professional ethos (production versus quality control public relations versus research) or similar. But differentiation of skills and competences will increase the scope and density of interdependencies among actors.

Actors may conversely deploy so as to minimize interdependencies between them, for instance, if bounded clusters of activities that are intensely interdependent can be identified and each handled by single actors (or small groups of collocated actors). Similarly, interdependence in action may of course be off-set if individual actors, typically through meticulous and systematic training, acquire the skills and competences required to perform the given cluster of activities. This way, the cooperative effort may be reduced to a set of relatively decoupled jobs that only interface occasionally or marginally or that are interdependent in relatively simple ways. But of course, reducing the differentiation of skills so as to reduce interdependencies among actors implies relatively higher costs of learning and training.

There is thus an obvious trade-off between the cost of learning and training and the cost of having to handle intense interdependencies.

The complexity of deployment arises from the infinite array of possible mappings and it is, again, handled by practices that drastically reduce the space of possibilities. Specific patterns of deployment become routine and taken for granted; a certain division of labor and responsibility among actors is institutionalized.

6.4. Irrespective of the particular deployment and mobilization practices, *a cooperative work arrangement invariably exhibits a characteristic 'topology' of 'local practices',* i.e., of particular concerns, priorities, criteria, principles of aggregation and abstraction, technically specific routines, principles of ordering, concepts, etc. that are 'localized' to certain members in certain physical or organizational locations, to members dealing with certain tasks, to members having particular professional responsibilities, etc. Cutting sheet metal, for example, involves radically different technical skills and practical orientations than machining, drilling, and grinding, even though they all are operations of changing the geometry of a work piece through mass reduction, and such processes of mass reduction are in turn radically different from operations of deformation such as bending, forging, and rolling, or operations of joining pieces such as welding, gluing, and mechanical assembly. Radically different principles of temporal and spatial ordering are involved.

The point is not that these skills (or any other combination of skills) cannot be mastered by a single individual, or that radically different principles of ordering in general cannot be integrated in the competence of one person, which they obviously can, but that the acquisition of skills and the integration of radically different skills takes time and effort. More generally, the integration of skills, strategies, conceptualizations, etc. is *work*, and as all work foremost the practical issue is one of weighing costs and benefits.

All this is, of course, just another way of saying that cooperative work is *distributed*, but the point of expressing it this way is to emphasize that not only are issues, concerns, priorities, criteria, concepts, strategies not globally and permanently shared, but they are typically unevenly distributed. Firstly, cooperative work arrangements are, so to speak, often 'lumpy', in that there typically are virtual or actual localities of work where actors have comparable skills and competences or where differently skilled actors have the same or compatible tasks and responsibilities: trades, lines, specialties, professions, communities of practice, sections, departments, institutes, offices, groups, teams, task forces, projects, etc. Secondly, cooperative work arrangements should be seen as superimposed and intersecting networks of *differently* interdependent activities. What can be conceived of as a cooperative work arrangement from one perspective may from another perspective be seen as a subset of a wider arrangement or may conversely be seen as comprising multiple constituent arrangements. Thirdly, cooperative work arrangements intersect in different ways. For instance, as noted in passing above, if a particular actor is contributing to different and otherwise independent cooperative efforts, these efforts are then coincidentally linked, and this linkage then gives rise to particular local coordinative issues and concerns. Cooperative work arrangements may similarly intersect by 'sharing' resources such as archives, buildings, rooms, machinery, equipment, etc.[9] In sum, cooperative work is characterized by a fundamental and inevitable interplay of local and global contingencies and issues.

7 The Complexities of Articulation Work

7.1. As indicated above, the activities of mobilization and deployment—the very constitution and reconstitution of the cooperative work arrangement—can be seen as articulation work 'of the first order,' so to speak, as opposed to articulation work 'of the second order' as the activities through which the activities of the cooperative work arrangement, as already constituted, are coordinated and integrated. The distinction is not categorical but pragmatic. It is not relevant in so far as the cooperative work arrangement is transient. In typical cooperative work settings, however, cooperative work arrangements are enduring, or they recur regularly, and the specific articulation activities involved in the initial constitution of the cooperative work thus do not have to be reiterated, or reiterated completely. It is the 'second-order' articulation work involved in the enduring or recurring cooperative effort that concern us here, and in order to avoid confusion the term 'articulation work' will be used in the remainder of this paper in the latter sense.[10]

Since the different distributed actors, while facing local contingencies and concerns, are *interdependent,* and since the cooperative work arrangement spans an array of local practices, the cooperative work arrangement, in its totality, involves activities performed under different, perhaps incongruent criteria, concerns, procedures, strategies, etc., which have to be aligned, coordinated, integrated, in short, articulated.

From this perspective, then, articulation work ('of the second order') is to be conceived of as the system of activities by means of which the members of the cooperative work arrangement, as deployed and configured with respect to a particular field of work, align and integrate the (differentiated and intersecting) local issues, concerns, priorities, criteria, and so forth, not only 'locally', as in 'just here and now', but also with respect to the remote but related or intersecting local issues etc. of other localities of the cooperative work arrangement and of the wider network of cooperative work arrangements.

[9]Cf. Leigh Star's concept of 'boundary objects' (Star and Griesemer, 1989; Star, 1989).

[10]It is worth noticing that the two usages of the term 'articulation work' are reflected in the literature. On one hand, Anselm Strauss consistently uses the term 'articulation work' in the sense of 'first order' articulation work. To him the 'granularity' of the concept is defined by the concept of 'tasks' and articulation work is simply same as the 'articulation of tasks' as he also puts it (Strauss, 1985). By contrast, Gerson and Star do not treat 'task' as a black box; they open up the box and generalize the concept of articulation work to denote the ongoing adjustment of action in view of inexorable contingencies: the concretization, instantiation, adaptation, modification, etc. of routines, plans, and representations (Gerson and Star, 1986).—In my usage of the term 'articulation work' I am following Gerson and Star.

As already mentioned, cooperative work is reflexive in that working involves secondary work to make cooperative work work. The point is that articulation work is cooperative work from another perspective; its field of work being the given cooperative work arrangement. The complexity of cooperative work is thus reflexively also the complexity of articulation work.

7.2. The claim that articulation work *is work* and thus requires effort is not self-evident, however. Are the practices of articulation work indeed distinct from work practices? An immensely complicating issue here is that cooperating actors in typical work settings continually align their activities with those of their colleagues and that they do so in a fluent and seemingly effortless manner. It thus may seem meaningless to talk about complexity of articulation work in the first place.

7.2.1. The practices of mutual alignment appear to be effortless for two quite distinct reasons.

Firstly, competent actors make sense of and understand what colleagues are doing on the basis of their knowledge of and familiarity with the structural and behavioral characteristics of the field of work. Changes to the state of the common field of work will, so to speak, emit signals that can be observed, not only by the actor directly effecting the changes but also by colleagues, perhaps in the same location, perhaps in a different location, perhaps 'directly' (i.e., with their senses through 'natural' media), perhaps technically mediated (i.e., by means of sensors and representations and other intermediate technologies). From observing the state of the field of work and the changes to it, that is, without necessarily being able to directly perceive each other and each other's bodily conduct, competent members normally understand the situations faced by their colleagues and what their colleagues are doing, are not doing and will be doing, which enables them to align their own activities with the conduct of their colleagues and thus to accomplish the joint work in an orderly fashion. This appears effortless, because, to a competent member—in the flux of doing the work and thus attuned to the changing state of the field of work—what the colleagues next to him are doing is immediately meaningful; it does not require interpretation, reflection, contemplation to know why they are doing what they are doing or why they are not doing something else. Competent actors do not *infer*, they *see* the problems and intentions of their colleagues. In that sense, the mutual awareness among cooperating actors that can be observed in many cooperative work settings is practically effortless.

Secondly, the practices of mutual alignment play out without obviously interrupting the flow of work, in contrast to other coordinative practices, such as directing attention to something, searching for information, asking about something, requesting or ordering some action, negotiating actions to be taken, etc., which are all characterized by being intrusive in that they as 'speech acts' (under conditions of social accountability) 'enforce' a response or some other interruption of ongoing action. The practices of mutual alignment thus do not, normally, appear distinct from primary work practices and thus do not appear as specific coordinative practices. However, in spite of this virtually seamless integration, the practices of mutual alignment are not strictly speaking *effortless*. Actors' mutual alignment is predicated on selective and active monitoring and displaying. A range of workplace studies (e.g., Heath and Luff, 1992a; Harper and Hughes, 1993; Shapiro et al., 1994; Heath and Luff, 1996) have demonstrated that the apparently effortless mutual alignment and integration of activities do not occur through osmosis or some other 'automatic' process. In doing their individual parts of the joint effort, actors will typically *modulate* their own activities in such a way that they provide their colleagues with cues and other coordinative resources pertinent to what concerns they may have. In doing that, however, competent actors do not display their own local agendas and issues conspicuously and comprehensively, since doing that would add to the complexity of the work of colleagues; actors rather make their own activities publicly available in a form and at a level of granularity which is appropriate to the situation facing the colleagues. In short, the way activities are modulated is tailored to the particular situation at hand. Conversely, competent actors continually 'monitor' what colleagues are doing so as to ascertain how these activities are being performed and whether they are progressing as expected, to determine exactly how their own activities should be adjusted to be integrated with the unfolding work of the colleagues, and so forth. Again, this monitoring of the work of colleagues typically is not done conspicuously, because doing so might make the colleague being watched *aware* of being watched—unless, of course, *that* is desirable, for instance for safety purposes (Schmidt, 2000).

The coordinative practices of mutual awareness and alignment are highly efficient and effective for the simple reason (a) that they utilize signals incidentally emitted by the field of work and thus exploit what is there for the picking, so to speak, and (b) that the subtle practices of directing attention by modulating activities are integrated with the 'first-order' work practices of simply doing the work. In short, these practices appear effortless because the indicators of states or state changes in the field of work and of colleagues' intentions are ready-at-hand or are easily made ready-at-hand.

7.2.2. The virtually effortless character of mutual alignment of cooperative activities is bounded, however, as it is rooted in practices of limited scope.

Actors may have differential access to assessing the state of the field of work of each other. To understand this, it is helpful to consider a cooperative work setting where the cooperating actors are unable to directly perceive each other and each other's bodily conduct (Popitz et al., 1957). That is, the actors are restricted to interacting by changing the state of the common field of work and to assess the state of affairs through observing the state of the field of work. The actors may be deployed in such a way with respect to the field of work that they are all practically able to directly perceive the state of the field of work in its entirety or they may be deployed in such a way that they can directly perceive only a particular region of it. Likewise, changes to the state of the common field of work will unfold and propagate within the field of work in different ways, through different routes and at different velocities; state changes will cross boundaries between regions and will be distorted accordingly by various mechanisms of selection, abstraction, aggregation, concatenation, etc., and as they unfold and propagate they will be observable by members in different ways due to different representations. If the field of work is 'tightly coupled' (e.g., a power plant or an aircraft), changes may affect the work of others instantly and without exception; in a 'loosely coupled' system, by contrast (e.g., a building under construction or a software design under development), changes may propagate sporadically and contingently.

Moreover, different fields of work may offer different affordances (degrees of freedom, means of expression) for actors to 'modulate' their activities so as to display pertinent aspects of their work or direct attention to certain aspects of the field of work to others or, conversely, for actors to monitor the activities of others. In some work domains, objects that are part of the field of work may be used for conveying cues to colleagues. In office settings, for example, workers can be seen placing a file on the corner of the desk to indicate that they have finished using it or leaving a file on the chair of a colleague to indicate that this is somehow urgent. Similarly, but in a quite different domain, fighter pilots will tip the wings of their aircraft to signal some intention to other pilots. On the other hand, the operators of a hot rolling mill do not have such degrees of freedom (Popitz et al., 1957). They are restricted to the un-modulated changing state of the of the field of work as a source of knowledge of the state of the work of their colleagues, their plans and intentions, the disturbances they are facing, etc.

Collocated cooperating actors are of course typically not confined to the changing state of the field of work and the action modulations afforded by the field of work as a means of mutual awareness and alignment. They will for example often be able to observe each other's bodily conduct, overhear each other's conversations with other actors, direct attention, engage in conversations, etc. For example, a nuclear power plant operator notices his colleague moving to the other end of the control room, to a particular set of control panels, and may assume that the colleague is going to initiate certain changes which in turn will affect himself in his own work (Kasbi and Montmollin, 1991), or an air traffic controller overhears radio conversations between his colleague and a pilot and takes appropriate steps (Harper and Hughes, 1993).[11] But what was said above about actors' ability to modulate their activities, applies to bodily conduct as well, in so far as the modulation of activities in order to convey coordinative cues to colleagues in some settings may disturb or even perturb work. It may, for instance, be deemed unacceptable if a violinist in a chamber ensemble jumps up and down for coordinative purposes, whereas the very same form of conduct may be perfectly acceptable in a rock band.

7.2.3. Anyway, the costs of learning the subtle coordinative practices of picking up signals and cues and of modulating action may be significant due to the inevitable

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[11] It should be emphasized, however, that actors observe and understand the bodily conduct of colleagues, not simply on the basis of some putatively generic and innate semantic scheme of postures and gestures, but *with respect to* and *in terms of* the actor's knowledge of the state of the field of work. I am stressing this because the role of bodily conduct in cooperative work is often idealized and overstated. An actor's observation of the bodily conduct of a co-worker may or may not be critical in a particular setting or situation but it is always grounded in the observer's understanding of the structural and behavioral characteristics of the common field of work, its current state, as well as the operational constraints and procedures and is thus an integrated aspect of their domain-specific professional competences.

heterogeneity of the cooperative work arrangement. Since the skills of a given individual or ensemble are limited at any point in time, members' knowledge of 'remote' regions of the field of work may be limited and patchy and their ability to make sense of what actors engaging these remote regions of the field of work are doing may be equally limited and patchy. That is, the scope of effortless mutual alignment is limited by the parochial character of actors' domain knowledge. The specific topology of the cooperative work arrangement is thus reproduced in the practices of articulation work. In this sense, the complexity of cooperative work is directly reflected in the complexities of articulation work. Articulation work accordingly involves the coordination and integration of local practices with practices 'beyond the pale', at remote regions of the field of work.

7.2.4. In sum, then, the practices of mutual awareness and alignment enable practitioners to coordinate and integrate their individual activities in practically effortless ways, but only within the scope of local practices. That is, articulation work inevitably requires effort.

7.3. The specific complexity of articulation work arises and becomes intractable as interdependencies transcend local practices.

(i) Cooperating practitioners are typically faced with remote and perhaps incongruent local routines, agendas, criteria, perspectives, principles of interrelation, aggregation, and abstraction, principles of temporal ordering (urgency, reversibility), etc., with which they nonetheless have to coordinate and integrate their own local activities. As already pointed out, we need not and should not presume that the required translations and re-conceptualizations are impossible. The important point is that such translations and re-conceptualizations *are work*; it takes time and effort. But since the requirement of general or global consistency is only one among multiple issues and constraints, consistency can only be achieved, under normal practical conditions of expediency and urgency, at the expense of local issues and constraints.

(ii) To local actors, the ramifications and repercussions of their local activities to other regions of the cooperative effort are not immediately and straightforwardly evident; the same applies, vice versa, to the local implications of remote activities, of course. There are several reasons for this, in addition to the costs of translation and conceptualization. On one hand, remote state changes to the field of work are subjected to various deformations (delay, abstraction, transformation, distortion, etc.) as they propagate through the field of work. On the other hand, for actors to compensate for these deformations through communication takes time and effort and may interrupt the flow of action.

For example, take Peter Carstensen's study of a software development project (Carstensen, 1996; Carstensen and Sørensen, 1996). In previous projects the systems they had been constructing had been small (as measured, for example, by the number of lines of code) and the programming work had been done by single programmer or perhaps a couple of programmers. In these projects they had been able to manage their interdependencies practically effortlessly. They had been working next to each other and had had practically unconstrained access to consulting each other and to monitoring each other's work. At the time of the study, however, a new project had been undertaken in which the engineers were building a significantly larger system comprising many hundred thousands lines of code. Their traditional coordinative practices were now quite inadequate. The interdependencies of their cooperative effort now transcended the local practices, and they were faced with interdependencies that had ramifications and potential repercussions that were unknown and unknowable to them with the available resources. To deal with the ensuing crisis, the ensemble developed a new work organization with new roles and with a set of coordinative practices based on constructs such as coordinative protocols and artifacts.

7.4. In order to handle the complexities of articulation work that arise as interdependencies transcend local practices, practitioners routinely and pragmatically develop a variety of practices that serve to regulate (curtail, contain, suppress, harmonize, standardize, interrelate, synchronize, etc.) local practices.

The articulation of cooperative work is first of all typically a *practice* in the sense of distinct established practice involving a repertoire of specialized techniques and procedures that are developed and refined as well as taught and learned by members. Articulation work involves cooperative work practices by means of which *local* cooperative work practices are integrated; *practices that integrate practices*: *coordinative practices*.

Such coordinative practices are characterized by the crucial role played by *coordinative artifacts*: calendars; memos, agendas, and minutes; records and archives; catalogues and taxonomies; maps and charts; standard operating procedures; forms and templates; schedules and production plans, and so on and so forth. Contemporary workplaces are littered with coordinative artifacts of different kinds that serve different coordinative functions:

(i) Product standards, blueprints, drawings, 'style sheets' may serve as 'templates', that is, artifacts that specify the properties of the result of individual contributions (cf., e.g. Turnbull, 1993). By defining interfaces between local practices, such artifacts may provide means for a relative decoupling of interdependent activities and hence for reducing the complexity of articulation work.

(ii) Bulletin boards, archives, taxonomies, maps, charts, procedures, schedules, etc. may reduce the cost of taking remote or global concerns into account by making remote or global concerns 'visible, e.g., by providing representations of interdependencies, by providing representations of the state of affairs, by highlighting crucial issues, by affording the calculation of possible or probable remote effects of local actions, etc.

(iii) Calendars, clocks, agendas, time tables, flight plans, project schedules, production plans, production control systems (MRP systems, kanban systems), etc. may be instrumental in synchronizing local activities that are otherwise decoupled and performed concurrently or intermittently. Alerts, calls, and reminders can be seen as similar but more intrusive means of synchronization.

(iv) Flight deck checklists, safety procedures, flight databases, workflow systems, production control systems, etc. may be instrumental in prescribing certain aspects of local activities (steps to be taken or not taken, the sequence of steps, alternative steps, criteria, etc.). By stipulating the course of action in certain ways and thus curtailing parochial aspects of those practices, such coordinative artifacts are introduced and used as means of increasing operational safety, reliability, efficiency, etc.

(v) Complex artifact-based coordinative practices such as notations, nomenclatures, and classification schemes may serve to promote consistency of certain aspects across boundaries between local practices. By offering standard schemes of description, naming, classification, identification, etc. such coordinative artifacts are instrumental in reducing the cost of translation and re-conceptualization across regions of practice. At the same time, of course, due to the implicit bias in favor of global concerns such artifacts may also be instrumental in increasing the complexity of local practices.

What seems to obtain as a general characteristic is this. A given coordinative artifact offers competent members a limited selection of options that are considered necessary, mandatory, relevant, safe, secure, legal, valid, advisable, efficient or otherwise prescribed with respect to the field of work in general and to the current state of the field of work in particular, while excluding options that generally would be considered irrelevant, superfluous, unsafe, etc. By reducing the space of possible options, coordinative artifacts assist competent actors in reducing the complexity of coordinating their activities. Under conditions of limited resources, practical exigencies, and social accountability actors rely on such prescriptions to get the job done, unless they have good reasons not to do so (Schmidt, 1997).

7.5. Coordinative artifacts are specialized constructs, devised to deal with certain aspects of interdependencies. The reason for this is not only practical. It is not just that such artifacts are constructed in a distributed fashion, although this is obviously an issue. Nor is it that their construction requires effort and time and that resources finite, although this is most certainly also an issue. The reason is, *au fond*, that 'No representation of the world is either complete or permanent (Gerson and Star, 1986, p. 257).

On closer inspection, one finds that these specialized constructs form complexes of interrelated coordinative practices and artifacts. Ina Wagner and I have suggested to call such complexes *ordering systems* (Schmidt and Wagner, 2002a). For example, in most if not all contemporary work settings one will find a complex of artifacts and practices that in their totality are devised and used for organizing meetings: not only agendas and minutes, but also clocks, calendars, room numbers on floor plans and doors, and so on. Other ordering systems can be distinguished in the composite practices of, e.g., handling large-scale collections of items or complex work flows. What we observe in any event are ordering systems consisting of interrelated artifacts, notations, nomenclatures, standard formats, validation procedures, schemes of temporal, spatial, conceptual, etc. ordering, and so on.

To members these complex practices are instrumental in managing interdependencies that transcend local interactions. They are deliberately and carefully devised in a cooperative process to regulate and align local practices and are being used that way, that is, to enforce requisite coherence, keep track of potential repercussions, ensure accountability, etc. Moreover, to handle the inevitable contingencies of work, ordering systems are open ended. Classification schemes, for instance, are amended or extended to deal with specific features of particular projects; new notations are invented and introduced as required, and so forth.

In the forming of ordering systems, coordinative artifacts and practices form intricately recursive relationships where one distinct coordinative practice may regulate, constrain, define, modify, etc. another.

The reflexivity of cooperative work has no end.

Chapter 10
Ordering Systems (2004)

Coordinative Practices and Artifacts in Architectural Design and Planning

1 Introduction

For years, mainstream CSCW research has been focusing on understanding and developing technologies that can support the immediate interaction in small groups. In fact, the field has often been defined in terms of 'group work' (e.g., Greif, 1988b) or even 'small groups' (e.g., Grudin, 1994, 1999). Face-to-face conversation is implicitly taken as the paradigm of human interaction, and in comparison all other forms of human interaction are seen as impoverished. The motivation for this focus has generally been the putative need for technologies that can help cooperating actors to emulate such immediate interaction over physical distance. Hence CSCW's perennial obsession with various brands of the conversation paradigm such as 'media spaces', 'collaborative virtual environments', 'virtual rooms', 'virtual work spaces', 'instant messaging', etc.

Because of this, however, mainstream CSCW has been and remains preoccupied with issues that may not be strikingly relevant to the bulk of real-world cooperative work settings.

As pointed out already in 1989, 15 years ago, 'the concepts of "group" and "group work" designate specific types of cooperative relations characterized by shared responsibilities.' It was furthermore argued that 'we certainly do not want to restrict the scope of CSCW to those cases where the responsibility of performing a task has been allocated to or assumed by a relatively closed and fixed collective [. . .]. Cooperative work comprises indirect as well as direct and distributed as well as collective modes of interaction.' (Bannon and Schmidt, 1989, pp. 361 f.).[1]

Teamwork is a fairly common phenomenon, of course. But in the contemporary world of work, teamwork is not the typical form of work organization, and it is, in any event, almost always embedded in wider cooperative work arrangements. The

Kjeld Schmidt and Ina Wagner: 'Ordering systems: Coordinative practices and artifacts in architectural design and planning', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 13, no. 5–6, 2004, pp. 349–408.

[1]Schmidt and Bannon presented a more thorough version of this argument in a later article (Schmidt and Bannon, 1992).

K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1_10,

fact of the matter is that contemporary cooperative work is generally characterized by heterogeneous and often widely ramified arrangements of actors immersed in complex interdependencies of varying scope, intensity, and degrees of coupling. Ample proof of that has been provided, over the years, by the continuous trickle of ethnographic studies of actual cooperative work, 'in the wild', i.e., in domains such as air traffic control, hospitals, manufacturing, aircraft maintenance, software engineering, etc.

Ordinary discursive interactions such as conversations over the telephone, in hallways, or on the shop floor, in scheduled meetings, or in the form of email exchanges, and so forth, are of course an integral part of cooperative work. But in the larger scheme of things, cooperative work is characterized by an infinite variety of non-discursive interactions. That is, for example, in doing his or her individual work one actor changes the state of an object (a machine part, a drawing, a piece of software code), not for the purpose of conveying a message, but for the purpose of simply doing the job; noticing the result of this, another actor, perhaps at an other location, perhaps at a later stage, makes other changes to the state of the object or other objects, and so on. All kinds of variation of this pattern are at play. Parts, materials, documents, etc. may pass from actor to actor, in the sequential pattern of a 'work flow'; or the responsibility of undertaking certain tasks may pass on from actor to actor, in a sequential 'task flow'. The cooperative interaction may be strictly sequential, but it may also take the form of reciprocal interaction, just as it may be interspersed with stretches of concurrent activities. In fact, cooperative work may involve any mixture of concurrent, sequential, and reciprocal action and interaction. In some cases the actors know each other, but often they do not. In some cases the interaction may involve conversation, but often it does not.

More than that. It is typical of cooperative work in modern work settings that multiple actors so to speak interact 'through' a *collection* of artifacts of various kinds. An actor may for example place a document in a location where others can see it and use it. The actor may place it there for his or her own later use, but at the same time others may access it too, or the actor may place it there deliberately for the benefit of others. Perhaps the identity of the others cannot be known in advance, perhaps it can. Anyway, whatever the reason for putting it there, since other actors have access to the document, those others may retrieve and use it at some point in time, perhaps for purposes or in contexts that are quite different from those of its origination. It may also be the case that another actor may change the content of the document, change its location, rename it, trash it, etc. Such practices are ubiquitous in contemporary cooperative work.

Again, the location of the collection may be a particular shelf, rack, room, or building, or the collection may be distributed over a number of shelves, racks, rooms, and buildings. The collection may comprise digital documents as well as paper and other tangible artifacts. The items in the collection may be documents such as books, reprints of scientific articles, technical reports, invoices, design specifications, software code, blueprints, etc. Or the collection may contain parts and sub-assemblies, zoological or geological samples, works of art, forensic evidence, etc. Or the collection may be a heterogeneous assembly of such artifacts. Anyway, however distributed and heterogeneous the collection may be, in their cooperative activities actors will (in some ways) interact and coordinate their activities through this collection, by changing its state.

The salient characteristic of contemporary cooperative work is that the actors generally interact 'at arm's length'.

As soon as we remove the blinders of the 'group work' paradigm and look at cooperative work in its rich diversity and complexity, it also becomes abundantly clear that contemporary cooperative ensembles would fail—completely and utterly—in their collaborative effort if they could only coordinate and integrate their activities by means of ordinary discursive practices, whether face-to-face or remote, oral or written. In fact, contemporary cooperative ensembles depend heavily on a range of highly specialized, standardized coordinative practices involving a concomitant repertoire of equally specialized coordinative artifacts. It is such practices we are investigating here.

We are of course not the first to address the crucial role of inscription and material artifacts in cooperative work. A number of interesting studies have been published over the years (e.g., Haas, 1996; Henderson, 1999) and some very insightful analyses have been made as well (e.g., Nardi, 1993; Sellen and Harper, 2001). Our work builds on this work and can be seen as an attempt to take the issue a bit further by adopting a both more comprehensive and more systematic stance.

Nor are we, of course, the first to address specialized, standardized coordinative practices. Recognizing the critical importance of such coordinative practices, a number of CSCW researchers from different research traditions have developed a range of more or less elaborate and more or less successful frameworks that address specific types or aspects of coordinative practice such as, to take some examples, 'organizational memory' (Conklin and Begeman, 1988; Conklin, 1989; Ackerman and Malone, 1990; Conklin, 1993; Ackerman, 1994); 'common information spaces' (Schmidt and Bannon, 1992; Bannon and Bødker, 1997); 'workflow systems' (Grinter, 1996a; Bardram, 1997); 'coordination mechanisms' (Carstensen and Sørensen, 1996; Divitini et al., 1996; Schmidt and Simone, 1996; Simone and Divitini, 1997); 'boundary objects' (Star and Griesemer, 1989; Star, 1989; Bowker and Star, 1999; Lutters and Ackerman, 2002), and so forth.

None of these frameworks tells the whole story; nor does any of them claim to. They are also of rather different status. Where 'common information spaces' and 'organizational memory' are metaphorical rather than conceptual constructs, others like 'coordination mechanisms' aim to offer more concise conceptual frameworks for analysis and design. Moreover, the different frameworks highlight and express different aspects of coordinative practices. For example, while the concepts of 'workflow' and 'coordination mechanism' focus on coordinative practices of accomplishing temporal order (schedule, sequence), concepts such as 'common information spaces' and 'organizational memory' focus on coordinative practices of managing collections of discrete items or artifacts, whereas the concept of 'boundary objects', in turn, highlights practices in which activities in distinct local settings are partially concerted by ‘objects’ on the ‘boundary’ between the settings. This is all well and fine but cannot hide the fact that this array of frameworks, when taken together as a conceptual foundation for CSCW, is defective. The picture they together let us see and conceive of is not only incomplete but in fact rather patchy and incoherent. Not only do each of them suffer from a variety of ambiguities and shortcomings. The different conceptualizations they offer are not orthogonal. And, worse, how the conceptualizations (workflow, procedure, object, collection, classification, etc.) underlying the different frameworks might be interrelated has not been specified or investigated. Consequently, by virtue of their partial character, the various frameworks treat as separate what is logically interconnected. Since the distinctions they impose are largely arbitrary, no rigorous systems architecture can be built on this as a basis. This state of affairs is obviously unsatisfactory.

Based on an in-depth ethnographic study of coordinative practices and artifacts in architectural design and planning, the present article is intended as a first attempt at a systematic and comprehensive analysis of coordinative practices in their complex interconnectedness. We do not offer a conceptual framework, not to mention a theory, but we hope to be able to outline a substantiated approach.

Our article is based on systematic fieldwork carried out in the course of 5 years in an architectural office.[2] In this period, the planning of several large buildings was studied. One of them, *Pleasure Dome*, an entertainment center in the *Gasometer* area in Vienna, houses a cinema center with several movie theatres, a shopping mall, and a garage complex (Fig. 10.1).

Fig. 10.1 *Pleasure Dome*: Patchwork façade (*left*) and interior view

[2]The field work was carried out at Architekturbüro Rüdiger Lainer in Vienna. When we, in the following, use the term ‘the architects’ we are referring to the architects in this office. In addition, however, a series of interviews were made with architects in other Austrian offices so as to corroborate that, with variations, the practices we observed are common practices. We have finally also, for purposes of comparative analysis, used findings from Danish studies of coordinative practices in rather different domains such as pharmaceutical product development and manufacturing.

In addition to this project, an urban planning project as well as the preparation of a variety of design proposals for various competitions were studied. A combination of participant observation with interviews was used. A large number of cooperative work situations was observed. The field work included collecting (scanning, taking screenshots or photographs of) artifacts used and produced by the architects.

2 The Complexity of Architectural Work

Architectural planning and building today face increasingly complex demands. Modern architects have to deal with issues and interdependencies which previous generations of architects did not have to handle. In the course of the industrial revolution, the needs of industry led to the introduction of facilities for heating, ventilation and sanitation, which began to be applied to domestic architecture as well. Central heating in the form of steam-heating systems appeared in the early nineteenth century; cold- and hot-water systems and sanitary plumbing developed rapidly in the second half of the same century. Gas lighting came to London in 1809 and, in the words of a historian of architecture, by the 1880s electric light was 'available to those who could afford it and were prepared to take the risk of using it.' Elevators, telephones, and mechanical ventilation were introduced in the last decades of the nineteenth century. These developments of course impacted on the work of architects. As the historian puts it, 'However much people may have regretted or been frightened by the scale and rapidity of the changes, what had been produced in a 100 years was a whole new range of possibilities, and therefore a new aesthetic and a new challenge to the designer. How was he to cope with change and express architectural qualities in such a revolutionary milieu?' (Nuttgens, 1997, p. 245).

In the course of the last century, the 'range of possibilities' was further extended by the introduction of air-conditioning systems, facilities for tele-communications, security, safety, firefighting, etc. Moreover, architecture has grown into an important marketing instrument, for business and politics alike, and architects are expected to create images and develop novel visual strategies. The ecology of materials and techniques is of growing importance and is connected to requirements such as energy efficiency and renewable building materials. A whole new range of building materials and methods of pre-fabrication has been developed, requiring planners to select, combine, and assemble different materials and construction packages in innovative ways. Using the example of a façade, Pietroforte argues:

> The representational fragmentation of its components, such as granite veneer panels, supporting steel trusses, windows, insulation, water drainage system and caulking, makes the overall assessment and the achievement of the system's functional continuity more difficult. This problem is coupled with the fragmentation of the façade engineering knowledge as it is reflected in the iterative development of the shop drawings, their lengthy review process with hundreds of notes and corrections, many coordination meetings and, above all, in a time consuming testing of full-size façade mockups (Pietroforte, 1997, p. 77).

In addition to the technical complexity of buildings and their construction, the increasing cost-consciousness of clients forces planners to consider maintenance costs, special services for users, and changing social uses from an early stage on. The challenge to architects is not only an aesthetic one, but an immensely practical one, as the vastly increased 'range of possibilities' implies a vastly increased number of structural and functional elements of the building to be integrated in the design and in the construction plans.

As a result, the work of modern architects is intensely collaborative. It is a fine example of what we in the introduction described as a mixture of concurrent, sequential, and reciprocal action, partly co-located and partly spatially distributed, sometimes involving conversation and more often not. In a typical large building project various actors work on different sections of the building and they may be responsible for particular design tasks. Typical of a normal workday within an architectural office is that work proceeds quietly, with little movement, a pattern now and then interrupted by bursts of conversation and discussion. People will occasionally convene for an internal project meeting or for a meeting with an external consultant. At intervals, too, scheduled coordination meetings are held in which all team members participate; they here assess the progress of work, settle dates, confirm responsibilities, redistribute work (if required), and talk about common issues such as, e.g., design changes. Participants leave the meeting with personal notes of what to do and what to be aware of in their work, with some common understanding of the progress of the work, open issues, dates, and deadlines.

Beyond the project team, a building project engages many external actors: technical consultants (for construction, electricity, heating and ventilation, the lighting concept, the façade, etc.), a client and perhaps one or several users, numerous authorities, building companies, contractors, and sometimes, depending on the size of the project and the contract, a general contractor. Enlisting these many professional competencies, mobilizing their support, and integrating their perspectives is a major task that requires careful preparations and ongoing communication. The planning process is a 'collective problem-solving process in which different parties tend to influence the process at different stages' (Pietroforte, 1997, p. 79).

There is also a critical time aspect involved in planning. Negotiations with relevant parties not only involve multiple complex issues but also engaging in time-consuming procedures, e.g., those of local authorities that follow their own logic of bureaucratic functioning and political compromising. The total time span from preliminary design to construction may be several years, and it may happen that well thought-out design decisions eventually turn out to be too costly or no longer technically feasible. As some of these external parties have the power to legally approve or not approve the design, recurrent negotiations have to be conducted (Table 10.1).

The planning process is organized into legally defined stages with defined products: pre-design, design, construction planning, and implementation. The ways these stages and the roles of different participants in the process are defined vary from country to country. Each stage results in a corresponding set of CAD plans for

Table 10.1 Example of a network for a large building project

<table>
<tr><td rowspan="2">Users
Bring in their 'Corporate Identity.'
Define requirements.</td><td rowspan="2">Client
Formulates 'room program.'
May contract architect and technical consultants.</td><td rowspan="2">General contractor
Moderates and controls the planning and implementation process.</td><td>Local authorities
For different aspects of the building
Control, authorize.</td></tr>
<tr><td>Jury
Sets criteria, evaluates, authorizes.</td></tr>
<tr><td colspan="2" rowspan="3">Consultants
Construction engineer, heating and ventilation specialist, building engineer, traffic specialist, specialist for façades, lighting designer, etc.
Give technical advice.
Cooperate in design of details and call for tender.
Formally approve of design details.</td><td colspan="2">Architectural office
'Artistic director' - responsible for overall design
Coordinates planning process.</td></tr>
<tr><td colspan="2">Partner office
Supports costing and call for tender, surveys implementation at construction site.</td></tr>
<tr><td colspan="2">Vendors and other building professionals
Provide components, materials, skills etc.</td></tr>
</table>

submission, construction, municipal trade license, etc. In practice, stages overlap a great deal, and investors increasingly seek to install fast-track delivery processes in order to meet the need for shorter building delivery cycles (Sabbagh, 1989; Pietroforte, 1997).

3 A Plethora of Representational Artifacts

When one enters the field site, one is in an office consisting of several interconnected large rooms, each with several desks, each of these with a workstation.

As in virtually any modern work place, the office is replete with all sorts of representational artifacts. But what is perhaps particularly striking, is the absolute abundance of them. Most of the desks are covered with various artifacts: plans, sketches, notes, photographs, faxes, books, samples. On shelves are large collections of binders for each of the current projects; in the entrance area a collection of scale models is on display, and on the walls are 3D visualizations, sketches, photographs, and newspaper clippings from previous and current work. The walls close to the architects' workspaces too are used as an exhibition space and decorated with materials from current work.

Sketches play a large role in conceptualizing the design, drafting solutions, explaining an idea, or working out design changes. A series of scale models will be built for different purposes, each model visualizing different aspects of the design. Print-outs of plans will be made, photocopied in A3 format, discussed, and modified (on layers of transparent paper). Spreadsheet and word processing documents will be created for the host of information that is accrued in the process and needs to be

Fig. 10.2 The architects' workplaces and artifacts

organized, distributed, held in evidence. Copies of material will be sent by fax to a consultant for commenting and returned with suggestions and calculations, to be discussed and further annotated. Files will be exchanged via email. Consultants will extract the layers from a CAD plan that matter for their work, do their own drafting, and send the file back to the architects' office for approval and re-incorporation into the CAD plan. Meanwhile, the architects talk on the phone, call for an *ad hoc* meeting, send an email, etc. What is remarkable is not just the diversity of artifacts but the intricacy of the workflow: the ways in which actors align these different materials and perspectives into highly detailed and well coordinated set of plans.

3.1 Conceptual Visualizations

While some of the architects' design artifacts, such as CAD drawings, are precise and highly detailed, others, among them sketches, hand-written notes, images, and models, are informal, imaginative, and open. They may, however, assume a high degree of conceptual clarity and coordinative efficiency. Different architects have different preferences and styles in this regard. While some architects use sketches and pictorial material for generating and expressing their ideas, others prefer poetry and metaphorical text; whereas others build their designs on (historical) research, the assembling of facts or 'datascapes' (Maas and Van Rijs, 1998), and others again work with scale models from the start of a project, working out their ideas by experimenting with different spatial configurations.[3]

One particularly beautiful and rich example is the loose *ad hoc* assembly of sketches, metaphorical text, association images, physical models, and photographic material in Fig. 10.3. The collage represents some of the central features of the initial design concept for *Pleasure Dome*. The 3D visualization and the image of a wall in the cathedral of Chartres express the idea of a 'big wall, impregnated with color and light'. The sketch and the metaphorical text capture the evolving idea of the building's façade as a 'cutting edge' between the rough quality of the concrete on the one hand and the lucidity and colorfulness of the glass skin on the other hand: the movie theatres as 'stones that dip into water'.

A scale model was built at the very beginning of the project and was used within the office as well as in meetings with external specialists for visualizing the complex roof structure and the internal space of the building. Later, a much more elaborate version of this model was used in convincing the key user of the architects' color

[3]Frank O. Gehry's handmade models, for example, are digitized and then rationalized, to achieve repetition without sacrificing form. In the case of the Walt Disney Concert Hall in Los Angeles, the curved outer surface is covered by a 'skin' of Italian lime sandstone. The most economic way of cutting and producing the complex curvatures was calculated by computer, and a physical model was milled. The model was then compared to the original cardboard model and adjusted when necessary (LeCuyer, 1995).

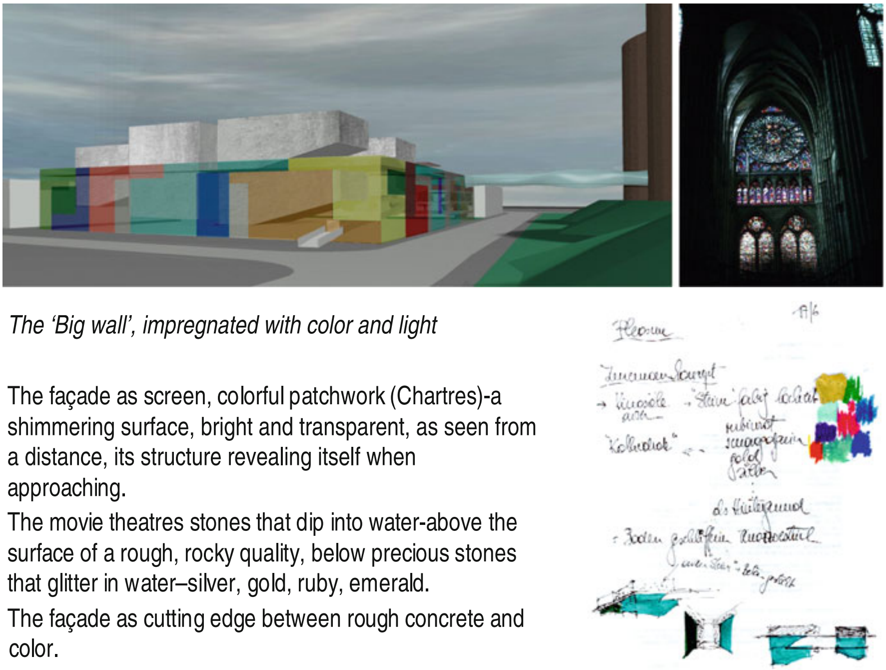

Fig. 10.3 A collage of a 3D visualization, an 'association image' (Chartres), a sketch, and a metaphorical text. The 3D visualization was generated from a first, rough scale model

concept for the movie theaters, that they be painted 'silver, gold, ruby, emerald' (Fig. 10.4). Altogether, more than 30 models of *Pleasure Dome* were built, made from different materials in different sizes, each visualizing different aspects and qualities of the building-in-design.

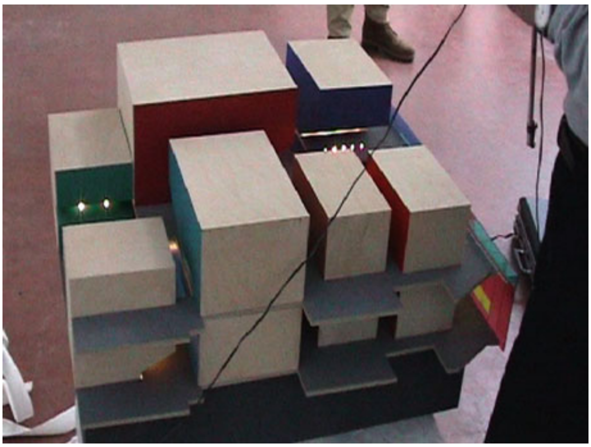

Fig. 10.4 A model visualizing the colored movie theatres

Characteristic of these artifacts is their conceptual and metaphorical character. To architects, their sketchy and informal representations capture the mixture of symbolic richness and abstraction which allows them to express qualities of space, light, atmosphere, and materials. Similarly, abstract 3D visualizations of spaces, places, and artifacts may be used for conveying a concept, metaphor, or some symbolism. In this context, 'abstract' does not mean the striving for purity, as in an abstract painting; on the contrary, visualizations like the 3D images produced for *Pleasure Dome* are highly theatrical; they use the language of 'artistic impurity, hybridity, and heterogeneity' (Mitchell, 1994) for communicating certain ideas and qualities of an object. Another feature of these informal representational artifacts is their openness to extensions, modifications, and novel interpretations.

A different example of an informal representational artifact is the sketch, in Fig. 10.5, of a work plan for an urban planning study. It reflects the particular style of the principal architect, who produced it as part of a first planning session while discussing the project.

The architects use this type of artifact in various ways. In this particular project, members of the project team placed copies of the sheet on their desks, using it as a reminder of design principles and the overall work to do. It served as a template for project meetings. In one of those meetings the sheet was annotated and enriched. The sketches are pointers to a series of more detailed drawings exemplifying 'rules'. Finally, the sheet also represents the structure of the deliverable—a project report with different types of visualization of the urban design. Although informal, incomplete, and not very detailed, this work plan served a powerful integrative function in the project, providing a vision for and cohesion within the designer team.

The sketch to the left contains, *inter alia*:

- A first work plan – things to do, phases, how to proceed.
- Specification of visual material that should be collected or created (pictures, collages, association images, shadow plans, etc.) - how to represent the design of the urban area.
- Metaphors – how to talk about the urban area.
- A specification of methods – to define spatial qualities, to 'intensify rules', etc.
- Explanatory sketches.
- References to material to look for.
- Names of responsible actors.

Fig. 10.5 Initial sketch for an urban planning project

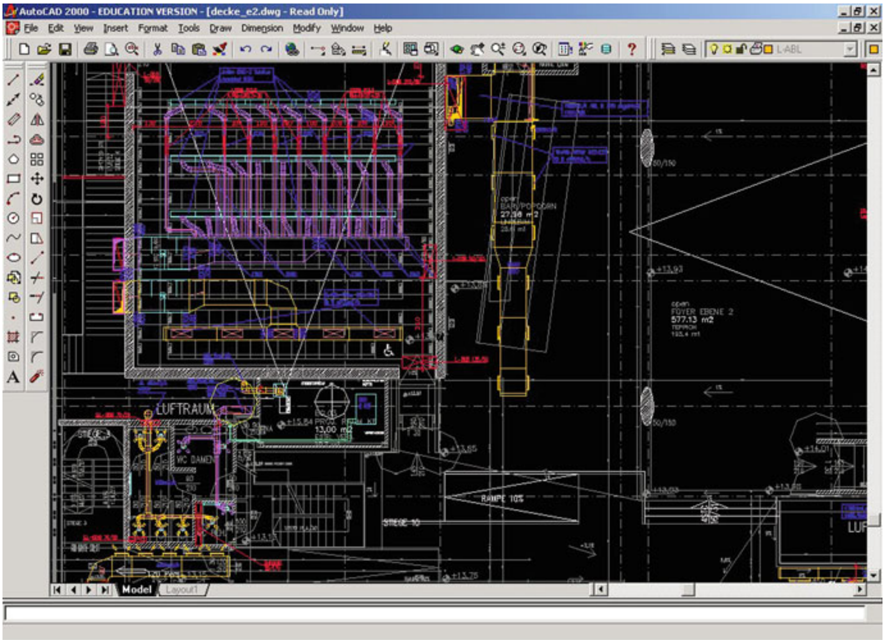

Fig. 10.6 A CAD plan with multiple layers collapsed into one representation

3.2 The System of CAD Plans and Drawings

In a modern architectural office, the central representational artifact is the system of CAD plans. They incorporate, as an ensemble, a project's trajectory from draft to implementation; they absorb and reflect all decisions taken and changes made, as plans are gradually detailed and modified.

Typically, the CAD model of a large building is divided into 15–20 sections. Altogether about 30 plans, including 11–12 floor plans, have to be drafted and coordinated. Each plan in turn is decomposed into a large number of layers, often more than 100, each devoted to the representation of one specific feature or set of features of the building (one for brickwork, one for concrete structures, one for windows, one for ventilation systems, etc.). All plans are stored on the central server, using a structured file system with different subdirectories for each project period and with predefined file-naming conventions.

Print-outs of the CAD plan in A3 format are used for discussing design details and working out design changes. This is either done directly on the plan copy or on a blank transparent sheet of tracing paper that is placed over the printed plan and 'anchored' by positional markers. The tracing paper is then used for experimenting with design ideas. Fig. 10.7, for example, shows one of the many representations that were produced when the architects were looking for another way of guiding visitors entering the *Pleasure Dome* through the interior space of the building. It

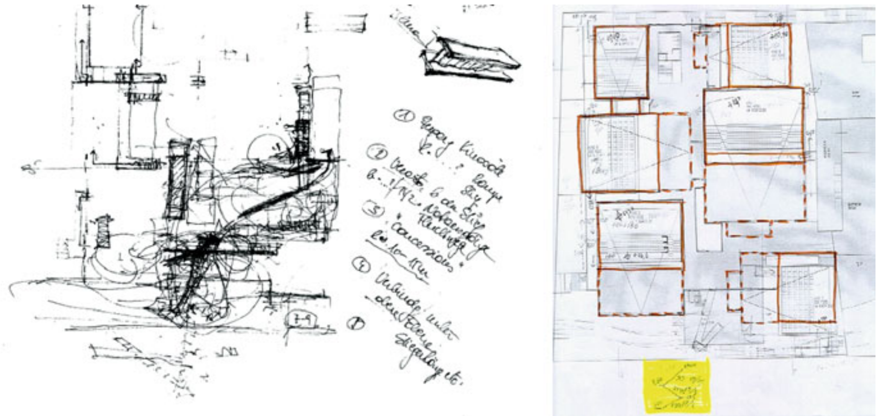

Fig. 10.7 Working out design changes on top of a CAD drawing (*right*). On the *bottom margin* of the amended drawing is a post-it with some annotations

was realized that, to achieve this, the interior path as well as the spatial distribution and positioning of the movie theaters and the ticket booths had to be changed. The principal architect produced a first sketch on top of the print-out of the CAD plan, including a list of to-dos at the margin. One of the project team members used transparent paper over the original plan to mark the new configuration in orange.

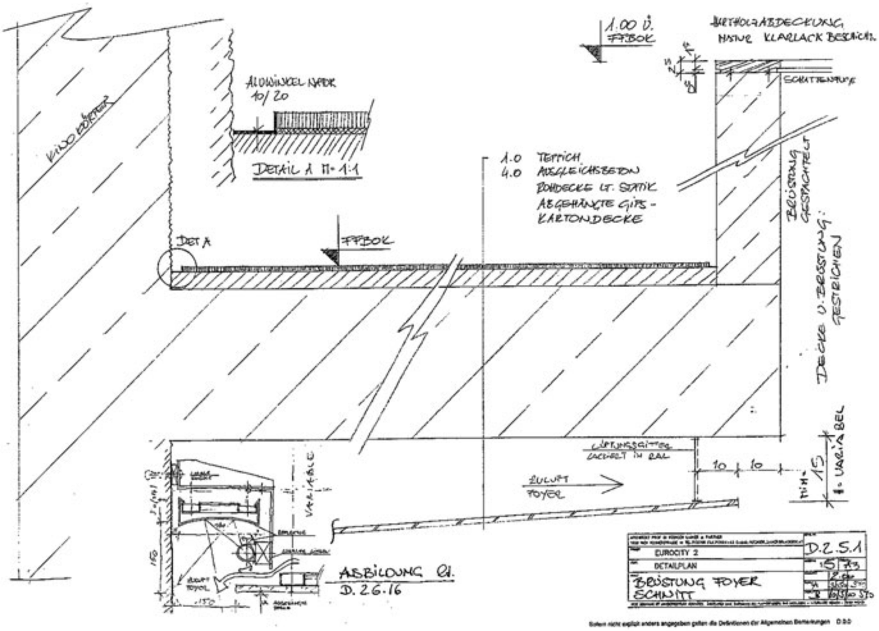

Fig. 10.8 A detail drawing

As planning progresses, more and more details have to be specified and filled in. A large building contains hundreds of details, which can either be left open, to be decided upon later by the construction company or craftspeople, or they can be carefully designed up front. Much of the aesthetic quality and individual character of a building depends on the design of these details (Fig. 10.8). However, detail drawings are of a scale of 1:5 or even 1:1 and therefore cannot be fitted into the central CAD construction drawing.

There are different types of detail. For example, a building such as *Pleasure Dome* involves a large number of recurring components such as inner and outer walls, ceilings, floors, roofs, stairs, balustrades, etc. For this particular building with its 200 different types of doors, also simple components such as doors had to be specified.

3.3 And so on. . .

As construction of the *Pleasure Dome* began, the architects' workspace became populated with photographs from the construction site. They visualized the progress of work on the one hand, acted as reminders of problems to be solved on the other hand, like the photo of a certain problem on the site which the building company forwarded to the architects via email (Fig. 10.9, *bottom*). Documenting the progress of work is a legal requirement, and the photos are archived and kept after the completion of the building in case a liability issue arises.

3.4 Representational Artifacts as Objectifications

In order to understand this plethora of representational artifacts, one should take into account that architectural work is different from many other types of work insofar as the 'field of work' does not exist, that is, does not exist *objectively*, in advance, but is constructed in and through the process of design and planning and, ultimately, construction. Architectural work proceeds through the architects' producing successive objectifications of the design and interacting with them in a variety of ways, inspecting them, comparing them, assessing them, etc. That is, the conspicuous display of representational artifacts can be seen as the fundamental means of making the not-yet-existing and in-the-process-of-becoming field of work immediately visible, at-hand, tangible.

A comparison with radically different work domains such as process control may help to clarify the point. As far as the operators of, say, a power plant are concerned, the plant as a whole, the different functional parts, the valves and pumps, the energy transformation processes, the mass flows, the power grid to which it is connected, etc. can be conceived of as their common field of work. It is there, in an important sense, before they start their shift, and it is still there when they clock out. However, due to the sheer scale of the plant as well as due to the intangibility of the processes, various representations of the plant and processes are generated and made available

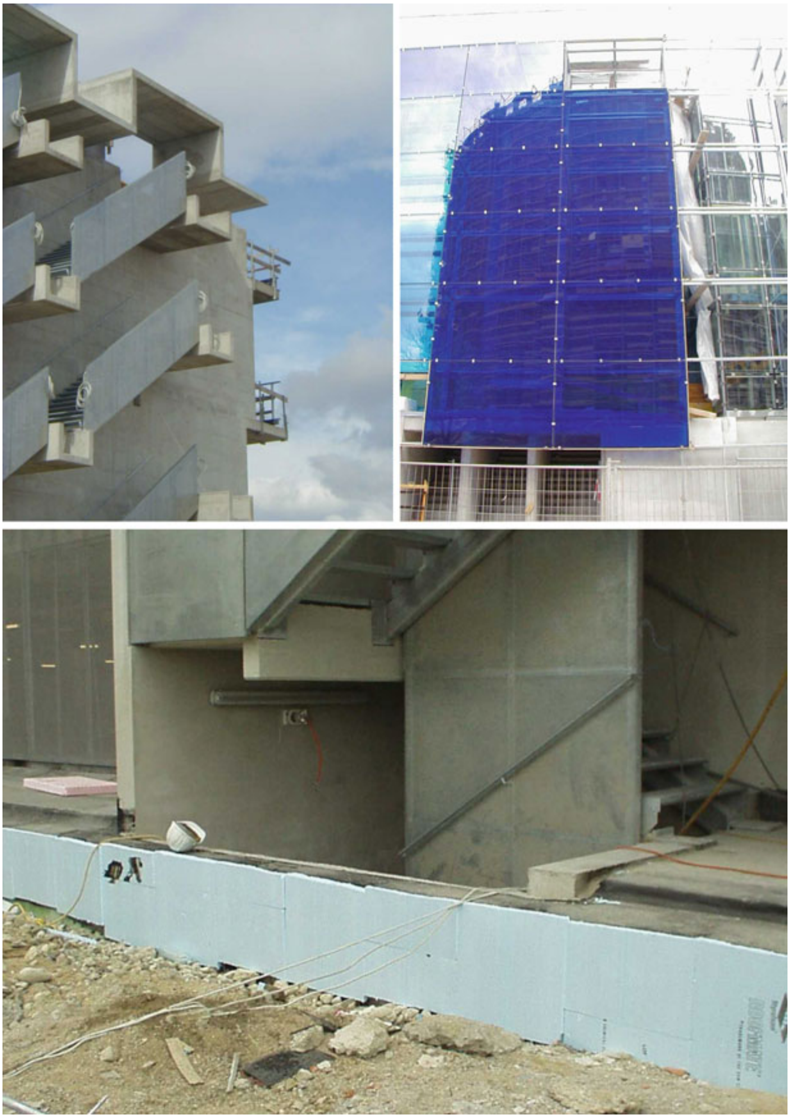

Fig. 10.9 Photographs from the construction site

in the control room, and the operators there take them to stand proxy for the plant and processes beyond the control room. For all practical purposes, the operators work with these representations.

By contrast, the field of work of architects is notional. The building does not exist prior to their work but only as a result of their work. More than that, the representational artifacts do not exist prior to their work either.

This is of course an exaggeration. Architects do reuse previous designs and have a vast array of preexisting resources at hand, such as catalogues of materials, parts, etc. readily available on the market. Anyway, the point we are trying to make is that the motley collection of representational artifacts plays a role quite similar to the nuclear power plant and the representations of it to the operators in control room. For architects, in the absence of an objective, material field of work, the representational artifacts constitute the field of work. They serve as objectifications of the construction-in-the-making and are, as such, the immediate object of their work, they are what is looked upon, inspected, gestured at, discussed, modified, annotated, etc.[4]

These similarities notwithstanding, the fact that the field of work of architects is notional and that they thus are required to work with representational artifacts as objectifications of things-to-come, has important implications. Representations are not the real thing, of course; they are fundamentally 'under-specified' with respect to that which is represented (Suchman, 1987); they are local and temporary constructs (Gerson and Star, 1986). More than that, representations are essentially conventional constructs based on rules of mapping and translation between representation and the object that is represented. Consequently, the infinity of affordances and cues offered by the pre-existing material field of work of plant operators is not available but has to be painstakingly emulated by a rich variety of sign systems, notations, and other conventional practices.

3.5 Notations: Standard and ad hoc

Architectural plans are extremely complex artifacts, based on highly sophisticated notations and drawing conventions that have developed over centuries and are systematically taught at educational institutions. They figure prominently in any introduction to architectural drawing and planning (for elementary English textbooks cf., e.g., Wakati and Linde, 1994; Reekie, 1995; Styles, 1995). To a large extent these notations and conventions are reflected as standard primitives in the CAD application, others are specific for the particular office or even for the particular project. For example, CAD drawings use a color code for line thickness. This code originates in the technology preceding CAD. Prior to CAD technology, line thickness had developed as a standard way of denoting different structural elements of a building (different line thicknesses representing outer wall, inner

[4] Marx famously stated that 'what distinguishes the worst architect from the best of bees is that he has built the cell in his head before he builds it in wax. At the end of the labor process, emerges a result that, at its commencement, already was present in the *worker's imagination*, that is, *ideally*' (Marx, 1867a, p. 129). Although this careless statement probably should not be taken too seriously as an empirical proposition, it is nonetheless worth noticing that it is simply wrong. Architects do not work like that and never have. Worse, this unfortunate aphorism has been taken as a definition of 'human nature' and thus paved the way for endless mentalist speculation. Marx should have kept to his previous position, namely, that 'human nature is no abstraction inherent in each single individual. In reality, it is the ensemble of the social relations' (Marx, 1845a, § 6).

walls, windows, etc.), and for these purposes architects used a repertoire of pens of different caliber (‘Rotring’ drawing pens). For obvious usability reasons, a standard color coding for line thickness had been established, with, e.g., a white ring on a pen denoting 0.25 mm line thickness, yellow 0.35 mm, green 0.50. Early CAD systems adopted this color code and used it to represent line thickness and, by implication, structural element. Later on, free choice of color became an option and one can now choose between 30 and 40 colors and the respective line thicknesses. In view of this and to maintain general internal agreement about the color code in the office, line thickness codes have been defined in a separate file, which is also normally forwarded together with CAD files to those external consultants that need to plot out and work directly on the file. While a general norm (ÖNORM) concerning the submission of CAD drawings is being developed in Austria, each architectural office still has its own in-house color coding standard, whereas other professions (e.g., geometers) use different conventions.

In addition to the standardized notations and drawing conventions, architects use a variety of more or less conventionalized techniques for highlighting items that need to be checked or changed. Among those are: making annotations on a document, e.g., putting a red circle around a problem, adding details (correct measures, materials), marking a part of a drawing with a post-it note with some instructions for changes, corrections (e.g., in pencil directly on a plan), sketching either directly on a plan copy or on transparent tracing paper.

At times, new notations are established on an *ad hoc* basis. Take for instance the plan in Fig. 10.10. It is an A3 copy of a CAD plan showing the upper level of the shopping mall in *Pleasure Dome*. This drawing was used in a meeting of the architect with the lighting designer. During the meeting, the lighting designer got to know the building by ‘walking through’ it, pointing to particular places and elements, while the architect was thinking aloud, describing the space, listening to questions and suggestions, simultaneously sketching the lighting concept. While talking, the architect developed a notation, using different colors for different types of lighting. This notation was then, subsequently, used in all documents concerning lighting. Colored photocopies of the lighting design were, for example, used as part of presentations to different audiences.

Architects’ ‘conceptual visualizations’ as well as their CAD drawings describe the building-in-design at multiple levels of detail, completeness, and ‘technicality’, using different notations, according to scale, materials, construction stage, etc. The plethora of artifacts compensates for the ‘abstractness’ of the various representational artifacts and of their limited scope.

To architects this ‘under-specification’ of representational artifacts is of particular relevance, as one can see from ‘conceptual visualizations’ such as sketches and ‘association images’. Connected with this is the ‘openness’ of such artifacts: ‘open’ in the sense that they facilitate and accommodate contributions from others; they serve to stimulate their imagination and to make them perceive the novel within the familiar, discover relations between seemingly incongruent objects and notions, relate the ‘unrelatable’—and to jointly take a step further in the design process (Wagner and Lainer, 2001).

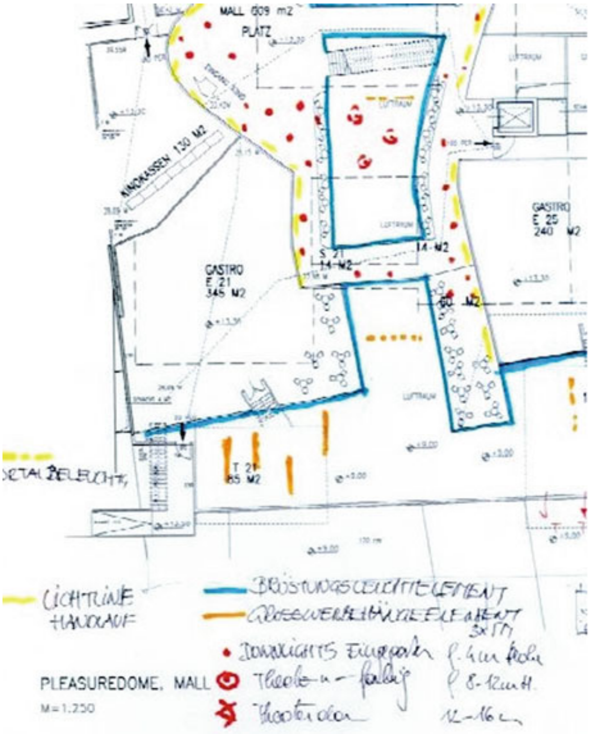

Fig. 10.10 *Ad hoc* notations

4 Coordinative Practices and Artifacts

> List making is foundational for coordinating activity distributed in time and space.
> (Bowker and Star, 1999, p. 138)

Embedded in this web of architectural artifacts and practices, there is obviously a flow. There are stages in the life of a project, even legally prescribed ones, a schedule defined by deadlines. But the progression is overall one of ongoing refinement and increased specificity. From time to time, of course, what has been done is undone, decisions are revised, progression reversed. That is, the overall process is an iterative one.

Moreover, the actors not only interact by changing the state of some part of the world (which is characteristic of cooperative work in general), they interact in a highly distributed manner by changing the state of discrete items in vast heterogeneous and physically distributed collections: sketches, models, plans, drawings, calculations, specifications, etc. What is most striking is that they manage to interact in this highly distributed and mediated manner without succumbing to disorder and utter chaos.

How do they do that? The simple answer is that this is accomplished by means of a large variety of mundane coordinative practices and concomitant coordinative artifacts that are not simple at all. In fact, they typically form complexes of interrelated coordinative practices and artifacts. In this section we will briefly discuss the most important ones.

4.1 Plan Identification

A range of 'generic' coordinative practices will be found wherever large collections are handled in a distributed manner. The 'title block' is a good example for illustrating these practices. It is a template 'stamped' onto each plan at the bottom (Fig. 10.11).

This simple artifact illustrates a feature which we find in all of the coordinative artifacts and also in the key representational artifacts (CAD plans, detail drawings,

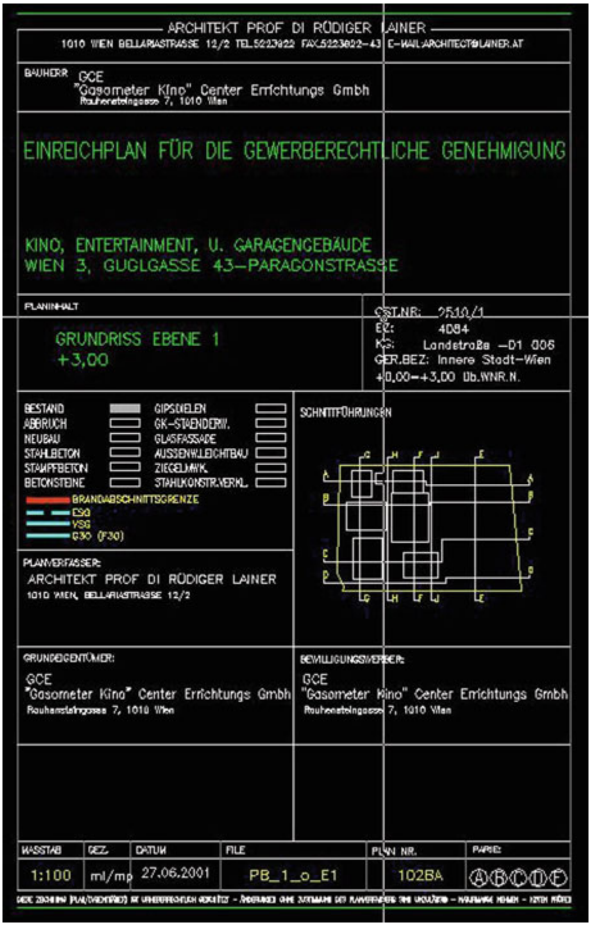

Fig. 10.11 The title block ('Plankopf')

etc.), namely that they have what in CSCW has been termed a *standardized format* (Harper et al., 1989b, pp. 15 f.). For others to be able to easily identify and make sense of the items in a collection, their form must be standardized according to certain criteria. If the artifact is a text, the document may be formatted according to a genre (e.g., the different standards for formatting scientific articles in the different disciplines, or the conventions for formatting agendas or minutes of meetings).

The title block is, graphically, a bounded space divided into fields of different sizes. Each field is dedicated to display a specific category of information. Thus, if a field is left blank, this is immediately obvious to whom it may concern.

The spatial arrangement does not reflect a systematical grammar. Read from top to bottom it rather loosely reflects a certain order of priority (in conjunction with font sizes, styles, and colors): the name and address of the office, the name and objective of the project name, the type of plan, the cross section represented in the plan, the name of the client, the authority concerned, and at the very bottom a set of identification codes. So far, the arrangement is quite similar to the title page and the colophon in a book; and like these, artifacts such as the title block serve what could be called *practices of identification and validation*.

Practices of identification: When accessing an artifact produced and submitted by somebody else, the actor who may need to retrieve it must be able to establish its type and particular identity. The item thus has to be named or otherwise identified so that a potential user will know 'what' and 'which' it is. This may involve a more or less elaborate convention for naming items, from the usual way of doing it to a nomenclature, to a 'system of designation' (Harris, 1986, p. 130). In our case, the architects' practices of naming plans are based on a systematic system of designation, namely the 'plan identification code' found at the bottom, which will be discussed in more detail in the next section. Here, in the title block, the 'plan identification code' has two parts, the file name (PB_1_o_E1) and the 'plan number' (102BA).

Practices of validation: When accessing an artifact produced by somebody else, the actor retrieving it will also need to somehow assess its relevance, validity, veracity, etc. Actors will here, among many other factors, rely on their knowledge of the evaluation procedures, that is, the approbation procedures according to which items have been assessed and approved (e.g., the editorial review procedures of scientific journals, the acquisition procedures of museums, etc.). Also, in some domains of work, more or less sophisticated procedures of validation are developed and enforced (e.g., journalism, accounting, criminal investigation, experimental science, historiography). In the case at hand, the title block reflects a variety of validation procedures. It contains a date as well as information about author (e.g., the architect), the client, the owner of the plot, the object (name, address), the authority to which the plan is to be submitted, and the legal authority to be addressed in case of a complaint. The title block also specifies the type of plan at hand, in this case a 'application for trade license, floor-plan, level 1'.

In addition, however, in the middle section (in the left hand side of the header), the title block also defines a notation for structural elements (the set of color codes used in the plan to designate parts made of concrete, steel, brick, etc.). The line drawing (to the right) introduces a specific alphabetic code for the cross sections of the building, which are both visualized and named (using the letters of the alphabet from A to I as codes).

4.2 The Plan Identification Code and Circulation List

The plan identification code reflects the cooperative and highly distributed nature of the planning process. In a large building project, different actors work on planning different parts of the building and on different problems. There are different plans for different stages of a project, for different parts and levels of the building, and for different purposes. Each of the about 30 plans that have to be drafted and coordinated has a particular trajectory. The first version of floor-plans and cross sections is usually produced at an early stage ('pre-design'). Then, over time, the plans become more and more detailed and also specialized, with different specialists contributing their expertise. This is a procedure in many loops, involving numerous design changes. Many versions of each plan are produced; in a large project such as *Pleasure Dome* altogether 580 plan versions were plotted out and distributed, 20–30 versions of each plan on average.

To ensure some kind of order in all this, each plan is identified by a unique alpha-numerical code (e.g., 'PW-1-M-E1-103-V1'). Parsed from left to right, the code identifies the plan in terms of a set of attribute types:

Fragment 1. Plan identification attribute types.

```
project_acronym /
stage_of_the building_project {pre-design, submission,
     construction, etc.} /
author {architect, construction engineer, building engineer,
    etc.} /
section_of_building {garage, mall, cinema, bridge} /
level_of_building /
type_of_plan {ground floor, cross section, front, escape
    routes, etc.}, <number> /
version <number>.
```

The identification code 'PW-1-M-E1-103-V1' thus reads: *Pleasure Dome/ design/architect/mall/level 1/ground plan no. 3/first pre-plot*.

On closer inspection, however, the technicalities of this identification scheme are rather intricate. Since we will meet many examples of the same kind of identification scheme, an analysis of the logic of the scheme is in place.

The identification scheme belongs to a category of nomenclature that in theories of classification is sometimes termed 'polynomial nomenclature', i.e., a system of names consisting of multiple terms (cf., e.g., Mayr and Ashlock, 1991). For each position in the code (apart from version number, of course), valid values are selected from a pre-defined and finite list, as indicated in Fragment 1. The values for `author`, for example, are restricted to a list of options (`architect`, `construction engineer`, `building engineer`, etc.). Each of these 'supporting' lists can be considered a notation, 'a set of graphic units with its own structure' (Harris, 2000, p. 114).[5] That is, the system of designation underlying the plan identification scheme can be seen as a sequentially ordered set of attributes, for each of which is attached a specific notation in the form of a list. Graphically, the whole construct can be depicted as the shaft-and-barb structure of a feather.

Now, polynomial nomenclatures are notoriously cumbersome.[6] To avoid the unwieldy names that would result from using words or phrases for each attribute, the architects code attributes by single digit signs: for `author`, a set of numerals (1, 2, 3, ...), for `section`..., simple abbreviations ('g' for garage, 'm' for mall, etc.).

For the same purpose of parsimony, probably, the particular nature of the relationship between the terms for which the hyphen stands proxy is not expressed at all. But in fact, the hyphens implicitly denote *different kinds* of relationship, determined by the categories to which the preceding and subsequent attributes belong. If we replace the hyphens with explicit relationship markers, the code 'PW-1-M-E1-103-V1' would read: *[this plan pertains to project:] Pleasure Dome; [concerns project stage:] design; [produced by:] architect; [concerns building section:] the mall; [concerns level of building]: level 1; [for which this plan is a] ground plan no. 3; [of which this is the:] first pre-plot.*

That is, the plan identification scheme does not designate plans uniformly in terms of a consistent principle of ordering. This heterogeneity notwithstanding, there is a practical logic to this identification scheme. It identifies plans, not just by a set of arbitrary signs, but in terms of attributes that are taken to be *significant*. Hence, this system of designation and identification is also an expression of a classification scheme of sorts. It is a classification scheme in that related plans are grouped together: All plans for the *Pleasure Dome* project are named 'P...'; all plans for the mall are named 'P...-...-M...' It is not a strict hierarchy, however, in so far as plans are not classified according to a tree structure (a 'dendrogram'). The point is that a specific attribute (e.g., `Section`...) may be subordinate to more than one superordinate attribute (e.g., `Author`). That is, 'P...-1-M...' and 'P...-2-M...' are both

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[5] 'Notations exemplify a type of structure which, far from being confined to writing, is one of the most basic structures in the domain of signs. It is the structure characteristic of any set of items fulfilling the following conditions. (1) Each member of the set has a specific form which sets it apart from all others in the set. (2) Between any two members there is either a relation of equivalence or a relation of priority. Thus every member has a determinate position with respect to all other members in the set. (3) Membership of the set is closed. Such a structure constitutes what in integrationist terms is an emblematic frame. The very simplest emblematic frames comprise just two members: examples are an on-off switch with only two possible positions and a red-green traffic light where the two colours alternate but never show simultaneously.' (Harris, 2000, p. 106).

[6] This is the reason why Linnaeus (1735–1777) proposed a binomial nomenclature (based on genus and species) to replace the then current polynomial nomenclature. Thus, what had previously been named *Mentha floribus capitatus foliis lanceolatis serratis subpetiolatis* (which reads: 'mint plant/flowers arranged in a head/lance-shaped serrated leaves with short petioles') would henceforth be simply named *Mentha piperita*.

valid codes. The 'syntax' of the plan identification code rather reflects an 'unprincipled hierarchy', beginning with the most 'weighty' attributes, the code for project and stage, shifting to the features of the building, and eventually back to the process, in a sort of progressive exclusion strategy. As a result, in spite of its lack of rigor as a graph, the classification scheme not only provides a procedure for producing a unique name for each plan; it also tells the architects what kind of plan this is, where in the process it belongs, what it is about, and so on.

The apparent lack of rigor is accidental, however. The logic underlying the architects' system of designation is similar to that of the chess notation, where the 'algebraic' system assigns a letter of the alphabet to each file and a number to each rank on the chess board, and as a result, each square on the board is identified by means of a letter-numeral combination (*f*3, *b*4, etc.). In the words of Roy Harris, the 'algebraic system superimposes a structure of its own upon the distinctions required for an adequate notation' (Harris, 1986, p. 130). In our case, the classification scheme is the 'structurally superimposed' system.

There is a strong rationale underneath the combined identification and classification scheme. To make the point of this clear, consider alternative forms of systems of designation for the purpose of providing unique names for the myriad plans and drawings. One could, for instance, by giving each plan and detail drawing an arbitrary name (Peter, Paul, and Mary, say), arrive at an equally unequivocal system of designation. After all, streets, buildings, lecture rooms, hotel suites, etc. are often named that way. But such a system of designation implies housekeeping activities that may become forbidding when the number of items becomes very high. On one hand, one would need to maintain an inventory of names already assigned or an inventory of available names (i.e., a nomenclature), so as to prevent duplication and confusion. On the other hand, to be able to locate any particular object, one would also need a system of matching the names and the particular items (indexed maps, floor plans, etc.). With a systematic system of designation, these housekeeping activities become superfluous. In a typical system of systematic designation of rooms in a large building complex, a room identified as '3.2.36' would, for instance, be room 36 on the 2nd floor in building 3. If one knows the system and has been issued with the room code, finding room 3.2.36 is straightforward. Such systems of designation also allow actors to detect inconsistencies or omissions in the application of the scheme (such as the omission of floor 13).

As a system of designation the plan identification code is open-ended. While the values for each position in the code are selected from a pre-defined list, a notation, one can add another valid value to the notation, if that is deemed appropriate.

The architects do not strive for timeless consistency. To the contrary. The coding scheme varies from project to project. Thus, at the beginning of each project the architects discuss the plan designation system and adapt it to the exigencies of the project at hand. Such modifications may even occur during a project. Very pragmatic considerations are at play here, such as the complexity of the building and of the division of labor. For example, as the division of labor in the *Pleasure Dome* project became established and it became obvious that actors had become permanently assigned to different sections of the building, the plans for the building were

A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R
Bezeichnung	Filename	Plannummer		Index	KONSULENTEN												DEVELOPER
					STATIK		HT		ELEKTRO		BAUPH.		VERKEHR		BRANDSC.		KINOBETR.
					File	Plan	File	Plan	File	Plan	File	Plan					
Lageplan	PVD_1_k_lp																
Ebene Untergescho	PVD_1_k_eu	100															
Ebene 0	PVD_1_k_e0	101															
Ebene 1	PVD_1_k_e1	102															
Ebene 2	PVD_1_k_e2	103															
Ebene 3	PVD_1_k_e3	104															
Ebene 4	PVD_1_k_e4	105															
Ebene 5	PVD_1_k_e5	106															
Ebene Draufsicht	PVD_1_k_ed	107															
Schnitt A	PVD_1_k_sa	300															
Schnitt B	PVD_1_k_sb	301															
Schnitt C	PVD_1_k_sc	302															
Schnitt D	PVD_1_k_sd	303															
Schnitt E	PVD_1_k_se	304															
Schnitt F	PVD_1_k_sf	305															
Schnitt G	PVD_1_k_sg	306															
Schnitt H	PVD_1_k_sh	307															
Schnitt I	PVD_1_k_si	308															
Ansicht Nord	PVD_1_k_an	400															
Ansicht Ost	PVD_1_k_ao	401															
Ansicht Süd	PVD_1_k_as	402															
Ansicht West	PVD_1_k_aw	403															

Fig. 10.12 The plan circulation list (fragment)

split into two sub-collections (cinema and mall/garage, respectively), and to handle that the attribute type `section...` was introduced and used as an additional criterion for identifying plans.

The same pragmatism is exhibited in the application of the coding scheme for other purposes than simply giving unique names to plans. As mentioned, layers of the CAD plans are extracted and sent to external stakeholders for their contribution or comment. To keep track of plans as they traverse the network of consultants, local authorities, and of course the client, and thus ensure accountability, another coordinative artifact, the 'plan circulation list', is used (Fig. 10.12). The graphical format is that of a matrix (using a spreadsheet as a template). The 'plan circulation list' itemizes vertically all plans with their identification code, and horizontally it records data about who received and returned which plan at which date. For practical reasons, the syntax has been modified to: `type_of_plan` / `file_name`/ `plan_number`/, and the file name is here composed of: `project_acronym`/ `stage...`/ `level...`/.

The modified syntax reflects the pragmatic fact that the circulation of plans is anchored in the division of labor in terms of sections of the building. The different sections and hence groupings within the team are simply and pragmatically taken to be represented by the type of plan (the particular cross section, the particular façade, etc.) which is therefore the deciding attribute type.

4.3 The CAD Layer Organization

In a large building project, different actors work on planning different parts of the building and on different kinds of problem. As mentioned earlier, the central CAD plan is the representational nexus of a project. The CAD plan is the artifact in which all the design decisions that have been worked out in various forms—sketches, calculations, technical descriptions, product specifications, etc.—are recorded and specified. CAD plans assume this central coordinating role in the process of planning, because modern CAD applications support the cumulative representation of the design within one and the same 'document', i.e., an integrated file structure, organized in layers, each of which addresses a particular aspect of the design.

The division of labor within the office is facilitated by the subdivision of the system of CAD plans into partial plans (section, floor) and layers. Someone responsible for specific tasks such as, say, ‘fire escapes’ may work on building parts or layers simultaneously used by others. That is, the subdivision and layered organization of the CAD plans enable the actors, for long or short periods, to proceed concurrently, with only occasional communication, while still acting concertedly.

The layer organization is furthermore crucial for maintaining the fit between the contributions of the different external specialists to the detailed planning of a building. All information pertaining to a particular task and purpose, such as fire protection, escape routes, ventilation system or structural elements, can be extracted from the file by copying the relevant set of layers. The construction engineer or the heating and ventilation specialists will for instance receive a copy of the relevant layers from the central CAD plan, work on them, and return them to the architects for re-integration into the system. Other external specialists may receive a print-out and produce their own drawings, which the architects will inspect, possibly discussing modifications and alternatives. They then may copy these drawings into their CAD plans or draft their version of the specialist’s suggestion. Again others will receive a photocopy of one of the plans and return it with comments, calculations, sketches, etc. It is the architects who monitor and control this process of viewing, detailing, and adding to.

The layer organization finally provides a (collective or individual) space for experimentation and change. An architect who is experimenting with how to carry a shaft through an open space may not only produce a series of sketches (some of them on tracing paper) but may define a special layer for the drawing (e.g., ‘Mike’s layer’). This example also shows that layers may be defined in terms of ownership and/or professional competence (e.g., the construction engineer’s).

The CAD plan is an integrated representation of the design, as opposed to the collection of separate hand-drawn plans that had to be produced before the advent of this technology. That is, the central CAD plan is a fine example of a boundary object (Star and Griesemer, 1989; Star, 1989) in as much as it is an integrated system of representations that provides an infrastructure which enables distributed actors to make their individual contributions to the overall design in a distributed, incremental, and yet concerted manner.

Essential for coordinating the successive detailing of plans is a uniform layer organization. However, layer structures vary widely, between the involved professions as well as from office to office (Laiserin, 2002). They also reflect the specific features of a building and they are organized in groups. Each of the 30 plans for *Pleasure Dome* contains up to 160 layers.

The layer organization primarily expresses the temporal-spatial logic of the construction process. Most of the layers describe spatial relationships within a building, which can be mapped, assembled and dissembled. The first versions of a CAD plan will contain the layers representing basic structural elements. For each subsequent step of detailing, new layers will be added, such as layers for the heating and ventilation system, the escape routes, or the exact positioning of stairs and elevators. This means that the CAD plan’s layer organization not only supports the cooperative nature of work, with different professions ‘owning’ particular sets of layers. It also takes care of the incremental nature of the design process. Finally, in addition to spatial elements and their relationships, the layer organization incorporates ‘housekeeping’ aspects, with specific layers reserved for comments, such as text (in different scales), measurements, ordering elements, special signs,—all in accordance with the architects’ drafting conventions.

Fragment 2. Notation for generating CAD layer codes.

```
0-1 Auxiliary layers
2   Planes/solids/hatches of different materials (e.g.,
    concrete, plaster, brick)
3   Special signs (e.g., for fire sections, escape lines,
    lines of the plot)
A   Completion (e.g., windows, intermediate walls, stairs,
    sanitary equipment)
B   Existing structures
I   Infrastructure (electrical; sanitary, etc.)
O   Ordering elements (axes, cross sections)
R   Rough (brick) work
SC  Cross sections/views
T   Text ‘labels’
U   Environment
V   Various
```

The naming of layers builds on a pragmatic mix of codes (see Fragment 2). A layer name consists of four parts: the layer group, a text describing the content, the building element, and the scale. ‘At-f100’ means: *Ausbau <Text> Fenster im Maßstab 1:100* (that is, *completion stage <text> windows in scale 1:100*). Similarly ‘R FLUCHT 200’ means: *Rohbau, Flucht im Maßstab 1:200* (that is, *rough brick work, escape routes in scale 1:200*).

Developing a standard notation for layers and their sequencing is a recurrent concern within the office. It is an issue which is taken up at the beginning of each new project, with actors looking through and discussing practices and experiences from previous work. These discussions are consolidated in two documents defining the layer organization—a Word document and an AutoCAD prototype drawing—which are accessible to the team members and also forwarded to those specialists who need to work directly on the architects’ files. However, the development and maintenance of the convention is not supported by systems such as AutoCAD. This is a problem, since maintaining the disciplined use of the notation—with up to 20 actors working on the CAD plans in parallel and a number of external specialists involved as well—turns out to be extremely difficult.

Not surprisingly, in the course of a project the layer lists tend to grow in non-systematic ways. Several reasons can be identified:

- Parts drafted by an external professional, e.g., the heating specialist, are copied into the current plan, although based on a different layer structure (which then is added to the one in use in the office).

- Details that can be imported from another project (e.g., a glass railing) or have been drafted separately because of their complexity (e.g., the bridge between *Pleasure Dome* and the *Gasometers*) are also copied and inserted (or cross referenced).
- There may be good reasons to preserve the history of some detail (e.g., the new seat arrangement for the movie theatres has been drafted on the appropriate layer while the previous arrangement is preserved, in another color).

4.4 The Component Catalogue

As already noted, a building such as *Pleasure Dome* involves a large number of recurring components such as, for instance, 200 different types of door, that have to be specified and indexed. The standard components are defined in the component catalogue. It provides the data for mass and cost calculations and needs to be prepared before the call for tender is due.

The component catalogue serves purposes of economy in the planning and construction process. Components used in many places in a building are only specified once and can then be referred to by their identification codes within the CAD plans. Also, components are typically re-used in other projects, possibly in a modified version.

The component catalogue for *Pleasure Dome*, which contains hundreds of items, went through several cycles of discussion and annotations. The architect in charge prepared a first listing of all components, which was first reviewed by the principal architect, who scribbled a number of remarks on the list, including small sketches, and then by the project manager, who also added handwritten comments. Later, the building engineer and construction engineer made corrections, added comments, and cross-checked. The architects worked on each item, adding to them and modifying them according to their notes.

The architects use yet another notation and syntax for naming components. In Fig. 10.13, the nested list (on the right hand side) contains the construction specifications for three components, all of them floor elements for the cinema. The top one reads: 'K-FB 08/Projektionsraum gegen Foyer (F90 Stahlkonstruktion)/Aufbau 12 cm/' (that is, ''K-FB 08/projection room towards foyer (F90 steel construction)/12 cm construction work').

The scheme for naming the component uses a quite simple notation. In this instance, 'K' stands for 'Kino' ('cinema'), 'FB' for 'Fussbodenaufbauten' ('floor components'), and '08' is a consecutive (and thus arbitrary) number, followed by a textual description and a measurement. As it happens, 'K-FB 08' is a component for which a detail drawing has been produced, and the identification code of the detail drawing—'K-1.10.1'—is given as a reference in the lower, right-hand corner of the segment of the fragment in Fig. 10.13. Important remarks are highlighted in red color, such as 'Neu' ('new').

The detailed specification of each component in the catalogue is process-oriented and anticipates the division of labor in the construction process. The different parts of the component are listed (from bottom up) in the order in which they will be

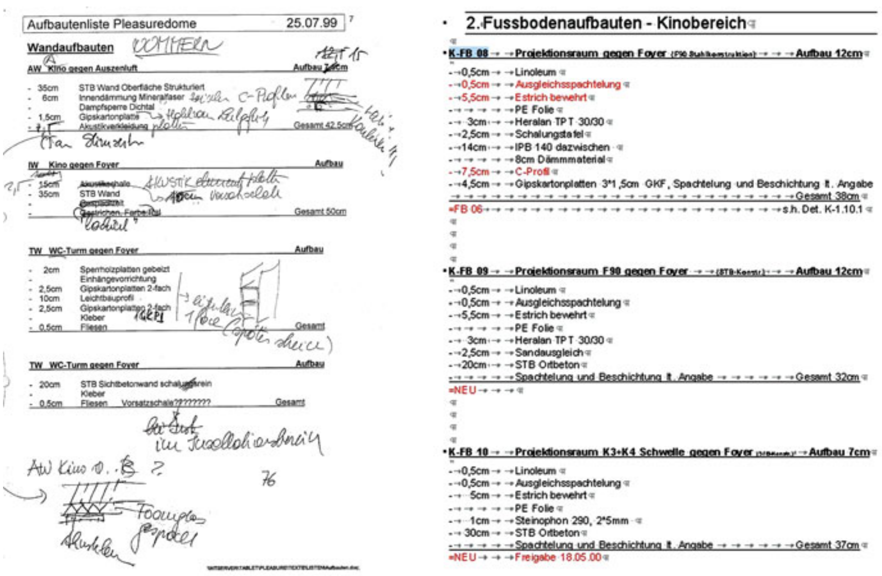

Fig. 10.13 The list of components: A page of the first draft (*left*) and a fragment of the final document

assembled, each preceded by a measurement, e.g., ‘0.5 cm Linoleum’. Some parts are standard elements, others designed for the current project; some will be prefabricated, others will be constructed on the site. This is the reason why some parts are also briefly described: for some detail drawings have been produced; for others instructions are added, such as *2 cm in Wand eingefräst* (*milled 2 cm into wall*), and for other parts again the architects use product names (e.g., *Heralan TP T 30/30*), while sometimes adding references to standards, such as ‘B1’, ‘Q1, or ‘TR1’.

Although the notation and the syntax are homegrown, the architects make use of reference catalogues, including standard tender specifications of components, many of which are also available on the Internet. As the architects adapt many of these specifications to the requirements of their design, they use them as ‘templates’ for constructing project-specific catalogues, sometimes without modifications, sometimes in a modified form.

4.5 The Detail Drawings: The Identification Code and List

As we saw, the component catalogue may have references to design details. A large building has hundreds of such details.

The detail drawings (Fig. 10.8), are inventoried in the ‘list of detail drawings’, an Excel sheet (Fig. 10.14). Similar to the plan circulation list, it contains completion and modification dates and helps maintain an overview of the circulation of drawings within the network of actors involved in planning and construction. Architects

DET.NR. B=BRÜCKE K=KINO M=MALL G=GARAGE					DETAILNAME	GEZ.	DATUM	INDEX	AUSGEGEBEN AN / AUSZUGEBEN AN VORSTATIK	FCP Details Ausschreibung	STATIK Schedler	BAUPHYSIK Gollner	ZFG / HLKS Milosovsky	ILBAU Feuchter	ÖBA	Schlosser	Spengler
	7	0			**Geländer/Brüstungen**												
	7	1			*spezil. Geländer*												
K	7	1	5		Brüstung Kino Rolltreppenplattform +8,80	AS	12/16/2000										
	7	2			*Brüstungen*												
M	7	2	1	B	Brüstung Mall innen E 4, variabel	Liz	7/20/1999	B(23.6.00)	9/2/1999	####		####		######	####		
M	7	2	2	A	Brüstung Mall innen E 5, variabel	Liz	8/25/1999	A(01.10.99)	9/2/1999	####		####		######	####		
M	7	2	3		Brüstung Mall Terrasse E 4 - siehe Detail M 3.1.1	AS	11/15/2000							######	####		
M	7	2	4		Brüstung Mall Terrasse Nord E 3/ E 4 Stahlrahmen, Glas	Sei	3/24/2000							######	####		
M	7	2	5		Brüstung Mall Eckausbildungen (überholt siehe Freigabe Tamussino!)	AS	10/18/2000	12/16/2000									
M	7	2	6		Brüstung Mall, Begleitstiege 3/ Rolltreppe Bereich/4 18-19 E4 + 13.00	AS	11/15/2000										
M	7	2	7		Brüstung Mall Anschluß STB Wand Achse 19/c-d E4	AS	11/15/2000										
	7	3			*Brüstungen und Geländer Glas / Kaltluftauslässe*												
K	7	3	1	A	Glasbrüstung Luftauslässe/ Brüstungselemente Foyer +13.00	Sec.	8/24/1999	A(13.03.00)	9/2/1999	####		####		######	####		
M	7	3	2		Glasgeländer Mall Terrasse - siehe Detail M 3.1.2	Liz	8/31/1999		9/2/1999	####		####		######	####		
	7	4															
K	7	4	;4;		Handlauf/ Geländer/ Fluchtstiegenhäuser												
	8	0			**Innentüren**												
K	8	0	1		Beschläge	AS	8/4/2000							8/3/2000			
	8	1			*Holz*												
K	8	1	1		Innentüren Holz WC-Türen	Sec.	9/1/1999		9/2/1999	####		####		######	####		
K	8	1	2		Innentüren Holz Lagerbox Ebene +3,00	Sec.	9/1/1999		9/2/1999	####		####		######	####		
K	8	1	3		Innentüren Holz Personal WC Westseite	Sec.	9/1/1999		9/2/1999	####		####		######	####		
K	8	1	4		Innentüren Holz Büro + Nebenräume	Sec.	9/1/1999		9/2/1999	####	####	####	####	######	####		
K	8	1	5	B	Innentüren Holz Büro + Nebenräume	Sec.	9/1/1999	B(20.9.00)	9/2/1999	####	####	####	####	######	####		

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Tabelle1 Tabelle2 Tabelle3

Fig. 10.14 The list of detail drawings

can see from the list which detail drawings have been sent to whom, for comments or for approval.

As with plans and components, detail drawings are given unique names, in this case based on a three-digit numerical notation. The identification code for detail drawings is loosely organized by the 'function' of the object, starting with floors and ceilings (for which the first digit is given the code '1'), walls ('2'), roofs and terraces ('3'), interior stairs and ramps ('4'), etc., altogether 32 categories. In the identification code '8 11', for example, the value of the first digit ('8') denotes 'interior doors' while the value of the second digit ('1') denotes parts made of 'wood'. However, the last digit ('1') is an arbitrary number in the sense that its meaning is not determined by any superimposed structure. That is, the last digit expresses local variations of different kinds. In this case, the value of the third digit ('1') means 'restrooms'. In short then, '8 11' reads '*interior doors/wood/restrooms*'.

However, the second digit may also be arbitrary. While the second digit, in some cases (such as doors), denotes materials ('1' for wood, '2' for steel, '3' for glass, etc.), other types of distinction may be made in other cases. An example is 'roofs and terraces', were '3 1' means terraces, '3 2' green roofs, '3 3' cinema roofs, and '3 4' roofs of projection cabins.

That is, the system of designation for detail drawings is not overall organized by a superimposed classification scheme. The result is more a notation than a classification. Its main function is to provide some kind of order in the repository of 300 detail drawings and a code for making references to the drawings within the

CAD plans. Asked about this, the project manager pointed to the 'impossibility of classifying everything'. He described how the classification of details broke down, as more and more distinctions had to be accounted for. For some particular details solutions were worked out as late as in the implementation phase on the construction site together with a craftsman. These details were not 'classified' at all but simply documented by the sketch that was produced during that conversation. This creates no problem, the project manager argued, as long as it is possible to maintain an overview. After all, the detail identification code does not allow one to identify the particular nature, the significant attributes, of the detail, anyway.

The required overview is maintained by collating all detail drawings in a special binder in A3 format. It contains the list of detail drawings as well as all detail drawings arranged by the three-digit identification code. The list and the drawings are used together, with the list providing an index to the drawings. The binder also contains the component catalogue.

The binder is located centrally on a table by the window in a room shared by the architects working on the construction plans. Whenever someone needs information about details, he or she walks over to the table, looks up a particular 'class', e.g., 'interior doors', and browses through the items in the list and/or through the detail drawings, takes the relevant documents out for photocopying, and places them back in the binder.

The ways in which the architects compose the detail identification code is influenced by a standard classification of 'deliverables' that has been developed for the building trade and which provides the agencies who produce or deliver building elements with a coherent notation for naming those elements. The architects use the same syntax but change the notation according the local exigencies of the particular project. The notation the architects developed for the *Pleasure Dome* project is based on one they used in a previous project. However, it turned out that they needed more categories, such as, for example, for different types of door made of different materials and for balustrades that include glass. Also, since the building was decomposed into sections—cinema, mall, garage, bridge—they wanted to preserve this as a significant attribute. This again reflects the internal division of labor according to which different actors are responsible for different details. Thus, a detail that has been developed as part of the planning of the cinema section gets a 'K', even though it may also be used in other sections of the building. This is a way of ensuring internal accountability for specific tasks. The code for the section of the building appears as a prefix to the identification code. In the list of detail drawings, one will find that the code '8 11' is actually 'K 8 11'. The prefix does not affect the order of the list, however. It is merely an annotation that is given this protruding position because of its importance in the division of labor.

As already mentioned, the list of details provides a code for referencing details in the CAD plans. Apart from the fact that details are drawn in scale 1:5 or even 1:1 and therefore cannot be included in the CAD drawings (which are drawn in scale 1:100 or 1:50), referencing to details, instead of integrating them, greatly simplifies the CAD plan. Furthermore, changes at the detail level, which are often made rather late in the process, do not have to be taken care of in the CAD plan itself. That is, making references to details, components, and product specifications increases the

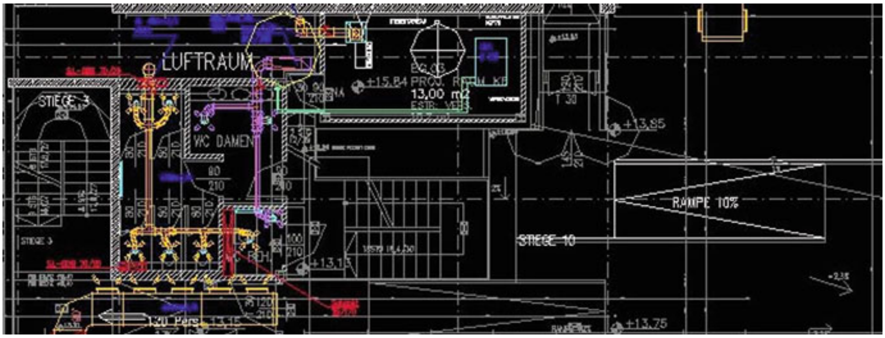

Fig. 10.15 References to detail drawings (in circles) in a CAD plan

robustness of the CAD drawing. To indicate references in the CAD plan, the architects use simple symbols such as ‘circle’ for details, ‘rectangle’ for components, and ‘square’ for product specifications, or they invent symbols on the fly, for instance for different types of lighting (Fig. 10.15).

4.6 The Binder System

At the beginning of each project a project archive organized in binders is constructed. This ‘binder system’ is a heterogeneous collection of documents ranging from plans, drawings, sketches, correspondence (faxes, letters), to minutes, contractual information, legal documents, and product specifications, often arranged in chronological order within each binder.

Binders for different projects have different colors (‘blue’ for the more than 150 binders for *Pleasure Dome*). Standard labels are printed out and adapted to the nature of the project. In the course of a project additional labels will be generated, for example to take care of unforeseen types of archival material, or when the need to reorganize some of the material arises.

The subdivisions of the filing system reflects the architects’ interactions with different professions—consultants, client, local authorities, and companies—and the domains these represent.

The binder label refers to: `role type {internal (coded: 1); client (2); authority (3); consultant (4); vendor (5)}/ area of expertise/`. Thus, for example, the inscription ‘4.1’ on a binder label reads ‘consultant/building engineer’, whereas the inscription ‘5.6’ reads ‘vendor/escalators’.

While plan versions that move across organizational boundaries, say to the heating and ventilation specialist, are filed in binder 4.3 (‘consultant/heating, ventilation, and sanitary’), the final versions of the central CAD plans are filed in separate binders, as are the detail drawings.

Depending on the diversity and quantity of documents, additional criteria may be introduced The binder coded ‘3.4’ (‘authority/trade law’), for example, contains

Fig. 10.16 The binders for *Pleasure Dome*. The two pictures below show that many binders are created and labeled ad-hoc on the one hand, and that there are always things that remain outside the classification in use

extracts from particular laws, hand-written notes, sketches representing things that have been negotiated, and project-related decrees. The print-outs of important emails—up to more than a hundred pieces of correspondence may go out on a single day—are kept in a particular binder.

Practically all written (textual, graphical) material that is generated in a project is filed in the binders. The most common way for the architects to retrieve a paper document is by remembering with whom and (approximately) when they talked about a problem that is related to this document. The chances of finding a copy of the document, often with annotations, are high.

The closing down of a project requires looking through the binders and deciding which ones to keep and archive. There are some general rules for this in the office concerning material that needs to be kept by law. Still, decisions have to be made about which of the material to archive for possible re-use in the future.

5 The Clustering of Coordinative Practices

The diversity of coordinative artifacts reflects the fact that the planning of a building involves a number of concerns and issues: the division of labor among architects and the other participants in the process; the multiplicity and interrelatedness of representational artifacts; the different competencies and ordering preferences of different types of actor; the incremental and iterative nature of the process; the different priorities of different contributing activities, stages, etc.; the separation and partial overlapping of planning and construction; the different stages of evaluation and approbation; the multifarious types of component and material; and the different critical temporal orders.

The diverse specialized artifacts are designed to address specific issues and concerns. They represent different 'local' principles of ordering. But the design and planning process is not a loose assemblage of fragmentary practices. To the contrary. The coordinative practices and artifacts at the architects' office are used in conjunction with each other, and together they are instrumental in ensuring and maintaining a workable degree of order in a variety of respects:

- achieving some degree of consistency in the graphical expressions and objectifications of vague notions, mere ideas, concepts, specifications;
- identifying and validating individual artifacts and versions of artifacts;
- maintaining a practical degree of consistency across the local activities as constituted by division of labor and specialization, over time, etc.
- keeping track of and providing access to the vast and perpetually changing collection of representational artifacts;
- ensuring timely progression across the myriad local activities;
- documenting that actors, including external partners and authorities, meet agreed-to or statutory deadlines;
- recording and filing evidence of due process;
- and so forth.

In addition to the specific purposes they serve, but at another level of abstraction, the coordinative practices and artifacts we find in architectural design and planning provide what one could call 'interoperability' among hundreds of plans and drawings; they objectify—in the form of pointers, references, indices, lists, classifications, etc.—the *relationships* between the myriad building elements and details in the plans and drawings and between plans and drawings as well.

And at yet another level of abstraction, by mapping plans and drawings onto scheduled transactions, and vice versa, the coordinative practices and artifacts provide means for ensuring that the incremental and iterative aggregation and specification of the collection of plans and drawings on one hand are integrated with the scheduled progression of the endeavor as a whole and of the various milestones and deadlines on the other hand.

In this multifarious web of interdependent artifacts and practices, schemes and notations, we can identify clusters of coordinative artifacts, each of which supports a set of interrelated practices. Figure 10.17 gives a schematic overview of the coordinative artifacts we have described and their interrelationships. Centered around key representational artifacts such as the central CAD plan and the two collections of drawings (detail and component drawings, respectively), we find amassed

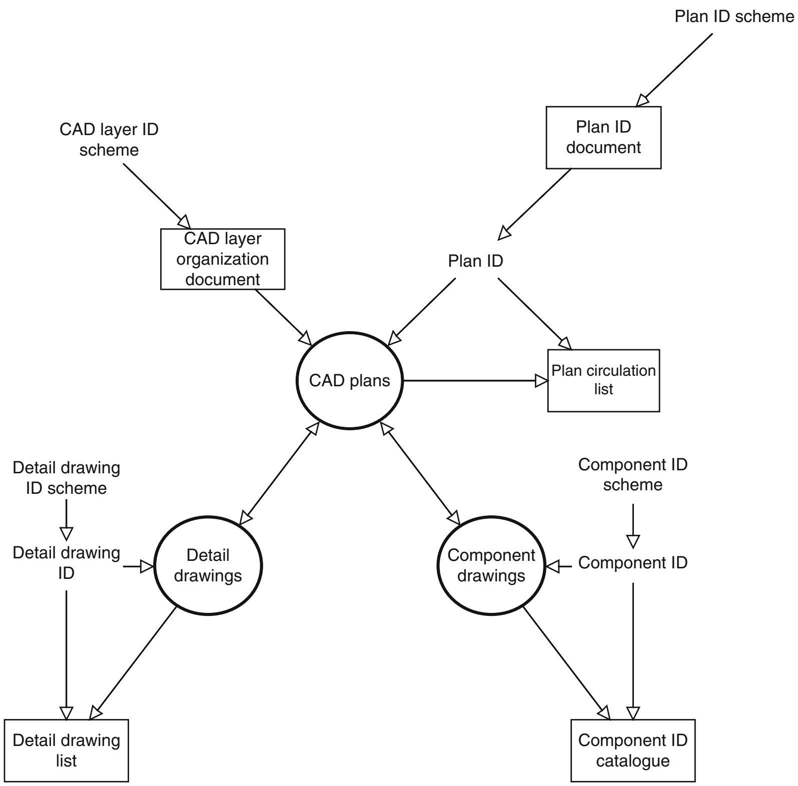

Fig. 10.17 Key ordering systems in architectural practice and associated coordinative artifacts and schemes. (Highly simplified)

something like a planetary system of coordinative artifacts devoted to maintaining order in and with respect to those central collections.

The CAD plan system comprises the CAD plans with layers, the layer list, color codes, and connected to this, in an outer orbit, the plan number document, the plan identification code, the title block, and the plan circulation list.

A major function of this cluster (the CAD plans, the layer organization, and the layer organization document) is to facilitate the division of labor in the planning process. The layer organization also reduces the complexity of the overall work by modularizing the planning document. It is a device that to a significant degree allows activities to be decoupled. Extracting sets of layers makes it possible for different views to be brought to bear on the design. Similarly, making references (pointers) to detail drawings, components, or standard products from within a CAD drawing, shifts some of the work of detailing and accounting for the numerous design changes to other workplaces and actors.

The plan number document, the plan identification code, and the title block together ensure that there is a unique identification code for each CAD plan. Lists such as the plan circulation list serve as indexation devices. They also help actors in keeping track of the exchanges of plans with external stakeholders.

The drawing systems. The component catalogue as well as the detail drawings together with the list of detail drawings, are used for specifying components and design details, for keeping order in the repository of detail drawings, and for reference and retrieval. All of the artifacts comprising this ordering system are assembled and kept in a large binder. The component catalogue provides a detailed description of components and how to assemble them (and sometimes component drawings). It also contains references to standard specifications and requirements. It maintains evidence of the relationships between components and detail drawings, which are referred to in the catalogue by their identification code. Both, the catalogue as well as the list, also serve as a kind of checklist, with empty spots attracting attention to components or details that have not yet been specified. The architects can see at a glance from the lists how far work has progressed. With many actors working on different sections of the building and on different problems, and specific actors responsible for the drawing of details or components, such an overview of process and status supports the scheduling and coordination of activities. Like the plan circulation list, the list of detail drawings helps actors to keep track of the exchanges of drawings with external stakeholders.

The binder system. The binder system with the associated labels, have been designed for documenting and maintaining an overview of the architects' interactions with external specialists. The binders contain an almost complete documentation of these transactions.

We call these clusters of practices and artifacts *ordering systems*.

Orderings systems are not restricted to the handling of collections. Some ordering systems are ubiquitous in the modern world, such as ordering systems for organizing meetings (a complex of calendars, clocks, agendas, minutes, mailing lists, room IDs, etc.) or for ensuring due process and administrative accountability (files and folders, archives, standards operating procedures, organizational charts, circulation lists, schedules), etc. In fact, they are not only ubiquitous; their development and widespread use are preconditions for modern industrial society (Yates, 1989; Olson, 1994; Crosby, 1997). Other ordering systems are highly domain specific, such as those used in the production industries where one will find specific complexes comprising drawings, bills of materials, process cards, routing schemes, etc.

Now, whatever their specific purpose, ordering systems are based upon the combination of specialized coordinative practices and concomitant artifacts. These specialized practices are elaborate literate practices involving standardized inscribed artifacts. That is, to coordinative practices the role of coordinative artifacts is essential and indispensable.

This proposition is not uncontroversial. In fact, the underlying distinction between the concept of conduct and the concept of artifact, between the mental and the material, is generally confused and even seen as meaningless by schools of thought that play a central role in CSCW (not to mention HCI and cognitive science). Therefore, in order to begin to understand coordinative practices, we have to clarify the concept of artifact, in particular inscribed artifact, and analyze the role of such artifacts in human activity. That is, we need to digress from our analysis of ordering systems in architectural practices for a brief interlude.

The challenge is to understand these coordinative practices and artifacts in their specificity and complex interrelationships.

6 Interlude: The Specificity of Coordinative Practices

> Hunger is hunger, but the hunger gratified by cooked meat eaten with a knife and fork is a different hunger from that which bolts down raw meat with the aid of hand, nail and tooth.
> (Marx, 1857)

> Hegel seems to me to be always wanting to say that things which look different are really the same. Whereas my interest is in showing that things which look the same are really different.
> Wittgenstein to Drury, 1948 (Drury, 1974)

Coordinative practices are historically specific practices, grounded in the use of material artifacts. The practices of architectural design and planning—while exhibiting a host of local idiosyncrasies, particular to the country and office in question—incorporate standardized domain-specific practices of drafting, not least the array of representational conventions, notations, etc., that are partly embodied in the standard 'tools of the trade' (technical pens and pencils, drafting implements such as rulers and curves, CAD tools, etc.). These practices of drafting in turn build upon and are specializations of more generic genres and techniques of visualization and annotation. Going deeper into this formation of practices, architectural design and planning also incorporate relatively common conventional practices of naming and validating artifacts and items; standard practices of classification and of ordering classification schemes; standard practices of expressing and organizing hierarchies, matrices, and lists; and, deep down in the baggage of civilization, standard practices of measurement, mapping, calculating, coding, writing, counting; uses of emblems and tokens.

All of this, the formation of superimposed and interlaced practices, deposited historically, as it were, as layer upon layer of sedimented practices, is effaced by in the tradition which we, following Ryle (1949), call the 'intellectualist legend.'

6.1 The Intellectualist Legend

> Intellectualism is inscribed in the fact of introducing into the object the intellectual relation to the object, of substituting the observer's relation to the object for the practical relation to practice.
> (Bourdieu, 1980, p. 58)

The intellectualist legend works this way. In the various appellations of the intellectualist legend (Lockean empiricism, Cartesianism, mentalism, cognitive science, etc.) human mentality is ascribed properties, ways of functioning, that are, really, modeled on historically specific practices such as the procedures of scientific investigation and intellectual discourse. Learning something, the story normally goes, consists in formulating hypotheses, testing them, making inductions, debating alternative accounts, articulating rules, etc., and when we then act in a rational manner, we are applying these 'rules'. When we recognize something, the story goes on, the mind is comparing the data it receives from the sensory organs with what is stored in the form of, say, 'chunks', 'schemes', 'representations', or 'mental models.' Again, when we speak, we encode what we have 'on mind', the message, which is then transmitted to the listener who then, in turn, decodes it and thus retrieves the message.—What this amounts to is not only the ascription of the characteristics of propositional reasoning and knowledge onto the immensely variegated practices of pre-propositional rational conduct, but the hypostatization of historically specific cultural forms of rational conduct that involve and completely depend upon the use of physical signs: writing, calculating, listing, etc.

Activity theory is an excellent example of this confusion and its consequences, precisely because of its laudable aspirations and achievements. Activity theory emerged in opposition to and as a break with the fundamental presumption of behaviorist psychology, viz. that human cognition is to be understood in terms of generic abilities. Against these presumptions, the Russian psychologist L. S. Vygotsky suggested a conception of human action that was heavily influenced by Marxian theory, arguing that cognitive phenomena such as logical reasoning are grounded in historically evolving and culturally specific material practices. Thus, to Vygotsky and his followers, the skills involved in the production and use of tools, in the techniques of reading and writing, arithmetic, etc. are of central concern to psychology.

Vygotsky's laudable ambition was undermined, however, by his concept of 'psychological tools':

> 1. In the behavior of man we encounter quite a number of artificial devices for mastering his own mental processes. By analogy with technical devices these devices can justifiably and conventionally be called psychological tools or instruments [. . .]. 3. Psychological tools are artificial formations. By their nature they are social and not organic or individual devices. They are directed toward the mastery of [mental] processes — one's own or someone else's — just as technical devices are directed toward the mastery of processes of nature. [. . .] 4. The following may serve as examples of psychological tools and their complex systems: language, different forms of numeration and counting, mnemotechnic techniques, algebraic symbolism, works of art, writing, schemes, diagrams, maps, blueprints, all sorts of conventional signs, etc. 5. By being included in the process of behavior, the psychological tool modifies the entire course and structure of mental functions by determining the structure of the new instrumental act, just as the technical tool modifies the process of natural adaptation by determining the form of labor operations. (Vygotskij, 1930)

One should, of course, grant Vygotsky the right to use metaphor and (ignoring his insistence that his choice of words is 'justifiable') simply take his notion of 'psychological tools' as a somewhat awkward term for what we now would call sign systems. However, on closer inspection his analysis turns out to be quite equivocal, as it conflates techniques, practices, skills, signs, notations, with their material counterparts such as diagrams, maps, and blueprints. That is, his understanding of sign systems is rooted in mentalist preconceptions (Goody, 1987, p. 216). Not only does the concept of 'psychological tools' reify the skills involved in speech, writing, numeration, counting, etc., in that it suggests that skillful action is somehow 'determined' by some putative mental structures. More than that, by subsuming mental as well as material phenomena under the category of 'tool', any notion of materiality is eradicated from the concept of tool as well. 'Mental processes' are reified while material artifacts are spiritualized. In short, no sooner had the use of artifacts been made a central issue (and rightly so), before the materialist notion of artifact was conceptually dissolved.[7]

This de-materialization of the concept of artifact has been continued uncritically in the subsequent activity theory tradition. Not only is the term 'psychological tools' in continued use (cf., e.g., Wertsch, 1985; Wertsch, 1991). But it is also evident that activity theory, as an intellectual tradition and as a conceptual framework, makes it difficult to systematically address the role of material artifacts in work. In an introductory article, Kuutti for instance, mentions 'instruments, signs, procedures, machines, methods, laws, forms of work organization' as examples of 'artifacts' (Kuutti, 1997, p. 26). Similarly, following Engeström. Kuutti mentions in passing, without further arguments, that 'An object can be a material thing, but it can also be less tangible (such as a plan) or totally intangible (such as a common idea) as long as it can be shared for manipulation and transformation by the participants of the activity.' (Kuutti, 1997, p. 27). At this stage, the concept of artifact has become utterly vacuous, as it simply denotes anything we can give a name, a point Kaptelinin brings home, unwittingly, by stating that 'Activity theory itself is a special kind of artifact' (Kaptelinin, 1997, p. 36).

The 'distributed cognition' framework developed by Hutchins and associates can be seen as a further development of the activity theory framework, in that it insists on studying human cognition in terms of historically and culturally localized practices. As opposed to activity theory, however, Hutchins pays detailed attention to trajectories of action 'distributed' over actors and artifacts in what he terms 'a system of distributed cognition.' (Hutchins and Klausen, 1996, pp. 16–17). In doing so, Hutchins directs attention to the specific format of the artifact and its role in human action.[8]

In spite of this, Hutchins de-materializes artifacts no less than Vygotsky, albeit in a different way. While Vygotsky talked about 'psychological tools' and thus only indirectly dissolved the concept of artifact, Hutchins does it directly, by conceiving of artifacts merely as vehicles of so-called 'representations' and 'representational

[7]Our immediate concern here is with Vygotsky's use of the concept of 'tool', but it should be pointed out in passing that his use of the term 'psychological' is equally problematic. Are for instance diagrams, maps, and blueprints 'devices for mastering [. . .] mental processes'? One would rather think that diagrams are devices for specifying analytical distinctions, that maps were devices for navigating, and blueprints devices for construction processes. After all, what is particularly 'psychological' about these artifacts? If maps and algebraic symbolism are to be taken as 'psychological tools', should we then also think of, say, money and marriage licenses as other instances of this category?

[8]This attention to the specific format is especially pronounced in Hutchins' earlier work (e.g., Hutchins, 1986, pp. 47 f.)

states' on par with 'internal memories'. Thus, when summarizing their analysis of cooperative work in an airline cockpit, Hutchins and Klausen state:

> We can see that the information moved through the system as a sequence of representational states in representational media. From speech channels to internal memories, back to speech channels, to the physical setting of a device. Its representation in each medium is a transformation of the representation in other media. (Hutchins and Klausen, 1996, p. 27)[9]

The notion that an invariant and immaterial being, 'the information', migrates from mind to artifact to mind is extremely problematic. The notion of a ghostlike entity that in turn takes residence in people and artifacts and yet somehow manages to maintain its unity and identity, is not just a mentalist notion, as Vygotsky's notion of 'psychological tools'; it is what Taylor and Harris have aptly termed 'telementational' (Taylor, 1992; Harris, 1995, 2000): the transfer of invariant entities from mind to mind or even, according to Hutchins, from 'medium' to 'medium'.

In short, the intellectualist tradition, of which 'activity theory' and 'distributed cognition' are important branches, is painting a picture of 'the mind' as something like a scientific laboratory equipped with tools and artifacts, a repository of representations, and—concomitantly—a world populated with ghostlike artifacts, little more than transitory incarnations of 'the information'; a picture of the mind as a collection of immaterial things and the world as a collection of disembodied inscriptions.

Now, the fact that the various 'representational media' are of a different nature and have different characteristics is far from ignored by Hutchins, of course. In fact, it is one of his key concerns:

> In the cockpit, some of the relevant representational media are located within the individual p[i]lots. Others, such as speech, are located between the pilots, and still others are in the physical structure of the cockpit. Every representational medium has physical properties that determine the availability of representations through space and time and constrain the sorts of cognitive processes required to propagate the representational state into or out of that medium. (Hutchins and Klausen, 1996, pp. 27, 32)

But this recognition of the different characteristics of minds and dials brings us nowhere, as long as the telementational notion of the infallible reincarnation of a mystical unitary being, 'the information', is presumed.

By presuming 'the information' as a unitary entity that propagates in the system while retaining its integrity, Hutchins and associates elide the practices of producing this continuity and integrity. The orderly alignment of activities is simply taken for granted, like an invisible hand that somehow, quite mysteriously, creates order behind the back of the actors. Moreover, by tacitly presuming that artifacts are mere successive representational incarnations of 'the information', the 'distributed

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[9] The same kind of analysis can be found in Hutchins' study of maritime navigation: 'The representations of the position of the ship take different forms in the different media as they make their way from the sighting telescopes of the alidades to the chart. [. . .] Representational states are propagated from one medium to another by bringing the states of the media into coordination with one another.' (Hutchins, 1995, p. 117).

cognition' framework implicitly treats the materiality of the artifact as immaterial, so to speak.

The proponents of distributed cognition rightly criticize the Cartesian version of mentalism for construing the mind as an inner stage, ontologically separated from the social and material world. But they do so by construing the social and material setting as the carrier of some mysterious 'distributed cognition', a rather spooky place. Instead of Cartesian dualism we have been offered distributed mentalism.

The mind is not a place. The mental is not an entity at all and cannot be 'inside' or 'external'; nor can it be 'distributed.' 'Cognition' is neither a 'mental process' nor 'socially distributed.' When we talk of 'the mind' we are not referring to a place or an ability but to the mindfulness exhibited by actors when they sometimes perform competently, smartly, vigilantly, wittily, etc. (Ryle, 1949; Coulter, 1983; Shanker, 1998).

The purpose of this argument is not to belittle, across the board, the value of the contribution of 'activity theory' or 'distributed cognition' to CSCW, but simply to point out that while the central role of artifacts in cooperative work has been recognized and applauded, the concept of artifact as used in CSCW is murky, ripe with all sorts of mentalist and cognitivist precepts that impede and confuse the investigation of the infinitely variegated array of actual material practices.

When we have liberated representations, notations, etc. from the mentalist House of Usher we can begin to see them as and investigate them as a range of very specific conventionalized material practices involving the production and use of inscribed artifacts.

The practices of classification are the case in point.

6.2 Practices of Categorization and Classification

> the appropriate criteria for deciding whether two people are engaged in the same kind of activity [. . .] belong to that activity [. . .] itself.
> (Sharrock and Read, 2002, p. 136)

As concepts of practices, the concepts of classification and categorization are victims of enormous confusion.

On the prevailing intellectualistic notion of cognition, the categorical differences between practices such as the practical categorizations inherent in everyday speech or the systematic and coherent classification systems based on writing systems are elided or denied. Even authors as conceptually incisive as Bowker and Star, whose seminal studies of practices of categorization and classification have served us as a guide, do not distinguish such practices (Bowker and Star, 1999).[10] But, surely,

[10]The purpose of the work of Bowker and Star is also somewhat different from ours, in that their overriding concern is that of identifying the tacit ideological presumptions and values they find at work in classificatory practices. To this 'moral and ethical agenda' (Bowker and Star, 1999, p. 5), the distinctions we find critically important might be seen as of little or no consequence.

there is a significant difference between the practice of saying something like 'That's not a seagull, it's an albatross', and the practice of, for instance, composing a plan identification code such as 'PW-1-M-E1-103-V1' by consulting various supporting lists and written instructions to obtain the respective codes for stage, section, type of plan, previously issued plan and version numbers, etc. The practices are to be considered different because different 'rules' are involved (cf., Winch, 1958), i.e., discrimination criteria, principles of ordering and deciding, procedures of designation and validation, etc.

With respect to practices of categorization and classification we should therefore observe the following very subtle but categorical distinctions:

(1) When you look out the window and see sky, clouds, trees, birds, and so forth, you do not thereby categorize or classify these phenomena as sky, clouds, trees, birds and so forth. You just see them.

This does not imply that 'seeing' does not involve culturally specific skills. Seeing a house for what it is, i.e., *as a house,* is of course not an innate ability. You may have learned to discriminate a cottage from a mansion (what Ryle calls 'thick description' (1968)), but merely discriminating is not categorizing but merely the exercise of the skill of noticing specific features that to the 'untrained eye' are of no consequence.

Seeing is not the same as recognizing, either. When you see birds in the sky, you do not recognize them as birds, unless, of course, doubt was possible.[11] If you for a brief moment took the black shape in the grass for, say, a kitten, and then see that it is indeed a bird, then you have recognized a bird. When you open your eyes in the morning and see the same old wall, you do not recognize the wall, for doubt was not an option.

Categorization, by contrast, is a linguistic operation of ascribing a category or concept to a particular phenomenon by the means of signs. Merely talking about phenomena, however, is not necessarily categorizing them, although talking involves the application of concepts. To categorize is to make a conceptual proposition ('red is a color').

In categorizing what you see as trees and birds you emphasize certain aspects of the world while abstracting from others, for instance that the trees and birds may all have green colors or that clouds and leaves may all be moved by the wind. An act of categorization cuts the world into pieces in that it emphasizes certain features at the expense of others ('x belongs to category C').

(In themselves acts of separating objects are not acts of categorization, as they are not necessarily linguistic operations. Peeling onions or removing dirt from one's body by means of soap and water are acts of separation but not acts of categorization, although they may be subjected to acts of categorization, for instance when one is instructing children in how to do it. Similarly, when sorting the garbage (putting paper in this container, potato peels in that container) one may, or may not, be following instructions involving categorizations.)

[11] 'Doesn't one need grounds for doubt?' (Wittgenstein, 1949–1951, § 123)

(2) Classification, in turn, is a special practice of categorization, involving pre-established and systematic systems of signs. That is, classification is a linguistic operation of applying a *classification scheme*, i.e., an ordered set of signs that is pre-established according to (*a*) some general principles and criteria of ordering and (*b*) some procedures of identification and naming. In short, an act of classification is an application of a classification scheme. *Classification systems* (such as thesauri) can thus be seen as instantiations of classification schemes.

(3) Classifications and categorizations are both convention-based practices and equally so. But classifications are convention-based in a quite specific sense. In the case of categorization there are no pre-established principles and criteria for determining the correctness of an act of categorization. With acts of classification, however, such pre-established principles and criteria exist, in that they specify relationships between items in terms of, for example, class/membership, part/whole, composition, cause/effect, origin/fate, function, ownership, value/risk, location, or state. Accordingly, an actor applying a classification scheme in a particular case can be held accountable in terms of the principles, criteria, and procedures of the classification scheme.[12]

The point we want to make is that we have to be quite specific in distinguishing different types of discriminative practice: *seeing* something, seeing something for what it is as opposed to something else (reflecting on what one is seeing), physically *separating* things in some regular way, *saying* that *x* is C ('categorizing' *x* as C), and *classifying x* as C according to an *inscribed*, publicly available classification system. These are radically different practices, involving radically different forms of convention, principles of abstraction, etc. (Just like following a rule is radically different from acting according to a rule). We are here, of course, following Wittgenstein, for instance when he in *Zettel* observes that

> We do not see facial contortions and make inferences from them (like a doctor framing a diagnosis) to joy, grief, boredom. We describe a face immediately as sad, radiant, bored, even when we are unable to give any other description of the features. - Grief, one would like to say, is personified in the face. [In short] an *interpretation* is something that is given in signs (Wittgenstein, 1945–1948, §§ 225, 229).

(4) Classification schemes differ according to forms and procedures of abstraction, systematization, and accountability. They may, as in the case of certain scientific classification systems, be sophisticated and rigorous theoretical constructs (e.g., cladistic classification in biology (Hull, 1988; Mayr and Ashlock, 1991)). Other professions such as architects use classification schemes that, although not strictly speaking theory-based, draw upon engineering and aesthetic frameworks and methodologies and yet, at the same time, have strong elements of practical sense.

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[12] This does not, of course, prevent technical terms derived from the application of a classification system from becoming part of vernacular vocabulary, for example as a result of ordinary education and popular science. It goes without saying that the use of a term such as *Homo sapiens* by a lay person is not an indication that the person has applied the classification scheme of systematic zoology.

(5) Classification schemes are institutionalized conceptual, linguistic, and procedural constructs that are part of the practices of professional communities (accountants, biologists, engineers, architects, etc.). Classification schemes are developed by and for these communities, and members typically learn how to use them competently at schools and universities or in the course of their practical training.

(6) A specific classification scheme is often superimposed on a system of designation and is thereby expressed in a particular notation. As a result, the classification of an item as expressed in the notation is identical to the name of the item, and vice versa: the name is a direct expression of the classification. There are, as we have seen, significant economic advantages of this dual function, in that one does not have to maintain two distinct systems and, more importantly, does not have to institute a procedure for maintaining consistent mapping between them.

(7) Classification systems are often used as indexation systems but again the two concepts are not coextensive.

An indexing system is a special kind of ordering system that gives the location of an item. The obvious example is the index in a book: an alphabetical list of selected names or terms with associated page numbers. Indexes may be more or less elaborate. The primitive form of index is a simple pointer, as can be found in the form of links in millions of web sites: 'To read more about x, click here'. The meaning of the pointer is here given by the sentence in which it is situated. In database systems, indexes are based on sets of keywords, sometimes just a simple list of terms. Sometimes, however, the index is organized as a classification system, that is, as a set of terms ordered according to some semantically meaningful principle.

While indexes are not necessarily based on classification systems, classification systems may be devised and used for other purposes than indexation, as in the cases of the systems of classification of stars, elementary particles, and biological species. On the other hand, however, classification systems developed for other purposes than indexation may of course be applied as indexation systems in the organization of artifacts of interest to the related communities (photos, papers, samples).

In any event, for a classification system to serve as an indexation system in the handling of a large collection of artifacts in cooperative work, it must be inscribed upon some artifact or system of artifacts (sheets, cards, binders, rolodex, catalogue, shelves, doors) with an associated 'syntax' (list, matrix, hierarchy, map). Without inscription and the 'technologies of the intellect' (Goody, 2000) based on inscription, the classification system will decay, unnoticeably and unstoppably (Goody, 1977, 1986).

6.3 Technologies of The Intellect

What coordinative artifacts afford, *as* inscribed artifacts, goes beyond mere retention, storage. They certainly do enable actors to express items of coordinative relevance in a durable and yet mobile form and they, hence, make it possible for others to revisit these items at a later stage. But they do far more than that. Their format evidently betrays that they are not just somebody's memoirs or a pad of letters. Coordinative artifacts concatenate and configure items in certain spatio-graphical ways.

Jack Goody's observations on the affordances of lists are most pertinent to this discussion:

> The list relies on discontinuity rather than continuity; it depends on physical placement, on location; it can be read in different directions, both sideways and downwards, up and down, as well as left and right; it has a clear-cut beginning and a precise end, that is, a boundary, an edge, like a piece of cloth. Most importantly it encourages the ordering of the items, by number, by initial sound, by category, etc. And the existence of boundaries, external and internal, brings greater visibility to categories, at the same time as making them more abstract. (Goody, 1977, p. 81)

Similarly, 'the formalized graphic arrangements in matrices' provide precise spatial locations for items.

> they not only extract, codify and summarize a great deal of information otherwise embedded in the flux of experience, but they also make it possible to manipulate, reorganize and reformulate this information in a manner that is virtually inconceivable in the purely oral context. (Goody, 1987, p. 276)

More specifically, the formalized graphic arrangements in matrices highlight omissions and inconsistencies: 'The table abhors a vacuum' in the sense that 'anyone composing a matrix is almost forced to fill all the gaps, to leave no "empty box"' (Goody, 1987, p. 275 f). As a result, due to their specific graphic format, matrices are instrumental in maintaining consistency and completeness in what would otherwise be a chaotic assemblage of items.

They also, as in the cases of the lists of components and detail drawings and the plan circulation list, represent in what Cournot aptly calls a 'synoptic' format what is otherwise spatially and temporally disjoint and distributed thus suspending and canceling time and space:

> We try to correct [the imperfections inherent in discourse] by constructing synoptic tables, trees, and historical atlases: types of tables of double entry, in the outlining of which we are more or less successful in representing two dimensions of an extended surface, so as to indicate systematically relations which are difficult to disentangle within the concatenation of discourse. (Cournot, 1851, § 243, p. 357)

That is, as Bourdieu, invoking Cournot's observations, puts it, 'the synoptic diagram enables one to apprehend simultaneously and in a single glance, [...] meanings that are produced and used *polythetically*, i.e., not only one after the other, but one by one, step by step.' (Bourdieu, 1980, p. 140).[13]

Now, consider again the various representational and coordinative artifacts presented earlier in this article. The contrast between the graphical format of the representational artifacts and the coordinative ones is remarkable. The coordinative artifacts exhibit a striking graphical frugality. As a rule, they all employ what Goody calls 'crude written techniques' such as lists or hierarchical lists, and matrices. Compared to the expressiveness of the representational artifacts, the

[13] Harris (1995) provides detailed and incisive analysis of the semiology of tables and charts.

appearance of the various coordinative artifacts is boringly monotonous. Even an artifact as rigorously standardized as the CAD plan, appears almost frivolous in comparison.

Although they all invoke and incorporate various conventional practices and, in the case of the CAD plan, at times even highly formalized ways of graphical communication, representational artifacts are subservient to their function; namely, to represent. They show, in terms of certain conventionally established criteria, how the eventual building will look like, its internal structure and outward appearance, the materials of which it will be composed, etc. Representational artifacts are in the service of depicting the infinitely multifarious material world.

The coordinative artifacts, by contrast, are not in the service of depicting the material world or, indeed, of depicting anything. They are in the service of *imposing order.* The graphic techniques employed in coordinative artifacts serve the purposes of coordinating distributed activities, not in spite of but exactly because of their excessive formality.

In making this point, we are stepping into a methodological controversy in sociology, a veritable hornets' nest. The tricky issue arises from the fact that practitioners such as the architects in this case use representations of their own work to achieve workable order in their own work. We thus need to carefully sort out the relationship between actual practice and representations of practice.

6.4 'Crude Written Techniques' As Members' Practices

The historical specificity of practice, and of the 'technologies of the intellect' in particular, is not only ignored by psychologists of the various mentalist orientations but also by anthropologists and sociologists when they, in their analysis of social practices such as kinship systems or mythologies, presume that their models of these systems somehow exist 'out there' (e.g., Lévi-Strauss, 1962).

In Jack Goody's words: 'The trouble arises from applying a crude written technique (the table) to a complex oral process, then claiming one has the key to a culture, to a symbolic system.' He goes on to say that 'this simplification produces a superficial order that reflects the structure of a matrix more obviously than the structure of the (or a) human mind' (as claimed by Lévi-Strauss), and thus 'produces gross general similarities in all this type of construct' (Goody, 1977, pp. 67 f.). Goody's astute observation has later been developed significantly by Bourdieu:

> It [...] took me a long time to understand that the logic of practice can only be grasped through constructs which destroy it as such, so long as one fails to consider the nature, or rather the effects, of instruments of objectification such as genealogies, diagrams, synoptic tables, maps, etc., among which, thanks to the recent work of Jack Goody (1977), I would now include mere transcription in writing (Bourdieu, 1980, p. 24).

Taking the anthropologists' construction of models of kinship systems as his example, Bourdieu argues that the status of such models of relations with respect to members' 'practical' relations ('practical because continuously practiced, kept up and cultivated') 'is identical to the status of the geometrical space of a map, a representation of all possible routes for all possible subjects, with respect to the network of pathways that are actually maintained and used, "beaten tracks" that are really practicable for a particular agent.' (Bourdieu, 1980, pp. 58 f.).

In line with Schutz' concept of 'the natural attitude', Bourdieu points out that practitioners do not quest for certainty and infallible consistency. To practitioners, he claims, the application of the same general schemes to different local practices is 'highly economical but necessarily approximate'. This necessarily gives rise to inconsistencies that, however, 'pass unnoticed' because 'the successively performed practices are only apprehended successively'. It is 'very unlikely that two contradictory applications of the same scheme will collide' in the same local practice, and, accordingly, practitioners do not take 'the trouble to systematically record and compare the successive products of the application of the generative schemes.' (Bourdieu, 1980, p. 145).

These distinctions and methodological observations are crucial to any study of practice, because 'Practice unfolds in time [. . .]. Its temporal structure, that is, its rhythm, its tempo, and above all its directionality, is constitutive of its meaning'. Only for the analyst, who is external to 'the urgency, the appeals, the threats, the steps to be taken' of actual practice, 'can the temporal succession be seen as a pure discontinuity' (Bourdieu, 1980, p. 137 f.).

But these insights should not be applied ahistorically. In our investigations of ordering systems in architectural work as well as in other areas of cooperative work in modern economies, we are dealing not only with 'complex oral processes' but also, and most importantly, with members' using various 'crude written techniques' to organize their work. Bourdieu sometimes argues as if literate practices are not genuine practices and as if the characteristics of practices are not historically specific. But this is evidently erroneous, as copiously demonstrated by Goody and many others scholars.

If we conceive of the analyst's model of members' (oral) categorizations and *members'* own use of standardized (written) constructs as *identical* phenomena, we are in the same kind of trouble as the anthropologists and sociologists so appropriately criticized by Goody and Bourdieu. A community's own, inscribed classification systems surely involve *completely different practices* than the categorization practices of an oral culture (of which an analyst may build a model in the form of an inscribed classification system).

In fact, in architectural planning as well as in other complex cooperative work settings we find practices in which it is of critical importance to members to be able to see 'temporal succession' as a 'pure discontinuity.' The 'crude written techniques' such as lists and tables are not just something that belong to the practices of anthropologists but are also, massively, part of the everyday practices of ordinary cooperative work in modern economies.

The point is that to the architects the purpose of producing and using classification schemes (and ordering systems in general), is to integrate distributed activities and thus, in certain respects, serve to regulate (curtail, contain, suppress, harmonize, standardize, interrelate, synchronize, etc.) local activities and thereby align or unite what would otherwise be intolerably inconsistent local practices.

In a cooperative work setting such as the architects' office the whole purpose of the application of ordering systems is exactly to do what practitioners, according to Bourdieu, do *not* do. The architects own point of applying these 'crude written techniques', is to 'systematically record and compare the successive products of the application of the generative schemes'.

What goes on in this setting is not just any kind of practice; it is cooperative work, massively technically constrained work. The individual activities and local practices of the cooperative effort are not 'discrete, self-sufficient units'; to the contrary, the individual activities and local practices are complexly interdependent. Hence, in a setting like this 'two contradictory applications of the same schemes' *are* likely to 'collide'. Members therefore actually *do* 'take the trouble to systematically record and compare' how schemes and procedures are applied. Their highly elaborate literate practices have been painstakingly developed and are being meticulously applied for exactly this reason.

7 The Economy of Ordering Systems

> The economy of logic [. . .] dictates that no more logic
> is mobilized than is required by the needs of practice
> (Bourdieu, 1980, p. 145)

As we have tried to show, the architects' ordering systems are instrumental in producing and maintaining a workable degree of order in a variety of respects. They are assemblies of specialized practices and artifacts, each of which have been designed to address very specific coordinative issues and concerns. We have also tried to show that their specialized coordinative artifacts are used in conjunction, in a set of interrelated practices; they form clusters, ordering systems.

The integrative role of the entire assemblage of coordinative artifacts and practices is facilitated by their underlying uniformity.

(1) One can conceive of a specific coordinative artifact as a specific configuration of a set of elementary and general ordering techniques: notations that restrict the valid code to a finite set of signs; systems of designation that produce a unique name for items and artifacts; synoptic arrangements of items in matrices and nested lists; classification schemes that concatenate items and artifacts in an certain systematic order; classification systems that enumerate the entire collection of items and artifacts; identifiers that certify the validity of particular artifacts; time series that prescribe and record transactions in sequential order. These elementary techniques are combined and recombined infinitely in the various artifacts.

(2) The different ordering techniques are used recursively: notations are defined by means of lists and the construction of lists is regulated by notations, lists are used for the construction of codes for the ordering of lists, etc. There is an obvious economy in keeping the repertoire of ordering techniques and schemes at a minimum.

(3) The principle of structural superimposition is used ubiquitously. Classification systems are superimposed upon systems of designation. The result of this is the practical integration of classification scheme, system of designation, and coding notation. From the identification code of an item, architects can determine how it is related to other items, who is responsible, how it is located in the general flow of work, etc.

(4) The classification schemes incorporated in the different ordering systems show certain family resemblances; they are obviously based on the same general principles; e.g.:

```
Plan identification code:
     'PW - 1- M - E1 - M2 - 103 - V1'
Component catalogue:
     'K-FD 01 <text>' ('cinema / flat roof, to be walked
     upon')
List of detail drawings:
     'K 8 11 <text>' ('cinema / interior doors / wood /
     restrooms').
Binder system:
     '4.3' ('consultant / heating, ventilation, and
     sanitary')
```

The general format, which is that of a 'polynomial nomenclature', is very simple and can be expressed this way:

```
<attribute a> / <attribute b> ... / ... <attribute n>
```

There are several reason for this uniformity. First of all, the positional syntax has obvious pragmatic advantages. It is open to additions and amendments; new positions and hence attribute types can be added, and for each position the set of valid values can be amended as well. Moreover, the sequence in which the attributes are itemized, and hence the resulting sorting order, can be changed to reflect the varying weight attributed in practice to different attribute types at different stages of the project and in different types of activity. The order in which different attribute types are listed can also be modified to accommodate for local and temporary exigencies. It is, in short, a quite flexible syntax.

The uniformity is of course an expression of the availability of instantiations of classification systems from the architects' previous projects, but it is also an expression of the presence of 'wider' classification schemes, inherent in modern architectural and related professional and industrial domains. They are inculcated as part of the training of architects and are applied in handbooks, product catalogues, and thesauri. The commercial databases in the area of architecture and construction use particular classification systems which in turn are based on thesauri in the field. Furthermore, each CAD system comes with its predefined layer organization which then can be adapted to practices within the office. In addition, these classification schemes reflect standard classifications of ‘deliverables’ in the building trades as well as municipal etc. standards for tender specifications. In short, the regularity is a manifestation of a specific professional culture.[14]

Underlying the manifest uniformity of the architects’ coordinative artifacts and practices is finally the fact that architectural work, like any other practice, is subjected to the economical logic of practice. Techniques that have proved effective are reused, to keep the repertoire of techniques to be learned and mastered small and wieldy. Hence the tedious monotony of instance after instance of identification and classification schemes of the same format, with only slight variations.

It is the ‘openness’ of the schemes and formats, their flexibility, that affords these obvious economical advantages. We have emphasized this aspect repeatedly.

However, what we have also found is that the ordering systems at the office are exposed to dissipating forces, so to speak. Their individual scope is limited in that each of them only apply to certain stages in the project or to a certain scale or level of detail. Furthermore, classification systems are subverted by the particularities of the planned building (different structural elements imply different classificatory concerns and principles). Multiple professions and other stakeholders are involved in different capacities, all of them introducing extraneous principles of classification into the ordering systems of the office. Finally, for obvious reasons, the architects reuse part designs from previous projects and thereby introduce other sources of disintegration.

To the architects, this poses a practical challenge. The challenge is a particularly hard one in their effort to maintain order in the CAD level organization and in the CAD notations, because CAD plans cross organizational and professional boundaries. But in any case, the challenge is a real one. The different identification and classification systems, and the underlying schemes and notations, are constructed and maintained manually. It is not only a labor-intensive task but an error-prone one as well. The requisite consistency in the practical application of the schemes and notations is accomplished through scrupulous attention to deadening details. More than that, while the syntax is open-ended, the cost of changes to the schemes increases as the project evolves, since the number of affected documents then is higher. This especially applies to changes to attribute types pertaining to the entire population of documents, such as, for example, the inclusion of the attribute type `section_of_building` in the plan identification scheme for the *Pleasure Dome* project. Integrating plans and drawings from previous projects or from external collaborators using a deviating system of designation may be even more costly, and in face of this it may be decided to live with inconsistency and tolerate the ‘alien’ code as a local dialect.

[14] In fact, it would seem as if the ‘polynomial nomenclature’ format of architectural identification and classification systems has been adopted from engineering practices. At any rate, one will find schemes of the exactly identical format at work in manufacturing design and in engineering in general, where these practices began to develop after the Second World War (Mitrofanov, 1959; Opitz et al., 1969; Opitz, 1970).

In the case at hand, the incremental disintegration of the ordering systems is not a major concern to the architects, for the simple practical reason that work is organized in projects and that the projects normally only last a few years. After that, when the building has been constructed and handed over, the entire collection of carefully indexed and cross-referenced collection of plans, drawings, lists, emails, letters, photographs, and so forth is put in boxes and stored in the basement, together with all the inconsistencies and irregularities that have crept in. At that stage, practitioners certainly do not take 'the trouble to systematically record and compare the successive products of the application of the generative schemes.' That is, at the level of granularity of the history of the office, spanning a succession of projects, Bourdieu is right. At *this* level of granularity, in the great scheme of things, inconsistencies in the application of the general schemes 'pass unnoticed' because 'the successively performed practices are only apprehended successively'.

But in the long run we're all dead anyway. The study shows that coordination and integration of interdependent activities across local practices is crucial and part of architects' work practice and that ordering systems, although they tend to decay over time, play a key part in this effort. In other areas of architectural work and civil engineering, in the planning and construction of much larger and more complex buildings (office towers, industrial plants, urban infrastructure), the incremental decay that can be tolerated here, in this case, would pose serious problems. In other domains still, where the design life cycle is very long (airplane manufacturing, for example), the observed incremental decay of the ordering systems would be intolerable.

There is, of course, no way that ordering systems could be made immune to decay. They are, to reiterate, local and temporary closures. There is an inescapable 'permanent tension' between the need for global ordering systems and the enduring or emerging local concerns and issues (Bowker and Star, 1999, p. 139). Local interests, local practices, and local issues and concerns will continually undermine the ordering systems (Bowker and Star, 1991, 1999). The issue is not that of achieving infallibility, but to *reduce the cost* of maintaining a tolerable degree of order in the cooperative effort.

Here lies a major challenge for CSCW.

8 The Challenge of Ordering Systems For CSCW

Coordinative practices and the coordinative artifacts upon which they rely obviously form complexes of interrelated practices and artifacts. We have proposed to call these complexes ordering systems.

To members these complex practices are instrumental in managing interdependencies that transcend local interactions. They are deliberately and carefully devised in a cooperative process to regulate and align local practices and are being used that way, that is, to enforce requisite consistency, keep track of potential repercussions, ensure accountability, etc. Moreover, to handle the inevitable contingencies of work, ordering systems are open ended. Classification schemes are amended or extended to deal with specific features of particular projects; classes are added and deleted, promoted or demoted, as needed; new notations are invented and introduced as required, and so forth.

The major challenge to CSCW emerging from this is the very fact that ordering systems are constructed and maintained in a cooperative process. How do we reduce the cost and increase the reliability of the distributed cooperative processes of producing and maintaining classification systems, notations, nomenclatures, procedures, etc.?

Furthermore, ordering systems are immensely composite practices, consisting of interrelated artifacts, classification schemes, notations, nomenclatures, standard formats, validation procedures, schedules, routing schemes, etc. This of course raises non-trivial technical issues of interoperability, as digital artifacts such as CAD systems, Excel sheets, lists, photos and email messages are incorporated in these complexes. How do we support the orderly interoperability of these artifacts, formats, schemes? More importantly, however, how do we support the (practically consistent) expression of the heterogeneous schemes of classification, designation, scheduling, routing, etc.?

Ordering systems are finally multi-level constructs in the sense that identification and classification schemes are expressed at different levels of abstraction. They are like generative schemes, flexibly instantiated in different forms as required. Thus we can observe that the schemes underlying different ordering systems have strong family resemblances. On the other hand, multiple schemes may be expressed in one particular ordering system. How do we support the flexible and recursive expression of schemes at different levels of abstraction and for different purposes? And conversely, how do we support the combination and recombination of different schemes for a particular purpose?

Part III
CSCW Reconsidered

We shall not cease from exploration
And the end of all our exploring
Will be to arrive where we started
And know the place for the first time.

T. S. Eliot: Four Quartets

Chapter 11
Formation and Fragmentation

There is an old Danish maxim, befitting a nation of seafarers: 'When there is wind, it's time to make sail'. For CSCW, now is such a time.

CSCW is unique in being not merely an interdisciplinary research field but a field of technological research that depends critically on in-depth studies of actual cooperative work practices in material settings. In accordance with this commitment CSCW has articulated and undertaken a critical examination and revision of fundamental assumptions and tenets in computing concerning socially distributed control of computationally distributed systems. At the same time, and by virtue of this commitment to development of technology, CSCW has been a major force in developing an understanding of work practices that has upset and overthrown the intellectualist and mechanistic (or 'cognitivist') notions and theories of orderly activities that only one or two decades ago seemed unassailable and unquestionable. In the process, conceptual frameworks and investigative strategies and techniques have been developed that help us to hone in on the ways in which mundane artifacts and clusters of artifacts are deployed and developed by practitioners. This has set a new and very high standard for rigorous analysis of actual work practices.

On the other hand, however, many signs indicate general perplexity and a sentiment that the research program that has brought the field this far no longer offers reliable directions. New computing technologies are redefining the general technological matrix in which CSCW was originally formed and have spawned new areas of technological research. For example, technologies such as wireless networks (GSM, WiFi, Bluetooth), positioning technologies (GPS, RFID, etc.), sensor and actuator technologies, handheld and wearable devices, etc., have given rise to research areas next to CSCW such as 'ubiquitous' or 'pervasive' computing, while the very same technologies obviously offer great potential for one of the central issues in CSCW, namely the support of 'mutual awareness'. Is that an indication that CSCW is or is on the way to become a thing of the past (cf., e.g., Crabtree et al., 2005)? Similar concerns are engendered by the development of technologies such as high-level computational notations for 'business process modelling' (e.g., BPEL), 'peer-to-peer' protocols, 'service-oriented architectures' (SOA), and

K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1_11,

so on (cf. van der Aalst, 2007).[1] In the course of two decades, the technological matrix of CSCW has become extremely heterogeneous. Moreover, new application areas for 'collaborative computing' (broadly defined) have developed, such as, for instance, large-scale cooperative efforts in scientific communities ('e-science', 'cyberinfrastructures').[2] In return, these application areas are bound to have significant influence on 'collaborative computing' (e.g., computational 'ontologies'), but will undoubtedly also add to the heterogeneity of the matrix.[3]

These centrifugal forces are strengthened by a surge of studies of the various ways in which well-known 'collaboration technologies' are being used: the new patterns and styles of social interaction that reportedly can be observed among people communicating 'on-line' by means of what is often—awkwardly—called 'social software' or 'social media', a motley of protocols, services, and facilities ranging from simple protocols such as instant messaging and chat to blogs to 'social networking' services and facilities such as Facebook, Twitter, Second Life, Flickr, YouTube, MySpace, etc. These new 'collaboration' facilities, currently appearing in rapid succession, have enchanted sectors of the general public. It is hardly surprising, therefore, that this surge of public interest would captivate CSCW researchers as well; for it is of course quite interesting to sociologists and psychologists alike when a mass audience discovers and appropriates a technology, especially one that enables people to experience new forms of intercourse and interaction. However, the focus of this line of research is on the new forms of social life and human interaction these resources are reported to afford and engender. For CSCW, this represents a major problem shift in as much as the commitment to development of technology is receding. By focusing on the presumptive 'effects' of the various 'social' software and media designs, or on the new forms of interaction they afford, this line of research has, for all practical purposes, abandoned the ambition of contributing constructively, systematically, or simply accountably to the development of new technology. In short, this research is reactive with respect to technology and design (cf. Schmidt, 2009). In a related development, to some extent overlapping, CSCW's focus on 'work' is increasingly considered a historical relic of no great importance (Crabtree et al., 2005). On this view, it is considered of little or no import whether the envisioned 'collaboration technologies' are used for ordinary work in hospitals and factories or for games and gossip (for a critique, cf. Schmidt, 2010).

[1]For an impression of the rich variety of collaborative computing technologies that are currently being investigated, cf. the recent conferences on Computer Supported Cooperative Work in Design (e.g., Shen et al., 2008). (Cf. also: http://www.cscwd.org/).

[2]These research programs have been outlined by the Atkins committee under the US National Science Foundation (Atkins et al., 2003) and by the e-Infrastructure Working Group under the Office for Science and Innovation in the UK (2004). These new application areas are already the object of intense interest in CSCW (cf., e.g., Jirotka et al., 2006; Lee et al., 2010).

[3]There is a considerable literature on 'ontologies'. For an overview, cf. Gruber's short article (2009). For initial studies of the actual construction of computational 'ontologies' by members of scientific communities cf. the studies by Dave Randall, Wes Sharrock and their colleagues (Randall et al., 2007b).

The field is in flux. There is confusion as to the direction of the field. There is even confusion as to what constitutes an interesting and relevant problem or a valid contribution. The field is becoming fragmented. It is time to reappraise the field. It's time to make sail.

The objective cannot be to restate or rephrase the traditional definitions of CSCW, for the fragmentation is a clear indication that the received understandings of the field's program and its conceptual foundation are now deficient. We need to reconsider CSCW's research program.

It is customary to say that CSCW emerged in the late 1980s. And it is of course a fact that CSCW as an institutionalized research field was formed at this time. And indeed, this was no accident. CSCW formed in response to the possibilities and challenges posed by the then novel network technologies as represented by the Internet. All this is true and important. However, the problem with this way of framing CSCW historically is that it elides the central role that the challenges facing cooperative work practices have played in the development of computing technologies in general. It tempts us to think of CSCW as a field concerned with the application of an already existing technology and with the effects of that.

The coupling of computing technologies with cooperative work is not a historical accident, nor is it, for that matter, a recent event. The challenges of enabling the actual formation of cooperative work relations in the first place, or of reducing the complexity of coordinating cooperative work, or even of eliminating the overhead cost of coordination by making cooperative work superfluous by automating the work process—have been significant motivating forces in the development of machine technologies in general and of computing technologies in particular. It is therefore quite appropriate to start there.

'Collaborative computing', understood broadly and informally as computing technologies that facilitate, mediate, or support collaboration in general and cooperative work in particular, is not a yesterday's child. Understood this broadly, 'collaborative computing' is about as old as travelling by jet airplane. However, continuities notwithstanding, 'collaborative computing' has undergone major shifts of technological paradigm. And for the purpose of reconsidering CSCW's research program, identifying and articulating these paradigm shifts are a crucial first step. The aim of this chapter is to do just that. In fact, the bulk of the chapter is devoted to an outline of the major shifts in the *prehistory and emergence* of computing technologies, from the development of the concept and techniques of systematic division of labor in the eighteenth century to the development of the technologies of mechanical control in the Industrial Revolution, to the development of the 'universal' stored-program computer in the middle of the twentieth century as a generic control mechanism, to the development of real-time computing during the Cold War as a means of facilitating large-scale cooperative work.

This rather extensive discussion of technological developments and shifts that predate CSCW by decades and even centuries will show that CSCW, with benefit, can be seen an endeavor in continuation of the long tradition of development of work organization and technology. More than that, the outline of technological development will sketch the background against which the practice-oriented research program of CSCW is to be understood: computing technologies offer unparalleled means of constructing machine systems that facilitate, mediate, and regulate cooperative work—unparalleled, not just in terms of speed of operation but also, and most importantly, in terms of operational flexibility and cost of construction and modification of designs. This has, in turn, made it technically and economically realistic to develop types of machine system, *coordination technologies*, that support cooperative work by regulating interdependent activities (workflow systems, etc.) but would have been practically impossible until about 20 years ago. Seen in this light, CSCW is not a *avant-gardist* fancy but, rather, a research effort that arises from practical concerns of ordinary work organizations.

Having recounted the prehistory and formation of CSCW, I turn to the development of the different research programs in CSCW. I will first recount the story of the development, prior to the official inauguration of CSCW, of research programs concerned with 'computer-mediated communication' and 'office automation'. By the time when CSCW formed as an institutionalized research field, these research programs had landed in a situation where they were seen as inherently flawed. Key researchers in the constituent communities realized that these research programs, in different ways, were based on assumptions about cooperative work practices that were fundamentally problematic: both conceptually and methodologically. This prompted what the philosopher of science Imre Lakatos (1970) has termed a 'problem shift'. A research program was developed and articulated in which ethnographic and other kinds of in-depth workplace studies would play a key role in developing computing technologies that are adequate for the task of 'supporting' cooperative work by uncovering and conceptualizing the logics of cooperative work practices.

After having outlined the main stages of this development, I will move on to discuss the accomplishments of this practice-oriented program: or rather, its patchy and tentative achievements. I will here focus on the complicated and apparently puzzling relationship between ethnography and the development of technology; the reason being that much of the confusion about CSCW's program may be due to simplistic notions about the relationship between ethnography and development of technology in CSCW.

My aim in all this is not to write a *history* of CSCW but to outline the space CSCW occupies in the greater scheme of things.

1 Cornerstones: The Concepts of 'Practice' and 'Technology'

Concepts are institutions. They change over time; not by fiat but as the accumulated result of their distributed use—sometimes coinciding, sometimes contradictory—in the normative activities of people. In the words of John Austin, 'Our common stock of words embodies all the distinctions men have found worth drawing, and the connexions they have found worth marking, in the life-times of many generations' (Austin, 1961, p. 130). The concepts of 'practice' and 'technology' come with a suite of connotations and references, indeed a load of baggage, that we can only ignore at our peril, for then we do not know what we are in fact saying.

That is, we first of all have to briefly clarify the concepts of 'technology' and 'technology development' and their relationship to concepts such as 'artifact', 'system', and 'design'—and how these concepts are related to the concept of 'practice'.

In these matters, gross simplifications abound. While 'technology' to the practicing engineer is a complex of principles, models, concepts, and while it to the anthropologist is the material aspect of a culture, it is, to the sales manager of an electronics store, the gadgets on the shelves. This is confusing enough but can, with a little care, be handled as an instance of peaceful coexistence of diverging concepts. But with the notion of 'technology development' the situation is one of outright strife. For some, technology is simply the practical brother of science, with science providing the theories and models and with technology merely 'applying' the insights of science by 'deriving' technical principles etc. from scientific theories. For others, technology is not only a scientific discipline in its own right but, what's more, the breadwinner of the family of the sciences. And for others again, technology is largely parasitic on the accumulated practical experience of ordinary workers. The first camp will refer to science-based technologies such as semiconductors and pharmaceuticals to justify their claims, while the third camp will refer to the record of several thousand years of history of technological development (agriculture and metallurgy, the wheel and the steam engine). Passions are running high, so high, indeed, that it is tempting to think of it all as simply different groups of social actors clamoring for honor, attention, funding, patent royalties, and a place in the sun. Anyway, in this ideological fog of scientism against romanticism against technocratic exploiters of both, the appreciation of the enormous complexity and variation of technological development is trampled flat. However, over the last few decades historians of technology have managed to develop a quite differentiated picture. Since CSCW has had its share of simplistic notions about technology development, an understanding of the complexities and variability of technology development will be essential for the purpose of reconsidering CSCW as a technological research area and of identifying the technical paradigms and program shifts that has characterized its course so far.

With the concept of 'technology' we are fortunate in that the pedigree is fairly well documented. It originates as part and parcel of the 'practice turn' initiated by the intellectual movement of the seventeenth and eighteenth centuries that is generally referred to as the Enlightenment. The concept of 'practice', by contrast, was developed in the course of life-times of very many generations and has grown out of a tradition that goes back to early thinking about *work,* the *nature of work,* and the *role of work* in human life and in the very concept of humanity, and, as such, it is a tradition that has followed a course determined by both practical and intellectual concerns. And, indeed, the modern concept of 'practice' was developed in intimate relationship with the development of the concept of 'technology'.

The concept of *praxis* (or practice) derives from antique Greek thinking of the fourth century BCE. Both Plato and Aristotle made a distinction between *theoria* (contemplative activity), *poesis* (making, production), and *praxis* (mere activity). While the distinction between *theoria* and *praxis* does not seem alien to modern readers, the one between *poesis* and *praxis* certainly seems odd. The distinction reflects the extremely sharp class divisions that characterized Greek society of that time.[4]

In his *Nicomachean Ethics* Aristotle stated that the concepts of 'making [*poesis*] and acting [*praxis*] are different', pointing out that different kinds of reasoning are involved: 'the reasoned state of capacity to act is different from the reasoned state of capacity to make.' He was also careful to point out that *praxis* is not subsumed under *poesis*; they are of different kinds: 'they are not included one in the other; for neither is acting making nor is making acting.' On the other hand, *poesis* involves art (*techne*): 'art is identical with a state of capacity to make, involving a true course of reasoning' (*Nic. Ethics*, 1140a).

Now, in contrast to Plato, Aristotle did not belittle experience. It is, Aristotle remarked, '*through* experience' that 'science and art come to men'. This is achieved by abstraction: 'art arises when from many notions gained by experience one universal judgement about a class of objects is produced' (*Metaphysics*, 981a). In the same vein, he recognized the importance of experience in an ordinary line of action such as treating a patient:

> With a view to action experience seems in no respect inferior to art, and men of experience succeed even better than those who have theory without experience. (The reason is that experience is knowledge of individuals, art of universals, and actions [*praxis*] and productions [*poesis*] are all concerned with the individual [. . .]). (*Metaphysics*, 981a)

In the art of medicine, that is, a man that has 'theory without experience' is likely 'to fail to cure', because he does not know the individual or particular instance included in the universal, that is, because he is unlikely to be able to deal with the contingencies of the particular case.

However, torn between, on one hand, a striving for knowledge of the divine and universal 'forms' of which particular things are but instantiations, and on the other hand a redeeming curiosity in the rich multiplicity of the world of experience, that is, trapped in an insurmountable dichotomy of the universal and the particular, Aristotle's thinking exhibited great strain. So, having acknowledged the importance of experience for useful mundane purposes, he went on to apply the same dichotomy of the universal and the particular to his analysis of the role of experience:

> But yet we think that *knowledge* and *understanding* belong to art rather than to experience, and we suppose artists to be wiser than men of experience [. . .]; and this because the former know the cause, but the latter do not. For men of experience know that the thing is so, but do not know why, while the others know the 'why' and the cause. Hence we think also that the master–workers in each craft are more honourable and know in a truer sense and are wiser than the manual workers, because they know the causes of the things that are

[4]The authority on the question of slavery in ancient Greek society is Finley (Finley, 1959, 1973). For an updated synthesis, cf. Garlan's study (1988). For Aristotle's view on slavery more specifically, cf. the penetrating study by Garnsey (1996). Wiedemann (1981) has produced a comprehensive collection of antique texts on slavery.—My understanding of the relationship between *theoria* and *praxis* in Aristotle in indebted to Farrington (1944/49), Redlow (1966), and Bien (1968–1969, 1989).

> done (we think the manual workers are like certain lifeless things which act indeed, but act without knowing what they do, as fire burns, – but while the lifeless things perform each of their functions by a natural tendency, the labourers perform them through habit); thus we view them as being wiser not in virtue of being able to act, but of having the theory for themselves and knowing the causes. (*Metaphysics*, 981a–b)

The, for us remarkable, statement that manual workers are 'like certain lifeless things' that 'act without knowing what they do, as fire burns' is not accidental, a slip of the pen; it expresses a view with deep roots in Greek thinking, especially in the thinking of Plato and Aristotle and their schools. And it is this contempt in which they held ordinary manual work, the work of carpenters and shoemakers and ploughmen, that underlies their distinction of *poesis* and *praxis*. The slave is, Aristotle said, 'the minister of action' (*Politics*, 1254a).

The underlying rationale for this categorization was first of all that the work of slaves and work that could be performed by slaves, in short, *manual work*, consists in bodily activities to meet bodily needs, and that whatever activity involves a significant element of bodily action, action on the condition of the material world, is slave work, *praxis*. His ordering can be conceived of as a scale of relations of superiority and inferiority: 'the rule of the soul over the body, and of the mind and the rational element over the passionate, is natural and expedient', and the same applies to men and animals, and the male and the female: 'the one rules, and the other is ruled; this principle, of necessity, extends to all mankind'. He then went on to considering the ordering of work activities:

> Where then there is such a difference as that between soul and body, or between men and animals (as in the case of those whose business is to use their body, and who can do nothing better), the lower sort are by nature slaves, and it is better for them as for all inferiors that they should be under the rule of a master. [. . .] Whereas the lower animals cannot even apprehend a principle; they obey their instincts. And indeed the use made of slaves and of tame animals is not very different; for both with their bodies minister to the needs of life. (*Politics*, 1254b)

Aristotle extended this to include any action that serves to satisfy a need, be it by producing necessities of life or pleasure, as opposed to knowledge that does not 'aim at giving pleasure or the necessities of life':

> At first he who invented any art that went beyond the common perceptions of man was naturally admired by men, not only because there was something useful in his inventions, but because he was thought wise and superior to the rest. But as more arts were invented, and some were directed to the necessities of life, others to its recreation, the inventors of the latter were always regarded as wiser than the inventors of the former, because their branches of knowledge did not aim at utility. Hence when all such inventions were already established, the sciences which do not aim at giving pleasure or the necessities of life were discovered, and first in the places where men first began to have leisure. This is why the mathematical arts were founded in Egypt; for there the priestly caste was allowed to be at leisure. (*Metaphysics*, 981b)

That is, the criterion of Aristotle's ranking of activities is the inverse of utility. Inventions aiming a giving pleasure are in turn topped by 'the sciences which do not aim at giving pleasure or at the necessities of life', science pursued in order to simply know. This ranking of activity and knowledge reflects two related circumstances.

Aristotle took for granted that the arts that 'aim at giving pleasure or the necessities of life', had already completed their task. The very idea of technical development beyond what had already been accomplished, and hence the notion of building theoretical development upon practical experience, was alien to this view. This view, in turn, is intimately related to his political philosophy and his effort to perpetuate the system of slavery: 'as the man is free [. . .] who exists for his own sake and not for another's', that is, the master as opposed to the slave, 'so we pursue this as the only free science, for it alone exists for its own sake' (*Metaphysics*, 982b).

Finally, as a third criterion for the ranking, Aristotle mobilized the intellectual incapacity of the slave: 'in general it is a sign of the man who knows, that he can teach, and therefore we think art more truly knowledge than experience is; for artists can teach, and men of mere experience cannot' (*Metaphysics*, 981b). Manual workers, 'men of mere experience', rank low, in Aristotle's view, not because their *work* is deficient in some way (what they do is obviously generally successful: 'men of experience succeed even better than those who have theory without experience'), but because they cannot *explain* what they are doing in terms of 'first causes and principles'.

In sum: 'the man of experience is thought to be wiser than the possessors of any sense-perception whatever, the artist wiser than the men of experience, the master–worker than the mechanic, and the theoretical kinds of knowledge to be more of the nature of Wisdom than the productive.' (*Metaphysics*, 981b–982a). And 'the slave has no deliberative faculty at all' (*Politics*, 1260a). The distinction between *theoria*, *poesis*, and *praxis* is an expression of this ranking scheme.

Where Plato and Aristotle (and the generations of Christian scholastics and theologians who followed in their footsteps) praised *bios theoretikos*, the renaissance thinkers of the new and rapidly expanding world of bourgeois society had an entirely different agenda. They certainly did not subscribe to the idea that technical knowledge had achieved what could be achieved. They had *things to do* in this world. Frances Bacon, for example, to take perhaps the clearest voice among them, rejected the notion, received from 'the ancients', that anything useful at all could be accomplished when men, in 'mad effort and useless combination of forces' 'endeavor by logic (which may be considered as a kind of athletic art) to strengthen the sinews of the understanding' (Bacon, 1620, Preface). Thus, in explicit contradiction of Plato and Aristotle, Bacon argued that theory and practice are equals, so to speak, and was thereby able to even conceive of *theorizing proved wrong in practice*: 'sciences fair perhaps in theory, but in practice inefficient' (Bacon, 1620, §II:xlv):

> Although the roads to human power and to human knowledge lie close together and are nearly the same, nevertheless, on account of the pernicious and inveterate habit of dwelling on abstractions it is safer to begin and raise the sciences from those foundations which have relation to practice, and to let the active part itself be as the seal which prints and determines the contemplative counterpart. (Bacon, 1620, §II:iv)

On this view, ordinary working practices and practical knowledge were no longer categorially separated from scientific knowledge. It was conceivable to 'begin and raise the sciences from those foundations which have relation to practice'.

However, Bacon's 'practice turn' was of course largely programmatic. Production was craft-based and science immature: Galilei had just started his career when Bacon published his *Novum Organon*.

However, a century or so later, when Denis Diderot, together with d'Alembert, edited the famous *Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers* (1751–1766), the relationship between science and practice that Bacon had vaguely sensed and promulgated was becoming reality. Diderot thus wrote an article on 'Arts', i.e., the *practical* crafts, arts, techniques, and sciences, for the first volume of the *Encyclopédie* in which he, following Bacon, flatly observed that 'It is man's work [l'industrie de l'homme] applied to the products of nature', his effort to satisfy 'his needs', 'that has given birth to the sciences and the arts' (Diderot, 1751, pp. 265 f.). He then went on to describe the relation between 'theory' and 'practice' as a reciprocal one:

> every art has its speculation and its practice: the speculation is nothing but the idle knowledge of the rules of the art, the practical aspect is the habitual and unreflective application of the same rules. It is difficult, if not impossible, to develop the practice without speculation, and, reciprocally, to have a solid grasp of the speculation without the practice. There are in every art with respect to the material, the instruments, and the operation a multitude of circumstances which can only be learned in practice [usage]. It is for practice to present difficulties and pose phenomena, while it is for speculation to explain the phenomena and dissolve the difficulties; from which follows that hardly any but an artisan who masters reasoning that can talk well about his art. (Diderot, 1751, p. 266, emphases deleted)

To illustrate his argument, Diderot discussed the relationship between academic geometry and the practical geometry as exercised in workshops:

> Everyone will readily agree that there are few artists who can dispense with the elements of mathematics. However, a paradox, the truth of which is not immediately obvious, is that, in many situations, these elements would actually harm them if the precepts were not corrected in practice by knowledge of a multitude of physical circumstances: knowledge of location, position, irregular forms, materials and their properties, elasticity, rigidity, friction, texture, durability, as well as the effects of air, water, cold, heat, dryness, etc. (Diderot, 1751, p. 271)

He went on to argue that, for instance, no levers exist 'for which one could calculate all conditions'. Among these conditions are a large number that are very important in practice:

> From this follows that a man who knows only intellectual [academic] geometry is usually rather incompetent and that an artist who knows only experimental geometry is very limited as a worker. But, in my opinion, experience shows us that it is easier for an artist to dispense with intellectual geometry than for any man to dispense with some experimental geometry. In spite of the calculus, the entire issue of friction has remained a matter for experimental and handicraft mathematics. [...] How many awful machines are not proposed daily by men who have deluded themselves that levers, wheels, pulleys, and cables perform in a machine as they do on paper and who have never taken part in manual work, and thus who never have known the difference in effect of one and the same machine in reality and as a plan?. (Diderot, 1751, p. 271)

In other words, following Bacon, Diderot completely reversed the internal relationship of Aristotelian concept-pair *theoria* / *praxis*. When we talk of 'practice' we no longer conceive of it as mere regular activity devoid of 'reasoning' and 'deliberation'. The notional separation of *praxis* and *poesis* has been dissolved, and both the 'capacity of make' and the 'capacity to act' have been united in the modern concept of practice—united but not conflated. The modern concept of 'practice' expresses and is used for emphasizing the complex dialectics of general precepts and action.[5]

The aim of Diderot and his fellows was not merely to celebrate of the practices of artisans and handicraft workers, but to find ways to improve received practices. The modern concept of 'practice' was developed as an integral intellectual component of this interventionist endeavor, as a conceptual resource for a movement devoted to understanding and transforming actual productive practices thorough investigation and rationalization. And it was in the nexus defined by the modern conception of practice in its relationship to the concepts of experience, techniques, skills, and knowledge that the concept of technology was developed.

The concept of technology was developed and articulated to express and reflect the effort of developing the practices of the arts through 'speculation', that is, through systematic rationalization of the techniques applied in those practice. In 1675 the French minister of finance Jean-Baptiste Colbert, in his persistent effort to improve the state of the economy and promote the development of manufacturing, invited the *Académie royale des sciences* (founded in 1666) to produce comprehensive and detailed descriptions of the 'arts and trades', that is, the manifold techniques applied in the various branches of production: 'The king wished the Academy to work unceasingly upon a treatise on mechanics, in which theory and practice should be explained in clear manner that could be grasped by everyone' (Académie Royale des Sciences, 1729–1734, p. 131). As expressed in the Academy's own *mémoire* from 1699, the Academy 'voluntarily accepted' the assignment of 'describing the crafts in their present condition', knowing full well that the task was 'dry, thorny, and not at all dazzling'. The resulting *Description* was to 'penetrate to the ultimate details, although it would often prove very difficult to acquire them from artisans or to explain them'. Thereby 'an infinity of practices, full of spirit and inventiveness, but generally unknown, will be drawn from their shadows'. In this way the crafts would be preserved for posterity, but in addition 'able men' who lack the leisure to visit the artisans' workshops would be able to 'work on the perfection' of these practices, just as the Academy itself would not fail to remark if something might usefully be amended. (Académie Royale des Sciences, 1702, pp. 117 f).

This was of course not the first attempt to give in-depth accounts of specific work practices based on on-site observations.[6] But with hundreds of 'arts and trades' to

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[5]Kant summarized the modern concept of 'practice' when he in an essay started out by recapitulating: 'One calls a conceptualization of rules, even of practical rules, a *theory* when these rules, as principles, are thought of in a certain generality and thus have been abstracted from a multitude of conditions that nonetheless necessarily influence their application. On the other hand, one does not call just any operation a *praxis*; rather, only such a purposive endeavor is considered a *praxis* that is taken to be attained by following certain generally accepted principles of procedure.' (Kant, 1793, p. 127).

[6]Agricola's description of the metal trades is a case in point (1556).

investigate and describe, the task undertaken by the Academy was of course enormous, as it involved extensive fieldwork in different lines of trade in different parts of the country. What is more, a systematic approach was required and had to be developed. It is not surprising if the research progressed only glacially. Anyway, after a couple of decades, a number of researchers (such as Billettes, Jaugeon, Carré, and others) were assigned to the task and began producing reports to the Academy, to some extent by requesting staff at the provincial *prefectures* to undertake the fieldwork and submit reports based on their observations. Papers were read at the regular meetings of the Academy and then filed. In 1720, the scientist René-Antoine Ferchault de Réaumur was put in charge of the cooperative effort, but by his death in 1757 only a few pieces of the accumulated analyses had been made publicly available. The reason for the lack of obvious progress—apart from the enormity of the task, of course—seems to have been dissatisfaction with the quality of the initial analytical work which was seen as not sufficiently systematic and accurate. In fact, some of the papers read at the Academy were not included in the Academy’s printed proceedings (cf. e.g., Peaucelle and Manin, 2006; Peaucelle, 2007, pp. 140 ff.). However, Duhamel du Monceau, who replaced Réaumur as project manager, succeeded in getting the publication process organized and from 1761 to 1788 altogether 81 treatises (about 100 volumes) were published under the title *Descriptions des arts et métiers*.[7]

The aim of all this, as re-stated in the Academy’s preamble to the *Descriptions*, was not merely to ‘examine and describe in turn all operations of the mechanical arts’ but also and equally ‘to contribute to their progress’. The Academy expected that ‘new degrees of perfection of the arts’ would be achieved when scholars undertake the effort of investigating and developing the ‘often ingenious operations performed by the artisan in his workshop; when they see by themselves the needs of the art, the boundaries at which it stops, the difficulties that prevent it from going further, the assistance that one could transfer from one art to another and which the worker is rarely expected to know.’ Subjecting work practices as they have slowly evolved from ‘obscurity’ to systematic study, rationalizing them, would show the competent worker a way to ‘overcome the obstacles that they have been unable to cross’, a way to ‘invent new tools’, etc. (Académie Royale des Sciences, 1761, pp. xvi f.).

The ‘dry, thorny, and not at all dazzling’ effort of the Academy had huge impact: ‘there can be no doubt’ that contemporaneously these scientific descriptions of arts and handicrafts ‘exerted a potent influence in western Europe’ (Cole and Watts, 1952, p. 1). It first of all provided a reservoir of empirical findings for much of the contents of Diderot and d’Alembert’s highly influential *Encyclopédie*. Although the *Encyclopédie* started appearing a decade ahead of the *Descriptions*, many of the authors who contributed to the *Encyclopédie* had access to the reports of the

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[7] A revised edition, collected in 20 volumes, started appearing in Neuchâtel in Switzerland shortly after (Académie Royale des Sciences, 1771–1783). A German translation, also in 20 volumes, was published from 1762 to 1795 under the title *Schauplatz der Künste und Handwerke* (Cole and Watts, 1952, p. 18).

Academy researchers or were involved in both projects, so that the *Encyclopédie* on many topics anticipated the published contents of the *Descriptions*, but typically in a less detailed and accurate form.[8] From the *Descriptions* and the *Encyclopédie*, the scientific description and reconstruction of the techniques of handicraft production percolated out into French and European economic life, via dictionaries and journals and other forms of popularization.

Furthermore, the *Descriptions* provided a model for scholars that received practices were accessible to scholarly analysis and might be much improved by application of the insights, methods, etc. of the physical, chemical, mechanical, etc. sciences. Such systematic studies of work practices with a view to their rationalization were given the name *technology* by the contemporary German scholar Johann Beckmann (1777).[9] Referring to the *Descriptions des arts et métiers* and similar works as the 'most esteemed general writings on technology' (p. 39), that is, the model of such research, he defined 'technology' as follows:

> Technology is the science of the transformation of materials or the knowledge of handicrafts. Instead of merely instructing workers to follow the master worker's prescriptions and habits in order to fabricate a product, technology provides systematically ordered fundamental directives; how one for exactly these ends can find the means on the basis of true principles and reliable experiences, and how one can explain and exploit the phenomena occurring in the process of fabrication (Beckmann, 1777, p. 19).

Technology, he stated, provides 'complete, systematic, and perspicuous explanations of all works, their outcomes, and their grounds' (p. 20).

Beckmann, in his prolific research, continued the paradigm defined by the Academy in Paris. In fact, he even integrated it in his teaching, in that his students were taken to local workplaces: 'One must have tried, without any preparation or instruction, to get acquainted with factories and manufactories to know how difficult it is' (p. a7). One of these students, Johann Poppe, continued this work and wrote the first systematically organized history of technology.[10] His brief summary of the emergence of technology as a scholarly discipline deserves quoting in this context: 'In the eighteenth century many scholars undertook the arduous task of obtaining a precise understanding of handicraft, manufactures, and factories. Some have even made it into an specific research topic. One had become sufficiently convinced of the benefits that these efforts would have for the manual worker himself and for many managers in public office' (Poppe, 1807, p. 62 f.). Pointing out that

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[8]The competition posed by the *Encyclopédie* may have provoked a 'sense of urgency' at the Academy and encouraged it to speed up it own publication effort (Cole and Watts, 1952, p. 10).

[9]Beckmann (1777, p. 20) mentioned that he suggested the term with some trepidation in 1772, namely, in a review of a book by a French upholsterer on the principles of this trade: 'Technologie oder Kenntnis der Handwerker', or 'technology or knowledge of the craftsman' (1772, p. 309). (On Beckmann, cf. Exner, 1878; Beckert, 1983)

[10]'As a history of technology of substantial scope, Poppe's book remained almost unique for a century and a half, during which nearly everyone forgot that it existed' (Multhauf, 1974, p. 2)—with the notable exception of Karl Marx who immersed himself in this work (Marx, 1861–1863, vols. 3.1 and 3.6).

the 'stipulations and customs' that regulate handicraft 'were often based on deficient or even non-existent principles' and were often influenced by 'strong prejudices', Poppe emphasized the role of *technology*: 'Different scholars that have obtained knowledge of several handicrafts have recently, by virtue of their sciences, already cleared away many things that obstructed the greater perfection of these trades' (ibid., p. 63), and referring explicitly to the *Descriptions*, he stated that 'The meticulous description of the arts and handicrafts that the Academy brought to light belongs to the greatest scholarly works of the eighteenth century. It engendered several works of a similar kind, to some extent very valuable works, not only in French but also in other languages' (ibid., pp. 91 f.).

In short, the concepts of 'technology' and 'practice' were from birth joined at the hips, with technology as a systematic effort to investigate and transform the techniques applied in the practices of the useful arts. Accordingly, technology is traditionally and usefully defined as *rationalized* or *systematic* knowledge of the useful 'arts' or techniques (cf., Layton, 1974). Development of technology, then, is essentially a systematic conceptual endeavor that results in *technical knowledge, methods, principles*, etc. 'Technology' is an ability-word.[11]

The term 'technology' has of course been adopted for other purposes. In anthropology, for example, 'technology' is used as a designation for the ensemble of techniques of a particular collective of people. In short, it is used as a synonym of 'material culture'. Now, there is certainly an important and ineluctable element of craft skill and know-how to the development and application of technology. But then there is most certainly also an important element of know-how to scientific work, as there is to all work, however 'intellectual' it may be. However, the problem with equating technology with technique is that this usage tends to make us blind to those *practices* that are *specific* to science and technology: systematic application of received knowledge (theoretical or empirical), rationalization of principles and methods, rigorous testing and observation, candid reporting among peers, replicability in experimental results, etc. For scientist and technologist alike, 'similar normative imperatives remain: no engineer, anymore than a scientist, can get away with fudged data, obscure concepts, or imprecise, inadequately-described measurements' (Constant, 1984, p. 32). In short, technology, like science, is systematic knowledge. The difference lies in technology's 'emphasis on purpose', as the historian of technology Donald Cardwell puts it (1994, p. 486). The technological artifact is proof that the idea actually works and works as intended.

As noted by the Academy in its preamble, techniques are 'born in obscurity'; they become technology only as a result of systematic analysis and rationalization. And in fact, the great majority of inventions are and have always been what Cardwell calls 'empirical': inventions made 'by arranging familiar components or materials in a novel way and without resort to abstract or scientific thinking' (Cardwell, 1994,

[11] This also means that an account of technological development cannot adequately be done in the form of a list of inventions and artifacts but must be an account of development of technical knowledge, focusing on continuity and diffusion of knowledge, including conceptual fractures and problem shifts. This, of course, also applies to an account of the prehistory and formation of CSCW.

p. 492). Inventions that spring from craft techniques may at a later stage be subjected to reflection and undergo conceptual systematization. In fact, the development of a technology by way of systematization of empirically developed techniques have, in many cases, led to major scientific insights. Thus thermodynamics was, by an large, created by engineers and not by scientist studying heat (Cardwell, 1994). 'Whether one takes steam power, water power, machine tools, clock making or metallurgy, the conclusion is the same. The technology developed without the assistance of scientific theory, a position summed up by the slogan "science owes more to the steam engine than the steam engine owes to science"' (Laudan, 1984, p. 10).

Now, not all technologies develop in this way: born as techniques 'in obscurity', which then evolve through a distributed process of incremental improvements, only to be subjected, eventually, to systematic rationalization and thus turned into a technology. Some technologies are born in the bright light of scientific insights which are then transformed into technologies: the standard example of that is of course the dramatic history of the semiconductor technology on the basis of the theories of quantum mechanics and solid state physics. Still, the transformation of articulated scientific theories and models into workable technologies is not a straightforward process of deduction; it requires a 'separate and additional act of invention' (Cardwell, 1994, p. 491). All kinds of practical issues have to be identified, understood, and resolved in the course of transforming scientific insight into a useful technique. As vividly illustrated by the development of semiconductor technology, theories of quantum mechanics and solid state physics only provided the essential theoretical framework of understanding the observed phenomena. To develop these insight into workable technologies, required decades of innovation work by thousands of technicians.—That is, according to Cardwell:

> We have on the one hand crafts, technics and inventions; on the other hand technology, applied science and inventions. And common to both sides we have innovation, which we interpret as the action needed to put an invention into practice. Generally speaking, we say that inventions related to or springing from technics and crafts do not involve systematic knowledge and are, in a sense, empirical; inventions deriving from technology or applied science involve systematic or scientific knowledge. (Cardwell, 1994, p. 4)

But, as Cardwell then points out, it is, of course, much more complicated than that: 'there is a great deal more to effective innovation than these simple definitions suggest' (ibid.).

CSCW is often loosely described as a field ultimately devoted to the *design* of collaborative *systems*. However, this manner of speaking is misleading, for the term 'systems design' normally refers to the engineering practices of devising a *specific* configuration of existing, typically well-known, technologies (such as software architectures, protocols, modules, interfaces, etc.) so as to meet *specific* requirements. But CSCW is not a specialized branch of practical engineering addressing the specific technical issues of designing, building, introducing, and evaluating 'collaborative' systems for specific settings or types of setting; it is rather a field of research devoted to the *development of technologies* that system designers can apply, together with other technologies, old or new, in building systems for cooperative work settings. The terms 'technology development' and 'systems design' are used for distinctly different types of socio-technical transformation.

The concepts of 'invention' and 'design' are similarly categorially distinct. One could, roughly, say that 'design' is an intention-word, whereas 'invention' is an outcome-word. The concept of 'design' suggests premeditation, forethought in devising a plan, while 'invention' emphasizes the creation of something quite new. We can exhibit *forethought* in devising an artifact when we master the required techniques and know the odds; we can then proceed 'by design'. When it comes to *inventing* something, however, we are, to some extent, in certain critical areas, fumbling, stumbling, searching for solutions. To design an artifact we rely on systematic technical knowledge; without it, we cannot exhibit forethought in devising a plan for its construction.

Now, in a particular design effort there may of course be subordinate elements of invention, just as there typically are subordinate elements of design in any invention. That is, the boundary is blurred in so far as technologies are multi-level complexes. The point is that technological development provides the basis for design work, the general systematic knowledge which is then applied in the particular design solutions, while the incremental improvements represented by successive or competing design solutions in turn contributes to the further development and maturation of the technology.

Technologies are, as a rule, not *designed*; they are developed through a series of inventions and conceptualizations. It is typically an open-ended process with all kinds of false-starts and dead-end paths, with deliberate search as well as serendipitous discoveries, with protracted periods of incremental innovation that may be interrupted by abrupt changes.

The conflation of technology and systems design is rooted in the notion that technology does not fall under the broad category of knowledge but rather is a category of artifact. A candid formulation of this position can be found in George Basalla's *The Evolution of Technology* (1988):

> The artifact – not scientific knowledge, nor the technical community, nor social and economic factors – is central to technology and technological changes. Although science and technology both involve cognitive processes, their end result are not the same. The final product of innovative scientific activity is most likely a written statement, the scientific paper, announcing an experimental finding or new theoretical position. By contrast, the final product of innovative technological activities is typically an addition to the made world: a stone hammer, a clock, an electric motor. [...] The artifact is a product of the human intellect and imagination and, as with any work of art, can never be adequately replaced by a verbal description. (Basalla, 1988, p. 30)

In some respects, Basalla's argument is rather obscure, for instance when he says that 'the artifact [...] can never be adequately replaced by a verbal description': in which sense of the words 'adequately' and 'replace' could a 'verbal description' possibly 'replace' 'the artifact'? Is this even a meaningful proposition? What seems to be said here is anyway oddly fetishistic, in that it hinges on the implicit assumption that the product of scientific or technological work somehow *speaks for itself*. Basalla's central claim here is of course that what is essential to a technology is somehow embodied in the thing itself; that being a hammer, a clock, an electric motor is somehow an intrinsic material property of the thing. But technological artifacts that are not integral to a living practice are merely a heap of junk. Or perhaps they are on exhibit in a museum as a representation of a past technology the use of which may now be unknown. In fact, it so happens that modern archeology has had to develop experimental methods in order to understand the 'real-life processes' through which the physical remnants were produced. For example, the experimental archeologist Nicholas Toth has reconstructed the seemingly simple task of fabricating stone hammers similar to those produced by hominids living in East Africa between 2.4 and 1.5 million years ago. The research effort was extensive and exhausting, but the conclusion clear: 'Although the *products* of Oldowan technology are quite simple, the *processes* required in the hominid mind to produce these forms show a degree of complexity and sophistication: in other words, *skill*' (Schick and Toth, 1993, pp. 133 f.). In sum, 'Technology is something much larger than the tool itself. It refers to the system of rules and procedures prescribing how tools are made and used' (ibid., p. 49). Or in the words of the historian Rachel Laudan, the 'mute presence of the remaining artifacts does not speak for itself', and this is the obvious reason why 'technological knowledge can easily be lost' (Laudan, 1984, p. 7).

I quote Basalla because his position, though deeply problematic, has been influential and is widely cited. Moreover, Basalla's claim has been mobilized in the general area of Human-Computer Interaction (HCI) in an attempt to understand the apparently surprising, and to some unsettling, observation that the technologies of interactive computing were developed well in advance of any formulated theory of interactive computing and that the technical solutions were not 'deduced' from psychological theory. In 1991 John Carroll thus noted that 'some of the most seminal and momentous user interface design work of the last 25 years' such as Sutherland's Sketchpad and Engelbart's NLS in the 1960s 'made no explicit use of psychology at all' (Carroll, 1991, p. 1), and that 'the original direct manipulation interfaces' as represented by the Xerox Alto (released in 1973) and the Xerox Star (1981) 'owed little or nothing to scientific deduction, since in fact the impact went the other way round' (Carroll et al., 1991, p. 79). These observations are well founded. More than that, when Carroll and his colleagues then argued that it was not embarrassing but quite legitimate for HCI to engage in *post hoc* 'scientific investigations' of the techniques of interactive computing that had developed incrementally through 'emulation of prior art' (ibid., p. 75), this was again quite justified, for this has been and remains the normal form of technology development.

However, Carroll and his colleagues then slips into conceiving of technical artifacts in the fetishistic manner propounded by Basalla: 'it seems typical in HCI for new ideas to be first codified in exemplary artifacts and only later abstracted into discursive descriptions and principles' (Carroll et al., 1991, p. 79). The problem with this interpretation lies in the very notion that 'ideas' are somehow 'codified' 'in' 'artifacts'. *Codified*? As pointed out by the molecular biologist Jacques Monod years ago, there is nothing *in* the form, structure, behavior of an object that makes it an artifact (Monod, 1970). What makes the thing an artifact is the practices to which it belongs, the practices for which it has been designed and built, for which it has been appropriated, and in which it is used on a regular basis, and one can only ‘decode’ the artifact if one knows and understand these practices (cf. Bannon and Bødker, 1991). After all, ‘reverse engineering’ requires the same general competencies as the engineering of a device. That is, the problem here is that, in focusing on the artifact itself, the practices as part of which the techniques developed and in which they have become integrated have been effectively expunged from consciousness.

In short, technology cannot be reduced the technical artifact although the artifact, of course, plays a pivotal role in the demonstration and application of the technology. ‘Technology’ is an ability-word.

To complicate matters even more, immensely more, technical knowledge has what one could call systemic character. The development of a technology will depend upon received knowledge (scientific or practical) that, although it does not provide a theory of the technology under development, is nevertheless indispensable. Thus even in the case of a technical development as conspicuously craft-based as the development of the steam engine, the mechanists who developed it would have been at a complete loss without their mastery of geometry and arithmetic and techniques of measurement. More than that, a technology typically comprises a set of what could be termed constituent technologies, that is, technologies that have been adopted, reconfigured, and put to novel uses as components of the new technology. Thus distinctly different technologies, each of which may have evolved incrementally in a distributed way, may deliberately be ‘shifted laterally’, from their context of origin, to become part of or be combined in a novel technology. A case in point is the railroad. When, in 1829, the Stephensons demonstrated their prototype steam locomotive, the *Rocket*, and won the competition,

> the individual components of the railroad, considered as a system, had been assembled over the previous fifty years or so. The canal builders had mastered the techniques of drilling tunnels. building up embankments and digging out cuttings. They had established the legal precedents for compulsory purchase of land and they had learned how to muster and manage large bodies of skilled and unskilled men. The millwrights, of the mining industry had invented and developed the locomotive. The stage coach system had popularized the idea of public passenger transport at so much per mile and the discipline of the timetable. These components had been progressively refined. What was required was the ability to put them all together and weld them into the public railroad so very different from the little mine trackways with their rough, primitive steam locomotives, “iron horses”. The Stephensons came to realize that the railroad could extend to working people a luxury that only the affluent had been able to afford. Their vision of a rail network to provide ordinary folk with a cheap, fast. safe and comfortable means of travelling wherever they wanted to go amounted, when combined with their practical abilities to bring it about, to the achievements of genius. (Cardwell, 1994, p. 234 f.)

Not only were received technologies appropriated and combined in the formation of railroad technology but the accumulated experience with railroad technology prompted the deliberate development of other technologies. When railroad networks began to operate and spread, issues of coordination began to emerge and be understood. The operation of geographically dispersed but interdependent and temporally critical activities, as those of a railway service, requires communication facilities that operate at a much higher speed than the train service itself. Thus the 'rapid spread of railroads heightened the demand for the fastest means of communication along the lines' (Cardwell, 1994, p. 251). This practical need, to a large degree, then drove the development of the technologies of the electric telegraph. The discovery of the electromagnetic effect (by H. C. Ørsted in 1820) was by then already being applied experimentally to communication over distances of several kilometers (for research purposes) but the coordination needs of the new railroad operations in Britain soon became a major motivation for developing this technology. It was tried successfully in 1837–1838, and from 1842 practical implementation got underway. By 1848, operations of about half of the British railroad lines were coordinated by telegraph (Standage, 1998, p. 61).

Now, with expanding networks of rail services, coordinated by means of telegraph services, another 'bottleneck' emerged that required additional technological developments. The coach lines had been operating in an environment with a multitude of local times as determined by longitude, and the railroad companies initially tried to continue the practice of the coach lines. But, as the historian of technology David Landes puts it, 'trains moved too fast for this, continually exposing passengers and crew to discrepancies and confusions'. The first step was to use the telegraph 'to transmit almost instantaneously an exact hour and minute from the central office to every point on the line'. This established 'a standard time for all those served by a given network', but this of course caused coordination problems across railway networks. The next step was therefore to unify local railway practices by agreeing to adopt the local time at the Greenwich observatory as a national standard. This was done by the end of 1847. But, as Landes comments, 'The effectiveness of the change depended, of course, on the creation of a national time service, communicating precise time signals at regular intervals to clocks and stations around the country.' (Landes, 1983, pp. 285 f.). This was, in turn, made possible by the development of the 'use of galvanism', i.e., telegraphic signals, to synchronize timekeepers in different locations and, indeed, in the words of the Astronomer Royal, Airy: 'the extensive dissemination throughout the Kingdom of accurate time-signals, moved by an original clock at the Royal Observatory' (quoted in Howse, 1980, p. 95). Implementation of this new technique of automatic synchronization was begun on the South Eastern Railway in September 1851 and was completed by August the next year. It was in the following years extended to post and telegraph offices, town hall clocks, workshops of chronometer makers, and to factory clocks (ibid., pp. 96–105).

The case of railroad technology illustrates the systemic nature of technology. A given technology presumes a—sometimes vast—network of other technologies that serve as component technologies or as part of the technical platform for the one under consideration. Sometimes the need of such auxiliary or component technologies is only understood as the new technology gets under way, problems are identified, and practitioners begin to search for solutions. And sometimes the relationship between two otherwise different technologies is such that one can conceive of their development as something akin to co-evolution: the further development of one technology depends on the progress of the other, and the advances in one technology may be held up for a long time until another technology has achieved a certain level of maturity (stability, performance/cost ratio, etc.).

The systemic character of technology is a key to understanding the dramatic technological changes that sometimes occur. A new technology may be developed for certain purposes and it may then be realized by researchers or engineers that it can substitute a quite different component technology in an otherwise unrelated technological complex—and it may turn out that the substitute offers solutions to known problems or even to bottlenecks or limitations that had not yet been perceived. Sometimes the component technologies has been developed for other purposes but are then shifted laterally and combined in innovative ways to form a new technology (in the case of railroads: rails, heat engines, machine tools, time tables, etc.).

The systemic character of technology has important methodological implications, in that the challenge of uncovering and grasping ‘the internal development of technology’ (Laudan, 1984) should be an overriding concern of any effort to understand the development of technologies—be it railways or CSCW. That is, it is essential to conceive of technology as systemic and hence of the research and engineering efforts of developing technical knowledge as (loosely) coherent or (tentatively) converging ‘paradigms’ (Constant, 1984; Laudan, 1984; Hughes, 1987).

Before I move on, I should point out that the Kuhnian notion of ‘paradigm’ should not to be taken as equivalent to the notion of ‘theory’ but as a set of ‘universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners’ (Kuhn, 1962, p. x), that is, a complex of notions of what constitutes a researchable, relevant, and interesting problem as well as notions of what constitutes findings and solutions; measurement standards, protocols, procedures, methods, instruments, equipment; forms of apposite argument and conceptualization as well as genres of presentation of data and findings; as well as the institutionalized forms of these notions, standards, and forms: channels, policies, and procedures of publication, review, etc.; research review boards, schemes, etc.; educational institutions, textbooks, professional organizations, and so on—all centered on and incarnated in those contributions that are considered exemplary, the ‘paradigm’ (Sharrock and Read, 2002). The development of technical knowledge exhibits similar characteristics. The most important difference is, again, the ‘emphasis on purpose’ in technological research, which, in practice, is bound up with the concerns for usefulness, costs, stability, performance, maintenance, etc.

There is an inherent risk in focusing on the ‘internal development of technology’, however. The concept of technology may then simply be confounded with that of engineering, and particular technologies may become categorized according to certain components technologies. This has as happened rather frequently in the historiography of computing, when ‘computing’ as a technology is dated from the Colossus (1943) and the ENIAC (1946) because they were the first electronic digital computers or the Manchester ‘Baby’ (1948) because it was the first stored-program digital computer. They were all important steps and their design embodied technologies that have later been found important. But technology is nothing if it is not ‘a useful art’. The relationship between technology and practice is internal, that is, *conceptual*, like ‘figure’ and ‘ground’: you can’t have one without the other.

A given technology is only a technology in relation to its application context and we can only compare and identify paradigmatic shifts in relation to that specific application context.

Does this mean, then, that the 'internal development of technology' is a chimera? Not at all. In fact, technological development is often or, indeed, typically driven by technologists' addressing known or anticipated internal 'anomalies' in the established technology, trying to overcome instabilities and performance limitations, reduce energy consumption, increase reliability, etc. The paradox dissolves as soon as we remember that technologies are systemic and that the immediate target of component technologies, perhaps technologies way down in the hierarchy (e.g., in the case of the computer technologies assembled in my laptop computer, technologies such memory circuits, screen drivers, compilers, parsing algorithms, etc.), is not the practice of the so-called 'end user' but the practice of the engineers who design and build specific laptop products (as configurations of component technologies).

The point of these brief remarks on the systemic nature of technology is that, what we, in the context of CSCW, *focus* on are computing technologies *at the 'level' of cooperative work practices*. This does not just apply to the history of technology but equally to our understanding of the processes of technological development in which CSCW is engaged.

2 Computing Technologies and Cooperative Work

> By thinking about the history of "technology-in-use" a radically different picture of technology, and indeed of invention and innovation, becomes possible. A whole invisible world of technologies appears.
> Edgerton (2006, p. xi)

From its very beginning, digital computing technology has been applied to cooperative work. Or rather, computing technology was not simply *applied* to cooperative work; the effort to meet various challenges of cooperative work has had significant impact on the development of the concept of computing. Addressing challenges of cooperative work has played a central role in developing 'the computer' into what we know today.

I will briefly recount the major technological paradigm shifts in ordinary work settings. The point of departure is the development, prior to the industrial revolution, of systematic cooperative forms of work based on advanced division of labor that, in turn, was premised on having developed and mastered techniques of decomposition and recomposition of work processes However, this form of cooperative work had quite limited development potentials in terms of productivity, labor shortages, etc., and the development of machine technology can be seen as a means to overcome these restraints. And again, while the emergence of machinery at the point of production in the form of 'machine tools' and eventually 'machine systems' and concomitant forms of cooperative work relations showed enormous development potential, it too, eventually, had limitations rooted in the costs of constructing and modifying mechanical means of operational control. For that reason, mechanical machine technologies were, largely, confined to areas of mass production where the construction costs could be offset by high volumes. However, computing technologies, which were initially developed as a means of overcoming the bottleneck of increasingly complex cooperative forms of calculation work for scientific and engineering purposes, provided the technical basis for constructing and modifying control mechanisms at costs that were extremely low and that have, in fact, now been continually falling for half a century. The costs of constructing machine systems have in fact become so insignificant that relatively flexible machine systems for vast and varying arrangements of cooperative work have become economically viable. This is where the CSCW research program arises, not just as an academic pastime, but as a practical problem for modern industrial civilization.

2.1 Division of Labor: Progressive Forms of Work Organization

The development of computing can only be understood when seen as an integral aspect of the development of work practices and their technologies over the last three centuries.

To make this claim understandable it is useful to briefly introduce the taxonomy of work organization that has been in widespread use in economic and technological historiography: (i) 'craft work' or domestic handicraft, (ii) the 'putting out' system or the *Verlagssystem*, (iii) the 'manufactures', and (iv) the machine-based 'factory' (e.g., Sombart, 1916; Braudel, 1979). The key distinguishing feature among these forms is the different arrangements of division of labor; although the machine-based 'factory' should be seen as a form of work organization in which the systematic division of labor in production is transcended, if only tentatively.

A note of caution, before we move on. These progressive forms of work organization should not be thought of as *stages*, each stage leading inexorably to the next as a necessary step in the development of the work organization. The picture is far more complex than that. Firstly, technological development is 'uneven': different forms of work organization coexist across branches of production, across geographic regions, even within the production of the same product. Work organization based on handicraft work continues in small scale production and is indeed reinvented time and again when novel kinds of products are first produced. One can for example observe the same forms at play in the history of the electronic and computer industries. In fact, in the modern automotive industry some components may be produced in domestic handicraft settings, others by advanced machinery, while the ultimate product is assembled in a work organization based on systematic specialization of handicraft. Secondly, retrograde development may take place, so that technologically backward forms become competitive again and gain a new lease of life when, for instance, large reservoirs of dispossessed peasants or underemployed workers in rural areas are created or become available or when dramatic technological changes put working conditions under pressure. That is, to repeat, the forms of work organization that we observe are not *stages*. Nor are they to be thought of as 'models' or 'ideal types'. They are rather like recurring themes that are played again and again but in varying ways and contexts, often in the same sequence but sometimes not. So, instead of conceiving of these forms as stages, one might use a term such as progressive forms of work organization, if we let the term ‘progressive forms’ refer to two important features: On one hand, they represent a (precariously) cumulative process of development of organizational and technological *knowledge*. On the other hand, this cumulative effect is, of course, strengthened by the totalizing effect of the *market*. In so far as practical applications of this knowledge turns out to be successful in some recognized way (profitable, dependable, viable), the specific form of work organization feeds back as a competitive challenge to other actors in the market.

Handicraft: In craft work, each worker typically masters the entire range of tasks involved in fabricating the ultimate product. In its simplest and most widespread form, craft work was an indispensable aspect of the subsistence economy of traditional rural life (e.g., spinning and weaving). These activities were performed by peasants as a sideline, that is, in periods or at times when agricultural tasks did not fully occupy their capacity. The unit of production was the individual peasant household, with division of labor, if any, based on age and gender. In each of these ‘monocellular’ units, tasks were ‘undifferentiated and continuous’ (Braudel, 1979, p. 298). On the other hand, in domains of work that demanded a degree of skill that in turn required full-time engagement with the work (e.g., blacksmith, cutler, nailmaker, shoemaker, etc.), the work would not be carried out as a secondary occupation; specialization would be required. However, this type of craft work would be organized in a way rather similar to the sideline work in peasant households: as tiny family workshops, each with a master tradesman, two-three journeymen, and one or two apprentices. In both types, adult workers would possess the skills to undertake the entire range of tasks, and any division of labor between them would be occasioned and transient.

The ‘putting out’ system: As an integral part of the development of the world market, especially from the sixteenth century, this ‘horde of little establishments, where family craft working was carried on’ (Braudel, 1979, p. 298), gradually became geared to the market and subsumed under new and systematic schemes of division of labor. Instead of being autonomous units working in parallel without any overriding plan or scheme, the individual units now became units of a larger scheme devised and coordinated by ‘merchant entrepreneurs’: ‘a number of individual units spread over a wide area but interconnected’ (ibid., p. 300). Coordinating the work of the many interconnected units, the merchant entrepreneur would advance the raw materials, take care of transportation of goods from one unit to the next (spinner, weaver fuller, dyer, shearer). ‘The pattern in every case was a sequence of manufacturing operations, culminating in the finished product and its marketing.’ (ibid.).

This system for which no generally accepted term exists,[12] developed from the end of the middle ages. It spread across the non-agricultural economy at large

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[12]The ‘putting-out’ system is sometimes dubbed ‘proto-industrialization’ in modern economic historiography (e.g., Mendels, 1972) but this notion is typically used as a designation for a stage in economic history (as pointed out by Coleman, 1984).

but became particularly dominant in the textile trades (spinning, weaving, etc.). However, the reason for paying attention to it here is its historical outcome; namely, that it promoted specialization at the level of production units. This was most pronounced in branches of production that fabricated 'small and delicate objects' such as lace and pins and needles, etc. (needles would for instance pass through 72 hands during the production process). But perhaps more importantly, the 'putting out' system occasioned merchant entrepreneurs and their assistants to develop practices of analyzing and coordinating work processes (standardization of materials, relative proportions of component processes, logistics) (Sombart, 1916).

Manufactures: From the middle of the sixteenth century and towards the end of the eighteenth century the process of deepening and systematic division of labor took a new form: 'manufactures'. Its characteristic feature was the centralization under one roof of the workforce. This made possible not only systematic supervision (and reduced costs of transportation etc.), but also 'an advanced division of labor' (Braudel, 1979, p. 300). What defines this movement is the systematic decomposition of received work processes into component processes and the corresponding specialization of individual workers to perform specific component processes. In many domains of work the form of work organization that characterized handicraft work, namely, that the individual worker mastered the entire range of tasks and that the different workers at the workshop could replace each other in performing different kinds of task, was transformed into an organization based on systematic division of labor in the workshop.

In 1597 Thomas Deloney published a eulogy to the 'the famous Cloth Workers in England' as represented by the cloth maker John Winchcombe, 'a famous and worthy man'. The text is of interest because it, as pointed out by the editor of Deloney's works, shows 'a detailed knowledge of Newbury, its surroundings, and the county families of Elizabethan Berkshire, which could only have been obtained by an actual residence there' and, more to the point, because it offers a contemporary, if colorful and apologetic, description of the work organization in an early manufacture:

> Within one roome being large and long,
> There stood two hundred Loomes full strong:
> Two hundred men the truth is so,
> Wrought in these Loomes all in a row.
> By euery one a pretty boy,
> Sate making quils with mickle ioy;
> And in another place hard by,
> An hundred women merrily,
> Were carding hard with ioyfull cheere,
> Who singing sate with voices cleere. (Deloney, 1597, pp. 20 f.)

The poet goes on to enumerate the various handicrafts at this site by taking the reader through a imaginary tour of the establishment, pointing out—in addition to the 200 weavers, each assisted by a 'pretty boy', and the 100 joyfully singing carders—200 'pretty maids' ceaselessly engaged in spinning in an adjoining chamber, and in another room 150 'children of poore silly men' 'picking wool', and further, in yet other rooms, 50 male shearers, 80 rowers, 40 dyers, and 20 fullers.

The manufactures were not only a form of organization that was seen as profitable and laudable; they were the object of scholarly interest, as a technology. The principles of systematic division of labor was formulated repeatedly the course of the seventeenth and eighteenth centuries, for instance by William Petty:

> the Gain which is made by *Manufactures*, will be greater, as the *Manufacture* it self is greater and better. For in so vast a City *Manufactures* will beget one another, and each *Manufacture* will be divided into as many parts as possible, whereby the work of each *Artisan* will be simple and easie; As for Example. In the making of a *Watch*, if one Man shall make the *Wheels*, another the *Spring*, another shall Engrave the *Dial-Plate*, and another shall make the *Cases*, then the *Watch* will be better and cheaper, than if the whole work be put upon any one Man. (Petty, 1683, p. 472)

Diderot went further and formulated the principles of the systematic division of labor in manufactures in his article on 'Art' in the first volume of the *Encyclopédie*. Discussing the advantages of manufactures, Diderot argued:

> The speed of work and the perfection of the product both depend entirely on the number of workers assembled. When a manufacture is large, each operation is dedicated to a different man. This worker here only makes one unique thing his entire life, that one there another thing, so that each operation is performed well and promptly, and so that each product, while the best made, is also the cheapest. (Diderot, 1751, pp. 275 f.)

However, the classic example of this form of work organization is undoubtedly that of pin manufactures, as given by Adam Smith in the first chapter of his *Wealth of Nations*. Arguing that the manufacture of pins, though 'a very trifling manufacture', is one 'in which the division of labour is very often taken notice of' and thus a paradigmatic case of 'division of labour', Smith goes on to describe the organization of a particular workshop:

> One man draws out the wire, another straights it, a third cuts it, a fourth points it, a fifth grinds it at the top for receiving the head; to make the head requires two or three distinct operations; to put it on, is a peculiar business, to whiten the pins is another; it is even a trade by itself to put them into the paper; and the important business of making a pin is, in this manner, divided into about eighteen distinct operations, which, in some manufactories, are all performed by distinct hands, though in others the same man will sometimes perform two or three of them. I have seen a small manufactory of this kind where ten men only were employed, and where some of them consequently performed two or three distinct operations. (Smith, 1776b, pp. 4 f.)

In reading Smith's analysis it is important to realize that what he conceptualizes as division of labor is also, by the same token, cooperative work. The systematic division of labor is necessarily complemented by systematic cooperative work. This insight, which was implicit in Smith's argument, was explicitly made by his followers such as Edward Wakefield and John Stuart Mill, although their discussion is confused because they, as dedicated followers of Smith, confounds the systematic and regular division of labor at the point of production with the disorganized and spontaneous division of labor at the level of the economy at large (cf., e.g., Mill, 1848, § I.VIII.1). The point is made more clearly by Mill's contemporary, the German political economist Friedrich List who, as a protectionist, was prone to be critical of Smith:

> a division of the operations, without the *association of productive powers for a common end*, would be of very little help in the production. That such a result may be obtained, it is necessary that the different individuals be associated intellectually and bodily and that they cooperate. He who makes the head of pins must count upon the labors of him who makes the points, so that he does not run the risk of making pin heads in vain. The work activities should be in a suitable proportion to each other; the workers ought to be located as near to each other as possible; and their cooperation should be insured. (List, 1841a, p. 165; cf., List, 1841b, p. 231)

For our purposes, this insight is important. Cooperative work is found in the entire course of human history, but until the systematic division of labor in manufactures it either occurred sporadically (e.g., hunting of large game, clearing of forest) or was marginal to ordinary production (construction of sacral buildings, fortifications, dams, roads), and the work organization of the manufactures is thus the first example systematic and continual cooperative work at the point of production.

Smith was of course not particularly interested in the technological and managerial issues of work organization, but in the effects of work organization on the creation of 'wealth', and he therefore quickly moved on to expound three advantageous effects of division of labor (Smith, 1776b, p. 7): 'First, the improvement of the dexterity of the workman necessarily increases the quantity of the work he can perform; and the division of labour, by reducing every man's business to some one simple operation, and by making this operation the sole employment of his life, necessarily increased very much dexterity of the workman.' However, in his analysis Smith stayed well within the limits of the manufactures, emphasizing the advantages over artisanal work but blind to the inherent limitations of the manufactures. He made some very enthusiastic claims concerning the gains in productivity as a result of division of labor. To determine the effect of division of labor on productivity, Smith compared his case with that of a single polyvalent worker:

> Those ten persons [. . .] could make among them upwards of forty–eight thousand pins in a day. Each person, therefore, making a tenth part of forty–eight thousand pins, might be considered as making four thousand eight hundred pins in a day. But if they had all wrought separately and independently, and without any of them having been educated to this peculiar business, they certainly could not each of them have made twenty, perhaps not one pin in a day; that is, certainly, not the two hundred and fortieth, perhaps not the four thousand eight hundredth part of what they are at present capable of performing, in consequence of a proper division and combination of their different operations. (Smith, 1776b, p. 7)

That is, Smith asserted that the production of pins, under the scheme of division of labor, had increased labor productivity by a factor 4,800, or at least 240. However, the claim that a single worker, on his own, would hardly be able to produce one pin per day, leave alone twenty, was sheer guesswork on Smith's part. In fact, in artisanal pin-making in France of the eighteenth century an apprentice should be able to produce 1,000 pins in half a day, or 2,000 on a daily basis (Peaucelle, 2007, p. 196). A 100% increase in productivity is of course significant by any standard, but it is not the mind-blowing rate that has mesmerized Smith's readers since then. The point of this is not to be pedantic but rather that the manufactures work organization was severely limited by the characteristics of the human sensory-motor system. Productivity may increase as a result of the increase in dexterity of the individual but with a few weeks of training, the learning curve would reach a plateau.

As a second advantage of division of labor, Smith pointed to the possibility of 'saving the time commonly lost in passing from one sort of work to another is much greater than we should at first view be apt to imagine it'. Productivity will of course also increase as continuity of operation eliminates the cost of transiting from one operation to the next (set-up costs, in modern terminology) but, again, when the set-up costs have been eliminated the productivity rate will also flatten out.

And as the third and last advantage, Smith argued that specialization engenders technical innovation:

> everybody must be sensible how much labour is facilitated and abridged by the application of proper machinery. It is unnecessary to give any example. I shall only observe, therefore, that the invention of all those machines by which labour is so much facilitated and abridged seems to have been originally owing to the division of labour. Men are much more likely to discover easier and readier methods of attaining any object when the whole attention of their minds is directed towards that single object than when it is dissipated among a great variety of things. (Smith, 1776b, pp. 7–10)

Again, despite the use of the term 'machinery', Smith's analysis remained firmly entrenched within the conceptual horizon of the manufactures. When talking about 'machinery' he was not talking about what we today understand by machinery, namely devices that can operate automatically (under human supervision, of course, but without continual human involvement in the transformation process). This issue will be discussed at length later, but it has to be pointed out here that Smith, like his contemporaries, was using the terms 'machine' and 'machinery' very loosely to denote any kind of composite tool that somehow augments human capacity such as, for example, 'the ship of the sailor, the mill of the fuller, or even the loom of the weaver' (Smith, 1776b, p. 12).

The way in Smith presents the case ('I have seen a small manufactory of this kind') and not least the way he has been read may lead one to assume that he in his analysis described something that to his contemporaries was breaking news. It was not. As pointed out above, the manufactures form of work organization had been the object of study for a century before Smith described the pin manufacture. More than that. The pin manufacture had been a case of special interest to scholars several decades before Smith wrote about it. And although Smith claimed to have observed the workshop he described, the evidence indicates that this is actually not true (Peaucelle, 2006, 2007). As Smith himself pointed out, the work organization of pin making had, at the time, 'very often' been 'taken notice of'. In fact, the division of labor in the manufacture of pins had been described long before Smith made use of the case (in his *Lectures on Jurisprudence* from 1762 to 1764 and in his *Wealth of Nations*) and it was common knowledge among scholars who had an interest in technology and economy. Thus, Ephraim Chambers's *Cyclopædia* from 1728, well-known and respected at the time, emphasized the advanced division of labor in the manufacture of pins.[13] The primary sources for Smith's description were French. As mentioned earlier, the French *Académie des sciences* had since 1675 been collecting data concerning manufacturing processes based on field reports from local observers who were attached to the prefectures but were doing their research at the direction of scholars attached to the *L'Académie* (Peaucelle and Manin, 2006). One of the production processes on which reports were collected (since 1693) was that of the pin manufactures in Normandy (Peaucelle and Manin, 2006; Peaucelle, 2007). The analyses were eventually made publicly available in a various documents, first of all in one of the volumes of *Descriptions des arts et métiers*, a collection of writings devoted to the *Art of pin-making* edited by Duhamel de Monceau and containing texts by Réaumur, Perronet, and Chalouzières (Réaumur et al., 1761) but also, and better known, in the very detailed accounts in Diderot's *Encyclopédie* (Delaire, 1755; Perronet, 1765). The findings of these reports and analyses were then further distributed in various forms: in journal reviews (e.g., in the *Journal des sçavans,* 1761), in pocket dictionaries (such as Macquer's *Dictionnaire portatif des Arts et Métier,* 1766). In short, the work organization of manufactures was the topic of great interest, and the organization of the manufacture of pins was becoming a paradigm case for the technological and managerial knowledge of this kind of work organization (Fig. 11.1).

That is, the form of cooperative work that is characteristic of the manufactures was investigated, analyzed, and referred to by contemporaries as extant and ongoing cases; they became an element of common knowledge: hailed by poets and propagandists such as Deloney, studied and described in detail by scholars such as Delaire and Perronet, and ultimately elevated to paradigm status by Adam Smith.

The manufactures play an important historical role in the development of machinery in production processes. The analysis of work processes involved in the decomposition of craft work, the standardization of methods for each constituent process, and their recomposition and planned integration paved the way for the mechanization of constituent processes. Automatic machines had, of course, existed for centuries at this time. Clocks are a case in point. At the point of production, however, machinery only became significant by the end of the eighteenth century. Now, manufactures were never simply transformed into machine-based factories, nor did machinery always emerge from manufactures. Mechanization could also occur from the construction of machinery to automate an entire process. However, the manufactures play a critical role in the development of machinery for the automatic enactment of production processes, in that it developed widespread and (fairly) systematic knowledge of the principles of division of labor: the concept of division of labor, of sequences of elementary operations, of productivity as measured by output per worker per day, of different cadencies in a composite process, of 'line balancing', etc. The transformation of craft work into a form based on systematic division

[13] 'Notwithstanding that there is scarce any Commodity cheaper than *Pins*, there is none that passes thro' more hands e're they come to be sold.—They reckon 25 Workmen successively employ'd in each *Pin*, between the drawing of the Brass-Wiar, and the sticking of the *Pin* in the Paper' (Chambers, 1728).

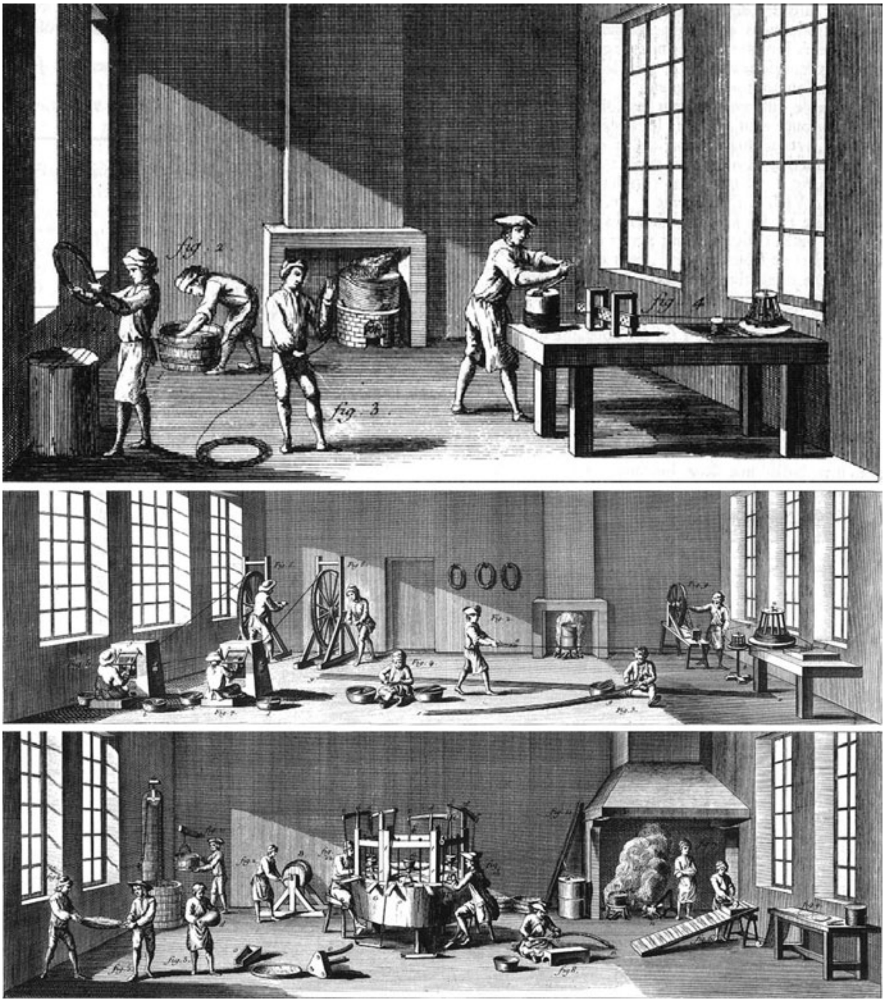

Fig. 11.1 Illustrations of the pin making manufacture from Diderot's *Encyclopédie* (the supplementary series of plates: *Recueil de planches, sur les sciences, les arts liberaux, et les arts méchaniques, avec leur explication*, vol. 3, Paris, 1765). Pins were made from brass wire that is drawn and cut and so forth. The *topmost plate* shows winding, unwinding, and washing coils of wire before drawing it. The *second plate* shows the processes of drawing the wire (*right*), cutting it (*center*), and pointing the pins (*left*). The *bottom plate* depicts the final process of heading the pins by annealing (*right*, at the back) and hammering (at the contraption in the *center*). The pins are finally tin-coated, washed, polished, and packed (*left*). The pin making process was described in detail by Delaire in vol. 5 of *l'Encyclopédie* (1755), by Duhamel du Monceau in a volume of the *Descriptions* specifically devoted to the art of making pins (Réaumur et al., 1761), and by Perronet in vol. 3 the *Recueil de planches* (1765). (Cf. also Gillispie, 1959, vol. 1, plates 184–186 and associated text)

of labor can be seen as having been instrumental in developing the requisite technical and managerial practices for mechanization of production to be *conceivable as a practical option*. That is, *the very idea that a received work practice could be analyzed, decomposed, and composed anew was the decisive concept*. In fact, our very concept of *technology* is predicated on this insight.

In the development of technology—of technical knowledge, that is—Smith's version of the story is hugely important, since it provided the paradigmatic account: that a concerted arrangement of workers, each specialized in performing a simple operation in a systematically planned sequence, could achieve a productivity greatly surpassing that of the same number of skilled workers, even artisans, working in parallel. More importantly, it highlighted and directed the attention of engineers, technologists, and managers as well as scholars to the fact that in a carefully planned division of labor, the collaborating workers, as a concerted collective, could master a process that none of them could master and perhaps even account for individually. This was underscored, albeit indirectly, by Charles Babbage when he, in a comment on Adam Smith's analysis of the advantages of the division of labor in manufactures, made some critical corrections. Going back to one of sources used by Smith (namely the abovementioned Perronet), Babbage pointed out that a major factor making division of labor economically advantageous is the circumstance that, whereas workers with the skills of artisans, and corresponding salaries, would be required for the most complex operations, operations, such as heading pins, could be performed by workers with very little training and similar wages (women and children):

> it appears to me, that any explanation of the cheapness of manufactured articles, as consequent upon the division of labour, would be incomplete if the following principle were omitted to be stated. That the master manufacturer, by dividing the work to be executed into different processes, each requiring different degrees of skill or of force, can purchase exactly that precise quantity of both which is necessary for each process; whereas, if the whole work were executed by one workman, that person must possess sufficient skill to perform the most difficult, and sufficient strength to execute the most laborious, of the operations into which the art is divided. (Babbage, 1832, § 226)

This has been much discussed, and rightly so, for it is an important corrective to Smith's analysis of division of labor. The point in our context, however, is that Babbage here underscores a lesson that is easily ignored: that the individual worker in such arrangements only needs to understand and master his or her partial task and that the partial task might be made so simple that it can be mechanized. From this insight emerges the modern concept of the control function, predicated on a distinction of planning and execution of operations.

The systematic division of labor that characterizes manufactures has inherent limitations, as pointed out by Marx:

> For a proper understanding of the division of labour in manufacture, it is essential that the following points be firmly grasped: First, the *analysis of the production process into its specific phases* coincides, here, strictly with the *dissolution of a handicraft into its various partial operations*. Whether complex or simple, the performance retains the character of handicraft and, hence, remains dependent on the strength, skill, quickness, and sureness, of the part-worker in handling his instruments. The handicraft remains the basis. This *narrow technological basis* excludes a really scientific analysis of the production process, since

> each partial process to which the product is subjected must be capable of being carried out as partial handicraft. Precisely because handicraft skill, in this way, remains the foundation of the process of production, each worker becomes exclusively *appropriated* to a partial function and that, for the duration of his life, his labour power is turned into the organ of this detail function. (Marx, 1867a, pp. 274 f.)[14]

The manufactures work organization was not only severely limited by the characteristics of the human sensory-motor system and by the fact that the learning curve would reach a plateau; it was also, Marx argued (with Ure), limited by its severe dependence on the handicraft worker and thus by workers' resistance to attempts to speed up the process.

Still, the fact that the performance retains the character of handicraft has not prevented foremen and engineers in the twentieth century from taking specialization to even greater heights than that represented by the classic manufactures. In order to fully exhaust the performance potential of workers, work processes and operations were subjected to careful systematic analysis and redesign, and for this purpose engineers such as Frederick W. Taylor (1911), Henri Fayol (1918) and their many followers (e.g., Urwick and Brech, 1945, 1946, 1948) developed and applied advanced methods and techniques of observation such as stopwatches, video cameras, etc., as well as methods of work design such as instruction cards and slide rules, etc. Likewise, to optimize the performance of the human sensory-motor system physiologists became involved in the design of elementary operations, tools, and work stations and eventually developed 'human factors' or ergonomics as a engineering discipline with its own repertoire of methods and techniques.

In a parallel attempt to overcome workers' resistance, social psychologists became involved in developing methods of increasing workers' 'motivation'. This effort is famously represented by the so-called 'Hawthorne experiments' carried out between 1924 and 1933 (e.g., Roethlisberger and Dickson, 1939). Where Taylor consistently argued for financial means for motivating workers, the social psychologists concluded from these 'experiments' that measures such as friendly supervision and 'social relations' in 'work groups' were of overwhelming importance. However, more recent and more rigorous studies comparing the Hawthorne conclusions and the Hawthorne evidence show 'these conclusions to be almost wholly-unsupported' (Carey, 1967) and that there is 'no evidence of Hawthorne effects' (Jones, 1992). This has not prevented the formation of a 'human relations' profession with its own repertoire of methods and tests, however (for discussions of this phenomenon of managerial ideology, cf. Gillespie, 1991; Brannigan, 2004).

While initially focused on increasing daily output per worker by optimizing the part-performance of the part-worker, the methods and techniques developed by the 'scientific management' school were not restricted to that. Major improvements in

[14] Author's translation from the German original. The standard English translation (by Moore and Aveling) is unfortunately rather inaccurate and, indeed, occasionally misleading. Anyway, the English translation of the relevant passages can be found in Chapter XIII ('Co-operation'), Chapter XIV ('Division of labour and manufacture'), and Chapter XV ('Machinery and modern industry'), esp. Section 1 ('The development of machinery', pp. 374 ff.). (Marx, 1867c).

overall productivity have been obtained by optimizing the flow of work pieces from one part-worker to the next, in particular by motorizing the transportation of work pieces, typically by means of conveyor belts. The assembly line one finds in, e.g., automobile assembly plants, and which many sociologist of work have taken as exemplary of the machine-based factory system, is in fact and on the contrary the most advanced form of the systematic division of labor exemplified by the pin manufacture. The confusion is nurtured by imagery in which workers, to the naïve eye, appear as small cogwheels in a vast automaton and it is reinforced by overindulgence in metaphorical discourse where certain organizational forms are talked of a 'machines'.[15] But even superficial observation of the organization of work in these plants shows that is generally that of extreme division of labor (for a collection of historical photos, cf. Marius Hammer, 1959). What became 'mechanized' in automobile assembly and in other kinds of assembly work in the course of the twentieth century was the transportation of parts and assemblies between workstations, not the operations at the various work stations, and the 'mechanization' of the transportation system merely consisted in a conveyer belt arrangement powered by electrical motors. In fact, it is only quite recently, after the microprocessor technology began to stabilize in the late 1970s, that self-acting machinery is being applied in significant ways in the automotive industry (e.g., welding robots). Moreover, the assembly line was never representative of industrial production, not even of mass production. Because the assembly line requires a large investments in an inflexible workflow layout, it was and remains an exceptional case in modern manufacturing. In the words of Taylor's biographer, Robert Kanigel: 'While the assembly line remains a common if tired metaphor, it defines surprisingly little of modern manufacturing. The Taylorized workplace, on the contrary, appears everywhere, heedless of the lines between one industry and the next'. In short, 'Fordism was the special case, Taylorism the universal' (1997, p. 498).

Later in the twentieth century, engineers such as Taiichi Ohno of Toyota developed and refined the repertoire of methods and techniques developed by the 'scientific management' school (Ohno, 1988). Although the principles he and his colleagues developed, widely known as the 'Toyota production system', are hailed as a break with 'Taylorism', they are at least as rigorous in their attention to systematic analysis of operations and continuity of the flow of work as previous forms of 'scientific management'. What is new is rather, on one hand, the refinement of the manufactures form of work organization to take into account *other performance criteria* than output per worker per day, namely, criteria such as the flexibility and responsiveness of the overall cooperative work effort (by 'just-in-time' principles) as well as product quality (cf. Peaucelle, 2000), and on the other hand a strong emphasis on 'continual improvement' and the involvement of workers in achieving this (cf. Spear and Bowen, 1999),

[15] The distinguished historian Siegfried Giedion, to take but one example, posited that 'The symptom of full mechanization is the assembly line, wherein the entire factory is consolidated into a synchronous organism' (1948, p. 5).

Marx's observation remains valid: the performance of cooperative work based on advanced de- and recomposition of labor 'remains dependent on the strength, skill, quickness, and sureness, of the part-worker in handling his instruments'. With the introduction of machinery, this dependence is broken and with this also the dependence of human productivity on progressive deepening of the division of labor.

2.2 Machinery: The Issue of the Control Function

The factory, the form of organization of work based on mechanization of the constituent work processes, is typically seen as yet another progressive form of work organization. The reason for this is that the advanced division of labor, based on decomposition of received handicraft and the recomposition of the production process as a systematically designed process, historically provided the *technology of work analysis* that is an essential foundation of machinery. At the same time, however, the factory represents a form of work organization that cannot be grasped as yet another permutation of handicraft and division of labor.

Mechanization can be conceived of as a process that unfolds in two dimensions: on one hand the process of transfer of operational control from workers to (thereby increasingly 'self-regulating') technical implements, i.e., *machines*, and on the other hand the process of technical integration of multiple machines into *machine systems*. It is the latter process, the development of machine systems, that is of particular concern here, as computational coordination technology is a special but now dominant type of machine system.

In manual work, as exemplified by traditional craft work, the process of transformation—the operation of the grindstone, spinning wheel, handloom—is literally 'in the hands' of the worker. Some external source of power may be employed (e.g., animal, wind, water, steam), but the operation of the tool (stone, spindle, shuttle) is controlled by the worker, and the performance thus depends on his or her skills. With machinery, the control of the operation of the tool is performed 'automatically', by the implement, without human continual intervention or mediation.

This conception is a modern one; it originates in the industrial revolution. One of the first to make this distinction was Charles Babbage who, for most of the 1820s, conducted extensive field work in 'a considerable number of workshops and factories, both in England and on the Continent, for the purpose of endeavouring to make myself acquainted with the various resources of mechanical art' (Babbage, 1832, p. iii). Faced with 'the wide variety of facts' obtained at these visits, he found it 'impossible not to trace or to imagine' 'some principles which seemed to pervade many establishments', in particular the principles of division of labor and the 'mechanical principles which regulate the application of machinery to arts and manufactures' (ibid., pp. iii f.). It was, presumably, his extensive exposure to the facts of the ground, coupled with his skilled ability for generalization, that then enabled him to see, with some clarity, what was specific in the new machine technologies that were then being developed and applied, namely that a set of tools are 'placed in a frame', moved 'regularly by some mechanical contrivance', and thus 'acted on by a moving power' (ibid., §§ 10, 224, pp. 12, 174): 'When each process has been reduced to the use of some simple tool, the union of all these tools, actuated by one moving power, constitutes a machine.' (ibid, § 225, p. 174).

However, the first to express the notion of 'control' succinctly was Andrew Ure in 1835. Like Babbage before him, he had engaged in what we today would call field studies in the factory districts of England. In his own words, he 'spent several months in wandering through the factory districts of Lancashire, Cheshire, Derbyshire, &c., with the happiest results to his health; having everywhere experienced the utmost kindness and liberality from the mill-proprietors' (Ure, 1835, p. ix). Having returned from his field trip, he summarized his findings in a book that, while shamelessly apologetic of the factory regime, contains astute observations of the then emerging technologies of 'automatic' production:

> The principle of the factory system [. . .] is, to substitute mechanical science for hand skill, and the partition of a process into its essential constituents, for the division or graduation of labour among artisans. On the handicraft plan, labour more or less skilled, was usually the most expensive element of production [. . .] but on the automatic plan, skilled labour gets progressively superseded, and will, eventually, be replaced by mere overlookers of machines. (Ure, 1835, p. 20)

The concept underlying Babbage and Ure's distinction is that of what we today call *the control function* and the related notion of transfer of control from human to implement; that is, the (partial) elimination of human labor in so far as the actual transformation process is concerned. Simply put, on this view a machine differs from a tool by being able to perform autonomously, within certain limits, by virtue of a mechanical control function.[16]

Following Babbage and Ure, but conceptually more stringent, Marx made the concept of the control function the cornerstone of his analysis of the transformation of work in the course of the industrial revolution. The classic statement on machinery is in the discussion of 'Machinery and big industry' in *Capital*:

> On a closer examination of the *machine tool* or the *working machine* proper, one recognizes by and large – though often, no doubt, in very modified form – the apparatus and tools used by the handicraftsman or the worker of manufactures, but instead of as tools of humans, now as tools of a mechanism, or mechanical tools. [. . .] The working machine is therefore a mechanism that, after being set in motion, performs with its tools the same operations that were formerly done by the workman with similar tools. Whether the motive power is derived from humans or from some other machine, makes no difference in this respect. After the transfer of the tool proper from humans to a mechanism, a machine replaces a mere tool. The difference strikes one at once, even when the human remains the prime mover. (Marx, 1867a, pp. 303 f.)

This was not an casual remark. Marx had first read Ure and Babbage 40 years earlier (Marx, 1845b, pp. 325–351) and had made this concept of machinery a pivotal element of his understanding of contemporary developments. Shortly after these

[16] Charles Kelley (1968) offers a good introduction to the modern concept of control.

initial studies he wrote a small book in which he, quoting Babbage and Ure, stated rather unequivocally that

> What characterizes the division of labor in the automatic workshop is that labor has there completely lost its character of specialism. But the moment every special development stops, the need for universality, the tendency towards an integral development of the individual begins to be felt. The automatic workshop wipes out specialists and craft-idiocy. (Marx, 1847, Chapter 2.2)

And again, 10 years later, in the first outline of his economic theory, the *Grundrisse* from 1857 to 1858, he makes this point forcefully:

> Work no longer appears so much as included within the production process; but rather the human engages in that process as its overseer and regulator. [. . .] It is no longer the worker who interposes the modified natural object [i.e., the tool] as an intermediate between the object and himself; but rather, he now interposes the natural process, which he transforms into an industrial one, as a means between himself and inorganic nature, which he masters. He stands beside the production process, rather than being its main agent. (Marx, 1857–1858a, p. 581)

He subsequently reread Babbage and Ure in the context of the extensive studies of the development of production technologies and work organization (cf. Marx, 1861–1863, pp. 229–318, 1895–2090) that found their ultimate expression in the chapters on 'Cooperative work', 'Division of labor', and 'Machinery' in *Capital*.

In view of the fact that the mathematical theory of control functions had not been proposed yet when Babbage, Ure, and Marx offered these initial formulations of the modern concept of machinery, this is of course quite remarkable. Their ability to do so, however, was not the result of prescience but was a consequence of their keen interest in what we today would call the changing allocation of function between human and machine which they considered of the utmost economic and social importance. This methodological preferences is made explicit at several places, for instance in a letter from 1863 in which Marx summarized his initial conclusions from a thorough study the development the progressive forms work organization and especially machinery:

> there is considerable controversy as to what distinguishes a *machine* from a *tool.* After its own crude fashion, English (mathematical) mechanics calls a *tool a simple machine* and a *machine a complicated tool*. English technologists, however, who take rather more account of economics, distinguish the two (and so, accordingly, do many, if not most, English economists) in as much as in one case the *motive power* emanates from man, in the other from *a natural force*. [. . .] However, if we take a look at the machine in its *elementary* form, there can be no doubt that the industrial revolution originates, not from *motive power,* but from that part of machinery called the *working machine* by the English, i.e., not from, say, the use of water or steam in place of the foot to move the spinning wheel, but from the transformation of the actual spinning process itself, and the displacement of that part of human labor that was not mere *exertion of power* (as in treading a wheel), but was concerned with processing, working directly on the material to be processed. [. . .] To those who are merely mathematicians, these questions are of no moment, but they assume great importance when it comes to establishing a connection between human social relations and the development of these material modes of production. (Marx, 1863, pf. 320 f.) (Cf. also Marx, 1861–1863, pp. 1915–1917)

Now, what is interesting from a CSCW perspective, as opposed to a history of science and technology point-of-view, is not these early developments of the concept of machinery. What is very relevant to CSCW is rather the concept of *machine systems* these authors suggested. In formulations that are strikingly visionary, Ure observed:

> The term *Factory,* in technology, designates the combined operation of many orders of work-people, adult and young, in tending with assiduous skill a system of productive machines continuously impelled by a central power. This definition includes such organizations as cotton-mills, flax-mills, silk-mills, woollen-mills, and certain engineering works; but it excludes those in which the mechanisms do not form a connected series, nor are dependent on one prime mover. But I conceive that this title [the term *Factory*], in its strictest sense, involves the idea of a vast automaton, composed of various mechanical and intellectual organs, acting in uninterrupted concert for the production of a common object, all of them being subordinated to a self-regulated moving force. (Ure, 1835, pp. 13 f.)

In his conception of machine systems Marx again followed Ure, but his conception of machine system is significantly more developed. This of course reflects the rapid development of machine technology had undergone since Ure was roaming the factory districts of England some 33 years earlier. Based on studies of 'automatic workshops' as represented by paper and envelope factories, power looms, and printing presses, Marx stated:

> A proper *machine system* only takes the place of the *particular independent machines*, where the work piece [Arbeitsgegenstand] undergoes a continuous series of different process steps, carried out by a chain of machine tools that, while of *different species*, complement one another. (Marx, 1867a, p. 309) (cf. also Marx, 1861–1863, pp. 1940–1946)

He compared this system of interoperating machines with the division of labor that was characteristic of the manufactures:

> The cooperation by division of labor that characterizes Manufacture here reappears, only now as a *combination of partial working machines*. The specialized tools of the various specialized workmen, such as those of the beaters, cambers, twisters, spinners, etc., in the woollen manufacture, are now changed into the tools of *specialized* machines, each machine constituting a special organ, with a particular function, in the combined tool-mechanism system. (Marx, 1867a, p. 309)

However, Marx went on, 'an essential difference at once manifests itself':

> In the manufacture, it must be possible for each particular partial process to be performed by workers, individually or in groups, by means of their manual tools. While the worker is adapted to the process, then on the other hand, the process was previously adapted to the worker. This *subjective* principle of division of labor vanishes in production by machinery. The process as a whole is here considered *objectively*, in and for itself, analyzed into its constituent phases; and the problem of how to execute each partial process and connect the different partial processes into a whole, is solved by technical application of mechanics, chemistry, etc. (Marx, 1867a, pp. 309 f.)

According to Marx then, in the advanced factory, based on machine systems, the worker is no longer subsumed under the regime of extreme specialization that characterizes the manufactures but become a member of a cooperative ensemble that, as a collective, supervises the operation of the machine system:

> The combined working machine, now an organized system of *different species* of individual machines and of *groups* of such machines, becomes increasingly perfect in so far as the process as a whole becomes a continuous one, i.e., the less the raw material is interrupted in its passage from its first phase to its last; in other words, the more its passage from one phase to another is conveyed by the mechanism itself, not by human hand. (Marx, 1867a, p. 310)

Marx here clearly conceived of machine systems in terms of a system of automatically interacting machines, as a system of interoperating control functions. With the development of *systems of machines* that interoperate, cooperative work becomes (to some extent and at different levels of granularity) mediated and regulated by these machine systems:

> The implements of labor acquire in machinery a material mode of existence that implies the substitution of human force by natural forces and of experience-based routine by the conscious application of science. In the manufacture, the organization of the social work process is *purely subjective*, a *combination* of partial workers; in the machine system, modern industry creates an *objective* productive organism, which the worker meets as an existing material condition of production. In simple cooperation, and even in cooperation predicated on division of labor, the displacement of the individualized worker by the socialized worker still appears to be more or less accidental. Machinery [. . .] operates only in the hand of directly associated or *communal* work. Hence the *cooperative* character of the work process now becomes a *technological necessity* dictated *by the nature of the implement of work itself*. (Marx, 1867a, p. 315)

With the industrial mode of production, on this view, cooperative work is more than an economically advantageous arrangement; it is 'a technological necessity'. The machine system presumes a cooperative work arrangement for its operation and the individual activities of the cooperating workers are in turn mediated and regulated by the machine system (Fig. 11.2).

While remarkably modern, the conception of machine systems developed by contemporary analysts such as Ure and Marx is, not surprisingly, limited by the practical horizon of the factory system of the nineteenth century. As is evident from the above quotations, Ure's conception of 'the factory' is predicated on not only 'the idea of a vast automaton, composed of various mechanical and intellectual organs, acting in uninterrupted concert for the production of a common object' but also on the premise that 'all of them being subordinated to a self-regulated moving force'. What, on Ure's conception, makes the 'system of productive machines' *a system*, 'a vast automaton', is the fact that multiple machines are being 'continuously impelled by a central power' (Ure, 1835, pp. 13 f.). The same ambiguity can be found in Marx:

> A system of machinery, whether based on mere cooperation of working machines *of the same kind*, as in weaving, or on a combination of machines *of different kinds*, as in spinning, constitutes in and for itself *one* huge *automaton*, as soon as it is driven by a self-acting prime mover. [. . .] The machinery-based enterprise achieves its most developed form as an organized system of working machines that receives its motion from a *central automaton* by transmission machinery. (Marx, 1867a, pp. 310 f.)

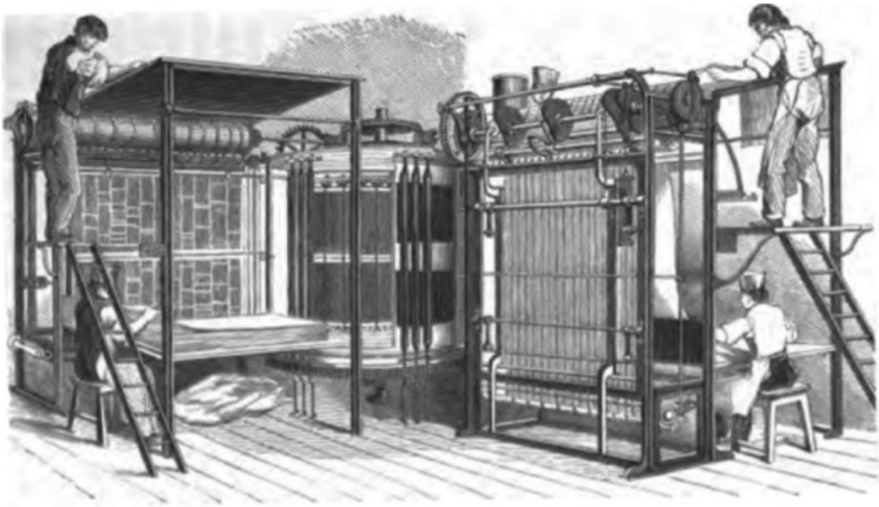

Fig. 11.2 Machine system: The printing press of *The Times*, *c*. 1851, built by Cowper and Applegath. From a 'Survey of the Existing State of Arts, Machines, and Manufactures' by a Committee of General Literature and Education appointed by the Society for Promoting Christian Knowledge (1855, p. 211), one of Marx' primary sources on 'machine systems'

The influence from Ure is obvious in that Marx, in discussing machine *systems*, also made a 'prime mover' the defining feature of machine systems, as opposed to *control* of operations. The picture of the central steam engine that, while entrenched in the basement of the factory, drives all machines in the factory via a highly visible and comprehensive power-transmission system of driving belts, shafts, and gear trains, surely must have been as evocative then as it is now. It is nevertheless, *prima facie*, somewhat puzzling that Marx, who confidently and clearly stated that 'the industrial revolution originates, not from *motive power*, but from that part of machinery called the *working machine*' and that a 'the elimination of that part of human labor that was not mere *exertion of power* [. . .], but was concerned with processing, working directly on the material to be processed', would include 'a self-acting prime mover' as defining of a machine *system*. This ambiguity is not accidental, however. It is rooted in historically given conceptual limitations. The transmission of power to the tool and the automatic control of the movements of the tool had not yet been *technically* separated. Ure and Marx therefore basically defined 'machinery' in terms of the role of the worker vis-à-vis the implement as such, that is, in terms of the 'self-acting' character of its operation, not in terms of specific *technical* features. For Marx the defining feature was that it is the machinery, not the worker, that immediately controls the movements of the tool.

The notion of a *distinct* control mechanism was absent from the reasoning of Marx. For good reasons. First of all, in spite of his attention to technical issues in the historical development of machinery, his primary concern was the changing role of workers in production, for, as he put it in the letter cited above, the questions of the role of the worker vis-à-vis the implement was 'of no consequence' to 'pure mathematicians', 'but they become very important when it is a question of demonstrating the connection of human social relations to the development of these material modes of production' (Marx, 1863, p. 321). Thus, when he, following Ure, made the 'prime mover' a defining feature of 'machine systems', he was considering the obvious fact that, while a worker might be the source of energy with a single working machine, as had been the case for centuries in the case of the spinning wheel, it was practically impossible for a worker to move a connected system of machines. That is, a continuous 'external' supply of energy to drive the machine system was simply a necessary precondition for these to be workable. Consequently, while his definition of machinery as such is clearly based on the notion of a control *function*, this concept is abandoned when the phenomenon of machine systems is considered.

Distinct control mechanisms, physically separate from the energy transmission systems of the implement, were extremely rare. Generally the control function was performed by the *physical union* of the mechanism of transfer of power and the mechanism of controlling the movements of the tool. This practico-technical circumstance made it difficult to formulate and apply a strict concept of control, for Ure and Marx and for generations of technologists too.

For more than a century, machine systems were a rare phenomenon, restricted to a limited set of branches of mass production. The reason for this is of critical importance for understanding the role of computing technologies and, by implication, CSCW.

In early machines, such as Richard Roberts' 'self-acting mule' (1825–1830), the control of the behavior of the tool was completely integrated with the system of transmission and transformation of energy. That is, the movement of the tool and the workpiece was regulated by the very same parts of the machine (driving belts, rack-and-pinion, gears, clutches, camshafts, crankshafts, etc.) that transferred energy to the tool (or the workpiece), that is, made it *move*. For reasons of economy and reliability, the overwhelming concern of mechanics would therefore be to keep the number of parts to a minimum. In the words of Larry Hirschhorn in his brilliant study of mechanization and computer control in the work place: 'in a good mechanical design the same part or series of parts simultaneously transmits power, transforms motion, and controls the speed and direction of movement, in this way minimizing the number of parts and preventing unwanted action' (Hirschhorn, 1984, p. 16). Because of this, it is difficult and expensive to modify the design of such machines. This is crucial for understanding the characteristics of mechanical technologies and, by contrast, of computing. As Hirschhorn puts it: 'well-designed machines are also highly constrained ones, single-purpose in character and design and hard to modify. [...] In general, since the systems of transmission, transformation, and control share the same parts, modifying one system inevitably means modifying the others. [...] In becoming more productive, they lose flexibility' (Hirschhorn, 1984, p. 18). As a result, for more than a century the domains in which machinery could be applied productively were quite limited. Machinery was generally only applied to branches of mass-production where the investment in special-purpose machinery could be amortized.

And as far as machine systems are concerned, the scope of productive application was even more limited. The cost of building and modifying machine systems prevented machine system technology to spread beyond a few branches of mass-production such as, for example, the production of envelopes, newspaper printing, chemical production, power production and distribution, metallurgy, and of course transportation.

For machines to be effectively flexible requires the mechanism of control be ‘physically separate’ from the mechanism of energy transmission (Mayr, 1969). In retrospect it is possible to discern distinctive control mechanisms in the form of devices such as float valves (devised by, e.g., Ctesibios in the third century BCE, Heron in the first century CE, Banū Mūsā in the 9th), thermostats (by e.g., Drebbel in the seventeenth century), regulators in windmills (by British millwrights in the eighteenth century), and speed regulators in steam engines (Watt) (Mayr, 1969). But it is significant that even Watt does not seem to have recognized the ‘universality’ of his invention. Not only did he not think of it as a ‘new invention’ but merely as an application of an invention made by others for the regulation of water and windmills. But he does not seem to have commented on the underlying ‘feedback’ principle. This leads the authority on the history of control mechanisms, Otto Mayr, to conclude: ‘One might infer from his silence that he did not see anything particularly interesting in this principle. Compared with the large but straightforward task of producing power, the regulating devices and whatever theory they involved may have appeared to the sober Watt as secondary, if not marginal’ (Mayr, 1969, pp. 112 f.). The prospect of mastering a vast reservoir of motive power and to be able to apply it to augment human productive capacity was so dominant that it was difficult to discern that something ultimately stupendous was underway: the development of technologies of control systems. Even those who understood the essentials of the paradigm shift—Ure and Marx—did not get it quite right. When discussing machine systems and what made the interconnected machines a *system*, they were still limited by the concept of the ‘prime mover’.

The conceptual distinction between the *mechanism* of provision of energy and the *mechanism* of control, or between ‘energy flow’ and ‘control flow’, only became articulated and stable as the distinction was made in practice: when machine builders in actual practice began to physically separate the mechanism for provision of energy from the mechanism for control of operations. And in actual practice the distinction was only made in the course of a very long process of technological development.[17]

For a century only sporadic progress was made in terms of physically separate and hence easily replaceable control mechanisms. Henry Maudslay’s slide rest lathe for cutting screws from around 1800 represents one of the earliest and, for ages, prevalent approaches to relaxing the separation of control and power mechanisms.

[17] For an overview of this development, cf. Stuart Bennett’s two volume *History of Control Engineering* since 1800 (1979, 1993).

As expressed in a state-of-the-art review from 1855, published on the occasion of the World Exhibition in London in 1851, by the end of the eighteenth century 'nearly every part of a machine had to be made and finished to its required form by mere manual labour; that is, we were entirely dependent on the dexterity of the hand and the correctness of the eye of the workman, for accuracy and precision in the execution of the parts of machinery'. However, with the 'the advances of the mechanical processes of manufacture [...] a sudden demand for machinery of unwonted accuracy arose'. Maudslay's new approach consisted in introducing a 'slide rest', i.e., a template whereby the movements of the lathe's cutting tool would be determined:

> The principle here alluded to is embodied in a mechanical contrivance which has been substituted for the human hand for holding, applying, and directing the motion of a cutting-tool to the surface of the work to be cut, by which we are enabled to constrain the edge of the tool to move along or across the surface of the object, with such absolute precision, that with almost no expenditure of muscular exertion, a workman is enabled to produce any of the elementary geometrical forms – lines, planes, circles, cylinders, cones, and spheres – with a degree of ease, accuracy, and rapidity, that no amount of experience could have imparted to the hand of the most expert workman. (Committee of General Literature and Education, 1855, pp. 238 f.)

This technology was soon applied as 'part of every lathe, and applied in a modified form in the boring mill, the planing machine, the slotting engine, the drilling machine, &c. &c' (ibid.) where geometrically accurate lines, planes, circles, cylinders, cones, and spheres were required and gave rise to simple automatic machines for manufacturing parts. 'Soon after its introduction the slide-rest was made self-acting, that is, its motion along or across the surface to which the tool it held was applied were rendered independent of the attention of the workman in charge of it.' (ibid.). (Cf. also, Gilbert, 1958).

In his discussion of this technology, Hirschhorn makes an important point: 'From the beginning, automatic machine tools required some form of template so that the eyes and hands of the worker, which once guided the tool, could be replaced by a machine piece. The template became the control element of the machine, determining the feed rate and movement of the tool.' (Hirschhorn, 1984, p. 22). What Hirschhorn does not say but implies is that a control mechanisms based on a template is of course an pattern transference mechanism in that it transfers its shape or some transformation of its shape. Jacquard's famous punched-card device for controlling silk-weaving looms (also from the beginning of the nineteenth century) operated in a similar manner, the string of cards imparting—again in an analog manner—a particular pattern to the fabric (cf. English, 1958). An impressive piece of design in its own right, the Jacquard loom is not belittled by observing that it is farfetched and rather fanciful to see in it some kind of anticipation of numerically controlled machinery (e.g., Essinger, 2005). In its own very sophisticated way it basically imparts patterns the way a boot leaves a mark in the snow: by pattern transference.

Control devices based on templates have taken many forms and do not really concern us here. It is sufficient to point to the large family of control devices based on a so-called cam, often an irregularly shaped component, that while rotating guide the motions of a 'cam follower' in a way similar to the way in which a rotating axis guides the movement of the piston. Compared to other linking mechanisms (gear trains, etc.), the cam provides increased flexibility in developing 'the form and timing of a particular motion', since 'one cam can simply be replaced by another'. 'In effect', Hirschhorn states, 'the control of movement is no longer contained in an array of gears, clutches, and mechanical stops that together make up the structure of the machine, but rather in the cam, which is separate from the body of the machine' (Hirschhorn, 1984, pp. 22 f.). However, as a control technology camshafts and similar mechanisms again suffered from severe limitations. On one hand, the construction and production of an irregularly shaped object that can impart a sequence of precise movements to a tool posed serious challenges. And on the other hand, and worse, although the cam is physically separate from the machine and can be replaced, it is a part of the energy transmission system in that it both provides the guide for the tool and the force to move it:

> The linkage between the control and transformation systems of the machine places limits on the economics of cam making. Not only must the cam be powerful enough—that is, of sufficient metallic thickness and mass—to impart the necessary force; it must also be shaped so. as to guide the tool accurately. Cams tend to be difficult and costly to shape and quick to wear out. Cam-following machines were thus used mostly for large runs that produced universal pieces such as screws and bolts. (Hirschhorn, 1984, p. 23)

To put it crudely, mechanical control mechanisms were about as costly to construct and modify as the mechanism they were designed to control. It was for this reason that *machine systems* remained an economically marginal phenomenon, confined to paper production and similar branches of manufacturing that were producing vast quantities of simple and identical products. The exceptional cases such as transfer lines in automotive industry (car frames, cylinder blocks) were truly impressive but were just that: impressive exceptions in an ocean of highly specialized manual work combined with islands of semiautomatic machine tools and motorized flow lines (Wild, 1972).

Machinery only began to spread beyond the confines of mass-production when portable electrical motors began to appear at the end of the nineteenth century. They were initially applied in machinery such as sewing machines, lathes, drilling machines, and printing presses. But by the 1920s the major part of industry was 'electrified'. The electrical motor technology offered several advantages but, most importantly, it made it technically and economically feasible to make machines more flexible:

> as long as power was obtained from a water wheel, a steam engine, or from a large electrical motor that powered a number of devices, the problems of transmitting power to each mechanism and component of a machine required considerable ingenuity. The mechanical problems of transmitting mechanical energy where it was needed to all portions of the machine greatly complicated machine structure. (Amber and Amber, 1962, p. 146)

With the portable electric motor these almost paralyzing constraints could be relaxed and this made it possible to create machinery that could be modified at lower costs:

> The mechanic could now place two or more motors in a particular machine. The constraint on machine design was reduced, since different parts could move at different speeds without being connected to the same primary power source. Long gear trains were eliminated. In large machines, independent portable motors could now direct individual segments moving in different planes, eliminating the need for linkages that translated motions in one place to motions in another. [. . .] To change the relative speeds of different machine sections, the mechanic or engineer, instead of stripping the machine and replacing old gears and cams with new ones, need only adjust the relative speeds of the different electric motors. [. . .]. The relaxation of machine constraints opened the way to increasingly general-purpose machines, machines that could be modified at reasonable cost. (Hirschhorn, 1984, pp. 19, 21)

It was, in practice, only with electromagnetic and electronic control devices (switches, valves, transistors) that the concept of separate control devices began to become articulated, but it was only with the advent of the electronic computer that an economically feasible control technology became available. Machine systems remained an economically marginal phenomenon until—not just the development of the electronic computer in the late 1940s, but until microprocessors around 1980 made it technically and economically feasible not only to design control mechanisms and incorporate them in industrial machinery in the form of Computer-Numerical Controlled or CNC machinery (as reflected at the time in, e.g., Groover, 1980; Kochhar and Burns, 1983; Koren, 1983) and robotics (e.g., Engleberger, 1980) but also feasible to interlink machines to form automatically coordinated machines systems in the form of Flexible Manufacturing Systems or FMS (e.g., Merchant, 1983) and begin to explore ideas like Computer Integrated Manufacturing or CIM (e.g., Harrington, 1979). In short, it was only with the electronic computer in the form of the microprocessor that an economically viable technical basis for separating control and transformation devices was established.

Now, computer technologies did not, of course, originate from the challenges of controlling machines in manufacturing. The stored-program electronic computer was developed for the purpose of automatic processing of massive calculation work.

2.3 The Universal Control System: The Stored-Program Computer

The electronic computer is just as much a machine, i.e., an automatically operating material artifact, as a ‘self-acting mule’ or a Jacquard loom. It is *conceivable* that it could be constructed by wheels and gears or electromechanical switches. The difference is that in the former case it is the substantive nature of the moving parts, the interaction of ‘rigid bodies’, that causes the state change; in the electronic mechanism it is the electrical charge that causes the state change. The (enormous) advantage of the latter is first of all that electrons, due to their small mass compared to gears, can travel at a velocity close to the speed of light with minimal expenditure of energy. Where one of Babbage's gears may have had a mass of, say, 100 g, or the parts of electromechanical switch circuits in tabulating machines that were in use for most of the twentieth century may have had a mass of only 1 g and hence could work so much faster, electrons have a mass of 9×10^{-28} g (Goldstine, 1972, p. 144). This of course means that 'turning a bit' 'on' or 'off' requires an insignificant amount of energy. Instead of state changes propagating in a system of interconnected gears and shafts, clouds of electrons are milling about in an equally causal way. In a 'macroscopic' mechanism, one cogwheel meshes with another, the movement of the first causing the next cogwheel to change to another position, and a discrete state change has been propagated within the mechanism. Similarly, in an electronic mechanism electrons are amassed at a certain 'gate' and, when the charge has reached a pre-specified threshold value, the state of the gate switches. In either case, the pattern of state changes is an observable and highly regular correlation. But just as gears may jam, a wayward cloud of electrons amassing at a particular spot of the circuit (triggered by, say, energetic particles from cosmic radiation) may cause a gate to open and thus turn a bit. Accordingly, a lot of engineering skill goes into the design of both types of mechanism—'macroscopic' as well as 'microscopic'—in order to ensure that state changes that are *supposed* to propagate, and *only* those, *do* propagate; in short, that state changes propagate in a dependable way.[18] That is, electronic computers are causal systems just as much as any other machines, from the medieval water clock to the mechanical calculator to the punched-card tabulator. Whether macroscopic or microscopic, the behavior of a machine is a causal process, or a configuration of causal processes, that has been harnessed and thus, under certain conditions, behaves in an extremely regular fashion.

What we normally call 'a computer'—the Macintosh laptop in front of me, say, or a mainframe computer somewhere in a climate-controlled window-less room—of course consists of a number of machines that are quite tangible: one or more CPUs as well as various specialized integrated circuits such as arithmetic-logic unit, data bus, input and output devices, network connections, etc. In addition, however, 'the computer' consists of machines, 'software programs', that are just as material as 'chips': they are just invisible and intangible, but so are X-rays and TV signals.

The computer owes its advantages not just to the enormous speeds afforded by electronics but to the stored program architecture originally outlined by Alan Turing in his famous article on computable numbers (Turing, 1936).[19] Programs are treated as data and, when launched, reside in the computer's storage as—invisible but no less physical—electronic patterns that can be activated and deactivated virtually instantaneously.

[18] As Herman Goldstine puts it, the ENIAC 'had to operate with a probability of malfunction of about 1 part in 10^{14} in order for it to run for 12 h without error. Man had never made an instrument capable of operating with this degree of fidelity or reliability, and this is why the undertaking was so risky a one and the accomplishment so great.' (Goldstine, 1972, p. 153).

[19] 'In the conventional literature, von Neumann is often said to have invented the stored-program computer, but he repeatedly emphasized that the fundamental conception was Turing's' (Copeland and Proudfoot, 2005, p. 114 et passim).

In short, the computer can be seen as the ultimate control mechanism; or rather, in Turing's words in a talk on his design for the Automatic Computing Engine (ACE) given in 1947, the computer can 'imitate' the control mechanism of any machine: a typesetter, a printing press, a lathe, a machining center, a jukebox. Turing started by referring to his paper 'On computable numbers' (1936):

> Some years ago I was researching on what might now be described as an investigation of the theoretical possibilities and limitations of digital computing machines. I considered a type of machine which had a central mechanism, and an infinite memory which was contained on an infinite tape. This type of machine appeared to be sufficiently general. (Turing, 1947, p. 378)

He then elaborated on the implications of the stored program architecture:

> It can be shown that a single special machine of that type can be made to do the work of all. It could in fact be made to work as a model of any other machine. The special machine may be called the universal machine; it works in the following quite simple manner. When we have decided what machine we wish to imitate we punch a description of it on the tape of the universal machine. This description explains what the machine would do in every configuration in which it might find itself. The universal machine has only to keep looking at this description in order to find out what it should do at each stage. Thus the complexity of the machine to be imitated is concentrated in the tape and does not appear in the universal machine proper in any way.
>
> If we take the properties of the universal machine in combination with the fact that machine processes and rule of thumb processes are synonymous we may say that the universal machine is one which, when supplied with the appropriate instructions, can be made to do any rule of thumb process. This feature is paralleled in digital computing machines such as the ACE. They are in fact practical versions of the universal machine. There is a certain central pool of electronic equipment, and a large memory. When any particular problem has to be handled the appropriate instructions for the computing process involved are stored in the memory of the ACE and it is then "set up" for carrying out that process. (Turing, 1947, p. 383)

The Universal Turing Machine is a mathematical construct, not a real-world machine; it has infinite storage, which no real machine can have, of course. So, while mathematics deals with 'theorems, infinite processes, and static relationships', 'computer science emphasizes algorithms, finitary constructions, and dynamic relationships'. This means that 'the frequently quoted mathematical aphorism, "the system is finite, therefore trivial," dismisses much of computer science' (Arden, 1980, p. 9). That is, the Universal Turing Machine cannot be taken simply as the theoretical model of the computer. However, the concept of the stored-program computer as 'universal' or, better, *inexhaustibly malleable*, is the key concept in computing technology.

The stored-program computer can be reconfigured endlessly, manually or automatically, in response to internal and external state changes. In fact, what we call a computer forms a hierarchy of distinct but interacting control mechanisms: an 'operating system' that itself is a hierarchical system of control mechanisms devoted to the managing of data and programs stored in RAM, input and output and external storage devices ('drivers'), and so on, as well as so-called application programs that may also be hierarchical systems and form hierarchical relations with other application programs. This means that the configuration and reconfiguration of the system of machines constituting the computer, the programs in RAM as well as programs on local harddisks and remote 'servers', can be controlled automatically, one machine triggering the execution of another in response to certain conditions, and that human intervention, if required, can be performed semi-automatically or 'interactively' as, for instance, a mouse click making one software machine activate another.

What is more, also the design and construction of software machines (i.e., programming, compiling, testing) can be performed semi-automatically. This again means that designing and constructing software machines is immensely cheaper and faster than designing and constructing, say, a camshaft or a gear train, or for that matter rewiring the 'switchboard' control panel of an punched-card tabulator. And it goes without saying that what applies to designing and constructing software machines applies equally to *re*designing and *re*constructing software machines. Moreover, of course, software machine design specifications can be copied and transported automatically and at insignificant cost. And, finally, the stored-program technology also makes the construction and modification of large-scale machine systems incomparably inexpensive. In short, with the electronic stored-program computer, we have the technology for *the production of machines by machines* vaguely but perspicaciously anticipated by Marx (1867a, p. 314).

However, computing technologies did not come out a box, ready to 'plug and play'. They do not, first of all, originate from a particular body of mathematical theory; to be sure, their development have depended critically upon a host of mathematical theories (recursive function theory, Boolean algebra, Shannon's information theory, etc.), but they were not the result of the application of any particular theory. As pointed out by the eminent historian of computation Michael Mahoney, computer science has taken 'the form more of a family of loosely related research agendas than of a coherent general theory validated by empirical results. So far, no one mathematical model had proved adequate to the diversity of computing, and the different models were not related in any effective way. What mathematics one used depended on what questions one was asking, and for some questions no mathematics could account in theory for what computing was accomplishing in practice.' (Mahoney, 1992, p. 361). It is no surprise, then, that 'the computer' was not 'invented': 'whereas other technologies may be said to have a nature of their own and thus to exercise some agency in their design, the computer has no such nature. Or, rather, its nature is protean' (Mahoney, 2005, p. 122). It would be more accurate to conceive of this in terms of *costs* and thus say that computing technology is protean in that the costs of construction and modification of software machines are drastically reduced compared to those of previous machine technologies. Anyway, according to Mahoney, there therefore was a time, a rather long time, 'when the question "What is a computer, or what should it be", had no clear-cut answer' and the computer and computing thus only acquired 'their modern shape' in the course of an open-ended process that has lasted decades (Mahoney, 1992, p. 349). And there is no reason why one should assume that the concept of computing as we know it has solidified and stabilized: the jury is still out, as the immense malleability of ‘the computer’ is being explored in all thinkable directions.

In other words, it is confused to conceive of ‘the computer’ as *one* technology. Not only does ‘the computer’ in front of me incorporate a host of technologies, from metallurgy to semiconductor technology to programming languages and operating systems; it can assume an endless range of very different incarnations. It is a protean technology, and how it develops is determined by its applications in a far more radical sense than any other technology.

To understand the place of CSCW in this open-ended array of known and possible forms of technology requires that we have an idea of the received concepts of computing: the practical applications for which various computing technologies were developed and that, accordingly, have formed our conceptions of computing and computational artifacts. To do so, I will highlight some of the conceptually distinct forms.

2.4 Origins of Computing Technologies in Cooperative Work

Electronic computing technologies initially arose from the development of technologies of large-scale calculation work in science and engineering, on one hand, and in administrative work organizations on the other.

The progressive forms of work organization discussed above also appear as a recurring theme in the development of the practices of computing. For a couple of centuries the development of these technologies followed the pattern of development we have dubbed progressive forms of work organization. While this might be taken as proof that these forms are indeed universally necessary forms of development, the banal truth may be simply that managers of large-scale computing work knew of the ‘putting out’ system and the systematic division of labor of the manufactures and applied these as tested technologies. Whatever it is, the major milestones are the following.

2.4.1 Division of ‘Mental Labor’

In the first year of the French Revolution the new revolutionary regime decided to scrap the received systems of measurement and introduce a new and conceptually coherent system based on the decimal notation. The motive was no secret: it was an intervention to ensure that the myriad of local measurement systems did not pose obstacles for the new regime in its need for raising taxes: ‘At that time, each region was free to establish its own set of measures. Local officials easily manipulated these measures to their own advantage in a number of ways. Commonly, they could keep a large measure to collect taxes of grain and produce but reserve smaller measures for the payment of their own debts’ (Grier, 2005, pp. 33 f.). On top of these mundane motives, however, the new metric system, as it is now known, should be devised and presented in such a way as to demonstrate the *grandeur* of the revolutionary regime. Hence the new metric system would only allow decimal fractions for the sub-divisions of the units of measure 'of whatever type', and accordingly the quadrant of a circle and angular measures were also to be made to conform to this rule.

The task of producing these table was assigned to the director of the *Bureau du cadastre* at the *École des ponts et chaussée*, Gaspard de Prony, who later related that this implied that 'all existing trigonometric tables, whether presented in natural or logarithmic form [. . .] became useless; and it was found necessary to calculate new ones' (de Prony, 1824). Moreover, also in the name of *grandeur*, de Prony 'was engaged expressly not only to compile tables which left nothing to be desired about their accuracy, but also to make of them "a monument to calculation the greatest and the most impressive that had ever been executed or even conceived"'. Adding that these were 'the exact expressions that were used in the brief' he was given, de Prony specified that this meant that the tables would have to be extended and calculated to 14 or 15 decimal places instead of 8 or 10.

De Prony accepted the assignment 'unconditionally' but, realizing that he 'could not hope to live long enough to finish the project', he found himself an 'embarrassment more arduous than [he] could hide'. However, a happy circumstance 'unexpectedly' helped him out of this 'embarrassment':

> Having one day noticed, in the shop of a seller of old books a copy of the first English edition 1776, of Smith's "Treatise on the Wealth of Nations", I decided to acquire it, and on opening the book at random, I came across the chapter where the author had written about the division of labour; citing, as an example of the great advantages of this method, the manufacture of pins. I conceived all of a sudden the idea of applying the same method to the immense job with which I had been burdened, to manufacture my logarithms as one manufactures pins. I have reasons to believe that, without realising it, I had already been prepared for this realisation from certain parts of mathematical analysis, on which I had then been giving tuition at the École Polytechnique. (de Prony, 1824)

De Prony may have been exaggerating somewhat in this account, given about 30 years after the event, perhaps as a rhetorical gesture, for the pin manufacture paradigm was very well known in the circles of engineers and scientists he belonged to. In fact, his teacher at *École des ponts et chaussée*, benefactor, and later immediate superior and predecessor as director at the *École*, was the very same Jean-Rodolphe Perronet who had provided the most thorough analysis of the very same pin manufacture in the Normandy that Smith had described (Bradley, 1998). Be that as it may, de Prony certainly applied the principles of manufactures to accomplish the task at hand.

In a way, the division of labor had already been applied some 50 years before by Alexis-Claude Clairaut in an effort to calculate the orbit of the comet Halley had identified in 1682 and thereby its next perihelion. The task was massive because the calculation of the orbit of the comet required a solution to a 'three body problem', as the orbit is influenced by the gravitational fields of large bodies such as the Sun, Saturn, and Jupiter. Halley did not manage to arrive at a satisfactory solution but Clairaut invented a method of dividing the calculation in such a way that it could be performed in a system of division of labor. Together with two friends, Joseph-Jérôme Lalande and Nicole-Reine Lepaute, Clairaut launched on the massive task. Almost 5 months later, in November 1758, they were able to publish the prediction that the next perihelion would occur on 13 April 1759 (Grier, 2005). Although the prediction was 1 month off the mark, the principle of performing massive computations in parallel, as a cooperative effort based on systematic division of labor, was picked up and refined for similar purposes. Lalande himself, from 1759 tasked with calculating astronomical tables published annually by the *Académie des sciences* under the title *Connaissance des temps*, employed a small number of skilled 'computers working out of their own homes' to do so (Croarken, 2003). In the UK, Nevil Maskelyne, the Astronomer Royal at Greenwich, similarly tasked with the provision of British sailors with a practical technique of determining the longitude, advocated a method (the 'lunar distance method') that also required annual calculation and publication of tables (in an *Nautical Almanac*), and he thus had to devise a computing system that would enable him to accomplish this as a practical task. He was familiar with the form of work organization that had been developed by Lalande (whom he knew well) and adopted this 'distributed structure, albeit on a slightly larger scale': 'Maskelyne proposed employing a number of computers, each of whom was to undertake the complete set of calculations for specified months of the *Nautical Almanac*', and for that purpose he 'designed a distributed system using relatively skilled workers and which had more in common with cottage industries such as lace making than in the factory manufacture of pins' (Croarken, 2003, p. 52). Maskelyne provided not only paper and ink, but also 'computing plans', that is, instructions that were written on one side of a sheet of folded stationery and that would summarize each step of the calculation. 'On the other side of the paper he drew a blank table, ready for the computer to complete' (Grier, 2001, p. 30).

De Prony, who knew Maskelyne well and would have been familiar with the British arrangement of mass-calculation work (Grier, 2001, p. 35), did not have to start from scratch. However, whereas the previous experiments in cooperative calculation based on division of labor remained entrenched in artisanal work, as in the example of calculating the perihelion of Halley's comet, or in the 'putting out' system, as in the examples of calculating astronomical tables, de Prony went all the way, so to speak, and adopted the form of full-fledged manufactures. He devised 'his new manufacture', as he later called it, as a 'system of the division of labour' in three sections: the first was composed of four or five 'geometricians of very high merit' that were given the task of choosing the mathematical formulae to be used for calculation and checking; the second section was composed of 'calculators, who possessed a knowledge of analysis', and whose task it was to construct the 'spreadsheets' to be filled in by the members of the third section. The sheets were divided into 100 intervals, with the numbers of the top line was provided by the 'calculators' of the second section.

> This third group comprised no less than seventy or eighty individuals; but it was the easiest to form, because, as I had foreseen, they did not need, in order to be admitted [to this group] any preliminary instruction; the one essential condition, for their admission [to the group], was for them to know the first two rules of arithmetic [. . .]. The ninety-nine remaining lines were then filled in by means of purely mechanical operations carried out by the 3rd section, each of whom was performing 900 to 1000 additions or subtractions per day, nearly all of whom not having the least theoretical notion on the work which they were doing. (de Prony, 1824)

The claim that the computers were only required to master addition and subtraction is emphasized by the fact that the ranks of the third group were staffed by former 'hairdressers', that is, wig dressers. They were in deep trouble after the aristocratic fashion of wearing wigs suddenly had become a hazardous one (Grattan-Guinness, 1990). De Prony said almost as much when he in his account decades later said 'that many among them came to seek and find, in this special workshop, a safeguard, a refuge which, happily, was not violated, and that the political circumstances of that time rendered these fully necessary' (de Prony, 1824). The 'calculators' certainly had no prior understanding of interpolation but that did not prevent the cooperative effort from working very efficiently, producing 700 results per day (Grattan-Guinness, 1990). When the project was completed, in 1801, the concerted work of the former hairdressers had produced 'about 2,300,000 numbers, of which 4 or 500,000 consisted of 14–25 digits' of which '99%' had been calculated 'by means of a manufacturing procedure'. The tables were ultimately collected in 'seventeen grand in-folio volumes' and were deposited at the Paris Observatory. The quality of the work of the calculators was as flawless as it gets. Having explained that 'All the calculations were done twice: expedite means of verification, but very rigorous, were prepared in advance', de Prony emphasized that he 'noticed that the sheets the most exempt from error were, in particular, furnished by those who had the most limited intelligence [education], an existence, so to speak, "automatique"'(de Prony, 1824). The former hairdressers were thus working in an arrangement very similar to that of the workers in the pin manufactures: they mastered their local task fragment but not the larger scheme as conceived by de Prony and his master planners.

The historical role of de Prony's calculation manufacture is similar to Adam Smith's pin manufacture case, although it has not been as dramatic and hyped. First of all, the cooperative arrangement of calculation organized by de Prony became widely known, not least due to the prominence given to it by Charles Babbage. In 1821 Babbage and his friend John Herschel undertook to produce a set of mathematical tables for the British *Nautical Almanac*. Organizing the work in accordance with the principles devised by Maskelyne, Babbage and Herschel employed two skilled calculators under the 'putting out' scheme.—Now, to understand the work the two men were engaged in, one should be aware that mathematical tables were of enormous practical importance in the emerging industrial economy but were also ridden with calculation and typesetting errors. The *Nautical Almanac*, a set of tables that from the point of view of maritime safety was 'crucially significant', 'contained at least one thousand errors, while the multiplication tables its computers used sometimes erred at least 40 times per page' (Schaffer, 1996, p. 277). Herschel later commented that 'an undetected error in a logarithmic table is like a sunken rock at sea yet undiscovered, upon which it is impossible to say what wrecks may have taken place' (Swade, 2003, p. 157). In short, assured reliability of calculation was of critical economic and social importance.—So, while Babbage and Herschel in 1821 were engaged in the tedious task of proofreading the manuscripts they had received from their two calculators, it was suggested by one of them, 'in a manner which certainly at the time was not altogether serious, that it would be extremely convenient if a steam-engine could be contrived to execute calculations for us, to which was replied that such a thing was quite possible, a sentiment in which we both entirely concurred' (Babbage, 1822, quoted in Campbell-Kelly, 1994b, p. 14).

The shared sentiment should be understood on this background. At that time, in 1821, Babbage and Herschel knew of de Prony's work. A few years earlier, in 1817, both of them had signed a letter recommending de Prony's appointment to the British Royal Society (Bradley, 1998, p. 209), and in 1819, the two of them had visited Paris, and during this trip Babbage was able to inspect, to some extent, the *Tables du cadastre* that—in spite of having been produced 20 years earlier—still had not been printed (for financial reasons). During a visit at the designated publisher of the tables, Didot, Babbage was able to see typeset pages and was given a copy of the section of the sine tables (Schaffer, 1996, p. 278).

Anyway, shortly after realizing that calculations could be realized 'by steam', Babbage began to design an experimental prototype to demonstrate the feasibility of mechanical production of tables on the basis of the method of finite differences (with which he was 'completely familiar' well before his visit to Paris, cf. Lindgren, 1987, pp. 44 f.). We do not know to which extent he in this effort took de Prony's table manufacture as a guide or model, but when he in June 1822 presented the prototype of the Difference Engine to the public—he did that in an open letter to the resident of the Royal Society to obtain official support (Babbage, 1822)—Babbage used de Prony's *Tables du cadastre* as a proof of concept: if a carefully arrangement of workers, based on division of labor, can produce sophisticated mathematical tables of high quality by means of straightforward but repeated addition, then a properly designed machine could do the same, thus making 'the intolerable labour and fatiguing monotony of a continued repetition of similar arithmetical calculations' redundant while at the same time reducing the required labor force by about 88%.

Babbage is often described as one of the earliest pioneers of mechanical computing. His work is of course well-known already and this is not the place for an account and discussion of his impressive *oeuvre*.[20] But from a practical point of view, Babbage's work on the Difference Engines and the Analytical Engine could be seen as a wasted effort. In spite of the large sums that were invested in their design and construction, none of the projects were brought to completion. However, his work is of course of great historical interest in its own right. First of all, it had direct influence on the design of a series of difference engines that were built and used over the next years by engineers such as Scheutz, Wiberg, Grant, and Hamann, and in contrast to Babbage's designs, some of these machines were actually used (Lindgren, 1987). But on balance, the approach developed by Babbage turned out to be of marginal practical utility (Williams, 2003). Or in the words of Alan Bromley, 'That these were not extensively used or developed, despite the apparent complete success of the Wiberg machine, indicates that the entire idea was not well judged.

[20]For general descriptions of the development of Babbage's work, cf. his autobiography (Babbage, 1864, chapters V, VII, and VIII), the host of studies of history of technology devoted to Babbage (e.g. Collier, 1970; Lindgren, 1987; Bromley, 1990; Schaffer, 1996), as well as a few reliable popular biographies (e.g., Swade, 2000).

The sub-tabulation task, though laborious, was not the dominant mathematical task in the preparation of tables nor, with adequate organization and management, was it of overwhelming practical importance' (Bromley, 1990, p. 96). One is thus led to the conclusion that the 'fruits of Babbage's considerable genius' were 'effectively wasted as far as practical influence is concerned' (ibid., p. 97).

One should of course not underestimate the moral example of his projects. They demonstrated to computing researchers in the twentieth century (such as Howard Aiken and Vannevar Bush) that complex computation by means of automatic digital artifacts was feasible. However, the inspiration has never been transformed into anything technologically specific. In fact, it is when 'we come to examine the facilities available for programming the Analytical Engine that Babbage's designs begin to look strange to modern eyes' (Bromley, 1990, p. 87). 'The conclusion seems inescapable that Babbage did not have a firm command of the issues raised by the user-level programming of the Analytical Engine. It would be quite wrong to infer that Babbage did not understand programming per se.' In so far as programming is concerned, his focus was what we now call microprogramming and it was from this base that Babbage explored the ideas of user-level programming. However,

> The issues of data structuring simply did not arise at the microprogramming level. There is some evidence to suggest that Babbage's ideas were moving in the directions now familiar in connection with the control mechanisms for loop counting in user-level programs. Had an Analytical Engine ever been brought to working order, there can be no doubt that Babbage's programming ideas would have been developed greatly. (Bromley, 1990, p. 89)

In other words, if one considers the contribution of the Babbage engines narrowly from the point of view of the sophisticated technicalities and divorced from its use, one is likely to miss the fact that the technology he developed was not used and that he did not arrive at the point where use issues did arise.

The real importance of de Prony's example is not its probable impact on de development of Babbage's engine designs. It was, in the words of Babbage, 'one of the most stupendous moments of arithmetical calculation which the world has yet produced' (Babbage, 1822, p. 302) and in his *On the Economy of Machinery and Manufactures* he not only gave a detailed account of de Prony's accomplishment but he did so under the heading 'On the division of mental labour' (Babbage, 1832, §§ 241–247), stating the conclusion to be drawn from de Prony's example sharply in the opening sentence of the chapter: 'We have already mentioned what may, perhaps, appear paradoxical to some of our readers that the division of labour can be applied with equal success to mental as to mechanical operations, and that it ensures in both the same economy of time.' (Babbage, 1832, § 241). To see the historical importance of this, one should know that it was this book, not the ill-fated design projects, that defined Babbage's reputation among his contemporaries, and it was reprinted many times and translated to a large number of European languages. He was, in fact, better known as an economist than as a technologist. His book was based on his extensive and conscientious field work in the textile manufacturing districts of England and on the Continent undertaken in course of the 1820s, and as pointed out by the historian Campbell-Kelly in his introduction to Babbage's autobiography (1994b), the *Economy of Machinery and Manufactures* is generally seen as a 'being in the direct line of descent' from Adam Smith's *Wealth of Nations* to Frederick Winslow Taylor's *Principles of Scientific Management* (1911).

Accordingly, to the generations of scientists, economists, managers, and workers who read Babbage's *On the Economy of Machinery and Manufactures*, de Prony's example was the 'proof of concept' that the principle of division of labor, which they of course knew from Adam Smith and from their daily work, could be applied equally well to 'mental labour'. Cooperative work based on advanced division of 'mental labor', as devised by de Prony in the wake of the French Revolution, became wide-spread in course of the nineteenth and early twentieth centuries. It not only became the standard way of handling the increased load of scientific and engineering calculation (Grier, 2001), but it also, with the rise of large-scale financial and industrial corporations, became the predominant way of organizing administrative work in such settings. That is, the real importance of de Prony's example lies in the model it provided for the organization of mental work in the next two centuries: the organization of 'human computers' and 'calculators' in scientific and engineering laboratories, in insurance companies and accounting offices, in inventory management and production planning in manufacturing, and so forth. (For a description of a classical case, the Railway Clearing House, cf. Campbell-Kelly, 1994a). Babbage's direct assault on the mechanization of mental labor came to naught. It was de Prony's scheme that ruled the day for more than 150 years.

For a century after de Prony's calculation manufacture and for more than half a century after the difference engines designed on the Babbage model, calculation work remained strictly 'manual', as one is awkwardly tempted to call it, meaning that all operations are performed by mind and hand, assisted by the use of pen and paper and perhaps an abacus or a slide rule.

The technology of mechanical calculation machines only matured in the course of the nineteenth century and only matured at a glacial speed. For although mechanical calculating machines date back to the eighteenth century (e.g., Schickard, ca. 1620; Pascal, 1642; Leibniz, 1674), we should not (again) be misled by the chronology of inventions and disregard technology in actual use. As pointed out by a historian of computing, 'Mechanical calculating machines were essentially useless toys during the first two centuries of their development. The level of [metalworking] technology of the day guaranteed that any attempt to produce a reliable, easy to use instrument was doomed to failure.' (Williams, 1990, p. 50). The 'first machine that can be said to have been a commercial success' was a calculator created by Thomas de Colmar in 1820, but the technology of mechanical calculation only stabilized late in the nineteenth century with the development of the Baldwin-Odhner calculator (patented 1875) that offered a practical and robust solution to the carry issue (a variable-toothed gear). The most famous design based on this technology was perhaps the calculators produced by the Brunsviga company in Germany from 1892. The scope and level of automatic control remained quite narrow and low, however; restricted to, for instance, control of carry operations in addition tasks.

As in the cotton trades 100 years earlier, mechanization of 'mental labor' was an incremental process.

Mechanical calculation machines were operated in isolation from each other, by the individual part-worker, as a means of speeding up the tedious task of doing massive arithmetical tasks. The reason for this is that the level of automatic control of operations was so rudimentary (automatic carry was the overwhelming issue) that one can safely say that 'In in these machines the control function was provided by the human operator' (Bromley, 1990, p. 59). They were little more than sophisticated tools. In these conditions, mechanical calculation machines did not permit the operator to escape the subjection to specialization in performing a specific operation (or a narrow range of related operations) and take on the role of a supervising the operations of the machine. The devices were simply incorporated into the received division of labor as a means for increasing the speed at which individual operations could be performed.

2.4.2 Mechanization of 'Mental Labor'

Calculation in administrative work began to become mechanized around the beginning of the twentieth century, with the invention and dissemination of punched-card tabulators (and associated punching equipment, sorters, printers, etc.). Tabulating machines were soon equipped with plugboards or switchboards, so that the machines could be reconfigured to handle other tasks and card formats.

Invented for use in the processing of massive statistical data (the US census 1890), punched-card machinery quickly became appropriated and used by railroad and utilities companies as well as by manufacturers and government agencies. However, because of their costs, punched-card tabulators were generally confined to use in settings such as these that were in need of (or could exploit) large-scale data processing:

> Punched-card machinery was expensive to rent and consequently was only used, at first, by very large organizations that could make good use of its ability to make short work of a large volume of transactions; the needs of small businesses could be met adequately by less automatic but lower-cost bookkeeping machines, such as those made by Burroughs. The Hollerith [punched-card] machines, however, arrived at a critical period in the development of large-scale American enterprise; it was during this period in the late nineteenth and early twentieth centuries that much of modern business accounting practice came into existence, particularly cost accounting in manufacturing. (Campbell-Kelly, 1990, p. 145)

As large-scale economic organizations evolved, the use of punched-card tabulating machinery became widespread. By 1913 a journalist reported that

> the system is used in factories of all sorts, in steel mills, by insurance companies, by electric light and traction and telephone companies, by wholesale merchandise establishments and department stores, by textile mills, automobile companies, numerous railroads, municipalities and state governments. It is used for compiling labor costs, efficiency records, distribution of sales, internal requisitions for supplies and materials, production statistics, day and piece work. It is used for analyzing risks in life, fire and casualty insurance, for plant

> expenditures and sales of service, by public service corporations, for distributing sales and cost figures as to salesmen, department, customer location, commodity, method of sale, and in numerous other ways. The cards besides furnishing the basis for regular current reports, provide also for all special reports and make it possible to obtain them in a mere fraction of the time otherwise required. (quoted in Campbell-Kelly, 1990, p. 145)

That is, with the use of tabulating machinery important sections of administrative work assumed the character of the machine-based factory.

It is of some relevance here to note that, in some instances, the technology was used also in the coordination of cooperative work, in a manner that in some ways anticipates *kanban* systems, as opposed to off-line administration of work settings and processes. In the 1920s and 1930s 'the increasing variety of styles, colors, and options began to slow production and delay delivery', and 'automobile companies turned to the use of tabulator machinery to overcome these delays' (Norberg, 1990, p. 774). Norberg describes an innovative use of tabulating machinery in Chrysler Corp. for purposes of coordination:

> Upon receipt of a dealer's order, two cards were punched with the essential information supplied by the dealer: routing, region, district, dealer's name, order number, item number, model, body type, paint, trim, wheels, transmission, radio, heater, and other options. One card went to the equivalent of a production planning office where a "Daily Master Building Schedule" was prepared, and one went to the car distribution department where it was filed according to region and dealer. Multiple copies of the production card went to the various inventory-control points for parts, while several copies stayed with the car as it was constructed. When the car reached the shipping department, one of the last cards remaining was checked to see that the order was correct. If so, the car was shipped and the dealer was notified when to expect it. (Norberg, 1990, p. 774)

In this case the punched-card is not simply as used as a record of an event to be processed at a later stage for secondary use, e.g., for statistical purposes, for purposes of payment, etc. The card is a mechanically generated coordinative artifact that provides the various stations in the large-scale network of activities with appropriate information about the particular order.

Like desk-top calculators before them, tabulating machinery was and remained stand-alone machines, with operators supervising the operation of the individual machines and handling the transfer of cards between machines: punchers, sorters, tabulators, printers. On the basis of stacks of discrete cards a higher degree of automatic integration of operations was not feasible, but the technology remained in use until the cost of electronic computing made it a viable option for ordinary work settings to move beyond the confines of punched-card tabulator technology. By the late 1960s traditional punched-card machines had effectively gone out of production, and by the late 1980s punched cards had all but vanished (Campbell-Kelly, 1990, p. 151).

So, although component technologies of punched-card tabulating were appropriated and used in the first generations of electronic computers (punched cards, card readers, printers, plugboards, etc.), the electronic digital computer did not grow out of this technology, nor did it grow out of the needs of administrative work. The first generations of electronic digital computers were designed and built for massive scientific and engineering calculation.

While administrative work became mechanized, the mechanization of scientific and engineering calculation, remained sporadic and fragmentary. This lasted until the Second Work War, still relying on desktop calculators. In a few large-scale research settings that could afford the cost, punched-card tabulators were appropriated for the purposes of scientific and engineering calculation (e.g., Snedecor, 1928; Eckert, 1940; McPherson, 1942), but in general human computers were still tasked with calculating by rote in accordance with a systematic method supplied by an overseer.

Donald Davies, who was to play a key role in the development packet-switched digital networks, recalls from his work as a young scientist in the UK during the Second World War:

> The Second World War saw scientific research projects of a size and complexity that reached new levels. Underlying much of the work were complex mathematical models, and the only way to get working solutions was to use numerical mathematics on a large scale. In the Tube Alloys project, for example, which became the UK part of the Manhattan Project to make a fission bomb, we had to determine the critical size of a shape of enriched uranium and then estimate mathematically what would happen when it exploded. For this problem we used about a dozen "computers" – young men and women equipped with hand calculators (such as the Brunsviga). These human computers were "programmed" by physicists like myself. (Davies, 2005, p. vii)

This setting was not a unique. As Davies puts it, the 'same story, with different physics and different mathematics, was repeated in many centres across the United Kingdom'. In the words of Jack Copeland, 'The term "computing machine" was used increasingly from the 1920s to refer to small calculating machines which mechanized elements of the human computer's work. For a complex calculation, several dozen human computers might be required, each equipped with a desktop computing machine.' (Copeland, 2006b, p. 102). However, just as the vastly increased scope of administrative data processing had put the calculation manufacture form of work organization under increasing pressure and thus engendered the rapid growth of punched-card technologies, the scale of calculations required in modern science and engineering caused similar tensions to arise: 'By the 1940s, [. . .] the scale of some of the calculations required by physicists and engineers had become so great that the work could not easily be done with desktop computing machine. The need to develop high-speed large-scale computing machinery was pressing.' (Copeland, 2006b, p. 102). In short, in the domains of science and engineering too, cooperative calculation work, organized on the de Prony model, had exceeded its capacity for further development.

In sum, it was the problems facing the cooperative efforts of human computers in science and engineering that motivated the first significant steps towards electronic digital computers, in particular the Colossus, designed by Thomas Flowers and Max Newman at Bletchley Park in the UK during 1943 for breaking the encrypted messages produced by the German *Geheimschreiber*, a family of sophisticated teletype cipher machines used for strategic communication within the Nazi military leadership (Copeland, 2006a; Gannon, 2006),[21] and the ENIAC, designed in the US in 1944–1945 for calculating projectile trajectories (Goldstine, 1972; Van der Spiegel et al., 2000). The need for similar applications motivated the subsequent series of experimental stored-program computers such as, in the US,

- the EDVAC, 1945–1952, designed by John Mauchly, Presper Eckert, and John von Neumann (von Neumann, 1945; Eckert, 1946);
- the Princeton IAS computer, 1946–1952, designed by von Neumann (Aspray, 2000), etc.,

and in Britain,

- the ACE, 1945–1950, designed by Turing and Wilkinson (Turing, 1945; Turing and Wilkinson, 1946–1947; Turing, 1947; Copeland, 2005);
- the Manchester Mark I, 1946–1948, designed by Newman (Napper, 2000); and
- the EDSAC, 1946–1949, designed by Maurice Wilkes.

(For general accounts of these efforts, cf. Augarten, 1984, Chapters 4–5; Campbell-Kelly and Aspray, 1996, Chapter 4)

If we take Turing's ACE as an example, the motivation was clearly laid out. In his proposal, written towards the end of 1945. Turing opened the report by stating:

> Calculating machinery in the past has been designed to carry out accurately and moderately quickly small parts of calculations which frequently recur. The four processes addition, subtraction, multiplication and division, together perhaps with sorting and interpolation, cover all that could be done until quite recently [. . .]. It is intended that the electronic calculator now proposed should be different in that it will tackle whole problems. Instead of repeatedly using labour for taking material out of the machine and putting it back at the appropriate moment all this will have to be looked after by the machine itself. This arrangement has very many advantages.
> (1) The speed of the machine is no longer limited by the speed of the human operator.
> (2) The human element of fallibility is eliminated, although it may to an extent be replaced by mechanical fallibility.
> (3) Very much more complicated processes can be carried out than could easily be dealt with by human labour.
> Once the human brake is removed the increase in speed is enormous. (Turing, 1945, p. 371)

The same motivation was underscored when Charles G. Darwin, the director of the UK National Physical Laboratory, in April 1946 wrote a memorandum in which he argued the case for building the computer proposed by Turing:

> In the past the processes of computation ran in three stages, the mathematician, the [human] computer, the machine. The mathematician set the problem and laid down detailed instructions which might be so exact that the computer could do his work completely without any understanding of the real nature of the problem; the computer would then use the arithmetical machine to perform his operations of addition, multiplication, etc. In recent

[21] The British government kept the very existence of the Colossus secret until 1975, and its function was not publicly known until 1996 when the US Government declassified documents, written by US liaison officers at Bletchley Park during the war, in which the function of the Colossus was described. However, a 'vital report' (Good et al., 1945) was only declassified in June 2000 (Copeland, 2006c).

> times, especially with use of punched card machines, it has been possible gradually for the machine to encroach on the [human] computer's field, but all these processes have been essentially controlled by the rate at which a man can work. (Darwin, 1946, p. 54)

Darwin went on by stressing that 'The possibility of the new machine started from a paper by Dr. A. M. Turing some years ago when he showed what a wide range of mathematical problems could be solved, in idea at any rate, by laying down the rules and leaving a machine to do the rest' (Darwin, 1946, p. 54).

Computing technology as represented by these pioneering calculating machines were designed to eliminate the 'human brake', that is, the cooperative work of human computers and punched-card operators, just as automatic machine systems a century previously, in other domains of work, had eliminated the cooperative work of workers in paper mills, etc., while of course constituting cooperative work of an entirely different sort, performed by machine operators and technicians.

The first electronic digital computers such as the Colossus and the ENIAC were not stored-program computers. They were specifically designed for performing massive calculations, not as general purpose computers. Thus, to facilitate configuration and reconfiguration the machines were equipped with plugboards similar to those used in punched-card tabulators and, in the case of the Colossus, also switches. However, the configuration work was tedious and the cost in terms of time significant. For example, configuring the ENIAC by plugging cables, 'its users were literally rewiring the machine each time, transforming it into a special-purpose computer that solved a particular problem'; and, consequently, it took 'up to 2 days' to configure the ENIAC to solve a new problem, which it might then solve in a minute (Ceruzzi, 1990, p. 241). Moreover, the ENIAC had been designed for calculating projectile trajectories and was not particularly suited for other types of massive calculation such as solving partial differential equations (Campbell-Kelly and Aspray, 1996, p. 91). The impetus to develop stored-program computers came from such limitations.

The stored-program digital computer technology (e.g., EDVAC and ACE) had been developed, as a 'universal' or 'general' technology of large-scale calculation machinery that was far less expensive and far more flexible than building series of *specialized* calculation machine systems. But it was then—in one of those lateral shifts in which a technology developed for one domain of work is picked up and appropriated for another domain—gradually and hesitatingly transformed and appropriated for administrative purposes.

The application of stored-program electronic computers in work settings only began in the 1950s. The first 'business' application, a payroll program, ran on 12 February 1954, on the then newly finished LEO computer (based on the Manchester Mark I architecture), calculating the wages of bakery staff of the Lyons teashop chain in the UK (Ferry, 2003). This application is typical for the use of electronic computers for business purposes, that is, for economic, commercial, organizational, managerial, etc., purposes (beyond applications of scientific calculation): batch processing of large numbers of transaction records.

The stored-program electronic computer was appropriated for business administration purposes as a substitution technology; that is, the new computers were designed for automating the work of the central computing departments of large-scale organizations, replacing entire batteries of punched-card tabulators by a single computer such as the IBM 1401. Most of the computer systems installed in commerce and government during the 1950s and 1960s were 'simply the electronic equivalents of the punched-card accounting machines they replaced.' (Campbell-Kelly and Aspray, 1996, p. 157). The punched-card technology had helped to shape the organization of business, and by the 1950s and 1960s

> the highly centralized accounting systems of industry were very much geared to what was technically achievable with the commercially available machines, and a generation of accountants between the two world wars grew upon a diet of the standard textbooks on mechanized accounting. When computers became available in the 1950s and 1960s, they tended to be used at first as glorified electric accounting machines and were simply absorbed into old-fashioned accounting systems. (Campbell-Kelly, 1990, pp. 146 f.)

What characterized this computing technology was automatic processing (recording, sorting, merging, aggregating, calculating, printing) of data concerning economic activities: statistical analysis (actuarial data, sales analysis), invoicing, sales reports, payroll, inventory control, financial reporting. That is, this was a technology developed and used for administrative and logistical purposes in ordinary business settings (payroll calculation, production planning). The computer systems were used for performing various house-holding tasks in organized cooperative work settings but the interdependent activities of the cooperating workers were neither facilitated by the system, nor mediated in and through the system, nor regulated by the system: one worker's actions were not effectuated and propagated to other workers by means of the system. The system remained 'outside' of the practices and settings whose economic transactions it was processing.

2.5 Facilitation of Cooperative Work: Real-Time Computing

Parallel to the development of these technologies of automatic handling of administrative calculation, an entirely different computing technology was being developed that directly addressed the facilitation of cooperative work, namely 'online' 'real-time' computing systems such as air defense systems (SAGE) and airline reservation systems (SABRE). With this technology computational machine systems were constructed that would constitute the common field of work of multiple cooperating actors interacting 'in real time'.

2.5.1 Project Whirlwind

The increased role of airplanes in warfare in World War II caused two bottlenecks: testing new airplane designs and training crews for them were costly and caused intolerable delays: 'in 1943 it was taking far too much time and money to train flight crews to man the more complex, newer warcraft in production, and it was taking far too much time and money to design high-performance airplanes' (Redmond and Smith, 1980, p. 1). The obvious path of developing a particular flight simulator for each particular airplane model was not sustainable, as it would lead to increased costs of ‘providing a new and different flight trainer for each warplane model in combat use’. In 1943, this prospect led researchers at MIT’s Servomechanisms Laboratory together with US Navy planners to develop the alternative strategy of developing configurable flight simulators that could match the flight characteristics of any particular airplane design: ‘a protean, versatile, master ground trainer that could be adjusted to simulate the flying behavior of any one of a number of warplanes’ (Redmond and Smith, 1980, p. 2). The project, named Whirlwind, was undertaken in 1944 under the leadership of Jay W. Forrester.[22]

To realize the idea of a ‘protean and versatile’ simulator, the simulator should incorporate a computer and, what is more, a computer with the capacity to process incoming data and solve the system of differential equations at a rate that was sufficient to match the rate of external state changes. That is, the computer should be able to handle external events rapidly enough to be ready for the next external event or ‘in real time’.

Forrester initially opted for a design based on an analog computer, but it was clear that an analog computer ‘would not be nearly fast enough to operate the trainer in real time’ (Campbell-Kelly and Aspray, 1996, p. 159). However, in the summer of 1945 he learned that digital computer technology was a viable option. He learned this from another MIT student, Perry Crawford, who had developed some initial concepts for real-time process control based on digital electronic calculating systems. Noting that it had ‘recently’ been proposed at MIT ‘that electronic calculating systems can perform a valuable function in fire-control operations’, his thesis set out to ‘describe the elements and operation of a calculating system for performing one of the operations in the control of anti-aircraft gunfire, which is, namely, the prediction of the future position of the target.’ (Crawford, 1942, p. 1). At the time, the control systems for gun control and so on were still invariably based on mechanical or electromechanical technologies. What Crawford suggested was that the mathematical functions could be modelled in a digital computer that could thereby be made to control real-world processes such as tracking a moving target: ‘Crawford was the first person to fully appreciate that such a general-purpose digital computer would be potentially faster and more flexible than a dedicated analog computer’ (Campbell-Kelly and Aspray, 1996, p. 160). Crawford explained all this to Forrester in 1945 and, as Forrester later put it, it ‘turned on a light in [my] head’ (ibid.).

The stored-program computer technology was made the foundation of the subsequent development work in Project Whirlwind (by virtue of access to the ongoing EDVAC design work, cf. Goldstine, 1972), and as the project progressed, the

[22]The following account of Whirlwind and its legacy, the SAGE system and beyond, is based on the insightful studies by O’Neill (1992) and by Campbell-Kelly and Aspray (1996). The two volumes by Redmond and Smith (1980, 2000) offer a detailed and accurate account; however, the information that is relevant from the perspective of technology-in-practice has to be dug out from an account that is overwhelmingly focused on issues of research governance and project management; this makes this major piece of research less immediately useful for researchers with CSCW or HCI interests and concerns.

objective of building a flight simulator receded into the background; instead, the effort focused increasingly on the challenge of building a real-time digital computer system (Crawford, 1946). 'The two young men quickly realized that they would not be developing simply a sophisticated flight trainer. Instead, they had stumbled onto a design concept so fundamental that its generality of application was almost staggering to contemplate' (Redmond and Smith, 1980, p. 217).

A major technical issue in developing Whirlwind was the speed and reliability of computer memory. Consequently, significant effort was devoted to developing the new technology of magnetic core memory, based on a web of tiny magnetic ceramic rings. Core memory technology, which did not become operational until the summer of 1953, made Whirlwind 'by far the fastest computer in the world and also the most reliable' (Campbell-Kelly and Aspray, 1996, p. 167). It was a 'monumental achievement' (O'Neill, 1992, p. 13).[23] The operational speeds offered by core memory was further underscored by the development for Whirlwind of

> the intricate details of "synchronous parallel logic" – that is, the transmitting of electronic pulses, or digits, simultaneously within the computer rather than sequentially, while maintaining logical coherence and control. This feature accelerated enormously [. . .] the speeds with which the computer could process its information. (Redmond and Smith, 1980, p. 217)

Whirlwind was also 'first and far ahead in its visual display facilities' which, among other things, facilitated the 'plotting of computed results on airspace maps' (Redmond and Smith, 1980, p. 216). Complementary to this feature, was a 'light gun' with which the operator could select objects and write on the display: 'As a consequence of these two features, direct and simultaneous man-machine interaction became feasible' (ibid.).

That the Whirlwind research continued and eventually became massively funded despite the fact that no stored-program digital computer existed at the time (and would not exist until the Manchester 'Baby' ran its first test 21 June 1948) was due to external events in 1949. The US intelligence services revealed that the Soviet Union had exploded a nuclear bomb in August that year and furthermore possessed bomber aircraft capable of delivering such weapons at targets in the US. Quickly a committee was put to work to evaluate the implications for US air-defense. In 1950, the committee concluded that the existing air defense system was wholly inadequate for the current situation. It candidly compared the existing system 'to an animal that was at once "lame, purblind, and idiot-like"', adding, in order not to leave readers guessing, that 'of these comparatives, idiotic is the strongest' (ADSEC Report, October 1950, quoted in Redmond and Smith, 1980, p. 172). The problem was basically that the coordination effort of the existing system severely limited its capacity. As O'Neill explains the conundrum:

> In the existing system, known simply as the "manual system", an operator watched a radar screen and estimated the altitude, speed, and direction of aircraft picked up by scanners. The operator checked the tracked plane against known flight paths and other information.

[23]Together with printed circuits, core memory made possible the serial production of computers such as the IBM 1401, announced in October 1959.

> If it could not by identified, the operator guided interceptors into attack position. When the plane moved out of the range of an operator's radar screen, there were only a few moments in which to "hand-over" or transfer the direction of the air battle to the appropriate operator in another sector. (O'Neill, 1992, p. 15)

This cooperative work organization, the 'manual system', did not scale up to meet the challenge of large numbers of aircraft with intercontinental reach and carrying nuclear weapons. The problem was systemic (Wieser, 1985). In the words of O'Neill again:

> In a mass attack, the manual handling of air defense would present many problems. For example, the manual system would be unable to handle detection, tracking, identification, and interception for more than a few targets in the range of anyone radar. The radar system did not provide adequate coverage for low altitude intrusion. The way to get around the lack of low altitude surveillance was the "gap filler" radar. Because this "gap filler" radar was limited to a few tens of miles, the system required more frequent hand-overs, and further taxed the manual-control system and its operators by reducing the time available to intercept an attacking bomber. (O'Neill, 1992, pp. 15–16)

It was concluded, then, that a radical transformation of the organization of the air-defense organization and its technical infrastructure was required. The new air defense system that was eventually built and was named Semi-Automatic Ground Environment, or SAGE, was divided into 23 Direction Centers distributed throughout the USA. Each of these centers would be responsible for monitoring the airspace of the sector and, if required, for directing and coordinating military response. The work of each Direction Center was supported by a high-speed electronic digital processing machine that would receive and process data from radar sites via a system of hundreds of leased telephone circuits. In 1950 it was decided that the SAGE computer system would be based on the Whirlwind design.

Whirlwind was, of course, an experimental system and was not fit for production. An engineered version of Whirlwind was developed which was initially simply known as Whirlwind II, but then named XD-1 until it finally, as a production version manufactured by IBM, was renamed AN/FAQ-7 (or the Q-7 as it was often called).

> The direction center itself was to be semiautomatic; that is, routine tasks would be done automatically under the supervision of operators. A high-speed digital computer would collect target reports from the radar network, transform them into a common coordinate system, perform automatic track-while-scan [...], and compute interceptor trajectories. Operators filtered the radar data, had override control (i.e., could initiate or drop tracks), performed friend-or-foe identification function, assigned interceptors to targets, and monitored engagements through voice communication with the interceptor pilots. (Wieser, 1985, p. 363)

To determine the feasibility of the plan, a 'computer-controlled collision-course interception' test was undertaken on 20 April 1951 above Bedford, Massachusetts. According to the test report by C. Robert Wieser dated 23 April 1951, 'three successive trial interceptions were run with live aircraft under control of the WWI [Whirlwind I] computer' (quoted in Redmond and Smith, 2000, p. 1). The pilot of the intercepting aircraft reported that from a distance of about 60 km (40 miles) he was brought to within 1,000 m of his target. Three days later it was decided to build a prototype of the SAGE system, 'an elaborate, multi-radar experimental system tied into Whirlwind I' (Redmond and Smith, 2000, p. 2). The development of this experiment prototype, called the Cape Cod system, proceeded in an entirely iterative manner. Robert Wieser, who was deeply involved in the development of the Cape Cod prototype as an engineer, recalls that the 'development of the new concept and its embodiment in the Cape Cod System relied heavily on iterative cycles of experiment-learn-improve. [. . .] However inelegant, the approach worked very well. The ever-present realism of radar clutter, telephone-line noise, and limited computer memory drove the development pace faster than a mathematical analytical approach could ever have done' (Wieser, 1985, p. 364).

The Cape Cod system was based on the 'engineered version' of Whirlwind, the XD-1, and was ready for experiments in 1952. It supported 30 air-force operators working next to each other at consoles equipped with CRT displays on which digitized radar data could be selected for analysis by means of a light pen (Campbell-Kelly, 2003, p. 37).

In March 1953, Robert Wieser gave a talk to visitors to a Cape Cod demonstration. Introducing them to what they were about to see, he said:

> The radar data is fed into the Whirlwind I computer at the Barta Building in Cambridge, which processes the data to provide 1) vectoring instructions for mid-course guidance of manned interceptors and 2) special displays for people who monitor and direct the operation of the system. [¶] In processing data, the computer automatically performs the track while-scan function, which consists of l) taking in radar data in polar coordinates, 2) converting it to rectangular coordinates referred to a common origin, 3) correlating or associating each piece of data with existing tracks to find out which pieces of data belong to which aircraft, and 4) using the data to bring each track up-to-date with a new smoothed velocity and position, and 5) predicting track positions in the future for the next correlation or for dead reckoning if data is missed. Once smoothed tracks have been calculated, the computer then solves the equations of collision-course interception and generates and displays the proper vectoring instructions to guide an interceptor to a target.
>
> This process is not, however, wholly automatic. The initiation of new tracks can be done automatically or manually, or both methods can be used, each in different geographical areas of the system. Also the decision as to which aircraft tracks are targets and which tracks are interceptors is made by people and inserted manually into the machine by means of a light gun. The light gun is a photocell device which is placed over the desired blip on the display scope and then sends a pulse into the computer to indicate to the computer that action (for example, "start tracking") is to be taken on that particular aircraft. The action which the machine takes is defined by manually setting a selector switch, which the computer automatically senses and interprets (for example, "handle this aircraft as an interceptor"). The human beings make decisions and improvise while the computer handles the routine tasks under their supervision. In order to facilitate human supervision, a rapid, flexible display system is required. The principal means of display is the cathode-ray tube, which can accept information very rapidly and present both symbols and geographical positions of aircraft. Flexibility is achieved by programming the computer to display various categories of information on different display cables. The human operator can switch these cables at his scope and thus select at any time the type of information (or combination of types) which he wishes to observe. (Wieser, 1953, p. 2)

From the beginning, that is, the system was planned as 'semi-automatic', that is, the computer system was designed to work in a mode radically different from the automatic calculations for which other contemporary computers were being designed. The Cape Cod prototype and the ultimate SAGE computer systems (the Q-7s) were deliberately designed for interactive computing, complete with real-time computing, graphical CRT displays, handheld selection devices (light guns, joysticks), direct manipulation.

The SAGE system was an enormous machine system, a system of 23 interconnected computers (and an equal number of backup computers) connected to a vast array of radar stations, intercept fighter aircraft, etc., that afforded a large-scale cooperative work effort encompassing a distributed ensemble of 2,300 operators. Again, O'Neill's account is very informative about the actual work at the centers:

> As many as 100 Air Force personnel used a SAGE computer in a single facility at the same time. They used an assortment of equipment to communicate to the computer, such as cathode ray tube (CRT) displays, keyboards, switches, and light guns. This equipment provided the operators with the information they needed to make decisions and provided them a way to send commands to the computer.
>
> The SAGE system was designed to make use of consoles with CRTs for displaying the tracks of radar-reported aircraft and providing visual maps to improve comprehension. Computer programs controlled the beam that created a track's image on the CRT surface by supplying the coordinates needed in drawing and refreshing the image. Light guns allowed the operators to aim at an unidentified plane image on a screen and the system would then give information about the specified image. If the operator determined that a plane was hostile, the system would tell the operator which of the many interceptors or guided missiles on hand were in the best position-to intercept it. The operators filtered the radar data, had the ability to override, performed the friend-or-foe identification function, assigned interceptors to targets, and monitored engagements through voice communication with the interceptor pilots. SAGE required visual displays, real-time responsiveness, and communication between computers. These are all elements of interactive computer use. From the beginning, SAGE was designed to replace the manual system only partially. It was semi-automatic; a human element remained an important part of the system. The splitting of tasks was seen as a good approach to the problem; machines were necessary because human operators working without computers could not make the necessary calculations fast enough to counter an attack. But the person was also very important. The computers could keep the minds of the human operators "free to make only the necessary human judgements of battle – when and where to fight". (O'Neill, 1992, pp. 19–21)

Whirlwind and its aftermath, Cape Cod and SAGE, inaugurated and defined a new technological paradigm, typically referred to as computerized real-time transaction processing systems (O'Neill, 1992, p. 22). This is a technology that facilitates workers in a cooperative effort in as much as the system provides them with a common field of work in the form of a data set or other type of digital representation (possibly coupled to facilities outside of the digital realm) and thus enables them to cooperate by changing the state of the data set in some strictly confined way:

> On-line transaction processing systems were programmed to allow input to be entered at terminals, the central processor to do the calculation, and the output displayed back at the terminal in a relatively short period of time. These systems could be used, for example, to allow several users to make inquiries or update requests, which were handled as they arrived with only a short delay. The services that could be requested were restricted to those already pre-programmed into the system. (O'Neill, 1992, pp. 22–23)

The SAGE system certainly *facilitated* cooperative work, in that state changes initiated by one worker propagated via the system to other workers, but from the point of view of CSCW, one would not categorize it as a system that *supported* cooperative work. The operators were interdependent in their work by virtue of the system (radar stations, communication lines, tracking devices, handovers) and interacted in an orderly way regulated by the system but were strictly limited in what action and interaction they could undertake in and through the system. As O'Neill points out, 'Although they could specify what information was displayed, the operators could not change the program or the situations' (O'Neill, 1992, p. 21). Thus, when a US Air Force colonel at the time characterized the SAGE system as 'a servomechanism spread over an area comparable to the American Continent', he was quite right (Mindell, 2002, p. 313).

The limits of this paradigm were evident from the very beginning. It was, for example, obvious that there would be situations where air-defense operators at Direction Centers would need to interact with colleagues, perhaps at another center, in ways different from 'those already preprogrammed into the system'. Thus, to enable operators to handle contingencies, it was realized at a very early stage in the development of the system that operators would need to be able to interact outside of the functionalities afforded by the system. Thus, in the summer of 1953, a '"man-to-man" telephone intercommunication system' was installed so as to enable operators in the Combat Center (of the Direction Center) and at some remote locations to simply talk to one another (Redmond and Smith, 2000, pp. 310 f.). That is, operators were not supported in enacting or developing coordinative practices by the computational system. The handling of contingencies had to be carried out outside of the computational system, as ordinary conversations among operators within each center or as telephone conversations among operators across centers. CSCW begins with the realization of these limitations in the Whirlwind paradigm.

2.5.2 The Whirlwind Legacy

The SAGE system had immediate and far-reaching impact on computing technologies. From the early 1960s, applications of the paradigm epitomized by SAGE such as computerized transaction processing systems for air traffic control, banking, manufacturing production planning and control, and inventory control began to upset the forms of work organization that had dominated administrative work settings for ages, either in the form of armies of 'human computers' organized on the basis of the de Prony principles or in the form of punched-card machine operators working in configurations similar to those of the cotton mills of Lancashire around 1830.

It began with airline reservation systems. The first major civilian project to exploit the Whirlwind paradigm was the SABRE airline reservation system developed by IBM for American Airlines. It was, again, a critical situation in a cooperative work setting that motivated the development effort

In 1954 a airline reservations office would come across somewhat like this portrayal of American Airline's Chicago office:

> A large cross-hatched board dominates one wall, its spaces filled with cryptic notes. At rows of desks sit busy men and women who continually glance from thick reference books to the wall display while continuously talking on the telephone and filling out cards. One man sitting in the back of the room is using field glasses to examine a change that has just been made on the display board. Clerks and messengers carrying cards and sheets of paper hurry from files to automatic machines. The chatter of teletype and sound of card sorting equipment fills the air. As the departure date for a flight nears, inventory control reconciles the seating inventory with the card file of passenger name records. Unconfirmed passengers are contacted before a final passenger list is sent to the departure gate at the airport. Immediately prior to take off, no-shows are removed from the inventory file and a message sent to downline stations canceling their space. (McKenney, 1995, p. 97)

An office like this would house about 60 reservation clerks and 40 'follow-up clerks' and 'board maintainers'. The logic of this scenery is as follows. In the 1930s American Airline had adopted a decentralized reservations system maintained at the particular flight's departure point. This system, called 'request-and-reply', involved significant coordination work, as it required agents to coordinate with the inventory control department (two messages) before confirming the reservation with a third message. In addition, passenger-specific data (name, telephone number, and itinerary) had to be recorded at the time of confirmation on a 'passenger name record card' and subsequently transmitted via telephone or teletype to the inventory control department. To reduce the cost of coordination, an amended system was introduced a decade later: until a flight was 80% sold out, agents were free to accept bookings and were only required to report actual sales, allowing passenger requests to be accommodated quickly. This reduced message volumes to half. At the same time, the inventory control department monitored sales, and when available seats decreased to a prescribed level, a 'stop-sale message' was broadcast to all agents. To make this 'sell and report' system work required a buffer of seats. As McKenney observes, 'These largely manual systems were time-consuming, requiring from several hours to several days to complete a reservation. Moreover, they were cumbersome and unreliable, plagued by lost names, reservations made but not confirmed with travelers, and inaccuracies in the passenger lists held at boarding gates, with the result that most business travelers returning home on a Friday would have their secretaries book at least two return flights.' (McKenney, 1995, p. 98)

At that point, although thousands of reservations were processed every day, the operation ran virtually without the use of machinery, apart from an electromechanical system at the inventory control department for maintaining an inventory of sold and available seats. In fact, as pointed out by Campbell-Kelly and Aspray, the reservations office 'could have been run on almost identical lines in the 1890s' (1996, p. 170). The reason for this was that existing data-processing technologies operated in batch-processing mode. The primary goal of punched-card tabulating machinery that dominated the administrative domain in large-scale business operations until this time was to reduce the cost of each transaction. This was done by completing each operation for all transaction records before the next operation on the records was begun. An individual transaction simply had to wait until an economically viable batch of transactions had been accumulated. That is, the cost of each transaction was traded off against the lapse time of processing the transaction. According to Campbell-Kelly and Aspray, 'the time taken for each transaction to get through the system was at least an hour, but more typically half a day' (ibid.). As noted above, the new business computing technologies were designed and applied as little more than advanced punched-card tabulators. What these technologies offered was not another mode of operation but batch-processing at a higher level of automation. With punched-card tabulating machinery human involvement was required in the transition from one batch process to the next: cards had to be picked up and carried from, say, sorting to tabulating to re-sorting. But with computers equipped with tape stations this vestige of human intervention could be eliminated. When the transaction represented by the punched cards had been 'read' by the system's card reader, the worker could stand back and supervise the entire process. For most businesses, batch processing was a cost-effective mode of operation; it was how accounting offices updated accounts, banks processed checks, insurance companies issued policies, and utility companies invoiced clients. But the airline industry, by contrast, had 'instantaneous processing needs', and trading transaction cost off against lapse time was not an option.

Hence the overwhelmingly manual character of the entire reservations operation by 1954. But in the 1950s the volume of traffic and the number of flights increased. Thus 'the ability to process passenger reservations assumed increased importance. Given the highly perishable nature of the airlines' product, reservation personnel needed to have rapid access to seat availability, be able to register customer purchases instantly, and in the event of a cancellation quickly recognize that a valuable item of stock had been returned to inventory and was available for resale.' (McKenney, 1995, pp. 98 f.). Consequently, operating the reservations office as a manufactory was no longer viable. The 'boards became more crowded' and the clerks 'had to sit farther and farther away from them'. So, by 1953 'American Airlines had reached a crisis.' (Campbell-Kelly and Aspray, 1996, pp. 170 f.).

Having learned of the real-time transaction processing technology that was then under rapid development, American Airlines decided to undertake a thorough transformation of its reservation system based on this paradigm. The new system was to be developed by IBM (where Perry Crawford had been employed in 1952 to develop real-time applications). The project, later dubbed SABRE, was formally established in 1957 and was the 'at the time easily the largest civilian computerization task ever undertaken'. The system was implemented in the early 1960s on IBM 7090 computers, which were for all practical purposes solid-state versions of the Q-7. The final system, which was fully operational in 1964, connected some 1,100 agents using desktop terminals across the US, who had access to 'a constantly updated and instantly accessible passenger name record' containing various information about the passenger, including telephone contacts, hotel and automobile reservations, etc. (Campbell-Kelly and Aspray, 1996, pp. 172–174; Campbell-Kelly, 2003). As in the SAGE system, the SABRE system facilitated cooperative work: one worker's action on computational objects (or structured data sets) represented in the system in some form, e.g., his or her entering a flight reservation (name, date, flight number, etc.), is stored (and perhaps transmitted to another center) to allow other workers (perhaps elsewhere) to perceive and act on this information. In other words, the system mediates the propagation of changes to the state of the common field of work of the many workers involved in the large-scale cooperative effort of handling flight reservations.

In this paradigm the computational system does not support articulation work. What characterizes these systems is real-time online transaction processing. 'Transaction processing allows a person to interact with the system in ways that are limited and predetermined' (O'Neill, 1992, p. 2). Whatever might be required to handle a given task in addition to and beyond what the transaction contains, e.g., sorting out ambiguities, mistakes, etc., is done outside of the system, through some other medium, e.g., by telephone. In short and rather schematically, real-time online transaction processing systems facilitate cooperative work but not articulation work.

From the vantage point of CSCW, there are many reasons to take note of the Whirlwind project and its aftermath. As already described, Whirlwind provided the paradigm for a computing technology in which the interdependent activities of multiple of workers (hundreds, thousands even) are facilitated and mediated by the system. More than that: Whirlwind played a crucial and unique role in the development of computing technology beyond the paradigm of massive calculation: digital communication networks, real-time computing, interactive computing, direct manipulation, etc. The conceptual foundation of much of modern computing was developed in the course of this effort.

2.5.3 Interactive Computing

Interactive computing grew out of technologies and practices far away from scientific and administrative calculation. It grew out of technologies of real-time control of external processes. The stored-program digital computer provided the very possibility of extending and transforming the technologies of real-time control of external processes, but it is no accident that Whirlwind was developed by MIT's Servomechanism Laboratory. The interactive computing paradigm as developed in the course of Whirlwind and so on extended and transformed technologies of gunnery control that in turn build on knowledge of real-time processes such as anti-aircraft control, bomb aiming, automatic aircraft stabilizers, etc.

However, the Whirlwind legacy is not simply a faint pattern constructed in retrospect by the historian. The continuity was real to actors at the time.

First of all, there was a direct personal continuity: 'The people who worked on the two projects gained an appreciation for how people could interact with a computer. This appreciation helped the development and maturing of interactive computing, linked with time-sharing and networking' (O'Neill, 1992, p. 11). More than that, the Whirlwind project and the subsequent development of the SAGE air defense system were research and development projects on a huge scale. The overall cost of the SAGE system exceeded the cost of the Manhattan project by a wide margin. The project therefore had tremendous impact on American computer science and engineering communities. In fact, half of the trained programming labor force of the US at the time (1959) were occupied with the development of the software for the SAGE system (Campbell-Kelly, 2003, p. 39). (Cf. also Baum, 1981).

Secondly, when the Whirlwind project was finished in 1953 and many researchers from the Servomechanisms Lab were relocated to work on Cape Cod and SAGE at a new laboratory (the Lincoln Laboratory), the original Whirlwind prototype was not scrapped but remained available on the MIT campus. Furthermore, at the Lincoln Laboratory researchers developed transistorized experimental computers based on the Whirlwind architecture. Named TX-0 and TX-2, they 'provided quick response and a variety of peripheral input/output equipment which allowed these machines to be used interactively'. These computers were also handed over to MIT campus in 1958 and were used intensively by graduate students and researchers: 'The attitude of the people using the Whirlwind computer and these test computers was important in the establishment of a "culture" of interactive computing in which computers were to be partners with people in creative thinking' (O'Neill, 1992, p. 24). For example, Fernando Corbató, who later played a central role in the development of time-sharing, recalls that 'many of us [at MIT] had cut our teeth on Whirlwind. Whirlwind was a machine that was like a big personal computer, in some ways, although there was a certain amount of efficiency batching and things. We had displays on them. We had typewriters, and one kind of knew what it meant to interact with a computer, and one still remembered.' (Corbató, 1990, p. 14). In fact, the interactive computing 'culture' O'Neill refers to was so ingrained among Whirlwind programmers that they were not particularly interested in time-sharing when this movement got under way but were rather, in Corbató's words, preoccupied with 'creating in some sense a humongous personal computer (laugh). This included people like Ivan Sutherland, for example, who did the Sketchpad using TX-2. That was a direct result of being able to have it all to himself, as though it were a big personal computer.' (Corbató, 1990, p. 15).

Thus the protracted and costly research effort that went into building the Whirlwind and its offspring provided the basic principles and techniques of the interactive computing paradigm. Later developments extended and refined the notion of 'interacting' with a computer in 'real time' far beyond the initial and quite narrow constraints of the concept of real-time transaction processing and also beyond other Whirlwind technologies such as CRT displays, and so on. Major steps were taken already in the 1960s, in laboratories using Whirlwind-type computers such as the TX-2. Ivan Sutherland's Sketchpad (1963) is probably the first documented implementation of modern computer graphics technologies.

A series of major advances resulted from the tenacious research effort of Douglas Engelbart who, of course, was well aware of the interactive computing paradigm when this effort began (Engelbart, 1962, §IIIA5; cf. also Bardini, 2000, passim). It is well known that the first experimental versions of the computer mouse was developed by Engelbart's laboratory at the Stanford Research Institute (English et al., 1967), but a significantly more important step was taken in 1968 when Engelbart at an AFIPS conference demonstrated that many of the technologies that we now consider essential components of interactive computing (direct manipulation, bit-mapped displays, mouse, message handling, etc.) could be realized in an *integrated* fashion on the basis of an architecture providing unrestrained access to *multiple* application programs (Engelbart and English, 1968). Some of the crucial points on the further development trajectory of this technology (or web of technologies) include the Xerox Alto from 1973 (Thacker et al., 1979; Lampson, 1988; Thacker, 1988) and the Xerox 8010 'Star' from 1981 (Smith et al., 1982a, b; Johnson et al., 1989). Interactive computing, as we know it, finally became a practical reality with the release of the Apple Macintosh in 1984.

2.5.4 The Arrested Growth of Interactive Computing

Whirlwind went online in 1951, development of the Cape Cod system was finished around 1956, and the SAGE system went operational around 1960. Still, it took a generation for the technologies of interactive computing to become a practical reality for workers outside of a small population of computer scientists and workers in certain time-critical work settings such as air defense and airline reservation. What impeded the further development of interactive computing, beyond on-line transaction processing? The answer is not only that the development of Whirlwind was funded at a generous level that hardly has been seen since then, but also that the basic principles of interactive computing could not be disseminated beyond the few areas where their application was economically sustainable. For computing equipment was enormously expensive until the development of integrated circuits in the 1960s and especially microprocessors in the 1970s made mass production of computers economically viable.[24]

Semiconductor technology was very long in the making, and involved advances in fundamental theories of physics as well as the development of mass markets for electronics products. Researchers in the area of solid state physics had investigated the electrical properties semiconductors since the 1830s (although not by that name). What caught their attention was the fact that the electrical resistance of substances such as silver sulphide *increased* with increasing temperature, contrary to the behavior of ordinary conductors. In 1874 it was discovered (by Ferdinand Braun) that a metal wire contacting a crystal of lead sulphide (another semiconductor) would conduct electricity in one direction only. The rectifying effect, as it was called, was used in very simple radio receivers ('crystal radio') in the first decades of the twentieth century, but the effect could not be not explained until the development of quantum mechanics in the 1920s and 1930s provided the theoretical framework for beginning to model the behavior of electrons in solids. But it was

[24]Excellent general accounts of the history of semiconductor technology can be found in the studies by Braun and Macdonald (1978) and by Orton (2009). The development of the transistor and integrated circuit technologies is recounted by the historians Riordan and Hoddeson (1997). Based on interviews with key actors, Reid (2001) gives a readable journalistic account of the semiconductor history from the beginnings to the invention of the microprocessor but his account is unfortunately marred by American hero-worship.—On the other hand, Bo Lojek (2007) offers a detailed and candid story of this development, reminding us that the history of modern technology is somewhat distorted by the picture of technology development as 'a systematic effort of exceptional leadership' carefully nurtured by corporate PR departments and patent lawyers and that, based on his own experience as an engineer in the semiconductor industry, 'the company establishment was frequently one of the biggest, if not the biggest, obstacle'.

not until the end of the 1930s that the rectification effect was explained (by Mott, Schottky, and Davydov) and the theoretical basis for the invention of the transistor was laid, and even then an enormous systematic effort—e.g., charting the properties of semiconductors such as germanium and silicon in the course of developing radar technology during World War II—was required to arrive at the point when the transistor effect was actually discovered (by Bardeen, Shockley, and Brattain at Bell Labs on 23 December 1947). Still, as Braun and Macdonald put it in their history of semiconductor technologies, *even then* 'a vast amount of development work remained to be done if the transistor was ever to become a technological achievement rather than just a scientific curiosity'. 'This is a case of a very large gulf of ignorance separating the early ideas from any possibility of realisation. Much knowledge had to be accumulated before the original ideas could be modified so as to create practical devices' (Braun and Macdonald, 1978, pp. 24 f., 47). So, although transistors were in commercial production from 1951, the technology did not affect the computer industry for more than a decade. The production of transistors posed 'staggering problems'. Vital parameters such as conductivity of a semiconductor crystal depend critically on the nature, amount, and distribution of non-native atoms in the crystal lattice (at degrees of accuracy of 1 of 10^8), which made semiconductor production an exceedingly difficult art to master. In the early 1950s the price for one transistor was about $20 while the price for a thermionic valve (or vacuum tube) was about $1; by the end of 1953, when the annual production reached 1 million transistors, the price was about $8. This initially excluded transistors from supplanting thermionic valves in most civilian applications. One of the first applications was in hearing aids, which began around 1953, and by the end of 1954 the portable radio was launched, but in general the technology only developed slowly, largely driven by military applications and the US space program.

Anyway, at this time the transistor was not nearly as reliable as the thermionic valve. For several years, the choice material for transistors was germanium, which is far more pliant in the production process but also highly sensitive to variations in temperature and not very suitable for making the switches that make up digital circuitry in the first place. Silicon, on the other hand, would afford a far more reliable transistor but was also very difficult to work with. Silicon transistors therefore only became an economic reality for the computer industry with the invention of integrated circuits in the early 1960s. Consequently, the computer industry stayed with the thermionic valve until around 1960 when printed circuits provided a passable way to apply transistor technology in serial production of computers such as the IBM 1401.

In addition to the 'staggering problems' encountered in the production of transistors, the application of the technology in computing was impeded by another problem, an impasse called the 'tyranny of numbers'. Circuits consisted of discrete components (transistors, resistors, capacitors, etc.) that had to be handled individually. In the words of Jack Morton, a manager of semiconductor research at Bell Labs, 'Each element must be made, tested, packed, shipped, unpacked, retested, and interconnected one-at-a-time to produce the whole system'. To engineers it was evident that this meant that the failure rate of circuitry would increase exponentially with increased complexity. Thus, so long as large systems had to be built by connecting 'individual discrete components', this posed a 'numbers barrier for future advances' (quoted in Reid, 2001, p. 16). No matter how reliable the individual components were, the circuits were only as reliable as the connections, and connections were generally manually wired: 'The more complex the system, the more interconnections were needed and the greater the chance of failure through this cause' (Braun and Macdonald, 1978, p. 99).

The integrated circuit—an entire circuit made out of one monolithic crystal—was developed to break this barrier. The idea was conceived and developed by several researchers independently of each other (Jack Kilby at Texas Instruments and Robert Noyce at Fairchild Semiconductors). As Kilby expressed his idea in his notebook on 24 July 1958: 'The following circuit elements could be made on a single slice: resistors, capacitor, distributed capacitor, transistor' (quoted in Reid, 2001, p. 77). The tedious and error-prone manual process of testing and assembling circuits from discrete components could be eliminated. However, it was not the application of integrated circuits in computers that primarily motivated and drove the development of this technology but more or less frivolous applications such as digital wristwatches and pocket calculators. But as the integrated circuit technology matured and prices fell, small computer manufacturers such as Digital Equipment and Data General adopted the integrated circuit as the basis of compact 'minicomputers' that could be sold for less than $100,000. At this stage, interactive computing in the form of engineering workstations slowly began to become a reality beyond a few exceptionally well-equipped computing research laboratories. However, it took the microprocessor to make interactive computing an economic reality.

It is worth noting that the microprocessor was not developed for the computer industry at all, but was conceived as a means of reducing the cost and time required to design circuitry for appliances such as electronic calculators. One could say that the 'numbers barrier' had been replaced by a 'circuit designer barrier'. In 1969, Intel, then a new company, landed a contract to develop a set of calculator chips for Hayakawa Electric Co. Tasked to work on the project, Marcian E. Hoff, suggested to make a general-purpose computer chip instead of a set of hard-wired calculators. The idea was subsequently developed by Stan Mazor and Federico Faggin and others and the first 'micro-programmable computer on a chip', the Intel 4004, was eventually released at the end of 1971, about 35 years after the discovery of the transistor and the demonstration of the first stored-program digital computer and almost 30 years after the conception of interactive computing was first realized in Whirlwind.—Intel 4004 was followed by the 8008 (8-bit word-length processor) chip, presented in 1973. Parallel developments occurred at Texas Instruments. By the mid-seventies the microprocessor was an established and accepted technology complete with computer-aided design environments: 'In no more than 5 years a whole industry had changed its emphasis from the manufacture of dedicated integrated circuits [for pocket calculators, etc.], to the manufacture of what in effect were very small computers' (Braun and Macdonald, 1978, p. 110). Where the 4-bit Intel 4004 of 1971 contained 2,300 devices (transistors, etc.), the 32-bit processors that began to be produced about 10 years later integrated about 200,000 transistors and were comparable with mainframe computers in performance (ibid., pp. 108–112).

As pointed out by Braun and Macdonald (ibid.), underlying this remarkable leap was a 'favorable constellation' of factors in the semiconductor industry. Firstly, the invention of electronic calculators and their success had created a mass market for integrated circuits. Advances in solid-state physics in general and semiconductor manufacturing technologies in particular (e.g., clean-room fabrication of 'metal–oxide–semiconductors' or MOS by means of photolithography) made it possible to produce integrated circuits with increasing densities and low power consumption. Secondly, related technical developments in high-density MOS technology made it possible to produce semiconductor memory chips. Thirdly, minicomputer technology had matured to the stage that their hardware architectures could serve as a model for microprocessor design. And so on.

The microprocessor, initially developed for calculators, was shifted laterally and formed a critical component technology in the personal computer that was then created. In the words of Douglas Bell, the designer of many of Digital's PDP computers, in his keynote address to the ACM conference on the *History of Personal Workstations*, 'The microprocessor, memory, and mass storage technology appearing in 1975 lead directly to the personal computer industry'. Improvements in these technologies were 'the sole enabling determinants of progress in computing' (1988, pp. 9 f.).

That is, when Engelbart undertook his experimental work on the NLS in the second part of the 1960s, integrated circuits were in their infancy. The computer platform that was available to his Augmentation Research Center for its experiments consisted of a couple of CDC minicomputers, but in 1967, the laboratory, thanks to a grant from ARPA, was able to acquire its first time-sharing computer; it cost more than $500,000 (Bardini, 2000, pp. 123, 251). The Xerox Alto, released only 7 years later, in 1974, had a performance comparable to that of a minicomputer but the cost had dwindled to about $12,000. This was still well above what was commercially viable, but the Alto was anyway primarily developed as an experimental platform. Eventually 1,500 machines were built and used by researchers at Xerox and at a few universities (Thacker, 1988).

> Alto served as a valuable prototype for Star. Over a thousand Altos were eventually built, and Alto users have had several thousand work-years of experience with them over a period of eight years, making Alto perhaps the largest prototyping effort in history. There were dozens of experimental programs written for the Alto by members of the Xerox Palo Alto Research Center. Without the creative ideas of the authors of those systems, Star in its present form would have been impossible. (Smith et al., 1982b, p. 527 f.)

In other words, due to the drastically reduced costs of computing power due to microprocessor technology, an experimental platform for interactive computing research had become available. And then, eventually, one day in 1975 Apple cofounder Stephen Wozniak could walk in and buy a MOS Technology 6502 microprocessor for $20 (Freiberger and Swaine, 2000, p. 264), and less than ten years later Apple could release the first Macintosh at a price about $1,200. The technology of interactive computing was, so to speak, dormant for a decade or more, until Moore's law propelled the technology forward (cf. also Grudin, 2006a, b).

The long gestation period was not due to theoretical shortcomings. For the technologies of interactive computing were never derived from any preexisting theoretical knowledge. In fact, after Whirlwind the technologies of interactive computing were developed by computer technicians to satisfy requirements they themselves had formulated on the basis of principles and concepts known from their own daily work practices. On one hand, in the design of the Alto and the Star the technicians built on and generalized concepts that were deeply familiar to any Western adult working in an office environment. In their own words, 'the Star world is organized in terms of objects that have properties and upon which actions are performed. A few examples of objects in Star are text characters, text paragraphs, graphic lines, graphic illustrations, mathematical summation signs, mathematical formulas, and icons' (Smith et al., 1982b, p. 523). On the other hand, in interpreting these everyday concepts technically the designers applied the conceptual apparatus of object-oriented programming (objects, classes of objects, properties, messages) that had been developed by Kristen Nygaard and Ole-Johan Dahl as a technique to obtain 'quasi-parallel processing' (Dahl and Nygaard, 1966; cf. also Nygaard and Dahl, 1978) and had later been further developed in the 1960s by Alan Kay and others in the form of Smalltalk:

> Every object has properties. Properties of text characters include type style, size, face, and posture (e.g., bold, italic). Properties of paragraphs include indentation, leading, and alignment. Properties of graphic lines include thickness and structure (e.g., solid, dashed, dotted). Properties of document icons include name, size, creator, and creation date. So the properties of an object depend on the type of the object. (Smith et al., 1982b, p. 523)

Similarly, the technicians could build on 'fundamental computer science concepts' concerning the manipulation of data structures in order to provide application-independent or 'generic' commands that would give a user the ability to master multiple applications and to 'move' data between applications:

> Star has a few commands that can be used throughout the system: MOVE, COPY, DELETE, SHOW PROPERTIES, COPY PROPERTIES, AGAIN, UNDO, and HELP. Each performs the same way regardless of the type of object selected. Thus we call them generic commands. [. . .] These commands are more basic than the ones in other computer systems. They strip away extraneous application specific semantics to get at the underlying principles. Star's generic commands are derived from fundamental computer science concepts because they also underlie operations in programming languages. For example, program manipulation of data structures involves moving or copying values from one data structure to another. Since Star's generic commands embody fundamental underlying concepts, they are widely applicable. Each command fills a host of needs. Few commands are required. This simplicity is desirable in itself, but it has another subtle advantage: it makes it easy for users to form a model of the system. (Smith et al., 1982b, p. 525)

The important point here is that the design concepts reflected the technicians' own practical experience, in that they could generalize the concepts of their own work (typographical primitives such as characters, paragraphs, type styles, etc.) and from similar concepts already generalized in computer science (MOVE, COPY, DELETE, etc.).

No ethnographic fieldwork was required to develop an understanding of concepts like character, paragraph, line, and illustration or MOVE, COPY, and PASTE. For a long stretch, then, interactive computing technologies could be further developed and refined in a manner very similar to the way in which email was developed: by technicians building tools for their own use, based on their daily practices and in a rather intuitive iterative design and evaluation process. Thus, for example, when Steve Wozniak was designing the Apple II, he 'was just trying to make a great computer for himself and impress his friends at the Homebrew Computer Club. [...] Most of the early Apple employees were their own ideal costumers.' The members of the Macintosh design team were similarly motivated: 'we were our own ideal customers, designing something that we wanted for ourselves more than anything else' (Hertzfeld, 2005, pp. xvii f.). They knew the concept of interactive computing from Engelbart's work (who had been building on the Whirlwind experience); they knew from their own lives what they would require of its design; and they had the advanced technical skills to realize their ideas.

That is, the technologies of interactive computing were initially developed in the course of a deliberate design effort (in the Whirlwind and Cape Cod projects), drawing upon on principles of 'man-machine systems' based on experiences from servomechanisms. After that, the technologies of interactive computing were subjected to two decades of almost 'arrested growth'. However, the technology of microprocessors, mass-produced CPUs, provided a burgeoning platform of development on which the computer scientists at SRI, at Xerox PARC, at Apple, and elsewhere could extend, elaborate, and refine the principles of interactive computing on the basis of their own practical experiences

The first CHI conference convened in 1983, that is, two years *after* the release of the Xerox Star and one year before the release of the first Macintosh. That is, as an institutionalized research field, HCI only emerged *after* the technology had matured and was on the way out of the laboratories. This has caused all kinds of soul searching.

As shown earlier, John Carroll noted that 'some of the most seminal and momentous user interface design work of the last 25 years' such as Sutherland's Sketchpad and Engelbart's oN Line System (NLS) 'made no explicit use of psychology at all' (Carroll, 1991, p. 1). Carroll's observation is fully vindicated by the original documents, although it should be mentioned that psychologists at RAND and the SDC such as Allen Newell were involved in the development of the SAGE system software already from the 1950s, tasked with testing the software modules that were to support operators at the air defense direction centers (Chapman et al., 1959; Baum, 1981). That is, psychology *did* contribute to the development of interactive computing but the contribution was made almost a decade prior to Sutherland and Engelbart's work and had become incorporated in the interactive computing paradigm as represented by real-time transaction processing technology that formed the technological baseline for their work.[25]

[25] Stuart Card remarks cannily that what Newell was engaged in at RAND and SDC today would be called 'Computer Support for Cooperative Work' (1996, p. 259), a claim not without merit.

As already noted, Carroll's observations also apply to the development of the Xerox Alto and the Xerox Star. But again a little nuance is required, in as much as human factors researchers were involved in 'human factors testing' of the design of the Star. Some of that work was reported at the first CHI conference by Bewley et al. (1983). However, in the same paper Bewley et al. claim that 'Recognizing that design of the Star user interface was a major undertaking, the design team approached it using several principles, derived from cognitive psychology' (p. 72). The authors provide no justification for this claim, nor is it supported by the available contemporary documentation concerning the 'Star' design process. In fact, the very general features they cite were already present in the design of the Alto that was designed 10 years earlier. The claim thus has the appearance of a rationalization. On the other hand, just for the record, the development of the mouse did draw upon principles from experimental psychology, in particular 'Fitts' law' (1954) concerning the capacity of the human motor system (cf. Smith et al., 1982b).

Anyway, and as a matter of fact, the principles of 'direct manipulation' were only articulated and systematized a decade after the release of the Xerox Alto (Shneiderman, 1983; Hutchins et al., 1986). HCI research developed as a *post festum* systematization effort devoted to understanding the principles of the interactive computing techniques that had emerged over three decades, from the Whirlwind to the Alto, the Star, and the Macintosh.

2.5.5 Interactive Computing and the Cybernetic Notion of 'Human-Computer System'

Interactive computing had its intellectual roots in the real-time transaction processing technologies that in turn emerged from control-engineering conceptions of human control of processes in 'real time' (gun control, bomb aiming, servo steering). Generating research traditions such as 'man-machine systems' research, this conception of machinery has left a rich and quite influential legacy: a conceptual framework that has been expressed in different terms over time—from the original notions of 'man-machine system' (Sutherland, 1963), 'man-computer symbiosis' (Licklider, 1960), and 'human intellect' 'augmentation system' (Engelbart, 1962) to contemporary notions of 'human-computer interaction' and 'interaction design'—but the underlying conception is the same. What unites all these different notions is the concept of *unity of control* in the man-machine system dyad: the notional steersman directing the dyad towards his or her 'goal'. It is thus, invariably, presumed that control is vested in an individual or in a 'team' or 'community' acting in such as way that unity of control can be presumed as a given.

The argument can be made that the basic concept of HCI—the human-computer dyad—still reflects this outlook and that the defining conceptual axis of HCI research has remained that of the human-machine dyad. It goes without saying that communication and interaction among people is not excluded from this conception, but the conception is incommensurable with conceptions of *the essentially distributed nature of cooperative work* and of the sociality of human competencies. This becomes evident when attempts are made to address technical requirements of cooperative work under the aegis of this conception, such as, for example, when Engelbart and Lehtman in 1988 outlined their vision of a 'community handbook' as 'a monolithic whole', 'a uniform, complete, consistent, up-to-date integration of the special knowledge representing the current status of the community' (Engelbart and Lehtman, 1988). The essentially distributed nature of such a construct was entirely absent from the conception.

The very same conception underlies the 'groupware' research paradigm: computing technologies that facilitate the exchange of messages and files and thereby 'collaboration' or 'group work' or 'shared knowledge' among dispersed individuals working together 'at a distance' as a 'group' or 'team' towards a 'collaborative goal' (cf., for example, Olson and Olson, 2003). The problem with such notions is that the issue of the distributed character of cooperative work—the issue of the distributed control of coordination—is reduced to issues of geographical dispersion: the issues of heterogeneity of practices, incongruence and incommensurability of conceptual schemes, etc. are not reflected and cannot be integrated in the cybernetic 'human-machine system' conception.

2.6 Facilitation of Articulation Work: Computer-Mediated Communications

The notion of computer-mediated communications began with the notion of 'time-sharing' operating systems that matured around 1960. Since computer systems at the time were excessively expensive (printed circuit boards and core memory was only then becoming standard technologies), it was mandatory that computer systems were operating close to full capacity. Consequently, most of the few computers that were around were running in a batch-processing mode, one job after another on a 'first-in, first-served' basis, or, as it was aptly expressed by J. C. R. Licklider, the 'conventional computer-center mode of operation' was 'patterned after the neighborhood dry cleaner ("in by ten, out by five")' (Licklider and Clark, 1962, p. 114). This economic regime could be tolerated for administrative purposes such as payroll and invoice processing that, since punched-card tabulating had been adopted earlier in the century, was organized on batch-processing principles anyway. But in most work settings, this regime effectively precluded all those applications that require high-frequency or real-time interactions between user and the digital representations. In particular, the 'in by ten, out by five' regime made programming, especially debugging, a deadening affair. This gave ordinary computer technicians a strong motive for devising alternative modes of operation. As described by O'Neill, researchers at MIT, who had 'cut their teeth' on Whirlwind and had experienced interactive computing first-hand, 'were unwilling to accept the batch method of computer use for their work' and 'sought ways to continue using computers interactively'. However, 'it would be too costly to provide each user with his or her own computer' (O'Neill, 1992, p. 44). So, around 1960 the idea of letting a central computer system service several users 'simultaneously' was hatched. In the words of John McCarthy, one of the fathers of the idea, the solution was an operating system that would give 'each user continuous access to the machine' and permit each user 'to behave as though he were in sole control of a computer' (McCarthy, 1983). The first running operating system of this kind seems to have been the Compatible Time-Sharing System or CTSS. It was built at MIT by a team headed by Fernando Corbató and was first demonstrated in 1961 (Corbató et al., 1962). The various users were connected to the 'host' computer via terminals and each would have access to the computing power of the 'host' as if he or she was the only user.

Now, the users of the first of these systems were typically engaged in cooperative work. Some were engaged in developing operating systems or other large-scale software projects and were, as a vital aspect of this, engaged in various forms of discourse with colleagues within the same project teams and research institutions, that is, with colleagues already connected to the central computer system. Likewise, software technicians would need to coordinate with system operators about possibly lost files to be retrieved, about eagerly-awaited print jobs in the queue, etc. The time-sharing operating system they were building or using provided a potential solution to this need, and the idea of using the system to transfer text messages from one worker to another did not require excessive technical imagination. As one of the designers of one of the first email systems recalls:

> [CTSS] allowed multiple users to log into the [IBM] 7094 from remote dial-in terminals[] and to store files online on disk. This new ability encouraged users to share information in new ways. When CTSS users wanted to pass messages to each other, they sometimes created files with names like `TO TOM` and put them in "common file" directories, e.g. `M1416 CMFL03`. The recipient could log into CTSS later, from any terminal, and look for the file, and print it out if it was there. (Van Vleck, 2001)

A proper mail program, 'a general facility that let any user send text messages to any other, with any content' was written for CTSS by Tom Van Vleck and Noel Morris in the summer of 1965 (ibid.). It allowed one programmer to send a message to other individual programmers, provided one knew the project they worked on, or to everybody on the same project. The message was not strictly speaking 'sent'; it was appended to a file called `MAILBOX` in the recipient's home directory. The same year Van Vleck and Morris also devised a program (`.SAVED`) 'that allowed users to send lines of text to other logged-in users', that is, a primitive form of 'instant messaging' (ibid.).

The scope of the exchange of messages with these and similar programs was limited by the boundary of the hierarchy comprising the local central computer system and the terminals connected to it. Messages could not travel beyond the event horizon of this black hole. This world of isolated systems dissolved with the development of network computing. The motivation driving the development was (again) not to develop facilities for human interaction, not to mention cooperative work, but to utilize scarce resources in a more economical way.

For Licklider, who also initially headed the development of ARPANET, the motivation for the network was to reduce 'the cost of the gigantic memories and the sophisticated programs'. When connected to a network, the cost of such shared resources could be 'divided by the number of users' (Licklider, 1960, p. 8).

This vision was spelled out by Lawrence Roberts, who took over as manager of the development of ARPANET in 1966. Noting that the motivation for past attempts at computer networks had been 'either load sharing or interpersonal message handling', he pointed to 'three other more important reasons for computer networks [. . .], at least with respect to scientific computer applications', namely:

> *Data sharing*: The program is sent to a remote computer where a large data base exists. [. . .]
> *Program sharing*: Data is sent to a program located at a remote computer and the answer is returned. [. . .]
> *Remote Service*: Just a query need be sent if both the program and the data exist at a remote location. This will probably be the most common mode of operation until communication costs come down [since this] category includes most of the advantages of program and data sharing but requires less information to be transmitted between the computers. (Roberts, 1967, p. 1)

On the other hand, electronic mail was explicitly ruled out as of no significance:

> *Message Service*: In addition to computational network activities, a network can be used to handle interpersonal message transmissions. This type of service can also be used for educational services and conference activities. However, it is not an important motivation for a network of scientific computers. (Roberts, 1967, p. 1)

These motivations for developing the ARPANET were upheld and confirmed as the net began to be implemented:

> The goal of the computer network is for each computer to make every local resource available to any computer in the net in such a way that any program available to local users can be used remotely without degradation. That is, any program should be able to call on the resources of other computers much as it would call a subroutine. The resources which can be shared in this way include software and data, as well as hardware. Within a local community, time-sharing systems already permit the sharing of software resources. An effective network would eliminate the size and distance limitations on such communities. (Roberts and Wessler, 1970, p. 543)

Thus, as summarized by Ian Hardy, in his very informative history of the origins of network email, the primary motive was economic.

> ARPANET planners never considered email a viable network application. [They] focused on building a network for sharing the kinds of technical resources they believed computer researchers on interactive systems would find most useful for their work: programming libraries, research data, remote procedure calls, and unique software packages available only on specific systems. (Hardy, 1996, p. 6)

After pioneering work on the underlying packet-switching architecture and protocols, the experimental ARPANET was launched in 1969, connecting measly four nodes.[26] In the summer of 1971, when the network had expanded to fifteen nodes, a programmer named Ray Tomlinson at BBN (Bolt, Beranek, and Newman), devised

[26]Much of the pioneering work on packet switching was done by Donald Davies and his colleagues at the British National Physical Laboratory (Davies, 1965, 1966). The NPL network, which was also launched in 1969, was built with rather similar objectives in mind (On NPLNet, cf. also Abbate, 1999).

a program for sending email over the network. He recalls that, while he was making improvements to a single-host email program (SNDMSG) for a new time-sharing operating system (TENEX) for the PDP-10 minicomputer, 'the idea occurred to [him]' to combine SNDMSG with en experimental file-transfer protocol (CPYNET) to enable it to send a message across the network, from one host to another, and append it to the recipient's MAILBOX file. For that purpose he also devised the address scheme NAME@HOST that has become standard (Hardy, 1996; Tomlinson, 2001). 'The first message was sent between two machines that were literally side by side. The only physical connection they had (aside from the floor they sat on) was through the ARPANET', that is, through the local Interface Message Processor (IMP) that handled packet switching. To test the program, he sent a number of test messages to himself from one machine to the other. When he was satisfied that the program seemed to work, he sent a message to the rest of his group explaining how to send messages over the network: 'The first use of network email announced its own existence' (Tomlinson, 2001). The program was subsequently made available to other sites on the net that used TENEX on PDP-10s and was soon adapted the IBM 360 and other computers (Salus, 1995, p. 95).

An instant success within the tiny world of ARPANET programmers, this very first network email program triggered a chain reaction of innovation that within less than a couple of years resulted in the email designs we use today: a list of available messages indexed by subject and date, a uniform interface to the handling of sent and received mail, forwarding, reply, etc.—all as a result of programmers' improving on a tool they used themselves. In 1977, an official ARPANET standard for electronic mail was adopted (Crocker et al., 1977).

The history of network email after that is well known. The technology migrated beyond the small community of technicians engaged in building computer networks to computer research in general and from there to the world of science and eventually to the world at large.

What is particularly remarkable in this story, and what also surprised those involved when they began to reflect on the experience, was 'the unplanned, unanticipated and unsupported nature of its birth and early growth. It just happened, and its early history has seemed more like the discovery of a natural phenomenon than the deliberate development of new technology' (Myer and Dodds, 1976, p. 145). And at a meeting in January 1979, convened to discuss the 'the state of computer mail in the ARPA community and to reach some conclusions to guide the further development of computer mail systems', it was 'noted' as a fact 'that most of the mail systems were not formal projects (in the sense of explicitly sponsored research), but things that "just happened"' (Postel, 1982, p. 2). In the same vein, the official *ARPANET Completion Report* notes that 'The largest single surprise of the ARPANET program has been the incredible popularity and success of network mail' (Heart et al., 1978, p. III-110).

Network email was not only 'unplanned' and 'unanticipated'; it was 'mostly unsupported' (Heart et al., 1977, p. III-67; Abbate, 1999, p. 109), for, as noted already, the objective of the ARPANET was resource sharing. Not only had Lawrence Roberts not included network mail in the original plans for the network, he had excluded it as 'not an important motivation for a network of scientific computers' (Roberts, 1967, p. 1). However, as Janet Abbate points out in her history of the emergence of the Internet, the aimed-for resource sharing failed to materialize. By 1971, when the original fifteen sites for which the net had been built were all connected, 'most of the sites were only minimally involved in resource sharing' (Abbate, 1999, p. 78). She notes that the 'hope that the ARPANET could substitute for local computer resources was in most cases not fulfilled', and adds that, as the 1970s progressed, 'the demand for remote resources actually fell', simply because minicomputers 'were much less expensive than paying for time on a large computer' (ibid., pp. 104 f.). The budding network technology represented by ARPANET was on the verge of being superseded and outcompeted by the proliferation of relatively inexpensive minicomputers. Thus, 'Had the ARPANET's only value been as a tool for resource sharing, the network might be remembered today as a minor failure rather than a spectacular success' (ibid, pp. 104–106).

As it happened, email quickly 'eclipsed all other network applications in volume of traffic' (Abbate, 1999, p. 107). In fact, according to Hafner and Lyon email amounted to 75% of the traffic on the net as early as 1973 (2003, pp. 189, 194). One reason why the ARPANET succeeded as an experiment was that 'since members used the network protocols in their own work, they had the incentive and the experience to create and improve new services' (Abbate, 1999, p. 69).

That is, as in the case of local email on time-sharing operating systems, network email came as an afterthought, devised by computer technicians for their own use, as a means for coordinating their cooperative effort of building, operating, and maintaining a large-scale construction, in this case the incipient Internet. Email was thrown together like the scaffolding for a new building, only to become a main feature, relegating the resulting building itself, which had been the original and official objective, to something close to a support structure.

This pattern—technicians building tools for use in their own laboratories—was to be repeated again and again, as evidenced by, for example, CSNET developed by Larry Landweber and others 1979–1981 to provide under-privileged computer scientists access to ARPANET as well as mail, directory services, news, announcements, and discussion on matters concerning computer science; USENET which was developed in 1979 by Jim Ellis and Tom Truscott as a news exchange network for Unix users without access to ARPANET (cf. Quarterman, 1990, pp. 235–251; Hauben and Hauben, 1997); the World Wide Web developed in 1989 at CERN by Tim Berners-Lee and Robert Caillau (Berners-Lee, 1990; Gillies and Cailliau, 2000); and the ARCHIE and GOPHER network file search protocols designed by Alan Emtage in 1989 and Mark P. McCahill in 1991, respectively (Gillies and Cailliau, 2000).

3 The Formation of CSCW

In important respects, digital computing technologies developed in response to challenges faced by cooperative work. Firstly, the digital stored-program computer was developed as a technology for *automating* the large-scale calculation work that since the time of the French Revolution had been performed cooperatively, in an advanced form of division of labor ('human computers') but by the middle of the twentieth century was becoming far too complex to be carried out by the received methods. Secondly, the basic concepts and techniques of interactive computing were developed as a technology of *facilitating* large-scale cooperative work (air defense, air traffic control, airline reservation) that had become too complex to be done by conventional means, manual or mechanical. Thirdly, the basic concepts and techniques of distributed messaging (network email, etc.) were developed as a technology for *facilitating and mediating* the coordinative activities required by the cooperative effort of building and managing a large-scale infrastructure, namely, the ARPANET, and were quickly complemented by a host of other technologies such as mailing lists, news groups, file sharing, etc.

Furthermore, of course, with the arrival of affordable personal computer (such as the Apple II in 1977, the IBM PC in 1981, and the Macintosh in 1984) and the establishment of the basic Internet protocols (TCP/IP in 1983), interactive computing was reaching a state where it was becoming realistic for ordinary workers, not merely to have access the strictly constrained repertoire of functionality available in real-time transaction processing systems, but to have unrestricted local command of a computer with a palette of applications. It was thereby becoming realistic for computer technologies to become an integral part of cooperative work practices in entirely new ways.

3.1 Proto-CSCW: 'Computer-Mediated Communications'

The new possibilities opened for several partly overlapping research areas with different conceptual frameworks.

First of all, of course, email and other technologies of computer-based message handling caught the attention of researchers and technology pundits as a fascinating new topic for studies of communication in organizations, as a new management technology along with the telegraph, teleprinters, fax, microfilm, etc., and engendered a small corpus of evaluation studies of the presumptive effects and impact of electronic mail (Uhlig, 1977; Kiesler et al., 1984; Eveland and Bikson, 1987) as well as studies that can better be categorized as market analyses (Panko, 1977, 1984).

More importantly, a small but persistent research area was formed that focused on developing a computing technology generally known as 'computer conferencing' (for an overview of this work, cf. Kerr and Hiltz, 1982). In fact, computer conferencing research developed simultaneously with network email, in the course of the 1970s. But in contrast to email, in a 'conference' communications were not restricted to point-to-point message exchanges but were as a default 'public' exchanges among attendees at the online 'conference'. That was the idea, anyway. 'Computer conferencing' was originally merely a variant of the online transaction processing paradigm, with dispersed participants logging-in to a central host computer, with 'messages' treated like transactions, and with exchanges similarly constrained by a pre-established structure. In EMISARI, for instance, each 'message' was confined to 10 lines of text (Kerr and Hiltz, 1982) while control was centralized and fixed. 'Computer conferencing' was later, as distributed network architectures became ubiquitous, implemented on a message-passing model; but the notion of centralized control of the communication structure has remained fundamental. In fact, the more ambitious experiments in this line of research, such as EMISARI and EIES by Turoff et al. (Turoff, 1971, 1972, 1973; Hiltz and Turoff, 1978) or FORUM and PLANET by Vallee et al. (Vallee, 1974), explored the rather grand design vision of 'group communication' structured according to some presumptively rational model.

Later, 'computer conferencing' was often advocated as a remedy for the 'information overload' which was seen as an inexorable consequence of point-to-point message exchange (Palme, 1984; Hiltz and Turoff, 1985), but, ironically, in the 1980s 'computer conferencing' was often used by users who at the time did not have direct access to the Internet, as a platform for exchanging emails.

Anyway, 'computer conferencing' soon became categorized together with email as 'computer-mediated communication', a label still very much in use (Kerr and Hiltz, 1982; Hiltz and Turoff, 1985; Rice, 1987; Quarterman, 1990; Rice, 1990; Turoff, 1991; Barnes, 2003). However, the research agenda was never succinctly defined but the general drift of the efforts was clear enough. In an early central study of the field, commissioned by the NSF (Hiltz and Kerr, 1981), the authors attempted to 'collect and synthesize current knowledge about computer-mediated communication systems' (ibid., p. viii). However, the 'synthesis' focused on issues of 'acceptance'. The authors note that the idea of computer-mediated communication 'seems simple enough at first glance', but 'It is the applications and impacts that are startling, and the acceptance of the technology that is problematical' (Kerr and Hiltz, 1982, p. ix). That is, there was a problem with 'acceptance' but the technology was anyhow 'startling'. Instead of reflecting on underlying design assumptions, the authors argued that, 'computer-mediated communication [. . .] requires that people accept fairly radical changes in the way they work and even in the way they think, if they are to reap the potential benefits' (ibid.).

In presenting the objectives of their book, Kerr and Hiltz highlighted the following issues in 'computer-mediated communications':

> 1. What are the important considerations in designing software or choosing a system from the many available options and capabilities?
> 2. What factors determine whether such systems are likely to be accepted or rejected?
> 3. What are the likely impacts of such systems upon the individuals, groups, and organizations which use them? It is not the economic costs and benefits, but the social problems and "payoffs" in the form of enhanced performance and organizational efficiency that should be the main considerations in deciding whether or not to use a computer-mediated communication system. (Kerr and Hiltz, 1982, p. x)[27]

This research agenda has, in a way, become classical: it can be seen as a model of an approach to CSCW research that has been going on stubbornly ever since: which

[27] A fourth issue was also listed, but is not really an 'issue' but a call for 'formal evaluation and feedback from users to guide the implementation' (Kerr and Hiltz, 1982, p. x).

factors determine user 'acceptance' and what are the likely impacts of such systems?[28] So, while the experiments with conferencing systems sometimes allowed for long-term use and thus evolution of 'user behavior' (e.g., Hiltz and Turoff, 1981) and considerable effort was devoted to extensive empirical evaluation, the evaluation studies associated with computer conferencing never were such that underlying principles and concepts of the conferencing idea were questioned: the evaluations invariably focused on 'acceptance' and 'satisfaction'.

In any event, this whole line of research on 'computer-mediated communication' remained mired in common-sense notions of 'communication' with respect to work practices and never began to address the principles and concepts underlying the various communication techniques that had been developed by practitioners and visionaries. The systematic conceptual effort required to transform technique into technology was absent.

However, the critical questions *were* raised, but outside of the small coterie of computer conferencing researchers. The systematic conceptual effort was undertaken by a separate research program, normally and confusingly also categorized as 'computer-mediated communication'. The European 'computer-mediated communications' community emerged in the wake of the European efforts to develop computer networking (cf. Gillies and Cailliau, 2000). As TCP/IP slowly became available in operating systems and developers began to be able to take it for granted, and as the 'message handling' standards stabilized in the first half of the 1980s, European researchers, organized under the aegis of the European Commission's COST-11 program, embarked on what was seen as the logical next step, namely, developing the standards required for putting it all together: email as well as directories, calendars, schedules, and so on.

The point of departure for these efforts is well summarized and exemplified by this observation made by Bowers et al. and presented at a European conference on 'teleinformatics' (Speth, 1988): 'Simple electronic mail meets only the most basic messaging requirements of group communication, while relatively sophisticated conferencing and bulletin board systems offering supposedly "all-purpose" facilities reflect their designers' limited intuitions about what users will wish to do, and such systems can be difficult to adapt to support specific tasks' (Bowers et al., 1988, p. 195). That is, as opposed to the previous line of research on computer-mediated communications, the CMC research undertaken under COST-11 was proactive, predicated on the insight that, for instance, the email technology that had been developed by the enthusiastic ARPANET technicians for their own use was rudimentary. The extant email protocol did not, for example, allow users to configure the protocol for special purposes, nor was the protocol sufficiently expressive for general use

[28]Take for example the research agenda of Sproull and Kiesler: 'How will these technologies influence and change organizations? Does a computer network make work groups more effective? How do people treat one another when their only connection is a computer message? What kinds of procedures best suit long-distance management using a computer network? What problems do these technologies alleviate-and what problems do they create?' (Sproull and Kiesler, 1991, p. ix).

in work settings. What was required was a specification language that would allow users to express more than, say, 'TO', 'FROM', and 'SUBJECT'. Accordingly, ambitious attempts to extend the email protocol by using speech-act theory as a grammar so as to enable users to express the illocutionary point of a message by categorizing it as a 'REQUEST' or 'PROMISE', cf. THE COORDINATOR by Winograd and Flores (Flores et al., 1988) and CHAOS by di Cindio, de Michelis, and Simone (De Cindio et al., 1986).

Similarly, it was realized that 'email' and 'conferencing' should be seen as merely instances of a range of 'group communication' facilities that therefore had to be systematically conceptually integrated. Realizing this, research under the COST-11 program aimed at developing such a conceptual foundation. As a representative of one of the research efforts, the AMIGO project, Hugh Smith noted that 'a number of electronic message systems provide the necessary low-level support for a variety of higher-level structured group communication activities', such as news distribution, conferencing, information storage and remote retrieval but he then added that 'there is no direct high-level processing support for these activities within the messaging system'. Specifying a number of critical shortcoming, he emphasized that it was a 'fundamental requirement that communicated information should be able to be used for more than one function' but that this requirement was not met, and he went on to note that neither was the requirement that many kinds of services were needed to 'support structured communication activities in addition to the basic information transfer capability provided'. He further noted that there were 'no standardized services to support group communication', and that most existing systems were 'inflexible and not easy for the end-users to adapt to their specific purposes' (Smith, 1988, pp. 90 f.). He called attention to the critical issue that conferencing systems such as those mentioned above 'often have a centralized resource architecture' and added that these architectures therefore do not scale up: 'In the future the sheer size and political/geographical separation of networked communities will demand that the resources and the management of group communication activities be distributed' (ibid., p. 90). The COSMOS project, also under COST-11, was moving along parallel lines, focusing on developing 'a high-level, user-oriented language by means of which users can alter the structure of their communication environment' (Bowers et al., 1988, p. 195; cf. also Bowers and Churcher, 1988, p. 125).

However, the COST-11 researchers' critical studies of existing 'computer-mediated communications' techniques and the rigorous attempts to reconstruct 'computer-mediated communications' on a systematic conceptual foundation led to unanticipated conclusions: instead of concluding in a systematically reconstructed technology for 'group communications', this line of research ended in the realization that the shortcomings they had identified were even deeper. Because of their rigorous approach they realized that the problem was rooted in treating 'communication' as a *separate kind of activity* that, presumably, generally is (or can be) carried out divorced from work practices. This realization led to a program shift in CSCW as the focus was moved from the concept of 'communication' to the concept of 'cooperative work practices'.

3.2 The Crisis of the Message-Handling Paradigm

Although the experiments with 'computer conferencing' at the time were reported as very promising and successful, this particular research program ran out of steam. This had to do with these underlying conceptual limitations. 'Computer conferencing' research shared with the standard message exchange paradigm the presumption that human communication generally is or can be treated as a distinct activity. True, workers do interrupt their primary work to have conversations and exchange notes, letters, memos about their work (and about other matters). They also, occasionally, put their work aside to go to meetings. For some workers, e.g., managers, the major part of their work day may be spent in conversations and meetings. But apart from managerial work and in the greater scheme of things, conversations and meetings are exceptions, interruptions, 'a necessary evil' perhaps, or simply considered 'a waste of time'. And even when workers engage in conversations and meetings, such discourses are generally related to the state of affairs in their work, to the flow of work, the schedule, the production facilities, and the archives, and in their deliberations workers will discuss schedules, plans, schemes, and so on; they will collate, arrange, distribute, present, hand out, walk up to, gather around, point to, gesture at, inspect, amend, etc. all sorts of artifacts.

By the mid-1980s this insight began to mature and be voiced (cf., e.g., Bannon, 1986, p. 443). The 'computer-mediated communications' research program had arrived at a critical junction. The European 'computer-mediated communications' researchers soon realized that the 'message-handling' model underlying 'computer-mediated communications' was quite limited (Pankoke-Babatz, 1989a). In work practices, communication is normally not a separate activity; it is typically an integrated aspect of doing the work. It was therefore considered necessary to be able to incorporate communication functionality in the various domain-specific applications. On the other hand, the European 'computer-mediated communications' researchers rejected the 'computer conferencing' paradigm as a way to provide structure to the exchange of messaging. Guided by 'a strong commitment to the actual situation in working life' (Pankoke-Babatz, 1989c, p. 20), they rejected the idea underlying the 'computer conferencing' paradigm of providing 'a new model' of communication. Instead, they aimed at providing a model that 'might be used in the design and implementation' of local and temporary 'patterns' of interaction. That is, instead of deciding on a particular preconceived conception of communication functionalities and applications, they 'chose [. . .] to look at activities and the regulations required by a group of people to co-operatively execute a particular activity. The model we want to develop should therefore allow specification of such regulations' (Pankoke-Babatz, 1989c, p. 20). That is, the aim was to build what one could call an abstract model or a notation that would make it possible 'to model the activities, businesses, tasks, actions or work-flow[s], which are performed by a group of co-operating people', so as to, in turn, 'facilitate the required co-ordination and possibly to automate co-ordination, thus reducing the co-ordination effort required of the participants in an activity' (Pankoke-Babatz, 1989b, p. 14).

The European 'computer-mediated communications' researchers knew very well that the development of such computational models and architectures would have to be grounded in 'fundamental understanding of Group Communication processes' (p. 14), which in turn, because of the complexity and variability of working practices, would need contributions from 'sociology, anthropology, economics and political science' (p. 21). Their 'strong commitment to the actual situation in working life' was amply demonstrated in the pre-dominance of the practice-oriented program in the European CSCW research community that began to coalesce as these research activities ended in 1988.

This critique of the underpinnings of 'computer-mediated communications' was also expressed—clearly and succinctly—by Irene Greif in her 'Overview' of CSCW in her influential *CSCW: A Book of Readings* (1988b). Having noted the rapid development of 'computer-mediated communications' from electronic mail to computer conferencing she then observed:

> Computer conferencing has since been expanded to support a wide range of "many-to-many communication" patterns. However, when computer conferencing is applied to some task, the model breaks down. The unstructured body of messages is suitable for the free-flowing text of natural language, but does not let us set the computer to work on our problems. Designers who draw pictures, software developers who jointly write code, financial analysts who collaborate on a budget – they all need coordination capabilities as an integral part of their work tools. That means coordination support within the CAD engineer's graphics package, within the programmer's source-code editor, within the budget writer's spreadsheet program. It means support for managing versions of objects, be they pictures, programs, or spreadsheets. It means ways to distribute parts of the object for work by contributing group members, ways to track the status of those distributed parts, ways to pull completed objects back together again. The limit of electronic mail and computer conferencing is that they have such features for managing messages only. CSCW widens the technology's scope of application to all the objects we deal with. (Greif, 1988b, pp. 7 f.)

Greif's judgment that 'the model breaks down' completely matched the diagnosis that had been made in the European 'computer-mediated communications' research community. It is also significant that Greif had reached strikingly similar conclusions with respect to the new research program: 'Methodologies for testing individual user interfaces don't apply as well to group support systems. As a result, CSCW is looking more to anthropology to find methodologies for studying groups at work in their natural settings' (Greif, 1988b, p. 10).

In short, it was becoming clear that the 'computer-mediated communications' research program was deeply flawed in its underlying 'message handling' outlook, in its focus on communication in the abstract, divorced from the work practices of which it normally is an integral part, but also severely limited in the way it conceived of the role of empirical studies in technological development. It was becoming clear, at least to some, that in-depth studies of cooperative work practices in 'natural settings' was a prerequisite.

The technologies of computer-mediated communication had not failed in any direct sense, nor had their potentials been exhausted. What had happened was what the historian of technology Edward Constant, adopting Kuhn's concept of 'anomaly', has termed 'presumptive anomaly'. It occurs, not when systems based on the technology fail ‘in any absolute or objective sense’ but when it is assumed or known that the technology in question is seriously limited and that it will fail or be inadequate under certain conditions: ‘No functional failure exists; an anomaly is presumed to exist; hence presumptive anomaly’ (Constant, 1980, p. 15). Take, for example, the turbojet revolution as described by Constant. This technological development did not come about because conventional propeller aircraft technology had ‘failed or faltered by any means or measure: it still held out a great deal of development still to be done; it still promised and in the event delivered greatly increased performance’. But the insights of aerodynamics indicated that the conventional technology would fail when aircraft approached the speed of sound and probably would become inefficient (in terms of speed, fuel efficiency) relative to alternative technologies such as turbojet propulsion (Constant, 1980, pp. 15 f. *et passim*).

The message-handling technologies were seen as having landed in a similar presumptive crisis: on the communication-mediation paradigm, predicated on technologies of message handling, it would not be possible to address the coordination challenges of ordinary cooperative work in a way that integrated communication and coordination with everyday work practices and techniques. It was clear that message-handling technologies had critical limitations. That message-handling technologies were found critically limited with respect to work practices does not mean, of course, that they were found useless for cooperative work settings or for other settings. Nor does it mean that message-handling technologies could not be further developed, refined, etc. It just means that they were and remain of marginal relevance to key coordinative issues in cooperative work settings.[29]

3.3 Automation of Articulation Work: ‘Office Automation’

At the same time as it was becoming clear to many ‘computer-mediated communications’ researchers, especially in Europe, that the ‘message handling’ paradigm was at odds with typical everyday cooperative work practices and that the paradigm thus had to be overcome, researchers in the ‘office automation’ movement were arriving at similar conclusions, although their point of departure was of course entirely different.

The ‘office automation’ movement had begun in high spirits in the 1970s, stimulated by different but intersecting technical developments. As with ‘computer-mediated communications’, the baseline was the advent of computer networks. But the approach was radically different. Instead of conceiving of computer networks as a ‘medium’, that is, as a facility that regulates human interaction in negligible ways, the ‘office automation’ program deliberately aimed at regulating interaction in significant ways. The seminal idea was that various new techniques for constructing

[29] However, this also meant that CSCW as an institutionalized research field would be an arena in which different research programs that do not address the same problem and are characterized by distinctly different research modalities would co-exist.

executable models that had been invented made it worthwhile to explore whether and to which extent such representations might be exploited as a means of modelling and regulating 'office procedures' and other kinds of workflows: on one hand, the algebraic techniques for building computational models of distributed systems developed by Petri and others since the early 1960s (cf., e.g., Zisman, 1977; Ellis, 1979) and, on the other hand, the equally sophisticated techniques for constructing complex adaptive models developed under the Artificial Intelligence label (cf., e.g., Hewitt, 1977; Fikes and Henderson, 1980; Barber and Hewitt, 1982). These hopes were soon defeated, however. Experimental applications such as DOMINO turned out to be felt like 'straitjackets' in actual use (Kreifelts, 1984; Victor and Sommer, 1989; Kreifelts et al., 1991a, 1993). Comparable lessons were learned from the CHAOS experiment (De Cindio et al., 1988; Bignoli and Simone, 1989; Simone, 1993; Simone and Divitini, 1999). That is, 'office work' was not at all as easily captured and modelled as had been presumed. Handling contingencies and dealing with inconsistencies turned out to be an essential aspect of cooperative work practices. The 'office automation' program had landed in a crisis of its own.

At this point a new approach to technological research was devised: a few sociologists became involved in the effort to understand the status of 'office procedures' and cooperative work in general, on one hand Lucy Suchman and Eleanor Wynn (Wynn, 1979; Suchman, 1982, 1983; Suchman and Wynn, 1984) and on the other Eli Gerson and Susan Leigh Star (Gerson and Star, 1986).

That this coupling of sociological and technological research would first occur in the 'office automation' movement was hardly accidental. Email and most other communication technologies were devised by computer technicians *for their own use*. That is, they were developed in a distributed and incremental fashion to solve local problems in practices that were well-known to the designers; and as they were found to be of general utility they were then—*post festum*—subjected to standardization and design. Their development did not require workplace studies of any kind. On the other hand, computer-conferencing systems were developed in a proactive manner; they were strictly speaking *designed*. But their design was based on normative models of what was claimed to be rational decision making, not on what was taken to be a well-grounded understanding of an actual practice. By contrast, however unrealistic the experimental designs of the 'office automation' movement turned out to be, nobody were under the illusion that one workflow model would fit all, and each workflow model was presumed to be empirically valid. That is, building technical systems that regulate actions and interactions in the strong sense envisioned by the 'office automation' movement was unproblematically thought to require some kind of analysis and modelling of existing procedures. When the models ultimately turned out not to work as anticipated, the natural next step was to look more carefully at the reality of 'office work'.

This is anyway what happened. And it was also realized, eventually, that the problem was not just with this or that particular model or modelling technique. It was realized that the problem was conceptual. Those early studies of 'office work' indicated that received concepts of cooperative work as mere 'execution' of preconceived 'procedures' were inherently problematic. This point was driven home, emphatically, both by Gerson and Star and by Suchman in her contemporaneous critique of the concept of ‘plans’ in cognitive science (Suchman, 1987).

This insight was a fatal blow to the conceptual basis of the ‘office automation’ movement.

3.4 The CSCW Research Program

The work of Suchman, Wynn, Gerson, and Star had significance beyond these, as it were, *immediate* implications. It showed, *by way of example*, that in-depth studies of actual working practices could have strong impact on conceptual issues in the development of computing technologies. It demonstrated the adequacy of, and indeed need for, empirical studies of work practices on the model of Réaumur, Perronet, and Beckmann—even in this area of, putatively, ‘applied’ science.

This, in my view, was the defining moment of CSCW. The early contributions by Wynn, Suchman, Gerson, and Star provided the ‘exemplars’, in a Kuhnian sense, for defining a new research program in which in-depth studies of cooperative work ‘in the wild’ were considered a prerequisite for developing computer technologies for human interaction. However, we should remember that new research paradigms are not necessarily heralded as such when they arrive on the scene. In fact, as pointed out by Kuhn, ‘we must recognize how very limited in both scope and precision a paradigm can be at the time of its first appearance’. Thus the ‘success of a paradigm [. . .] is at the start largely a promise of success discoverable in selected and still incomplete examples.’ (Kuhn, 1962, pp. 23 f.). This observation certainly applies to the emergence of the practice-oriented research program of CSCW.

The exemplary role of these studies were not only a function of the findings or of the role of field work in producing them. In both cases the research was integral to settings in which computer scientists and sociologists were addressing the same set of problems. The work of Suchman and Wynn was, of course, an important part of the research at Xerox PARC where Suchman would later head a highly influential interdisciplinary group of researchers. It is less well known but important to note that the work of Gerson and Star anticipated much of was later to unfold in CSCW in that their research was part of a collaborative research network involving both sociologists and computer scientists. The network, which also included Carl Hewitt, Anselm Strauss, Rob Kling, Adele Clarke, Joan Fujimura, Walt Scacchi, and Les Gasser, brought together sociologists with a track record in workplace studies of health care and biological research work *as well as* computer scientists engaged in developing what would later be known as distributed AI and agent-based architectures.

So, when Liam Bannon and I wrote our programmatic article for the first European CSCW conference in 1989, *this* was the kind of work we had in mind:

> CSCW should be conceived as an endeavor to understand the nature and characteristics of cooperative work with the objective of designing adequate computer-based technologies. That is, CSCW is a research area addressing questions like the following: What are the specific characteristics of cooperative work as opposed to work performed by individuals

> in seclusion? What are the reasons for the emergence of cooperative work patterns? How can computer-based technology be applied to enhance cooperative work relations? How can computers be applied to alleviate the logistic problems of cooperative work? How should designers approach the complex and delicate problems of designing systems that will shape social relationships? And so forth. The focus is to *understand*, so as to *better support*, cooperative work. (Bannon and Schmidt, 1989, p. 360)

In sum, two intellectual movements merged in the formation of CSCW. On one hand, 'computer-mediated communication' as a technologically oriented research program had arrived at a stage where it was beginning to dawn on many participants that the program was barking up the wrong tree. It had been focusing on aspects of interaction ('communication') that were conceived of as divorced from work practices but which normally are an integral part of doing the work and deeply enmeshed in the materiality of the setting and its organizational conventions and procedures. To move beyond that impasse, it was found necessary to develop an understanding of actual cooperative work practices. On the other hand, the 'office automation' program had landed in a situation where it had become clear that formal organizational constructs such as prescribed procedures are not mere algorithmic subroutines but part and parcel of professional work practices. It was, again, found necessary to develop an understanding of actual cooperative work practices. Here the history of CSCW proper begins.

A note of clarification. When I point to the early work of Suchman, Wynn, Gerson, and Star as 'exemplars' of practice-oriented contributions to technological research, this of course does not mean that the formation CSCW was not part of a wider intellectual movement than circumscribed by Ethnomethodology and Symbolic Interactionism. To the contrary. It was, and is, a distinct research effort within a much broader movement that, in different ways, strives to understand computing in its social context. Thus the Participatory Design movement, which originated in Scandinavia (e.g., Bjerknes et al., 1987; Ehn, 1988), brought together computer scientists and others striving to understand the development and use of computing systems as embodied social practices (building on the work of Marx, Heidegger, Wittgenstein). Likewise, subversive elements within Artificial Intelligence such as Terry Winograd quite early had serious doubts as to the conceptual foundations of AI and defected, drawing on as diverse philosophical traditions as those of Heidegger and Austin (1986; 1986). In fact, AI was at the time under strong and effective external criticism by philosophers from the same traditions by, e.g., Dreyfus (1979) and Searle (1987, 1989) and from a Wittgensteinian tradition (e.g., Shanker, 1987b, c). At about the same time, a related movement away from cognitive science towards an 'ecological' and 'naturalistic' conception of computing based on Gibsonian psychology was unfolding in Human Factors engineering (e.g., Vicente and Rasmussen, 1992; Flach et al., 1995). When I nonetheless point to these early 'exemplars' it is because they, in different ways and from different intellectual traditions, demonstrated that in-depth studies of work practices could contribute to the development of the conceptual foundations of computing technology.

The fecundity of CSCW's practice-oriented program became evident immediately, even as the program was being tentatively articulated. The first report on the Lancaster group's study of air traffic control was presented to the incipient CSCW community in 1989 (Harper et al., 1989a) and was quickly followed by the equally emblematic study of the London Underground control room (Heath and Luff, 1991a). Nor did it take long for it to become clear that these new insights would have radical implications for not only the development of certain classes of applications but for underlying computer technologies. This was, for example, made explicit with respect to the research area of distributed systems by Rodden and Blair in their classic paper from 1991. Referring to the 'the rich patterns of cooperation found in CSCW' as depicted in the early harvest of ethnographic studies, the authors emphasized that coordinative practices are specific to work domain and setting and that cooperating ensembles work 'in dynamic and unexpected ways and are themselves dynamic'. Having examined distributed systems architectures in the light of this insight, Rodden and Blair concluded that 'existing approaches to control in distributed systems are inadequate' (Rodden and Blair, 1991, p. 49) and that the implications for technological research are fundamental:

> For example, consider the problem of shared access to resources. In most distributed systems this is dealt with by masking out the existence of other users. Hence sharing is transparent with each user unaware of the activity of others. This clearly contradicts the needs of CSCW. [. . .] The problem with this approach is that presumed control decisions are embedded into the system and hence cannot be avoided or tailored for specific classes of application. *This is the root of the problem in supporting CSCW*. Because of the dynamic requirements of CSCW applications, it is very unlikely that such prescribed solutions will be suitable. (Rodden and Blair, 1991, p. 59)

Rodden and Blair concluded that 'CSCW demands a fresh approach to control which is specifically tailored for cooperative working' (1991, p. 60). This was a crucial programmatic proposition. The key problem for CSCW is not 'communication' or 'resource sharing' but cooperating actors' control of their interaction and, by implication, of the computational regulation of their interaction. The 'root of the problem in supporting CSCW', they pointed out, is that coordinative protocols cannot be prescribed once and for all and that 'control decisions' must, ultimately, be in the hands of practitioners. This problem is fundamentally different from the issue of user control of system behavior in HCI, in that control in cooperative work settings is, in principle, distributed. In sum, then, informed by the findings of the initial ethnographic studies of cooperative work in real-work settings, Rodden and his colleagues, quite succinctly, articulated a line of research that radically challenges fundamental assumptions in computer science as expressed in the architecture of operating systems, network services (client-server protocols), database systems, etc. The 'root of the problem in supporting CSCW' has since then been spelled out and explored from different perspectives: 'event propagation mechanisms' for 'awareness' support, 'coordinative artifacts and protocols', and so on.

With the initiation of this line of research, the CSCW research program had so to speak been fully specified. The early studies by Wynn, Suchman, Gerson, and Star had demonstrated how sociological inquiries could address conceptual issues in technological research. The potential of the practice-oriented program, as exemplified by these early paradigms, had been decisively demonstrated in the studies of control room work and other studies that then made Rodden and his colleague subject distributed systems theory to critical scrutiny. Furthermore, the work of Rodden and his colleagues also had significance beyond its *immediate* implications for computer science. With their re-conceptualization of fundamental issues in distributed computing, CSCW's practice-oriented research program had been complemented by an exemplar of the correlative technological research. The reciprocality of the contributions of sociology and computer science respectively had been exemplified.

In sum, what had been articulated was a *technological* research program devoted to the development of computing technologies—but not just that: it was a quite special technological research program, namely, a technological research program that depended critically on conceptual contributions from sociology and anthropology. That is, the required empirical work was proactive in its orientation: intended to contribute to the conceptual foundation of the technology. In this regard, CSCW is quite extraordinary: technological development predicated on ethnographic studies

This research program thus differed distinctly from the kinds of sociological and socio-psychological studies of technology that had been conducted previously, in that the emphasis was on *development of technology*, not on evaluation of 'user satisfaction' with particular systems, and even less so on prognostications about the organizational or behavioral 'effect' and 'impact' of certain technologies.

3.5 Technology and Ethnography: An 'Odd Mix'?

CSCW seems like a strange bird. To some it looks like 'requirements engineering', 'systems development', or participatory design. Others, however, simply take it to be a special area within HCI. To others again it seems like a version of technology assessment or quality evaluation, or even an extension of media studies. What causes the confusion may be the seemingly paradoxical combination of technology development grounded in, of all things, ethnographic studies of work practices: an 'odd mix' indeed! (Finholt and Teasley, 1998).

The sense of 'oddity' is dispelled when CSCW is looked at in the light of history of technology. As pointed out above, technological research and development is immensely variegated. Techniques are generally developed in an 'empirical' process of incremental improvement of existing techniques, typically in reaction to known or anticipated bottlenecks, problems of reliability, performance limitations, production and maintenance costs, etc. The same, of course, applies to technologies, only that, with technologies, the development involves systematic analysis and rationalization of existing techniques and technologies. Still, technologies have systemic character; they form complexes of component and enabling technologies. Fortuitous arrival of new component or enabling technologies may suddenly propel a given technology forward. The microprocessor is a case in point. It was developed for desktop calculators but was then laterally shifted and appropriated as an essential component technology for personal computers.

Now, some technologies are developed in a proactive way, in an open but directed search for solutions to deal with a (known, anticipated, or imagined) societal problem or to create new possibilities. Obvious examples are railway technology, telegraphy, radio communication, turbojet, radar, space flight, satellite navigation, nuclear fusion energy—as well as digital electronic computing, real-time transaction processing and digital computer networking. In its orientation, CSCW belongs to this family of technology development efforts.

That is, in its orientation, the CSCW research program runs counter to the research program that dominates HCI. HCI represents a quite normal modus of technological research in computing technologies and in general. HCI research arrived on the scene after interactive computing, and it was from the beginning devoted to the conceptual rationalization and refinement of technologies for which the basic principles had already been developed, albeit not systematically articulated and conceptualized. But for CSCW's practice-oriented research program, the requisite technologies—the basic concepts and principles—hardly exist yet but have to be conceived and developed in a proactive technological research effort informed by ethnographic and other forms of workplace studies. That is, the two research fields, although closely related with respect to many component technologies but also, to some extent, with respect to the disciplines and methods involved, represent radically different modalities of research. This difference in research modus has been and remains a source of much confusion.

To make the confusion worse, and as shown above, email and other forms of 'computer-mediated communication' developed in a 'spontaneous' or 'empirical' (unintentional, distributed) process of innovation and dissemination made possible by the fact that technicians 'used the network protocols in their own work' and therefore had 'the incentive and the experience to create and improve new services' (Abbate, 1999, p. 69). Thus, just like HCI, CSCW was anticipated by decades of incremental innovation of computer-mediated communication techniques such as email, instant messaging, shared data sets, and so on, and to continue in this modus would therefore easily appear the natural and unproblematic way of proceeding. However, CSCW's practice-oriented research program reversed CSCW's research modus in the course of the early years of formation of CSCW. The practice-oriented CSCW research program is oriented in the opposite direction *vis-à-vis* practical technical innovation.

There is yet another source of confusion, however. The design of the very first computer applications for commercial purposes (payroll systems, etc.) were based on studies of actual practices. As early as 1953, the requirements analysis for one of the first business applications, the design of a program for the ordering of goods for Lyons Teashops in the UK, involved genuine field work (Ferry, 2003, pp. 121–129). What was new in CSCW was not the idea of doing requirements analysis as an integrated part of the process of building a particular system for a particular setting, incorporating an array of more or less well-known technologies in a way that serves the identified needs. What was new in CSCW was the idea of doing *workplace studies for the purpose of developing new technologies*.

In-depth studies of coordinative practices in the age-old tradition exemplified by the 'dry, thorny, and not at all dazzling' studies conducted two or three centuries ago by French academicians such as Réaumur and Perronet or by German scholars such as Beckmann and Poppe are critically important for CSCW. In that respect, there is nothing ‘odd’ in technological research informed by studies of work practice. However, the *specific function* of these studies in CSCW is not the improvement of the investigated practices in any immediate or direct sense[30] but the development of novel coordination technologies that may *then* be appropriated for the investigated practices (as well as others) in order to improve and transform these practices. In other words, in contrast to the classic technological studies of handicraft-based work practices, the practice-oriented research program of CSCW is not reactive (*post hoc* systematization and rationalization) but essentially proactive. In CSCW, the path from ethnographic studies to the conception of technical solutions to systems design is far more convoluted than in the classic technology studies and far more convoluted than suggested by the notion of ‘implications for design’ (Dourish, 2006b; Schmidt et al., 2007).

4 Accomplishments and Shortcomings

It is often intimated, if not indeed insinuated, that CSCW has not accomplished anything substantial. Or it is bluntly alleged that CSCW has not yet resulted in technologies in actual use.

The situation is more nuanced than that. Take Lotus Notes, for example. Developed in the late 1980s and early 1990s by a team at Lotus headed by Irene Greif, and originally released in 1989, it not only had a certified CSCW pedigree but also incorporated important early CSCW insights concerning the importance of integrating the provision of message-handling functionalities such as email and computer conferencing with a shared document database system over the Internet (Greif and Sarin, 1986; Kawell et al., 1988). More importantly, its example spawned a large and rather messy market for what is sometimes called ‘collaborative working environments’ or ‘integrated groupware platforms’ (e.g., Lotus Notes/DOMINO Microsoft SharePoint, Novell SiteScape, etc.). By 2000, about 70 different commercial platforms of this kind had been registered, and at about the same time more than 19,000 software houses world-wide were offering products for the Lotus Notes/DOMINO platform alone (Wilczek and Krcmar, 2001). It is estimated that the global market for these systems by 2005 was in the magnitude of $2 billion (Prinz, 2006). The number of users of such systems is estimated to be well above 200 million, a number that does not include Google’s Mail, Wave. Calendar, etc. So, if market size and actual use are the criteria, then something must be said to have been accomplished.

But in fact, the vast majority of what is being marketed as ‘collaborative environments’ represents the state of CSCW by the end of the 1980s, that is, prior to the

[30] Studies of coordinative practices with a view to their systematic rationalization would fall under the rubrics of operations research, logistics, etc. (e.g., Morse and Kimball, 1951). Some of the work of ‘scientific management’ practitioners also belong here. On the other hand, ‘Business Process Reengineering’ is too Bolshevistic in its general stance towards actual work practices to be considered in this context.

systematic coupling of workplace studies to the development of technologies could have an effect. Even more so, the design principles underlying these systems—one could call it the 'groupware' paradigm—derives from the prehistory of CSCW, not from ethnographic work in CSCW.

'Groupware' is basically defined as a package of computer-mediated communication functionalities such as email and instant messaging combined with shared repositories. These basic functionalities are now generally supplemented by calendar and address book functions. These functionalities are to some extent integrated, typically by offering elementary interoperability of calendar and messaging in the form of alarms, but also shared address lists, and so on. 'Groupware' is a category of products that have been shaped with a view to a mass market that not only includes team communications in work settings but also any setting where people may form teams or groups. It is as close to a generic product as it can get. 'Groupware' packages are expressions of a design strategy that offers integration of messaging and file repositories *within the framework of existing operating systems architectures*, that is, as yet another application next to other applications. From the point of view of commercial software design, this may be rational, for one thereby dodges the challenge of developing computational coordinative protocols that can be invoked by any application.

The point is that in the design of these products the thicket of cooperative work practices has been carefully avoided. The technical solutions to the problems CSCW is addressing will have to be incorporated in all kinds of software machines, from operating systems and network protocols to domain-specific applications (desktop publishing, computer-aided design, production planning and control, electronic patient records, and so forth). CSCW and Groupware are not co-extensive fields. The conceptual horizon of groupware ends where that of the CSCW research program begins.

4.1 Ethnography and Technology: A 'Big Discrepancy'?

When allegations about CSCW's lack of fecundity are being made, what is implicitly expected is that *what is unique* to CSCW should have led to novel technologies, namely, the systematic coupling of ethnographic and other forms of in-depth workplace studies to the development of technologies for cooperative work settings. And the impression is that *this* coupling is not working properly.

A widely cited source of the impression that there is such a problem, a 'gap', in the transmission of insights from workplace studies to 'design' is an article by Plowman, Rogers, and Ramage in which they reported on a survey of a large part of the workplace studies that had by then been published in the area of CSCW (1995). In the article, the authors asserted to have found a 'paucity of papers detailing specific design guidelines' (p. 313). They carefully and explicitly refrained from concluding that 'workplace studies do not produce specific design guidelines', but nevertheless suggested that the observed paucity 'can be attributed to the lack of reported research which has developed to the stage of a system prototype' (ibid.).

They went on to surmise that what impeded the progression from workplace studies to 'the stage of system prototype' could be what they described as 'a big discrepancy between accounts of sociality generated by field studies and the way information can be of practical use to system developers' (p. 321). Not surprisingly, this conjecture (and others in the same vein) has fuelled widespread concern and continual discussion about the role of workplace studies in CSCW (Dourish and Button, 1998; Crabtree et al., 2005; Dourish, 2006b; Randall et al., 2007a; Crabtree et al., 2009).

It is undoubtedly true that the sample analyzed by Plowman, Rogers, and Ramage showed only few papers 'detailing specific design guidelines'. However, their assertion that there is a 'lack of reported research' informed by workplace studies 'which has developed to the stage of a system prototype' was false. As pointed out in my contribution to the collection on *Workplace Studies* edited by Luff et al. (2000), the alleged 'paucity' does not 'reflect on the actual impact of workplace studies on the development of CSCW technologies' (Schmidt, 2000, p. 146). It is simply an artifact of a flawed method: the 'histories of how workplace studies inform the development of CSCW technologies' are 'not always readily visible in papers reporting on findings from workplace studies. The transfer of findings and insights typically happens in the course of discussions within cross-disciplinary research teams and are often only documented in design-oriented papers' (Schmidt, 2000, p. 148, n. 3).

In other words, the presupposition that there is an immediate and direct route from workplace study to 'implications for design' to 'detailed design guidelines' to the 'stage of system prototype' reflects a widespread but simplistic notion of the development of technology. To investigate the development of knowledge in any field, and most certainly the development of technology in an interdisciplinary field such as CSCW, would require that one traces and maps out the complex pattern formed by the more or less tentative articulations, interpretations, categorizations, conceptualizations of observed phenomena; the ways in which concepts and ideas percolate and propagate within the research community, as manifested in more or less sporadic citation patterns or in the often elusive cross-fertilization among researchers in collaborating laboratories and research projects; and the various simultaneous or subsequent generalizations and transformations of these concepts and ideas as they are appropriated by other actors, perhaps pursuing diverging aims and addressing different problems and issues. Thus, in the case of the impact of workplace studies in CSCW on technological development one would have to investigate the (partial and patchy) conceptual framework emerging from the propagation and transformation of concepts derived from workplace studies, such as 'awareness', 'boundary object', 'artifact', 'work setting', 'coordinative protocol', etc. and how this (partial and patchy) conceptual framework is being interpreted and appropriated in different ways in technological research.

There is another source of ambiguity in determining the form and extent of the impact of workplace studies on technical research in CSCW. Workplace studies in CSCW play two major roles. On one hand, workplace studies have served the critical role of demonstrating the 'socially organized' character of human activity. This critical role is obvious and has been the cause of justifiable celebration.

For instance, in 2002 Jack Carroll published a collection of articles (under the title *Human-Computer Interaction in the New Millennium* to take stock of the accomplishments of HCI and discuss the challenges and opportunities facing the field, in which he makes the following observation:

> At first, the focus [of CSCW research] was on collaborative systems (groupware) and human-computer interactions with collaborative systems. But the more significant impact of CSCW, one that goes beyond the conference, is the recognition that all systems are used in a social context. In this sense. CSCW has become more a view of HCI than a subcommunity with[in] it. CSCW has served as a conduit for the expansion of the science foundation of HCI to incorporate activity theory, ethnomethodology, and conversation analysis, among others. (Carroll, 2002, p. xxxiii)

This is a distinguished accomplishment by any standard. On the other hand, however, workplace studies also serve the constructive role of being instrumental in providing, inductively, the empirical and conceptual basis for the technology development that CSCW is ultimately all about. This role is much less obvious and not at all well understood.

4.2 The Case of 'Awareness Engines'

To take but one example: the concept of 'awareness'. As CSCW formed as a research field, it was quickly established that the coordination and integration of cooperative work activities typically involve highly sophisticated 'awareness' practices, i.e., practices of 'monitoring' unfolding occurrences in the setting and, conversely, of making local occurrences 'publicly visible' for others in the setting (e.g., Harper et al., 1989a; Heath and Luff, 1991a). These practices, that are generally named 'mutual awareness', are crucial because they are practically effortless. Although they involve delicate techniques, they typically require no noticeable effort of skilled practitioners because they are spatio-temporally integrated with the activities of doing the work. Skilled actors are able to coordinate and integrate their interdependent activities without having to interrupt the flow of work. In fact, to be able to do so is typically what is required to be considered competent in a given cooperative work practice.

This insight prompted intensive research into different ways of providing computational support for these subtle practices, and over the years different approaches have been developed and explored (for an overview, cf. Schmidt, 2002b). However, what is interesting here is, first of all, that this rich line of research, while inconclusive, has resulted in facilities that are in actual use.

The obvious instance of this is the early web-based groupware system named Basic Support of Cooperative Work (BSCW), initially developed at GMD in Germany in the mid 1990s. In contrast to the line of 'collaborative environments' in the Lotus Notes tradition, BSCW offers elementary support for 'mutual awareness'. Users can see traces of actions on documents in the workspace in which they are participants. Each user can, for example, see if and when another user has 'read' a document, changed its name and description, moved it, and so on. Not only is

BSCW of CSCW provenance; the design of the system was clearly informed by the findings of early ethnographic studies (Mariani and Prinz, 1993; Bentley et al., 1997a, b; Appelt, 1999).

BSCW is currently installed on more than 1,000 servers, typically at research institutions, servicing 'tens of thousands of workgroups and hundreds of thousands of users' (Orbiteam, 2009). Log-analysis studies of patterns of usage in BSCW show that the 'awareness service' is widely used, especially by experienced users (Appelt, 2001). Nevertheless, the 'awareness service' technology as represented by BSCW is crude and primitive. Its major limitation is that the massively distributed nature of awareness practices is severely curtailed by the architecture. The shared 'workspace' (i.e., a directory) is a distinct set of objects to which a user obtains access by invitation (although 'rights' may differ), and events thus cannot propagate beyond the 'workspace' as defined by the inheritance rules of the directory structure. In short, BSCW's 'awareness service' should merely be taken as an inspired first shot at how computational artifacts might be designed to support the observed 'awareness' practices (for a critical analysis of BSCW, cf. Schümmer and Lukosch, 2007, pp. 501–525).

The picture of 'awareness' practices that emerged from the initial ethnographic studies, was one of horizontal adaptation in a massively distributed mode of interaction. Cooperative work is essentially characterized by distributed control. Now, work plans and conventions and the concomitant coordinative artifacts of course play a critical role in reducing the effort of handling the complex interdependencies by providing the normative background of taken-for-granted expectations ('He's supposed to have finished *x* by now, and... yes indeed, there it is!'). But they can be seen as 'local and temporary closures', to use the apt expression of Gerson and Star (1986), in contrast to which 'awareness' practices can very well be understood as 'the work to make work work'. The importance attributed to the concept of 'awareness' in CSCW research largely derives from this insight, and there is a close conceptual affinity between concepts like 'situated action', 'articulation work', and 'mutual awareness'. That is, the defining feature of 'awareness' practices as the concept emerged from the ethnographic record is that of massive distributedness.

In direct continuation of these findings, CSCW research into how to 'support' awareness practices has increasingly and predominantly pursued calculi of distributed computation so as to be able to express, facilitate, emulate collaborating workers' practices of paying heed to occurrences in their work settings. An early example is the 'spatial' or 'aura-focus-nimbus' model that was developed by Benford and others in the COMIC project (Benford and Fahlén, 1993; Benford et al., 1994a, b) and was later generalized by Rodden (1996) and Sandor et al. (1997). Another approach, suggested by Simone and Bandini (1997, 2002), explored the possible utility of the reaction-diffusion model as a way of representing and facilitating the distributed propagation of state changes within a cooperative work setting.

What characterizes these otherwise very different approaches is that they all, in the words of Sandor et al., attempt 'to integrate awareness support at fundamental levels of cooperative system architecture' (Sandor et al., 1997, p. 222). As noted above, this approach was explicitly 'informed' by the ethnographic record (cf., e.g., Rodden and Blair, 1991; Rodden et al., 1992). In other words, it was realized at a very early stage in CSCW that, since 'awareness' practices are an integral aspect of cooperative work practices, 'awareness support' cannot be provided by a special kind of application, on par with, say, email or instant messaging; it has to be provided as a service available to *all* applications. It was therefore realized that 'awareness support' would have to be provided 'at fundamental levels of cooperative system architecture', that is, at the level of the operating system of networked computational artifacts. In short, the early ethnographic studies were almost immediately seen to have radical 'implications for design' with respect to operating systems architectures, database technology, and so on.[31] The 'groupware' model was no longer viable as a model for CSCW.

This had further implications. It meant that the transition from workplace studies to the development of new technologies for, e.g., 'awareness support' would become quite complex. Major conceptual and technical issues had—and have—to be identified and resolved, and at the same time the enormous investment in the installed base of computer platforms and application software poses a major buffer against anything like swift change. This in turn has had the effect that 'awareness support' provided by IT systems in actual use has stayed at the level represented by the rather crude facilities of BSCW or the even cruder 'awareness support' offered by instant messaging where users are notified (by icons or sounds) of the on-line presence of 'buddies'.

The case of 'awareness support' is illuminating in that it also demonstrates *internal* sources of seeming stagnation. The trajectory of the line of technological research on 'awareness support' that is informed by ethnographic studies is strikingly convoluted.

What was initially being pursued was a calculus for modelling massively distributed interactions among entities. It goes without saying that facilities for integrating 'awareness support' at fundamental levels of operating system architectures will have to be quite generic, but these facilities will at the same time also have to be able to support the domain-specific character of these practices.

The ethnographic finding that practices of *heeding* (to use a less misleading term than 'awareness') are domain-specific practices seem to have been put aside, temporarily perhaps, in favor of advancing the ability to model distributed social processes in general. In cooperative work, actors skillfully 'monitor for' certain cues, signals, etc. in the setting and, conversely, skillfully 'display' certain cues, signals, etc. concerning their local activities as relevant to colleagues. That is, practices of heeding in cooperative work is first of all not a mental state acquired 'passively' (as the term 'awareness' might suggest) but a practical stance of monitoring (or displaying), and the categories of cues, signals, etc. that actors are monitoring for (or displaying) are specific to the given domain of work and the work setting.

[31] Identical conclusions have been drawn from other workplace studies investigating 'coordination mechanisms' in cooperative work settings (Schmidt et al., 1993; Schmidt, 1994d).

Practices of heeding are not generic abilities; they are skills practitioners only master with training and experience, highly elaborate practices that are virtually effortless because they are an integral aspect of the domain-specific work practices. And like any other practice, they are essentially conventional and thus normative ('What are you doing?! Are you asleep?' or 'Don't worry; I see the problem and can handle it!'). Without facilities that support actors in these skilled techniques, by allowing them to express the categories of cues, signals etc. they monitoring for (or displaying), in short, the protocols of their heeding practices, computational support of 'mutual awareness' is unlikely to proceed significantly beyond the rudimentary 'awareness service' of BSCW or similar.

There may have been sensible intellectual economy in putting these issues aside. It is interesting, however, that recent research has begun to address those very issues. Gross and Prinz have for example presented an 'Event and Notification Infrastructure' that aims at providing 'awareness information in a way that is adequate for the current situation of the user'. Gross and Prinz emphasize that 'context itself depends on parameters like the current task, the current type of cooperation, the artefacts and tools used, and so forth' (Gross and Prinz, 2003, p. 296). In fact, recognizing that heeding practices, like any other practice, are essentially normative, this research aims at developing facilities for contextual awareness support that 'will allow users to establish conventions' (Gross and Prinz, 2004, p. 300).

This brings us back to the problem of the putative 'gap' or 'discrepancy' in CSCW. The problem is not that there is a 'lack of reported research [informed by workplace studies] which has developed to the stage of a system prototype'. There is evidently no such lack; in fact, the ethnographic findings have obviously engendered a rich tradition even in the fairly esoteric area of 'awareness support'.

This should not be taken to imply that the relationship between workplace studies and development of technology is unproblematic. Workplace studies are no substitute for serious and meticulous conceptualization. On the contrary, and to paraphrase Garfinkel, workplace studies in CSCW serve 'as aids to a sluggish imagination' (Garfinkel, 1967, p. 38). The function of workplace studies is not to produce 'implications for design' or anything of the sort but first of all to challenge taken-for-granted assumptions about cooperative work and coordinative practices and thus kindle an otherwise sluggish technical imagination. Thus, bringing findings from ethnographic studies of cooperative work to bear on technological development involves conceptual work—or rather: it essentially consists in conceptual work. As suggested above, there are two aspects to this: a critical one and a constructive one.

The conceptual work of bringing findings from ethnographic studies of cooperative work to bear on technological development involves dissolving run-of-the-mill constructions seeping in from the various disciplines and from the metaphysics of common-sense theorizing (e.g., 'shared goals'), questioning what is inadvertently taken as prototypical (e.g., 'face-to-face' interaction in 'teams'), sorting out category mistakes and the ubiquitous transgressions of sense (e.g., 'media richness'), and so on. However, the research on 'awareness support' stumbled in this critical effort.

The early ethnographic studies did not interpret the findings in terms of 'awareness'. In fact, the term 'awareness' does not appear there. The findings were nonetheless soon interpreted in the light of that concept. Now, since 'awareness' is an 'attention word', not a 'heed word' (Ryle, 1949; White, 1964), this caused significant confusion. The ethnographic findings were implicitly given a mentalistic interpretation by being subsumed under the notion of 'awareness' that seems to have been imported from social psychology and small-group sociology. The findings produced in the early ethnographic studies were thereby, so to speak, contaminated by abstract constructs, obviously modelled on so-called 'face-to-face interaction', that had more affinity to Goffman's notion of 'focused' and 'unfocused' interaction (Goffman, 1961, 1963) than with the highly specific findings articulated by the early ethnographic record. The result was a mentalistic notion of 'awareness' from which actual practices and protocols had somehow been excluded. The mentalistic interpretation in turn encouraged design experiments such as 'media spaces' in which much effort was spent on trying to recreate, with as much fidelity as possible, the putative paragon of all human interaction: the 'face-to-face' chat in the office corridor (for critical analyses, cf. Gaver, 1992; Heath and Luff, 1993).

As a result, much of the crucial insight gained by the ethnographic findings was easily overlaid by concepts from social psychology such as 'shared understanding', 'shared goals', etc. in which the very practices through which 'understanding' or 'goals' become 'shared', i.e., unproblematically aligned, taken-for-granted, etc., are glossed over. Consequently, many of the initial explorations into 'awareness' services were experiments with—exactly—shared data sets facilitated by joint access to a directory on a server, perhaps augmented by a notification service. Sometimes this was graphically dressed up in a 'room' metaphor. However, it was quickly realized that the conception of cooperative work arrangements underlying this design was faulty, predicated as it was on the deeply problematic idea of the 'group' or 'team' as a well defined and clearly bounded unit. Researchers accordingly began to deconstruct the notion of cooperative work arrangements as defined in terms of physical space (cf., e.g., Fitzpatrick et al., 1996; Harrison and Dourish, 1996; Dourish, 2006a; Ciolfi et al., 2008).

Bringing findings from ethnographic studies of cooperative work to bear on technological development also requires the constructive task of reconstructing the logic of 'awareness' practices, the protocols or conventions that competent actors routinely follow and expect others to follow, and the ways in which such protocols or conventions are established and maintained. This kind of investigation cannot be driven by the way sociology or anthropology frames problems. To be able to 'inform design' this kind of investigation must be informed by the way technological research tentatively frames *its* problems. It is in this context noteworthy that another group of researchers, led by Carla Simone, in pursuing what seems to be the exact same research problem, has resolved that the ethnographic evidence of heeding practices that was produced about twenty years ago (in a different intellectual milieu and in pursuit of far less specific research questions), now needs to be complemented and enriched. Simone and her colleagues have therefore undertaken new ethnographic studies of awareness practices with an explicit focus on the 'conventional' or normative nature of these practices (Cabitza et al., 2007; Cabitza and Simone, 2007; Cabitza et al., 2009).

The point of all this, then, is that the transition from ethnographic studies to technological development is an immensely complex effort. On closer inspection, what may look like a 'gap' turns out to be a maze of pathways. Some lines of research lead straight to useful and innovative applications. Other lines of research turn out to be futile. Yet other lines of research turn out to raise questions that require additional ethnographic work. And so on.

That said, while there is no 'gap' or 'discrepancy' between workplace studies and technological development, the impression that *something* is amiss in CSCW is not entirely wrong. I suggest that the perceived sluggishness, the impression that progress is at best intermittent and hesitant, at worst that things are going in circles or backwards—to a large extent is precipitated by the increased fragmentation of CSCW.

4.3 Logics of Fragmentation

There is an interesting logic to the eventual fragmentation of CSCW.

As always with any kind of research, the presence of distinctly different paradigms is of course a recipe for trouble. CSCW is certainly no exception. More specifically, the continued presence of research within CSCW conducted on the 'groupware' paradigm would itself make the very concepts of 'work' and 'work practice' problematic. In a 'groupware' perspective the term 'work' does not mean *work practice*, for that concept is alien within a 'groupware' paradigm; but rather, the term 'work' merely stands proxy for 'workplace' or 'team', for what is addressed is *a priori* delimited to communication in abstraction from the specific skills, techniques, procedures, and material settings involved in work. In 'computer-mediated communications' or 'groupware' systems research, 'work' as a research phenomenon has been transubstantiated, in much the same way as 'work' is spirited away from much of 'sociology of work' (Sharrock and Anderson, 1986, Chapter 6). It was therefore but a small step for 'groupware' research to abandon the pretense that this research has anything to do with 'work' in any ordinary sense.

On the other hand, however, the practice-oriented research program crumbled from within. It has made significant progress but has not succeeded in showing convincing technical solutions to the problem of computational regulation of cooperative work in complex settings. This is not surprising. In contrast to 'groupware' research and development, the practice-oriented research program is not about developing a new class of applications on par with other applications but is about developing technologies that will provide all applications with coordinative functionality. That is, CSCW has to break the 'groupware' barrier. What is required is technologies to support workers in coordinating their cooperative activities in the domain-specific categories of their work and from within their domain-specific application programs, and the coordinative functionality thus has to be provided by computational artifacts that—as separate control mechanisms residing in the network operating system—have to interface with the data structures and transformation processes of the application programs. This applies to 'awareness support' as well as 'workflows' or 'ontologies'. To put it directly, CSCW research is developing a new kind of technology that, compared to the large installed base of polished 'groupware' products, does not have much to show for it—not until all the elements are there and have been put together, that is.

But these problems of public perception are the least of it. The real issue is that the CSCW research program was never clearly articulated. Great progress was achieved in involving workplace studies in technological research, but at the same time the very notion of *work practice* has been vacated, emptied of other content than the notion of mere contingent activity, disconnected from the concepts of rules and regulations. The practice-oriented program of CSCW—the very idea of computational regulation of interaction—has ended in contradiction. On one hand 'groupware' is seen as severely limited because it is predicated on communication divorced from work practice, but on the other hand the integration of computational functionality into the coordinative practices is seen as conceptually wrongheaded. This contradiction has, in the end, made the CSCW research program largely paralyzed.

Critical workplace studies certainly have flourished and have been justly celebrated, but constructive workplace studies, goal-directed studies of coordinative practices, protocols, logics of combination and recombination, have only progressed slowly and sporadically. So, when frivolous applications of computing technologies became a mass market and seized the imagination of the public, with such frail foundations the practice-oriented research program was a push-over.

Chapter 12
Frail Foundations

CSCW, as a research area, is in disarray. Not only are there different schools of thought, but the different communities are not investigating the same phenomenon or the same kind of phenomena, nor do they engage in any kind of discourse about findings. In fact, they would not even be able to compare notes.

This would be a serious predicament for any research area. For an interdisciplinary area this is fatal. What normally unites a research area or a discipline is the common framework of exemplars, methods, techniques, textbooks, educational programs and institutions, etc. In an interdisciplinary field, all this is missing, for here all or most of this scaffolding belongs to the constitutive disciplines. What unites an interdisciplinary area is, by and large, merely the research problem its members have gathered to investigate in their different ways and the 'cornerstone' concepts in which the problem is identified and expressed. But if the very conceptual foundation, the set of cornerstone concepts, is muddled, incoherent, disputed, then none of the other factors that otherwise assist an area in conceptual crisis to get through to the other side are there to stabilize the area. Accordingly, without reasonable clarity about CSCW's conceptual foundation, its research problem, the research community has no accountable criteria of quality, relevance, priority, directions. Its research program dissolves; surpassed paradigms linger on among the living; arguments erupt about shifting the area's focus this way or that way or even 'widening' it. CSCW is therefore subjected to centrifugal forces that are tearing it to pieces: disciplinary chauvinism, distractions of shifting funding schemes, changing technical fashions, frivolous media interests, etc.

Why is CSCW is such disarray? Why has CSCW been unable to effectively supersede the various lines of research focusing on mediation of communication? Why has there, in fact, been a regression to versions of 'computer-mediated communications' that focus on evaluation studies of new applications of the well-known technologies of exchanges of messages and files? There are several reasons for that. One is surely that CSCW researchers have been reluctant to pursue the design of computational artifacts for regulating cooperative work and thus are inclined to conceive of 'Computer-Supported...' as something close to 'Computer-Mediated...'. Their motives may be different: they may, based on experience, find existing coordination technologies of this sort intolerably rigid; they may be inhibited for ideological reasons; or they may be of the persuasion that regulation by machines

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of human activity inherently impossible, a conceptual chimera. In fact, the motives may be mixed, but the latter seems to be the one that has carried most weight.

Reluctance to pursue the design of computational artifacts for regulating cooperative work may also go a long way to explain the urge to move the focus of CSCW 'away from work', for in leisure and domestic settings there is no compelling need for regulation of interaction by means of sophisticated coordinative artifacts and, hence, neither by means of computational artifacts, and even less so is there a need for actors to modify and construct computational artifacts to regulate interaction. Such practices typically belong to professional work in complex settings. And on the other hand, communication technologies such as email, http, messaging, chat, etc. are not domain-specific; they do not even have a domain bias. They are as generic as pen and paper. That is, when the focus is on these generic communication technologies there is no reason whatsoever for focusing on cooperative work. Hence, I suggest, the fragmentation of CSCW.

That is, there is deep confusion in our understanding of work practice, of action and plan, of computation, and hence of the very issue of computational regulation of cooperative work. In short, there is considerable confusion in CSCW concerning the field's conceptual foundation.

The conceptual confusion is double sided: on one hand, the notion of 'plans' or 'procedures' versus 'situated action', and on the other hand, the notion of 'computation' inherent in any conception of computational regulation. These notions are all fraught with confusion and mystification. I made a modest attempt to elucidate the concepts of 'plans' and 'situated action' many years ago (Schmidt, 1994d, 1997), albeit with little apparent success. Suchman, for instance, never dignified the critique with a reply (cf., for example, Suchman, 2007). There may be several reasons for this. Some would, for example, argue that I misunderstood Suchman's argument and made a fuss about nothing. Alternatively, though, my argumentation may have been too hushed to be heard and too timid to cut the mustard. In any event, I was certainly influenced by what I was trying to critique, sharing in some ways the confusion and mystification I was trying to dispel, and hence not able to articulate my concerns and objections with sufficient clarity to be effective. Therefore, as a contribution to clarifying CSCW's program and thereby, I hope, help bringing it out of the current crisis, I will try to disentangle the issue of 'plans and situated actions' and then move on to the concepts of computation and computational regulation.

1 Suchman *vs.* Cognitivism

Suchman's book is a sharp and generally well-articulated critique of 'cognitive science' as exemplified by the cognitivist tradition within cognitive psychology as well as by its intellectual counterparts in 'artificial intelligence' and the offspring of this in the form of 'expert systems' and 'office automation'. The focal point of her critique is the concept of 'plans' as it was conceived of by cognitivist theorists. Her target was the view 'that purposeful action is determined by plans' and that this was considered '*the* correct model of the rational actor', and what motivated her critique was the observation that this view was 'being reified in the design of intelligent machines' (Suchman, 1987, p. ix).

In this regard, the formulation given by George Miller, Eugene Gelernter, and Karl Pribram in their book *Plans and the Structure of Behavior* (1960) is representative:

> Any complete description of behavior should be adequate to serve as a set of instructions, that is, it should have the characteristics of a plan that could guide the action described. When we speak of a Plan in these pages, however, the term will refer to a *hierarchy* of instructions, and the capitalization will indicate that this special interpretation is intended. *A Plan is any hierarchical process in the organism that can control the order in which a sequence of operations is to be performed*.
>
> A Plan is, for an organism, essentially the same as a program for a computer. (Miller et al., 1960, p. 16; quoted in Suchman, 1987, pp. 36 f.)

Suchman summarized the basic tenets of the movement as follows:

> The cognitivist strategy is to interject a mental operation between environmental stimulus and behavioral response: in essence, to relocate the causes of action from the environment that impinges upon the actor to processes, abstractable as computation, in the actor's head. The first premise of cognitive science, therefore, is that people–or "cognizers" of any sort–act on the basis of symbolic representations: a kind of cognitive code, instantiated physically in the brain [. . .]. The agreement among all participants in cognitive science and its affiliated disciplines, however, is that cognition is not just potentially *like* computation, it literally is computational. (Suchman, 1987, p. 9)

In opposition to this view and drawing on various resources, principally anthropology and sociology (e.g., ethnomethodology and conversation analysis), Suchman developed a powerful argument based on the concept of 'situated actions': 'By situated actions I mean simply actions taken in the context of particular, concrete circumstances' (p. viii). Her key argument is that

> however planned, purposeful actions are inevitably *situated actions* [. . .] because the circumstances of our actions are never fully anticipated and are continuously changing around us. As a consequence our actions, while systematic, are never planned in the strong sense that cognitive science would have it. Rather, plans are best viewed as a weak resource for what is primarily *ad hoc* activity. It is only when we are pressed to account for the rationality of our actions [. . .] that we invoke the guidance of a plan. Stated in advance, plans are necessarily vague, insofar as they must accommodate the unforeseeable contingencies of particular situations. Reconstructed in retrospect, plans systematically filter out precisely the particularity of detail that characterizes situated actions, in favor of those aspects of the actions that can be seen to accord with the plan. (pp. viii f.)

Whereas the course of rational action, on the cognitivist view, is causally determined by some, putative, preformed 'plan', 'scheme', etc., Suchman argued for an alternative account:

> The alternative view is that plans are resources for situated action, but do not in any strong sense determine its course. While plans presuppose the embodied practices and changing circumstances of situated action, the efficiency of plans as representations comes precisely from the fact that they do not represent those practices and circumstances in and of their concrete detail. (p. 52)

This account is expressed in a slightly more specified manner by the end of the book:

> 'For situated action [. . .] the vagueness of plans is not a fault, but is ideally suited to the fact that the detail of intent and action must be contingent on the circumstantial and interactional particulars of actual situations'. 'Like other essentially linguistic representations, plans are efficient formulations of situated actions. By abstracting uniformities across situations, plans allow us to bring past experience and projected outcomes to bear on our present actions. As efficient formulations, however, the significance of plans turns on their relation back to the unique circumstances and unarticulated practices of situated activities.' (pp. 185 f.)

The influence Suchman's book has had is of course to a large extent due to this critique of cognitivism. In fact, for many researchers in the communities concerned with human factors of computing (HCI, PD, CSCW), the book had the effect of something like a declaration of independence. But it also, and not least, provided initial formulations of the practice-oriented research program for which her previous work on office procedures had provided one of the exemplars:

> I have introduced the term *situated action*. That term underscores the view that every course of action depends in essential ways upon its material and social circumstances. Rather than attempting to abstract action away from its circumstances and represent it as a rational plan, the approach is to study how people use their circumstances to achieve intelligent action. Rather than build a theory of action out of a theory of plans, the aim is to investigate how people produce and find evidence for plans in the course of situated action. More generally, rather than subsume the details of action under the study of plans, plans are subsumed by the larger problem of situated action. (p. 50)

Not only did Suchman offer an initial formulation of the practice-oriented research program; she did so in a way that from the very outset pointed to the concept of the embodiment of action and the materiality of work practices as foundational: 'all activity, even the most analytic, is fundamentally concrete and embodied' (p. viii); accordingly, the materiality of the work setting is not a liability but an asset to the practitioner: 'the world is there to be consulted should we choose to do so' (p. 47); 'plans presuppose the embodied practices and changing circumstances of situated action' (p. 52). In doing so, she emphasized and outlined critical aspects of the understanding of cooperative work that subsequently evolved in CSCW.

However, Suchman's book has also left an intellectual legacy that hampers CSCW with respect to addressing critical aspects of the real-world problems that it initially set out to address and which remain a domain it is uniquely equipped to address: the design and use of computational regulation devices as a means of dealing with the complexities of coordinative practices in cooperative work.

Suchman of course did not deny that 'plans' are produced and used, nor did she say or imply that 'plans' are more or less useless. The problems with her account are far more subtle than that. In my attempt to unravel these problem, I will first show that Suchman, unwittingly and tacitly, accepted basic premises of the cognitivist position she was trying to dismantle. I will argue that she, because of this, was unable to dispose of with cognitivism's confusion of normative and causal propositions. At the end she therefore wound up with an account that effectively reproduced cognitivism's view on the nature of computational artifacts.

1.1 Suchman's Strategy

First of all, what kind of argument is presented in *Plans and Situated Actions*?

It has been argued that the focus of Suchman's argument 'is on the notion of plan *as deployed in cognitive science* (plans-according-to-cognitive-science)' and that, therefore, much of her 'argumentation does not concern "plans" as we might use them in ordinary affairs' (Sharrock and Button, 2003, p. 259). It is certainly true that it is the cognitive science notion of 'plans' Suchman is critiquing in her book and that her propositions should be read in that context. However, this gallant reading is not supported by the text, and it has in fact been rejected by Suchman: 'My aim was to take on both senses of "plan," [. . .] and to explore the differences between them' (Suchman, 2003, p. 300). Indeed, Suchman's research prior to writing the book had focused on '"plans" as we might use them in ordinary affairs', namely, organizational procedures, and in that work she did talk about ordinary 'procedures' in *exactly the same terms* as she talked about 'plans' in the book (cf. Suchman, 1982, 1983; Suchman and Wynn, 1984). This concordance justifiably led Agre to read the book as a book about ordinary plans (Agre, 1990). However, for unknown reasons, those earlier and concordant studies seem to have been forgotten by those who have later defended the book against its critics.

Anyway, her exploration of the differences between the cognitivist and the ordinary sense of 'plan' was not the *conceptual analysis* that would have been required to do the job. If it had been *that* kind of argument she would have been trying to expose the deep conceptual confusion underlying cognitivism. But that she did not do.

Cognitivism is a special version of what Ryle calls 'the intellectualist legend'. This legend is characterized by having implicitly taking the assumed pattern of intellectual conduct (e.g., a cycle of theorizing, planning, acting, evaluating) as the paradigm of all intelligent conduct. The theorist thus explains intelligent action by ascribing an anterior 'plan' to the action, the latter being the execution of this occult 'plan'. But as pointed out by Ryle, 'Intelligent practice is not a step-child of theory. On the contrary theorising is one practice amongst others and is itself intelligently or stupidly conducted' (Ryle, 1949, p. 26). Preconceived plans, whether ascribed or avowed, can be as smart or as stupid as any action, planned or not. A tennis player who is playing without a preconceived plan, or who does not stop and contemplate her next move, is not necessarily playing mindlessly. We sometimes make the effort of developing plans for our actions and we sometimes postpone action until we have a plan, but we most often simply act and, thankfully, in doing so we normally act intelligently, competently, heedfully, etc. If anterior plans were a requisite for intelligent conduct, then the development of plans would in turn require anterior plans to be intelligent, and so on. 'To put it quite generally, the absurd assumption made by the intellectualist legend is this, that a performance of any sort inherits all its title to intelligence from some anterior internal operation of planning what to do' (Ryle, 1949, p. 31).

The cognitivist version of 'the intellectualist legend' is one that accounts for intelligent practice in terms of (postulated, occult) *causal* processes. As already shown, Miller et al. (1960) jumps from 'description of behavior' to 'a set of instructions' to 'plan' to organic 'process' to sequential 'control' to 'computer program'. In the words of Stuart Shanker in his incisive critique of cognitivism, this 'muddle' is the 'result of trying to transgress logical boundaries governing the employment of concepts lying in disparate grammatical systems' (Shanker, 1987c, p. 73). In other words, the muddle is the result of an entire series of category mistakes in close order.

I will develop this line of argument below. The point here is that Suchman does not deploy this kind of argument. Instead she counters the conceptual confusion of cognitivism by propounding what is basically an empirical argument, an 'alternative account of human action' (Suchman, 1987, p. x), drawing on observational studies and conceptualizations from social science. This mismatch, an empirical argument against conceptual confusion, is a significant source of ambiguity.

One more observation on the kind of argument that is presented in *Plans and Situated Actions* is required. Instead of a conceptual critique of cognitivism, Suchman mobilized an array of social science accounts of human action and interaction: ethnomethodology, conversation analysis, studies of instructions, etc.—as if to say, 'Look, the cognitivist account is not realistic. The sociological account offers a richer picture'. Paradoxically, however, if this was indeed her aim, the examples of 'plans and situated actions' she offered and discussed are far from representative of ordinary plans in ordinary affairs. Here, in a critique of cognitivism premised on presenting an alternative account, one would have expected that the rich multiplicity would have been demonstrated. For does it make any sense at all to talk about 'plans' in abstract generality, as if it is a genuine concept? Should we not say of the word 'plan' what Wittgenstein says of the word 'to think': 'It is not to be expected of this word that it should have a unified employment; we should rather expect the opposite' (Wittgenstein, 1945–1948, § 112).

The concept of plan is certainly multifarious in its uses. We talk about the floor plan of a building, CAD plans, production plans or schedules, maintenance plans, project plans, cancer treatment plans, and so on. What these uses have in common is the central role of some artifact. In fact, the English word 'plan' is derived from French 'plan' (a ground plan or map, as in 'plan de ville', from the Latin 'planum', a flat surface). The notion is of a drawing on a flat surface, a standard of correctness that can be used as instruction for action and guide in action and that can be inspected and consulted in case of doubt, used as proof in case of dispute, and so on.

We certainly also talk about plans in a derived sense in other contexts of ordinary discourse. We use the term, for instance, to claim not only intent but *considered* intent: 'I plan to leave early so that I can meet you by noon', meaning something like 'I have indeed been considering when to leave and decided to depart early so that I'm certain to arrive at our rendezvous by noon.' By using the term 'plan' in that way in such a context one is declaring not only commitment but also that one has given one's promise some serious thought. One might elaborate by saying: 'I plan to depart from point A at 9:00, which means that I'll be at point B at noon: As promised.' Such avowals of considered commitment derive their force from using the term 'plan', with its received connotations of publicly visible inscription, metaphorically: my commitment is as firm as if it was on public record.

Now, Suchman also ascribed 'plans' to situations where such avowals may not or need not to have been uttered, for example to what goes on prior to white water canoeing:

> in planning to run a series of rapids in a canoe, one is very likely to sit for a while above the falls and plan one's descent. The plan might go something like "I'll get as far over to the left as possible, try to make it between those two large rocks, then backferry hard to the right to make it around that next bunch." (p. 52)

It should be noticed, in passing, that this example, possibly due to its brevity, is ambiguous. Somebody is contemplating how to approach a line of action and we are told that the plan may require a 'great deal of deliberation, discussion, simulation, and reconstruction'. We are not told if the plan has any public status, whether the articulation of the plan amounts to an avowal of commitment. That is, what would happen if the planner or her co-canoeist disregards the plan they have contemplated? If one of them breaks the plan and they end up wet, cold, and bruised, could the other not then object, 'But we agreed to backferry hard *to the right*, not to the left'. As it stands, the example reads as if the 'plan' and its possible 'abandonment' is somehow of no consequence to either of them or to others. In what sense is it a *plan* then?

Suchman also referred to 'plans' belonging to practices that, by contrast, involve the use of inscribed artifacts, such as a traveller (a lone traveller!) using a map to find his way (p. 189). But a map in the hands of a traveller belongs to an entirely different kind of practice than someone's contemplating the course of a canoe trip. And it can, in turn, be used in quite different ways, as a representation of the geography and what it may offer to a traveller with time to spare, or as a representation of the trajectory one wants to or, indeed, *is obliged to* follow.

And, again, 'plans' incorporated in the help system of a photocopier, the use of which Suchman analyzed in the book, belong to practices of yet another kind. In this case the putative 'plans' are computational artifacts that are supposed to regulate peoples' use of the photocopier. That is, as opposed to the map in the hands of the bored traveller, this artifact has the capacity to execute controlled state changes which may physically prevent a user from (or enable him in) doing this or that in certain circumstances. Whatever merit the particular design may have or lack, a practice in which a causal mechanism (a computational artifact) is used as part of a technique of planning is certainly quite different from a practice in which the artifact of planning is static, which again is quite different from a practice in which one has promised, on one's honor, to perform a task in a certain way, which, finally, surely is different from a practice in which a person is pondering the best course of canoeing. These various practices may have something in common (beyond the noun 'plan' we tend to use when referring to these practices) but nothing that would make 'plans' in all fuzzy generality a researchable phenomenon.

What is most remarkable, however, is that any discussion of *ordinary plans*—time tables, production schedules, project plans, clinical protocols—is absent from the analysis. It would not be surprising were such real-world plans systematically omitted from cognitivist discourse,[1] but their absence in the context of a call for ethnographic studies of actual socially situated action is very remarkable indeed. Their absence is all the more remarkable in light of the fact that Suchman's early work exactly focused on practices in which such ordinary plans loom large (e.g., Suchman, 1983).

1.2 Counter-Cognitivism

On a decisive issue Suchman's critique of cognitivism is very clear and firm. She stated—emphatically and repeatedly—that 'our actions, while systematic, are never planned in the strong sense that cognitive science would have it' (p. ix); that 'plans are resources for situated action, but do not in any strong sense determine its course' (p. 52), and so on. By the expression 'strong sense' she obviously meant 'causal sense'. That is, she took issue with the basic cognitivist proposition that rational action is to be explained by reference to some (obscure) causal 'process'.

However, she did not realize that in ordinary language the concept 'plan' ('following a plan', 'agreeing to a plan', 'violating a plan') is a concept of normative behavior. Hence an expression such as 'planning in the strong [causal] sense' is unintelligible, as it suggests *the very possibility* of a causal determination of action by a plan. Plans are normative constructs; that is, they provide criteria for whether or not a particular action is correctly executed.

On the other hand, if Suchman was using the word 'plan' in the sense underlying cognitivism's metaphysical language (material processes that somehow maintain other material processes in a certain state) then her key propositions in turn loose their sense. What might it, for instance, *then* mean to say that, 'as essentially linguistic representations, plans are efficient formulations of situated actions' (p. 186)? The problem is, of course, that if one uses the word 'plan' in a contrived sense, transposed from a quite different domain of discourse, then one has lost the ability to account for ordinary plans in our everyday life. (At the same time one has introduced a source of confusion in computer science as well).

Thus, when Suchman talked about 'plans', it is generally not clear if the word is to be read as 'plans-according-to-cognitive-science' or as '"plans" as we might use them in ordinary affairs' (Sharrock and Button, 2003, p. 259). As noted above, this was deliberate on Suchman's part. This is more than a source of ambiguity, however. Suchman was herself obviously not aware of this rather important distinction. The normative status of plans is completely absent from her account. Cognitivism's foundations were thus left effectively untouched and intact.

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[1] Miller et al. actually do refer to ordinary plans: 'A public plan exists whenever a group of people try to cooperate to attain a result that they would not be willing or able to achieve alone. Each member takes upon himself the performance of some fragment of the public plan and incorporates that fragment into his individual, personal Plans' (Miller et al., 1960, p. 98). However, they obviously do not notice that they are here implying 'plans' in a normative sense.

Having from the outset accepted the cognitivist concept of ‘plan’ as meaningful, albeit empirically objectionable, and having thus conceded the high ground, how could Suchman then possibly mount anything like an attack on cognitivism? She did so by deploying what could be dubbed a strategy of containment. That is, she tried to keep the cognitivist peril at bay by neutralizing its account of rational action and the pernicious implications that flow from it, by insisting that ‘plans are best viewed as a weak resource for what is primarily *ad hoc* activity’, etc. The intellectual costs of this strategy are very high indeed, however. To protect the phenomenon of real-world human action—socially organized, materially situated, embodied, intentional action—from being reduced to a mere epiphenomenon of some hidden causal process, Suchman introduced a categorical gap between plan and situated action. She did that, for instance, when she stated that ‘situated action turns on local interactions between the actor and contingencies that, while they are made accountable to a plan, remain essentially outside of the plan’s scope’ (pp. 188 f.). The proposition is somewhat ambiguous in as much as it is not entirely clear what is meant by the term ‘scope’, but the expression ‘essentially outside’ would normally serve to indicate that the concepts of ‘situated action’ and ‘plan’ are conceptually unrelated, i.e., that they are unrelated by definition, and that any relation therefore would be contingent. This reading is confirmed by statements throughout the book in which ‘plans’ are granted a role prior to and subsequent to action, but not *in* action. For example, and as shown above, Suchman asserted that ‘It is only when we are pressed to account for the rationality of our actions, given the biases of European culture, that we invoke the guidance of a plan’ (p. ix). In a similar vein she posited that ‘plans are a constituent of practical action, but they are constituent as an artifact of our *reasoning about* action, not as the generative *mechanism of* action’ (p. 39). The reading I am suggesting, that Suchman construed a categorical separation of ‘plans’ and ‘situated actions’, is further confirmed by her discussion of the canoe example mentioned in passing above:

> A great deal of deliberation, discussion, simulation, and reconstruction may go into such a plan. But, however detailed, the plan stops short of the actual business of getting your canoe through the falls. When it really comes down to the details of responding to currents and handling a canoe, you effectively abandon the plan and fall back on whatever embodied skills are available to you. The purpose of the plan in this case is not to get your canoe through the rapids, but rather to orient you in such a way that you can obtain the best possible position from which to use those embodied skills on which, in the final analysis, your success depends. (p. 52)

That is, on Suchman’s view, when one starts acting, one ‘effectively abandons’ the plan; its only purpose now is to ‘orient’ one so as to be able to improvise smartly. The same picture of plans or ‘abstract representations of situations and of actions’ was painted in the conclusions of the book:

> The foundation of actions [...] is not plans, but local interactions with our environment, more and less informed by reference to abstract representations of situations and of actions, and more and less available to representation themselves. The function of abstract representations is not to serve as specifications for the local interactions, but rather to orient

> or position us in a way that will allow us, through local interactions, to exploit some contingencies of our environment, and to avoid others. (p. 188)

Again action was portrayed as essentially *ad hoc* action, 'local interactions', and again the role of 'plans' was seen as merely that of affording improvisation: 'orient or position us in a way that will allow us, through local interactions, to exploit some contingencies of our environment, and to avoid others'.

Now, Suchman did state, quite clearly, that 'the essential nature of action, however planned or unplanned, is situated' (p. x). I can of course have no problem with that statement. All action, planned or improvised, is situated, i.e., contingent, embodied, materially contextualized, etc. But it is important to realize that this is not a substantive or empirical proposition but a logico-grammatical one. It simply states that action and context are internally related concepts. But Suchman tended to forget this. As already shown, 'situated actions' were often characterized as 'essentially *ad hoc*' (e.g., pp. ix, 48, 61, 78), which is plain nonsense. Action is *essentially* (!) situated, and *some* actions are *ad hoc*, while *other* actions certainly are not *ad hoc*. The action of executing a plan is, being an action, situated; but it is not, again by definition, *ad hoc*.

In sum, it is internal to the concept of action that action is situated, contingent. To say that action is essentially *ad hoc* or that plans flounder on the contingent nature of action is deeply confused. Some action is characterized by being overwhelmingly spontaneous, unpremeditated, *ad hoc*, improvised, etc. Other action is planned in the sense that there is an obligation to execute the action in certain ways: steps to be taken in a certain sequence, by certain actors, at certain times, by using certain resources, etc. *Plans do not cause action* to take a particular course, for they cannot *cause* anything. Just like rules, conventions, notations, etc., plans are normative constructs of our common practices.

1.3 Transcendental Judgments

> We understand what it means to set a pocket-watch to the exact time, or to regulate it to be exact. But what if it were asked: Is this exactness ideal exactness? [. . .] No single ideal of exactness has been envisaged; we do not know what we are to make of this idea
> (Wittgenstein, 1945–46, § 88)

The costs of Suchman's strategy of containing cognitivism are not limited to the categorial separation of 'plans' and 'actions' and to the 'abandonment' of plans at a desolate place outside of situated action.

As we have seen, the cognitivist notion of 'plan' is predicated on a notion of 'a complete description of behavior' (Miller et al., 1960). Within a specific practice the notion of a complete description or a completely specified plan, instruction, recipe, etc. of course make sense, in as much as there are criteria for what can be taken to be a completely specified plan, etc. That is, the plan, the instruction, etc. is complete when it for an ordinary practitioner is *unproblematic* to follow it, apply it, use it, etc.

However, the cognitivist notion of a 'a complete description of behavior' presumes completeness in the abstract, irrespective of any specific practice.

The cognitivist notion of a 'complete' plan is therefore as absurd as the fantastic story of the 'perfect map' that Borges has given us. The story is presented as a text fragment entitled 'Of Exactitude in Science' that Borges, playfully, pretends to have found in an old book, '*Travels of Praiseworthy Men* (1658) by J. A. Suarez Miranda':

> ... In that Empire, the craft of Cartography attained such Perfection that the Map of a Single province covered the space of an entire City, and the Map of the Empire itself an entire Province. In the course of Time, these Extensive maps were found somehow wanting, and so the College of Cartographers evolved a Map of the Empire that was of the same Scale as the Empire and that coincided with it point for point. Less attentive to the Study of Cartography, succeeding Generations came to judge a map of such Magnitude cumbersome, and, not without Irreverence, they abandoned it to the Rigours of sun and Rain. In the western Deserts, tattered Fragments of the Map are still to be found, Sheltering an occasional Beast or beggar; in the whole Nation, no other relic is left of the Discipline of Geography. (Borges, 1946, p. 141)

Here the absurdity is obvious enough. But the very idea that a 'complete description' could be given *outside of a practice* in which the criterion for completeness is given is equally absurd.

Suchman was at pains to demonstrate that the cognitivist completeness notion is untenable, which it certainly is. The problem with Suchman's strategy is that she engaged in an overwhelmingly empirical argument to demonstrate that the metaphysical assumptions of cognitivism are factually groundless. But they are not groundless; they are meaningless. Her strategy therefore, unwittingly, reproduced the metaphysics of cognitive science, albeit in the inverse. This shows in several ways.

Suchman repeatedly stated that plans are inherently 'vague' compared with action: 'Stated in advance, plans are necessarily vague, insofar as they must accommodate the unforeseeable contingencies of particular situations' (p. ix). Similarly, she stated that 'plans are inherently vague' (pp. 38, 185) or refers to the 'representational vagueness' of plans (p. 185). But could action be *anything but* 'situated' in the sense used by Suchman: contextual, embodied, etc.? Would '*doing x*' not be categorically different from '*thinking* about doing *x*' (or 'describing doing *x*' or 'planning doing *x*')? But of course it would! This is as trivial as saying that there is a difference between the landscape and the map of the landscape.

How does one compare the 'vagueness' or 'completeness' of 'plans' *vis-à-vis* 'actions'? Is there a metric out there one can employ? Surely not. But what does it mean, then, that 'plans are inherently vague'? When Suchman was making these statements she was falling back into the metaphysical framework of the cognitivist tradition she was otherwise trying to demolish. The cognitivists' transcendental use of the term 'completeness' and Suchman's equally transcendental use of the opposite term 'vagueness' are both examples of the kind of metaphysical smoke that is produced when language is idling. The notion of 'vague' plans, 'incomplete' plans, etc. presupposes *the logical possibility* of distinct plans, complete plans, etc. The terms ‘vague’ and ‘complete’ are used as characterizations of *specific* plans with respect to *specific* practices.

According to which criteria can a plan be said to be ‘vague’? Are there criteria of ‘vagueness’ or specificity or adequacy outside of the particular practice? Suchman pointed out herself that ‘While plans *can be* elaborated indefinitely, they elaborate actions just to the level that elaboration is useful; they are vague with respect to the details of action precisely at the level at which makes sense to forego abstract representation, and rely on the availability of a particular, embodied response’ (p. 188). True: provided one has indefinite time and resources, then plans can be elaborated indefinitely. But if the criterion of a plan’s appropriate level of elaboration is a practical one, then surely *some* plans are vague (with respect to the criteria internal to the practice in question) and *others not*. In fact, we would hardly make plans if they were not, generally, suitably specific and complete for our practical purposes.

The same metaphysical form of discourse is in evidence when Suchman stated that the ‘circumstances of our actions [...] are continuously changing around us’ (p. viii). Sure, there is a sense in which we can say that the world changes continually: electrons jump about, molecules form and dissolve, cells reproduce and decay, etc. But the notion of the ‘circumstances of our actions’ refers to the circumstances that are of practical significance, those that to a practitioner make a difference. And in that sense the ‘circumstances of our actions’ may or may not change. Hence, we can unproblematically talk about *doing the same* thing under the *same* circumstances. That is, in her honorable effort to turn the table on the cognitivism’s realist notion of ‘complete description’ in the abstract, Suchman fell back into the trite nominalist notion that ‘everything is unique’: one cannot jump into the same river twice, nothing stays the same, all action is therefore *ad hoc*.[2]

[2] Suchman has recently, in response to criticisms of propositions such as ‘situated actions are essentially ad hoc’, made some rather guarded comments: ‘I see my choice of the term ad hoc here as an unfortunate one, particularly in light of subsequent readings of the text. The problem lies in the term’s common connotations of things done anew, or narrowly, without reference to historically constituted or broader concerns. Perhaps a better way of phrasing this would be to say that situated actions are always, and irremediably, contingent on specific, unfolding circumstances that are themselves substantially constituted by those same actions. This is the case however much actions may also be informed by prescriptive representations, past experience, future considerations, received identities, entrenched social relations, established procedures, built environments, material constraints and the like. To be rendered effective the significance and relevance of any of those must be reiterated, or transformed, in relation to what is happening just here and just now.’ (Suchman, 2007, p. 27, n. 4).—It first of all needs to be said that the ‘choice of the term ad hoc’ is not simply ambiguous but confusing in as much as ‘ad hoc’, as any dictionary will confirm, has a stable meaning, namely, something done ‘for the particular end or purpose at hand and without reference to wider application or employment’, *in contrast to* ‘planned’ or ‘coordinated’. Anyway, removing the term from the text does not alter anything. Should the phrase ‘contingent on specific, unfolding circumstances’ be read as a logico-grammatical one or an empirical one? In case the former reading is intended, the clause simply states that action is irremediably situated or that planning is not acting. In the latter reading, the question remains: ‘irremediably contingent’ according to which criteria? Does it, on this account, even make sense to talk of *an action* as an identifiable course of conduct?

The source of these confused ideas is, I suggest, the cognitivist notion that plans are 'descriptions', 'representations', etc. On the cognitivist account, 'plans' are hypotheses derived inductively that (in some unintelligible way) are operating causally. To this Suchman countered with the classical objection that inductively derived hypotheses (and theories) are underdetermined with respect to manifold reality and that a given hypothesis therefore is only one possible interpretation out of many. Hence, I presume, the confusing talk about essential vagueness of 'plans'.

In sum, Suchman was trying to demolish the cognitivist program but she did not, from the very outset, dissolve cognitivism's obscure notion of 'plans'. That is, she accepted to oppose the cognitivist notion of 'plans' on the very battle field chosen and defined by cognitivism. The cognitivist notion of 'plans', phantasmal objects that supposedly determine rational action in a causal sense, is a meaningless construct and would have to be demolished as such. By accepting this construct as having sense, Suchman also, unwittingly and against everything she otherwise stood for, engaged in a metaphysical discourse and got trapped as a fly in a bottle.

For CSCW the immediate problem with the metaphysics underlying cognitivism as well as that retained in Suchman's reversal of cognitivism is that this form of theorizing tends to make us blind to the multiplicity of practices and hence to phenomena that are crucially important to us. Consequently, although Suchman's book was justly received enthusiastically as portending a liberation from the cognitivist dogma and from its stifling effects on socio-technical research, and while it offered vital initial formulations of the practice-oriented program of CSCW, it also, at the same time, contributed to stifling that program by reproducing the metaphysical form of reasoning characteristic of cognitivism.

1.4 Regularity and Normativity

> A rule stands there like a signpost.—Does the signpost leave no doubt about the way I have to go? Does it show which direction I am to take when I have passed it, whether along the road or the footpath or cross-country? But where does it say which way I am to follow it; whether in the direction of its finger or (for example) in the opposite one? [. . .]
> The signpost is in order—if, under normal circumstances, it fulfils its purpose.
> (Wittgenstein, 1945–46, §§ 85, 87)

The extent to which Suchman in her book stayed within the cognitivist framework is particularly clear in her understanding of the term 'plan'. For cognitivism, for which persons are mere carriers of 'plans', a 'plan' is a description of action. On this view, the *normative* sense of plans, as *prescribed* or *agreed–to* courses of action, is not considered at all. Suchman conceived of plans in exactly the same way:

> in our everyday action descriptions we do not normally distinguish between accounts of action provided before and after the fact, and action's actual course' (p. 38 f.).

> Like all action descriptions, instructions necessarily rely upon an implicit et cetera clause in order to be called complete (p. 62).
> The general task in following instructions is to bring canonical descriptions of objects and actions to bear on the actual objects and embodied actions that the instructions describe (p. 101).

This is again remarkable. As already noted, the term 'plan' is generally used in ways comparable to those of the term 'rule'. We use the term 'rule' both descriptively, to indicate *regularity* ('As a rule, I clock out when the bars open'), or as a criterion of *correct* conduct ('The house rule says no fighting and no spitting on the floor!'). Similarly, we certainly sometimes use the term 'plan' as an 'action description': 'He seemed to be working according to a plan', or 'He acted in conformance with Plan B'; but we also use the term to refer to normative constructs: 'This is our plan...' or 'According to the production plan, we have to be finished today.'

How can we talk and reason sensibly about rules and plans and *ad hoc* activities and about the role of 'formalisms' in work practices?

To extract ourselves from the quagmire of cognitivism, which Suchman set out to do but did not succeed in doing, we should first of all heed some cardinal distinctions of which Wittgenstein has reminded us.[3] A major source of confusion is many social scientists' apparently stubborn refusal to distinguish between, on one hand, *mere regularity* of behavior, and on the other hand, *following a rule*.

Mere *regularity*: that is, people's exhibiting observable and reasonably predictable patterns of behavior, or their acting in observable conformity with a rule. What many sociologists and psychologists try to tease out by trying to detect correlations in behavior (e.g., differing suicide rates) is this: mere regularity. Such patterns may be important 'sociological facts', but they may also be of the same stuff as the notorious channels on Mars.

When we talk about *rule following*, by contrast, we talk about something entirely different: practices that not only involve observable regularity of conduct but also the ability of actors to explain, justify, sanction, reprimand, etc. actions with reference to rules, and often also the ability to teach rules, formulate rules, debate rules, etc. In his *Remarks on the Foundations of Mathematics*, Wittgenstein gives an wonderfully straightforward illustration of this point:

> Let us consider very simple rules. Let the expression be a figure, say this one:
>
> |——|
>
> and one follows the rule by drawing a straight sequence of such figures (perhaps as an ornament).
>
> |— —||— —||— —||— —||— —|

[3]The following discussion is, of course, primarily based on Wittgenstein's famous analysis in *Philosophical Investigations* (1945–1946, esp. §§ 82–87 and 185–242) but also his *Remarks on the Foundations of Mathematics* (1937–1944). For excellent but not entirely mutually congruent commentaries on this subtle analysis, cf. the works of Winch (1958), Pitkin (1972), von Savigny (1974, 1994), Baker and Hacker (1984, 2009), Malcolm (1986, 1995), Hacker (1989, 1990), and Williams (1999).

> Under what circumstances should we say: some gives a rule by writing down such a figure? Under what circumstances: someone is following this rule when he draws this sequence? It is difficult to describe this.
>
> If one of a pair of chimpanzees once scratched the figure |— —| in the earth and thereupon the other the series |— —||— —| etc., the first would not have given a rule nor would the other be following it, whatever else went on at the same time in the mind of the two of them.
>
> If however there were observed, e.g., the phenomenon of a kind of instruction, of shewing how and of imitation, of lucky and misfiring attempts, of reward and punishment and the like; if at length the one who had been so trained put figures which he had never seen before one after another in sequence as in the first example, then we should probably say that the one chimpanzee was writing rules down, and the other was following them. (Wittgenstein, 1937–1944, VI §42)

The difference between 'regularity' and 'rule following' is a categorial one. The formulation of a rule is not an empirical proposition, whereas the formulation of a regularity is; the formulation of a rule is a normative one, it provides criteria for what is correct and what is not, what is right and what is wrong. One may, for example, observe that people of a certain age are disproportionately represented among those who commit suicide; this would be an observation of a regularity. But making this observation is obviously *not* the expression of a rule. If I said to a particular person that he had tried to commit suicide *at the wrong age*, I would be most certainly be regarded as demented, and rightly so. This should be clear enough, or so I should like to think. Let us therefore move on to the more tricky issue of the relationship between rule and action.

What may make Wittgenstein's observation on our concept of 'following a rule' particularly hard to accept by sociologists and other social scientists, is the strong inclination in social theory to conceive of rational action as necessarily involving some kind of 'interpretation' of rules, instructions, precedents, etc. on the one hand and 'interpretation' of the situation on the other. This is the intellectualist legend at work: the actor is portrayed in the image of the scholar bent over an ancient text fragment trying to develop a version that is both loyal to the original and at the same time understandable to the modern reader.

Wittgenstein reminds us, however, not to confound *following a rule* and *interpreting a rule*. To do so he takes the reader of his *Philosophical Investigations* through a lengthy *reductio ad absurdum*, demonstrating that a course of action that exhibits regularity can be made out to conform with multiple and mutually contradictory rule formulations. After having done that, Wittgenstein stops the *reductio* and lets his fictional interlocutor ask: 'But how can a rule teach me what I have to do at *this* point? After all, whatever I do can, on some interpretation, be made comparable with the rule'. Wittgenstein replies:

> No, that's not what we should say. Rather: every interpretation hangs in the air together with what it interprets, and cannot give it any support. Interpretations by themselves do not determine meaning. (Wittgenstein, 1945–1946, § 198)

The operative word here is *interpretation*. A few sections later, Wittgenstein emphasizes this point:

> This was our paradox: no course of action could be determined by a rule, because every course of action can be brought into accord with the rule. The answer was: if every course of action can be brought into accord with the rule, then it can also be brought into conflict with it. And so there would be neither accord nor conflict here.
>
> That there is a misunderstanding here is shown by the mere fact that in this chain of reasoning we place one interpretation behind another, as if each one contend us at least for a moment, until we thought of yet another lying behind it. For what we thereby show is that there is a way of grasping a rule which is *not* an *interpretation,* but which, from case to case of application, is exhibited in what we call "following the rule" and "going against it".
>
> That's why there is an inclination to say: every action according to a rule is an interpretation. But one should to speak of *interpretation* only when one expression of a rule is substituted for another. (Wittgenstein, 1945–1946, § 201)

In following a rule there is no space for interpretation: 'an interpretation gets us no closer to an application than we were before. It is merely an alternative formulation of the rule, another expression in the symbolism which paraphrases the initial one' (Baker and Hacker, 2009, p. 92).

To be sure, a course of action *sometimes* involves interpretation, but it does not necessarily do so. As Wittgenstein puts it in *Zettel*, 'an *interpretation* is something that is given in signs. It is this interpretation as opposed to a different one (running differently)' (Wittgenstein, 1945–1948, § 229). That is, we talk of 'interpretation' when referring to the substitution of one linguistic construct (rule formulation, instruction, command, statement, representation, etc.) by another and, supposedly, more useful construct. In following a rule, the rule is not 'interpreted' or the like; it is simply applied or enacted, because that is what following a rule means. Understanding a rule means that I can apply it without engaging in interpreting the rule. As succinctly summarized by Baker and Hacker, 'to grasp a rule is to understand it, and understanding a rule is not an act but an ability manifested in following the rule' (Baker and Hacker, 2009, p. 96). And Wittgenstein again: 'What happens is not that this symbol cannot be further interpreted, but: I do no interpreting. I do not interpret because I feel at home in the present picture. When I interpret, I step from one level of thought to another' (Wittgenstein, 1945–1948, § 234).

In fact, action normally does not involve interpretation. In our ordinary work practices we do not normally engage in interpretation work whenever we follow instructions or execute plans. We sometimes have to, of course, but generally we do not. That is the whole point of the concept of 'the natural attitude'. Interpretation is required when doubt is a *practical* issue, and doubt needs grounds too (Wittgenstein, 1949–1951, § 124). Endless doubt is impossible: 'If you tried to doubt everything you would not get as far as doubting anything. The game of doubting itself presupposes certainty' (§ 115). We interpret when it is *conceivable to us* that we could be wrong, e.g., when we are uncertain about the meaning of a rule formulation or do not yet fully understand it.

At the end of his discussion of the notion of following a rule in the *Philosophical Investigations*, Wittgenstein lets his interlocutor ask: 'How am I able to obey a rule?' To which Wittgenstein replies:

> If this is not a question about causes, then it is about the justification for my acting in *this* way in complying with the rule.
>
> Once I have exhausted the justifications, I have reached bedrock, and my spade is turned. Then I am inclined to say: "This is simply what I do." (Wittgenstein, 1945–1946, § 217)

Two paragraphs later, Wittgenstein wraps up this line of argument by saying:

> When I follow the rule, I do not choose.
> I follow the rule *blindly.* (§ 219)

If read out of context, the phrase 'I follow the rule *blindly*' can be misunderstood as suggesting that normative behavior is irrational, or non-rational: 'But in context it signifies not the blindness of ignorance, but the blindness of certitude. I know *exactly* what to do. I do not *chose*, after reflection and deliberation, I just ACT—in accord with the rule' (Baker and Hacker, 1984, p. 84). In other words, what is meant is not that the actor in following the rule proceeds mindlessly but that he or she goes on *as a matter of course.* What is meant by saying 'I follow the rule *blindly*' is made perfectly clear in the *Remarks on the Foundations of Mathematics*:

> One follows the rule mechanically. Hence one compares it with a mechanism.
> "Mechanical"—that means: without thinking. But *entirely* without thinking? Without *reflecting*. (Wittgenstein, 1937–1944, VII §61).

When following the rule, that is, the actor proceeds as 'a matter of course' (Wittgenstein, 1945–1946, § 238), for doubt is not an option. The rule 'always tells us the same, and we do what it tells us': 'One does not feel that one has always got to wait upon the nod (the prompt) of the rule. On the contrary, we are not on tenterhooks about what it will tell us next, but it always tells us the same, and we do what it tells us.' (Wittgenstein, 1945–1946, § 223). The rule 'is my *final* court of appeal for the way I'm to go'. On Wittgenstein's account, then, the concept of 'rule' should be understood in its 'internal' or 'logico-grammatical' relations to the concept of 'practice' and thereby to the concept of 'techniques' (Baker and Hacker, 2009, pp. 140–145). Understanding a rule means possessing the ability to do certain things *correctly* and is manifested in following a rule and mastering the appropriate techniques.

If the question 'How am I able to obey a rule?' is about *causes*, however, then the answer is simply, in Wittgenstein's words, that 'We are trained to do so; we react to an order in a particular way' (Wittgenstein, 1945–1946, § 206). That is, 'there is no explanation of our ability to follow rules—other than the pedestrian but true explanation that we received a certain training.' (Malcolm, 1986, p. 180).[4]

For Wittgenstein, then, to follow a rule *is* a practice. A 'person goes by a signpost only in so far as there is an established [ständigen] usage, a custom' (§ 198). The rule, in contrast to the various spoken or written expressions and representations of the rule, does not exist independently of the action, as some mysterious mental

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[4]Meredith Williams has an interesting discussion of the key issue of learning in normative behavior (Williams, 1999, Chapter 7).

entity. But nor does it make sense to think of rule following as something only one person could do only once in his or her life.

> It is not possible that there should have been only one occasion on which only one person followed a rule. It is not possible that there should have been only one occasion on which a report was made, an order given or understood, and so on.—To follow a rule, to make a report, to give an order, to play a game of chess, are *customs* (usages, institutions) (Wittgenstein, 1945–1946, § 199)

To follow a rule means mastering a technique (§ 199) and is thus 'a practice' (§ 202).

The concept of 'practice', then, should not be conceived of as mere conduct or behavior, nor as incessant improvisation or 'irremediably contingent' action. A practice is constituted by a rule (or an array of rules) that provides the standard of correct or incorrect conduct. It is the rule (or array of rules) that identifies a course of action as an instance of *this* practice, as opposed to an instance of *another* practice. In the words of Peter Winch, 'what the sociologist is studying, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as doing "the same kind of thing" in relation to that kind of activity' (Winch, 1958, p. 87, emphasis deleted). The identity and integrity of rules and practices over time are themselves the result of practitioners' 'reflective' efforts of instructing and teaching, of commanding and correcting, of emulating and practicing, of correcting own transgressions and asking for guidance, and of contemplating and negotiating new ways of doing things. Practices are *upheld*.

2 Work and Interpretation Work

Why would Suchman be conceptually blind for the role of plans *in* action, that is, of the normative character of plans?

The reason seems to be that she interposed *interpretation* between the plan and the action. Thus, to act the actor must first *interpret* the situation and the plan with respect to the situation and only then act. She seemed to believe that she was following Garfinkel in this, but that would be a misrepresentation of Garfinkel. Garfinkel is quoted (ibid., p. 62) for this observation:

> To treat instructions as though *ad hoc* features in their use was a nuisance, or to treat their presence as grounds for complaining about the incompleteness of instructions, is very much like complaining that if the walls of a building were gotten out of the way, one could see better what was keeping the roof up. (Garfinkel, 1967, p. 22)

It is a wittily put but carelessly general observation, in that the term '*ad hoc*' of course presumes the logical possibility that instruction *can* be complete. The criterion of the completeness and incompleteness of instructions is internal to the particular practice. However, Suchman then elaborated:

> Like all action descriptions, instructions necessarily rely upon an implicit et cetera clause in order to be called complete. The project of instruction-writing is ill conceived, therefore,

> if its goal is the production of exhaustive action descriptions that can guarantee a particular interpretation. What "keeps the roof up" in the case of instructions for action is not only the instructions as such, but their interpretation in use. And the latter has all of the *ad hoc* and uncertain properties that characterize every occasion of the situated use of language. (Suchman, 1987, p. 62)

Suchman was here, again, making the argument that the map is not complete compared to the terrain and that its use therefore requires 'interpretation' and, accordingly, have 'all of the *ad hoc* and uncertain properties that characterize every occasion of the situated use of language'. There is no reason to reiterate why this was confused. My point here is that she, explicitly, interposed interpretation as a necessary intermediary between instruction and action. (For a parallel critique, cf. Sharrock and Button, 2003).

2.1 Garfinkel (Mis)interpreted

It is highly relevant and instructive to note the nature of the case Garfinkel is referring to in the above quote. The study, conducted in the early 1960s by Garfinkel in collaboration with Egon Bittner was concerned with selection activities at an outpatient psychiatric clinic: 'By what criteria were applicants selected for treatment?'. Their sources of information were the clinical records. The most important of these were intake application forms and the various contents of case folders. They take care to point out that clinical folders contain records that are generated by the activities of clinical personnel and that 'almost all folder contents, as sources of data for our study, were the results of self-reporting procedures' (Garfinkel, 1967, pp. 186 f.).

Altogether 1,582 clinic folders were examined by two graduate students of sociology who were tasked with extracting information and fill in a 'coding sheet'. In doing this, the coders were permitted to make inferences and encouraged to undertake 'diligent search'. Nonetheless, they were unable to obtain answers to quite many of the items in the coding sheet. For about half of the items dealing with clinical issues, the coders only got information from between 0 and 30% of the cases. Garfinkel and Bittner's thorough account of the reasons for this rather dismal performance is a most informative discussion of the methodological problems that arise in studies that depend on secondary use of clinical records produced for internal use in the clinical setting. The gist of it is this:

> We came to think of the troubles with records as "normal, natural" troubles. [...] "Normal, natural troubles" are troubles that occur because clinic persons, as self-reporters, actively seek to act in compliance with rules of the clinic's operating procedures that for them and from their point of view are more or less taken for granted as right ways of doing things. [...] The troubles we speak of are those that any investigator—outsider or insider—will encounter if he consults the files in order to answer questions that depart in theoretical or practical import from organizationally relevant purposes and routines under the auspices of which the contents of the files are routinely assembled in the first place. Let the investigator attempt a remedy for shortcomings and he will quickly encounter interesting properties of these troubles. They are persistent, they are reproduced from one clinic's files to the next, they are standard and occur with great uniformity as one compares reporting systems of

> different clinics, they are obstinate in resisting change, and above all, they have the flavor of inevitability. This inevitability is revealed by the fact that a serious attempt on the part of the investigator to remedy the state of affairs, convincingly demonstrates how intricately and sensitively reporting procedures are tied to other routinized and valued practices of the clinic. Reporting procedures, their results, and the uses of these results are integral features of the same social orders they describe. Attempts to pluck even single strands can set the whole instrument resonating. (Garfinkel, 1967, pp. 190 f.)

That is, the 'troubles' arise whenever clinical records are used to 'answer questions that *depart* in theoretical or practical import' from the purposes for which they were assembled and external to the practices that for clinicians 'are more or less taken for granted as right ways of doing things'. A clinic, like any enterprise, operates within a fixed budget and must, in its daily operation, consider the comparative costs of recording and obtaining alternative information. Some information items, such as sex and age of patients, are of course cheaply acquired, while other items, such as occupational history, require expensive reporting efforts (p. 192). At the same time, clinical records are assembled for future, variable, and generally unknown purposes. Consequently, such future purposes do not, in and of themselves, carry much weight in the busy daily life of the clinic.

The division of labor in the clinic adds another source of 'normal, natural troubles': 'The division of work that exists in every clinic does not consist only of differentiated technical skills. It consists as well of differential moral value attached to the possession and exercise of technical skills.' For instance, the role records play in the accomplishment of administrative responsibilities is quite different from the role they play in the pursuit of professional medical responsibilities, and Garfinkel and Bittner pointed to 'the wary truce that exists among the several occupational camps as far as mutual demands for proper record-keeping are concerned' (p. 194). Thus, clinicians exhibit 'abiding concerns for the strategic consequences of avoiding specifics in the record, given the unpredictable character of the occasions under which the record may be used as part of the ongoing system of supervision and review' (p. 194).

Now, the specific character of clinical work and the specific role of record keeping in these practices pose another source of trouble, a 'critical source of trouble' (p. 197). Clinical work consists in what Garfinkel and Bittner termed 'remedial activities'. One of the crucial features of these is that 'recipients [of treatment] are socially defined by themselves and the agencies as incompetent to negotiate for themselves the terms of their treatment'. Clinicians undertake to exercise that competence for them; they take responsibility for their patients. Accordingly 'the records *consist of procedures and consequences of clinical activities as a medico-legal enterprise'* (p. 198). This means that records are written and gathered for 'entitled readers'. A 'competent readership' is presumed. The 'contents of clinic folders are assembled with regard for the possibility that the relationship may have to be portrayed as having been in accord with expectations of sanctionable performances by clinicians and patients.' (p. 199). Thus 'terms, designations, and expressions contained in a document' in the records are not 'invoked in any "automatic" way to regulate the relationship' of the terms to therapeutic activities.

'Instead, the ways they relate to performances are matters for competent readership to interpret' (p. 199). That is, 'considerations of medico-legal responsibility exercise an overriding priority of relevance as prevailing structural interests whenever procedures for the maintenance of records and their eligible contents must be decided.' So, although records may be put to uses that are different from those that serve the interests of considerations of medico-legal responsibility, '*all* alternatives are subordinated' to considerations of medico-legal responsibility 'as a matter of enforced structural priority'. 'Because of this priority, alternative uses are consistently producing erratic and unreliable results' (p. 200).

All these conditions have important implications for the relationship between writer and reader of clinical records:

> As expressions, the remarks that make up these documents have overwhelmingly the characteristic that their sense cannot be decided by a reader without his necessarily knowing or assuming something about a typical biography and typical purposes of the user of the expressions, about typical circumstances under which such remarks are written, about a typical previous course of transactions between the writers and the patient, or about a typical relationship of actual or potential interaction between *the writers and the reader.* Thus the *folder contents much less than revealing an order of interaction, presuppose an understanding of that order for a correct reading.* (p. 201)

The records do work, however, because 'there exists an entitled use of records':

> The entitlement is accorded, without question, to the person who reads them from the perspective of active medico-legal involvement in the case at hand and shades off from there. The entitlement refers to the fact that the full relevance of his position and involvement comes into play in justifying the expectancy that he has proper business with these expressions, that he will understand them, and will put them to good use. The specific understanding and use will be occasional to the situation in which he finds himself. [...] The possibility of understanding is based on a shared, practical, and entitled understanding of common tasks between writer and reader. (p. 201)

That is, clinical records, especially records concerning legally touchy 'remedial activities', pose quite specific methodological challenges for secondary analytical use by non-competent readership. To emphasize this, Garfinkel and Bittner made a comparison with actuarial records:

> A prototype of an actuarial record would be a record of installment payments. The record of installment payments describes the present state of the relationship and how it came about. A standardized terminology and a standardized set of grammatical rules govern not only possible contents, but govern as well the way a "record" of past transactions is to be assembled. Something like a standard reading is possible that enjoys considerable reliability among readers of the record. The interested reader does not have an edge over the merely instructed reader. That a reader is entitled to claim to have read the record correctly, *i.e.,* a reader's claim to competent readership, is decidable by him and others while disregarding particular characteristics of the reader, *his* transactions with the record, or *his* interests in reading it. (ibid., p. 202)

Clinical records belong to practices quite different from actuarial records:

> In contrast to actuarial records, folder documents are very little constrained in their present meanings by the procedures whereby they come to be assembled in the folder. Indeed, document meanings are disengaged from the actual procedures whereby documents were

> assembled, and in this respect the ways and results of competent readership of folder documents contrast, once more, with the ways and results of competent actuarial readership. (p. 203)

The actuarial record 'is governed by a principle of relevance with the use of which the reader can assess its completeness and adequacy at a glance.' By contrast, with clinical records the reader, so to speak, reassembles the entries to 'make the case'.

It should be clear from this that no set of coding instructions, however elaborate and however meticulously designed, could have ameliorated the troubles Garfinkel and Bittner experienced. The troubles were found to be an inexorable feature of secondary use of clinical records.

In other words, the trouble with following instructions in this case is, essentially, the kind of trouble one will expect to experience when engaged in reusing records that have produced for specific purposes within one work practice—*outside* of that practice and thus for purposes for which these records were not originally intended. Garfinkel and Bittner were indeed quite adamant to point this out. What they described is the kind of trouble historians engage in when they immerse themselves in the archives, the collections of internal memos, minutes, and private letters, etc. This kind of work is, essentially, *interpretation work.*

These findings are important for sociological studies that depend on documentary evidence produced within a particular practice for local purposes, especially if issues of ethics and legal responsibility are at stake. They are also very informative with respect to the persistent troubles with 'organizational memory' and 'knowledge management' systems. But to construe Garfinkel's argument as positing that instructions, by virtue of being linguistic constructs, always, everywhere, under all circumstances, are 'incomplete' and require 'interpretative work' and, hence, have 'all of the *ad hoc* and uncertain properties that characterize every occasion of the situated use of language', is preposterous. In spite of Garfinkel's insistence that ethnomethodological studies are not meant to 'encourage permissive discussions of theory' (p. viii), Suchman's interpretation turns an incisive analysis of certain methodological issues in investigating certain kinds of work practice into a philosophical proposition.

That Suchman, at this crucial point in her argumentation, should have read Garfinkel in such a way is puzzling. Righteous fervor in the struggle against the cognitivist version of 'rule governance' would go some way towards explaining the urge to come up with a counter-theory. But there is also the special character of Garfinkel's *Studies in Ethnomethodology* to consider. In this book Garfinkel focused on what one could call problematic situations, either 'normally, naturally' problematic ones like the coding case, or contrived ones like the breaching experiments and the counselling experiment. This focus is hand-in-glove with his objective: showing that the taken-for-granted assumptions of everyday life, the 'natural attitude', are researchable phenomena in their own right. This focus may, however, leave the impression that, according to Garfinkel, ordinary people, in the natural attitude of their daily work, struggle to make sense of coding schemes, production plans, administrative procedures, time tables. But that actors are not 'judgmental dopes' does not mean that they make it through the day by engaging in endless interpretive work and *ad hoc* activities.[5]

2.2 Sources of the Interpretation Myth in Sociology

Rule–skepticism is strong in the social sciences. There is an urge to interpose 'interpretation work' to account for practitioners' acting in accordance with rules, plans, schemes, etc.,—an urge so strong, in fact, that even Ryle's and Wittgenstein's demolition of this myth seems exceedingly difficult to grasp and accept. One source of this strong skepticist urge is the bafflement of a field worker when faced with myriad activities and inscriptions that do not seem to add up and make sense. It is natural for the field worker to project this bafflement and ascribe interpretation work to the observed practices: a natural fallacy.

In the kinds of setting that are of primary concern to CSCW (cooperative work in organizational settings) the field worker will often find it difficult to align observable rule formulations (stipulated procedures, etc.) with the rules followed by practitioners. In such settings, the field worker will often find presumptive rule formulations that are not enforced, or seem to be in mutual contradiction, etc. One may furthermore come across rule formulations (stated procedures, work schedules, project plans) of which members seem ignorant or which they deliberately disregard. One may find rule formulations that seem to instruct actors to do a series of tasks in a specific order, for instance, 'first do A, then B, and finally C', but then members, sometimes or often, can be observed to jump from A to C while skipping B or to do B first and then A and C. Having experienced this, the field worker will be tempted to report that organizational rules do not instruct members what to do in a step-by-step manner, that they only convey general policies not operational guidelines, etc. An investigation of work practices that account for these practices in terms of the stated rules, by reference to the proverbial rule book, will obviously produce an utterly distorted account. In view of this, sociologists have adopted various strategies. Some will introduce a distinction between 'formal' and 'informal' organization or between 'formal' and 'informal' rules, etc. (e.g., Selznick, 1948). Others will effectively dissolve the very notion of rule-governed action by adopting the rule-skepticist position that work practices are 'essentially *ad hoc*' (e.g., Suchman, 1983, 1987; Bucciarelli, 1988a; Button and Harper, 1995).

The field worker's fallacy basically consists in mistaking the logical grammar of the concept of 'rule formulations' for that of 'rules' (Baker and Hacker, 2009, Chapter II). That we in our ordinary discourse do distinguish 'rule formulations'

[5] It is only fair to point out that Suchman is not alone in this rule-skepticist generalization of Garfinkel. An article by Button and Harper offers a case in point (1995). The same abrupt generalization of Garfinkel's very specific observations can be found in the critical comment on *Plans and Situated Actions* by Sharrock and Button (2003).

from ‘rules’ is evident. The same rule can be stated in different ways; it can be expressed orally, in writing, by gesturing, and so on; it can be formulated in different languages, by means of different notations, at different levels of detail, at different levels of formalization, by definition or by examples, by offering different examples, etc. One can make copies of rule formulations, but not of rules. However, the field worker’s rule–skepticist fallacy is also an natural one, in as much as we in our ordinary language do not *always* make a sharp distinction between the concept of ‘rule’ and ‘rule formulation’. In everyday life, when someone changes the formulation of a rule, the change will often be seen as a change of the rule. That is, in the words of Baker and Hacker, rules and rule-formulations cannot be simply segregated into ‘watertight compartments’, for ‘the grammars of “rule” and “rule-formulation” run, for a stretch, along the same tracks’ (Baker and Hacker, 2009, p. 47).

As pointed out by Egon Bittner (1965), the investigator does not have privileged access to determining the governing sense of a stated rule (as formulated in a standard operating procedure or in a graphical representation of a classification scheme). The field worker is, by definition, an outsider to the setting; he or she does not (yet) understand the rule and has not (yet) been trained in applying the rule. In this the investigator is in the exact same situation as a novice being taught in the use of the same rule: like the novice, the investigator does not master the rule, hence the doubt, the uncertainty, the tentative applications. In other words, for lack of understanding, investigators are left with trying to *interpret* the stated rule. This is a practical-epistemological condition that makes field work serious work.

It is important to keep in mind that there is nothing intrinsic in the *form* of a rule formulation that makes it a rule formulation; the sign does not need to have a specific form, such as, say, ‘If. . . then. . .’ or ‘Thou shall not. . .’. In the words of Baker and Hacker, again,

> For the architectural historian or engineering students the blueprint describes the building or machine. It is used as a description; and if the building is other than as is drawn in the blueprint, then the blueprint is false. But for the builder or engineer the blueprint is used as an instruction or rule dictating how he should construct the building or machine. If what he makes deviates from the blueprint then (other things being equal) he has erred — built incorrectly (Baker and Hacker, 2009, p. 52)

As field workers we cannot expect that rules are nicely stated in rule formulations that have the paradigmatic form of rule formulations; that is, we have to look at the actual practices of rule governed action and at the actual role of the various rule-stating artifacts (time tables, standard operating procedures, notation schemes, etc.) in those practices, to determine what the rule is.

But this does not mean, of course, that ethnographic accounts by necessity are capricious or subjective. For a field worker trying to establish empirically what the rules of a particular practice actually are, the stated rule (the printed schedule, the procedure description, etc.) is of course an important source of data; but only one source among many, and a source that the field worker has no privileged access to understand. To establish what the rules in fact are, the field worker will have to consider how the stated rule is observably used in the setting. How are members instructed in applying the rule? How is it explained, exemplified, etc.? Furthermore, how is it invoked to justify or explain, justify, excuse, rectify, chastise, reprimand actions? How are actions that to an outsider might be seen an transgression of the stated rule actually treated by members? Are they approved or applauded; are they countenanced or condoned; or are they corrected, censured, castigated? In short, the task of determining the operative sense of a stated rule 'is left to persons whose task it is to decide such matters' (Zimmerman, 1966, p. 155).

In all of this, the field worker is engaged in determining the rule of the practices *empirically*; their *normative* character to practitioners themselves can easily escape him or her. This is an insidious source of confusion and misrepresentation: the source of a natural fallacy. The fallacy arises when the inexorable investigational condition—that the operative sense of stated rules requires interpretation—is somehow construed as the human condition. That is, the interpretation work in which the field worker, *as an outsider*, is compelled to engage is conceived of as an inescapable condition *for all*, members and outsiders alike. The field worker's fallacy consists in elevating his or her own mundane epistemological problems to the level of a human condition. This is of course confused and is a variant of the intellectualist legend: the conditions of intellectual work are the paradigm of the human condition and specifically intellectual practices the model of rational conduct.

3 The Consequences of Counter-Cognitivism

Since CSCW aims at devising technologies that, when used, regulate aspects of interaction in cooperative work settings, and since work practices are both historically specific and specific to domains and settings, we need to understand the specificity of work practices: the rules, concerns, criteria, typifications, distinctions, priorities, notations, schemes, plans, procedures, etc.; how such rules etc. are applied unreflectively and unhesitatingly and also sometimes with some uncertainty; how they are sometimes questioned, debated, amended, elaborated; how they are taught and instructed; and how they evolve over time and how they propagate beyond local settings, are acquired, emulated, appropriated, etc.; what techniques practitioners bring to bear in their practices, how contingencies are dealt with routinely, how they employ the tools of the trade as intended in their design, how they often use them in 'unanticipated' ways, and how they often also seize incidental resources in the setting; and so on.

For CSCW researchers to be able to do so, preconceived constructions of 'human nature' or 'sociality', applied as templates in technological design or in the production and analysis of ethnographic findings, would cause immediate sterility, since the specificity of cooperative work practices then would have been lost. It is thus of vital importance for CSCW to heed Egon Bittner's call for 'realism in field work' and develop and maintain what he calls an 'unbiased interest in *things as they actually present themselves*' to practitioners (Bittner, 1973). For these reasons phenomenological sociology as represented by Alfred Schutz and the ethnomethodologists has played a very important role in CSCW. This was not, of course, preordained, as other intellectual traditions offer contributions that, *for these purposes*, are concordant with the phenomenological movement (e.g., the philosophies of Wittgenstein and Ryle, the tradition of 'symbolic interactionism', and in some respects also the psychology of Vygotsky). So far many if not most CSCW researchers will agree. The problem is that, to achieve this, 'the field worker needs not only a good grasp of the perspectives of those he studies but also', as pointed out by Bittner, 'a good understanding of the distortive tendencies his own special perspective tends to introduce'.

Because of the metaphysical tenet of *Plans and Situated Actions*, the version of ethnomethodology that has been widely received in CSCW is one in which phenomenological sociology has been deprived of its most important insights: the principle of specificity of practices ('zones of relevance', 'finite provinces of meaning', etc.), the notion of the 'natural attitude' of working, the principle of taking the point of view of practitioners (what are *they* up to? how does the world look when on does *that* kind of work?), the method of conceiving of work practices in practitioners' own terms, as opposed to through *universal* constructs, *externally imposed* criteria, etc.

While Suchman's book showed a way out of cognitivism, towards studies of actual work practices, it also—unwittingly but effectively—was instrumental in establishing another dogma, the dogma that plans play no role *in* action, i.e., in determining the course action. The dogma was not intended but was the consequence of Suchman's mirroring cognitivism's metaphysical form of reasoning. The existence of this dogma is evident. Consider, for example, this admonishment from a widely cited article written by Paul Dourish and Graham Button:

> The disturbingly common caricature of [Suchman's] position is that there are no plans, but only "situated actions" — improvised behaviors arising in response to the immediate circumstances in which actors find themselves and in which action is situated. In fact, as Suchman has been at pains to point out, she did, in fact, accord an important status to plans as resources for the conduct of work. Her argument was that plans are one of a range of resources that guide the moment-by-moment sequential organization of activity; they do not lay out a sequence of work that is then blindly interpreted. (Dourish and Button, 1998, pp. 405 f.)

This defense of Suchman stayed within the metaphysical discourse established by Suchman's original argument: 'plans [. . .] do not lay out a sequence of work that is then blindly interpreted'. Disregarding the perplexing expression 'blindly interpreted' (what would it mean to 'interpret blindly'?), I take it that the authors meant to say 'plans do not lay out a sequence of work that is then blindly [*followed*]'. Well, plans sometimes are. In fact, they are most of the time. They are routinely applied as unproblematic guidelines or instructions, and if plans do not lay out a sequence of work that is then normally followed without reflection then there *are* no plans as we ordinarily understand the term. The statement that 'plans do not lay out a sequence of work that is then blindly [followed]' only makes sense if 'plans' are not really *ordinary plans*, but rather 'plans' as conceived of in cognitivist theorizing.

But then 'plans' as conceived of in cognitivism cannot be *followed* (or 'interpreted'), for they are obscure causal mechanisms.

In another widely cited article Button and Harper cautioned 'designers' in CSCW not to misunderstand the concept of 'work practice' (Button and Harper, 1995). Their concern was occasioned by the increased use of the concept of 'work practice' in the 'design community', especially in the wake of Suchman's *Plans and Situated Action*. They noted with barely suppressed irritation that the concept of 'work practice' was 'being invoked more and more in the rhetoric that surrounds design' (p. 266) and had become 'something of a rallying cry in many quarters of CSCW' (p. 279); but what gave them particular cause for concern was that the concept of 'work practice' had its 'origins in sociology' and had 'a well established place in the sociology of work' and that the 'sociological underpinnings' of the concept and 'the order of work organisation to which Suchman refers' might not be properly understood (pp. 263–265). The problem, according to Button and Harper, was that the concept of 'work practice' in sociology of work was used for 'describing amongst other things the rule book formulations of work *as well as* the situated responses to contingent circumstances' (p. 265, emphasis added). The authors' issue with this use of the concept of 'work practice' seems to be that 'rule book formulations of work' are *even considered* in accounts of work practices. They contrasted this with what they presented as the ethnomethodological position. Referring to Garfinkel and Bittner's study of the 'normal, natural troubles' of coding clinical records, they made the following claims:

> The finding of Garfinkel's study was [. . .] that *in practice* it was not possible to exhaustively and explicitly stipulate the coding rules. However full and detailed the rules in the coders' handbook were made, each time coders had to administer the schedule, there was a need for decision and discretion. The coders would resort to a variety of *practices* to decide what the coding rules actually required of them and whether what they were doing was actually (or virtually) in correspondence with those rules. These were essentially *ad hoc* relative to the coding manual's purportedly systematic character. It was through the implementation of these *ad hoc* practices that coders achieved their work of coding. The formalised account of the work of coding as applying the rules omits the very practices that organise that work. (Button and Harper, 1995, p. 265)

According to Button and Harper, then, 'work-practices constitute, in its [sic] detail and in the face of the unfolding contingencies of work, the temporal order of work and the ordinary facticity of domains of work' (p. 264). In short, practices are 'essentially *ad hoc*'.

Now, it should be said immediately and with emphasis that this pallid notion of practice has not prevented *these* authors from investigating complex professional practices and, in doing so, blessing CSCW with some of the most influential studies of work practices (e.g. Harper et al., 1989a, b; Bowers et al., 1995). Harper and Button have provided us with very insightful analyses of normatively regulated conduct in ordinary work settings. What we have, then, is what looks like a clean separation of 'theory' and 'practice', the hallmark of a dogma. Its paralyzing effects show up *elsewhere*.

The upshot of all this is that *Plans and Situated Actions* has been instrumental in replacing the cognitivist dogma with another. The new dogma, while certainly far more fertile in terms of encouraging empirical work than that of the cognitivist wasteland, made CSCW largely numb to the massive web of coordinative practices and techniques that characterizes typical modern workplaces. Their presence is, of course, not flatly denied; it is even acknowledged that they serve as 'important' 'resources' for situated action. But it is, *ab initio* and dogmatically, posited that 'plans do not lay out a sequence of work that is then blindly [followed]', and that work practices are 'essentially *ad hoc*'. The concept of 'practice', which plays a defining role for CSCW's research program, is hereby emptied of content, rendered useless. The practice-oriented research program of CSCW is effectively undermined.

The implication is a strong methodological bias even in ethnographic fieldwork. To see this, take for instance the Bob Anderson's *support* of Suchman against her critics:

> If we set the context for the ethnography at the level at which many ethnographers feel most comfortable, we will find they are *almost obsessed with change of one sort or another*. In picking their way through the minutiae of routine action, *prominence is (endlessly) given to the innovative, the ad hoc, and the unpredictable rife in the workplace and elsewhere*. Change, here, is the very stuff of ethnography. (Anderson, 1997, p. 177, emphases added)

This is an accurate statement of the diagnosis. However, Anderson does not mean this as a critique but as a formulation of an *optional* methodological preference.

What Anderson does not take into account is Egon Bittner's warning, in the early days of ethnomethodology, against the fieldworker's fallacy: Because the ethnographer is 'a visitor whose main interest in things is to *see* them', to him or her 'all things are primarily *exhibits*'; they are 'never naturally themselves but only *specimens* of themselves' (Bittner, 1973, p. 121). These are unavoidable conditions for any ethnography. However, as a result, a certain intellectualism may distort the ethnography:

> Since the field worker [. . .] always sees things from a freely chosen vantage point—chosen, to be sure, from among actually taken vantage points—he tends to experience reality as being of subjective origin to a far greater extent than is typical in the natural attitude. Slipping in and out of points of view, he cannot avoid appreciating meanings of objects as more or less freely conjured. (Bittner, 1973, pp. 121 f.)

And in doing so, the field worker describes the setting 'in ways that far from being realistic are actually heavily intellectualized constructions that partake more of the character of theoretical formulation than of realistic description' (Bittner, 1973, pp. 123 f.). Bittner is here restating and underscoring Winch's fundamental proposition, namely, 'it is not open' to the sociological investigator 'to impose his own standards from without. In so far as he does so, the events he is studying lose altogether their character as *social* events' (Winch, 1958, p. 108).

If we in our analyses of work practices *give prominence* to 'the innovative, to the *ad hoc*, to the unpredictable rife in the workplace', or to the 'unfolding contingencies of work' we will be under the 'serious misunderstanding' of rendering practices in ways that are 'heavily intellectualized constructions.' That is, if we heed Bittner's warning, we cannot take Anderson's rather agnostic position and simply accept the reputed 'obsession with change' as a methodological preference some may wish to adopt and others not. For an ethnography characterized by an 'obsession with change' is a 'heavily intellectualized construction' just as much as one obsessed with stable structures, equilibrium, and what not.

Obviously, such 'heavily intellectualized constructions that partake more of the character of theoretical formulation than of realistic description' can only impede CSCW's ability to address the construction and use of plans in cooperative work, for the ordinary plans of ordinary cooperative work in ordinary organizational life are then rendered delusory, the natural attitude of practitioners excluded, *if they are seen at all*.

In sum, the consequence of all this is, at best, that our studies become unrealistic. At worst, CSCW abandons its practice-oriented technological research program and becomes irrelevant as yet another research area producing *post hoc* descriptions of the various uses of new products, services, and facilities coming onto the market.

4 The Problem of Computational Artifacts

I have focused on Suchman's account for obvious reasons. Her contribution is, justifiably, seen as a major contribution to the intellectual foundation of CSCW and HCI. By centering on the relationship between the concepts of 'plans' and 'situated action', it provided the foundational conceptual apparatus of CSCW: its axis of conceptualization. However, this conceptualization is no longer holding up. Confusion reigns with respect what constitutes CSCW's problem and its scope.

In fact, CSCW's central problem, its axis of conceptualization, can not be adequately defined in terms of 'plans and situated action'. CSCW's program is not usefully conceived of in terms of 'plans and situated action' or similar, for the problem CSCW has to address is not primarily how *normative* constructs such as ordinary plans and other organizational rules are applied in practice but how practitioners use or may use *machines* in following the rules of their cooperative work practices (schedules, procedures, categorizations, schemes). And that is an entirely different issue. I will suggest, therefore, that CSCW's problem is circumscribed by the concepts of rule-governed work practices on one hand and on the other highly regularized causal processes in the form of 'computational artifacts' used, within cooperative work practices, for purposes of coordinating interdependent activities. How do we incorporate mechanical regulation of action and interaction in cooperative work practices? How can we exploit the immense flexibility of computational technology to give ordinary workers effective control over the distributed execution, construction, and maintenance of mechanized coordinative protocols?

The point of Suchman's critique of cognitivism was of course to be able to address the problem of computational artifacts. The problem she set out to address was not the role of plans in situated action *in general* but the problem of how plans *incorporated in computational artifacts* ('reified in the design of intelligent machines') affect human action and interaction, how they can be appropriated in our practices, and what implications for design may be derived from that: 'I have attempted to begin constructing a descriptive foundation for the analysis of human-machine communication' (p. 180).

Based on her study of a problematic case of human-computer interaction (a computer-based system intended to instruct users of a complex photocopier), Suchman concluded:

> The application of insights gained through research on face-to-face human interaction, in particular conversation analysis, to the study of human-computer interaction promises to be a productive research path. The initial observation is that interaction between people and machines requires essentially the same interpretive work that characterizes interaction between people, but with fundamentally different resources available to the participants. In particular, people make use of a rich array of linguistic, nonverbal, and inferential resources in finding the intelligibility of actions and events, in making their own actions sensible, and in managing the troubles in understanding that inevitably arise. Today's machines, in contrast, rely on a fixed array of sensory inputs, mapped to a predetermined set of internal states and responses. The result is an asymmetry that substantially limits the scope of interaction between people and machines. Taken seriously, this asymmetry poses three outstanding problems for the design of interactive machines. First, the problem of how to lessen the asymmetry by extending the access of the machine to the actions and circumstances of the user. Secondly, the problem of how to make clear to the user the limits on the machine's access to those basic interactional resources. And finally, the problem of how to find ways of compensating for the machine's lack of access to the user's situation with computationally available alternatives. (Suchman, 1987, pp. 180 f.)

Suchman summarized the conclusion as follows:

> I have argued that there is a profound and persisting asymmetry in interaction between people and machines, due to a disparity in their relative access to the moment-by-moment contingencies that constitute the conditions of situated interaction. Because of the asymmetry of user and machine, interface design is less a project of simulating human communication than of engineering alternatives to interaction's situated properties (Suchman, 1987, p. 185)

The metaphor of 'asymmetry' is perplexing. Would it make sense to say that my relationship to my alarm clock is 'asymmetrical' because it seems completely insensitive to the effects of last night excesses, due to 'a disparity' in our 'relative access to the moment-by-moment contingencies'? Does a notion of 'asymmetry' not presume a common metric, some significant commonality? In which way, then, are the two parties commensurate? I take it that Suchman would have answered those expressions of disbelief by emphasizing what she already said in the quote above: that 'interaction between people and machines requires essentially the same interpretive work that characterizes interaction between people, but with fundamentally different resources available to the participants'. This is truly bewildering: 'essentially the same interpretive work'!

Now, Suchman certainly *did not intend* to advocate a cognitivist position but rather to provide an 'alternative account', but something was fatally amiss in this line of reasoning. The problem, or so it seems to me, is that she conceived of computational artifacts as linguistic constructs: 'I argue that the description of computational artifacts as interactive is supported by their *reactive, linguistic,* and internally *opaque* properties' (pp. 7, 16). On that view, it was not a dramatic jump to assert that 'interaction between people and machines requires essentially the same interpretive work that characterizes interaction between people'. But then the alternative account to cognitivism has already been effectively abandoned. The containment strategy turns out not to contain cognitivism at all. The high ground has been evacuated, the major highway intersections long since lost, defeat all but conceded. The strategy has landed us in something akin to a Green Zone, surviving at the mercy of the adversary we proudly set out to vanquish.

Still, the notion that computational artifacts have 'linguistic properties' is, of course, one that is taken for granted in the ordinary discourse of computer science where it is in common usage (witness terms like 'programming language' etc.). There, in the context of everyday reasoning about programming, it is (largely) unproblematic, as harmless and pragmatic as vernacular expressions such as 'sunrise' and 'sunset'. However, outside of the discourse of computer science, and especially in the context of reasoning about human-computer interaction and computer-supported cooperative work, the notion of computational artifacts as linguistic constructs is fatally misleading. This should be evident inasmuch as Suchman conceived of computational artifacts in exactly the same terms as she conceived of 'plans': as 'essentially linguistic representations' (p. 186). It is a category mistake of the first order.

The problem is, as should be clear by now, that the seeds of the defeat were there from the very beginning, inherent in Suchman's strategy of trying to demolish cognitivism while accepting cognitivism's metaphysical discourse and its mechanistic premises. In the cognitivist account, there is no room, i.e., no *logical space*, for considering normative regularity. There is only room for empirical observations of concomitance (and a significant amount of speculative theorizing, of course). On this view, 'rules' can only be conceived of as hypothetical propositions, derived inductively, by correlation, from observed regularity. The notion that rules, plans, etc., are an essential part of our practices, as public standards of correctness or incorrectness, is unintelligible on this view. Ordinary normative activities, such as asking for the way to the nearest grocery shop, suggesting a definition of a word, setting the table for a dinner party, executing a production plan, etc. have no place in this discourse. Suchman's 'alternative account' unfortunately left this view unchallenged. As a result, the normative nature of our practices was lost in the fire. For the same reason, computational artifacts are ascribed properties of the normative category, which means that we end up with trying to understand 'human-computer interaction' with the key concepts of 'practices' and 'computational artifacts' completely confounded.

Chapter 13
Dispelling the Mythology of Computational Artifacts

If calculating looks to us like the action of a machine,
it is the human being *doing the calculation that is the machine.*
(Wittgenstein, 1937–1944, IV § 20)

Turing's "Machines".
These machines are humans *who calculate.*
(Wittgenstein, 1946–1949, § 1096)

What is paralyzing CSCW is the assimilation of the normative concept of plans, schemes, schedules, and similar organizational constructs with the mechanist concept of causal determination of rational action. This leaves no conceptual room for CSCW. Given this confusion, any thought anybody may have of building or studying computational artifacts that embody plans, schemes, schedules, will unavoidably be met with disbelief or disinterest: 'But we know already that that's wrong/impossible, for action is essentially *ad hoc*!' So, instead the good CSCW researcher will focus on building or studying computational artifacts that embody as little computational regulation of human interaction as possible. The ideal, one might say, is computational artifacts on the model of plasticine.

The root of these problems is the concept of computational artifacts, or rather: the philosophy of computing, a channel of endless mystification. To take just one example, from a book by a well-respected computer scientist:

> I etch a pattern of geometric shapes onto a stone. To the uninitiated, the shapes look mysterious and complex, but I know that when arranged correctly they will give the stone a special power, enabling it to respond to incantations in a language no human has ever spoken. I will ask the stone questions in this language, and it will answer by showing me a vision: a world created by my spell, a world imagined within the pattern of the stone. (Hillis, 1998, p. vii)

The interesting thing is that Hillis then goes on to give an instructive account, completely free from such, well, incantations, of the concept of computing, showing how computing operations can be performed by means of causal processes in mundane (mechanical, hydraulic) devices.

What mystifies us is the 'mythology of a symbolism', as Wittgenstein puts it: in this case the mythology of computational artifacts. It is, *prima facie*, a mythology of our everyday dealings with computational artifacts. What mystifies us is, first of all, the computer's apparent vivacity: the impression of an infinite number of possible internal states and its dynamic reactivity. It is an unavoidable aspect of

K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1_13,

using computers that the user engages in forms of thinking in which 'interacting' with electronic processes is considered natural; it is, in a strict Schutzian sense, the user's natural attitude. As computer users and technicians we engage—as the most natural thing in the world—in ascribing linguistic and other anthropomorphic properties to computational artifacts: programming 'language', 'memory', 'information', 'ontology'. The computational artifact mystifies us in the same way as clocks and their ability to 'tell time' for ages have allured thinkers to speculate about the mystery of time. This mystification is unavoidable even if one knows that the time piece executes a causal process in a manner that is carefully devised to be exceptionally regular, and that it is this regular causal process that allows us to use the clock as a standard metric for measuring the sequential order or irreversible nature of other casual processes, from heart beats to train movements (Gell, 1992). A clock does not 'tell' us anything more about time than the meter rod 'tells' us about space. But the meter rod in Paris, which has served as standard referent for measuring spatial properties of things, does not mystify us, because it does not *do* anything. The clock, however, seems to *do* something when its springs uncoil, its gears turn, and its hands move; it is an automaton, and we therefore unhesitatingly ascribe the property of 'telling time' to it.

Or to take another, more mundane, example, there are devices for sorting eggs according to their size. The eggs will roll along a slightly declining plane that has holes in different, but increasing sizes: small eggs will drop into the first holes, medium size eggs into the next ones, and so on, until only XL eggs remain. The device does not measure the size of eggs; the operator measures the size of eggs by means of the device, according to the standards agreed to by the egg industry. What makes us engage in 'incantations' and ascribe 'egg-sorting' behavior to the 'egg sorter' is that the 'egg sorter' is, within the horizon of sorting eggs, self-acting. It is the automatic performance—the allocation of the control function to the technical implement—that prompts us to engage in such ascription of function.

Another source of mystification is the manner in which the software machine is built: it is built by a process that, to the programmer and his or her admiring observers, is a linguistic activity. The programmer *writes text: code*. What happens to the code afterwards is hidden from view, out of mind, for it is executed automatically: the transformation of the 'source code' by the compiler into 'object code' in binary format. In this process, a linguistic construct, the source code, is transformed into a binary machine that, in the form of electronic patterns, can loaded into RAM and then executed in conjunction with the hard-wired machinery of the CPU. The source code is not a machine but the blueprint for one; but in contrast to an ordinary blueprint, which is also a linguistic construct, the source code is not handed over to a machinist who then builds the intended machine but is submitted to a special software machine, the compiler, that automatically performs the construction of the intended machine as specified in the blueprint or source code.[1]

[1]This linguistic mystique also appears when a user 'interacts' with the computational artifact by writing 'commands' that then activate an entire system of machines under the control of the computer's operating system. It is worth noticing that such a mystique is absent when a user pushes the start button on a dish washer.

A deeper source of the mystification surrounding the computer, or rather: *a more insidious source*, is that we routinely use software machinery, i.e., regimented causal processes, as an integrated aspect of our *normative* or rule-following practices and that we are tempted to assimilate the two—the causal process and our use of it—and become mystified, struck with awe at the fruit of our hands. Just as it is not clocks that 'know' the time or 'tell us' what time it is, but we who use clocks to 'tell the time', it is not the computer that computes, calculates, executes plans, searches for information, etc., but we who apply computational artifacts to do so in our normative practices of computing, calculating, executing plans, searching for information, etc., just as we have previously employed abacuses, slide rulers, desktop calculators, time pieces, filing cabinets, etc.

1 Computational Artifacts, or Wittgenstein *vs*. Turing

The source of this conceptual muddle is, again, the failure to grasp the categorial distinction between normative behavior and mere regularity. To sort out this mess, which in the case of computational artifacts is exceptionally dense, we are in the fortunate situation that Wittgenstein was developing a critical conceptual analysis of the concept of calculation or computation that was then, in the late 1930s, being developed by Alan Turing and others.

It is fairly well-known that Turing and Wittgenstein knew each other from 1937, that Turing followed Wittgenstein's lectures on the foundations of mathematics (Wittgenstein, 1939), and that they in the course of these lectures engaged in discussions concerning the nature of mathematics. What is less well known is that Turing sent a reprint of his paper to Wittgenstein in February 1937 (Copeland, 2004, p. 130) and that Wittgenstein, in a bulky collection of manuscripts on the philosophy of mathematics written from 1937 to 1944, subjected (*inter alia*) Turing's mechanistic framing of his argument to incisive critical discussion (Wittgenstein, 1937–1944).[2]

A few remarks on the background and general context are required, however. Turing's machine was devised in the context of the 'foundations crisis' that mathematicians and philosophers then believed had struck mathematics, and Wittgenstein's critique of Turing and his mechanist thesis was but a theme in his critical discussion of the very notion that such as crisis existed and of the attempts to overcome it.

1.1 Foundations Lost

The conceptual underpinnings of computing technologies (symbolic logic, algorithmics, recursive functions, etc.) grew out of techniques that were developed in the

[2]For very useful commentaries on Wittgenstein's critique of Turing in particular and the 'mechanist thesis' in general, cf. the penetrating studies by Stuart Shanker (1987c, d, 1995, 1998).

last decade of the 19th and the first decades of the 20 centuries in an attempt to establish mathematics on a complete and consistent foundation.

By the end of the nineteenth century, leading mathematicians had become deeply concerned with the foundations of their science. It was felt by many that the impeccable certainty and consistency of mathematical knowledge could be questioned. It was widely perceived as a 'foundations crisis'. Developments in mathematics such as non-Euclidian geometry (by Lobachevsky, Riemann, and others) and Cantor's theory of sets and 'transfinite numbers' had made it problematic for many mathematicians to continue to rely on intuition in proving theorems. Their concerns became acute when paradoxes and absurdities began to emerge.

To save mathematics some mathematicians (especially L. E. J. Brouwer) went so far as to state that only those mathematical proofs that could be constructed by 'finite' methods, i.e., by definite and surveyable steps, and which therefore were deemed 'intuitively' evident, were acceptable as *bona fide* members of the body of mathematics. Going under the name 'intuitionism' or 'constructivism', this program was prepared to jettison whatever could not be proved by finite methods, although this would chuck off large chunks of highly useful mathematics. Unsurprisingly this Procrustean program was deemed unattractive by most of the mathematicians that participated in trying to overcome the 'foundations crisis'.

The main axis of attack on the crisis was developed by the British philosopher Bertrand Russell. He saw mathematics as an 'edifice of truths', 'unshakable and inexpugnable', a vehicle of reason that lifts us 'from what is human, into the realm of absolute necessity, to which not only the actual world, but every possible world, must conform' (Russell, 1902, pp. 69, 71). Little wonder that he felt an urge to safeguard mathematics. In his effort to do so, he developed a research program that in the foundational debates became known as 'logicism', a program to rebuild mathematics into a monolithic structure of propositions based on a drastically limited set of axioms and rules of inference. Building on the work of Frege and Peano, he first made an impressive attempt to reconstruct mathematics on the basis of first-order logic in the form of a theory of 'classes' or sets (Russell, 1903). However, he had barely finished the book before he realized that the edifice he had erected to salvage the 'realm of absolute necessity' by establishing mathematics on the unshakeable basis of logic—was deeply flawed: the concept of 'class' derived from Cantor's set theory led to absurdities. Mathematicians like David Hilbert were dismayed:

> In their joy over the new and rich results, mathematicians apparently had not examined critically enough whether the modes of inference employed were admissible; for, purely through the ways in which notions were formed and modes of inference used — ways that in time had become customary — contradictions appeared, sporadically at first, then ever more severely and ominously. They were the paradoxes of set theory, as they are called. In particular, a contradiction discovered by Zermelo and Russell had, when it became known, a downright catastrophic effect in the world of mathematics. (Hilbert, 1925, p. 375)

Russell spent most of a decade—together with Alfred Whitehead—building another foundation of mathematics (Whitehead and Russell, 1910). In order to exclude paradoxes, the new foundation was built by means of an elaborated logical calculus called the 'theory of types', 'a complicated structure which one could hardly identify with logic' in the normal sense (Davis and Hersh, 1980, p. 333). In Hilbert's words, 'Too many remedies were recommended for the paradoxes; the methods of clarification were too checkered' (Hilbert, 1925, p. 375).

In an effort to avoid these problems Hilbert developed another approach, generally known as 'formalism'. Where Russell dreamt of restoring the 'realm of absolute necessity', Hilbert's program was predicated on the 'conviction' that every mathematical problem is solvable: 'We hear within us the perpetual call: There is the problem. Seek its solution. You can find it by pure reason, for in mathematics there is no *ignorabimus*' (Hilbert, 1900, p. 248). For Hilbert, therefore, the situation was intolerable:

> Let us admit that the situation in which we presently find ourselves with respect to the paradoxes is in the long run intolerable. Just think: in mathematics, this paragon of reliability and truth, the very notions and inferences, as everyone learns, teaches, and uses them, lead to absurdities. And where else would reliability and truth be found if even mathematical thinking fails? (Hilbert, 1925, p. 375)

To salvage mathematics as an unshakeable edifice, as the 'paragon of reliability and truth', Hilbert formulated a grandiose 'formalist' program to demonstrate or ensure—in the most rigorous manner—that the corpus of mathematical calculi constitute a complete, consistent, and decidable formal system. His vision was to develop mathematics 'in a certain sense' 'into a tribunal of arbitration, a supreme court that will decide questions of principle and on such a concrete basis that universal agreement must be attainable and all assertions can be verified' (Hilbert, 1925, p. 384). Hilbert's strategy was to regain the certainty of 'intuitive' perceptual inspection even in regions of mathematics such as 'transfinite number' theory that defied perceptual inspection, and to do so by treating 'formulas' as 'concrete objects that in their turn are considered by our perceptual intuition' (Hilbert, 1925, p. 381). In other words, the approach he proposed was to encode mathematical propositions in a 'formal' language, that is, (a) to 'replace' 'contentual references' by 'manipulation of signs according to rules', (b) treat 'the signs and operation symbols as detached from their contentual meaning' and (c) thereby ultimately obtain 'an inventory of formulas that are formed by mathematical and logical signs and follow each other according to definite rules' (Hilbert, 1925, p. 381). Now, Hilbert most certainly did not think of mathematics as a meaningless game (in epistemological matters, he was a 'realist' or Platonist); but, to stave off impending skepticism, he was ready to advocate this radically formalistic approach as a methodology, and as a result his formalist program was accused of treating mathematics as a meaningless game. Unruffled, Hilbert retorted by appropriating the epithet: 'What, now, is the real state of affairs with respect to the reproach that mathematics would degenerate into a game? [. . .] This formula game enables us to express the entire thought-content of the science of mathematics in a uniform manner and develop it in such a way that, at the same time, the interconnections between the individual propositions and facts become clear' (Hilbert, 1927, p. 475).

Although Hilbert's program took a different course than Russell's, the two programs shared basic premises, namely that the problems facing mathematics at the time were *epistemological problems*: how could mathematicians ensure the impeccable and unassailable validity of the propositions of higher mathematics where 'perceptual intuition' seemingly had lost traction ('transfinite numbers' etc.)? What united the two camps was the belief that mathematics was in need of *epistemological* underpinnings and that recent extensions to mathematics made the received epistemological foundations questionable, as they no longer ensured 'self-evident' truths. And both programs shared the belief that if it could be shown that the whole of mathematics could be derived from a few 'self-evident' axioms and rules of inference, in a finite sequence of steps, then the state of impeccable certitude could be regained.

Wittgenstein played a key role in the development of mathematical logic and in the development of the philosophy of mathematics,[3] but did not share the sense of crisis. There was, in his view, nothing in the development of mathematics that should cause *angst* among mathematicians. In the midst of the intellectual turmoil he commented coolly: 'I have the impression that the whole question has been put wrongly. I would like to ask, Is it even *possible* for mathematics to be inconsistent?' (Wittgenstein, 1929–1932, p. 119). Observing the acute fear of deep-seated inconsistencies and antinomies, he commented that a contradiction 'is only a contradiction if it is there': 'People have the notion that a contradiction that nobody has seen might be hidden in the axioms from the beginning, like tuberculosis. You do not have the faintest idea, and then some day or other the hidden contradiction might break out and then disaster would be upon us' (ibid., p. 120). Russell's antinomies (e.g., his infamous paradox) had of course caused anxiety and wrought havoc, but in Wittgenstein's view these antinomies 'have nothing whatsoever to do with the consistency of mathematics; there is no connection here at all. For the antinomies did not arise in the calculus but in our ordinary language, precisely because we use words ambiguously' (ibid., p. 121). On Wittgenstein's diagnosis, the problem was not a problem *within* mathematics but was rooted in deep confusion in the way mathematicians and philosophers alike *interpreted* mathematics: 'If I am unclear about the nature of mathematics, no proof can help me. And if I am clear about the nature of mathematics, then the question about its consistency cannot arise at all' (ibid.). The source of the confusion, Wittgenstein found, was the view, shared by both logicists and formalists, that mathematics is an investigation that, on par with physics, investigates a mathematical reality that exists independently of mathematical practice and which serves to adjudicate the correctness of that practice and language. A major part of his critical examination of the philosophy of mathematics

[3] In fact, the philosophy of mathematics was Wittgenstein's central concern from the *Tractatus* until about 1945 (when he switched his attention to another muddle: the philosophy of psychology). This is evidenced by the records of his discussions with members of the Vienna Circle (1929–1932), the manuscripts published as *Philosophical Remarks* (1930) and *Philosophical Grammar* (1931–1934), his lectures at Cambridge (1932–1933, 1939), his *Remarks on the Foundations of Mathematics* (1937–1944), and of course the *Philosophical Investigations* (1945–1946). Indeed, it was his intention that the second volume of *Philosophical Investigations* should be based on the later manuscripts on mathematics.

was therefore devoted to exposing these dearly held epistemological assumptions (Shanker, 1986b, 1987a; Gerrard, 1991).

The overriding aim of his argument with philosophy of mathematics was to show that mathematical propositions are not empirical propositions about preexisting objects but, rather, that 'mathematics forms a network of norms' (Wittgenstein, 1937–1944, VII §67). Mathematical propositions do not appear 'hard', 'inexorable', nonnegotiable, because they are crystallizations of experiential evidence, for then they would be contingent; they appear 'hard' because they are normative: If one says 'If you follow the *rule*, it *must* be like this', then one does not have 'any *clear* concept of what experience would correspond to the opposite', for 'the word "must" surely expresses our inability to depart from *this* concept'. The statement that 'it must be like this', does not mean, 'it will be like this': 'On the contrary: "it will be like this" chooses between one possibility and another. "It must be like this" sees only one possibility'. Consequently, 'By accepting a proposition as self evident, we also release it from all responsibility in face of experience.' This, Wittgenstein argued, is the source of the 'hardness' of mathematical propositions (IV, §§ 29–31).

Mathematical propositions are rules, they specify the grammar of number words. Still, mathematical propositions are rules of a special kind, namely rules whose certainty is established by 'proofs'. There is nothing otherworldly about the proof and its objectivity: the proof is not—like a physical law—based on experiential evidence that can be produced to justify it; is *we* who accept the rule and accept to be guided the rule. The mathematical proposition can be said to be true in much the same way that one can ask: 'Is it true that there are 29 days in February this year?'. That is, its truth is grammatical, it refers to the actuality of a conventionally constituted fact. But the rule expressed by a mathematical proposition is not the result of an arbitrary choice; the proof 'confers certainty' upon the proposition by 'incorporating it—by a network of grammatical propositions—into the body of mathematics' and thereby making it incontestable (Hacker, 1989, p. 333). The proof establishes a connection between mathematical concepts and provides a concept of the connections. The proof 'introduces a new concept'; 'the proof changes the grammar of our language, changes our concepts. It makes new connexions, and it creates the concept of these connexions. (It does not establish that they are there; they do not exist until it makes them.)' (Wittgenstein, 1937–1944, III §31).

In accordance with this view of mathematics—as a network of concepts—Wittgenstein emphasized that mathematical calculi could not be reduced to one formalism: 'After all, the natural numbers are not identical with the positive integers, as though one could speak of plus two soldiers in the same way that one speaks of two soldiers; no, we are here confronted with something new' (Wittgenstein, 1929–1932, p. 36). That is, the concept of the natural number 2, the concept of the integer +2, and the rational number $\frac{2}{1}$ are not the same concepts; their grammars are distinctly different (cf. Waismann, 1930; Waismann, 1936, pp. 60 f.). One can subtract two people from the list of guests to be invited for dinner but one cannot have a negative number of guests for dinner. So, pointing to the grammatical heterogeneity of mathematics, Wittgenstein objected vehemently against the very idea that mathematics be subjected to forced formalization: '"mathematics" is not a sharply delimited concept'; rather, 'mathematics is a motley of techniques and proofs' (Wittgenstein, 1937–1944, III §§46, 48), and the 'invasion of mathematics by mathematical logic' is a 'curse', 'harmful', a 'disaster' (Wittgenstein, 1937–1944, V §§ 24, 46). Consequently, Wittgenstein saw 'mathematical logic [as] simply part of mathematics. Russell's calculus is not fundamental; it is just another calculus' (Wittgenstein, 1932–1933, p. 205). He similarly argued that what Hilbert thought of a 'metamathematics' was just another mathematical calculus next to the others and that the attempt to reduce the 'motley' of mathematics to that single calculus was harmful: 'There is no metamathematics' (Wittgenstein, 1931–1934, p. 296).

The point of Wittgenstein's critique of mathematical philosophy was not to denigrate or invalidate any part of mathematics. On the contrary. It deliberately and carefully left mathematics as it was, only—perhaps—unburdened of some of the metaphysical excretions that had accumulated in the form of mathematicians' prose interpretations of their achievements and problems and in the form of philosophical 'invasions'.

The epistemological *angst* that gripped mathematicians and philosophers of mathematics a century ago has long since dissipated. The foundational effort ended 'mid-air' around 1940 (Davis and Hersh, 1980, p. 323). As shown by Imre Lakatos (1967), some of the leading contemporary participants in foundational studies, from Russell to John von Neumann, concluded that the foundational program had collapsed. For instance, in 1947 von Neumann concluded that 'Hilbert's program is essentially dead' and that 'it is hardly possible to believe in the existence of an absolute immutable concept of mathematical rigour dissociated from all human experience' (von Neumann, 1947). It would of course be false to interpret this as an indication that Wittgenstein's critique of the foundational program has been accepted, for that is surely not the general situation.[4] Other developments have played a larger part.

Writing in 1936, Friedrich Waismann, Wittgenstein's ally in the early 1930s, observed that, although it had 'looked as if Hilbert's methods of attack would lead to the desired end', the situation 'changed essentially' with the publication of Gödel's article on 'undecidable propositions' (1936, pp. 100 f.). In Waismann's interpretation, Gödel showed that 'the consistency of a logico-mathematical system can never be demonstrated by the methods of this system':

> We had previously visualized mathematics as a system all of whose propositions are necessary consequences of a few assumptions, and in which every problem could be solved by a finite number of operations. The structure of mathematics is not properly rendered by this picture. Actually mathematics is a collection of innumerably many coexisting systems which are mutually closed by the rules of logic, and each of which contains problems not decidable within the system itself. [. . .] Mathematics is not one system but a multitude

[4] In fact, Wittgenstein's manuscripts on mathematics were met with sometimes excited opposition, at first at least (key texts from this debate can be found in Shanker, 1986a). The situation is now somewhat changed, though (cf., e.g., Shanker, 1987a; Tait, 2005).

> of systems; we must, so to speak, always begin to construct anew. (Waismann, 1936, pp. 102, 120)

This picture by a colleague of Wittgenstein may not quite be official doctrine, but it is close. The notion of a universal formal language that could encompass the motley of mathematics and subject them to one uniform representational form has all but evaporated. At any rate, Waismann's description seems like an accurate description of the proverbial 'the facts on the ground', for, as one observer has put it, 'Like the hordes and horses of some fabulous khan, today's mathematicians have ridden off in all directions at once, conquering faster than they can send messages home.' (Bergamini, 1963, p. 169). The 'motley of mathematics' has asserted itself.

There is also the possibility that mathematicians have simply learned to live without Russell's high-strung faith in 'the realm of absolute necessity' and have learned to subsist and get on with their business without being fortified by Hilbert's conviction of the guaranteed solvability of mathematical problems. And in this mathematicians may have, at least indirectly, been influenced by Wittgenstein's persistent attempts to develop a cure against epistemological hypochondria.

Anyway, what first of all killed off the foundational program was probably the mundane fact that, as pointed out by Davis and Hersh in their very insightful and balanced account of *The Mathematical Experience*, the entire program was 'not compatible with the mode of thought of working mathematicians', for 'From the viewpoint of the producer, the axiomatic presentation is secondary. It is only a refinement that is provided after the primary work, the process of mathematical discovery' (Davis and Hersh, 1980, p. 343).

No surprise then that mathematicians are now rather pragmatic about the epistemological issues that previously bewitched the foundationalists. In the words of the notable mathematician Paul Cohen:

> The Realist [i.e., Platonist] position is probably the one which most mathematicians would prefer to take. It is not until he becomes aware of some of the difficulties in set theory that he would even begin to question it. If these difficulties particularly upset him, he will rush to the shelter of [Hilbert's] Formalism while his normal position will be somewhere between the two, trying to enjoy the best of two worlds. (Cohen, 1967, p. 11)

Citing this, Davis and Hersh add—rather irreverently—that 'Most writers on the subject seem to agree that the typical working mathematician is a Platonist on weekdays and a formalist on Sundays. [. . .] The typical mathematician is both a Platonist and a formalist—a secret Platonist with a formalist mask that he puts on when the occasion calls for it' (Davis and Hersh, 1980, pp. 321 f.). That is, when engaged *in doing* mathematics mathematicians conceive of themselves as engaged in discovering existent objects and relations. This is, in Schutzian terms, the 'natural attitude' of mathematicians: the necessary assumptions of their daily work, the conceptual horizon that is, and has to be, taken for granted to get on with business. But when pressed to argue the status of these elusive objects and relations prior to their discovery, mathematicians will adopt the official policy of formalism: that all they do is really to manipulate (arcane) symbols.

The sound and fury of the 'foundations crisis' was not in vain, however; not at all. Sure, as far as regaining the former serene certitude of mathematics is concerned, the enormous effort Russell and Whitehead and many others put into the foundational program came to naught, but unlike Babbage's misguided attempts at 'unerringly certain' calculation 'by steam', these efforts, while also misguided on Wittgenstein's view, were not wasted. In their failed attack on the 'foundations crisis' the techniques of mathematical logic were developed immensely compared to what had been achieved by Frege and Peano before them, and in doing so they provided the means for the formalization of algorithmics. The notion of an algorithm had, of course, been known and applied for ages (cf., e.g., Chabert, 1999); but what the foundations program accomplished was to provide algorithmics with requisite techniques and thus make algorithm a rigorous concept.

The story is of interest to us because the foundationalists, in the course of trying to overcome the 'foundations crisis', developed what—incidentally—became essential parts of the conceptual foundation of computing technology. As Russell's biographer, Ray Monk, puts it:

> in the process (and this is perhaps where the lastingly important aspect of the work lies), [Russell and Whitehead] had given an enormous boost to the development of mathematical logic itself, inventing techniques and suggesting lines of thought that would provide the inspiration for subsequent mathematical logicians, such as Alan Turing and John von Neumann, whose work, in providing the theoretical basis for the theory of computing, has changed our lives. (Monk, 1996, p. 195)

When Monk uses a phrase like 'the theoretical basis for the theory of computing', a note of caution is required, however. Computing is not a sharply defined technology but rather a family of technologies; nor are computing technologies developed on the basis of a clearly delimited theoretical basis. Like mathematics, computing is a motley of techniques and concepts that defies overarching formalization. However, the techniques and concepts of mathematical logic have been essential to the development of computing technologies. The very concept of computational artifact originated in these 'metamathematical' efforts. The metaphysical excesses of 'metamathematics' stick to the philosophy of computing. This also means that Wittgenstein's critique of the way in which this work was understood by those involved is of the highest relevance for the way in which we in CSCW and related fields conceive of computational artifacts and investigate their use in cooperative work practices.

1.2 Turing's Ambiguous Machine

Turing's famous article, 'On computable numbers...', from 1936 was designed as an attempt to investigate a central problem in Hilbert's program that had not been addressed by Gödel, namely the problem of decidability. And like Gödel had done with respect to the question of completeness and consistency, Turing demonstrated that no consistent formal system of arithmetic is decidable.

Turing began the article by considering the work of a human computer, 'a man in the process of computing', Like de Prony and many others before him, he did so in a language that is deliberately mechanistic:

> We may compare a man in the process of computing a real number to a machine which is only capable of a finite number of conditions $q_1, q_2, \ldots, q_R$ which will be called "*m*-configurations". The machine is supplied with a tape [. . .] running through it, and divided into sections (called "squares") each capable of bearing a "symbol". At any given moment there is just one square [. . .] which is "in the machine". We may call this square the "scanned square". The symbol on the scanned square may be called the "scanned symbol". The "scanned symbol" is the only one of which the machine is, so to speak, "directly aware". (Turing, 1936, p. 231)

'We may compare a man [. . .] to a machine'. A this stage, Turing was explicitly engaged in a comparison. The mechanistic language was obviously used with some caution, as the use of citation marks and expressions such as 'so to speak' suggest.

After a lengthy argument describing the technical operations performed by the 'man in the process of computing a real number' in these mechanistic terms, Turing returned to his analysis of the work of the human computer, but now in less cautious mechanistic language:

> The behaviour of the computer at any moment is determined by the symbols which he is observing, and his "state of mind" at that moment. [. . .] Let us imagine the operations performed by the computer to be split up into "simple operations" which are so elementary that it is not easy to imagine them further divided. Every such operation consists of some change of the physical system consisting of the computer and his tape. We know the state of the system if we know the sequence of symbols on the tape, which of these are observed by the computer [. . .], and the state of mind of the computer. We may suppose that in a simple operation not more than one symbol is altered. Any other changes can be split up into simple changes of this kind. The situation in regard to the squares whose symbols may be altered in this way is the same as in regard to the observed squares. We may, therefore, without loss of generality, assume that the squares whose symbols are changed are always "observed" squares. (Turing, 1936, p. 250)

Turing then summarized the results of his argument so far:

> The simple operations must therefore include:
> (a) Changes of the symbol on one of the observed squares.
> (b) Changes of one of the squares observed to another square within L squares of one of the previously observed squares.
>
> The operation actually performed is determined [. . .] by the state of mind of the computer and the observed symbols. In particular, they determine the state of mind of the computer after the operation is carried out. (Turing, 1936, p. 251)

Having talked, so far, of a person involved in some kind of computing, Turing now—and rather inconspicuously—slipped into describing 'a machine to do the work of this computer' *in exactly the same language* that was earlier used to characterize the operations of the human computer: 'We may now construct a machine to do the work of this computer. To each state of mind of the computer corresponds an "*m*-configuration" of the machine. . .' (Turing, 1936, p. 251).

It was no longer simply a comparison. Turing now, at the end of his discussion and without further argument, conceptually equated the human computer's 'state of mind' to an '*m*-configuration', that is, to what today is called a program. Later he would do that without any of the caution he displayed at the beginning of his famous paper. For example, in the lecture he gave to the London Mathematical Society in 1947, the 'rules of thumb' of the human computer and the 'machine process' are without further ado referred to as 'synonymous': 'One of my conclusions was that the idea of a "rule of thumb" process and a "machine process" were synonymous' (Turing, 1947, p. 378).

What Turing implied in 1936 but made explicit in 1947 was the 'mechanist thesis' that underlies the cognitivist concept of rational behavior: the execution of 'plans', 'schemes', etc. in human practices and in machines are categorially identical.

1.3 Conceptions of the 'Mechanical'

In obvious reference to Turing's concept of mechanical calculation, Wittgenstein (in a manuscript written 1942–1944) raised the startling question: 'Does a calculating machine *calculate*?', and to answer that he introduced a (striking, if hastily sketched) thought experiment:

> Does a calculating machine *calculate*? Imagine that a calculating machine had come into existence by accident; now someone accidentally presses its knobs (or an animal walks over it) and it calculates the product 25 × 20.
>
> I want to say: it is essential to mathematics that its signs are also employed in *mufti*.
>
> It is the use outside mathematics, and so the *meaning* of the signs, that makes the sign-game into mathematics. (Wittgenstein, 1937–1944, V §2)

That is, for a calculating machine to be said to *calculate* it has to be part of a practice in which techniques of calculation are mastered and routinely performed, that is, 'employed in *mufti*', applied to do ordinary work of that sort.

Let me elaborate his scenario a little. Imagine some ecologist doing field work somewhere in the jungles of Central Africa. Let us say that he is collecting data on the local ecological system and that he at some point is doing some statistical calculation using one of the $100 computers designed by the MIT Media Lab. Now, for some reason - perhaps while taking a nap—he leaves abruptly, leaving the computer unattended until its battery runs out of power. Shortly afterwards a group of bonobo chimpanzees comes along and finds it. Out of idle curiosity one of them turns the handle, winding up the device that is then suddenly re-activated and continues the operations it was performing before its battery ran out of power. Is the computer still doing *calculations*? The computer is still the same *thing* and it still behaves in the usual highly regular manner, but does it still serve as a *computer*?

What Wittgenstein was suggesting with his scenario is that the concept of calculation is of the normative category; it implies that the whoever does the calculation understands the rules of the calculus in question; that is, that the calculator has the ability to apply the rules and can justify the procedure and the result with reference to the rules.

However, it is not as clear-cut as this, for we also have the concept of somebody doing calculations 'mechanically'. Thus, in the following section, Wittgenstein introduced another, quite different, scenario involving a human computer:

> But is it not true that someone with no idea of the meaning of Russell's symbols could work over Russell's proofs? And so could in an important sense test whether they were right or wrong?
>
> A human calculating machine might be trained so that when the rules of inferences were shewn it and perhaps exemplified, it read through the proofs of a mathematical system (say that of Russell), and nodded its head after every correctly drawn conclusion, but shook its head at a mistake and stopped calculating. One could imagine this creature as otherwise perfectly imbecile. (Wittgenstein, 1937–1944, V §3)

The question implicitly suggested here is whether the human computer, an 'otherwise perfectly imbecile', can in fact be said to be making logical inferences, even when the 'human calculating machine' has no idea of the meaning of Russell's system?

The case of the misplaced calculator and that of the 'perfectly imbecile' 'human calculating machine' point to a serious muddle in our notions of calculation, computing, and mechanical procedures. Let us try to sort out some of the pertinent distinctions:

(i) *Ordinary (competent) calculation*: A human computer has learned the rules of a certain calculus and masters the concomitant methods and techniques. Having learned to follow the rules, the human computer is able to apply the rules appropriately, explain and justify his or her moves with reference to the rules, etc.

(ii) *Routine calculation*: Again, a human computer has learned the rules of a certain calculus and masters the pertinent methods and techniques. However, since the human computer has performed this kind of calculation often, he or she proceeds confidently, unhesitatingly, and without forethought. For that reason this is often referred to as 'mechanical calculation' and can of course be said to be 'mechanical' in as much as the human computer proceeds steadily and effortlessly, but, as Wittgenstein points out, it is not performed *mindlessly*:

> One follows the rule *mechanically*. Hence one compares it with a mechanism.
> "Mechanical"? That means: without thinking. But *entirely* without thinking? Without *reflecting*. (Wittgenstein, 1937–1944, VII §60)

The human computer masters the rules but does not necessarily have them present and may need a minute or two to recall the rules, if asked for an explanation, for example. But still, we would find it very strange indeed if the human computer would not be able to recognize a fault if one was found and pointed out to him.

(iii) *Distributed routine calculation*: As exemplified by the manufactures form of cooperative calculation work devised by de Prony, this is the prototypical case of human computing. For a certain task, an algorithm has been specified, and the constituent partial or 'atomic' operations have been 'divided' and allocated among multiple ‘human computers’, in a carefully calculated proportion and sequence. Although the cooperative effort as a whole may be organized according to the rules of a sophisticated algorithm, the partial operations of the individual human calculator may be extremely rudimentary and, as in de Prony’s example, reduced to filling in a predesigned spreadsheet by operations of addition and subtraction.

The work of the human computers may also in this case be characterized as ‘mechanical’; but again, the human computers do not perform their operations thoughtlessly but rather ‘without reflection’, as a matter of course, with a confidence rooted in routine. What is specific is that they, while being able to explain and justify (by invoking the rules) what they do individually, in their ‘atomic’ operations, nevertheless typically will be unable to account for the rules of the algorithm governing the cooperative effort in its totality.

This is then often turned into a claim that multitude ‘meaningless’ operations on an aggregate level may appear as complex calculation. But in making such an inference one forgets the algorithm carefully designed by de Prony and his fellow mathematicians. Anyway, there was nothing ‘meaningless’ about the work of the hairdressers: they were conscientiously following the rules of additions and subtraction as well as the protocol embedded in the prefabricated spreadsheet: ‘it makes no sense to speak of a “meaningless sub-rule”, simply because it is unintelligible to speak of following a rule—no matter how simple or complex—without *understanding* that rule’ (Shanker, 1987c, p. 89).

One can compare with someone who has learned to move chess pieces according to the rules governing their movement (pawns move forward, bishops move diagonally, etc.) but, perversely, has never been taught that it is *a game* and that it is about *winning* by trapping the opponent’s king. The imbecile player, or rather, chess piece mover, is able to move the pieces about in accordance with the rules but cannot be said to *play chess*: ‘the game, I should like to say, does not just have rules; it has a point’ (Wittgenstein, 1937–1944, I §20, cf. also III §85). Similarly, the hairdressers cannot be said to be calculating trigonometric tables, but one could certainly say that the entire ensemble, that is, the 80 hairdressers in their specific formation *together with* de Prony and his assistants, as an ensemble, certainly *were* calculating trigonometric tables.

(iv) *The machine-as-symbol*: Mathematicians conceive of what they do in terms of the ‘necessity’ and ‘inexorability’ of mathematical propositions once ‘proved’ and this notion of ‘necessary’ and ‘inexorable’ inferences is often expressed in metaphors such as ‘machinery’ and ‘mechanical’. There is a age-long tradition for using such metaphors to express the sense that the result of a calculation is somehow already there even before the calculation started. Wittgenstein’s discussion of this is instructive (Wittgenstein, 1939, pp. 196–199). Speaking ‘against the idea of a “logical machinery”’, he is arguing that ‘there is no such thing’. He gives as an example the use of an imaginary mechanism such as an idealized crank shaft in a geometrical proof (Fig. 13.1).

In this kind of proof one works out how the piston will move if the crankshaft is moved in a particular way, and in doing so, ‘One always assumes that the parts are perfectly rigid.—Now what is this? You might say, “What a queer assumption, since

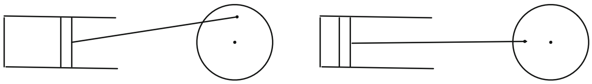

Fig. 13.1 Wittgenstein's 'kinematic' proof (after Wittgenstein, 1939, p. 195)

nothing is perfectly rigid." What is the criterion for rigidity? What do we assume when we assume the parts are rigid?', Wittgenstein asks and goes on:

> In kinematics we talk of a connecting rod—not meaning a rod made of steel or brass or what-not. We use the word "connecting rod" in ordinary life, but in kinematics we use it in quite a different way, although we say roughly the same things about it as we say about the real rod: that it goes forward and back, rotates, etc. But then the real rod contracts and expands, we say. What are we to say of this rod: does it contract and expand?—And so we say it can't. But the truth is that there is no question of it contracting or expanding. It is a *picture* of a connecting rod, a symbol used in this symbolism for a connecting rod. And in this symbolism there is nothing which corresponds to a contraction or expansion of the connecting rod.

The machine is not a machine but a symbolic machine that stands as a symbol for a certain mathematical operation:

> If we talk of a logical machinery, we are using the idea of a machinery to explain a certain thing happening in time. When we think of a logical machinery explaining logical necessity, then we have a peculiar idea of the parts of the logical machinery—an idea which makes logical necessity much more necessary than other kinds of necessity. If we were comparing the logical machinery with the machinery of a watch, one might say that the logical machinery is made of parts which cannot be bent. They are made of infinitely hard material—and so one gets an infinitely hard necessity. (Wittgenstein, 1939, p. 196)

When mathematicians use terms such as 'machine', 'mechanism', and 'mechanical' they are talking about a 'picture', a 'symbol': 'if I say that there is no such thing as the super-rigidity of logic, the real point is to explain where this idea of super-rigidity comes from—to show that the idea of *super-rigidity* does *not* come from the same source which the idea of *rigidity* comes from'. 'It seems as if we had got hold of a hardness which we have never experienced'. Like the idea of 'the inexorability or absolute hardness of logic' the idea of 'super-hardness' or 'super-rigidity' of the 'machine-as-symbol' is a *strangely inverted* expression of the normative nature of mathematics: the parts of the machine are super-hard because they are *not* part of any material machine but are rules we do not question simply *because they are rules* and are very carefully integrated and connected to and integrated in the grammar of our number words:

> It isn't a machine which might be explored with unexpected results, a machine which might achieve something that couldn't be read off from it. That is, the way it works is *logical*, it's quite different from the way a machine works. *Qua* thought, it contains nothing more than was put into it. As a machine functioning causally, it might be believed capable of anything; but in logic we get out of it only what we meant by it. (Wittgenstein, 1931–1934, p. 247)

In sum, Turing's machine anno 1936 is not a machine but a *symbolic* machine, that is, a calculus. 'If calculating looks to us like the action of a machine, it is *the* *human being* doing the calculation that is the machine' (Wittgenstein, 1937–1944, IV §20). We accept the rules as incontestable and let our calculations be guided by the rules: by doing so, we rely on the rules to lead us to correct results, just like we rely on the ruler to guide us to draw a straight line. It is *this* that in mathematical logic is meant by 'mechanical procedure'.

(v) *Machine calculation*: Now, here 'machine' (and 'mechanical', etc.) means something categorially different from the mathematician's 'machine-as-symbol', namely, the use of a *causal process* (classical mechanics, hydraulics, electromechanical, analog electronics, digital electronics, software machines), that has been carefully devised to operate in an *extremely regular* manner (behaving uniformly, dependably) and thus to transform specific input into specific output according to some pattern with extreme degrees of probability. Although an electronic computer operates in a highly *regular* manner, it is unintelligible to say that it *follows* rules when operating, when, for example, performing calculations; it is a transgression of the logical boundaries demarcating grammatically distinct categories to ascribe rules and rule-following to a machine: in short, it is a transgression of sense.

What *does* make sense is to say that *we* make the machine behave *as if* it follows rules and perform calculations, in the same way (only much faster) as we might use a mechanical calculator to do the same. Or better, it is *we who follow rules* by, in part, using mechanical calculators or digital computers. And it is *we* who use the machine to produce certain transformations of physical patterns that, within a certain domain of practice, *can* be taken as calculations, representations, letters, plans, etc. and are *routinely taken* as such. A computational artifact is a computational artifacts only *within a practice* of 'computation' that, like other forms of rule-following, is a *normative* activity, whereas the behavior of a computational artifact is the manifestation of causal processes. Now, computation by means of electronic computers is certainly different from computation by means of paper and pen, by means of using slide rulers or abacuses, and so on. Computational artifacts are *machines*: they operate more or less automatically. That is to say, they undergo highly dependable, causally regulated state changes without human intervention (at least for a period of time). But computational artifacts are still a technique of computation *by virtue of* being used by practitioners in their normatively defined practices.

The concept of 'mechanical' performance is obviously not a sharply defined concept. It is a concept developed over centuries by millwrights, blacksmiths, machinists, engineers, and—lately—mathematicians and computer scientists.[5] In short, it is a concept *with a history*, and a rather mixed one at that. So it may be warranted to call it a family-resemblance concept. But if so, it is a family-resemblance concept deeply divided by a categorial split, encompassing concepts in disparate grammatical domains. It is noteworthy, however, that our language has a large number of words that we use to make far more fine-grained distinctions, if not always

[5]It does not help that the terms 'machine' and 'mechanical' have been adopted by sociologists as a metaphor to characterize certain organizational forms such as, for example, the cold indifference and imperviousness of state bureaucracies: the 'mechanical efficiency' that 'reduces every worker to a cog in this machine' (Weber, 1909, p. 127).

very sharply or consistently: rule *formulation* in the form of prose, lists, structured English, blueprints, etc.; rule *codification*, i.e., rules arranged according to a specific scheme; rule *formalization*, i.e., rules encoded in a notation (a finite set of symbols); software *coding*, i.e., the formalized rules expressed in the calculus of a programming language; and *implementation*, i.e., incorporation of the code on a specific platform, taking into account the specific instruction set, word length, memory constraints, etc. That is, it is not so that the distinctions are not made by practitioners but different practitioners of different ilk make the distinctions differently. This is as it always is, and normally causes no harm, as these different uses typically co-exist without intersecting. The problem arises when practitioners—and philosophizing theorists—make general propositions about 'mechanical' with no regard for the conceptual multiplicity.

In much of the literature on the theory of computation, even among the most notable scholars, these different uses of the notion of 'mechanical' are thrown together in the most primitive manner. Take, for example, Hao Wang's discussion of Turing's contribution to our understanding of algorithms and mechanical calculation. He started by defining the concept of algorithm as 'a finite set of rules which tell us, from moment to moment, precisely what to do with regard to a given class of problems', and then, to illustrate the concept of algorithm, introduced the ubiquitous schoolboy as an example of a human calculator: 'a schoolboy can learn the Euclidian algorithm correctly without knowing why it gives the desired result' (Wang, 1974, p. 90). This argument is a central tenet in the philosophy of computing—but also deeply confused. There is, in Shanker's words, 'something profoundly out of focus' in this last sentence: 'the thoughts which it contains are pulling in opposite directions, crediting the schoolboy with the very piece of knowledge which is immediately denied him' (Shanker, 1986c, p. 22). Shanker then elaborates his objections:

> to learn to apply an algorithm correctly involves more than merely producing the "right" results. In order to say of the schoolboy that he has learnt—grasped—the rule, we will demand more than simply the set of his results to justify such a judgment. The criteria for crediting someone with the mastery of a rule are more complex than this; we place it against the background of his explaining, justifying, correcting, answering certain questions, etc. We would no doubt be very puzzled by the case of a schoolboy who could invariably give the "right" result for an algorithm and yet could not provide us with absolutely no information about how or why he had derived that result. (Shanker, 1986c, p. 22)

The source of the confusion is here that Wang, like so many other philosophers of mathematics and logic, confused the *mastery of the steps* of the algorithm with *mastery of the algorithm*. In the words of Shanker, 'All that we could rightly say in such a situation is that the schoolboy has learned a series of (for him) independent rules; but to learn how to apply each of these sub-rules does not amount to learning the algorithm' (Shanker, 1986c, p. 22). The confusion is so much more remarkable as Wang, immediately after having first credited and then debited the schoolboy with knowledge of the algorithm, commented that: 'In practice, when a man calculates, he also designs a small algorithm on the way. But to simplify matters, we may say that the designing activity is no longer a part of his calculating activity.' (Wang, 1974, p. 90). But in 'simplifying' matters this way Wang made himself guilty of setting up a thought experiment in which an algorithm is part of the experimental setup—and then surreptitiously removing the algorithm from his interpretation. This is hardly 'simplification' but rather obfuscation. Like Turing (and most mathematical logicians in general) Wang simply took for granted that routine ('noncreative') calculation is of the same category as a causal process performed by a machine. He did so quite explicitly by saying that what makes a process 'mechanical' is that it is 'capable of being performed by a machine, or by a man in a mechanical (noncreative) way' (ibid., p. 91). Wang here made the same transgression of logical boundaries as Turing did when he surreptitiously slipped from his 'mechanical' account of the human computer's work to devising 'a machine to do the work of this computer' *in exactly the same language*: 'What this means is that whenever the operations of a system or organism can be mapped onto an algorithm, there is no logical obstacle to the transfer of that algorithm to the system or organism itself' (Shanker, 1987b, p. 39).

Turing (and Wang and a host of other philosophers of mathematics, logic, and computing) are flickering between the *normative concept* of following a rule and the associated notions of correctness and justification etc., and the *causal concept* of machinery and mechanical. Although it 'is unintelligible to speak of a machine following a rule' (Shanker, 1987c, p. 89), a 'non-normative sense of following rules' has been introduced (Shanker, 1987b, p. 39). The upshot is a transgression of the boundaries of sense, which has created an intellectual muddle with detrimental consequences.

It is important to emphasize that their fault is not the notion that software machines can perform sophisticated operations and can do so in flexible ways hitherto unthinkable. Of course not: CSCW is predicated on those notions and the technologies that have been built on them. The issue is neither that software machines can be integrated in (normatively defined) practices so as to automatically regulate aspects of action and interaction. Of course not: this is an observable fact. In fact, strictly speaking, the issue is not even that computer scientists and logicians, in the natural attitude of their own habitat, resort to language that, outside of their domain, can be fatally inaccurate, ambiguous, misleading ('programming language', 'error', 'rule', 'calculation', etc.). It is clear that when computer scientists are engaged in programming, discussing programs, debating the complexity of an algorithm—then such usage is of course completely legitimate. It is simply a kind of shorthand. However, just as the 'talk of mathematicians becomes absurd when they leave mathematics' (Wittgenstein, 1932–1933, p. 225), the issue arises when computer scientists (and journalists!) bring these professional metaphors into ordinary language. *Then* a bonfire of metaphysics erupts. The issue is that they, having ventured outside of their respective fields and started philosophizing about 'a machine following a rule', degenerate to sheer unintelligible nonsense. This is the real issue. By transgressing the logical boundaries governing the use of concepts of disparate grammatical domains, Turing and his followers have created a conceptual imbroglio in which investigations of computing technologies in and for actual work practices are gravely handicapped. This concept of computational artifact, as received from the philosophy of mathematical logic, is confused and this leads us to ascribe causal characteristics to normative actions while others respond that actions are not causally determined and hence not rule-governed. This leaves no logical space for CSCW.

2 Room for CSCW

The point of all this is *not* to say that CSCW's research program is centered on the concept of computation, nor even that CSCW is or should be focused on dispelling the metaphysical fog in which the concepts of computing and computational artifact are still shrouded. The point of all this is *rather* to clear the construction site of the piles of debris and rubble that prevent us from proceeding and from even seeing the challenge and identifying the possibilities and issues. For CSCW to progress and even exist as an intellectually respectable research area requires that fundamental concepts such as 'computation', 'formalization', 'computational artifact', 'mechanical procedure', 'mechanization', 'causal process', 'regularity', 'rule following', etc. become surveyable, and, *a fortiori*, that we develop a differentiated conceptualization of the ways in which such techniques and practices function and develop. Without clarity on these issues CSCW will remain a degenerative research program, caught up in endless conceptual confusion and unprincipled design experiments.

The issues of the concept of computation and plans and computational artifacts are critically important but are not focal issues for CSCW. They are inevitable issues, not as the subject matter of CSCW, but in the sense that their clarification is the precondition for creating the 'logical space' in which CSCW research is intellectually legitimate.

CSCW's research program is centered on the issues of developing computing technologies that enable workers engaged in cooperative work to regulate their interdependent and yet distributed activities effectively, efficiently, dependably, timely, unhurriedly, with dignity, etc. and accordingly, *at the same time*, enable them to control, specify, adapt, modify, etc. the behavior of the regulating computational artifacts.

The point of clarifying and emphasizing that 'it is *we* who are executing the instructions, albeit with the aid of this sophisticated electro-mechanical tool' (Shanker, 1987c, p. 82), is to let the buzzing fly out of the bottle—to show that there is no *conceptual* obstructions (notions of rational action as '*essentially ad hoc*' or of 'rule following machinery') prohibiting CSCW from engaging in the development of coordination technologies: the technologies of focal interest to CSCW.

With classic machinery, be it a 'Spinning Jenny' or a CNC machining station or a digital photo-editing application like Photoshop, the worker interposes, in Marx's words, a 'natural process [. . .] as a means between himself and inorganic nature'. By contrast, with coordination technologies we are interposing a 'natural process', not as a means between us and the work piece—but as a regulating causal process into the web of our cooperative work relationships. What characterizes coordination technologies is that *causal processes* are specifically devised and employed as techniques of *coordinative practices*. That is, with coordination technologies, coordinative practices, the *normative* constructs of coordinative conduct such as plans, schedules, procedures, schemes, etc., are enacted and imposed (to some extent and in some respects) by means of *causally* unfolding processes as embodied by computational artifacts. This is not a paradox or a contradiction; it is simply a tension to be explored and mastered and thereby overcome. The fundamental problem of CSCW is defined by this preliminary tension: the design of (causal) mechanisms for (normative) practices.

What is new is that we are beginning to construct devices that we use in our coordinative practices—to regulate our interactions. Probably the first example of this kind of technique is the mechanical clock in the town hall towers of medieval Europe that regulated the life of the town. In the words of Lewis Mumford: 'the regular striking of the bells brought a new regularity into the life of the workman and the merchant. The bells of the clock tower almost defined urban existence' (Mumford, 1934, p. 14). The clock provided medieval townspeople with a criterion for determining what was the correct time (e.g., 'morning mass', 'bank hour') under given circumstances, in the form of a commonly accepted and publicly visible metric of time. And the townspeople would typically let their activities be guided by the hands of the clock, or by the sound of its bell, or they might ignore it, if it was of no consequence or if they for some reason had other plans for the day. Now, as mentioned earlier, the development of railroad operations required time-critical coordination on a large scale which in turn motivated the development and deployment of telegraph communication systems and furthermore the establishment of national time measurement conventions and the 'galvanic' system of automatic synchronization of time keepers. Like the town-hall clock, the latter system, the system of automatic synchronization, brought 'a new regularity' into the life of not just of townspeople within earshot but the lives of millions, in the UK and beyond. The (normative) standard method of time measurement (based on Greenwich time) was made effective on such a large scale by the highly regular periodic dissemination of electric pulses over the telegraph network. It was not the causal propagation of the electric pulse that forced railroad workers, travellers, post office clerks, watchmakers, etc. to synchronize their daily conduct but it was these people who followed the rule of standard time measurement by taking note of and adjusting to the time as read from their electromagnetically regularized timekeepers.

Now, both the town hall clock and the nationally synchronized clocks of course represented very simple coordination technologies: the clock has two states; it either sounds or it does not, and more sophisticated designs soon had the number of strikes correspond to the time of day. The coordination technologies we now have available are of course integrated into our work practices in significantly more sophisticated ways. A computational calendar system may issue notifications of up-coming tasks and appointments and may be used for calculating and identifying possible time slots for meetings under consideration, etc. Similarly, workflow management systems are used not only for routing documents, case folders, etc, through a system of division of labor, according to case types, formal roles, schedules, sequential dependencies, etc. but may be used also for keeping track of deadlines and for issuing reminders.

The point I want to make is that the problem with computational artifacts is not a principled one. Just as the ruler does not 'in any strong sense' make me, i.e., *cause* me to, draw a straight line but I who exploit its material properties (stiffness, geometry) to draw what is conventionally considered a straight line, it is we who follow rules by using computational artifacts. There is no conceptual problem in this, and the state of *angst* should be called off. The problems that we *do* have with using machines in our rule-based practices is a practical one: it is *a problem of cost*, namely, the cost (effort, time, resources, risk, etc.) of construction and modification of such artifacts.

3 The Practical Inexorability of CSCW

With electronic stored-program computing (equipped with high-level programming languages, code libraries, editors, compilers, interpreters, and what have you), the construction of machinery has become immensely inexpensive. The cost of modifying such machines is negligible compared to previous technologies. Consequently, over the last couple of decades it has become economically feasible to construct vast machine systems, even global ones.

Already in the case of the mechanical machine systems of the nineteenth century, such as printing presses and process industries, workers cooperated through causal processes and the coordination of their local tasks were to a large extent preordained by the structure of the machinery. But their ability to change the protocols embodied in the machine systems were practically non-existent. At best, the only means of coordination available to them during operations were monitoring of bodily conduct, shouting, conversations (cf. the description of the hot rolling mill in Popitz et al., 1957).

The stored-program architecture that made computing a practically universal control technology has reduced the cost of constructing machinery. And propelled by steady advances in semiconductor technology since the invention of the integrated circuit and especially the microprocessor, the development of computing technology is steadily eroding the cost of constructing and modifying software machinery.

As a result, the very concept of 'automation' has been transformed. Until quite recently the concept of automation remained identical to the concept of automation developed by Ure and Marx, namely a concept of a technical system that, with only occasional human intervention, provides virtually immutable operational continuity on a large scale, such as power looms, transfer lines, paper mills, nuclear power plants, oil refineries, etc. (cf., e.g. Diebold, 1952; Piel et al., 1955; Blauner, 1964; Pollock, 1964; Luke, 1972). With software machinery, this notion of 'wall-to-wall' automation has been superseded by the interactive-computing concept of fine-grained and highly reactive automatic control mechanisms (e.g., 'word wrap') that can interoperate in flexible ways. Computing technologies has provided the technical means for the integration of myriads of automatic processes. Ironically, this means *that large-scale machine systems are becoming ubiquitous*. Automatic control mechanisms can be deployed in domains and for activities for which classic 'wall-to-wall' automation was not feasible at all, simply because production conditions had been too uncertain or variable for it to have been economically viable. With software control mechanisms, making machines interconnect and form large-scale systems is inexpensive and effortless, compared to connecting by rods and gears, as it can be done by transmitting data or code between software machines. Thus, computing technologies, combined with network technologies, has made the construction of the vast machine systems (ERP systems, CAD/CAM systems, financial transaction systems, mobile phone systems) a viable option.

Machine systems are or are becoming a dominant technology in all domains of work: in manufacturing and mining; in transportation by air, sea, rail, or road; in construction; in administrative work such as accounting, trading, banking, and insurance; in retail trade (supermarkets); in hospital work; in architectural design, engineering design, and programming; in experimental science; in newspaper production, radio and television program production and broadcasting, and increasingly also in movie production. Consequently, cooperative work is increasingly carried out in and through the artificial causal processes constituted by computational machine systems and the realm of ordinary work correspondingly characterized by increasingly widely ramified cooperative work relationships.

On the other hand, and again by virtue of the stored-program architecture, computing technologies also provide the general basis for constructing, at another level of abstraction, *coordination technologies* that facilitate fine-grained automatic control of routine interdependencies.

Not only does computer technology offer the means of control mechanisms that are physically cleanly separated from the mechanisms of transforming, converting, and transmitting energy, and not only does computing technology offers the means of control mechanisms to interconnect machine systems; it also, by the same token, affords *the logical and physical separation of mechanisms for the regulation of interdependent cooperative activities from the mechanisms of controlling the various productive processes*, be they processes of fabrication, transportation, etc. or processes of accounting, financial transactions, etc. That is, it affords the emergence of coordination technologies as a distinct class of technologies, separate from those of machine systems, as it is becoming technically and economically feasible for ordinary workers to construct and modify the control systems that mediate and regulate their cooperative activities.

The concept of the distinct control mechanism is again crucial. In coordination technologies such as workflow systems, scheduling systems, etc., the computational protocols regulating the coordination of interdependent activities are—can be, or rather: *should be*—technically separate from the procedures of the 'first order' machine systems (calculating aircraft vectors or passenger lists, controlling production machinery, etc.). It is therefore technically feasible to modify the computational coordinative protocols at very low costs. In other words, interactive computing technologies make it *technically and economically* feasible even for ordinary workers to devise, adapt, modify the computational protocols of their coordinative practices.

Like the town hall clock, the computational artifact does not *cause* practitioners to behave in a certain way. Just like the medieval townspeople might let their activities guide by the hands of the clock, or by the sound of its bell, workers today may let computational artifacts guide their interactions. Or, like the medieval townspeople, workers may disregard the presumptive mechanically issued command and decide that they have reasons—valid reasons—not to act on the signals produced by the artifact.

Again, the problem is not conceptual; it is not principled; it is practical. The challenge is to devise technologies that allow ordinary workers to construct and modify computational artifacts for regulating their coordinative practices and to do so with minimal effort. To do this, however, requires that we understand how ordinary coordinative artifacts *are* being constructed, negotiated, adopted, amended, combined and recombined. Is there a pattern, a logic even, to the way they are constructed, put together, combined and recombined, and so on? To address those questions systematically requires that we overcome our intellectual paralysis.

One may think of CSCW as a fad; it certainly has had its share of folly. And one may find that it has had it time. But the research problem that prompted the formation of CSCW 25 years ago, was not an arbitrary construction, incidentally devised; it is a problem that emerges again and again in the practical development and application of software machinery in cooperative work settings, in the design of workflow systems and 'business-process execution languages', in building computational taxonomies or 'ontologies' for scientific work, in building models of product families, etc. It arises from the issues involved in interposing causal processes as regulatory mechanisms in and for cooperative work relations. That is, the CSCW research program arises from the organization of modern work, from the incessant attempts to make the coordination of interdependent activities more efficient, reliable, flexible, etc. by means of computational artifacts.

References

Abbate, Janet Ellen: *Inventing the Internet*, The MIT Press, Cambridge, Mass., and London, 1999. (Paperback ed., 2000).

Abbott, Kenneth R.; and Sunil K. Sarin: 'Experiences with workflow management: Issues for the next generation', in J. B. Smith; F. D. Smith; and T. W. Malone (eds.): *CSCW'94: Proceedings of the Conference on Computer-Supported Cooperative Work*, *24–26 October 1994*, *Chapel Hill*, *North Carolina*, ACM Press, New York, 1994, pp. 113–120.

Académie Royale des Sciences: *Histoire de l'Académie royale des sciences, M.DC.XCIX [1699], avec les mémoires de mathématique & de physique, tirez des registres de cette Académie*, vol. 1, Boudot, Paris, 1702. – Gabriel Martin etc., Paris, 1732.

Académie Royale des Sciences: *Histoire de l'Académie royale des sciences depuis son etablissement en 1666 jusqu'en 1699*, vol. 1–13, Compagnie des libraires, Paris, 1729–1734. – Gabriel Martin, etc., Paris, 1732.

Académie Royale des Sciences: *L'art du meunier, du boulanger, du vermicellier*. Text ed. by J.-E. Bertrand. Société typographique. Neuchatel, 1761. (*Descriptions des arts et métiers, faites ou approuvées par Messieurs de l'Académie royale des sciences de Paris*, vol. 1). (2nd ed., 1771).

Académie Royale des Sciences: *Descriptions des arts et métiers, faites ou approuvées par Messieurs de l'Académie royale des sciences de Paris*, vol. 1–20, Société typographique, Neuchatel, 1771–1783. Text ed. by J.-E. Bertrand. (Nouvelle édition publiée avec des observations, & augmentée de tout ce qui a été écrit de mieux sur ces matières, an Allemagne, en Angleterre, en Suisse, en Italie).

Ackerman, Mark S.; and Thomas W. Malone: 'Answer Garden: A tool for growing organizational memory', in: *Proceedings of the Conference on Office Information Systems*, *April 1990, Cambridge, Mass.,* ACM, New York, 1990, pp. 31–39.

Ackerman, Mark S.: 'Augmenting the organizational memory: A field study of Answer Garden', in J. B. Smith; F. D. Smith; and T. W. Malone (eds.): *CSCW'94: Proceedings of the Conference on Computer-Supported Cooperative Work*, *24–26 October 1994, Chapel Hill, North Carolina,* ACM Press, New York, 1994, pp. 243–252.

Agre, Philip E.: [Review of] L. A. Suchman: *Plans and Situated Actions* (Cambridge University Press, Cambridge, 1987), *Artificial Intelligence*, vol. 43, 1990, pp. 369–384.

Agricola, Georgius: *De Re Metallica*, 1556. Transl. from *De Re Metallica*, Froben 1556. Transl. by H. C. Hoover and L. H. Hoover. – Dover Publications, New York, 1950.

Allport, Alan: 'Attention and control: Have we been asking the wrong questions? A critical review of twenty-five years', in D. E. Meyer and S. Kornblum (eds.): *Attention and Performance XIV: Synergies in Experimental Psychology, Artificial Intelligence, and Cognitive Neuroscience*, MIT Press, Cambridge, Mass., 1993, pp. 183–218.

Amber, George H.; and Paul S. Amber: *Anatomy of Automation*, Prentice-Hall, Englewood-Cliffs, New Jersey, 1962.

K. Schmidt, *Cooperative Work and Coordinative Practices*, Computer Supported Cooperative Work, DOI 10.1007/978-1-84800-068-1,

Andersen, Hans H. K.: 'Classification schemes: Supporting articulation work in technical documentation', in H. Albrechtsen (ed.): *ISKO'94: Knowledge Organisation and Quality Management, 21–24 June 1994*, *Copenhagen, Denmark,* 1994a.

Andersen, Hans H. K.: 'The construction note', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994b, pp. 161–186. COMIC Deliverable 3.2. ftp://ftp.comp.lancs.ac.uk/pub/comic

Andersen, Hans H. K.: *Cooperative Documentation Production in Engineering Design: The 'Mechanism of Interaction' Perspective*, PhD dissertation, Risø National Laboratory, Roskilde, Denmark, June 1997.

Andersen, Niels E., et al.: *Professional Systems Development: Experience, Ideas, and Action*, Prentice-Hall, Englewood-Cliffs, New Jersey, 1990.

Anderson, Robert J.; Wes W. Sharrock; and John A. Hughes: 'The division of labour', (*Action Analysis and Conversation Analysis, Maison des Sciences de l'Homme, Paris, September 1987*). In: Réseaux, 1990, Hors Série 8, no. 2, pp. 237–252.

Anderson, Robert J.: 'Representations and requirements: The value of ethnography in system design', *Human-Computer Interaction*, vol. 9, Lawrence Erlbaum, Hillsdale, New York, 1994, pp. 151–182.

Anderson, Robert J.: 'Work, ethnography, and system design', in A. Kent and J. G. Williams (eds.): *The Encyclopedia of Microcomputing*, vol. 20, Marcel Dekker, New York, 1997, pp. 159–183.

Aoki, Masahiko: *A New Paradigm of Work Organization: The Japanese Experience*, vol. 36, World Institute for Development Economics Research, Helsinki, 1988.

Appelt, Wolfgang: 'WWW based collaboration with the BSCW system', *Lecture Notes in Computer Science*, vol. 1725, 1999, pp. 66–78.

Appelt, Wolfgang: 'What groupware functionality do users really use? Analysis of the usage of the BSCW system', in K. Klöckner (ed.): *PDP 2001: Proceedings of the Ninth Euromicro Workshop on Parallel and Distributed Processing*, *7–9 February 2001, Mantova*, IEEE Computer Society Press, Los Alamitos, 2001, pp. 337–341.

Arden, Bruce W. (ed.): *What Can Be Automated? The Computer Science and Engineering Research Study (COSERS)*, The MIT Press, Cambridge, Mass., and London, 1980.

Aristotle: *Nicomachean Ethics* (*c.* 334-322 BCE-a). Transl. by W. D. Ross and J. O. Urmson. In J. Barnes (ed.): *The Complete Works of Aristotle*, vol. 2, Princeton University Press, Princeton, New Jersey, 1984, pp. 1729–1867.

Aristotle: *Metaphysics* (*c.* 334–322 BCE-b). Transl. by W. D. Ross. In J. Barnes (ed.): *The Complete Works of Aristotle*, vol. 2, Princeton University Press, Princeton, New Jersey, 1984, pp. 1552–1728.

Aristotle: *Politics* (*c.* 334–322 BCE-c). Transl. by B. Jowett. In J. Barnes (ed.): *The Complete Works of Aristotle*, vol. 2, Princeton University Press, Princeton, New Jersey, 1984, pp. 1986–2129.

Aspray, William: 'The Institute for Advanced Study computer: A case study in the application of concepts from the history of technology', in R. Rojas and U. Hashagen (eds.): *The First Computers: History and Architectures*, The MIT Press, Cambridge, Mass., and London, 2000, pp. 179–194.

Atkins, Daniel E., et al.: *Revolutionizing Science and Engineering Through Cyberinfrastructure: Report of the National Science Foundation Blue-Ribbon Advisory Panel on Cyber-infrastructure*, National Science Foundation, Washington, DC, January 2003. http://www.nsf.gov/cise/sci/reports/atkins.pdf

Augarten, Stan: *Bit by Bit: An Illustrated History of Computers*, George Allen & Unwin, London, 1984.

Austin, John Longshaw: *Philosophical Papers*, Oxford University Press, Oxford, 1961. Text ed. by J. O. Urmson and G. J. Warnock. (3rd ed., 1979).

Babbage, Charles: *A Letter to Sir Humphry Davy, Bart., President of The Royal Society, etc. etc. on the Application of Machinery to the Purpose of Calculating and Printing Mathematical Tables* (London, 3 July 1822). Text ed. by P. Morrison and E. Morrison. In C. Babbage: *On* *the Principles and Development of the Calculator, and Other Seminal Writings by Charles Babbage and Others*. Dover Publications, New York, 1961, pp. 298–305.

Babbage, Charles: *On the Economy of Machinery and Manufactures*, Charles Knight, London, 1832. (4th ed., 1835). – Augustus M. Kelley, Publishers, Fairfield, New Jersey, 1986.

Babbage, Charles: *Passages from the Life of a Philosopher*, Longman, etc., London, 1864. Text ed. by M. Campbell-Kelly. – William Pickering, London, 1994.

Bacon, Frances: *The New Organon: Or True Directions Concerning the Interpretation of Nature* (1620). Transl. from *Novum Organum, Sive Vera de Interpretatione Naturae*. Transl. by J. Spedding; R. L. Ellis; and D. D. Heath. In F. Bacon: *The Works*. Taggard and Thompson, Boston, 1863.

Bahrdt, Hans Paul: *Industriebürokratie: Versuch einer Soziologie des Industrialisierten Bürobetriebes und seiner Angestellten*, Ferdinand Enke Verlag, Stuttgart, 1958.

Bahrdt, Hans Paul: 'Die Krise der Hierarchie im Wandel der Kooperationsformen', in: *Verhandlungen des 14. deutschen Soziologentages, Stuttgart 1959*, Deutsche Gesellschaft für Soziologie, 1959, pp. 113–121. – Reprinted in Renate Mayntz (ed.): *Bürokratische Organisation*, Kiepenheuer & Witsch, Köln-Berlin, 1971, pp. 127–134.

Baker, Gordon P.; and Peter M. S. Hacker: *Scepticism, Rules and Language*, Basil Blackwell, Oxford, 1984.

Baker, Gordon P.; and Peter M. S. Hacker: *Wittgenstein: Rules, Grammar, and Necessity: Essays and Exegesis of §§ 185–242*. Basil Blackwell, Oxford, 2009. (G. P. Baker and P. M. S. Hacker: *An Analytical Commentary on the Philosophical Investigations*, vol. 2). (2nd rev. ed.; 1st ed., 1985).

Bannon, Liam J.: 'Computer-Mediated Communication', in D. A. Norman and S. W. Draper (eds.): *User Centered System Design*, Lawrence Erlbaum, Hillsdale, New Jersey, 1986, pp. 433–452.

Bannon, Liam J.; Niels Bjørn-Andersen; and Benedicte Due-Thomsen: 'Computer support for cooperative work: An appraisal and critique', in: *Eurinfo'88: Proceedings of the First European Conference on Information Technology for Organisational Systems*, *16–20 May 1988, Athens, Greece,* 1988.

Bannon, Liam J.; and Kjeld Schmidt: 'CSCW: Four characters in search of a context', in P. Wilson; J. M. Bowers; and S. D. Benford (eds.): *ECSCW'89: Proceedings of the First European Conference on Computer Supported Cooperative Work*, *13–15 September 1989, Gatwick, London,* 1989, pp. 358–372.

Bannon, Liam J.; and Susanne Bødker: 'Beyond the interface: Encountering artifacs in use', in J. M. Carroll (ed.): *Designing Interaction. Psychology at the Human-Computer Interface*, Cambridge University Press, Cambridge, 1991, pp. 227–253.

Bannon, Liam J.; and Susanne Bødker: 'Constructing common information spaces', in J. A. Hughes, et al. (eds.): *ECSCW'97: Proceedings of the Fifth European Conference on Computer-Supported Cooperative Work*, *7–11 September 1997, Lancaster,* Kluwer Academic Publishers, Dordrecht, 1997, pp. 81–96.

Barber, Gerald R.; and Carl Hewitt: 'Foundations for office semantics', in N. Naffah (ed.): *Office Information Systems*, INRIA/North-Holland, Amsterdam, 1982, pp. 363–382.

Barber, Gerald R.: 'Supporting organizational problem solving with a work station', *ACM Transactions on Office Information Systems*, vol. 1, no. 1, January 1983, pp. 45–67.

Barber, Gerald R.; Peter de Jong; and Carl Hewitt: 'Semantic support for work in organizations', in R. E. A. Mason (ed.): *Information Processing '83: Proceedings of the IFIP Ninth World Computer Congress*, *19–23 September 1983, Paris,* North-Holland, Amsterdam, 1983, pp. 561–566.

Bardini, Thierry: *Bootstrapping: Douglas Engelbart, Coevolution, and the Origins of Personal Computing*, Stanford Research Institute, Stanford, Calif., 2000.

Bardram, Jakob E.: 'Plans as situated action: An activity theory approach to workflow systems', in J. A. Hughes, et al. (eds.): *ECSCW'97: Proceedings of the Fifth European Conference on Computer-Supported Cooperative Work*, *7–11 September 1997, Lancaster,* Kluwer Academic Publishers, Dordrecht, 1997, pp. 17–32.

Barnard, Chester I.: *The Functions of the Executive*, Harvard University Press, Cambridge, Mass., 1938.

Barnes, Susan B.: *Computer-Mediated Communication: Human-to-Human Communication Across the Internet*, Allyn and Bacon, Boston, etc., 2003.

Basalla, George: *The Evolution of Technology*, Cambridge University Press, Cambridge, 1988.

Baum, Claude: *The System Builders: The Story of SDC*, System Development Corporation, Santa Monica, Calif., 1981.

Beckert, Manfred: *Johann Beckmann*, B. G. Teubner, Leipzig, 1983.

Beckmann, Johann: *Politisch-Oekonomische Bibliothek worinn von den neusten Büchern, welche die Naturgeschichte, Naturlehre und die Land- und Stadtwithschaft betreffen, zuverlässige und vollständige Nachrichten ertheilt werden*, Vandenhoeck und Ruprecht, Göttingen, 1772.

Beckmann, Johann: *Anleitung zur Technologie, oder zur Kenntniß der Handwerke, Fabriken und Manufacturen, vornehmlich derer, welche mit der Landwirthschaft, Polizey und Cameralwissenschaft in nächster Verbindung stehn; nebst Beyträgen zur Kunstgeschichte*, Vandenhoeck und Ruprecht, Göttingen, 1777. (5th ed., 1802).

Bell, C. Gordon: 'Toward a history of (personal) workstations', in A. Goldberg (ed.): *A History of Personal Workstations*, Addison-Wesley, Reading, Mass., 1988, pp. 4–47.

Benford, Steven D.: 'A data model for the AMIGO communication environment', in R. Speth (ed.): *EUTECO '88: European Teleinformatics Conference on Research into Networks and Distributed Applications, 20–22 April 1988, Vienna, Austria. Organized by the European Action in Teleinformatics COST 11ter*, North-Holland, Brussels and Luxemburg, 1988, pp. 97–109.

Benford, Steven D.; and Lennart Fahlén: 'A spatial model of interaction large virtual environments', in G. De Michelis; C. Simone; and K. Schmidt (eds.): *ECSCW'93: Proceedings of the Third European Conference on Computer-Supported Cooperative Work, 13–17 September 1993, Milano*, Kluwer Academic Publishers, Dordrecht, 1993, pp. 109–124.

Benford, Steven D., et al.: 'Managing mutual awareness in collaborative virtual environments', in G. Singh; S. K. Feiner; and D. Thalmann (eds.): *VRST'94: Proceedings of the ACM SIGCHI Conference on Virtual Reality and Technology, 23–26 August 1994, Singapore*, ACM Press, New York, 1994a, pp. 223–236.

Benford, Steven D.; Lennart E. Fahlén; and John M. Bowers: 'Supporting social communication skills in multi-actor artificial realities', in: *ICAT'94: Proceedings of Fourth International Conference on Artificial Reality and Tele-Existence, 14–15 July 1994, Nikkei Hall, Otemachi, Tokyo,* 1994b, pp. 205–228. <ic-at/papers/list.html>

Benford, Steven D.; and Chris Greenhalgh: 'Introducing third party objects into the spatial model of interaction', in J. A. Hughes, et al. (eds.): *ECSCW'97: Proceedings of the Fifth European Conference on Computer-Supported Cooperative Work, 7–11 September 1997, Lancaster,* Kluwer Academic Publishers, Dordrecht, 1997, pp. 189–204.

Bennett, Stuart: *A History of Control Engineering, 1800–1930*, Peter Peregrinus, London, 1979. (Paperback ed., 1986).

Bennett, Stuart: *A History of Control Engineering, 1930–1955*, Peter Peregrinus, London, 1993.

Bentley, Richard; and Paul Dourish: 'Medium versus mechanism: Supporting collaboration through customization', in H. Marmolin; Y. Sundblad; and K. Schmidt (eds.): *ECSCW'95: Proceedings of the Fourth European Conference on Computer-Supported Cooperative Work, 10–14 September 1995, Stockholm,* Kluwer Academic Publishers, Dordrecht, 1995, pp. 133–148.

Bentley, Richard, et al.: 'Basic support for cooperative work on the World Wide Web', *International Journal of Human-Computer Studies*, vol. 46, 1997a, pp. 827–846.

Bentley, Richard; Thilo Horstmann; and Jonathan Trevor: 'The World Wide Web as enabling technology for CSCW: The case of BSCW', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 6, no. 2–3, 1997b, pp. 111–134.

Bergamini, David: *Mathematics*, Time Inc., New York, 1963.

Berners-Lee, Tim: *Information Management: A Proposal*, Technical report, CERN, Geneva, May 1990. (2nd ed.; 1st ed., March 1989). – Reprinted in Tim Berners-Lee: *Weaving the Web: The Past, Present and Future of the World Wide Web by Its Inventor*, London and New York, 1999. (Paperback ed., 2000), pp. 229–251.

Bertalanffy, Ludwig von: *General System Theory: Foundations, Development, Applications*, Braziller, New York, 1968. – Penguin Books, Harmondsworth, 1973.

Best, Michael H.: *The New Competition: Institutions of Industrial Restructuring*, Polity Press, Cambridge, 1990.

Bewley, William L., et al.: 'Human factors testing in the design of Xerox's 8010 "Star" office workstation', in R. N. Smith; R. W. Pew; and A. Janda (eds.): *CHI'83: Proceedings of the SIGCHI Conference on Human Factors in Computing Systems, 12–15 December 1983, Boston, Mass.,* ACM Press, New York, 1983, pp. 72–77.

Bien, Günther: 'Das Theorie-Praxis-Problem und die politische Philosophie bei Platon und Aristoteles', *Philosophisches Jahrbuch*, vol. 76, 1968–1969, pp. 264–314.

Bien, Günther: 'Praxis (Antike)', in J. Ritter and K. Gründer (eds.): *Historisches Wörterbuch der Philosophie*, vol. 7, 1989, pp. 1277–1287.

Bignoli, Celsina; and Carla Simone: 'AI techniques for supporting human to human communication in CHAOS', in P. Wilson; J. M. Bowers; and S. D. Benford (eds.): *ECSCW'89: Proceedings of the First European Conference on Computer Supported Cooperative Work, 13–15 September 1989, Gatwick, London,* 1989, pp. 133–147.

Bittner, Egon: 'The concept of organization', *Social Research*, vol. 32, no. 3, Autumn 1965, pp. 239–255.

Bittner, Egon: 'Objectivity and realism in sociology', in G. Psathas (ed.): *Phenomenological Sociology: Issues and Applications*, Wiley, New York, etc., 1973, pp. 109–125.

Bjerknes, Gro; Pelle Ehn; and Morten Kyng (eds.): *Computers and Democracy : A Scandinavian Challenge*, Avebury, Aldershot, 1987.

Blauner, Robert: *Alienation and Freedom: The Factory Worker and His Industry*, University of Chicago Press, Chicago, 1964.

Bly, Sara; Steve R. Harrison; and Susan Irwin: 'Media spaces: Bringing people together in a video, audio, and computing environment', *Communications of the ACM*, vol. 36, no. 1, January 1993, pp. 28–47.

Bogia, Douglas P., et al.: 'Supporting dynamic interdependencies among collaborative activities', in S. M. Kaplan (ed.): *COOCS'93: Conference on Organizational Computing Systems, 1–4 November 1993, Milpitas, California,* ACM Press, New York, 1993, pp. 108–118.

Bogia, Douglas P., et al.: 'Issues in the design of collaborative systems: Lessons from ConversationBuilder', in D. Z. Shapiro; M. Tauber; and R. Traunmüller (eds.): *The Design of Computer Supported Cooperative Work and Groupware Systems*, North-Holland, Elsevier, Amsterdam, 1996, pp. 401–422. – Originally published at the Workshop on CSCW Design, Schärding, Austria, 1–3 June 1993.

Borges, Jorge Luis: 'On exactitude in science' (*Los Anales de Buenos Aires*, 1946). Transl. from 'Del rigor en la ciencia'. Transl. by N. T. di Giovanni. In J. L. Borges: *A Universal History of Infamy*. Allen Lane, London, 1973, p. 149.

Bourdieu, Pierre: *Le sens pratique*, Les Éditions de Minuit, Paris, 1980.

Bourdieu, Pierre: *The Logic of Practice*. Transl. by R. Nice. Polity Press, Cambridge, 1990. (French original, *Le sens pratique*, 1980).

Bowers, John M.; John Churcher; and Tim Roberts: 'Structuring computer-mediated communication in COSMOS', in R. Speth (ed.): *EUTECO '88: European Teleinformatics Conference on Research into Networks and Distributed Applications, 20–22 April 1988, Vienna, Austria. Organized by the European Action in Teleinformatics COST 11ter*, North-Holland, Brussels, and Luxemburg, 1988, pp. 195–209.

Bowers, John M.; and John Churcher: 'Local and global structuring of computer-mediated communication', in I. Greif and L. A. Suchman (eds.): *CSCW'88: Proceedings of the Conference* *on Computer-Supported Cooperative Work*, *26–28 September 1988, Portland, Oregon,* ACM Press, New York, 1988, pp. 125–139.

Bowers, John M.; and Steven D. Benford (eds.): *Studies in Computer Supported Cooperative Work: Theory, Practice and Design*, North-Holland, Amsterdam, 1991.

Bowers, John M.: 'The Janus faces of design: Some critical questions for CSCW', in J. M. Bowers and S. D. Benford (eds.): *Studies in Computer Supported Cooperative Work: Theory, Practice and Design*, North-Holland, Amsterdam, 1991, pp. 333–350.

Bowers, John M.: 'The politics of formalism', in M. Lea (ed.): *Contexts of Computer-Mediated Communication*, Harvester, New York, 1992, pp. 232–261.

Bowers, John M.; Graham Button; and Wes W. Sharrock: 'Workflow from within and without: Technology and cooperative work on the print industry shopfloor', in H. Marmolin; Y. Sundblad; and K. Schmidt (eds.): *ECSCW'95: Proceedings of the Fourth European Conference on Computer-Supported Cooperative Work*, *10–14 September 1995, Stockholm,* Kluwer Academic Publishers, Dordrecht, 1995, pp. 51–66.

Bowker, Geoffrey C.; and Susan Leigh Star: 'Situations vs. standards in long-term, wide-scale decision-making: The case of the International Classification of Diseases', in J. F. Nunamaker, Jr. and R. H. Sprague, Jr. (eds.): *Proceedings of the Twenty-Fourth Annual Hawaii International Conference on System Sciences*, *7–11 January 1991, Kauai, Hawaii,* vol. 4, IEEE Computer Society Press, Los Alamitos, Calif., 1991, pp. 73–81.

Bowker, Geoffrey C.; and Susan Leigh Star: *Sorting Things Out: Classification and Its Consequences*, MIT Press, Cambridge, Mass., 1999.

Bradley, Margaret: *A Career Biography of Gaspard Clair François Marie Riche de Prony, Bridge Builder, Educator and Scientist*, The Edwin Mellen Press, Lewiston, New York, etc., 1998.

Brannigan, Augustine: *The Rise and Fall of Social Psychology: The Use and Misuse of the Experimental Method*, Aldine De Gruyter, New York, 2004.

Braudel, Fernand: *Civilization and Capitalism, 15th–18th Century. Volume II. The Wheels of Commerce*, Harper & Row, New York, 1982. Transl. from *Les jeux de l'echange*. Transl. by S. Reynolds. Librairie Armand Collin, Paris, 1979.

Braun, Ernest; and Stuart Macdonald: *Revolution in Miniature: The History and Impact of Semiconductor Electronics*, Cambridge University Press, Cambridge, 1978. (Paperback ed., 1980; 2nd ed., 1982).

Bromley, Allan G.: 'Difference and analytical engines', in W. Aspray (ed.): *Computing Before Computers*, Iowa State University Press, Ames, Iowa, 1990, pp. 59–98.

Brooks, Frederick P., Jr.: 'No silver bullet: Essence and accidents of software engineering' (*Computer*, 1987), in F. P. Brooks, Jr.: *The Mythical Man-Month: Essays on Software Engineering*. Addison-Wesley, Reading, Mass., 1995, pp. 177–203.

Bucciarelli, Louis L.: 'Reflective practice in engineering design', *Design Studies*, vol. 5, no. 3, July 1984, pp. 185–190.

Bucciarelli, Louis L.: 'An ethnographic perspective on engineering design', *Design Studies*, vol. 9, no. 3, July 1988a, pp. 159–168.

Bucciarelli, Louis L.: 'Engineering design process', in F. A. Dubinskas (ed.): *Making Time: Ethnographies of High-Technology Organizations*, Temple University Press, Philadelphia, 1988b, pp. 92–122.

Button, Graham; and Richard H. R. Harper: 'The relevance of "work practice" for design', *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 4, no. 4, 1995, pp. 263–280.

Cabitza, Federico; Marcello Sarini; and Carla Simone: 'Providing awareness through situated process maps: The hospital care case', in T. Gross, et al. (eds.): *GROUP 2007: International Conference on Supporting Group Work*, *4–7 November 2007, Sanibel Island, Florida,* ACM Press, New York, 2007, pp. 41–50.

Cabitza, Federico; and Carla Simone: '"...and do it the usual way": Fostering awareness of work conventions in document-mediated collaboration', in L. J. Bannon, et al. (eds.): *ECSCW 2007:*

Proceedings of the Tenth European Conference on Computer Supported Cooperative Work, 24–28 September 2005, Limerick, Springer, London, 2007, pp. 119–138.

Cabitza, Federico; Carla Simone; and Marcello Sarini: 'Leveraging coordinative conventions to promote collaboration awareness: The case of clinical records', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 18, no. 4, August 2009, pp. 301–330.

Campbell-Kelly, Martin: 'Punched-card machinery', in W. Aspray (ed.): *Computing Before Computers*, Iowa State University Press, Ames, Iowa, 1990, pp. 122–155.

Campbell-Kelly, Martin: 'The Railway Clearing House and Victorian data processing', in L. Bud-Frierman (ed.): *Information Acumen: The Understanding and Use of Knowledge in Modern Business*, Routledge, London, and New York, 1994a, pp. 51–74.

Campbell-Kelly, Martin: 'Introduction', in C. Babbage (ed.): *Passages from the Life of a Philosopher*, William Pickering, London, 1994b, pp. 7–36.

Campbell-Kelly, Martin; and William Aspray: *Computer: A History of the Information Machine*, Basic Books, New York, 1996.

Campbell-Kelly, Martin: *From Airline Reservations to Sonic the Hedgehog: A History of Software Industry*, The MIT Press, Cambridge, Mass., and London, 2003.

Card, Stuart K.: 'The human, the computer, the task, and their interaction: Analytic models and use-centered design', in D. Steier and T. M. Mitchell (eds.): *Mind Matters: A Tribute to Allen Newell*, Lawrence Erlbaum, Mahwah, New Jersey, 1996, pp. 259–312.

Cardwell, Donald: *The Fontana History of Technology*, Fontana Press, London, 1994.

Carey, Alex: 'The Hawthorne studies: A radical criticism', *American Sociological Review*, vol. 32, no. 3, June 1967, pp. 403–416.

Carroll, John M.: 'Introduction: The Kittle House manifesto', in J. M. Carroll (ed.): *Designing Interaction: Psychology at the Human-Computer Interface*, Cambridge University Press, Cambridge, 1991, pp. 1–16.

Carroll, John M.; Wendy A. Kellogg; and Mary Beth Rosson: 'The task-artifact cycle', in J. M. Carroll (ed.): *Designing Interaction: Psychology at the Human-Computer Interface*, Cambridge University Press, Cambridge, 1991, pp. 74–102.

Carroll, John M.: 'Introduction: Human-computer interaction, the past and the present', in J. M. Carroll (ed.): *Human-Computer Interaction in the New Millennium*, ACM Press, New York, 2002, pp. xxvii–xxxvii.

Carstensen, Peter H.: 'The bug report form', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994, pp. 187–219. COMIC Deliverable D3.2. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Carstensen, Peter H.; Carsten Sørensen; and Henrik Borstrøm: 'Two is fine, four is a mess: Reducing complexity of articulation work in manufacturing', in: *COOP'95: International Workshop on the Design of Cooperative Systems, 25–27 January 1995, Antibes-Juan-les-Pins,* INRIA Sophia Antipolis, France, 1995a, pp. 314–333.

Carstensen, Peter H.; Carsten Sørensen; and Tuomo Tuikka: 'Let's talk about bugs!', *Scandinavian Journal of Information Systems*, vol. 7, no. 1, 1995b, pp. 33–54.

Carstensen, Peter H.: *Computer Supported Coordination*, Risø National Laboratory, Roskilde, 1996. – Risø-R-890(EN).

Carstensen, Peter H.; and Carsten Sørensen: 'From the social to the systematic: Mechanisms supporting coordination in design', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 5, no. 4, 1996, pp. 387–413.

Carstensen, Peter H.; Kjeld Schmidt; and Uffe Kock Wiil: 'Supporting shop floor intelligence: A CSCW approach to production planning and control in flexible manufacturing', in S. C. Hayne (ed.): *GROUP'99: International Conference on Supporting Group Work, 14–17 November 1999, Phoenix, Arizona,* ACM Press, New York, 1999, pp. 111–120.

Carstensen, Peter H., et al.: *IT-støtte til produktionsgrupper: Debatoplæg fra IDAK-projektet*, CO-Industri etc., København, June 2001. <http://cscw.dk/schmidt/papers/idak_prod.gr.pdf>

Carstensen, Peter H.; and Kjeld Schmidt: ‘Self-governing production groups: Towards requirements for IT support’, in V. Marík; L. M. Camarinha-Matos; and H. Afsarmanesh (eds.): *Knowledge and Technology Integration in Production and Services: Balancing Knowledge and Technology in Product and Service Life Cycle. IFIP TC5/WG5.3. Fifth IFIP International Conference on Information Technology in Manufacturing and Services (BASYS’02), 25–27 September 2002, Cancun,* Kluwer Academic Publishers, Boston, Mass., 2002, pp. 49–60.

Ceruzzi, Paul E.: ‘Electronic calculators’, in W. Aspray (ed.): *Computing Before Computers*, Iowa State University Press, Ames, Iowa, 1990, pp. 223–250.

Chabert, Jean-Luc (ed.): *A History of Algorithms: From the Pebble to the Microchip*, Springer, Berlin, Heidelberg, New York, 1999. Transl. from *Histoire d’algorithmes. Du caillou á la puce*. Transl. by C. Weeks. Paris, 1994.

Chalmers, Matthew: ‘Awareness, representation and interpretation’, *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002.

Chambers, Ephraim: *Cyclopædia, or, an Universal Dictionary of Arts and Sciences [etc.]*, J. and J. Knapton, London, 1728. <http://digital.library.wisc.edu/1711.dl/HistSciTech.Cyclopaedia02>

Chapman, Robert L., et al.: ‘The Systems Research Laboratory’s air defense experiments’, *Management Science*, vol. 5, no. 3, April 1959, pp. 250–269.

Child, John: ‘Organizational design for advanced manufacturing technology’, in T. D. Wall; C. W. Clegg; and N. J. Kemp (eds.): *The Human Side of Advanced Manufacturing Technology*, Wiley, Chichester, 1987, pp. 101–133.

Ciborra, Claudio U.: ‘Information systems and transactions architecture’, *International Journal of Policy Analysis and Information Systems*, vol. 5, no. 4, 1981, pp. 305–324.

Ciborra, Claudio U.: ‘Reframing the role of computers in organizations: The transaction costs approach’, in L. Gallegos; R. Welke; and J. Wetherbe (eds.): *Proceedings of the Sixth International Conference on Information Systems, 16–18 December 1985, Indianapolis, Indiana,* 1985, pp. 57–69.

Ciborra, Claudio U.; and Margrethe H. Olson: ‘Encountering electronic work groups: A Transaction Costs perspective’, in I. Greif and L. A. Suchman (eds.): *CSCW’88: Proceedings of the Conference on Computer-Supported Cooperative Work, 26–28 September 1988, Portland, Oregon,* ACM Press, New York, 1988, pp. 94–101.

Cicourel, Aaron V.: ‘The integration of distributed knowledge in collaborative medical diagnosis’, in J. Galegher; R. E. Kraut; and C. Egido (eds.): *Intellectual Teamwork: Social and Technological Foundations of Cooperative Work*, Lawrence Erlbaum, Hillsdale, New Jersey, 1990, pp. 221–242.

Ciolfi, Luigina; Geraldine Fitzpatrick; and Liam J. Bannon (eds.): ‘Settings for collaboration: The role of place’. [Special theme of] *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 17, no. 2–3, April–June 2008, pp. 91–96.

Coase, Ronald H.: ‘The nature of the firm’, *Economica, New Series*, vol. 4, no. 4, November 1937, pp. 386–405.

Cohen, Gerald Allan: *Karl Marx’s Theory of History: A Defence*, Clarendon Press, Oxford, 1978.

Cohen, Jack; and Ian Stewart: *The Collapse of Chaos: Discovering Simplicity in a Complex World*, Penguin Books, New York, 1994.

Cohen, Paul J.: ‘Comments on the foundations of set theory’ (*Proceedings of the Symposium in Pure Mathematics of the American Mathematical Society, July 10–August 5, 1967, The University of California, Los Angeles, California*). In D. S. Scott (ed.): *Axiomatic Set Theory*. American Mathematical Society, Providence, Rhode Island, 1971, pp. 9–15.

Cole, Arthur H.; and George B. Watts: *The Handicrafts of France as Recorded in the Descriptions des Arts et Metiérs, 1761–1788*, Baker Library, Harvard Graduate School of Business Administration, Boston, 1952.

Coleman, D. C.: ‘Proto-industrialization: A concept too many’, *The Economic History Review, New Series*, vol. 36, no. 3, August 1984, pp. 435–448.

Collier, Bruce: *The Little Engines that Could've: The Calculating Machines of Charles Babbage*, PhD dissertation, The Department of History of Science, Harvard University, Cambridge, Mass., August 1970. <http://robroy.dyndns.info/collier/index.html>

Committee of General Literature and Education: *The Industry of Nations: A Survey of the Existing State of Arts, Machines, and Manufactures. Part II*, Society for Promoting Christian Knowledge, London, 1855.

Commons, John R.: *Institutional Economics*, University of Wisconsin Press, Madison, 1934.

Condillac, Etienne Bonnot, abbé de: *Traité des systêmes, où l'on en démelle les inconvénients et les avantages* (La Haye, 1749). – Librairie Arthème Fayard, Paris, 1991.

Conklin, Jeff; and Michael L. Begeman: 'gIBIS: A hypertext tool for exploratory policy discussion', in I. Greif and L. A. Suchman (eds.): *CSCW'88: Proceedings of the Conference on Computer-Supported Cooperative Work*, *26–28 September 1988, Portland, Oregon,* ACM Press, New York, 1988, pp. 140–152.

Conklin, Jeff: 'Design rationale and maintainability', in B. Shriver (ed.): *Proceedings of Twenty Second Annual Hawaii International Conference on System Sciences*, IEEE Computer Society, Los Alamitos, vol. 2, 1989, pp. 533–539.

Conklin, Jeff: 'Capturing organizational knowledge', in R. M. Baecker (ed.): *Readings in Groupware and Computer-Supported Cooperative Work: Assisting Human-Human Collaboration*, Morgan Kaufmann Publishers, San Mateo, Calif., 1993, pp. 561–565. – Originally published in D. Coleman (ed.): *Proceedings of Groupware '92*, Morgan Kaufmann, San Fransisco, 1992, pp. 133–137.

Constant, Edward W., II: *The Origins of the Turbojet Revolution*, Johns Hopkins University Press, Baltimore, Maryland, 1980.

Constant, Edward W., II: 'Communities and hierarchies: Structure in the practice of science of technology', in R. Laudan (ed.): *The Nature of Technological Knowledge: Are Models of Scientific Change Relevant*? D. Reidel Publishing, Dordrecht, Boston, and London, 1984, pp. 27–46.

Cook, Carolyn L.: 'Streamlining office procedures: An analysis using the information control net model', in D. Medley (ed.): *AFIPS'80: Proceedings of the National Computer Conference, 19–22 May 1980, Anaheim, California*, vol. 49, AFIPS Press, Arlington, Virginia, 1980, pp. 555–565.

Copeland, B. Jack (ed.): *The Essential Turing: Seminal Writings in Computing, Logic, Philosophy, Artificial Intelligence, and Artificial Life Plus the Secrets of Enigma*, Oxford University Press, Oxford, 2004.

Copeland, B. Jack (ed.): *Alan Turing's Automatic Computing Engine: The Master Codebreaker's Struggle to Build the Modern Computer*, Oxford University Press, Oxford, 2005.

Copeland, B. Jack; and Diane Proudfoot: 'Turing and the computer', in B. J. Copeland (ed.): *Alan Turing's Automatic Computing Engine: The Master Codebreaker's Struggle to Build the Modern Computer*, Oxford University Press, Oxford, 2005, pp. 107–148.

Copeland, B. Jack (ed.): *Colossus: The Secrets of Bletchley Park's Codebreaking Computers*, Oxford University Press, Oxford, 2006a.

Copeland, B. Jack: 'Colossus and the rise of the modern computer', in B. J. Copeland (ed.): *Colossus: The Secrets of Bletchley Park's Codebreaking Computers*, Oxford University Press, Oxford, 2006b, pp. 101–115.

Copeland, B. Jack: 'Introduction', in B. J. Copeland (ed.): *Colossus: The Secrets of Bletchley Park's Codebreaking Computers*, Oxford University Press, Oxford, 2006c, pp. 1–6.

Corbató, Fernando J.; Marjorie Merwin-Daggett; and Robert C. Daley: 'An experimental timesharing system', in G. A. Barnard (ed.): *SJCC'62: Proceedings of the Spring Joint Computer Conference*, *1–3 May 1962, San Francisco, Calif.*, vol. 21, AFIPS Press, San Francisco, Calif., 1962, pp. 335–344.

Corbató, Fernando J.: 'Oral history interview', Interviewed by A. L. Norberg, 18 April 1989, 14 November 1990, Cambridge, Mass. Interview transcript, Charles Babbage Institute, University of Minnesota, Minneapolis, (OH 162).

COSMOS: *Specification for a Configurable, Structured Message System*, Queen Mary College, London, August 1989. – 68.4 Ext/ALV.

Coulter, Jeff: *Rethinking Cognitive Theory*, Macmillan, London, 1983.

Cournot, Antoine Augustin: *An Essay on the Foundations of our Knowledge*, The Liberal Arts Press, New York, 1956. Transl. from *Essai sur les fondements de nos connaissance et sur les caractères de la critique philosophique*. Transl. by M. H. Moore. Hachette, Paris, 1851.

Crabtree, Andy; Thomas A. Rodden; and Steven D. Benford: 'Moving with the times: IT research and the boundaries of CSCW', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 14, 2005, pp. 217–251.

Crabtree, Andy, et al.: 'Ethnography considered harmful', in R. B. Arthur, et al. (eds.): *CHI 2009: Proceedings of the Twenty Seventh International Conference on Human Factors in Computing Systems*, *4–9 April 2009, Boston, Mass.,* ACM Press, New York, 2009, pp. 879–888.

Crawford, Perry Orson: *Automatic Control by Arithmetical Operations*, MSc Thesis, Department of Physics, Massachusetts Institute of Technology, Cambridge, Mass., 1942. – [The copy of Crawford's thesis available on-line from MIT is incomplete (it ends on p. 58). According to MIT's Document Services the 'thesis file has been corrupted' and Crawford's thesis 'appears to have gone missing from the collection' (Email communication, 2 and 8 December 2009).]. <http://hdl.handle.net/1721.1/11232>

Crawford, Perry Orson: 'Application of digital computation involving continuous input and output variables' (5 August 1946), in M. Campbell-Kelly and M. R. Williams (eds.): *The Moore School Lectures: Theory and Techniques for Design of Electronic Digital Computers*. (Revised ed. of *Theory and Techniques for Design of Electronic Computers*, 1947–1948). The MIT Press, Cambridge, Mass., 1985, pp. 374–392.

Croarken, Mary: 'Tabulating the heavens: Computing the *Nautical Almanac* in 18th-century England', *IEEE Annals of the History of Computing*, vol. 25, no. 3, July–September 2003, pp. 48–61.

Crocker, David H., et al.: 'RFC 733: Standard for the format of ARPA network text messages', *Request for Comments*, no. 733, Network Working Group, 21 November 1977. <http://www.rfc-editor.org/rfc/rfc733.txt>

Croft, W. B.; and L. S. Lefkowitz: 'Task support in an office system', *ACM Transactions on Office Information Systems*, vol. 2, 1984, pp. 197–212.

Crosby, Alfred W.: *The Measure of Reality: Quantification and Western Society, 1250–1600*, Cambridge University Press, Cambridge, 1997.

Cummings, Thomas; and Melvin Blumberg: 'Advanced manufacturing technology and work design', in T. D. Wall; C. W. Clegg; and N. J. Kemp (eds.): *The Human Side of Advanced Manufacturing Technology*, Wiley, Chichester, 1987, pp. 37–60.

Cyert, Richard M.; and James G. March: *A Behavioral Theory of the Firm*, Prentice-Hall, Englewood-Cliffs, New Jersey, 1963.

Dahl, Ole-Johan; and Kristen Nygaard: 'SIMULA: An ALGOL-based simulation language', *Communications of the ACM*, vol. 9, no. 9, September 1966, pp. 671–678.

Dahrendorf, Ralf: *Sozialstruktur des Betriebes*, Gabler, Wiesbaden, 1959.

Danielsen, Thore, et al.: 'The AMIGO Project: Advanced group communication model for computer-based communications environment', in H. Krasner and I. Greif (eds.): *CSCW'86: Proceedings. Conference on Computer-Supported Cooperative Work*, *3–5 December 1986, Austin, Texas,* ACM Press, New York, 1986, pp. 115–142.

Darwin, Charles G.: 'Automatic Computing Engine (ACE)' (NPL, 17 April 1946), in B. J. Copeland (ed.): *Alan Turing's Automatic Computing Engine: The Master Codebreaker's Struggle to Build the Modern Computer*. Oxford University Press, Oxford, 2005, pp. 53–57.

Davenport, Thomas H.: *Process Innovation: Reengineering Work Through Information Technology*, Harvard Business School Press, Boston, Mass., 1993.

Davies, Donald Watts: 'Proposal for the development of a national communication service for on-line data processing', Manuscript, [National Physical Laboratory, UK], 15 December 1965. <http://www.cs.utexas.edu/users/chris/DIGITAL_ARCHIVE/NPL/DaviesLetter.html>

Davies, Donald Watts: *Proposal for a Digital Communication Network*, National Physical Laboratory, June 1966. <http://www.cs.utexas.edu/users/chris/DIGITAL_ARCHIVE/NPL/Davies05.pdf>

Davies, Donald Watts: 'Foreword', in B. J. Copeland (ed.): *Alan Turing's Automatic Computing Engine: The Master Codebreaker's Struggle to Build the Modern Computer*, Oxford University Press, Oxford, 2005, pp. vii–ix.

Davis, Philip J.; and Reuben Hersh: *The Mathematical Experience*, Birkhäuser, Boston, 1980. – Pelican Books, Harmondsworth, England, 1983.

De Cindio, Fiorella, et al.: 'CHAOS as coordination technology', in H. Krasner and I. Greif (eds.): *CSCW'86: Proceedings. Conference on Computer-Supported Cooperative Work, 3–5 December 1986, Austin, Texas,* ACM Press, New York, 1986, pp. 325–342.

De Cindio, Fiorella, et al.: 'CHAOS: A knowledge-based system for conversing within offices', in W. Lamersdorf (ed.): *Office Knowledge: Representation, Management and Utilization*, Elsevier Science Publishers B.V., North-Holland, Amsterdam, 1988.

De Michelis, Giorgio; and M. Antonietta Grasso: 'How to put cooperative work in context: Analysis and design requirements', in L. J. Bannon and K. Schmidt (eds.): *Issues of Supporting Organizational Context in CSCW Systems*, Computing Department, Lancaster University, Lancaster, October 1993, pp. 73–100. COMIC Deliverable D1.1. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

de Prony, Gaspard F. C. M. Riche: 'Notice sur les Grandes Tables Logarithmique &c.' (Paris, 7 June 1824), in: *Recueil des Discourses lus dans la seance publique de L'Academie Royale Des Sciences, 7 Juin 1824*. – [Translation entitled 'On the great logarithmic and trigonometric tables, adapted to the new decimal metric system' (Science Museum Library, Babbage Archives, Document code: BAB U2l/l). Excerpt probably copied at Charles Babbage's request.]. <http://sites.google.com/site/babbagedifferenceengine/barondeprony'sdescriptionoftheconstructi>

Degani, Asaf; and Earl L. Wiener: *Human Factors of Flight-Deck Checklists: The Normal Checklist*, Ames Research Center, National Aeronautics and Space Administration, Moffett Field, California, May 1990. – NASA Contractor Report 177549; Contract NCC2-377.

Delaire, Alexandre [Delayre]: 'Épingle', in D. Diderot and J. L. R. d'Alembert (eds.): *Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers*, vol. 5, Briasson, David, Le Breton & Durand, Paris, 1755, pp. 804–807. Text ed. by R. Morrissey. – ARTFL Encyclopédie Projet, University of Chicago, 2008. <http://portail.atilf.fr/encyclopedie/>

Deloney, Thomas: *The Pleasant Historie of Iohn Winchcomb, in His Younger Yeares Called Iack of Newbery* (London, 1597; 10th ed., 1626). Text ed. by F. O. Mann. In T. Deloney: *The Works of Thomas Deloney*. Clarendon Press, Oxford, London etc., 1912, pp. 1–68.

Demchak, Chris C.: *War, Technological Complexity, and the U.S. Army*, PhD dissertation, Department of Political Science, University of California, Berkeley, Calif., 1987.

Descartes, René: *Meditations on First Philosophy* (Paris, 1641, vol. 2), in R. Descartes: *The Philosophical Writings of Descartes*. Cambridge University Press, Cambridge, 1984, pp. 1–62.

Diderot, Denis: 'Art' (*Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers*, Paris, 1751), in D. Diderot: *Œeuvres*, vol. 1 (*Philosophie*), Robert Laffont, Paris, 1994, pp. 265–276.

Diebold, John: *Automation: The Advent of the Automatic Factory*, Van Nostrand, New York, etc., 1952.

Divitini, Monica, et al.: *A Multi-Agent Approach to the Design of Coordination Mechanisms*, Centre for Cognitive Science, Roskilde University, Roskilde, 1995. *Working Papers in Cognitive Science and HCI*. – WPCS-95–5.

Divitini, Monica; Carla Simone; and Kjeld Schmidt: 'ABACO: Coordination mechanisms in a multi-agent perspective', in: *COOP'96: Second International Conference on the Design of Cooperative Systems, 12–14 June 1996, Antibes-Juan-les-Pins*, INRIA, Sophia Antipolis, France, 1996, pp. 103–122.

Dourish, Paul; and Victoria Bellotti: 'Awareness and coordination in shared workspaces', in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW'92: Proceedings of the Conference on Computer-Supported Cooperative Work*, *31 October–4 November 1992, Toronto, Canada*, ACM Press, New York, 1992, pp. 107–114.

Dourish, Paul; and Sara Bly: 'Portholes: Supporting awareness in a distributed work group', in P. Bauersfeld; J. Bennett; and G. Lynch (eds.): *CHI'92 Conference Proceedings: ACM Conference on Human Factors in Computing Systems*, *3–7 May 1992, Monterey, Calif.,* ACM Press, New York, 1992, pp. 541–547.

Dourish, Paul, et al.: 'Freeflow: Mediating between representation and action in workflow systems', in G. M. Olson; J. S. Olson; and M. S. Ackerman (eds.): *CSCW'96: Proceedings of the Conference on Computer-Supported Cooperative Work*, *16–20 November 1996, Boston, Mass.,* ACM Press, New York, 1996, pp. 190–198.

Dourish, Paul: 'Extending awareness beyond synchronous collaboration', in T. Brinck and S. E. McDaniel (eds.): *CHI'97 Workshop on Awareness in Collaborative Systems, 22–27 March 1997, Atlanta*, *Georgia,* 1997. <http://www.usabilityfirst.com/groupware/awareness>

Dourish, Paul; and Graham Button: 'On "technomethodology": Foundational relationships between ethnomethodology and system design', *Human-Computer Interaction*, vol. 13, no. 4, 1998, pp. 395–432.

Dourish, Paul: *Where the Action Is: The Foundation of Embodied Interaction*, MIT Press, Cambridge, Mass., 2001.

Dourish, Paul: 'Re-space-ing place: "Place" and "space" ten years on', in P. J. Hinds and D. Martin (eds.): *Proceedings of CSCW'06: The Twentieth Anniversary Conference on Computer Supported Cooperative Work*, *4–8 November 2006, Banff, Alberta, Canada,* ACM Press, New York, 2006a, pp. 299–308.

Dourish, Paul: 'Implications for design', in R. E. Grinter, et al. (eds.): *CHI 2006: Proceedings of the SIGCHI Conference on Human Factors in Computing Systems*, *22–27 April 2006, Montréal, Québec, Canada,* ACM Press, New York, 2006b, pp. 541–550.

Dreyfus, Hubert L.: *What Computers Can't Do: The Limits of Artificial Intelligence*, Harper & Row, New York, 1979. (Rev. ed.; 1st ed., 1972).

Drury, Maurice O'C.: 'Conversations with Wittgenstein' (1974). Text ed. by R. Rhees. In R. Rhees (ed.): *Ludwig Wittgenstein: Personal Recollections*. Rowman and Littlefield, Totowa, New Jersey, 1981, pp. 112–189.

Dubinskas, Frank A.: 'Cultural constructions: The many faces of time', in F. A. Dubinskas (ed.): *Making Time: Ethnographies of High-Technology Organizations*, Temple University Press, Philadelphia, 1988, pp. 3–38.

Eckert, J. Presper: 'A preview of a digital computing machine' (15 July 1946), in M. Campbell-Kelly and M. R. Williams (eds.): *The Moore School Lectures: Theory and Techniques for Design of Electronic Digital Computers*. (Revised ed. of *Theory and Techniques for Design of Electronic Computers*, 1947–1948). The MIT Press, Cambridge, Mass., 1985, pp. 109–126.

Eckert, Wallace J.: *Punched Card Methods in Scientific Computation*, The Thomas J. Watson Astronomical Computing Bureau, New York, 1940. – The MIT Press, Cambridge, Mass., and London, 1984.

Edgerton, David: *The Shock of the Old: Technology and Global History Since 1900*, Profile Books, London, 2006.

Egger, Edeltraud; and Ina Wagner: 'Negotiating temporal orders: The case of collaborative time management in a surgery clinic', *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 1, no. 4, 1993, pp. 255–275.

Ehn, Pelle: *Work-Oriented Design of Computer Artifacts*, Arbetslivscentrum, Stockholm, 1988.

Ellis, Clarence A.,; Simon J. Gibbs; and Gail L. Rein: 'Groupware: Some issues and experiences', *Communications of the ACM*, vol. 34, no. 1, January 1991, pp. 38–58.

Ellis, Clarence A.: 'Information control nets: A mathematical model of office information flow', in: *Proceedings of the ACM Conference on Simulation, Measurement and Modeling*, *August 1979, Boulder, Colorado,* ACM, New York, 1979, pp. 225–239.

Ellis, Clarence A.; and Gary J. Nutt: 'Office information systems and computer science', *Computing Surveys*, vol. 12, no. 1, March 1980, pp. 27–60.

Ellis, Clarence A.; and Marc Bernal: 'Officetalk-D: An experimental office information system', *ACM SIGOA Newsletter*, vol. 3, no. 1–2, June 1982, pp. 131–140. – *Proceedings of the ACM SIGOA conference on Office information system, 21–23 June 1982, Philadelphia, Pennsylvania*.

Ellis, Clarence A.: 'Formal and informal models of office activity', in R. E. A. Mason (ed.): *Information Processing '83. Proceedings of the IFIP Ninth World Computer Congress*, *19–23 September 1983, Paris,* North-Holland, Amsterdam, 1983, pp. 11–22.

Ellis, Clarence A.; Karim Keddara; and Grzegorz Rozenberg: 'Dynamic change within workflow systems', in N. Comstock, et al. (eds.): *COOCS'95: Conference on Organizational Computing Systems*, *13–16 August 1995, Milpitas, California,* ACM Press, New York, 1995, pp. 10–21.

Endsley, Mica R.; and Daniel J. Garland (eds.): *Situation Awareness Analysis and Measurement*, Lawrence Erlbaum Associates, Mahwah, New Jersey, and London, 2000.

Endsley, Mica R.; and William M. Jones: 'A model of inter- and intrateam situation awareness: Implications for design. training, and measurement', in M. McNeese; E. Salas; and M. R. Endsley (eds.): *New Trends in Cooperative Activities: Understanding System Dynamics in Complex Environments*, Human Factors and Ergonomics Society, Santa Monica, Calif., 2001, pp. 46–67.

Engelbart, Douglas C.: *Augmenting Human Intellect: A Conceptual Framework (Prepared for: Director of Information Sciences, Air Force Office of Scientific Research, Washington 25, D.C., Contract AF49(638)–1024)*, Stanford Research Institute, Menlo Park, Calif., October 1962. – AFOSR-3233. <http://www.dougengelbart.org/pubs/augment-3906.html>

Engelbart, Douglas C.; and William K. English: 'A research center for augmenting human intellect', in: *FJCC'68: Proceedings of the Fall Joint Computing Conference*, *9–11 December 1968, San Francisco, California.* Part I, vol. 33, AFIPS Press, New York, 1968, pp. 395–410. – Also in I. Greif (ed.): *Computer-Supported Cooperative Work: A Book of Readings*, Morgan Kaufmann Publishers, San Mateo, Calif., 1988, pp. 81–105.

Engelbart, Douglas C.: 'Authorship provisions in AUGMENT', in: *COMPCON '84 Digest: Procedings of the 1984 COMPCON Conference, San Francisco, CA, 27 February – 1 March*, 1984, pp. 465–472. – Also in I. Greif (ed.): *Computer-Supported Cooperative Work: A Book of Readings*. Morgan Kaufmann Publishers, San Mateo, Calif., 1988, pp. 107–126.

Engelbart, Douglas C.; and Harvey Lehtman: 'Working together', *Byte*, vol. 13, no. 13, December 1988, pp. 245–252.

Engleberger, Joseph E.: *Robotics in Practice: Management and Applications of Industrial Robots*, Kogan Page, London, 1980.

English, W.: 'The textile industry: Silk production and manufacture, 1750–1900', in C. Singer, et al. (eds.): *A History of Technology, Volume IV: The Industrial Revolution, c 1750 to c 1850*, Oxford University Press, Oxford, 1958, pp. 277–307.

English, William K.; Douglas C. Engelbart; and Melwyn L. Berman: 'Display-selection techniques for text manipulation', *IEEE Transactions on Human Factors in Electronics*, vol. HFE-8, no. 1, March 1967, pp. 5–15.

Essinger, James: *Jacquard's Web: How a Hand-loom Led to the Birth of the Information Age*, Oxford University Press, Oxford, 2005. (Paperback. ed., 2007).

Eveland, J. D.; and Tora K. Bikson: 'Evolving electronic communication networks: An empirical assessment', *Office: Technology and People*, vol. 3, no. 2, 1987, pp. 103–128.

Exner, Wilhelm Franz: *Johann Beckmann: Begründer der technologischen Wissenschaft*, Gerold, Wien, 1878. – Kessinger Publishing Co, Whitefish, Montana, 2009. – [The reprint title contains a typo: *Johann Beckmann: Segründer der technologischen Wissenschaft*].

Fayol, Henri: *Administration industrielle el générale*, Dunod, Paris, 1918, 1999. – Orginally published in *Bulletin de la Societé de l'Industrie minérale*, 1916. (English translation: *General and Industrial Management*, Pitman, London, 1949).

Farrington, Benjamin: *Greek Science*, Penguin Books, London, 1944/1949. – Spokesman, Nottingham, 2000.

Ferry, Georgina: *A Computer Called LEO: Lyons Teashops and the World's First Office Computer*, HarperCollins, London, 2003. (Paperback ed., 2004).

Fikes, Richard E.; and D. Austin Henderson, Jr.: 'On supporting the use of procedures in office work', in: *Proceedings of the First Annual Conference on Artificial Intelligence*, *18–20 August 1980, Stanford University, Stanford, Calif.,* American Association for Artificial Intelligence, Menlo Park, Calif., 1980, pp. 202–207.

Finholt, Thomas A.; and Stephanie D. Teasley: 'Psychology: The need for psychology in research on computer-supported cooperative work', *Social Science Computer Review*, vol. 16, no. 1, Spring 1998, pp. 40–52.

Finley, Moses I.: 'Was Greek civilization based on slave labour?' (*Historia*, 1959). In M. I. Finley (ed.): *Slavery in Classical Antiquity: View and Controversies*. W. Heffer & Sons, Cambridge, 1960, pp. 53–72.

Finley, Moses I.: *The Ancient Economy*, University of California Press, Berkeley, Calif., 1973. (3rd ed., 1999).

Fish, Robert S.; Robert E. Kraut; and Barbara L. Chalfonte: 'The VideoWindow system in informal communications', in T. Bikson and F. Halasz (eds.): *CSCW'90: Proceedings of the Conference on Computer-Supported Cooperative Work*, *7–10 October 1990, Los Angeles, Calif.,* ACM Press, New York, 1990, pp. 1–11.

Fitts, Paul M.: 'The information capacity of the human motor system in controlling amplitude of movement', *Journal of Experimental Psychology*, vol. 47, no. 6, June 1954, pp. 381–391.

Fitzpatrick, Geraldine; William J. Tolone; and Simon M. Kaplan: 'Work, locales and distributed social worlds', in H. Marmolin; Y. Sundblad; and K. Schmidt (eds.): *ECSCW'95: Proceedings of the Fourth European Conference on Computer-Supported Cooperative Work*, *10–14 September 1995, Stockholm,* Kluwer Academic Publishers, Dordrecht, 1995, pp. 1–16.

Fitzpatrick, Geraldine; Simon M. Kaplan; and Tim Mansfield: 'Physical spaces, virtual places and social worlds: A study of work in the virtual', in G. M. Olson; J. S. Olson; and M. S. Ackerman (eds.): *CSCW'96: Proceedings of the Conference on Computer-Supported Cooperative Work*, *16–20 November 1996, Boston, Mass.,* ACM press, New York, 1996, pp. 334–343.

Fitzpatrick, Geraldine, et al.: 'Supporting public availability and accessibility with Elvin: Experiences and reflections', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002, pp. 447–474.

Flach, John M., et al. (eds.): *Global Perspectives on the Ecology of Human-Machine Systems*, Lawrence Erlbaum, Hillsdale, New Jersey, 1995.

Flores, Fernando, et al.: 'Computer systems and the design of organizational interaction', *ACM Transactions on Office Information Systems*, vol. 6, no. 2, April 21988, pp. 153–172.

Freiberger, Paul; and Michael Swaine: *Fire in the Valley: The Making of the Personal Computer*, McGraw-Hill, New York, etc., 2000. (2nd ed.; 1st ed., 1984).

Fuchs, Ludwin; and Wolfgang Prinz: 'Aspects of organizational context in CSCW', in L. J. Bannon and K. Schmidt (eds.): *Issues of Supporting Organizational Context in CSCW Systems*, Computing Department, Lancaster University, Lancaster, October 1993, pp. 11–47. COMIC Deliverable D1.1. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Fuchs, Ludwin; Uta Pankoke-Babatz; and Wolfgang Prinz: 'Supporting cooperative awareness with local event mechanisms: The GroupDesk system', in H. Marmolin; Y. Sundblad; and K. Schmidt (eds.): *ECSCW'95: Proceedings of the Fourth European Conference on Computer-Supported Cooperative Work*, *10–14 September 1995, Stockholm,* Kluwer Academic Publishers, Dordrecht, 1995, pp. 245–260.

Gannon, Paul: *Colossus: Bletchley Park's Greatest Secret*, Atlantic Books, London, 2006. (Paperback ed., 2007).

Garfinkel, Harold: *Studies in Ethnomethodology*, Prentice-Hall, Englewood-Cliffs, New Jersey, 1967. – Polity Press, Cambridge, 1987.

Garfinkel, Harold; and Egon Bittner: '"Good" organizational reasons for "bad" clinic records', in H. Garfinkel: *Studies in Ethnomethodology*. Prentice-Hall, Englewood-Cliffs, New Jersey, 1967, pp. 186–207.

Garlan, Yvon: *Slavery in Ancient Greece*, Cornell University Press, Ithaca, and London, 1988. Transl. from *Les esclaves en Grèce ancienne*. Transl. by J. Lloyd. François Maspero, Paris, 1982. (Revised ed.).

Garnsey, Peter: *Ideas of Slavery from Aristotle to Augustine*, Cambridge University Press, Cambridge, 1996.

Gasser, Les; and Alan H. Bond: 'An analysis of problems and research in distributed artificial intelligence', in L. Gasser and A. H. Bond (eds.): *Readings in Distributed Artificial Intelligence*, Morgan Kaufmann Publishers, San Mateo, Calif., 1988, pp. 3–35.

Gaver, William W.: 'Sound support for collaboration', in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference on Computer-Supported Cooperative Work*, *24–27 September 1991, Amsterdam,* Kluwer Academic Publishers, Dordrecht, 1991, pp. 293–308.

Gaver, William W.: 'The affordances of media spaces for collaboration', in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW'92: Proceedings of the Conference on Computer-Supported Cooperative Work*, *31 October–4 November 1992, Toronto, Canada,* ACM Press, New York, 1992, pp. 17–24.

Gaver, William W., et al.: 'Realizing a video environment: EuroPARC's RAVE system', in P. Bauersfeld; J. Bennett; and G. Lynch (eds.): *CHI'92 Conference Proceedings: ACM Conference on Human Factors in Computing Systems*, *3–7 May 1992, Monterey, Calif.,* ACM Press, New York, 1992, pp. 27–35.

Gaver, William W.: 'Provocative awareness', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002, pp. 475–493.

Geertz, Clifford: *The Interpretation of Culture: Selected Essays*, Basic Books, New York, 1973.

Gell, Alfred: *The Anthropology of Time: Cultural Constructions of Temporal Maps and Images*, Berg, Oxford/Providence, 1992.

Gerrard, Steve: 'Wittgenstein's philosophies of mathematics', *Synthese*, vol. 87, 1991, pp. 125–142.

Gerson, Elihu M.; and Susan Leigh Star: 'Analyzing due process in the workplace', *ACM Transactions on Office Information Systems*, vol. 4, no. 3, July 1986, pp. 257–270.

Gibson, James J.: *The Ecological Approach to Visual Perception*, Houghton-Mifflin, Boston, 1979. – Lawrence Erlbaum Associates, Hillsdale, New Jersey, 1986.

Giedion, Siegfried: *Mechanization Takes Command: A Contribution to Anonymous History*, Oxford University Press, New York, 1948.

Gilbert, K. R.: 'Machine-tools', in C. Singer, et al. (eds.): *A History of Technology, Volume IV: The Industrial Revolution, c 1750–c 1850*, Oxford University Press, Oxford, 1958, pp. 417–441.

Gillespie, Richard: *Manufacturing Knowledge: A History of the Hawthorne Experiments*, Cambridge University Press, Cambridge, 1991. (Paperback ed., 1993).

Gillies, James; and Robert Cailliau: *How the Web was Born: The Story of the World Wide Web*, Oxford University Press, Oxford, 2000.

Gillispie, Charles Coulston (ed.): *A Diderot Pictorial Encyclopedia of Trades and Industry: Manufacturing and the Technical Arts in Plates Selected from 'L'Encyclopedie, ou dictionnaire raissoné des sciences, des arts et des métiers' of Denis Diderot*, vol. 1–2, Dover Publications, New York, 1959. Transl. from *Recueil de planches, sur les sciences, les arts liberaux, et les arts méchaniques, avec leur explication*, Paris, 1763–1772.

Goffman, Erving: *Encounters: Two Studies in the Sociology of Interaction*, Bobbs-Merrill, Indianapolis, 1961.

Goffman, Erving: *Behavior in Public Places: Notes on the Social Organization of Gatherings*, The Free Press, New York, 1963.

Goldstine, Herman H.: *The Computer from Pascal to von Neumann*, Princeton University Press, Princeton, New Jersey, 1972.

Good, Jack; Donald Michie; and Geoffrey Timms: *General Report on Tunny: With Emphasis on Statistical Methods*, Government Code and Cypher School, 1945. vol. 1–2. –

Public Record Office, HW 25/4 & HW 25/4. – Bibliograpical data based on <http://www.alanturing.net/tunny_report/. – Partial transcript by Tony Sale, March 2001: http://www.codesandciphers.org.uk/documents/newman/newman.pdf>

Goody, Jack: *The Domestication of the Savage Mind*, Cambridge University Press, Cambridge, 1977.

Goody, Jack: *The Logic of Writing and the Organization of Society*, Cambridge University Press, Cambridge, 1986.

Goody, Jack: *The Interface Between the Written and the Oral*, Cambridge University Press, Cambridge, 1987.

Goody, Jack: *The Power of the Written Tradition*, Smithsonian Institution Press, Washington DC, 2000.

Grattan-Guinness, Ivor: 'Work for the hairdressers: The production of de Prony's logartihmic and trigonometric tables', *IEEE Annals of the History of Computing*, vol. 12, no. 3, July–September 1990, pp. 177–185.

Greenberg, Saul: 'Sharing views and interactions with single-user applications', in F. H. Lochovsky and R. B. Allen (eds.): *COIS'90: Proceedings of the Conference on Office Information Systems, 25–27 April 1990, Cambridge, Mass.*, ACM Press, New York, 1990, pp. 227–237.

Greenberg, Saul: '[Introduction to] Computer-supported Cooperative Work and Groupware', in S. Greenberg (ed.): *Computer-Supported Cooperative Work and Groupware*, Academic Press, London, 1991, pp. 1–8.

Greif, Irene; and Sunil K. Sarin: 'Data sharing in group work', in H. Krasner and I. Greif (eds.): *CSCW'86: Proceedings. Conference on Computer-Supported Cooperative Work, 3–5 December 1986, Austin, Texas,* ACM Press, New York, 1986, pp. 175–183.

Greif, Irene (ed.): *Computer-Supported Cooperative Work: A Book of Readings*, Morgan Kaufmann Publishers, San Mateo, Calif., 1988a.

Greif, Irene: 'Overview', in I. Greif (ed.): *Computer-Supported Cooperative Work: A Book of Readings*, Morgan Kaufmann Publishers, San Mateo, Calif., 1988b, pp. 5–12.

Grier, David Alan: 'Human computers: The first pioneers of the information age', *Endeavour*, vol. 25, no. 1, March 2001, pp. 28–32.

Grier, David Alan: *When Computers Were Human*, Princeton University Press, Princeton, and Oxford, 2005.

Grinter, Rebecca E.: 'Supporting articulation work using software configuration management systems', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 5, no. 4, 1996a, pp. 447–465.

Grinter, Rebecca E.: *Understanding Dependencies: A Study of the Coordination Challenges in Software Development*, PhD dissertation, University of California, Irvine, 1996b.

Grønbæk, Kaj; and Preben Mogensen: 'Informing general CSCW product development through cooperative design in specific work domains', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 6, no. 4, 1997, pp. 275–304.

Groover, Mikell P.: *Automation, Production Systems, and Computer-Aided Manufacturing*, Prentice-Hall, Englewood-Cliffs, New Jersey, 1980.

Gross, Tom; and Wolfgang Prinz: 'Awareness in context: A light-weight approach', in K. Kuutti, et al. (eds.): *ECSCW 2003: Proceedings of the Eighth European Conference on Computer Supported Cooperative Work, 14–18 September 2003, Helsinki,* Kluwer Academic Publishers, Dordrecht, 2003, pp. 295–314.

Gross, Tom; and Wolfgang Prinz: 'Modelling shared contexts in cooperative environments: Concept, implementation, and evaluation', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 13, no. 3–4, August 2004, pp. 283–303.

Gruber, Tom: 'Ontology', in L. Liu and M. T. Özsu (eds.): *Encyclopedia of Database Systems*, Springer, New York, 2009.

Grudin, Jonathan: 'Why groupware applications fail: Problems in design and evaluation', *Office: Technology and People*, vol. 4, no. 3, 1989, pp. 245–264.

Grudin, Jonathan: 'CSCW: The convergence of two development contexts', in S. P. Robertson; G. M. Olson; and J. S. Olson (eds.): *CHI'91 Conference Proceedings: ACM SIGCHI Conference on Human Factors in Computing Systems*, *27 April–2 May 1991, New Orleans, Louisiana,* ACM Press, New York, 1991, pp. 91–97.

Grudin, Jonathan: 'Computer-Supported Cooperative Work: History and focus', *IEEE Computer*, vol. 27, no. 5, May 1994, pp. 19–26.

Grudin, Jonathan: 'CSCW and groupware: Their history and trajectory', in Y. Matsushita (ed.): *Designing Communication and Collaboration Support Systems*, Gordon and Breach Science Publishers, Amsterdam, 1999, pp. 1–15.

Grudin, Jonathan: 'The GUI shock: Computer graphics and human-computer interaction', *Interactions*, vol. 13, March–April 2006a, pp. 45–47, 55.

Grudin, Jonathan: 'The demon in the basement', *Interactions*, vol. 13, November–December 2006b, pp. 50–53.

Gunn, Thomas G.: *Manufacturing for Competitive Advantage. Becoming a World Class Manufacturer*, Ballinger, Cambridge, Mass., 1987.

Gutwin, Carl: *Workspace Awareness in Real-Time Distributed Groupware*, PhD dissertation, Department of Computer Science, The Universit of Calgary, Calgary, Alberta, December 1997.

Gutwin, Carl; and Saul Greenberg: 'The effects of workspace awareness support on the usability of real-time distributed groupware', *ACM Transactions on Computer-Human Interaction*, vol. 6, no. 2, September 1999, pp. 243–281.

Gutwin, Carl; and Saul Greenberg: 'A descriptive framework of workspace awareness for real-time groupware', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002.

Haas, Christina: *Writing Technology: Studies on the Materiality of Literacy*, Lawrence Erlbaum, Mahwah, New Jersey, 1996.

Hacker, Peter M. S.: *Insight and Illusion: Themes in the Philosophy of Wittgenstein*, Thoemmes Press, Bristol, England, 1989. (2nd ed.; 1st ed., 1972).

Hacker, Peter M. S.: 'Malcolm on language and rules' (*Philosophy*, 1990), in P. M. S. Hacker (eds.) *Wittgenstein: Connections and Controversies*, Clarendon Press, Oxford, 2001, pp. 310–323.

Hafner, Katie; and Matthew Lyon: *Where Wizards Stay up Late: The Origins of the Internet*, Simon & Schuster, London, etc., 2003. (2nd ed.; 1st ed., 1996).

Hammer, Marius: *Vergleichende Morphologie der Arbeit in der europäischen Automobilindustrie: Die Entwickling sur Automation*, Kyklos-Verlag, Basel, 1959.

Hammer, Michael; and Jay S. Kunin: 'Design principles of an office specification language', in D. Medley (ed.): *AFIPS'80: Proceedings of the National Computer Conference, 19–22 May 1980, Anaheim, California,* vol. 49, AFIPS Press, Arlington, Virginia, 1980, pp. 541–547.

Hammer, Michael; and Marvin Sirbu: 'What is office automation?', in: *Office Automation Conference, 3–5 March 1980, Atlanta, Georgia*, AFIPS Press, Reston, Virginia, 1980, pp. 37–49.

Hardy, Ian R.: *The Evolution of ARPANET Email*, History thesis paper, University of California at Berkeley, Berkeley, Calif., 13 May 1996. <http://www.ifla.org.sg/documents/internet/hari1.txt>

Harper, Richard H. R.; John A. Hughes; and Dan Z. Shapiro: 'Working in harmony: An examination of computer technology in air traffic control', in P. Wilson; J. M. Bowers; and S. D. Benford (eds.): *ECSCW'89: Proceedings of the First European Conference on Computer Supported Cooperative Work*, *13–15 September 1989, Gatwick, London,* 1989a, pp. 73–86.

Harper, Richard H. R.; John A. Hughes; and Dan Z. Shapiro: *The Functionality of Flight Strips in ATC Work: The report for the Civil Aviation Authority*, Lancaster Sociotechnics Group, Department of Sociology, Lancaster University, Lancaster, January 1989b.

Harper, Richard H. R.; John A. Hughes; and Dan Z. Shapiro: 'Harmonious working and CSCW: Computer technology and air traffic control', in J. M. Bowers and S. D. Benford (eds.): *Studies in Computer Supported Cooperative Work. Theory, Practice and Design*, North-Holland, Amsterdam, 1991, pp. 225–234.

Harper, Richard H. R.; and John A. Hughes: 'What a f—ing system! Send 'em all to the same place and then expect us to stop 'em hitting: Managing technology work in air traffic control', in G. Button (ed.): *Technology in Working Order: Studies of Work, Interaction, and Technology*, Routledge, London and New York, 1993, pp. 127–144.

Harrington, Joseph: *Computer Integrated Manufacturing*, Krieger, Malabar, Florida, 1979.

Harrington, Joseph: *Understanding the Manufacturing Process. Key to Successful CAD/CAM Implementation*, Marcel Dekker, New York, 1984.

Harris, Roy: *The Origin of Writing*, Duckworth, London, 1986.

Harris, Roy: *Signs of Writing*, Routledge, London and New York, 1995.

Harris, Roy: *Rethinking Writing*, Indiana University Press, Bloomington and Indianapolis, 2000.

Harrison, Steve; and Paul Dourish: 'Re-placing space: The roles of place and space in collaborative systems', in G. M. Olson; J. S. Olson; and M. S. Ackerman (eds.): *CSCW'96: Proceedings of the Conference on Computer-Supported Cooperative Work, 16–20 November 1996, Boston, Mass.,* ACM Press, New York, 1996, pp. 67–76.

Hauben, Michael; and Ronda Hauben: *Netizens: On the History and Impact of Usenet and the Internet*, IEEE Computer Society Press, Los Alamitos, Calif., 1997.

Heart, Frank, et al.: *Draft ARPANET Completion Report*, [Bound computer printout, unpublished], Bolt Beranek and Newman, 9 September 1977. – Quoted from Abbate: *Inventing the Internet*, The MIT Press, Cambridge, Mass., and London, 1999.

Heart, Frank, et al.: *ARPANET Completion Report*, Bolt Beranek and Newman, 4 January 1978. (Also published as *A History of the ARPANET: The First Decade*, BBN 1981, Report #4799.). <http://www.cs.utexas.edu/users/chris/DIGITAL_ARCHIVE/ARPANET/DARPA4799.pdf>

Heath, Christian C.; and Paul Luff: 'Collaborative activity and technological design: Task coordination in London Underground control rooms', in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference on Computer-Supported Cooperative Work, 24–27 September 1991, Amsterdam,* Kluwer Academic Publishers, Dordrecht, 1991a, pp. 65–80.

Heath, Christian C.; and Paul Luff: 'Disembodied conduct: Communication through video in a multi-media office environment', in S. P. Robertson; G. M. Olson; and J. S. Olson (eds.): *CHI'91 Conference Proceedings: ACM SIGCHI Conference on Human Factors in Computing Systems, 27 April–2 May 1991, New Orleans, Louisiana,* ACM Press, New York, 1991b, pp. 99–103.

Heath, Christian C.; and Paul Luff: 'Collaboration and control: Crisis management and multimedia technology in London Underground control rooms', *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 1, no. 1–2, 1992a, pp. 69–94.

Heath, Christian C.; and Paul Luff: 'Media space and communicative asymmetries: Preliminary observations of video mediated interaction', *Human-Computer Interaction*, vol. 7, 1992b, pp. 315–346.

Heath, Christian C.; and Paul Luff: 'Disembodied conduct: Interactional asymmetries in video-mediated communication', in G. Button (ed.): *Technology in Working Order: Studies of Work, Interaction, and Technology*, Routledge, London and New York, 1993, pp. 35–54.

Heath, Christian C., et al.: 'Unpacking collaboration: The interactional organisation of trading in a City dealing room', *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 3, no. 2, 1995a, pp. 147–165.

Heath, Christian C.; Paul Luff; and Abigail J. Sellen: 'Reconsidering the virtual workplace: Flexible support for collaborative activity', in H. Marmolin; Y. Sundblad; and K. Schmidt (eds.): *ECSCW'95: Proceedings of the Fourth European Conference on Computer-Supported Cooperative Work, 10–14 September 1995, Stockholm,* Kluwer Academic Publishers, Dordrecht, 1995b, pp. 83–99.

Heath, Christian C.; and Paul Luff: 'Convergent activities: Line control and passenger information on the London Underground', in Y. Engeström and D. Middleton (eds.): *Cognition and Communication at Work*, Cambridge University Press, Cambridge, 1996, pp. 96–129.

Heath, Christian C., et al.: 'Configuring awareness', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002.

Henderson, Kathryn: *On Line and On Paper: Visual representations, Visual Culture, and Computer Graphics in Design Engineering*, MIT Press, Cambridge, Mass., and London, 1999.

Heritage, John C.: *Garfinkel and Ethnomethodology*, Polity Press, Cambridge, 1984.

Hertzfeld, Andy: *Revolution in the Valley*, O'Reilly, Sebastopol, Calif., 2005.

Hewitt, Carl: 'Viewing control structures as patterns of passing messages', *Artificial Intelligence*, vol. 8, 1977, pp. 323–364.

Hewitt, Carl: 'Offices are open systems', *ACM Transactions on Office Information Systems*, vol. 4, no. 3, July 1986, pp. 271–287.

Hilbert, David: 'Mathematical problems' (*International Congress of Mathematicians*, Paris, 1900). Transl. from 'Mathematische Probleme'. Transl. by M. F. Winston. In J. J. Gray (ed.): *The Hilbert Challenge*. Oxford University Press, Oxford, 2000, pp. 240–282. – [German original in *Archiv für Mathematik und Physik*, vol. 1, 1901; English transl. in *Bulletin of the American Mathematical Society*, vol. 8, 1902]

Hilbert, David: 'On the infinite' (Münster, 4 June 1925). Transl. from 'Über das Unendliche'. Transl. by S. Bauer-Mengelberg. In J. van Heijenoort (ed.): *From Frege to Gödel: A Source Book in Mathematical Logic, 1879–1931*. Harvard University Press, Cambridge, Mass., 1967, pp. 367–392.

Hilbert, David: 'The foundations of mathematics' (Hamburg, July 1927). Transl. by S. Bauer-Mengelberg and D. Føllesdal. In J. van Heijenoort (ed.): *From Frege to Gödel: A Source Book in Mathematical Logic, 1879–1931*. Harvard University Press, Cambridge, Mass., 1967, pp. 464–479.

Hillis, W. Daniel: *The Pattern on the Stone: The Simple Ideas that Make Computers Work*, Weidenfeld & Nicolson, London, 1998. – Phoenix, London, 2001. (Paperback ed.).

Hiltz, Starr Roxanne; and Murray Turoff: *The Network Nation: Human Communication via Computer*, Addison-Wesley, Reading, Mass., 1978. – The MIT Press, Cambridge, Mass., 1993 (Rev. ed.).

Hiltz, Starr Roxanne; and Murray Turoff: 'The evolution of user behavior in a computerized conferencing system', *Communications of the ACM*, vol. 24, no. 11, November 1981, pp. 739–751.

Hiltz, Starr Roxanne; and Elaine B. Kerr: *Studies of Computer Mediated Communications Systems: A Synthesis of the Findings: Final Report on a Workshop Sponsored by the Division of Information Science and Technology, National Science Foundation*, Computerized Conferencing and Communications Center, New Jersey Institute of Technology, Newark, New Jersey, 1981. – Research Report Number 16. <http://library.njit.edu/archives/cccc-materials/>

Hiltz, Starr Roxanne; and Murray Turoff: 'Structuring computer-mediated communication systems to avoid information overload', *Communications of the ACM*, vol. 28, no. 7, July 1985, pp. 680–689.

Hirschhorn, Larry: *Beyond Mechanization: Work and Technology in a Postindustrial Age*, MIT Press, Cambridge, Mass., and London, 1984.

Hobbes, Thomas: *Leviathan, or The Matter, Forme, & Power of a Common-Wealth Ecclesiaticall and Civil*, London, 1651. – Penguin Books, Harmondsworth, Middlesex, 1968.

Hodgskin, Thomas: *Labour Defended Against the Claims of Capital, or, the Unproductiveness of Capital Proved with Reference to the Present Combinations Amongst Journeymen*, Knight & Lacey, London, 1825. – Augustus M. Kelley, Publishers, New York, 1969.

Holland, John H.: *Hidden Order: How Adaption Builds Complexity*, Addison-Wesley Publishing Co., Reading, Mass., 1995.

Holt, Anatol W.: 'Coordination Technology and Petri Nets', in G. Rozenberg (ed.): *Advances in Petri Nets 1985*, (*Lecture Notes in Computer Science*, vol. 222), Springer, Berlin, 1985, pp. 278–296.

Howard, Robert: 'Systems design and social responsibility: The political implications of "Computer-Supported Cooperative Work": A commentary', *Office: Technology and People*, vol. 3, no. 2, 1987.

Howse, Derek: *Greenwich Time and the Discovery of the Longitude*, Oxford University Press, Oxford, 1980. – Philip Wilson Publishers, London, 1997.

Hughes, John A., et al.: *The Automation of Air Traffic Control*, Lancaster Sociotechnics Group, Department of Sociology, Lancaster University, Lancaster, October 1988.

Hughes, John A.; David W. Randall; and Dan Z. Shapiro: 'CSCW: Discipline or paradigm? A sociological perspective', in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference on Computer-Supported Cooperative Work*, *24–27 September 1991, Amsterdam,* Kluwer Academic Publishers, Dordrecht, 1991, pp. 309–323.

Hughes, Thomas P.: 'The evolution of large technological systems', in W. E. Bijker; T. P. Hughes; and T. J. Pinch (eds.): *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*, The MIT Press, Cambridge, Mass., and London, 1987, pp. 51–82. (Paperback ed., 1989).

Hull, David L.: *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science*, The University of Chicago Press, Chicago, 1988.

Hutchins, Edwin L.; James D. Hollan; and Donald A. Norman: 'Direct manipulation interfaces', in D. A. Norman and S. W. Draper (eds.): *User Centered System Design*, Lawrence Erlbaum, Hillsdale, New Jersey, 1986, pp. 87–124.

Hutchins, Edwin L.: 'Mediation and automatization', *Quarterly Newsletter of the Laboratory of Comparative Human Cognition [University of California, San Diego]*, vol. 8, no. 2, April 1986, pp. 47–58.

Hutchins, Edwin L.: 'The social organization of distributed cognition', in L. B. Resnick; J. M. Levine; and S. D. Teasley (eds.): *Perspectives on Socially Shared Cognition*, American Psychological Association, Washington, DC, 1991, pp. 283–307.

Hutchins, Edwin L.: *Cognition in the Wild*, The MIT Press, Cambridge, Mass., and London, England, 1995.

Hutchins, Edwin L.; and Tove Klausen: 'Distributed cognition in an airline cockpit', in Y. Engeström and D. Middleton (eds.): *Cognition and Communication at Work*, Cambridge University Press, Cambridge, 1996, pp. 15–34.

Ishii, Hiroshi: 'TeamWorkStation: Towards a seamless shared workspace', in T. Bikson and F. Halasz (eds.): *CSCW'90: Proceedings of the Conference on Computer-Supported Cooperative Work*, *7–10 October 1990, Los Angeles, Calif.,* ACM Press, New York, 1990, pp. 13–26.

Ishii, Hiroshi; Minoru Kobayaski; and Jonathan Grudin: 'Integration of inter-personal space and shared workspace: ClearBoard design and experiments', in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW'92: Proceedings of the Conference on Computer-Supported Cooperative Work*, *31 October–4 November 1992, Toronto, Canada,* ACM Press, New York, 1992, pp. 33–42.

Jirotka, Marina, et al. (eds.): 'Special issue: Collaboration in e-Research'. [Special theme of] *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 15, no. 4, August 2006.

Johannsen, Gunnar, et al.: 'Final report of experimental psychology group', in N. Moray (ed.): *Mental Workload: Its Theory and Measurement*, Plenum Press, New York, and London, 1979, pp. 101–114.

Johansen, Robert: *Groupware: Computer Support for Business Teams*, Free Press, New York, and London, 1988.

Johnson, Allen W.; and Timothy Earle: *The Evolution of Human Societies: From Foraging Group to Agrarian State*, Stanford University Press, Stanford, Calif., 1987.

Johnson, Jeff, et al.: 'The Xerox Star: A retrospective', *IEEE Computer*, vol. 22, no. 9, September 1989, pp. 11–26, 28–29.

Johnson, Philip: 'Supporting exploratory CSCW with the EGRET framework', in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW'92: Proceedings of the Conference on Computer-Supported Cooperative Work*, *31 October–4 November 1992, Toronto, Canada,* ACM Press, New York, 1992, pp. 298–305.

Johnson-Lenz, Peter; and Trudy Johnson-Lenz: ‘Groupware: The process and impacts of design choices’, in E. B. Kerr and S. R. Hiltz (eds.): *Computer-Mediated Communication Systems*, Academic Press, New York, 1982.

Johnston, William A.; and Veronica J. Dark: ‘Selective attention’, *Annual Review of Psychology*, vol. 37, 1986, pp. 43–75.

Jones, Stephen R. G.: ‘Was there a Hawthorne effect?’, *The American Journal of Sociology*, vol. 98, no. 3, November 1992, pp. 451–468.

Jordan, Nehemiah: ‘Allocation of functions between man and machines in automated systems’, *Journal of Applied Psychology*, vol. 47, no. 3, June 1963, pp. 161–165.

Kaavé, Bjarne: *Undersøgelse af brugersamspil i system til produktionsstyring*, M.Sc. dissertation, Technical University of Denmark, Lyngby, Denmark, 1990.

Kanigel, Robert: *The One Best Way: Frederick Winslow Taylor and the Enigma of Efficiency*, The MIT Press, Cambridge, Mass., and London, 1997. (Paperback ed., 2005).

Kant, Immanuel: ‘Über den Gemeinspruch: Das mag in der Theorie richtig sein, taugt aber nicht für die Praxis’ (*Berlinische Monatsschrift*, September 1793). Text ed. by W. Weischedel. In I. Kant: *Werke in zwölf Bänden*. Suhrkamp Verlag, Frankfurt a. M., 1964, vol. XI, pp. 125–172.

Kantowitz, Barry H.; and Robert D. Sorkin: ‘Allocation of functions’, in G. Salvendy (ed.): *Handbook of Human Factors*, Wiley, New York, etc., 1987, pp. 355–369.

Kaplan, Simon M., et al.: ‘Flexible, active support for collaborative work with Conversation Builder’, in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW’92: Proceedings of the Conference on Computer-Supported Cooperative Work*, *31 October–4 November 1992, Toronto, Canada,* ACM Press, New York, 1992, pp. 378–385.

Kaptelinin, Victor: ‘Computer-mediated activity: Functional organs in social and developmental contexts’, in B. A. Nardi (ed.): *Context and Consciousness: Activity Theory and Human-Computer Interaction*, The MIT Press, Cambridge, Mass., 1997, pp. 45–68.

Kasbi, Catherine; and Maurice de Montmollin: ‘Activity without decision and responsibility: The case of nuclear power plants’, in J. Rasmussen; B. Brehmer; and J. Leplat (eds.): *Distributed Decision Making: Cognitive Models for Cooperative Work*, Wiley, Chichester, 1991, pp. 275–283.

Kauffman, Stuart: *At Home in the Universe: The Search for Laws of Self-Organization and Complexity*, Oxford University Press, New York, and Oxford, 1995.

Kawell, Jr., Leonard, et al.: ‘Replicated document management in a group communication system’, in I. Greif and L. A. Suchman (eds.): *CSCW’88: Proceedings of the Conference on Computer-Supported Cooperative Work*, *26–28 September 1988, Portland, Oregon,* ACM Press, New York, 1988, pp. 395[–404].

Kelley, Charles R.: *Manual and Automatic Control: A Theory of Manual Control and Its Application to Manual and to Automatic Systems*, Wiley, New York, 1968.

Kern, Horst; and Michael Schumann: *Industriearbeit und Arbeiterbewußtsein: Eine empirische Untersuchung über den Einfluß der aktuellen technischen Entwicklung auf die industrielle Arbeit und das Arbeiterbewußtsein*, vol. 1–2, Europäische Verlagsanstalt, Frankfurt am Main, 1970.

Kerr, Elaine B.; and Starr Roxanne Hiltz: *Computer-Mediated Communication Systems: Status and Evaluation*, Academic Press, Orlando, etc., 1982.

Kiesler, Sara; Jane Siegel; and Timothy W. McGuire: ‘Social psychological aspects of computer-mediated communication’, *American Psychologist*, vol. 39, no. 10, October 1984, pp. 1123–1134.

Klein, Mark; Chrysanthos Dellarocas; and Abraham Bernstein (eds.): ‘Adaptive Workflow Systems’. [Special theme of] Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing, vol. 9, no. 3–4, 2000.

Kling, Rob: ‘Social analyses of computing: Theoretical perspectives in recent empirical research’, *Computing Surveys*, vol. 12, no. 1, March 1980, pp. 61–110.

Kling, Rob: ‘Cooperation, coordination and control in computer-supported work’, *Communications of the ACM*, vol. 34, no. 12, December 1991, pp. 83–88.

Kochhar, A. K.; and N. D. Burns: *Microprocessors and Their Manufacturing Applications*, Edward Arnold, London, 1983.

Koren, Yoram: *Computer Control of Manufacturing Systems*, McGraw-Hill, New York, etc., 1983.

Korsch, Karl: *Karl Marx*, Chapman & Hall, London, 1938. – Russell & Russell, New York, 1963.

Kraut, Robert E.; Carmen Egido; and Jolene Galegher: 'Patterns of contact and communication in scientific research collaboration', in J. Galegher; R. E. Kraut; and C. Egido (eds.): *Intellectual Teamwork: Social and Technological Foundations of Cooperative Work*, Lawrence Erlbaum, Hillsdale, New Jersey, 1990, pp. 149–171.

Kreifelts, Thomas: 'DOMINO: Ein System zur Abwicklung arbeitsteiliger Vorgänge im Büro', *Angewandte Informatik*, vol. 26, no. 4, 1984, pp. 137–146.

Kreifelts, Thomas, et al.: 'Experiences with the DOMINO office procedure system', in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference on Computer-Supported Cooperative Work, 24–27 September 1991, Amsterdam*, Kluwer Academic Publishers, Dordrecht, 1991a, pp. 117–130.

Kreifelts, Thomas, et al.: 'A design tools for autonomous agents', in J. M. Bowers and S. D. Benford (eds.): *Studies in Computer Supported Cooperative Work: Theory, Practice and Design*, North-Holland, Amsterdam, 1991b, pp. 131–144.

Kreifelts, Thomas; Elke Hinrichs; and Gerd Woetzel: 'Sharing to-do lists with a distributed task manager', in G. De Michelis; C. Simone; and K. Schmidt (eds.): *ECSCW'93: Proceedings of the Third European Conference on Computer-Supported Cooperative Work, 13–17 September 1993, Milano,* Kluwer Academic Publishers, Dordrecht, 1993, pp. 31–46.

Kuhn, Thomas S.: *The Structure of Scientific Revolutions* (1962; 2nd ed., 1969). University of Chicago Press, Chicago, 1969.

Kuutti, Kari: 'Activity theory as a potential framework for human-computer interaction research', in B. A. Nardi (ed.): *Context and Consciousness: Activity Theory and Human-Computer Interaction*, The MIT Press, Cambridge, Mass., 1997, pp. 17–44.

La Porte, Todd R. (ed.): *Organized Social Complexity: Challenge to Politics and Policy*, Princeton University Press, Princeton, New Jersey, 1975.

La Porte, Todd R.; and Paula M. Consolini: 'Working in practice but not in theory: Theoretical challenges of "high-reliability organizations"', *Journal of Public Administration Research and Theory*, vol. 1, no. 1, 1991, pp. 19–47.

Laiserin, Jerry: 'The convergence of CAD standards', in: *Digital Architect*, April 2002.

Lakatos, Imre: 'A renaissance of empiricism in the recent philosophy of mathematics?' (1967). Text ed. by J. Worrall and G. Currie. In I. Lakatos: *Mathematics, Science and Epistemology: Philosophical Papers, Volume 2*. Cambridge University Press, Cambridge, 1978, pp. 24–42.

Lakatos, Imre: 'Falsification and the methodology of scientific research programmes', in I. Lakatos and A. Musgrave (eds.): *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, 1965, London,* Cambridge University Press, Cambridge, 1970, pp. 91–196. – Also in Imre Lakatos: *The Methodology of Scientific Research Programmes: Philosophical Papers, Volume 1*, Cambridge University Press, Cambridge, 1978, pp. 8–101.

Lampson, Butler W.: 'Personal distributed computing: The Alto and Ethernet software', in A. Goldberg (ed.): *A History of Personal Workstations*, Addison-Wesley, Reading, Mass., 1988, pp. 293–335.

Landes, David S.: *Revolution in Time: Clocks and the Making of the Modern World*, Harvard University Press, Cambridge, Mass., 1983.

Latour, Bruno: *Science in Action: How to Follow Scientists and Engineers Through Society*, Harvard University Press, Cambridge, Mass., 1987.

Laudan, Rachel: 'Introduction', in R. Laudan (ed.): *The Nature of Technological Knowledge: Are Models of Scientific Change Relevant?*, D. Reidel Publishing, Dordrecht, Boston, and London, 1984, pp. 1–26.

Lauwers, J. Chris; and Keith A. Lantz: 'Collaboration awareness in support of collaboration transparency: Requirements for the next generation of shared window systems', in J. C. Chew and J. Whiteside (eds.): *CHI'90 Conference Proceedings: ACM SIGCHI Conference on Human Factors in Computing Systems*, *1–5 April 1990, Seattle, Washington,* ACM Press, New York, 1990, pp. 303–311.

Lave, Jean: 'Situated learning in communities of practice', in L. B. Resnick; J. M. Levine; and S. D. Teasley (eds.): *Perspectives on Socially Shared Cognition*, Americal Psychological Association, Washington, DC, 1991, pp. 63–82.

Law, John; and John Hassard (eds.): *Actor Network Theory and After*, Blackwell, London, 1999.

Layton, Edwin T.: 'Technology as knowledge', *Technology and Culture*, vol. 15, no. 1, January 1974, pp. 31–41.

LeCuyer, A.: 'Design on the computer: Frank O. Gehry und Peter Eisenman', *ARCH+*, no. 128, September 1995, pp. 26–29.

Lee, Charlotte P., et al. (eds.): 'Special issue: Scientific collaboration through cyberinfrastructure'. [Special theme of] *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 19, no. 3–4, June–August 2010.

Lévi-Strauss, Claude: *La pensée sauvage*, Librairie Plon, Paris, 1962. – Plon, Paris, 1990.

Licklider, Joseph Carl Robnett: 'Man-computer symbiosis' (*IRE Transactions on Human Factors in Electronics*, March 1960). In R. W. Taylor (ed.): *In Memoriam: J. C. R. Licklider, 1915–1990*. Digital Systems Research Center, Palo Alto, Calif., 1990, pp. 4–11. – Also in Adele Goldberg: *A History of Personal Workstations* Addison-Wesley, Reading, Mass., 1988, pp. 131–140.

Licklider, Joseph Carl Robnett; and Welden E. Clark: 'On-line man-computer communication', in G. A. Barnard (ed.): *SJCC'62: Proceedings of the Spring Joint Computer Conference*, *1–3 May 1962, San Francisco, California,* vol. 21, AFIPS Press, 1962, pp. 113–128.

Lindgren, Michael: *Glory and Failure: The Difference Engines of Johann Müller, Charles Babbage and Georg and Edvard Scheutz*, Linköping University, Linköping, 1987.

List, Friedrich: *Das nationale System der politischen Ökonomie*, Cotta Verlag, Stuttgart u. Tübingen, 1841a.

List, Friedrich: *The National System of Political Economy*, J. B. Lippincott, Philadelphia, 1856. Transl. from *Das nationale System der politischen Ökonomie*. Transl. by G. A. Matile, Cotta Verlag, Stuttgart u. Tübingen, 1841b.

Lojek, Bo: *History of Semiconductor Engineering*, Springer, Berlin-Heidelberg, 2007.

Luff, Paul; Jon Hindmarsh; and Christian C. Heath (eds.): *Workplace Studies: Recovering Work Practices and Informing System Design*, Cambridge University Press, Cambridge, 2000.

Luke, Hugh D.: *Automation for Productivity* Wiley, New York, etc., 1972.

Lutters, Wayne G.; and Mark S. Ackerman: 'Achieving safety: A field study of boundary objects in aircraft technical support', in E. Churchill, et al. (eds.): *CSCW 2002: ACM Conference on Computer Supported Cooperative Work*, *16–20 November 2002, New Orleans, Louisiana,* ACM Press, New York, 2002, pp. 266–275.

Lynch, Michael: 'Extending Wittgenstein: The pivotal move from epistemology to the sociology of science', in A. Pickering (ed.): *Science as Practice and Culture*, Chicago University Press, Chicago, 1993, pp. 215–265.

Maas, W.; and J. Van Rijs: *FRANAX: Excursions on Density*, 010 Publishers, Rotterdam, 1998.

Mahoney, Michael S.: 'Computers and mathematics: The search for a discipline of computer science', in J. Echeverria; A. Ibarra; and T. Mormann (eds.): *The Space of Mathematics: Philosophical, Epistemological, and Historical Explorations*, Walter de Gruyter, Berlin and New York, 1992, pp. 349–363.

Mahoney, Michael S.: 'The histories of computing(s)', *Interdisciplinary Science Reviews*, vol. 30, no. 2, 2005, pp. 119–135.

Malcolm, Norman: *Nothing is Hidden: Wittgenstein's Criticism of His Early Thought*, Basil Blackwell, Oxford, 1986.

Malcolm, Norman: *Wittgensteinian Themes: Essays, 1978–1989*, Cornell University Press, Ithaca and London, 1995. Text ed. by G. H. von Wright.

Malone, Thomas W.; JoAnne Yates; and Robert I. Benjamin: 'Electronic markets and electronic hierarchies', *Communications of the ACM* vol. 30, no. 6, June 1987, pp. 484–497.

Malone, Thomas W.; and Kevin Crowston: 'What is coordination theory and how can it help design cooperative work systems', in T. Bikson and F. Halasz (eds.): *CSCW'90: Proceedings of the Conference on Computer-Supported Cooperative Work*, *7–10 October 1990, Los Angeles, Calif.,* ACM Press, New York, 1990, pp. 357–370.

Malone, Thomas W.; Hum-Yew Lai; and Christopher Fry: 'Experiments with OVAL: A radically tailorable tool for cooperative work', in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW'92: Proceedings of the Conference on Computer-Supported Cooperative Work*, *31 October–4 November 1992, Toronto, Canada*, ACM Press, New York, 1992, pp. 289–297.

Malone, Thomas W.; and Kevin Crowston: *The Interdisciplinary Study of Coordination*, Center of Coordination Science, MIT Press, Cambridge, Mass., 1992.

Malone, Thomas W., et al.: 'Tools for inventing organizations: Toward a handbook of organizational processes', in: *Proceedings of the Second IEEE Workshop on Enabling Technologies Infrastructure for Collaborative Enterprises*, *20–22 April 1993, Morgantown, West Virginia,* 1993.

Malone, Thomas W.; Hum-Yew Lai; and Christopher Fry: 'Experiments with OVAL: A radically tailorable tool for cooperative work', *ACM Transactions on Office Information Systems*, vol. 13, no. 2, April 1995, pp. 177–205.

Mantei, Marilyn M., et al.: 'Experiences in the use of a media space', in S. P. Robertson; G. M. Olson; and J. S. Olson (eds.): *CHI'91 Conference Proceedings: ACM SIGCHI Conference on Human Factors in Computing Systems*, *27 April–2 May 1991, New Orleans, Louisiana*, ACM Press, New York, 1991, pp. 203–208.

March, James G.; and Herbert A. Simon: *Organizations*, Wiley, New York, 1958.

Mariani, John A.; and Wolfgang Prinz: 'From multi-user to shared object systems: Awareness about co-workers in cooperation support object databases', in H. Reichel (ed.): *GI-Tagung: Informatik - Wirtschaft - Gesellschaft*, Springer, Berlin-Heidelberg, 1993, pp. 476–481. <http://www.fit.fraunhofer.de/~prinz/papers/gi93-awareness.html>

Mark, Gloria: 'Conventions and commitments in distributed CSCW groups', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002, pp. 349–387.

Marx, Karl: 'Thesen über Feuerbach' (Manuscript, Spring 1845a), in K. Marx and F. Engels (eds.): *Werke*. Dietz Verlag, Berlin, 1962, pp. 5–7.

Marx, Karl: *Brüsseler Hefte 1845* (Manuscript, 1845b). Text ed. by G. A. Bagaturija, et al. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)*. Akademie Verlag, Berlin, 1998, vol. IV/3, pp. 111– 433.

Marx, Karl: *Misère de la philosophie: Reponse a La philosophie de la misère de M. Proudhon*, A. Frank/C. G. Vogler, Paris/Bruxelles, 1847.

Marx, Karl: 'Der achtzehnte Brumaire des Louis Bonaparte' (*Die Revolution: Eine Zeitscrhift in zwanglosen Heften*, New York, 1852). Text ed. by M. Hundt, et al. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)* vol. I/11, Dietz Verlag, Berlin, 1985, pp. 96–189.

Marx, Karl: 'Einleitung' (Manuscript, August 1857). Text ed. by V. K. Brušlinskij; L. R. Mis'kevič; and A. G. Syrov. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)*, vol. II/1.1, Dietz Verlag, Berlin, 1976–1981 (*Ökonomische Manuskripte 1857/1858*), pp. 17–45.

Marx, Karl: *Grundrisse der Kritik der politischen Ökonomie* (Manuscript, 1857–1858a). Text ed. by V. K. Brušlinskij; L. R. Mis'kevič; and A. G. Syrov. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)*, vol. II/1, Dietz Verlag, Berlin, 1976–1981, pp. 47–747.

Marx, Karl: *Grundrisse der Kritik der politischen Ökonomie (Rohentwurf), 1857–1858. Anhang 1850–1859* (Manuscript, 1857–1858b). Dietz Verlag, Berlin, 1953.

Marx, Karl: *Zur Kritik der politischen Ökonomie (Manuskript 1861–1863)* (1861–1863). Text ed. by A. Schnickmann, et al. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)* vol. II/3.1–II/3.6, Dietz Verlag, Berlin, 1976–1982.

Marx, Karl: [Letter to Friedrich Engels], London, 28 January 1863. In K. Marx and F. Engels (eds.): *Werke*, vol. 30, Dietz Verlag, Berlin, 1964, pp. 319–323.

Marx, Karl: 'Ökonomische Manuscripte 1863–1867' (1863–1867). In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA ②)*, vol. II/4.2, Dietz Verlag/IISA, Berlin-Amsterdam, 1992. – The manuscript of *Das Kapital* vol. 3; primarily written 1864–1865

Marx, Karl: *Das Kapital. Kritik der politischen Ökonomie. Erster Band. Buch I: Der Produktionsprocess des Kapitals* (Hamburg, 1867a). Text ed. by E. Kopf, et al. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)* vol. II/5, Dietz Verlag, Berlin, 1983.

Marx, Karl: *Das Kapital. Kritik der politischen Ökonomie. Erster Band. Buch I: Der Produktionsprocess des Kapitals* (Hamburg, 1867b; 4th ed., 1890). Text ed. by F. Engels. In K. Marx and F. Engels (eds.): *Werke* vol. 23, Dietz Verlag, Berlin, 1962.

Marx, Karl: *Capital: A Critique of Political Economy. Book One: The Process of Production of Capital* (Hamburg, 1867c; 4th ed., 1890). Transl. by S. Moore and E. Aveling. Text ed. by F. Engels. In K. Marx and F. Engels (eds.): *Collected Works* vol. 35, Lawrence & Wishart, London, 1996.

Marx, Karl: 'À la rédaction de l'*Otečestvennyâ Zapiski*' (Draft letter, in French, October–November 1877). Text ed. by H. Schwab, et al. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)* vol. I/25, Dietz Verlag, Berlin, 1985, pp. 112–117.

Marx, Karl: 'Lettre à Vera Ivanovna Zasulič (Projet I-IV)' (Draft letters, in French, 18 February–8 March 1881). Text ed. by H. Schwab, et al. In K. Marx and F. Engels (eds.): *Gesamtausgabe (MEGA②)*, vol. I/25, Dietz Verlag, Berlin, 1985, pp. 217–240.

Mayr, Ernst; and Peter D. Ashlock: *Principles of Systematic Zoology*, McGraw-Hill, New York, 1991. (2nd ed.; 1st ed., 1969).

Mayr, Ernst; and Walter J. Bock: 'Classifications and other ordering systems', *Journal of Zoological Systematics & Evolutionary Research*, vol. 40, no. 4, December 2002, pp. 169–194.

Mayr, Otto: *The Origins of Feedback Control*, The MIT Press, Cambridge, Mass., and London, 1970. Transl. from *Zur Frühgeschichte der technischen Regelungen*, R. Oldenbourg Verlag, Wien, 1969.

McCarthy, John: 'Reminiscences on the history of time sharing', Stanford University, Winter or Spring 1983. <http://www-formal.stanford.edu/jmc/history/timesharing/timesharing.html>

McKenney, James L.: *Waves of Change: Business Evolution Through Information Technology*, Harvard Business School Press, Boston, Mass., 1995.

McNeese, Michael; Eduardo Salas; and Mica R. Endsley (eds.): *New Trends in Cooperative Activities: Understanding System Dynamics in Complex Environments*, Human Factors and Ergonomics Society, Santa Monica, Calif., 2001.

McPherson, J. C.: 'Mathematical operations with punched cards', *Journal of the American Statistical Association*, vol. 37, no. 218, June 1942, pp. 275–281.

Medina-Mora, Raul, et al.: 'The Action Workflow approach to workflow management technology', in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW'92: Proceedings of the Conference on Computer-Supported Cooperative Work*, *31 October–4 November 1992, Toronto, Canada*, ACM Press, New York, 1992, pp. 281–288.

Mendels, Franklin F.: 'Proto-industrialization: The first phase of the industrialization process', *The Journal of Economic History*, vol. 32, no. 1, March 1972, pp. 241–261.

Merchant, M. Eugene: 'Flexible manufacturing systems: Robotics and computerized automation', *The Annals of the American Academy of Political and Social Science*, vol. 470, no. 1, 1983, pp. 123–135.

Mickler, Otfried; Eckhard Dittrich; and Uwe Neumann: *Technik, Arbeitsorganisation und Arbeit: Eine empirische Untersuchung in der automatischen Produktion*, Aspekte Verlag, Frankfurt am Main, 1976.

Mill, John Stuart: *Principles of Political Economy, with Some of Their Applications to Social Philosophy*, John W. Parker, London, 1848.

Miller, George Armitage: 'What is information measurement?', *The American Psychologist*, vol. 8, January 1953, pp. 3–11.

Miller, George Armitage: 'The magical number seven, plus or minus two: Some limits on our capacity for processing information', *Psychological Review*, vol. 63, no. 2, March 1956, pp. 81–97.

Miller, George Armitage; Eugene Galanter; and Karl H. Pribram: *Plans and the Structure of Behavior*, Holt, Rinehart & Winston, New York, 1960.

Mills, Charles Wright: *White Collar: The American Middle Classes*, Oxford University Press, New York, 1951.

Mindell, David A.: *Between Human and Machine: Feedback, Control, and Computing before Cybernetics*, Johns Hopkins University Press, Baltimore, and London, 2002.

Mintzberg, Henry: *The Structuring of Organizations: A Synthesis of the Research*, Prentice-Hall, Englewood-Cliffs, New Jersey, 1979.

Mitchell, William J.: *Picture Theory: Essays on Verbal and Visual Representation* The University of Chicago Press, Chicago, 1994.

Mitrofanov, Sergei Petrovich: *Scientific Principles of Group Technology*, National Lending Library for Science and Technology, Boston Spa, Yorkshire, England. Transl. from *Nauchnye osnovy gruppovoi tekhnologii*, Leningrad, 1959. Transl. by E. Harris. Text ed. by T. J. Grayson, 1966.

Monden, Yasuhiro: *Toyota Production System: Practical Approach to Production Management*, Industrial Engineering and Management Press, Institute of Industrial Engineers, Norcross, Georgia, 1983.

Monk, Ray: *Bertrand Russell: The Spirit of Solitude*, Jonathan Cape, London, 1996. – Vintage, 1997 (Paperback ed.).

Monod, Jacques: *Chance and Necessity: An Essay on the Natural Philosophy of Modern Biology*, Collins, London, 1972. Transl. from *Le hasard et la nécessité. Essai sur la philosophie naturelle de la biologie moderne*. Editions Seuil, Paris, 1970. Transl. by A. Wainhouse.

Moore Jr., Barrington: *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World*, Boston, 1966. – Penguin University Books, Harmondsworth, 1973.

Moray, Neville (ed.): *Mental Workload: Its Theory and Measurement*, Plenum Press, New York, and London, 1979a.

Moray, Neville: 'Models and measures of mental workload', in N. Moray (ed.): *Mental Workload: Its Theory and Measurement*, Plenum Press, New York, and London, 1979b, pp. 13–21.

Morse, Philip M.; and George E. Kimball: *Methods of Operations Research*, The Technology Press of MIT, Cambridge, Mass., 1951. (Rev. ed.; 1st ed., US Navy, 1946). – Dover Publications, Mineola, New York, 2003.

Multhauf, Robert P.: 'Some observations on the state of the history of technology', *Technology and Culture*, vol. 15, no. 1, January 1974, pp. 1–12.

Mumford, Lewis: *Technics and Civilization*, Harcourt Brace & Co., New York, 1934. – Harcourt Brace & Co., San Diego, etc., 1963 (Paperback ed.).

Myer, Theodore H.; and David Dodds: 'Notes on the development of message technology', in D. M. Austin (ed.): *Berkeley Workshop on Distributed Data Management and Computer Networks, 1976*, Lawrence Berkeley Laboratories, 1976, pp. 144–154. – LBL-5315.

Napper, R. B. E.: 'The Manchester Mark I computers', in R. Rojas and U. Hashagen (eds.): *The First Computers: History and Architectures*, The MIT Press, Cambridge, Mass., and London, 2000, pp. 365–378.

Nardi, Bonnie A.: *A Small Matter of Programming: Perspectives on End User Computing*, The MIT Press, Cambridge, Mass., 1993.

Nardi, Bonnie A.; Steve Whittaker; and Erin Bradner: 'Interaction and outeraction: Instant messaging in action', in W. A. Kellogg and S. Whittaker (eds.): *CSCW 2000: ACM Conference on Computer Supported Cooperative Work, 2–6 December 2000, Philadelphia, Pennsylvania, USA*, ACM Press, New York, 2000, pp. 79–88.

Neisser, Ulric: *Cognition and Reality: Principles and Implications of Cognitive Psychology*, W. H. Freeman & Co., San Francisco, 1976.

Newton, Janice: 'Technology and cooperative labour among the Orokaiva', *Mankind*, vol. 15, no. 3, December 1985, pp. 214–222.

Nicolis, Grégoire; and Ilya Prigogine: *Exploring Complexity: An Introduction*, W. H. Freeman & Co., New York, 1989.

Norberg, Arthur L.: 'High-technology calculation in the early 20th century: Punched card machinery in business and government', *Technology and Culture*, vol. 31, no. 4, October 1990, pp. 753–779.

Norman, Donald A.; and Edwin L. Hutchins: *Computation via Direct Manipulation*, Institute for Cognitive Science, University of California, San Diego, La Jolla, California, 1 August 1988. – ONR Contract N00014-85-C-0133.

Norman, Donald A.: 'Cognitive artifacts', in J. M. Carroll (ed.): *Designing Interaction: Psychology at the Human-Computer Interface*, Cambridge University Press, Cambridge, 1991, pp. 17–38.

Nuland, Sherwin B.: [Review of] A. Gawande: *Complications: A Surgeon's Notes on an Imperfect Science* (Metropolitan Books), *The New York Review of Books*, vol. 49, no. 12, 18 July 2002, pp. 10–13.

Nurminen, Markku I.: *People or Computers: Three ways of looking at information systems*, Studentlitteratur, Lund, Sweden, 1988.

Nuttgens, Patrick: *The Story of Architecture*, Phaidon Press, London, 1997. (2nd ed.; 1st ed., 1983).

Nygaard, Kristen; and Ole-Johan Dahl: 'The development of the SIMULA languages', *ACM SIGPLAN Notices*, vol. 13, no. 8, August 1978, pp. 245–272.

O'Neill, Judy Elizabeth: *The Evolution of Interactive Computing through Time-sharing and Networking*, PhD dissertation, University of Minnesota, Minneapolis, 1992.

Odgaard, Irene: *"Praksis var værre end forventet": Producktionsgrupper på Ålestrup: Grundfos, Denmark. Casestudie-rapport om nye former for arbejdsorganisation*, IMF Project on new forms of work organization. Unpublished report, Specialarbejderforbundet i Danmark (SiD), København, 26 August 1994.

Odgaard, Irene, et al.: *Produktionsgrupper: Organisationsudvikling og IT-støtte*, CO-industri etc., København, 1999.

Ohmae, Kenichi: *Triad Power: The Coming Shape of Global Competition*, Free Press, New York, 1985.

Ohno, Taiichi: *Toyota Production System: Beyond Large-scale Production*, Productivity Press, New York, 1988.

Olson, David R.: *The World on Paper: The Conceptual and Cognitive Implications of Writing and Reading*, Cambridge University Press, Cambridge, 1994.

Olson, Gary M.; and Judith S. Olson: 'Groupware and computer-supported cooperative work', in J. A. Jacko and A. Sears (eds.): *The Human-Computer Interaction Handbook: Fundamentals, Evolving Technologies and Emerging Applications*, Lawrence Erlbaum, Mahwah, New Jersey, 2003, pp. 583–595.

Opitz, H.; W. Eversheim; and H. P. Wiendahl: 'Workpiece classification and its industrial application', *International Journal of Machine Tool Design Research*, vol. 9, 1969, pp. 39–50.

Opitz, H.: *A Classification System to Describe Workpieces*, Pergamon Press, Oxford, 1970. Transl. from *Werkstückbeschreibendes Klassifizierungssystem*, Verlag W. Girardet, Essen. Transl. by R. A. A. Taylor. Text ed. by W. R. MacConnell.

Orbiteam: 'BSCW at work: References', August 2009. <http://www.bscw.de/english/references.html>

Orlikowski, Wanda J.: 'Learning from NOTES: Organizational issues in groupware implementation', in M. M. Mantei; R. M. Baecker; and R. E. Kraut (eds.): *CSCW'92: Proceedings of the Conference on Computer-Supported Cooperative Work, 31 October–4 November 1992, Toronto, Canada*, ACM Press, New York, 1992, pp. 362–369.

Orton, John: *Semiconductors and the Information Revolution: Magic Crystals that Made IT Happen* Academic Press, Amsterdam, etc., 2009.

Ouchi, William G.: 'Markets, bureaucracies, and clans', *Administrative Science Quarterly*, vol. 25, March 1980, pp. 129–141.

Palme, Jacob: 'You have 134 unread mail! Do you want to read them now?', in H. T. Smith (ed.): *Proceedings of the IFIP Conference on Computer Based Message Services*, *Nottingham, U.K.,* Elsevier North-Holland, New York, 1984, pp. 175–184.

Panko, Raymond R.: ‘The outlook for computer mail’, *Telecommunications Policy*, June 1977, pp. 242–253.

Panko, Raymond R.: ‘Electronic mail’, in J. Slonim; E. A. Unger; and P. S. Fisher (eds.): *Advances in Data Communications Management*, vol. 2, Wiley & Sons, New York, 1984, pp. 205–222.

Pankoke-Babatz, Uta (ed.): *Computer Based Group Communication: The AMIGO Activity Model*, Ellis Horwood Publishers, Chichester, 1989a.

Pankoke-Babatz, Uta: ‘Preface’, in U. Pankoke-Babatz (ed.): *Computer Based Group Communication: The AMIGO Activity Model*, Ellis Horwood Publishers, Chichester, 1989b, pp. 13–16.

Pankoke-Babatz, Uta: ‘Group communication and electronic communication’, in U. Pankoke-Babatz (ed.): *Computer Based Group Communication: The AMIGO Activity Model*, Ellis Horwood Publishers, Chichester, 1989c, pp. 17–66.

Peaucelle, Jean-Louis: ‘From Taylorism to post-Taylorism: Simultaneously pursuing several management objectives’, *Journal of Organizational Change Management*, vol. 13, no. 5, 2000, pp. 452–467.

Peaucelle, Jean-Louis: ‘Adam Smith’s use of multiple references for his pin making example’, *The European Journal of the History of Economic Thought*, vol. 13, no. 4, 2006, pp. 489–512.

Peaucelle, Jean-Louis; and Stéphane Manin: ‘Billettes and the economic viability of pin-making in 1700’, in: *Eleventh World Congress of Accounting Historians, July 2006, Nantes*, 2006. <http://adamsmithslostlegacy.com/ftp.adamsmithslostlegacy.com/Pinmaking-Billettes.pdf>

Peaucelle, Jean-Louis: *Adam Smith et la division du travail: La naissance d’une fausse idée*, L’Harmattan, Paris, 2007.

Perronet, Jean-Rodolphe: ‘Epinglier: Description de la façon dont on fabrique les épingles à Laigle en Normandie’, in D. Diderot and J. L. R. d’Alembert (eds.): *Recueil de planches, sur les sciences, les arts liberaux, et les arts méchaniques, avec leur explication.Troisième livraison, 298 Planches*, Briasson, David & Le Breton, Paris, 1765, pp. 5:1–5:8. Text ed. by R. Morrissey. – ARTFL Encyclopédie Projet, University of Chicago, 2008. – [*Encyclopédie. . .*, vol. 21]. <http://portail.atilf.fr/encyclopedie/>

Perrow, Charles: ‘The organizational context of human factors engineering’, *Administrative Science Quaterly*, vol. 28, December 1983, pp. 521–541.

Perrow, Charles: *Normal Accidents: Living with High-Risk Technologies*, Basic Books, New York, 1984.

Perrow, Charles: *Complex Organizations: A Critical Essay*, Random House, New York, 1986. (3rd ed.).

Petty, William: *Another Essay in Political Arithmetick, Concerning the Growth of the City of London, with the Measures, Periods, Causes, and Consequences thereof* (1683, vol. II). Text ed. by C. H. Hull. In W. Petty: *The Economic Writings of Sir William Petty*. Cambridge University Press, Cambridge, 1899, pp. 449–487. – Reprinted by Augustus M. Kelley, New York, 1963.

Piel, Gerard, et al. (eds.): *Automatic Control*, G. Bell and Sons, London, 1955. (2nd ed.; 1st. ed., 1955).

Pietroforte, Roberto: ‘Communication and governance in the building process’, *Construction Managment and Economics*, vol. 15, 1997, pp. 71–82.

Pipek, Volkmar, et al. (eds.): End-User Development: 2nd International Symposium, IS-EUD 2009, Siegen, Germany, 2–4 March 2009: Proceedings, Springer, Berlin-Heidelberg, 2009.

Pitkin, Hanna Fenichel: *Wittgenstein and Justice: On the Significance of Ludwig Wittgenstein for Social and Political Thought*, University of California Press, Berkeley, Calif., 1972. (Paperback ed., 1993).

Plowman, Lydia; Yvonne Rogers; and Magnus Ramage: ‘What are workplace studies for?’, in H. Marmolin; Y. Sundblad; and K. Schmidt (eds.): *ECSCW’95: Proceedings of the Fourth European Conference on Computer-Supported Cooperative Work, 10–14 September 1995, Stockholm*, Kluwer Academic Publishers, Dordrecht, 1995, pp. 309–324.

Pollock, Friedrich: *Automation: Materialien zur Beurteilung der ökonomischen and sozialen Folgen*, Europäische Verlagsanstalt, Frankfurt a. M., 1964.

Popitz, Heinrich, et al.: *Technik und Industriearbeit: Soziologische Untersuchungen in der Hüttenindustrie*, J. C. B. Mohr, Tübingen, 1957.

Poppe, Johann Heinrich Moritz: *Geschichte der Technologie seit der Wiederherstellung der Wissenschaften bis an das Ende des achtzehnten Jahrhundert: Erster Band*, Röwer, Göttingen, 1807. – Georg Olms Verlag, Hildesheim etc., 1999.

Postel, Jonathan Bruce: 'RFC 808: Summary of computer mail services meeting held at BBN on 10 January 1979', *Request for Comments*, no. 808, Network Working Group, 1 March 1982. <http://www.rfc-editor.org/rfc/rfc808.txt>

Potts, Colin; and Lara Catledge: 'Collaborative conceptual design: A large software project case study', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 5, no. 4, 1996, pp. 415–445.

Pougès, Claude, et al.: 'Conception de collecticiels pour l'aide à la prise de décision en situation d'urgence: La nécessité d'une approche pluridisciplinaire et intégrée', in B. Pavard (ed.): *Systèmes coopératifs: De la modélisation à la conception* Octares Éditions, Toulouse, 1994, pp. 351–375.

Powell, Walter W.: 'Neither market nor hierarchy: Network forms of organization', in B. M. Staw and L. L. Cummings (eds.): *Research in Organizational Behavior* vol. 12, JAI Press, Greenwich, Conn., 1989, pp. 295–336.

Prigogine, Ilya; and Isabelle Stengers: *La nouvelle alliance: Metamorphose de la science*, Paris, 1979.

Prinz, Wolfgang: 'TOSCA: Providing organisational information to CSCW applications', in G. De Michelis; C. Simone; and K. Schmidt (eds.): *ECSCW'93: Proceedings of the Third European Conference on Computer-Supported Cooperative Work*, *13–17 September 1993, Milano*, Kluwer Academic Publishers, Dordrecht, 1993, pp. 139–154.

Prinz, Wolfgang: 'NESSIE: An awareness environment for cooperative settings', in S. Bødker; M. Kyng; and K. Schmidt (eds.): *ECSCW'99: Proceedings of the Sixth European Conference on Computer-Supported Cooperative Work*, *12–16 September 1999, Copenhagen*, Kluwer Academic Publishers, Dordrecht, 1999, pp. 391–410.

Prinz, Wolfgang: 'Industry perspective on Collaborative Environments', in I. L. Ballesteros (ed.): *New Collaborative Working Environments 2020: Report on Industry-led FP7 Consultations and 3rd Report of the Experts Group on Collaboration@Work*, European Commisssion, DG Information Society and Media, Bruxelles, February 2006, pp. 5–22.

Pycock, James; and Wes W. Sharrock: 'The fault report form: Mechanisms of interaction in design and development project work', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994, pp. 257–294. COMIC Deliverable D3.2. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Pycock, James: 'Mechanisms of interaction and technologies of representation: Examining a case study', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994, pp. 123–148. COMIC Deliverable D3.2. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Quarterman, John S.: *The Matrix: Computer Networks and Conferencing Systems Worldwide*, Digital Press, Bedford, Mass., 1990.

Randall, David W.; Richard H. R. Harper; and Mark Rouncefield: *Fieldwork for Design: Theory and Practice*, Springer, London, 2007a.

Randall, David W., et al.: 'Ontology building as practical work: Lessons from CSCW', in: *Proceedings of the Third International Conference on e-Social Science*, *7–9 October 2007, Ann Arbor, Michigan*, 2007b.

Rasmussen, Jens: *On the Structure of Knowledge: A Morphology of Mental Models in a Man-Machine Context*, Risø National Laboratory, Roskilde, Denmark, November 1979.

Rasmussen, Jens; and Morten Lind: *Coping with Complexity*, Risø National Laboratory, Roskilde, Denmark, June 1981. – Risø-M-2293, DK-4000 Roskilde, Denmark.

Rasmussen, Jens: 'The role of hierarchical knowledge representation in decisionmaking and system management', *IEEE Transactions on Systems, Man, and Cybernetics* vol. SMC-15, no. 2, March/April 1985, pp. 234–243.

Rasmussen, Jens: 'A cognitive engineering approach to the modelling of decision making and its organization in process control, emergency management, CAD/CAM, office systems, library systems', in W. B. Rouse (ed.): *Advances in Man-Machine Systems Research*, vol. 4, JAI Press, Greenwich, Conn., 1988, pp. 165–243.

Rasmussen, Jens; Berndt Brehmer; and Jacques Leplat (eds.): *Distributed Decision Making: Cognitive Models for Cooperative Work*, Wiley, Chichester, 1991.

Redlow, Götz: *Theoria: Theoretische und praktische Lebensauffassung im philosophischen Denken der Antike*, VEB Deutscher Verlag der Wissenschaften, Berlin, 1966.

Rèaumur, Renè Antoine Ferchault de; Henri Louis Duhamel du Monceau; and Jean-Rodolphe Perronet: *Art de l'èpinglier par M. de Reaumur; avec des additions de M. Duhamel du Monceau, & des remarques extraites des memoires de M. Perronet, inspecteur general des ponts & chaussees*, Saillant et Noyon, Paris, 1761.

Redmond, Kent C.; and Thomas M. Smith: *Project Whirlwind: The History of a Pioneer Computer*, Digital Press, Bedford, Mass., 1980.

Redmond, Kent C.; and Thomas M. Smith: *From Whirlwind to MITRE: The R&D Story of the SAGE Airdefense Computer*, The MIT Press, Cambridge, Mass., and London, 2000.

Reekie, Fraser: *Reekie's Architectural Drawing*, Architectural Press, Amsterdam etc., 1995. Text ed. by T. McCarthy. (4th ed.; 1st ed., 1946).

Reid, Tom R.: *The Chip: How Two Americans Invented the Microchip and Launched a Revolution*, Random House, New York, 2001. (2nd ed.; 1st ed., 1985).

Resnick, Lauren B.; John M. Levine; and Stephanie D. Teasley (eds.): *Perspectives on Socially Shared Cognition*, American Psychological Association, Washington, DC, 1991.

Reynolds, Peter C.: *On the Evolution of Human Behavior: The Argument from Animals to Man*, University of California Press, Berkeley, Calif., 1981.

Rice, Ronald E.: 'Computer-mediated communication and organizational innovation', *Journal of Communication*, vol. 37, no. 4, Autumn 1987, pp. 65–94.

Rice, Ronald E.: 'Computer-mediated communication system network data: Theoretical concerns and emprirical examples', *International Journal of Man-Machine Studies*, vol. 32, no. 6, June 1990, pp. 627–647.

Riordan, Michael; and Lillian Hoddeson: *Crystal Fire: The Invention of the Transistor and the Birth of the Information Age*, W. W. Norton & Co., New York, and London, 1997. (Paperback ed., 1998).

Rittel, Horst W. J.; and Melvin M. Webber: 'Dilemmas in a general theory of planning', *Policy Sciences* vol. 4, 1973, pp. 155–169.

Roberts, Lawrence G.: 'Multiple computer networks and intercomputer communication', in J. Gosden and B. Randell (eds.): *SOSP'67: Proceedings of the First ACM Symposium on Operating System Principles, 1–4 October 1967, Gatlinburg, Tennessee*, ACM Press, New York, 1967, pp. 3.1–3.6.

Roberts, Lawrence G.; and Barry D. Wessler: 'Computer network development to achieve resource sharing', in H. L. Cooke (ed.): *SJCC'70: Proceedings of the Spring Joint Computer Conference, 5–7 May 1970, Atlantic City, New Jersey*, vol. 36, AFIPS Press, Montvale, New Jersey, 1970, pp. 543–549.

Robertson, Toni: *Designing Over Distance: A Study of Cooperative Work, Embodied Cognition and Technology to Enable remote Collaboration*, Submitted for the Degree of Doctor of Philosophy, School of Computing Sciences, University of Technology, Sydney, 1997, 195 pp.

Robertson, Toni: 'The public availability of actions and artefacts', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002.

Robinson, Mike: 'Double-level languages and co-operative working', *AI & Society* vol. 5, 1991, pp. 34–60.

Robinson, Mike; and Liam J. Bannon: 'Questioning representations', in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference* *on Computer-Supported Cooperative Work*, *24–27 September 1991, Amsterdam*, Kluwer Academic Publishers, Dordrecht, 1991, pp. 219–233.

Rochlin, Gene I.; Todd R. La Porte; and Karlene H. Roberts: 'The self-designing high-reliability organization: Aircraft carrier flight operations at sea', *Naval War College Review*, vol. 40, Autumn 1987, pp. 76–90.

Rochlin, Gene I.: 'Informal organizational networking as a crisis-avoidance strategy: U. S. naval flight operations as a case study', *Industrial Crisis Quarterly* vol. 3, 1989, pp. 159–176.

Rodden, Tom A.; and Gordon Blair: 'CSCW and distributed systems: The problem of control', in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference on Computer-Supported Cooperative Work*, *24–27 September 1991, Amsterdam*, Kluwer Academic Publishers, Dordrecht, 1991, pp. 49–64.

Rodden, Tom A.; John A. Mariani; and Gordon Blair: 'Supporting cooperative applications', *Computer Supported Cooperative Work (CSCW): An International Journal* vol. 1, no. 1–2, 1992, pp. 41–68.

Rodden, Tom A.: 'Populating the application: A model of awareness for cooperative applications', in G. M. Olson; J. S. Olson; and M. S. Ackerman (eds.): *CSCW'96: Proceedings of the Conference on Computer-Supported Cooperative Work*, *16–20 November 1996, Boston, Mass.*, ACM Press, New York, 1996, pp. 87–96.

Roethlisberger, Fritz J.; and William J. Dickson: *Management and the Worker*, Harvard University Press, Cambridge, Mass., 1939.

Rogers, Yvonne: 'Coordinating computer-mediated work', *Computer Supported Cooperative Work (CSCW): An International Journal* vol. 1, no. 4, 1993, pp. 295–315.

Rønby Pedersen, Elin; and Tomas Sokoler: 'AROMA: Abstract representation of presence supporting mutual awareness', in S. Pemberton (ed.): *CHI'97 Conference Proceedings: ACM SIGCHI Conference on Human Factors in Computing Systems*, *22–27 March 1997, Atlanta, Georgia*, ACM Press, New York, 1997, pp. 51–58.

Roseman, Mark; and Saul Greenberg: 'TeamRooms: Network places for collaboration', in G. M. Olson; J. S. Olson; and M. S. Ackerman (eds.): *CSCW'96: Proceedings of the Conference on Computer-Supported Cooperative Work*, *16–20 November 1996, Boston, Mass.*, ACM Press, New York, 1996, pp. 325–333.

Roth, Emilie M.; and David D. Woods: 'Cognitive task analysis: An approach to knowledge acquisition for intelligent system design', in G. Guida and C. Tasso (eds.): *Topics in Expert System Design. Methodologies and Tools*, North-Holland, Amsterdam, 1989, pp. 233–264.

Rouse, William B.; and Sandra H. Rouse: 'Measures of complexity of fault diagnosis tasks', *IEEE Transactions on Systems, Man, and Cybernetics*, vol. 9, no. 11, November 1979, pp. 720–727.

Russell, Bertrand: 'The study of mathematics' (1902). in B. Russell: *Mysticism and Logic and Other Essays*. George Allen & Unwin, London, 1917, pp. 47–57. – [First published in *New Quarterly*, November, 1907].

Russell, Bertrand: *The Principles of Mathematics*, 1903. (2nd ed., 1937).

Ryle, Gilbert: *The Concept of Mind*, Hutchinson's University Library, London, 1949.

Ryle, Gilbert: 'The thinking of thoughts: What is "le Penseur" doing?' (*University Lectures*, University of Saskatchewan, 1968). In G. Ryle: *Collected Papers. Volume II: Collected Essays, 1929–1968*. Hutchinson & Co, London, 1971, pp. 480–496.

Sabbagh, Karl: *Skyscraper: The Making of a Building*, Macmillan, London, 1989. – Penguin Books, New York, 1991.

Salus, Peter H.: *Casting the Net: From ARPANET to Internet and Beyond*, Addison-Wesley, Reading, Mass., etc., 1995.

Sandor, Ovidiu; Cristian Bogdan; and John M. Bowers: 'Aether: An awareness engine for CSCW', in J. A. Hughes, et al. (eds.): *ECSCW'97: Proceedings of the Fifth European Conference on Computer-Supported Cooperative Work*, *7–11 September 1997, Lancaster*, Kluwer Academic Publishers, Dordrecht, 1997, pp. 221–236.

Sartre, Jean-Paul: *Critique de la raison dialectique. Tome I*, Gallimard, Paris, 1960.

Savage, Charles M.: *Fifth Generation Management for Fifth Generation Technology (A Round Table Discussion)*, Society of Manufacturing Engineers, Dearborn, Michigan, 1987.

Schaffer, Simon: 'Babbage's calculating engines and the factory system', *Réseaux*, vol. 4, no. 2, 1996, pp. 271–298.

Schäl, Thomas: 'System design for cooperative work in the language action perspective: A case study of The Coordinator', in D. Z. Shapiro; M. Tauber; and R. Traunmüller (eds.): *The Design of Computer Supported Cooperative Work and Groupware Systems*, North-Holland, Elsevier, Amsterdam, 1996, pp. 377–400.

Schick, Kathy D.; and Nicholas Toth: *Making Silent Stones Speak: Human Evolution and the Dawn of Technology*, Simon & Schuster, New York, 1993.

Schmidt, Kjeld: 'Fremmedgørelse og frigørelse' (1970). In K. Marx: *Kritik af den politiske økonomi: Grundrids*. Rhodos, København, 1970, pp. 7–19.

Schmidt, Kjeld; Kay Clausen; and Erling C. Havn: *Computer-integrerede produktionssystemer (CIM), teknologien og dens samfundsmæssige betingelser og konsekvenser* Roskilde University, Roskilde, Denmark, September 1984. – CIM-projektet, working paper, no. 1. <http://cscw.dk/schmidt/papers/cim1984.pdf>

Schmidt, Kjeld: 'A dialectical approach to functional analysis of office work', in: *IEEE International Conference on Systems, Man, and Cybernetics*, 14–17 October 1986, Atlanta, Georgia, IEEE Press, New York, 1986, pp. 1586–1591.

Schmidt, Kjeld: *Teknikerens arbejde og teknikerens arbejdsplads*, Dansk Datamatik Center, Lyngby, Denmark, December 1987. TIA projektet. <http://cscw.dk/schmidt/papers/tia.pdf>

Schmidt, Kjeld: 'Functional analysis instrument', in G. Schäfer, et al. (eds.): *Functional Analysis of Office Requirements. A Multiperspective Approach* Wiley, Chichester, 1988a, pp. 261–289.

Schmidt, Kjeld: 'Forms of cooperative work', in H.-J. Bullinger, et al. (eds.): *EURINFO'88: Proceedings of the First European Conference on Information Technology for Organisational Systems* 16–20 May 1988, Athens, Elsevier Science Publishers (North Holland), Amsterdam, 1988b.

Schmidt, Kjeld: 'Cooperative work: A conceptual framework', in: *New Technology, Distributed Decision Making, and Responsibility*, *5–7 May 1988, Bad Homburg, Germany*, 1988c.

Schmidt, Kjeld: *Analysis of Cooperative Work. A Conceptual Framework*, Risø National Laboratory, Roskilde, Denmark, June 1990. – Risø-M-2890.

Schmidt, Kjeld: 'Riding a tiger, or computer supported cooperative work', in L. J. Bannon; M. Robinson; and K. Schmidt (eds.): *ECSCW'91: Proceedings of the Second European Conference on Computer-Supported Cooperative Work*, *24–27 September 1991, Amsterdam*, Kluwer Academic Publishers, Dordrecht, 1991a, pp. 1–16.

Schmidt, Kjeld: 'Computer support for cooperative work in advanced manufacturing', *International Journal of Human Factors in Manufacturing*, vol. 1, no. 4, October 1991b, pp. 303–320.

Schmidt, Kjeld: 'Cooperative work: A conceptual framework', in J. Rasmussen; B. Brehmer; and J. Leplat (eds.): *Distributed Decision Making: Cognitive Models for Cooperative Work*, Wiley, Chichester, 1991c, pp. 75–109.

Schmidt, Kjeld; and Liam J. Bannon: 'Taking CSCW seriously: Supporting articulation work', *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 1, no. 1–2, 1992, pp. 7–40.

Schmidt, Kjeld, et al.: 'Computational mechanisms of interaction: Notations and facilities', in C. Simone and K. Schmidt (eds.): *Computational Mechanisms of Interaction for CSCW*, Computing Department, Lancaster University, Lancaster, October 1993, pp. 109–164. COMIC Deliverable D3.1. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Schmidt, Kjeld: 'Cooperative work and its articulation: Requirements for computer support', *Travail Humain*, vol. 57, no. 4, December 1994a, pp. 345–366.

Schmidt, Kjeld: *Modes and Mechanisms of Interaction in Cooperative Work*, Risø National Laboratory, Roskilde, Denmark, 1994b. – Risø-R-666(EN).

Schmidt, Kjeld: 'The organization of cooperative work: Beyond the "Leviathan" conception of the organization of cooperative work', in J. B. Smith; F. D. Smith; and T. W. Malone (eds.):

CSCW'94: Proceedings of the Conference on Computer-Supported Cooperative Work, 24–26 October 1994, Chapel Hill, North Carolina, ACM Press, New York, 1994c, pp. 101–112.

Schmidt, Kjeld: 'Mechanisms of interaction reconsidered', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994d, pp. 15–122. COMIC Deliverable D3.2. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Schmidt, Kjeld (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994e. COMIC Deliverable D3.2. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Schmidt, Kjeld, et al.: *A 'Contrat Sociale' for CSCW Systems: Supporting Interoperability of Computational Coordination Mechanisms*, Centre for Cognitive Science, Roskilde University, Roskilde, Denmark, 1995. *Working Papers in Cognitive Science and HCI*. – WPCS-95-7.

Schmidt, Kjeld; and Tom A. Rodden: 'Putting it all together: Requirements for a CSCW platform', in D. Z. Shapiro; M. Tauber; and R. Traunmüller (eds.): *The Design of Computer Supported Cooperative Work and Groupware Systems*, North-Holland Elsevier, Amsterdam, 1996, pp. 157–176. – Originally published at the Workshop on CSCW Design, Schärding, Austria, 1–3 June 1993.

Schmidt, Kjeld; and Carla Simone: 'Coordination mechanisms: Towards a conceptual foundation of CSCW systems design', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 5, no. 2–3, 1996, pp. 155–200.

Schmidt, Kjeld: 'Of maps and scripts: The status of formal constructs in cooperative work', in S. C. Hayne and W. Prinz (eds.): *GROUP'97: Proceedings of the ACM SIGGROUP Conference on Supporting Group Work, 16–19 November 1997, Phoenix, Arizona*, ACM Press, New York, 1997, pp. 138–147.

Schmidt, Kjeld: 'The critical role of workplace studies in CSCW', in P. Luff; J. Hindmarsh; and C. C. Heath (eds.): *Workplace Studies: Recovering Work Practice and Informing System Design*, Cambridge University Press, Cambridge, 2000, pp. 141–149.

Schmidt, Kjeld; and Carla Simone: 'Mind the gap! Towards a unified view of CSCW', in R. Dieng, et al. (eds.): *Designing Cooperative Systems: The Use of Theories and Models. Proceedings of the Fourth International Conference on the Design of Cooperative Systems (COOP 2000) [23–26 May 2000, Sophia Antipolis]*, IOS Press, Amsterdam etc., 2000, pp. 205–221.

Schmidt, Kjeld; and Ina Wagner: 'Ordering systems in architectural design and planning: A discussion of classification systems and practices', in G. C. Bowker; L. Gasser; and B. Turner (eds.): *Workshop on Infrastructures for Distributed Collective Practice, 6–9 February 2002, San Diego*, 2002a.

Schmidt, Kjeld; and Ina Wagner: 'Coordinative artifacts in architectural practice', in M. Blay-Fornarino, et al. (eds.): *Designing Cooperative Systems: A Challenge of the Mobility Age.* [*Proceedings of the Fifth International Conference on the Design of Cooperative Systems (COOP 2002), 4–7 June 2002, Saint Raphaël]*, IOS Press, Amsterdam etc., 2002b, pp. 257–274.

Schmidt, Kjeld: 'Remarks on the complexity of cooperative work', *Revue des sciences et technologies de l'information. Série Revue d'intelligence artificielle (RSTI-RAI)*, vol. 16, no. 4–5, Paris, 2002a, pp. 443–483.

Schmidt, Kjeld: 'The problem with "awareness": Introductory remarks on "Awareness in CSCW"', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002b, pp. 285–298.

Schmidt, Kjeld; and Ina Wagner: 'Ordering systems: Coordinative practices and artifacts in architectural design and planning', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 13, no. 5–6, 2004, pp. 349–408.

Schmidt, Kjeld; Ina Wagner; and Marianne Tolar: 'Permutations of cooperative work practices: A study of two oncology clinics', in T. Gross, et al. (eds.): *GROUP 2007: International Conference on Supporting Group Work, 4–7 November 2007, Sanibel Island, Florida*, ACM Press, New York, 2007, pp. 1–10.

Schmidt, Kjeld: ‘Divided by a common acronym: On the fragmentation of CSCW’, in I. Wagner, et al. (eds.): *ECSCW 2009: Proceedings of the Eleventh European Conference on Computer-Supported Cooperative Work, 7–11 September 2009, Vienna*, Springer, London, 2009, pp. 223–242.

Schmidt, Kjeld: ‘“Keep up the good work!”: The concept of “work” in CSCW’, in M. Lewkowicz, et al. (eds.): *COOP 2010: Ninth International Conference on the Design of Cooperative Systems, 18–21 May 2010, Aix-en-Provence, France*, Springer, London, 2010, pp. 265–286.

Schneider, Karin; and Ina Wagner: ‘Constructing the “dossier representatif”: Computer-based information-sharing in French hospitals’, *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 1, no. 4, 1993, pp. 229–253.

Schonberger, Richard J.: *Japanese Manufacturing Techniques: Nine Hidden Lessons in Simplicity*, Free Press, New York, 1982.

Schümmer, Till; and Stephan Lukosch: *Patterns for Computer-Mediated Interaction*, Wiley, Chichester, England, 2007.

Schütz, Alfred [A. Schutz]: ‘The problem of rationality in the social world’ (*Economica*, 1943). Text ed. by A. Brodersen. In A. Schutz: *Collected Papers. Vol. II. Studies in Social Theory*. Martinus Nijhoff, The Hague, 1964, pp. 64–90.

Schütz, Alfred [A. Schutz]: ‘On multiple realities’ (*Philosophy and Phenomenological Research*, June 1945). Text ed. by M. Natanson. In A. Schutz: *Collected Papers. Vol. I. The Problem of Social Reality*. Martinus Nijhoff, The Hague, 1962, pp. 207–259.

Schütz, Alfred [A. Schutz]: ‘The well-informed citizen: An essay on the social distribution of knowledge’ (*Social Research*, 1946). Text ed. by A. Brodersen. In A. Schutz: *Collected Papers. Vol. II. Studies in Social Theory*. Martinus Nijhoff, The Hague, 1964, pp. 120–134.

Schütz, Alfred [A. Schutz]: *Reflections on the Problem of Relevance* (Manuscript, 1947–1951). Text ed. by R. M. Zaner. Yale University Press, New Haven, 1970.

Schütz, Alfred [A. Schutz]: ‘Common-sense and scientific interpretations of human action’ (*Philosophy and Phenomenological Research*, September 1953). Text ed. by M. Natanson. In A. Schutz: *Collected Papers. Vol. I. The Problem of Social Reality*. Martinus Nijhoff, The Hague, 1962, pp. 3–47.

Schütz, Alfred [A. Schutz]: *The Phenomenology of the Social World*, Northwestern University Press, Evanston, Ill, 1967. Transl. from *Der sinnhafte Aufbau der sozialen Welt; eine Einleitung in die Verstehende Soziologie*, J. Springer, Wien, 1932. Transl. by G. Walsh and F. Lehnert.

Searle, John R.: ‘Minds and brains without programs’, in C. Blakemore and S. Greenfield (eds.): *Mindwaves: Thoughts on Intelligence, Identity and Consciousness*, Basil Blackwell, Oxford, 1987, pp. 209–233. (Paperback ed., 1989).

Searle, John R.: *Minds, Brains and Science: The 1984 Reith Lectures*, Pelican Books, London, 1989.

Sellen, Abigail; and Richard H. R. Harper: *The Myth of the Paperless Office*, MIT Press, Cambridge, Mass., 2001.

Selznick, Philip: ‘Foundations of the theory of organization’, *American Sociological Review*, vol. 13, 1948, pp. 25–35.

Shanker, Stuart G. (ed.): *Ludwig Wittgenstein: Critical Assessments: Volume Three: From the Tractatus to Remarks on the Foundations of Mathematics: Wittgenstein on the Philosophy of Mathematics*, Croom Helm, London, 1986a.

Shanker, Stuart G.: ‘Introduction: The portals of discovery’, in S. G. Shanker (ed.): *Ludwig Wittgenstein: Critical Assessments: Volume Three: From the Tractatus to Remarks on the Foundations of Mathematics: Wittgenstein on the Philosophy of Mathematics*, Croom Helm, London, 1986b, pp. 1–25.

Shanker, Stuart G.: ‘Introduction: The nature of philosophy’, in S. G. Shanker (ed.): *Ludwig Wittgenstein: Critical Assessments: Volume Four: From Theology to Sociology: Wittgenstein’s Impact on Contemporary Thought*, Croom Helm, London, 1986c, pp. 1–28.

Shanker, Stuart G.: *Wittgenstein and the Turning-Point in the Philosophy of Mathematics*, Croom Helm, London and Sydney, 1987a.

Shanker, Stuart G.: 'AI at the crossroads', in B. P. Bloomfield (ed.): *The Question of Artificial Intelligence: Philosophical and Sociological Perspectives*, Croom Helm, London, etc., 1987b, pp. 1–58.

Shanker, Stuart G.: 'The decline and fall of the mechanist metaphor', in R. Born (ed.): *Artificial Intelligence: The Case Against*, Routledge, London and New York, 1987c, pp. 72–131.

Shanker, Stuart G.: 'Wittgenstein versus turing on the nature of Church's thesis', *Notre Dame Journal of Formal Logic*, vol. 28, no. 4, October 1987d, pp. 615–649.

Shanker, Stuart G.: 'Turing and the origins of AI', *Philosophia Mathematica*, vol. 3, no. 1, 1995, pp. 52–86.

Shanker, Stuart G.: *Wittgenstein's Remarks on the Foundation of AI*, Routledge, London, 1998.

Shannon, Claude E.: 'A mathematical theory of communication', *The Bell System Technical Journal*, vol. 27, July, October 1948, pp. 379–423, 623–656.

Shannon, Claude E.; and Warren Weaver: *The Mathematical Theory of Communication*, University of Illinois Press, Urbana, 1949.

Shapiro, Dan Z., et al.: 'Visual re-representation of database information: The flight data strip in air traffic control', in M. Tauber; D. E. Mahling; and F. Arefi (eds.): *Cognitive Aspects of Visual Languages and Visual Interfaces*, Elsevier Science, Amsterdam, 1994, pp. 349–376.

Sharrock, Wes W.; and Robert J. Anderson: *The Ethnomethodologists*, Ellis Horwood Publishers, Chichester, 1986.

Sharrock, Wes W.; and Rupert Read: *Kuhn: Philosopher of Scientific Revolution*, Polity Press, Cambridge, 2002.

Sharrock, Wes W.; and Graham Button: '*Plans and Situated Action* ten years on', *The Journal of the Learning Sciences*, vol. 12, no. 2, 2003, pp. 259–264.

Sheil, Beau A.: 'Coping with complexity', *Office: Technology and People*, vol. 1, no. 4, January 1983, pp. 295–320.

Shen, Weiming, et al. (eds.): *CSCWD 2007: Computer Supported Cooperative Work in Design, IV: Eleventh International Conference*, *26–28 April 2007, Melbourne, Australia*, Springer, New York, 2008.

Shepherd, Allan; Niels Mayer; and Allan Kuchinsky: 'Strudel: An extensible electronic conversation toolkit', in T. Bikson and F. Halasz (eds.): *CSCW'90: Proceedings of the Conference on Computer-Supported Cooperative Work*, *7–10 October 1990, Los Angeles, Calif.*, ACM Press, New York, 1990, pp. 93–104.

Shneiderman, Ben: 'Direct manipulation: A step beyond programming languages', *IEEE Computer*, vol. 16, no. 8, August 1983, pp. 57–69.

Simon, Herbert A.: 'The architecture of complexity' (*Proceedings of the American Philosophical Society*, *December 1962*). in H. A. Simon: *The Sciences of the Artificial*. MIT Press, Cambridge, Mass., 1969, pp. 192–229.

Simon, Herbert A.: 'How big is a chunk?' (*Science*, 1974). in H. A. Simon (ed.): *Models of Thought*. Yale University Press, New Haven, and London, 1979, pp. 50–61.

Simon, Herbert A.: *Models of Thought*, Yale University Press, New Haven, and London, 1979.

Simone, Carla; and Kjeld Schmidt (eds.): *Computational Mechanisms of Interaction for CSCW*, Computing Department, Lancaster University, Lancaster, October 1993. COMIC Deliverable D3.1. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Simone, Carla: 'Integrating co-operative work supports from CHAOS perspective (abstract)', *SIGOIS Bulletin*, vol. 13, no. 4, 1993, p. 25.

Simone, Carla; and Kjeld Schmidt (eds.): *A Notation for Computational Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994. COMIC Deliverable D3.3. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Simone, Carla, et al.: 'A multi-agent approach to the design of coordination mechanisms', in V. Lesser (ed.): *Proceedings of the First International Conference on Multi-Agent Systems*, *12–14 June 1995, San Francisco, Calif.*, AAAI Press, Menlo Park, Calif., 1995a.

Simone, Carla; Monica Divitini; and Kjeld Schmidt: 'A notation for malleable and interoperable coordination mechanisms for CSCW systems', in N. Comstock, et al. (eds.): *COOCS'95:*

Conference on Organizational Computing Systems, *13–16 August 1995, Milpitas, California*, ACM Press, New York, 1995b, pp. 44–54.

Simone, Carla; and Stefania Bandini: 'Compositional features for promoting awareness within and across cooperative applications', in S. C. Hayne and W. Prinz (eds.): *GROUP'97: Proceedings of the ACM SIGGROUP Conference on Supporting Group Work*, *16–19 November 1997, Phoenix, Arizona*, ACM Press, New York, 1997, pp. 358–367.

Simone, Carla; and Monica Divitini: 'Ariadne: Supporting coordination through a flexible use of the knowledge on work processes', *Journal of Universal Computer Science*, vol. 3, no. 8, 1997, pp. 865–898.

Simone, Carla; and Kjeld Schmidt: 'Taking the distributed nature of cooperative work seriously', in: *Proceedings of the Sixth Euromicro Workshop on Parallel and Distributed Processing*, *21–23 January 1998, Madrid*, IEEE Computer Society Press, Los Alamitos, Calif., 1998, pp. 295–301.

Simone, Carla; and Monica Divitini: 'Integrating contexts to support coordination: The CHAOS project', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 8, no. 3, September 1999, pp. 239–283.

Simone, Carla; and Stefania Bandini: 'Integrating awareness in cooperative applications through the reaction-diffusion metaphor', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 11, no. 3–4, 2002.

Sluizer, S.; and P. Cashman: 'XCP: An experimental tool for supporting office procedures', in: *Proceedings of the First International Conference on Office Automation*, *17–19 December 1984, New Orleans, Louisiana*, IEEE Computer Society Press, Silver Spring, Maryland, 1984, pp. 73–80.

Smith, Adam: *An Inquiry into the Nature and Causes of the Wealth of Nations*, vol. 1–2, Printed for W. Strahan; and T. Cadell, in the Strand, London, 1776a.

Smith, Adam: *An Inquiry into the Nature and Causes of the Wealth of Nations*, Printed for W. Strahan; and T. Cadell, Strand, London, 1776b. Text ed. by E. Cannan. – The Modern Library, New York, 1994.

Smith, David Canfield, et al.: 'Designing the Star user interface', *Byte*, vol. 7, April 1982a, pp. 242–282.

Smith, David Canfield, et al.: 'The Star user interface: An overview', in R. K. Brown and H. L. Morgan (eds.): *AFIPS'82: Proceedings of the National Computer Conference*, *7–10 June 1982, Houston, Texas*, AFIPS Press, Arlington, Virginia, 1982b, pp. 515–528.

Smith, Hugh T.; P. A. Hennessy; and G. A. Lunt: 'An object-oriented framework for modelling organizational communication', in J. M. Bowers and S. D. Benford (eds.): *Studies in Computer Supported Cooperative Work: Theory, Practice and Design*, North-Holland, Amsterdam, 1991, pp. 145–157.

Smith, Hugh T.: 'The requirements for group communication services', in R. Speth (ed.): *EUTECO'88: European Teleinformatics Conference on Research into Networks and Distributed Applications*, *20–22 April 1988, Vienna, Austria. Organized by the European Action in Teleinformatics COST 11ter*, North-Holland, Brussels and Luxemburg, 1988, pp. 89–95.

Snedecor, George W.: 'Uses of punched card equipment in mathematics', *The American Mathematical Monthly*, vol. 35, no. 4, April 1928, pp. 161–169.

Sombart, Werner: *Der moderne Kapitalismus: Historisch-systematische Darstellung des gesamteuropäischen Wirtschaftslebens von seinen Anfängen bis zur Gegenwart: Band II: Das europäische Wirtschaftsleben im Zeitalter des Frühkapitalismus*, Duncker & Humblot, München and Leipzig, 1916. (2nd ed.; 1st ed., 1902). – Deutscher Taschenbuch Verlag, München, 1987.

Sørensen, Carsten: 'The augmented bill of materials', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994a, pp. 221–236. COMIC Deliverable D3.2. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Sørensen, Carsten: 'The CEDAC board', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994b, pp. 237–245. COMIC Deliverable D3.2. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Sørensen, Carsten: 'The product classification scheme', in K. Schmidt (ed.): *Social Mechanisms of Interaction*, Computing Department, Lancaster University, Lancaster, September 1994c, pp. 247–255. <ftp://ftp.comp.lancs.ac.uk/pub/comic>

Sørgaard, Pål: 'A cooperative work perspective on use and development of computer artifacts', in: *Proceedings of the Tenth Information Systems Research Seminar in Scandinavia (IRIS), 10–12 August 1987, Vaskivesi*, 1987.

Spear, Steven; and H. Kent Bowen: 'Decoding the DNA of the Toyota Production System', *Harvard Business Review*, vol. 77, September–October 1999, pp. 96–106.

Speth, Rolf (ed.): *EUTECO '88: European Teleinformatics Conference on Research into Networks and Distributed Applications, 20–22 April 1988, Vienna, Austria. Organized by the European Action in Teleinformatics COST 11ter*, North-Holland, Amsterdam etc., 1988.

Sproull, Lee; and Sara Kiesler: *Connections: New Ways of Working the the Networked Organization*, The MIT Press, Cambridge, Mass., 1991.

Standage, Tom: *The Victorian Internet: The Remarkable Story of the Telegraph and the Nineteenth Century's On-line Pioneers*, Walker Publishing Co., New York, 1998. (Paperback ed., 2007).

Star, Susan Leigh; and James R. Griesemer: 'Institutional ecology, "translations" and boundary objects: Amateurs and professionals in Berkeley's Museum of Vertebrate Zoology, 1907–1939', *Social Studies of Science*, vol. 19, 1989, pp. 387–420.

Star, Susan Leigh: 'The structure of ill-structured solutions: Boundary objects and heterogeneous distributed problem solving', in L. Gasser and M. Huhns (eds.): *Distributed Artificial Intelligence*, vol. 2, Pitman, London, 1989, pp. 37–54.

Stefik, M., et al.: 'WYSIWIS revised: Early experiences with multiuser interfaces', *ACM Transactions on Office Information Systems*, vol. 5, no. 2, April 1987, pp. 147–167.

Stinchcombe, Arthur L.: *Creating Efficient Industrial Administrations*, Academic Press, New York, and London, 1974.

Stinchcombe, Arthur L.: *Information and Organizations*, University of California Press, Berkeley, Calif., 1990.

Storrs, Graham: 'Group working in the DHSS large demonstrator project', in P. Wilson; J. M. Bowers; and S. D. Benford (eds.): *ECSCW'89: Proceedings of the First European Conference on Computer Supported Cooperative Work, 13–15 September 1989, Gatwick, London*, 1989, pp. 102–119.

Strauss, Anselm L.: 'Work and the division of labor', *The Sociological Quarterly*, vol. 26, no. 1, 1985, pp. 1–19.

Strauss, Anselm L., et al.: *Social Organization of Medical Work*, University of Chicago Press, Chicago, and London, 1985.

Strauss, Anselm L.: 'The articulation of project work: An organizational process', *The Sociological Quarterly*, vol. 29, no. 2, 1988, pp. 163–178.

Strauss, Anselm L.: *Continual Permutations of Action*, Aldine de Gruyter, New York, 1994.

Styles, Keith: *Working Drawings Handbook*, Architectural Press, Amsterdam etc., 1995. (3rd ed.; 1st ed., 1982).

Suchman, Lucy A.: 'Systematics of office work: Office studies for knowledge-based systems. Digest', in: *Office Automation Conference, 5–7 April 1982, San Francisco*, 1982, pp. 409–412.

Suchman, Lucy A.: 'Office procedure as practical action: Models of work and system design', *ACM Transactions on Office Information Systems*, vol. 1, no. 4, October 1983, pp. 320–328.

Suchman, Lucy A.; and Eleanor Wynn: 'Procedures and problems in the office', *Office: Technology and People*, vol. 2, 1984, pp. 133–154.

Suchman, Lucy A.: *Plans and Situated Actions: The Problem of Human-Machine Communication*, Cambridge University Press, Cambridge, 1987.

Suchman, Lucy A.: *Notes on Computer Support for Cooperative Work*, Department of Computer Science, University of Jyväskylä, Jyväskylä, May 1989. – WP-12.

Suchman, Lucy A.; and Randall H. Trigg: 'Understanding practice: Video as a medium for reflection and design', in J. Greenbaum and M. Kyng (eds.): *Design at Work: Cooperative Design of Computer Systems*, Lawrence Erlbaum, Hillsdale, New Jersey, 1991, pp. 65–89.

Suchman, Lucy A.: 'Response to Vera and Simon's situated action: A symbolic interpretation', *Cognitive Science*, vol. 17, no. 1, January–March 1993a, pp. 71–75.

Suchman, Lucy A.: 'Technologies of accountability: On lizards and airplanes', in G. Button (ed.): *Technology in Working Order. Studies of Work, Interaction, and Technology*, Routledge, London and New York, 1993b, pp. 113–126.

Suchman, Lucy A.: 'Writing and reading: A response to comments on *Plans and Situated Actions*', *The Journal of the Learning Sciences*, vol. 12, no. 2, 2003, pp. 299–306.

Suchman, Lucy A.: *Human-Machine Reconfigurations: Plans and Situated Actions*, 2nd ed., Cambridge University Press, New York, 2007.

Sutherland, Ivan Edward: 'Sketchpad: A man-machine graphical communication system', in E. C. Johnson (ed.): *SJCC'63: Proceedings of the Spring Joint Computer Conference, 21–23 May 1963, Detroit, Michigan*, vol. 23, AFIPS Press, Santa Monica, California, 1963, pp. 329–346.

Swade, Doron: *The Cogwheel Brain: Charles Babbage and the Quest to Build the First Computer*, Little, Brown & Co., London, 2000. – Abacus, London, 2001 (Paperback ed.).

Swade, Doron: 'The "unerring certainty of mechanical agency": Machines and table making in the nineteenth century', in M. Campbell-Kelly, et al. (eds.): *The History of Mathematical Tables: From Sumer to Spreadsheets*, Oxford University Press, Oxford, 2003, pp. 145–176. (Reprinted 2007).

Swenson, K. D., et al.: 'A business process environment supporting collaborative planning', *Collaborative Computing*, vol. 1, no. 1, March 1994, pp. 15–24.

Symon, Gillian; Karen Long; and Judi Ellis: 'The coordination of work activities: Cooperation and conflict in a hospital context', *Computer Supported Cooperative Work (CSCW): The Journal of Collaborative Computing*, vol. 5, no. 1, 1996, pp. 1–31.

Syri, Anja: 'Tailoring cooperation support through mediators', in J. A. Hughes, et al. (eds.): *ECSCW'97: Proceedings of the Fifth European Conference on Computer-Supported Cooperative Work, 7–11 September 1997, Lancaster*, Kluwer Academic Publishers, Dordrecht, 1997, pp. 157–172.

Tait, William W.: *The Provenance of Pure Reason: Essays in the Philosophy of Mathematics and Its History*, Oxford University Press, Oxford, 2005.

Tang, John C.: 'Findings from observational studies of collaborative work', *International Journal of Man-Machine Studies*, vol. 34, 1991, pp. 143–160.

Taylor, Frederick Winslow: *The Principles of Scientific Management*, Harper & Brothers, New York, 1911. – W. W. Norton & Co., New York, 1967.

Taylor, Talbot J.: *Mutual Misunderstanding: Scepticism and the Theorizing of Language and Interpretation*, Duke University Press, Durham, and London, 1992.

Taylor, Talbot J.: *Theorizing Language: Analysis, Normativity, Rhetoric, History*, Pergamon, Amsterdam etc., 1997.

Taylor, Talbot J.: 'Language constructing language: The implications of reflexivity for linguistic theory', *Language Sciences*, vol. 22, 2000, pp. 483–499.

Thacker, Charles P., et al.: *Alto: A Personal Computer*, Xerox PARC, 7 August 1979. – CSL-79–11. (Reprinted February 1984). <http://www.scribd.com/doc/2183225/Alto-A-Personal-Computer>

Thacker, Charles P.: 'Personal distributed computing: The Alto and Ethernet hardware', in A. Goldberg (ed.): *A History of Personal Workstations*, Addison-Wesley, Reading, Mass., 1988, pp. 267–289.

The OSI e-Infrastructure Working Group: *Developing the UK's E-infrastructure for Science and Innovation*, Office of Science and Innovation (OSI), 2004. <http://www.nesc.ac.uk/documents/OSI/report.pdf>

Thompson, James D.: *Organizations in Action: Social Science Base of Administrative Theory*, McGraw-Hill, New York, 1967.

Thorngate, Warren: 'Got a minute? How technology affects the economy of attention [Abstract of closing plenary talk]', in W. A. Kellogg and S. Whittaker (eds.): *CSCW 2000: ACM Conference* *on Computer Supported Cooperative Work, 2–6 December 2000, Philadelphia, Pennsylvania, USA*, 2000.

Tomlinson, Raymond S.: 'The first network email', 2001. <http://openmap.bbn.com/~tomlinso/ray/firstemailframe.html>

Trevor, Jonathan; Tom A. Rodden; and Gordon Blair: 'COLA: A lightweight platform for CSCW', in G. De Michelis; C. Simone; and K. Schmidt (eds.): *ECSCW'93: Proceedings of the Third European Conference on Computer-Supported Cooperative Work, 13–17 September 1993, Milano*, Kluwer Academic Publishers, Dordrecht, 1993, pp. 15–30.

Trevor, Jonathan; Tom A. Rodden; and Gordon Blair: 'COLA: A lightweight platform for CSCW', *Computer Supported Cooperative Work (CSCW): An International Journal*, vol. 3, no. 2, 1995, pp. 197–224.

Turing, Alan M.: 'On computable numbers with an application to the Entscheidungsproblem', *Proceedings of the London Mathematical Society*, vol. 42 (Series 2), no. 3–4, November–December 1936, pp. 230–265.

Turing, Alan M.: 'Proposed electronic calculator (1945)' (Report, 1945). in B. J. Copeland (ed.) *Alan Turing's Automatic Computing Engine: The Master Codebreaker's Struggle to Build the Modern Computer*. Oxford University Press, Oxford, 2005, pp. 369–454. – [Written late 1945; submitted to the Executive Committee of the NPL February 1946 as 'Report by Dr. A. M. Turing on Proposals for the Development of an Automatic Computing Engine (ACE)'].

Turing, Alan M.; and James H. Wilkinson: 'The Turing-Wilkinson lecture series (1946–1947)' (Lectures, London, 1946–1947). in B. J. Copeland (ed.) *Alan Turing's Automatic Computing Engine: The Master Codebreaker's Struggle to Build the Modern Computer*. Oxford University Press, Oxford, 2005, pp. 459–527.

Turing, Alan M.: 'Lecture on Automatic Computing Engine' (London Mathematical Society, 20 February 1947). in B. J. Copeland (ed.) *The Essential Turing: Seminal Writings in Computing, Logic, Philosophy, Artificial Intelligence, and Artificial Life Plus the Secrets of Enigma*, Oxford University Press, Oxford, 2004, pp. 378–394.

Turnbull, David: 'The ad hoc collective work of building Gothic cathedrals with templates, string, and geometry', *Science, Technology, & Human Values*, vol. 18, no. 3, Summer 1993, pp. 315–340.

Turoff, Murray: 'Delphi and its potential impact on information systems', in R. R. Wheeler (ed.): *FJCC'71: Proceedings of the Fall Joint Computer Conference, 16–18 November 1971, Las Vegas, Nevada*, vol. 39, AFIPS Press, Montvale, New Jersey, 1971, pp. 317–326.

Turoff, Murray: 'Delphi conferencing: Computer-based conferencing with anonymity', *Technological Forecasting and Social Change*, vol. 3, 1972, pp. 159–204.

Turoff, Murray: 'Human communication via data networks', *SIGCAS Computers and Society*, vol. 4, no. 1, May 1973, pp. 15–24.

Turoff, Murray: 'Computer-mediated communication requirements for group support', *Journal of Organizational Computing*, vol. 1, 1991, pp. 85–113.

Uhlig, Ronald P.: 'Human factors in computer message systems', *Datamation*, vol. 23, no. 5, May 1977, pp. 120–126.

Ure, Andrew: *The Philosophy of Manufactures: Or, an Exposition of the Scientific, Moral, and Commercial Economy of the Factory System of Great Britain*, Charles Knight, London, 1835. – Frank Cass & Co., London, 1967.

Urwick, Lyndall F.; and Edward F. L. Brech: *The Making of Scientific Management. Volume I: Thirteen Pioneers*, Sir Isaac Pitman and Sons, London, 1945. (Rev. ed., 1957).

Urwick, Lyndall F.; and Edward F. L. Brech: *The Making of Scientific Management. Volume II: Management in British Industry*, Management Publications Trust, London, 1946.

Urwick, Lyndall F.; and Edward F. L. Brech: *The Making of Scientific Management. Volume III: The Hawthorne Investigations*, Management Publications Trust, London, 1948.

Vallee, Jacques F.: *Group Communication through Computers. Volume 1: Design and Use of the FORUM System*, Institute for the Future, Menlo Park, Calif., July 1974. – IFF Report R-32.

van der Aalst, Wil M.P.: 'Exploring the CSCW spectrum using process mining', *Advanced Engineering Informatics*, vol. 21, 2007, pp. 191–199.

Van der Spiegel, Jan, et al.: 'The ENIAC: History, operation, and reconstruction in VLSI', in R. Rojas and U. Hashagen (eds.): *The First Computers: History and Architectures*, The MIT Press, Cambridge, Mass., and London, 2000, pp. 121–178.

Van Vleck, Tom: 'The history of electronic mail', 1 February 2001. Latest update, 25 May 2008. <http://www.multicians.org/thvv/mail-history.html>

Vicente, Kim J.; and Jens Rasmussen: 'Ecological interface design: Theoretical foundations', *IEEE Transactions on Systems, Man, and Cybernetics*, vol. SMC-22, no. 4, 1992, pp. 589–606.

Victor, Frank; and Edgar Sommer: 'Supporting the design of office procedures in the DOMINO system', in P. Wilson; J. M. Bowers; and S. D. Benford (eds.): *ECSCW'89: Proceedings of the First European Conference on Computer Supported Cooperative Work*, *13–15 September 1989, Gatwick, London*, 1989, pp. 148–159.

Victor, Frank; and Edgar Sommer: 'Supporting the design of office procedures in the DOMINO system', in J. M. Bowers and S. D. Benford (eds.): *Studies in Computer Supported Cooperative Work: Theory, Practice and Design*, North-Holland, Amsterdam, 1991, pp. 119–130.

Von Glinow, Mary Ann; and Susan Albers Mohrman (eds.): *Managing Complexity in High Technology Organizations*, Oxford University Press, New York, and Oxford, 1990.

von Neumann, John: *The First Draft Report on the EDVAC*, Moore School of Electrical Engineering, University of Pennsylvania, 30 June 1945. – Contract No. W–670–ORD–4926 Between the United States Army Ordnance Department and the University of Pennsylvania. Text ed. by M. D. Godfrey. <http://qss.stanford.edu/~godfrey/vonNeumann/vnedvac.pdf>

von Neumann, John: 'The mathematician', *Works of the Mind*, vol. 1, no. 1, 1947, pp. 180–196.

von Savigny, Eike: *Die Philosophie der normalen Sprache: Eine kritische Einführung in die 'ordinary language philosophy'*, Suhrkamp, Frankfurt a. M., 1974. (2nd ed., 1st. ed., 1969).

von Savigny, Eike: *Wittgensteins 'Philosophische Untersuchungen': Ein Kommentar für Leser. Band 1: Abschnitte 1–315*, Vittorio Klostermann, Frankfurt a. M., 1994. (2nd ed.; 1st ed., 1988).

Vygotskij, Lev Semenovič [L. S. Vygotsky]: 'The instrumental method in psychology' (Manuscript, 1930). In L. S. Vygotskij: *The Collected Works of L. S. Vygotsky. Volume 3: Problems of the Theory and History of Psychology*. Plenum Press, New York, and London, 1997, pp. 85–89. – [Theses of a talk read in 1930 at the N. K. Krupskaya Academy of Communist Education].

Wagner, Ina; and Rainer Lainer: 'Open planning: Inspirational objects, themes, placeholders, and persuasive artefacts', in: *Proceedings Colloque Architecture des systèmes urbains*, *5 July 2001, Université de Technologie de Compiègne*, Compiègne, 2001.

Waismann, Friedrich: 'The nature of mathematics: Wittgenstein's standpoint' (Königsberg, 1930). Transl. from 'Über das Wesen der Mathematik: Der Standpunkt Wittgensteins'. Transl. by S. G. Shanker. In S. G. Shanker (ed.) *Ludwig Wittgenstein: Critical Assessments: Volume Three: From the Tractatus to Remarks on the Foundations of Mathematics: Wittgenstein on the Philosophy of Mathematics*. Croom Helm, London, 1986, pp. 60–67.

Waismann, Friedrich: *Introduction to Mathematical Thinking: The Formation of Concepts in Modern Mathematics*, Hafner Publishing Co., London, 1951. Transl. from *Einführung in das mathematische Denken: Die Begriffsbildung der modernen Mathematik*, Gerold u. Co., Wien, 1936. Transl. by T. J. Benac.

Wakati, Osamu A.; and Richard M. Linde: *Study Guide for the Professional Practice of Architectural Working Drawings*, Wiley, New York, 1994. (2nd ed., Student ed.).

Wakefield, Edward: *A View of the Art of Colonization, with Present Reference to the British Empire; in Letters Between a Stateman and a Colonist*, John W. Parker, London, 1849.

Waldrop, N. Mitchell: *Complexity: The Emerging Science at the Edge of Order and Chaos*, Simon & Schuster, 1992. – Penguin Books, 1994 (Paperback ed.).

Wang, Hao: *From Mathematics to Philosophy*, Routledge & Kegan Paul, London, 1974.

Weber, Max: 'Die wirtschaftlichen Unternehmungen der Gemeinden' (28 September 1909). Text ed. by W. Schluchter. In M. Weber: *Studienausgabe der Max-Weber-Gesamtausgabe*. Mohr Siebeck, Tübingen, 1999, vol. I/8 (*Wirtschaft, Staat & Sozialpolitik: Schriften und Reden, 1900–1912*), pp. 127–130.

Weick, Karl E.; and Karlene H. Roberts: ‘Collective mind in organizations: Heedful interrelating on flight decks’, *Administrative Science Quaterly*, vol. 38, 1993, pp. 357–381.

Wertsch, James V.: *Vygotsky and the Social Formation of Mind*, Harvard University Press, Cambridge, Mass., and London, 1985.

Wertsch, James V.: *Voices of the Mind: A Sociocultural Approach to Mediated Action*, Harvard University Press, Cambridge, Mass., 1991.

White, Alan Richard: *Attention*, Basil Blackwell, Oxford, 1964.

White, Alan Richard: *The Philosophy of Mind*, Random House, New York, 1967.

White, Alan Richard: *The Nature of Knowledge*, Rowman and Littlefield, Totowa, New Jersey, 1982.

Whitehead, Alfred North; and Bertrand Russell: *Principia Mathematica*, Cambridge University Press, Cambridge, 1910. (2nd ed., 1927; Paperback ed. ‘to *56’, 1962).

Whitley, Richard: ‘Cognitive and social institutionalization of scientific specialties and research areas’, in R. Whitley (ed.): *Social Processes of Scientific Development*, Routledge & Kegan Paul, London, 1974, pp. 69–95.

Wiedemann, Thomas: *Greek and Roman Slavery*, Croom Helm, London, 1981. – Routledge, New York, 1988.

Wieser, C. Robert: *Cape Cod System and Demonstration*, MIT Lincoln Laboratory, Division 6, Cambridge, Mass., 13 March 1953. – Project Whirlwind Limited Memorandum VI – L-86. <http://dome.mit.edu/handle/1721.3/41510>

Wieser, C. Robert: ‘The Cape Cod system’, *IEEE Annals of the History of Computing*, vol. 5, no. 4, April–June 1985, pp. 362–369.

Wilczek, Stephan; and Helmut Krcmar: ‘Betriebliche Groupwareplattformen’, in G. Schwabe; N. Streitz; and R. Unland (eds.): *CSCW-Kompendium: Lehr- und Handbuch zum computerunterstützten kooperativen Arbeiten*, Springer, Heidelberg, 2001, pp. 310–320.

Wild, Ray: *Mass-Production Management: The Design and Operation of Production Flow-Line Systems*, Wiley, London, 1972.

Williams, Meredith: *Wittgenstein, Mind and Meaning: Toward a Social Conception of Mind*, Routledge, London and New York, 1999.

Williams, Michael R.: ‘Early calculation’, in W. Aspray (ed.): *Computing Before Computers*, Iowa State University Press, Ames, Iowa, 1990, pp. 3–58.

Williams, Michael R.: ‘Difference engines: From Müller to Comrie’, in M. Campbell-Kelly, et al. (eds.): *The History of Mathematical Tables: From Sumer to Spreadsheets*, Oxford University Press, Oxford, 2003, pp. 123–144. (Reprinted 2007).

Williamson, Oliver E.: *Markets and Hierarchies: A Transactional and Antitrust Analysis of the Firm*, Free Press, New York, 1975.

Williamson, Oliver E.: ‘Transaction-cost economics: The governance of contractual relations’, *Journal of Law and Economics*, vol. 22, no. 2, October 1979, pp. 233–261.

Williamson, Oliver E.: ‘The economics of organization: The transaction cost approach’, *American Journal of Sociology*, vol. 87, no. 3, November 1981, pp. 548–577.

Wilson, Paul: *Computer-Supported Cooperative Work. An Introduction*, Intellect, Oxford, England, 1991.

Winch, Peter: *The Idea of a Social Science and Its Relation to Philosophy*, Routledge & Kegan Paul, London, 1958. (2nd ed., 1990).

Winograd, Terry: ‘A language/action perspective on the design of cooperative work’, in H. Krasner and I. Greif (eds.): *CSCW’86: Proceedings. Conference on Computer-Supported Cooperative Work, 3–5 December 1986, Austin, Texas*, ACM Press, New York, 1986, pp. 203–220.

Winograd, Terry; and Fernando Flores: *Understanding Computers and Cognition: A New Foundation for Design*, Ablex Publishing Corporation, Norwood, New Jersey, 1986.

Wittgenstein, Ludwig: *Wittgenstein and the Vienna Circle: Conversations Recorded by Friedrich Waismann* (1929–1932). Transl. by J. Schulte and B. McGuiness. Text ed. by B. McGuiness. Basil Blackwell, Oxford, 1979.

Wittgenstein, Ludwig: *Philosophical Remarks* (Manuscript, 1930). Transl. from *Philosophische Bemerkungen*. Transl. by R. Hargreaves and R. White. Text ed. by R. Rhees. Basil Blackwell, Oxford, 1975.

Wittgenstein, Ludwig: *Philosophical Grammar* (Manuscript, 1931–1934). Transl. by A. Kenny. Text ed. by R. Rhees. Basil Blackwell, Oxford, 1974 (Paperback ed., 1980).

Wittgenstein, Ludwig: *Philosophy for Mathematicians: Wittgenstein's Lectures, 1932–33. From the Notes of Alice Ambrose* (1932–1933). Text ed. by A. Ambrose. In L. Wittgenstein: *Wittgenstein's Lectures: Cambridge, 1932–1935. From the Notes of Alice Ambrose and Margaret Macdonald*. Basil Blackwell, Oxford, 1979, pp. 203–225.

Wittgenstein, Ludwig: *The Yellow Book: Wittgenstein's Lectures and Informal Discussions During Dictation of the Blue Book 1933–34. From the Notes of Alice Ambrose* (1934–1935). Text ed. by A. Ambrose. In L. Wittgenstein: *Wittgenstein's Lectures: Cambridge, 1932–1935. From the Notes of Alice Ambrose and Margaret Macdonald*. Basil Blackwell, Oxford, 1979, pp. 41–73.

Wittgenstein, Ludwig: *Remarks on the Foundations of Mathematics* (Manuscript, 1937–1944). Transl. from *Bemerkungen über die Grundlagen der der Mathematik*. Transl. by G. E. M. Anscombe. Text ed. by G. H. von Wright; R. Rhees; and G. E. M. Anscombe. Basil Blackwell Publishers, Oxford, 1978.

Wittgenstein, Ludwig: *Lectures on the Foundations of Mathematics, Cambridge, 1939: From the Notes of R. G. Bosanquet, Norman Malcolm, Rush Rhees, and Yorick Smythies* (1939). Text ed. by C. Diamond. The University of Chicago Press, Chicago, and London, 1976.

Wittgenstein, Ludwig: *Philosophical Investigations* (Manuscript, 1945–1946). Transl. from *Philosophische Untersuchungen*. Transl. by G. E. M. Anscombe; P. M. S. Hacker; and J. Schulte. Text ed. by P. M. S. Hacker and J. Schulte. In L. Wittgenstein: *Philosophical Investigations*. Blackwell Publishers, Oxford, 4th ed., 2009, pp. 1–181. – TS 227: Previously published as *Philosphical Investigations, Part 1*.

Wittgenstein, Ludwig: *Zettel* (Manuscript, 1945–1948). Transl. by G. E. M. Anscombe. Text ed. by G. E. M. Anscombe and G. H. von Wright. Basil Blackwell Publishers, Oxford, 1967 (2nd ed., 1981).

Wittgenstein, Ludwig: *Philosophical Investigations* (Manuscript, 1945–1949). Transl. from *Philosophische Untersuchungen*. Transl. by G. E. M. Anscombe. Basil Blackwell Publishers, Oxford, 1958.

Wittgenstein, Ludwig: *Remarks on the Philosophy of Psychology. Volume I* (Manuscript, 1946–1949). Transl. by C. G. Luckhardt and M. A. E. Aue. Text ed. by G. H. von Wright and H. Nyman. Basil Blackwell Publisher, Oxford, 1980.

Wittgenstein, Ludwig: *On Certainty* (Manuscript, 1949–1951). Transl. by D. Paul and G. E. M. Anscombe. Text ed. by G. E. M. Anscombe and G. H. von Wright. Basil Blackwell Publishers, Oxford, 1967 (2nd ed., 1981).

Womack, James P.; Daniel T. Jones; and Daniel Roos: *The Machine that Changed the World: The Story of Lean Production*, Rawson Associates, New York, 1990. – Harper Collins Publishers, New York, 1991.

Woo, Carson C.; and Frederick H. Lochovsky: 'Supporting distributed office problem solving in organizations', *ACM Transactions on Office Information Systems*, vol. 4, no. 3, July 1986, pp. 185–204.

Woods, David D.: 'Commentary: Cognitive engineering in complex and dynamic worlds', *International Journal of Man-Machine Studies*, vol. 27, no. 5–6, November–December 1987, pp. 571–585.

Woods, David D.: 'Coping with complexity: The psychology of human behavior in complex systems', in L. P. Goodstein; H. B. Andersen; and S. E. Olsen (eds.): *Tasks, Errors and Mental Models. A Festschrift to Celebrate the 60th Birthday of Professor Jens Rasmussen*, Taylor & Francis, London, 1988, pp. 128–148.

Wynn, Eleanor H.: *Office Conversation as an Information Medium*, PhD dissertation, University of California, Berkeley, 1979.

Wynn, Eleanor H.: 'Taking practice seriously', in J. Greenbaum and M. Kyng (eds.): *Design at Work: Cooperative Design of Computer Systems*, Lawrence Erlbaum, Hillsdale, New Jersey, 1991, pp. 45–64.

Yates, JoAnne: *Control Through Communication: The Rise of System in American Management*, Johns Hopkins University Press, Baltimore, and London, 1989.

Zerubavel, Eviatar: *Patterns of Time in Hospital Life: A Sociological Perspective*, University of Chicago Press, Chicago, and London, 1979.

Zimmerman, Don Howard: *Paper Work and People Work: A Study of a Public Assistance Agency*, PhD dissertation, University of California, Los Angeles, 1966.

Zimmerman, Don Howard: 'Record-keeping and the intake process in a public welfare agency', in S. Wheeler (ed.): *On Record: Files and Dossiers in American Life*, Russell Sage Foundation, New York, 1969a, pp. 319–354.

Zimmerman, Don Howard: 'Tasks and troubles: The practical bases of work activities in a public assistance agency', in D. A. Hansen (ed.): *Explorations in Sociology and Counseling*, Houghton-Mifflin, New York, 1969b, pp. 237–266.

Zisman, Michael D.: *Representation, Specification and Automation of Office Procedures*, PhD dissertation, Department of Decision Sciences, The Wharton School, University of Pennsylvania, Philadelphia, Pennsylvania, 1977.

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