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BEYOND INVISIBLE COLLEGES: INSPIRATIONS AND ASPIRATIONS OF POST-1972 SOCIAL STUDIES OF SCIENCE*

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A ten-year perspective on studies of scientific specialties–theory, method, and focus–from the social studies of science literature is presented. The inspirationprovided by *Price*'s work on "invisible colleges" and *Crane*'s 1972 monograph of the same name is traced conceptually through the history, philosophy, and sociology of science. A decade later the literature on specialties is seen to aspire to interdisciplinary knowledge of scientific growth, fragmentation, consolidation, and supersession.

Introduction

"Specialization is the hallmark of modern science." With these words, I began a review of the "scientific specialties" literature in 1976, which I inventoried again in 1983.[1] Two decades earlier *Derek Price* introduced his Pegram Lectures by noting:

> My goal is not discussion of the content of science or even a humanistic analysis of its relations. Rather, I want to clarify these more usual approaches by treating separately all the scientific analyses that may be made of science. Why should we not turn the tools of science on itself?[2]

In 1962, when *Price* uttered these words, there was no "we" – just Derek. Today, there are students of science who take as problematic the research communities which produce knowledge claims. These students are part of a larger multidisciplinary and international enterprise known as "social studies of science." To call *Price* an inspiration to this enterprise would be trite. Besides, inspirations are better measured by the intellectual ferment of a literature, the mobilization

*This essay is based upon the introduction to my Sociology of Sciences: An Annotated Bibliography on Invisible Colleges, 1972–1981 (Garland, 1983).

of scholars to focus their craft and explore that to which aspirations never before applied. It is the legacy of melding inspiration and aspiration that endures, not merely in direct citation or eponymous recognition of a single individual, but in subtler "intergenerational" influences that infuse practitioners with analytical goals – and the tools for attaining them.

In a sense, this essay is a study within a study, mirroring a larger "community", a microcosm of the theories, methods, and typically implicit epistemological allegiances that compete within and fragment "social studies of science"[3]. If we celebrate anything here, it is not the memory of *Derek Price,* but the commingling of his ideas with those who helped to acknowledge the problematic character of the beast (on whose very name we still cannot agree)!

I begin with a construct – more accurately, a handful or two named below – and seek evidence (competing claims, if you prefer) that research communities *exist.* If you find such a claim mundane, then the evidence I have amassed on factors which engender and maintain identifiable collectivities may titillate further. To wit, who belongs to the community and how do we know? How is their activity linked – an array of causal relationships has been explored – to other constructs, e.g., the growth, obsolescence, transformation, or institutionalization of the community?

In the 300-plus item I compiled, classified, indexed and annotated, but will not present here[4], there is evidence only that social scientists (predominantly, not exclusively) are studying research communities. Such studies, I claim, belong to a genre of research. Of course, *all* of science is community-based so that my criterion for inclusion could be a simple, and simply overwhelming, one! Since that will not do, I've applied other criteria. To enumerate them all would not convince all because at least one criterion is aesthetic and difficult to rationalize, namely, something in the paper "clicked" for me during my deliberations on the topic. Specialization is delimiting: to discard a candidate article for inclusion in a bibliography is to reduce the forest by one so that another tree may emerge a bit from the shadows. My delimitation, however, is also self-serving; it makes my task more manageable. And so, it strikes me, that others who study communities similarly rationalize their choices and research decisions.

No doubt, therefore, the literature I selected to include in my annotated bibliography will raise eyebrows and bring both guffaws and cries of outrage from wronged readers and omitted authors alike. But if this essay is no spur to further research (and yes, even a little outrage), then my perception of the problems addressed in the specialties literature has been askew. Oftentimes sensibilities are not offended because incoherence, fragmentation, and scatter preclude perceptions. So much for the pragmatic conscience.

Crane's agenda – with reservations

In my volume, I returned to re-survey a literature spanning the decade 1972–1981. My principal reason for selecting this origin is that Diana *Crane*'s *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* was published that year[5]. Not only was her monograph theoretically bold, it was bibliographically resourceful. It set forth an agenda for a "second generation" of studies, thus spawning both emulation and recrimination. In the words of *Hagstrom*[6],

> It touches upon some of the problems currently central to the sociology of science: the existence and importance of invisible colleges, Thomas *Kuhn*'s paradigms and revolutions, and the measurement and form of scientific growth.

But reviewer *Hagstrom* had more than unqualified praise for *Crane*'s approach and analysis. Indeed, he articulated some theoretical and methodological reservations that researchers of specialties henceforth both doggedly pursued and blissfully ignored. To wit:

> The theory presented is simple, too simple in fact. It is argued that the growth of science and of scientific specialties follows the logistic curve because it is a social diffusion process . . .
> It seems to me that these results lend scant support to the notion that scientific growth typically follows the logistic curve. Linear or exponential curves fit most of the graphs (showing number of publications or of authors) just as well.
> *Crane* fruitfully compares the concept of 'invisible college' with Charles *Kadushin*'s 'social circle'[7], she measures the connectedness of these networks, and she suggests that such networks are necessary conditions for scientific growth . . . (but) that weakly organized areas may be those studied in their very early or late stages or may be areas not institutionalized in the disciplines in which they belong.
> Attempts to create a sociology of knowledge that fail to consider the micro-organization of culture producers are doomed to failure . . . (yet) *Crane* presents almost nothing about the intellectual content or the personalities working in (her mathematics specialties of) diffusion theory or finite groups.

Hagstrom's reservations were portentous indeed. Studies of scientific specialties flourished in an "exploding" literature that outgrew its disciplinary imprimaturs, e.g., sociology of science, and came to be known as "social studies of science"[8]. The problem with calculating the proportion of this literature that specialty studies represent is one of identification versus definition. As *Woolgar*[9] demonstrated, one researcher's "identification" (as in discovery) is another's "definition" (as in arbitrary inclusion or exclusion). I'm not inclined to wrestle with this problem here. Recognize, however, that it has promoted (and probably inhibited) a good deal of work, some of which I "define" as relevant to the task at hand. So although my impression is that the sheer number of specialty studies published since 1972 has outstripped the growth of the "science studies" literature in general, I'll leave the precise calculation of "doubling times" and "half lives" to the bibliometricians, and indicate instead how specialty analysts have addressed each of *Hagstrom*'s aforementioned reservations.

Researching Invisible Colleges: Intersecting forerunners and genres

1. *Hagstrom*'s first reservation concerns the distribution of artifacts, mainly publications, over time. The temporal connection of published research on a subject or set of problems has been taken, first by information scientists, and now by that exotic breed called "bibliometricians", as indicative of a collectivity producing that research literature. This genre of research, then, centers on communication among authors – both that formally signified through publication and that which occurs informally in "invisible colleges". As foreshadowed in the title of *Price*'s third Pegram Lecture, "Invisible Colleges and the Affluent Scientific Commuter" herald "the logistic transition from Little science to Big Science"[10]:

> . . . new groups of scientists emerge, groups composed of our maximal 100 colleagues. In the beginning, when no more that this number existed in a country, they could compose themselves as the Royal Society or the American Philosophical Society. At a larger stage, they could split into specialist societies this size. Now, even the smallest branches of subject matter tend to exceed such membership . . .

So it was science as a social system that some forerunners of bibliometrics sought to characterize[11]. This concern intersects with a second genre (detailed below) which focuses on "coherent groups" of interacting scientists as opposed to social and statistical categories of communication behavior, e.g., by discipline and age.

A related focus within this artifact-based genre, however, is the establishment and interpretation of growth curves per se. Illustrative of this approach are the various statistical bibliographies of specific subject literatures, e.g., of nitrogen fixation by plants[12] and of mammalogy[13]. According to detractors of this "S-curve" mentality[14], the "growth" that is being measured may be an artifact of counting. (A related charge fuels the controversy over *Lotka*'s Law, see Ref. 15). This is precisely the criticism leveled at *Crane* by *Gilbert* and *Woolgar.* They ask, in effect, what is a meaningful slice of the literature? Since specialties, like specialty literature, possess no inherent boundary, they must be defined in relative terms. As *Price*[17] warned,

> Even the splitting of chemistry from physics when the cake of natural philosophy was divided gave rise automatically to disciplines of physical chemistry and chemical physics, so that each section needed constant surveillance of the others adjacent. Overlap of research fields is a sort of embargo that nature exerts against the urge that man [sic] has to divide and conquer.

The shape of any curve, therefore, reflects the criteria by which publications are included in or excluded from that specialty. The social structural implication, of course, is that a few persistent authors will be seen as central to the specialty whereas a larger transient set of authors will "emerge" as peripheral. The danger in such an interpretation is that, if we subscribe to the Matthew effect and the accumulation of advantage[18], the central authors become a prestige-laden core while those on the periphery remain intellectually inconsequential for the subsequent growth of the specialty (for such an interpretation, see Ref. 19).

> The scientific elite have acquired prestige among the public in general and the employers in particular, which has given them a certain affluence and enabled them to commute. It incidentally replaces the kudos they have lost since the debasement of the coinage of scientific publication . . . So much for the elite, what of the masses? [20]

By resisting over-interpretation of the S-curve, the critics sensitize us to the arbitrariness of one's operationalizations which, thought tenable, are to many unconvincing. While such criticism has given rise to a more participant-based approach to specialties (discussed below), it is a sobering reminder that "specialty" is a construct and "membership" in a specialty, based on observer-dependent definitions, may be nothing more than a reification of that which we posit to exist.

2. This brings us to *Hagstrom*'s second reservation and another genre of specialty research. Cognizant of the slippage between postulated specialties and scientists' behavior, researchers adopted a panoply of concepts to discuss the elusive and complex phenomenon I've called "specialties": social circle, research area, community, cluster, network, problem area, problem domain, cognitive region, invisible college, subdiscipline, subfield, coherent group, paradigm group, theory group, and school of thought. Many of these terms are tied to a theory or a technique; some designate a state in a model of specialty development; others are just efforts to distinguish, connote, or innovate. None, save perhaps "invisible college," has enjoyed widespread usage, i.e., the meaning changes with the discipline and mood of the user. With little conceptual or operational comparability, communication among students of specialties has suffered. This, too, of course, serves a purpose of specialization: we communicate with whom we want by publishing in certain literatures.

For the moment let me seek some conceptual closure. In an effort to transcend the connotations of "invisible college" and move toward "social circles," I would opt for *Kadushin*'s[2] refinement of this term: "cultural circles" which attract members on the basis of "cognitive goals such as science and technology." As *Bystryn*[22] puts it, such circles are characterized by:

> (1) no clear boundaries; (2) indirect interaction (not everyone has to know everyone else or have contact with everyone else); (3) . . . there is no formal leadership; (4) it lacks

instituted structures or norms (circles arise to solve the problems of individual members who . . . have common needs and interests); and finally (5) because they tend to be pegged or draped around other structures.

Here is a definition less deterministic than *Crane*'s – especially regarding institutionalization – and intuitively appealing to a bibliographer like myself who perceives a literature with more "scatter" than "core." Researchers *do* run in research circles – sometimes simultaneously in two or three, often sequentially over the course of a career, frequently forming new and breaking old circles as they go. Research circles are also cosmopolitan and international, as well as regional and local. They vary in the extent of their visibility and collegiality, to "members" and "nonmembers" alike, and are fluid structures in that no rosters are maintained or inventory of acceptable problems publicized. They are, like other social systems, systematic and capricious in how they operate, whether they develop, and what they achieve. They are also ephemeral, and that may be their most intriguing feature. As David *Edge* conjectured years ago, "By the time we get to studying a specialty, it may have done its best work and is no longer viable." We are left with traces – and our favorite historiographic, bibliometric, or ethnographic devices – to prove that "certain scientists once ran in the same research circle for some very good reasons." Such circles, as *Price*[23] explains,

confer prestige, and, above all, they effectively solve a communication crisis by reducing a large group to a small select one of the maximum size that can be handled by interpersonal relationships. Such groups are to be encouraged, for they give status pay-off without increasing the papers that would otherwise be written to this end. I think one must admit that high-grade scientific commuting has become an important channel of communication, and that we must ease its progress.

Armed with various devices, the forerunners of the "research circles" literature sought to measure the factors that bring scientists together, forge their self-identification, and lead to our recognition of new disciplines[24], new problems and advances[25], and new levels of aggregation of scientific behavior and artifacts[26].

Of special significance in the "conceptual" genre is the exploration of a specific communication behavior – the citation of literature in one's publications – as providing an unobtrusive link[27] between the previous work of others and one's own, between what is systematically "signalled" amidst the publication "noise" in a specialty[28], and between scientists' private musing and their public reports. What are the social norms of citation behavior?

If, then, the prototype of the modern scientific paper is a social device rather than a technique for cumulating quanta of information, what strong force called it into being and kept it alive? Beyond a doubt, the motive was the establishment and maintenance of intellectual property[30].

How accurately do citations convey an author's intentions, evaluations, and intellectual processes? What do citations tell us and what do they obscure? And finally, how is life in social studies of science since the advent of citation analysis[31]? Price, for one, anticipated a diversity in citation practice and precept.

> We shall have to ignore the evident malpractice of some authors in preferentially citing their own papers, those of their special friends, or those of powerful or important scientists that confer status on their work. We shall also take a rosy view in supposing that the practice of first writing the paper and then adding for decoration some canonical quota of a dozen references . . . does not sensibly pervert the average conscientiousness in giving credit to papers that have provided the foundation for the work[32].

Views rosy and dim are plentiful in the literature on citation analysis (as we later review). For many students of specialties, citation analysis *is* life (and pique): it is, simultaneously, the panacea and the albatross, the height of objectivity and the depth of numerology, the wonder and the scourage, the reality and the phrenology, of social studies of science.

3. *Hagstrom*'s third reservation centers on *Crane*'s indifference to the content of the specialties she studied and the personalities populating them. This same reservation is echoed by another reviewer of *Invisible Colleges*[33] and a chorus of European historically- and philosophically-grounded sociologists; the call is for a sociology of knowledge approach to the sciences – natural as well as social. There are very few sociological forerunners to cite. Those of note who carried out empirical studies in which the intellectual and the social were presented in context, warts and all, were *Fisher, Krantz,* and *Swatez.*[34] The latter is a benchmark in the sociology of science literature for its focus on a laboratory and a research team led by an eminent scientist. It was a case study before such studies of science became fashionable (at least in North America) and before such a site became *au courant.*[35]

Insofar as the sociology of *knowledge* emphasis is concerned, Crane's bibliography of 181 sources is telling. Only a dozen reflect this emphasis, including *Kuhn*'s *The Structure of Scientific Revolutions,*[36] four philosophical works by Stephen *Toulmin,* four by British scholars, and three by North Americans. Inspecting the bibliography of my own review article on specialties[37] shows that among the pre-1972 citations (n=65), only seven reflect a sociology of knowledge perspective. My purpose, however, is not to dwell on the myopia of two North American sociologists, but to contrast our respectively narrow gazes at specialties with the post-1972 literature that framed developments in social studies of sciences during the decade that followed. These developments are highlighted in the following section.

For now, the case has been made that the research agenda set forth in *Invisible Colleges* and the reservations expressed by at least one reviewer of it about future specialty studies have been realized. This essay is a testimony that those who inherited the *Price* legacy breathed life into specialty studies, and although I like to claim that their lack of consensus is healthy, part of the claim is self-deluding. If authors were not publishing in so many diverse invisible colleges/research circles, one would not have had to run so vigorously among their archives to collect their artifacts.

> . . . the invisible colleges have a built-in automatic feedback mechanism that works to increase their strength and power within science . . . Worse, the feedback is such that we stand in danger of losing strength and efficiency in fields and countries where the commuting circuit has not yet developed[38].

Indeed, specialization is fractionating literatures into ever-smaller bits; retrieving them – marvelous libraries and information technologies notwithstanding – is a challenge. Most research scientists can afford neither the time nor the resources to meet the challenge. Such a situation does not bode well for the production of original research. Most claims to novel knowledge will be modest re-discoveries and re-statements of others' thoughts and findings about which we preserve our ignorance (despite innovations such as *Current Contents*).

To summarize our current ignorance and knowledge about scientific specialties, I would say that the reservations not only of *Hagstrom,* but those of *Edge, Griffith, Mullins, Small,* H. *Collins,* and *Chubin,* among others, have been a ticket to "go beyond" the Price-inspired study of invisible colleges and investigate the philosophical, historical, sociological, and bibliometric accounts of specialty formation, evolution, and absorption/demise. For some, knowledge of a specialty is a strong inference from a circumscribed literature. Such observer-dependent studies typically associate bibliometric characteristics with a social structure: community is a corollary of artifacts. The conceptual genre of specialty studies, however, leaves less of the "community under the curve" to chance. These studies seek to ascertain the linkages among specialists, i.e., they insist that categorical definitions will not suffice; only coherent groups with demonstrable communication ties will do.

Meanwhile, the third emphasis, framed by the sociology of knowledge, may be the most "incredulous" of all. While considering the first genre a fiction of the analyst and the second a leap of faith uninformed by the content of the science under study, the "cognitive sociologist" reconstructs the minutiae of specialization on a case-by-case basis. Shunning both the quantitative evidence of exponential-logistic growth and the network connectedness of core researchers surrounded by marginal contributors, this third genre researcher depends on the reports of the specialty participants themselves, undertakes on occasion first-hand observation, and draws inspiration from forerunners largely outside of both sociology per se and circles of North American sociologists.

With these three genres of studies firmly entrenched, differences in how to conceptualize and measure specialties abound. These differences extend to the very heart of the enterprise. What is taken by some as a legitimate focus for study and a methodology for executing it becomes a contentious issue for others. One researcher's fiction may be another's fact, but I, like others, can cite a body of literature that attests to the "fact" that others *share* my particular fiction. Such consensual pluralism serves to fragment a growing circle of researchers into ever-shrinking spinoff circles. Their intersection – if we believe the patterns of formal and informal communication that have been discerned – becomes infrequent. A concomitant of spiraling specialization could be a trivialization of knowledge. To students of specialties this prospect drives our aspirations higher – to build on the decade of research sparked by *Invisible Colleges.*

Plan of this essay

If my introductory remarks have been the least bit compelling, then what follows should be easier to bear. Nevertheless, there remains a dense forest of science studies. While not necessarily embracing specialties, invisible colleges, or research circles as prominent constructs or units of analysis, this forest has nurtured many studies which command our focus. Thus, I have divided my focus into two sections. Each provides an overview in the hope of unifying a fragmented literature. To lend some coherence to my annotated bibliography[39] is one goal; another is that what works for one reader may not work for plenty of others. Thus, my "cuts" are varied, and I hope that some slice will reveal an edge that is eminently usable in research or teaching. There is, however, a tradeoff here. As *Gusfield*[40] puts it:

> To define an area of study and describe its parts and direction provides readers with boundaries and channels that create needed organization and clarity. But boundaries are also cages that lock students into ways of thinking and studying that shuts them out from the complex and unexpected realities of life. There is a form of metaphysical arrogance in the process of field-building.

With *Gusfield*'s caveat in mind, the first section below chronicles theoretical developments since 1972 that have informed research on specialties. As in the succeeding sections, my form will be that of a bibliographic essay that traces traditions and themes without paying due respect to the programmatic intricacies of the publications themselves. The second section comments upon the search procedure and the resultant classification of the bibliographic entries. Also noted are the cognate themes deliberately omitted. At the end I offer a few observations on networking and building an interdisciplinary research circle, and on future trends in specialization, including its measurement and retrieval in annotated bibliographies.

The post-1972 decade: an overview of programmatic (theoretical and methodological) developments

In her introduction to the massive *Spiegel-Roesing* and *Price*-edited volume *Science, Technology, and Society: A Cross-Disciplinary Perspective*[41] *Spiegel-Roesing* reviews several "tendencies" in the literature. Her word choice is significant because tendencies need not be mutually exclusive or differ along disciplinary lines or, for that matter, categorize the work of a single author uniformly. As personal research programs evolve, authors tend to change, if not their orientation or style, then perhaps their subject focus or methodology.

Intellectual tendencies and territories

In surveying the theoretical and methodological developments in social studies of science since 1972, I am struck by shifting tendencies among authors. Sometimes these are subtle shifts which the authors themselves would disclaim. Rarer still are those proclamations that "historians have invaded sociology" or "philosophers have attempted historical analysis." The unspoken rule is that there is a "territorial imperative" which must be respected. To violate it may be permissible, but to claim such forays is tantamount to "intellectual imperialism." For it is the defensiveness of disciplines – replete with institutional traditions – that rejects such forays. Territoriality is the preserve of specialized professionalized science. It is the status quo of knowledge, the guardian of obsolescence, the knee-jerk response to the imminent threat posed by new, often programmatic, knowledge claims. Ironically, researchers are the source of such claims so that, as specialties institutionalize their knowledge rigidifies and becomes enveloped by a core of consensus. Out on the "research front," as *Price* often put it, the science is pliable and the claims are numerous. It is there that the "soft underbelly of science"[42] can be found.

With the proliferation of specialty studies in the 1970s, several tendencies have been manifested. Each sports a "soft underbelly" which is nevertheless connected to a "hard heart" of literature and identifiable proponent authors. In short, the study of scientific specialties is a microcosm of the programmatic theoretical and methodological tendencies that pervade science studies. The circles in which these tendencies are embedded thus can be distinguished by my reading of the literature and from personal contact I have had with various proponents. Both of these data sources can be considered fairly comprehensive but not exhaustive, and therefore fallible as selective perceptions. It should also become apparent that emphasizing differences or similarities are two sides of the same coin. Everyone claims uniqueness to protect some territory. Reviewers like myself defy such boundaries and audaciously plow through all territories, invariably "missing", "trivializing", and "aggrandizing" all they see. But *somebody* must do the plowing!

Such is the curse of the "outsider", as *Merton*[43] cautioned. Although his essay was atypical of North American sociology of science in the '70s (since it dealt with perspectives on "knowing"), it was followed by much more doctrinaire work that extolled the virtues of Kuhnian theory in demographic terms[44], defended the Mertonian tradition of internalist studies of scientists' status and social structure[45], and credited the accessibility of large computerized data bases such as the *Science Citation Index* with the formidable analytical gains in testing and quantifying generalizations about scientists' normative behaviour.[46] (Elsewhere these and related developments are reviewed in a more flattering light.[47]) Perhaps of greatest interest as a commentary on the North American contribution, however, was *Merton*'s[48] own "episodic memoir" that includes an intellectual history of *Kuhn* and other luminaries who advanced "research procedures" within social studies of science.

Among the minority of North Americans not enamored of the Mertonian approach and foci, another small sampling can be cited. Some reacted against normative and status preoccupations[49]; others[50] offered reflexive accounts of the movement away from positivism in science studies, or charted rationalistic tendencies. Finally, *Restivo*[51] asked "What is the epistemological relevance of the sociology of science? " and outlined three programs (highlighted below) that provide affirmative but disparate answers.

In terms of narrowing the gap between the sociology of science and related pursuits, e.g., sociology of knowledge and the history and philosophy of science, it was the European sociology literature that posed challenges and alternatives in theory and method.[52] *Lammers*[53] sounded the battle cry of a critical stance toward Kuhnian theory as a heuristic:

> The poly-paradigmatic character of the social sciences is probably not only a function of the vicissitudes of their study objects and of the deficiencies (or peculiarities) of their methods. It stands to reason that the institutional setting of the social sciences also has

> something to do with their plurality in paradigmatic assumptions . . . [T]he multiplicity of social scientific conceptions . . . guarantees that social sciences will never serve one master.

But the Europeans were by no means univocal. For while *Whitley* was lamenting the Mertonian "black box" approach and rallying researchers to look inside at the *content* of science,[54] *Law and French*[55] were calling for an "interpretive" approach that seemed to appeal more to British sociologists[56] than to those on the continent. The differential appeal was clear in the British advocacy of case studies of historical episodes vs. the predominantly German concern with contemporary science policy and the state. Yet these circles intersect at various points, the most obvious being a common tracing of intellectual heritage to *Kuhn.* Surely, the post-Kuhnian *spirit* is omnipresent in this literature, but more for the rhetorical purpose of distancing the research from *Merton* than due to uncritical acceptance of the "normal-revolutionary science" thesis. Indeed, *Mulkay*[57] could have had European cognitive sociology of science in mind when he argued that scientists routinely invoke "vocabularies of justification" in accordance with their interests and audience.

Ron *Johnston*[58] took another tack: he proposed a "contextual knowledge model" that "overthrows" the internal-external dichotomy in science. Hence, an Australian at Manchester tried to unite in a single statement the interpretive British "strong programme in the sociology of scientific knowledge" with politically-relevant continental research. Coincidentally or not, what followed was an array of policy-related case studies that exemplified versions of a relativistic epistemology. For example, *van den Daele* et al.[59] demonstrated the political direction of scientific development, *Gilbert*[60] presented a critique of indicators of scientific growth, and *Dolby*[61] reflected on "deviant" science as a temporally and culturally relative definition. *Mulkay*[62] epitomized this consolidation of European research by showing that

> in identifying scientific knowledge as epistemologically special, and as exempt from sociological analysis, sociologists have tended to make two basic assumptions . . . namely, that scientific theories can be clearly validated by successful practical application, and that the general theoretical formulations of science do regularly generate such practical applications . . . Both these assumptions are very doubtful.

Advancing concurrently, and in a sharply programmatic manner, were the respective works of first the Science Studies Unit at Edinburgh and then the so-called Bath school. Relying explicitly on historical and philosophical themes, Edinburgh[63], among others, championed the "strong programme". The themes, if not anathema to many historians and philosophers, as well as sociologists, certainly made them squirm. *Meynell*[64] and *Millstone*[65] assailed the *Barnes-Bloor* notion that "knowledge" should not be treated as a category analytically distinct from "accepted belief". *Trigg*[66], in reviewing *Bloor*'s *Knowledge and Social Imagery,* concluded that "the 'strong programme' of the sociology of knowledge attacks the basic assumptions of our thought and language". *Neve*[67] was more sanguine about the "naturalization of science".

Likewise, in one of his many statements on the "empirical programme of relativism", H. *Collins*[68] observes:

> [A]ssuming that the sociologist is not gifted with prescience . . . [to] foresee the future *content* of scientific knowledge better than the scientists, this leaves hindsight as the sole judge of what constitutes revolutionary activity . . .

It is such bold assertions that carry the theoretical and methodological proclivities of the "strong programmers" into confrontations with critics. Retorts like the following from *Barnes*[69] to a "rationalist" are not uncommon in their purpose or fervor:

> The thesis of the homogeneity of explanation . . . insists that scientific judgements are to be explained causally . . . without any regard for whether the judgements are favorably or unfavorably evaluated. Why then should I be in the slightest degree inclined to revise an explanation solely because part of the *explanation* is re-evaluated as rational rather than irrational? . . . [I]f a scientific rationalist, for example, had written of the *causes* of the reception of Mendelism, then perhaps *Roll-Hansen*'s work might give *him* (or her) food for thought . . . Scientific rationalists must face the fact that their opponents criticize them, not the natural sciences.

Such iconoclasm is similarly flaunted by yet another research circle that brought its force to bear on science studies of the post-1972 decade: social historians of science. Employing biography – individual and collective[70] – and quantitative measurement[71] as tools for fusing the history of ideas and the history of society, social historians reconstructed in radical, critical, and refreshing ways the role of science and scientists in society.[72] Again, this work was enriched both by the case studies of the sociological relativists and the more structural inquiries of the North American sociologists. The epistemological assumptions and implications that underlay this intersection themselves did not go unchallenged by the purists in history. As *Cantor*[73], in a review of *Barnes' Scientific Knowledge and Sociological Theory,* allowed:

> [E]ven if he has shown that sociology offers a possible method for interpreting science, this is not equivalent to the claim that social forces are the only factors shaping science or that they alone explain all science.

What could not be readily accepted – or its popularity explained – was the impact of *Kuhn*'s *The Structure of Scientific Revolutions* within social studies of science. The literature it engendered, especially the widely read *Lakatos* and *Musgrave*[74] collection, belied the "largely indifferent" reception it received, in the words of historian Nathan Reingold, "to the spirit and many of the specifics of *Kuhn*'s viewpoint".[75] If historians were indifferent, philosophers were downright hostile.[76] But in an exceptional display of disciplinary ecumenism, *Reingold*[77] credits *Kuhn* and the ensuing debate over paradigms, rationality and progress with fueling

> the distinction between those historians of science who resolutely consider their task as primarily the 'exposition and elucidation of substantial aspects of the scientific cultures', largely for their own sake, and those viewing their specialty as providing basic knowledge for application either by other historians or in such fields as science policy.

With these flowers in bloom in the history of science, the prospect of comparable fruition in philosophical circles arises. In the post-Popperian/Lakatosian philosophy of science, we find the staunchest guardians of scientific rationality and realism. Few, however, received serious consideration in *social* studies of science in the 1970s (a notable exception was *Bhaskar.*[78])

Instead, philosophers such as *Toulmin*[79] promoted the convergences between history and philosophy. *Elkana*[80] went further in suggesting that the distinction between realism and relativism

> is not a logical necessity but a historical situation in western scientific culture . . . [E]very problem has a realist and a relativist dimension, and the two views can be, and are actually being held simultaneously.

Such hypothesized "two-tier thinking" was small comfort to those wedded to the notion of scientific progress, as *Laudan* reminded in *Progress and Its Problems* and in response to its detractors.[81] What *Laudan* failed to recognize was the sociological significance of proposing the "research tradition" as the scientist's framework and the philosopher's unit of analysis. As I've noted elsewhere:[82]

> *Laudan* specifies a mechanism which commands a scientist's epistemological allegiance even in the face of evidence that would dispose of its associate theory or theories. A research tradition perserves because it is 'neither explanatory, nor predictive, nor directly testable'. Rather, it is a rallying-point much like *Kuhn*'s 'paradigm' that orients and sustains adherents: 'one's views about appropriate *methods* of inquiry are generally compatible with one's views about the objects of inquiry'.

What I later learned (in preparing this essay) was *Radnitzky*'s[83] anticipation of both *Laudan*'s formulation of "research traditions" and my radical sociological interpretation of it. Such a sequential convergence of thought is symptomatic of what the most disaffected Popperian and gadfly philosopher of this period, *Feyerabend,* warned in *Against Method* and in a reply to its critics[84] – "professionalized incompetence". In a similar vein, *Feyerabend*'s counterpart in sociology, *Gouldner*[85] denounced the "virtuosity of the intelligentsia".

Gropings toward rapprochement

Beyond the methodological anarchists and theoretical pessimist of the 1970s. there were tentative gropings toward rapprochement of disciplinary perspectives and research circle orthodoxies. As for residual disciplinary murmurings, two are of special import. First, the near-subterranean enterprise of the social psychology of science emerged in the form of a major empirical study[86], a methodological guide[87], and two conscientious attempts to codify the psychology of the scientist in the science studies literature.[88] Especially apropos here is the latter review. In it the author maintains that

> since most scientists today are specialists, their individual behavior may be differentially related to specific issues within their own specialty. An adequate model of scientific behavior cannot therefore presume to offer a global traitlike summary . . . it would be futile to offer a monolithic representation of the 'scientific personality.'

The second disciplinary murmuring came from anthropology, or more precisely, an embrace of the anthropological commitment to *in situ* analysis. The social historian of biology, *Goodfield*[89], emerged from a recombinant DNA laboratory with "a perspective and a plea" that we get closer to both our subject matter and its producers in their natural habitat. While *Geertz*[90] lent both eloquence and the appropriate disciplinary credibility to this perspective, European sociologists clamored to penetrate the mysteries of science "in the making".[91] After all, the essence of interpretive case studies is scientific practice.[92] Indeed, the so-called social process of scientific investigation summoned various ethnographic tools, prominently ethnomethodology and other elements of the relativistic programs reviewed above, e.g., discourse analysis.[93] This generated, again in a programmatic way, a more subjective, "constructivist" approach to science: how do scientists at work construct and negotiate the reality that is obscured by their written and oral accounts?

Finally, we arrive at the evidence for theoretical and methodological rapprochement in social studies of science. Few works make such overt claims, but the optimism of their authors can be inferred from a willingness to cite ecumenically and subject the arguments emanating from different circles to a critical reconnaissance. Thus, the debut volume of the *Sociology of the Sciences* yearbook[94] illustrated the convergence of philosophical, historical and sociological currents in eleven case studies. Besides the uneven "yearbook" series, few other book-length treatments can be cited.[95] Other examples of intersecting circles include *Restivo*'s edited collection of essays on topics ranging from laboratory life to citation theories, and his own[96] review and typology of programs in sociology of science: the "strong" program of *Barnes, Bloor, Collins* et al.[97]; evolutionary epistemology (or the more esoteric "moderate" program) of *Campbell*[98]; and meta-inquiry (or the "weak" program) which claims the "metaphilosophy" of *Hooker* and the "metascience" of Wartofsky as forerunners of the analysis of "complete systematic world views" labeled Mertonian, neo-Kuhnian, neo-Marxian, etc.[99] In its current innovative state, only the weak program of meta-inquiry would seem the pessimistic alternative in regarding world views as closed systems virtually immune to competing views[100]. *Mulkay*'s[101] endorsement of the strong program as embracing the most robust epistemology for fostering empirical insights into science leads to a similar pessimistic conclusion without calling it that.

Perhaps the most hopeful sign that the theory and method of non-intersecting circles may yet overlap to form new empirical connections is the translation and editing of *Fleck*'s *Genesis and Development of a Scientific Fact* (originally published in German in 1935).[102] Here, *Merton* collaborated with historian Thaddeus *Trenn* to liberate an essentially "contructivist" account for English-speaking authors. In the words of reviewer Barbara *Rosekrantz*[103], an historian:

> From the grab bag of laboratory life, *Fleck* draws insights that are not always logically compatible and that frequently scrape only the surface of historical and contemporary evidence, but they are nonetheless redolent of those links that tie our time to his . . . [T]he editor credits *Fleck* with 'prescience' because first Hans *Reichenbach* and later Thomas *Kuhn* found some of *Fleck*'s formulations congenial to their own . . . *Fleck* is better appreciated when his own modesty and specific objectives are remembered and intentions are not ascribed to him that diminish his actual achievement.

In a single majestic sweep, many of the dichotomies that have distinguished the theories and methods borne and promoted by studies of the research circles seem blurred in *Fleck*'s monograph: realism-relativism, internalism-externalism, process-product, normative-interpretive, descriptive-constructive, social-intellectual, discovery-justification. This is not to say that these dichotomous themes (which are more continuous that discrete anyway) are resolved by *Fleck*; they are not. They are, however, sufficiently employed to provoke a considerable critical response from those who seldom take notice in more than perfunctory ways of scholarship outside their home circle. Such parochial behavior is territorially defensive, as discussed earlier, and therefore safe. It is the act of the overspecialized (oversocialized?) professional.

The antidote, though not terribly contagious, is to wade into the literature of an "alien" circle and loose an outsider's fury. I am heartened by such offenses, even if they "miss the mark" – the inevitable insider's retort – because they represent attempts to surmount the "epistemological self-righteousness"[104] that specialization and intra-circle consensus breeds. *Gieryn*'s[105] recent review is just such an attempt. Though flawed by the (inescapable) self-righteousness of a (nominal) Mertonian, it illustrates how constructive discord in social studies of science promises a long life to specialty studies. *Gieryn* is dubious of

> constructivists' confidence that laboratory ethnographies or scientific discourse represents a more 'real' grasp on science than citations or other bibliometric data . . . The bugbear: can sociologists' interpretations of accounts or of ethnographic data be any more free of hidden presuppositions and theoretical constructs than interpretations of other forms of sociological data?

This is a fundamental question. If every bibliographic item were viewed skeptically as part of a genre of knowledge claims, and not dogma, about specialties, then what they encompass, exaggerate, and omit *a priori* would come to the fore as divergent conceptions of science. Thus, whether we "let the journals do the talking", believe that specialties exist only in one's mind, or demand that the phenomenology of scientists' routines be recorded by observers of specialties as well as participants in them, we orient our own work as our reference groups would have it. Trapped by circles, our self-definitions are self-serving and -defeating. Victims of a socially-constructed *dich an sich,* we choose to run in those research circles. But have we been running in place?

Constructing a bibliography: Search, classification and summary, characteristics, and uses and trends

As my overview of theoretical and methodological developments in science studies has indicated, specialty studies are the product of various research circles and programs, each of which often has its own specialized journals and newsletters. These periodicals, in turn, are typically components of larger disciplinary literatures, though a growing literature on interdisciplinary research processes (discussed later) is one recent exception to the general rule. As any bibliographer, it behooves me to describe my search procedure, outline my classification scheme, summarize the characteristics of the classified literature, and offer a prospectus on uses of and trends in this specialized literature on specialties – even though the bibliography itself appears elsewhere.

Search procedures

One soon exhausts one's personal card catalog and reprint/preprint file. In seeking other sources, I found nothing as comprehensive as *Dedijer*'s[106] edited "bibliography of bibliographies" for the subject and period commanding my interest, so I looked elsewhere. One of the periodic bibliographies compiled by *Crawford*[107] and published in *Social Science Information* gave me a lead on the "sociology of the social sciences" literature. Likewise, *Hahn*'s[108] bibliography provided guidance to some of the more obscure works in the quantitative history of science, and *Gaston*'s[109] was a check on my North American sociology coverage. The more I looked, however, the more narrow and centripetal to a specialty the bibliographies seemed to become, e.g., announcement of a new bibliography and index on bibliometrics, 1874–1959.[110] I supposed my own purview narrowed accordingly, as a few excellent reference guides to the literature of a specific circle, e.g., *Mitcham* and *Grote*'s[111] on technology assessment, were added.

The three bibliographies that were most valuable to me were the compilations of "Citation Analysis" studies by the institute for Scientific Information researchers[112], of "Studies of Scientific Disciplines" by the National Science Foundation's Office of Planning and Policy Analysis[113], and of "Sociology of Science in the West" by British sociologist Michael *Mulkay*.[114] In the NSF document a "snowball" technique was used to generate over 450 books and articles dealing with disciplines. More than one-third of these items are annotated. "The principal criterion for selection of items for annotation was that the item present data-based information on some enduring aspect of a disciplinary area." Eleven disciplinary categories were used to present the retrieved literature. *Mulkay*'s bibliography contains 342 items, nearly all annotated, largely on the post-1970 literature, and preceded by a lucid narrative on the "emergence of the specialty," "patterns of scientific growth," and "the social construction of scientific knowledge." It is a welcome complement to the present essay.

The final component of my search strategy was a systematic review of the indexes and contents of fifteen journals and two newsletters. These periodicals were selected for publishing works pertinent to social studies of science and having been in existence for at least half of the decade under scrutiny here.[115]

Classification and summary characteristics

The results of my search procedure is a bibliography of primarily the serial literature. A smattering of unpublished reports (working papers and conference presentations), doctoral dissertations, edited books and monographs is included. Of the latter, published reviews of eighteen major books are cited and excerpted.

In all, there are 324 unique entries in the subject classification of the bibliography. They are presented in six substantive sections:

General:	Theories, Methods and Comparative Studies of Scientific Specialties
Citation-based:	A Reference or Citation Approach to Specialty Definition and/or Analysis
Physical Science:	Physics, Astronomy, Chemistry, Geology and Mathematics Specialties
Biomedical Science:	Biological, Biomedical and Agricultural Specialties
Social Science:	History, Philosophy and any "Self" Study of a Social Science Discipline or Specialty
Lab-centered:	A Laboratory Site or Local Organization Focus Defines the Analysis

Each entry is classified into a section based on its *primary* focus (as best as I could ascertain). In 48 cases, the entry appears in one other section where the "secondary" focus is of primary interest, e.g., a cocitation study of collagen is cross-listed under "Citation-based" and "Biomedical Science." I attempted neither finer distinctions nor multiple cross-listings.

The entries can be summarized statistically in a table and two figures. Table 1 cross-tabulates the six subject classifications by five variables: (a) the number of unique entries, (b) the number of entries that appear elsewhere in the classification scheme as primary, (c) the total number of entries [(a) + (b)], (d) the proportion of unique entries that have been annotated (in %), and (e) the proportion of the 1972–81 literature (n=324) represented by the most recent, i.e., 1979–81 entries (n=125 or 38.6%).

Of interest in the summary table is the comparatively small number (n=36) of Citation-based studies. This may reflect the quirks of this bibliographer, however, who

Table 1
Summary Characteristics of the Specialty Studies
Classified in the Bibliography, 1972–81

Classification	(a) Number of Unique entries	(b) Number of entries appearing elsewhere as primary entry	(c) Total ‡ entries	(d) % of (a) annotated	(e) % of 1979–81 literature
General	67	–	67	88.1	15.2
Citation-based	36	26	62	94.4	12.0
Physical Science	60	15	75	78.3	17.6
Biomedical Science	57	6	63	73.7	17.6
Social Science	74	1	75	75.7	21.6
Lab-centered	30	–	30	76.7	16.0
All	324	48	361	80.6	38.6

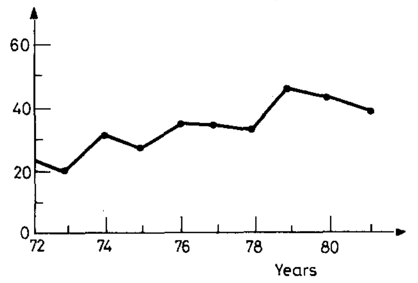

Fig. 1. Frequency distribution of specialty studies by year, 1972–81

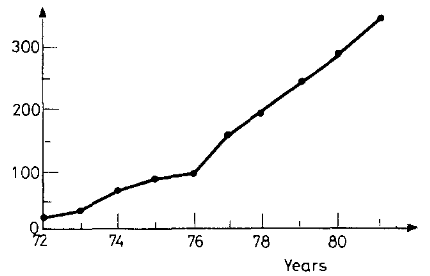

Fig. 2. Cumulative frequency distribution of specialty studies, 1972–81

assigned 26 entries a primary status in one of the five other sections. Note in column (d) that almost all 36 unique Citation-based entries have been annotated. This contrasts with the proportion annotated in all other non-general sections, which hovers around 75%. Column (e) is a crude measure of the recent growth of each subject relative to the others during the last three years. A caveat here is that 1981 is an under-enumerated year; my search was completed in August.

This under-enumeration is also evident in Fig. 1. A modest step occurs from 1975 to 1976 and a steeper one from 1978 to 1979. This becomes the down slope of a three-quartered 1981. For the inveterate "S-curvers," Figure 2 presents the cumulative frequency distribution for the bibliography. Instead of a logistic or decaying exponential curve, we see linear growth with a bump in 1979. I'll forego the second-guessing about "missing" entries (was I too restrictive, conservative, or uninformed of relevant literature?) and move instead to a concluding discussion of possible uses for and meanings of what is there.

Uses and trends: Will the circles be unbroken?

Second-guessing may be the prerogative of the critic – there's ample evidence in this very essay – but second *thoughts* are an affliction that properly seizes authors, editors, and compilers alike. Without retracting or repudiating that which has passed before me and has found its way into these pages, I must consider: What have I done here? In anticipating the critics and the critical users of this bibliography, my second thoughts gravitate to what has received short shrift.

I have applied a definition of scientific specialties that is tantamount to *knowledge* specialization, to the aggregation of ideas and people which gains coherence over time. This coherence flags our attention; we recognize an entity that can be circumscribed as intellectually and social distinct from others. But specialization is more; it is a claim to expertise, that specialists can provide knowledge which others – by training, certification and/or experience – cannot. This, of course, is how specialties are professionalized and how specialists within them claim distinctiveness, social value, and sometimes even indispensability.

Unlike the professions with a lay clientele, e.g., accounting, law and medicine[116], specific specialties relate chiefly to *other* specialties, other professionals, other experts. If specialization is the key to the economic survival of non-scientific professionals, it may be the key to the social survival of scientists and engineers. For as a science becomes more and more esoteric, its comprehension by the public plumments.[117] And what the non-scientist or non-specialist fails to understand, he/she begins to doubt and fear[118], as recent debates over genetic engineering, nuclear power, and proposed palliatives for dread disease have shown.[119]

My point is that scientific specialties as circles of researchers overlook or underplay the interest group behaviour which scientists, as members of an imaginary fraternity known as "the scientific community," display. As *Buck*[120] reminds us, this old-fashioned "community" is a highly skilled elite in a bureaucratized work force: Who are these people, and what do they want? We cannot begin to answer such pointed questions here; it is for this reason that I exhort readers *not* to seek generalizations, but rather to recognize that "who these people are and what they want" depends on who one talks to, what one reads and how one's own professional ideology predisposes acceptance or rejection of one claim, theory, method or shred of evidence over another.

Professional ideology is a cultural phenomenon that endows specialists with special privileges. When specialists act in self-interested ways to preserve their autonomy, expand their privileges and propagate their knowledge claims to whomever will listen, they are acting *politically*. In the name of objectivity and expertise, they are asserting temporary hegemony – and making scientists and non-scientists alike uncomfortable enough to prepare for the next round.

What is at stake here are sacrosanct research values invested in future outcomes. But seldom do we specialists call it that; talk of politics and ethics in science is still anathema to most – whether we answer to the title "sociologist," "philosopher," "doktor" or "professor"[121]. We prefer to list "progress" and "truth" as our most important products[122].

The bibliographer's errors of omission and commission are a manifestation of flawed professional judgment that is value-laden and tinged by the incompetence that over-specialization entails. The reader usually indulges these errors in the spirit that, to return to the forest metaphor I applied at the outset, a few trees have been extracted from the dense forest of "social studies of science." These trees look like "specialty studies." In removing a few of them from their natural environment, I've necessarily reduced the forest. Lest we forget, there is still substantial intertwining at the roots.

One way to rectify the artificiality of my purposive bibliographic "cuts" is by glimpsing some other groups which have received short or no shrift. One is "science policy." I would be naive (I hope) to think that specialty studies have not been used in the formulation of policies on research and training priorities. The patrons of science, especially national governments, have the power and resources to limit the amount and kinds of inquiry of scientists and other culture producers[123]. *Price*'s fourth and concluding Pegram Lecture, devoted to "Political Strategy for Big Scientists," ends with the injunction that

> we must look for considerable assumption of power by responsible scientists, responsible within the framework of democratic control and knowing better how to set their house in order than any other men at any other time[124].

The growth of "policy analysis" and the reassertion of authority by the chief U.S. federal research patrons, the National Institutes of Health and the National Science Foundation, cast doubt on the "considerable assumption of power" by scientists in the formulation and implementation of science policies[125].

Another under-represented issue here is the "career patterns" literature which takes scientists, both as a specialized labor force in society and as technically differentiated within the institution of science itself, as problematic. While the careers of intellectual leaders of schools and traditions, and the founders of entire disciplines, are central to many specialty studies[126], the manpower aspects of cohorts trained at a particular time in particular fields[127] have largely been omitted. Specialties encompass coherent groups and teams who regularly interact[128], not social "categories" which define members through a shared characteristic. The analytical difference is dramatized by a study of Nobel laureates on the one hand, and the biography of *one* laureate on the other[129]. The latter embeds the career in a research community, the former enshrines the career as rising above, and indeed catapulting out of, such a community.

A third grove of trees that, ironically, is implicit in most specialty studies is "peer review." After all, it is the research traditions, theoretical persuasions, and standards of evidence held by referees of submitted manuscripts, plus the vagaries of editors, that determine which manuscripts reach the light of print and join the dusty archives for posterity. This review process – or rather its results, since access to referee reports is rare – is occasionally among the most contentious issues within the scientific community. If what is being certified as "new knowledge" through publication is due to factors other than, or in addition to, the merits of manuscripts, then perhaps the system requires periodic reassessment and reform.[130] Likewise, if we substitute research proposals submitted for federal funding as the focus of peer reviews, we, as well as the guardians of the dole, begin to wonder: How is "peer" defined? What is the price of "merit"? And when can the public expect a return on its investment? All of these are legitimate questions which have only recently engaged the critical faculties of social scientists[131].

The interdisciplinary cluster

Taken together, the literatures on science policy, career patterns and peer review form the context for interaction with the literature on scientific specialties. Only with this broader perspective in mind will the trees assume their rightful place in the science studies forest. Indeed, if we look closely at the terrain, we will see that where there are disciplinary clusters, there are also "interdisciplines."

> The presumption that 'science' is conducted solely within disciplines dominates establishment practices in funding research, publishing findings, and advancing careers. Unfortunately, this not only occasions cracks between disciplines, it fails to provide adequate bridges across intellectual and societal chasms.[132]

Such cracks in institutionalized science give research its blurred and dynamic aura. Thus, the leading edge of a boundary that divides two disciplines is often fuzzy. Years ago *Campbell*[133] called the phenomenon a "fish scale," while others[134] have merely lamented the dearth of contact between disciplines that *should* have much in common.

Faithful to the trend that circles seem to foment, researchers who share an interest in exploring interdisciplinary research as a genre of scientific collaboration and output have moved toward visibility and legitimation in predictable ways. They have held three international conferences[135], formed an International Association for the Study of Interdisciplinary Research, and claim a journal, *Interdisciplinary Science Reviews* (which publishes only papers invited by its editor). All the social trappings of specialization, in other words, are present.

Here, then, is a contemporary example of a scientific specialty which emanates from no single discipline, is endemic to no single setting (if anything, it thrives in non-academic settings), and is not formally transmitted via a graduate curriculum. Indeed, specialists in "interdisciplinarity" are converts. Foremost among their missions is to introduce the *teaching* of interdisciplinary communication and collaboration in the university.[136] If the purpose of this specialty is to counter the trend toward fragmentation, then its cause is noble. But the tactics employed thus far indicate that interdisciplinarity, rather than overcoming parochialism, will become a victim of it. It claims to be bucking a trend, but may have to pursue an emulative tack if it is to develop and compete for the mechanisms that sustain modern science: its own journals, associations, meetings, funding programs, and no doubt, soon-to-be-heralded orthodoxies and heroes. Were I to volunteer a prognosis on the growth of interdisciplinarity as a genre of research, I would expect its literature to retain its extensive scatter – a few aberrant trees sprouting amidst various disciplinary clusters.

We could regard interdisciplinary research, then, as a test case for *Price*'s[137] prediction:

> In fields tending to honor their pioneers by eponymic fame – name laws, name constants, name species – one may find that good papers actually improve with age, and their chance of citation increases. In fields embarrassed by an inundation of literature there will be a tendency to bury as much of the past as possible and to cite older papers less often than their statistical due.

In the next decade or so perhaps others will incorporate this finding into the research agenda for the next generation[138]. And so the trees grow. . .

Conclusions

As with the theories and methods of post-1972 social studies of science recounted here, the circle of researchers on interdisciplinarity will continue running. But that is for others – participants and observers alike – to document and divulge. In terms of *this* essay, interdisciplinarity is just one more, albeit intriguing, form of specialization that appears to be synonymous with research circles. The very processes by which scientists defend intellectual territories, proclaiming and disclaiming knowledge, will continue to originate in and be shrouded by such circles. In the next decade, those who remain intrigued will share membership in a circle consisting of students of scientific specialties. It is to them – their peculiar and privileged tendencies – that we inevitably bequeath scientific literatures.

Interaction through the literature is still how invisible colleges first gain visibility and research circles open to new influences. Any student of science should welcome such interaction, and indeed, devise ways of consolidating unobtrusive methodologies with participant-centered ones. I'd like nothing more than to discover new "commuter" colleagues in distant sciences whose own parochial intellectual tendencies converge with my own. That is what interdisciplinarity, as well as disciplinarity, is all about – complementary perspectives on mutual research problems that insure new approaches and collaborative efforts. Specialization and social studies of sciences could have no more glorious an aspiration.

*

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Notes and references

1. D. E. CHUBIN, The Conceptualization of Scientific Specialties, *The Sociological Quarterly,* 17 (Autumn 1976) 448–476; D. E. CHUBIN, *Sociology of Sciences: An Annotated Bibliography on Invisible Colleges, 1972–1982,* Garland Press, New York, 1983.
2. D. de SOLLA PRICE, *Little Science, Big Science,* Columbia University Press, New York, 1963.
3. L. HARGENS, Theory and Method in the Sociology of Science, in *Sociology of Science,* J. GASTON (Ed.), Jossey-Bass, San Francisco, 1978, p. 121–139; D. E. CHUBIN, Constructing and Reconstructing Scientific Reality: A Meta-analysis, *International Society for the Sociology of Knowledge Newsletter,* 7 (May 1981) 22–28.
4. CHUBIN, 1983, op. cit., note 1.
5. D. CRANE, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities,* University of Chicago, Chicago, 1972.
6. W. O. HAGSTROM, Review of Crane's *Invisible Colleges, Contemporary Sociology,* 2 (July 1973) 381–383.
7. C. KADUSHIN, Power, Influence and Social Circles: A New Methodology for Studying Opinion Makers, *American Sociological Review,* 33 (1968) 685–699.
8. I. SPIEGEL-ROESING, *Science Studies:* Bibliometric and Content Analysis, *Social Studies of Science,* 7 (February 1977) 97–113.
9. S. W. WOOLGAR, The Identification and Definition of Scientific Collectivities, in: *Perspectives on the Emergence of Scientific Disciplines,* G. LEMAINE et al. (Eds), Aldine, Chicago, 1976, p. 233–245.
10. PRICE, op. cit., note 2, 83.
11. For example, H. MENZEL, Scientific Communications: Five Social Themes, *American Psychologist,* 21 (1966) 999–1004; W. D. GARVEY, B. C. GRIFFITH, Scientific Communication as a Social System, *Science,* 157 (1 September 1967) 1011–1016; W. D. GARVEY, N. LIN, C. E. NELSON, Communication in the Physical and Social Sciences, *Science,* 180 (11 December 1970) 1166–1173; *Communication Among Scientists and Engineers,* C. E. NELSON, D. K. POLLACK (Eds.), D. C. Heath, Lexington, Mass., 1970.
12. D. P. WILSON, E. B. FRED, The Growth Curve of a Scientific Literature: Nitrogen Fixation by Plants, *Scientific Monthly,* 41 (September 1935) 240–250.
13. S. ANDERSON, R. G. van GELDER, The History and Status of the Literature of Mammalogy, *BioScience,* 20 (1970) 949–957.

14. S. ROSE, The S Curve Considered, *Technology and Society,* 4 (1967) 33–39; K. E. STUDER, Interpreting Scientific Growth: A Comment on Derek Price's 'Science Since Babylon', *History of Science,* 15 (1977) 44–51.
15. P. D. ALLISON, D. de S. PRICE, B. C. GRIFFITH, M. J. MORAVCSIK, J. A. STEWART, Lotka's Law: A Problem in its Interpretation and Application, *Social Studies of Science,* 6 (1976) 269–276.
16. G. N. GILBERT, S. WOOLGAR, The Quantitative Study of Science: An Examination of the Literature, *Science Studies,* 4 (1974) 279–294.
17. PRICE, op. cit., nte 2, 74.
18. R. K. MERTON, The Matthew Effect in Science, *Science,* 159 (5 January 1968) 59–63.
19. E. SHILS, Centre and Periphery, in *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi on his Seventieth Birthday,* Routledge, London, 1961, p. 117–130.
20. PRICE, op. cit., note 2, 86.
21. C. KADUSHIN, Networks and Circles in the Production of Culture, in: *The Production of Culture,* R. A. PETERSON (Ed.), Sage, Beverly Hills, 1976, p. 107–122.
22. M. N. BYSTRYN, Variation in Artistic Circles, *The Sociological Quarterly,* 22 (Winter 1981) 120–132.
23. PRICE, op. cit., note 1, 85.
24. J. BEN-DAVID, R. COLLINS, Social Factors in the Origins of a New Science: The Case for Psychology, *American Sociological Review,* 31 (1966) 451–465; A. J. IHDE, An Inquiry into the Origins of Hybrid Sciences: Astrophysics and Biochemistry, *Journal of Chemical Education,* 46 (April 1969) 193–196; B. RUSSETT, Methodological and Theoretical Schools in International Relations, in: *Design for International Relations Research: Scope, Theory Methods and Relevance,* N. D. PALMER (Ed.), American Academy of Political and Social Science, Philadelphia, 1970, p. 87–105.
25. K. W. DEUTSCH, D. SENGHASS, J. PLATT, Conditions Favoring Major Advances in Social Sciences, *Science,* 171 (5 February 1971) 450–459.
26. D. de S. PRICE, D. de B. BEAVER, Collaboration in an Invisible College, *American Psychologist,* 2 (November 1966) 1011–1018; N. C. MULLINS, The Distribution of Social and Cultural Properties in Informal Communication Networks Among Biological Scientists, *American Sociological Review,* 33 (1968) 786–797; N. W. STORER, T. PARSONS, The Disciplines as a Differentiating Force, in: *The Foundation of Access to Knowledge – A Symposium,* E. B. MONTGOMERY (Ed.), Division of Summer Sessions, Syracuse University, Syracuse, N. Y., p. 101–121.
27. E. B. PARKER, W. J. PAISLEY, R. GARRETT, *Bibliographic Citations as Unobtrusive Measures of Scientific Communication,* Stanford University, Institute for Communication Research, Stanford, Cal., 1967.
28. A. J. MEADOWS, J. G. O'CONNOR, Bibliographic Statistics as a Guide to Growth Points in Science, *Science Studies,* 1 (January 1971) 95–99.
29. N. KAPLAN, The Norms of Citation Behavior: Prolegomena to the Footnote, *American Documentation,* 16 (1965) 179–184.
30. PRICE, op. cit., note 2, 65.
31. For some it is a growth industry; see E. GARFIELD, *Essays of an Information Scientist,* Vol. 1, 1962–1973; Vol. 2, 1974–1976, ISI Press, Philadelphia, 1977.
32. PRICE, op. cit., note 2, 78.
33. P. BOSSERMAN, Review of Crane's *Invisible Colleges, American Journal of Sociology,* 79 (July 1973) 180–182.
34. C. S. FISHER, The Death of a Mathematical Theory: A Study in the Sociology of Knowledge, *Archive for History of Exact Sciences,* 3 (1966) 137–159; D. L. KRANTZ

(Ed.), *Schools of Psychology,* Appleton Century-Crofts, New York, 1969; G. M. SWATEZ, The Social Organization of a University Laboratory, *Minerva,* 8 (January 1970) 36–58.
35. S. WOOLGAR, Laboratory Studies: A Comment on the State of the Art, *Social Studies of Science,* 12 (November 1982) 481–498.
36. T. S. KUHN, *The Structure of Scientific Revolutions,* University of Chicago Press, Chicago, 1962.
37. D. E. CHUBIN, 1976, op. cit., note 1.
38. PRICE, op. cit., note 2, 91.
39. CHUBIN, 1983, op. cit., note 1.
40. J. R. GUSFIELD, Historical Problematics and Sociological Fields: American Liberalism and the Study of Social Movements, *Research in Sociology of Knowledge, Sciences and Art,* 1 (1978) 121–149.
41. I. SPIEGEL-ROESING, The Study of Science, Technology, and Society (SSTS): Recent Trends and Future Challenges, in: *Science, Technology, and Society: A Cross-Disciplinary Pespctive,* I. SPIEGEL-ROESING, D. de S. PRICE (Eds.), Sage, Beverly Hills, 177, p. 7–42.
42. D. O. EDGE, Quantitative Measures of Communication in Science: A critical Review, *History of Science,* 17 (1979) 102–134.
43. R. K. MERTON, Insiders and Outsiders: A Chapter in the Sociology of Knowledge, *American Journal of Sociology,* 77 (July 1972) 9–47.
44. N. C. MULLINS, A Sociological Theory of Normal and Revolutionary Science, in: *Determinants and Control of Scientific Development,* K. D. KNORR et al. (Eds), D. Reidel, Boston, 1975.
45. J. R. COLE and S. COLE, *Social Stratification in Science,* University of Chicago Press, Chicago, 1973; J. BEN-DAVID, Organization, Social Control, and Cognitive Change in Science, in: *Culture and its Creators: Essays in Honor of Edward Shils,* J. BEN-DAVID, T. N. CLARK (Eds,), University of Chicago Press, Chicago, 1977, p. 244–265; J. BEN-DAVID, Emergence of National Traditions in the Sociology of Science: The United States and Great Britain, in *Sociology of Science,* J. GASTON (Ed.), Jossey-Bass, San Franciasco, 1978, p. 197–218; H. A. ZUCKERMAN, *Scientific Elite: Nobel Laureates in the United States,* Free Press, Newy York, 1977.
46. HARGENS, op. cit., note 3.
47. J. R. COLE, H. ZUCKERMAN, The Emergence of a Scientific Specialty: The Self-Exampli-fying Case of the Sociology of Science, in: *The Idea of Social Structure: Papers in Honor of Robert K. Merton,* L. A. COSER (Ed.), Harcourt Brace, New York, 1975. p. 139–174; J. GASTON, Sociology of Science and technology, in: *A Guide to the Culture of Science, Technology and Medicine,* P. T. DURBIN (Ed.), Free Press, New York, p. 465–526; H. ZUCKERMAN, R. B. MILLER (Eds), Science Indicators: Implications for Research and Policy, *Scientometrics,* 2 (special issue, October 1980) 327–448.
48. R. K. MERTON, The Sociology of Science: An Episodic Memoir, in: *The Sociology of Science,* R. K. MERTON, J. GASTON (Eds.), Carbondale, Southern Illinois University Press, 1977, p. 3–141.
49. I. I. MITROFF, *The Subjective Side of Science: A Philosophical Enquiry into the Psychology of the Apollo Moon Scientists,* Elsevier, New York, 1974; D. E. CHUBIN, op. cit., note 1; S. P. TURNER, D. E. CHUBIN, Chance and Eminence in Science: Ecclesiastes II, *Social Science Information,* 18 (1979) 437–449.
50. R. KROHN, Scientific Ideology and Scientific Process: The Natural History of a Conceptual Shift, in: *The Social Production of Scientific Knowledge,* E. MENDELSOHN, P. WEINGART, R. WHITLEY (Eds), D. Reidel, Boston, 1977, p. 69–99; B. GRUENBERG, The Problem of Reflexivity in the Sociology of Science, *Philosophy of the Social Sciences,* 8 (December 1978) 321–343; M. A. OVERINGTON, Doing What Comes Rationally: Some Developments in Metatheory, *American Sociologist,* 14 (February 1979) 2–12.

51. S. RESTIVO, Notes and Queries on Sicence, Technology, and Human Values, *Science, Technology and Human Values,* Science, Technology and Human Values, 6 (Winter 1981) 20–24.
52. For example, H. MARTINS, The Kuhnian 'Revolution' and its Implications for Sociology, in: *Imagination and Precision in the Social Sciences,* T. J. NOSSITER et al. (Eds.), Faber and Faber, London, 1972, p. 13–58; P. WEINGART, On a Sociological Theory of Scientific Change, in: *Social Processes of Scientific Development,* R. WHITLEY (Ed.), Routledge and Kegan Paul, London, 1974, p. 45–68.
53. C. J. LAMMERS, Mono- and Poly-paradigmatic Developments in Natural and Social Sciences, in: *Social Processes of Scientific Development,* R. WHITLEY (Ed.), Routledge and Kegan Paul, London, 1974, p. 123–147.
54. R. D. WHITLEY, Black Boxism and the Sociology of Science: A Discussion of the Major Developments in the Field, *The Sociological Review Monograph,* 18 (September 1972) 61–92; R. D. WHITLEY, *Social Processes of Scientific Development,* Routledge and Kegan Paul, London, 1974; R. D. WHITLEY, Components of Scientific Activities, Their Characteristics and Institutionalization in Specialties and Research Areas, in: *Determinants anc Controls of Scientific Development,* K. KNORR et al. (Eds), D. Reidel, Dordrecht, 1975. p. 37–73.
55. J. LAW, D. FRENCH, Normative and Interpretive Sociologies of Science, *Sociological Review,* 22 (1974) 581–595.
56. *Social Studies of Science,* Special issue: Aspects of the Sociology of Science, 6 (September 1976).
57. M. MULKAY, Norms and Idology in Science, *Social Science Information,* 15 (1976) 637–656.
58. R. JOHNSTON, Contextual Knowledge: A Model for the Overthrow of the Internal/External Dichotomy in Science, *Australian and New Zealand Journal of Sociology,* 12 (October 1976) 196–203.
59. W. van den DAELE, W. KROHN, P. WEINGART, The Political Direction of Scientific Development, in: *The Social Production of Scientific Knowledge,* E. MENDELSOHN et al. (Eds.), D. Reidel, Boston, 1977, p. 219–242. A Notable Predecessor of Such Work in: S. S. BLUME, *Toward a Political Sociology of Science,* The Free Press, New York, 1974.
60. G. N. GILBERT, Measuring the Growth of Science: A Review of Indicators of Scientific Growth, *Scientometrics,* 1 (1978) 9–34.
61. R. G. A. DOLBY, Reflections on Deviant Science, in: *On the Margins of Science: The Social Construction of Rejected Knowledge,* R. WALLIS (Ed.), University of Keele, Staffordshire, 1979, p. 9–47.
62. M. MULKAY Knowledge and Utility: Implications for the Sociology of Knowledge, *Social Studies of Science,* 9 (February 1979) 63–80.
63. B. BARNES, *Scientific Knowledge and Sociological Theory,* Routledge and Kegan Paul, London, 1974; J. LAW, Is Epistemology Redundant? A Sociological view, *Philosophy of the Social Sciences,* 5 (1975) 317–337; D. BLOOR, *Knowledge and Social Imagery,* Routledge and Kegan Paul, London, 1976.
64. H. MEYNELL, On the Limits of the Sociology of Knowledge, *Social Studies of Science,* 7 (197) 489–500.
65. E. MILLSTONE, A Framework for the Sociology of Knowledge,, *Social Studies of Science,* 8 (1978) 111–125.
66. R. TRIGG, The Sociology of Knowledge ,(Review of Bloor's *Knowledge and Social Imagery), Philosophy of the Social Sciences,* 8 (1978) 289–298.
67. M. NEVE, The Naturalization of Science, *Social Studies of Science,* 10 (August 1980) 375–391.

68. H. M. COLLINS, The Investigation of Frames of Meaning in Science: Complementarity and Compromise, *The Sociological Review,* 27 (1979) 703–718. A fuller, self-exemplifying statement of this program is H. M. COLLINS, The Sociology of Scientific Knowledge, *Annual Review of Sociology,* 9 (1983) 265–285.
69. B. BARNES, On the Causal Explanation of Scientific Judgement, *Social Science Information,* 19 (1980) 685–695.
70. J. E. McGUIRE, Newton and the Demonic Furies: Some Current Problems and Approaches in the History of Science, *History of Science,* 11 (1973) 21–48; S. SHAPIN, A. THACKRAY, Prosopography as a Research Tool in History of Science: The British Scientific Community, 1700–1900, *History of Science,* 12 (1974) 1–28; G. WERSKY, *The Visible College: The Collective Biography of British Scientific Socialists of the 1930s,* Holt, Rinehart and Winston, New York, 1978.
71. A. THACKRAY, Measurement in the Historiography of Science, in: *Toward a Metric Science,* Y. ELKANA et al. (Eds), Wiley, New York, 1977, p. 11–30; R. F. BUD, P. T. Caroll, J. L. STURCHIO, A. THACKRAY, *Chemistry in America, 1876–1976: An Historical Application of Science Indicators,* A Report to the National Science Foundation, University of Pennsylvania, 1978 (D. Reidel, forthcoming 1984).
72. M. TEICH, R. M. YOUNG (Eds), *Changing Perspectives in the History of Science,* Heinemann, London, 1972; R. MacLEOD, Changing Perspectives in the Social History of Science, in: *Science, Technology, and Society: A Cross-Disciplinary Perspective,* I. SPIEGEL-ROESING, D. de S. PRICE (Eds.), Sage, Beverly Hills, 1977, p. 149–195; M. P. CROSLAND, Aspects of International Scientific Collaboration and Organization Before 1900, in: *Human Implications of Scientific Advance,* E. G. FORBES (ed.), Edinburgh University Press, Edinburgh, 1978. p. 114–125; A. OLESON, J. VOSS (Eds.), *The Organization of Knowledge in Modern America, 1860–1920,* Johns Hopkins University Press, Baltimore, 1979. A recent and thorough review is S. SHAPIN, History of Science and its Sociological Reconstructions, *History of Science,* 20 (1982) 157–211.
73. G. N. CANTOR, Method in History' For and Against, *History of Science,* 14 (1976) 265–276.
74. I. LAKATOS, A. MUSGRAVE (Eds.), *Criticism and the Growth of Knowledge,* Cambridge University Press, Cambridge, 1970.
75. N. REINGOLD, Through Paradigm-land to a Normal History of Science, *Social Studies of Science,* 10 (November 1980) 475–496.
76. For example, D. SHAPERE, The Paradigm Concept, *Science,* 172 (14 May 1971) 706–709; I. SCHEFFLER, Discussion: Vision and Revolution: A Postrscript on Kuhn, *Philosophy of Science,* 39 (September 1971) 366–374.
77. REINGOLD, op. cit., note 75; for an extensive accreditation, see B. BARNES, *T. S. Kuhn and Social Science,* Columbia University Press, New York, 1982.
78. R. BHASKAR, *A Realist Theory of Science,* Leeds Books, Leeds, 1975.
79. S. TOULMIN, From Form to Function: Philosophy and History of Science in the 1950s and Now, *Daedalus,* 106 (Summer 1978) 143–162.
80. Y. ELKANA, Two-tier Thinking: Philosophical Realism and Historical Relativism, *Social Studies of Science,* 8 (1978) 309–326.
81. L. LAUDAN, *Progress and Its Problems,* University of California Press, Berkeley, 1977; L. LAUDAN, Views of Progress: Separating the Pilgrims from the Rakes, *Philosophy of the Social Sciences,* 10 (1980) 273–286.
82. D. E. CHUBIN, op. cit. note 3.
83. G. RADNITZKY, Towards a System Philosophy of Scientific Research, *Philosophy of the Social Sciences,* 4 (1974) 369–398.

84. P. FEYERABEND, *Against Method,* Verso, London, 1975; P. FEYERABEND, From Incompetent Professionalism to Professionalized Incompetence: The Rise of a New Breed of Intellectuals, *Philosophy of the Social Sciences,* 8 (1978) 37–53.
85. A. W. GOULDNER, Prologue to a Theory of Revolutionary Intellectuals, *Telos,* 26 (Winter 1957–76) 3–36.
86. MITROFF, op. cit, note 49.
87. I. I MITROFF, R. H. KILMANN, *Methodological Approaches in the Social Sciences,* Jossey-Bass, San Francisco, 1978.
88. R. FISCH, Psychology of Science, in: *Science, Technology and Society: A Cross Disciplinary Perspective,* I. SPIEGEL-ROESING, D. de S. PRICE (Eds), Sage, Beverly Hills, 1977, p. 227–318; M. J. MAHONEY, Psychology of the Scientist: An Evaluative Review, *Social Studies of Science,* 9 (August 1979) 349–375.
89. J. GOODFIELD, Humanity in Science: A Perspective and a Plea, *Science,* 198 (11 November 1977) 580–585.
90. C. GEERTZ, Blurred Genres: The Refiguration of Social Thought, *American Scholar,* 56 (Spring 1980) 165–179.
91. B. LATOUR, S. WOOLGAR, *Laboratory Life: The Social Construction of Scientific Facts,* Sage, Beverly Hills, 1979; K. D. KNORR-CETINA, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science,* Pergamon Press, New York, 1981.
92. R. KROHN, Introduction: Toward the Empirical Study of Scientific Practice, in: *The Social Process of Scientific Investigation,* K. D. KNORR et al. (Eds), D. Reidel, Boston, 1980, p. xxi–xxv.
93. G. N. GILBERT, M. J. MULKAY, Contexts of Scientific Discourse: Social Accounting in Experimental Papers, in: *The Social Process of Scientific Investigation,* K. D. KNORR et al. (Eds), D. Reidel, Boston, 1980, p. 269–294.
94. E. MENDELSOHN, P. WEINGART, R. WHITLEY (Eds.), *The Social Production of Scientific Knowledge,* D. Reidel, Dordrecht, 1977.
95. Two come to mind: S. S. BLUME (Ed.), *Perspectives in the Sociology of Science,* Wiley and Sons; Chistester, 1977 and the very recent K. D. KNORR-CETINA, M. MULKAY (Eds.), *Science Observed: Perspectives on the Social Study of Science,* Sage, Beverly Hills, 1983.
96. S. RESTIVO, New Directions in the Sociology ,of Science, *International Society for Sociology of Knowledge Newsletter,* 7 (May 1981, special issue) 3–35; S. RESTIVO, Commentary: Some Perspectives in Contemporary Sociology of Science, *Science, Technology and Human Values,* 6 (Spring 1981) 22–30; R. COLLINS, S. RESTIVO, Development, Diversity and Conflict in the Sociology of Science, *The Sociological Quarterly,* 24 (Spring 1983) 185–200; D. E. CHUBIN, S. RESTIVO, The 'mooting' of Science Studies: Research Programs and Science Policy, in: *Science Observed: Perspectives on the Social Study of Science,* K. D. KNORR-CETINA, M. MULKAY (Eds), Sage, Beverly Hills, 1983, p. 53–83.
97. H. M. COLLINS, T. J. PINCH, *Frames of meaning: The Social Construction of Extraordinary Science,* Routledge and Kegan Paul, London, 1982.
98. D. T. CAMPBELL, Evolutionary Epistemology, in: *The Philosophy of Karl Popper,* Vol. 14–1, P. A. SCHIPPP (Ed.), Open Court, LaSalle, Ill., 1974.
99. C. A. HOOKER, Philosophy and Meta-philosophy of Science: Empiricism, Popperianism and Realism, *Synthese,* 32 (1975) 177–231.
100. D. E. CHUBIN, S. RESTIVO, op. cit., note 96.
101. M. MULKAY, *Science and the Sociology of Knowledge,* Allen and Unwin, London, 1979.
102. L. FLECK, *Genesis and Development of a Scientific Fact,* T. J. TRENN, R. K. MERTON (Eds), F. BRADLEY, T. J. TRENN (Trans.), University of Chicago Press, Chicago, 1979.

103. B. G. ROSENKRANTZ, Reflecktions: Review of Fleck's *Genesis and Development of a Scientific Fact, Isis,* 71 (1981) 96–99.
104. A. MacINTYRE, Ideology, Social Science and Revolution, *Comparative Politics,* 5 (April 1973).
105. T. F. GIERYN, Relativist/Constructivist Programs in the Sociology of Science: Redundance and Retreat, *Social Studies of Science,* (May 1982) 279–297.
106. S. DEDIJER (Ed.), *An Attempt at a Bibliography of Bibliographies in the Science of Science,* Science Policy Center, Lund, Sweden, 1969.
107. E. CRAWFORD (compiler), The Sociology of the Social Sciences: An International Bibliography, *Social Science Information,* 13 (1974) 215–223.
108. R. HAHN, *A Bibliography of Quantitative Studies on Science and Its History,* Berkeley Papers in History of Science III, Berkeley, Calif., 1980.
109. J. GASTON op. cit., note 47.
110. A PRITCHARD, G. WITTIG, *Bibliometrics: A Bibliography and Index, Volume 1: 1874–1959,* ALLM Books, Watford, England, 1981.
111. C. MITCHAM, J. GROTE, Technology Assessment: Supplementary Bibliography, *Research in Philosophy and Technology,* 2 (1979) 357–370.
112. M. J. IVORY, J. LaPORTE, H. G. SMALL, J. STANLEY, *Citation-Analysis: An Annotated Bibliography,* Institute for Scientific Information, Philadelphia, 1976.
113. OFFICE OF PLANNING AND POLICY ANALYSIS, *Studies of Scientific Disciplines: An Annotated Bibliography,* D. C., National Science Foundation, 1979.
114. M. MULKAY, Sociology of Science in the West, *Sociology,* 18 (Winter 1980), Part I (p. 1–116), Bibliography (p. 133–184).
115. These periodicals in history, information science, management, philosophy, psychology and sociology, and the first years I reviewed of each through unbound issues in 1981, are as follows: *Academy of Management Review* (1976), *American Journal of Sociology* (1972), *American Sociological Review* (1972), *American Psychologist* (1974), *The American Sociologist* (1972), *4S Newsletter (Society for Social Studies of Science)* (1977), *Harvard Newsletter on Public Conceptions of Science* (now *Science, Technology, and Human Values)* (1974), *History of Science* (1972), *Human Development* (1972), *Information Storage and Retrieval* (now *Information Processing and Management)* (1972), *Isis* (1972), *Journal of the American Society for Information Science* (1972), *Journal of Documentation* (1972), *Minerva* (1972), *Philosophy of the Social Sciences* (1972), *Science Studies* (now *Social Studies of Science)* (1972), *Social Science Information* (1972), In addition, spot checks of the following journals wee made: *Journal of the History of the Behavioral Sciences, Philosophy of Science, Research in the Sociology of Knowledge, Sciences and Art* (an annual), *The Sociological Quarterly,* and *Sociology.* I think my search biases are apparent: history philosophy and information science are under-represented relative to sociology. Non-English journals have been ignored.
116. H. SHUCHMAN, E. ABEL, S. FRAMPTON, *Self-Regulation in the Professions: Accounting, Law, Medicine,* Final Report to the National Science Foundation, The Futures Group, 1981.
117. A. M. WEINBERG, *Reflections on Big Science,* MIT Press, Cambridge, Mass., 1967.
118. D. NELKIN, Threats and Promises: Negotiating the Control of Research, *Daedalus,* 107 (Spring 1978) 191–209; J. R. RAVETZ, Criticisms of Science, in: *Science, Technology and Society: A Cross Disciplinary Perspective,* I. SPIEGEL-ROESING, D. de S. PRICE (Eds), Sage, Beverly Hills, 1977, p. 71–89.
119. G. E. MARKLE, J. C. PETERSEN (Eds.), *Politics, Science, and Cancer: The Laetrile Phenomenon,* CO, Westview Press, Boulder, 1980; D. NELKIN, Science and Technology Policy and the Democratic Process, in: *The Five-Year Outlook: Problems, Opportunities and*

Constraints in Science and Technology, Vol. II, National Science Foundation, Washington, D. C., 1980, p. 483–492; J. D. MILLER, Atittudes Toward Genetic Modification Research: An Analysis of the Views of the Sputnik Generation, *Science, Technology and Human Values,* 7 (Spring 1982) 37–43.

120. P. BUCK, Images of the Scientific "Community": Commentary on Papers by Alice Kimball Smith and Dorothy Nelkin, *Newsletter on Science, Technology and Human Values,* 24 (June 1978) 45–47.

121. L. RAINWATER, D. J. PITTMAN, Ethical Problems in Studying a Politically Sensitive and Deviant Community, *Social Problems,* 14 (Spring 1967) 357–366; K. E. STUDER, D. E. CHUBIN, Ethics and the Unintended consequences of Social Research: A Perspective from the Sociology of Science, *Policy Sciences,* 8 (1977) 111–124; S. BOK, Freedom and Risk, *Daedalus,* 107 (Spring 1978) 115–127.

122. J. H. COMROE, R. D. DRIPPS, Scientific Basis for the Support of Biomedical Science, *Science,* 192 (9 April 1976) 105–111; N. RESCHER, *Scientific Progress: A Philosophical Essay on the Economics of Research in Natural Sciences,* University of Pittsburgh Press, Pittsburgh, 1978.

123. For example, J. BEN-DAVID, *The Scientist's Role in Society,* Prentice-Hall, Englewood Cliffs, NJ, 1971; P. FORMAN, The Financial Support and Political Alignment of Physicists in Weimar Germany, *Minerva,* 12 (1974) 39–66; M. HEIRICH, Why We Avoid the Key Questions: How Shifts in Funding of Scientific Inquiries Affect Decision-making About Science, in: *The Recombinant DNA Debate,* S. STICH, D. JACKSON (Eds), University of Michigan Press, Ann Arbor, 1977, p. 234–260; G. HOLTON, R. S. MORISON (Eds), Limits of Scientific Inquiry, *Daedalus,* 107 (special issue Spring, 1978); K. E. STUDER, D. E. CHUBIN, *The Cancer Mission: Social Contexts of Biomedical Research,* Sage, Beverly Hills, 1980.

124. PRICE, op. cit, note 2, 115.

125. My own assessment of this situation is contained in D. E. CHUBIN, A Philosophy of Knowledge Application: Unauthorized Science Policy, unpublished paper, 1984. Another notable assessment is J. SCHMANDT (guest co-editor), Linking Science to Policy: The Role of Technical Knowledge in Regulatory Decisionmaking, *Science, Technology and Human Values,* 9 (Winter 1984, special issue) 14–133.

126. E. SHILS, Intellectuals, Tradition, and the Traditions of Intellectuals: Some Preliminary Considerations, *Daedalus,* 101 (Spring 1972) 21–34; F. R. WESTIE, Academic Expectations for Professional Immortality: A Study of Legitimation, *The American Sociologist,* 8 (February 1973) 19–32.

127. For example, B. F. RESKIN, Sex Differences in Status Attainment in Science: The Case of the Postdoctoral Fellowship, *American Sociological Review,* 41 (August 1976) 597–612; L. R. HARMON, *A Century of Doctorates: Data Analyses of Growth and Change,* National Academy of Sciences, Washington, D. C., 1978; J. S. LONG, Productivity and Academic Position in the Scientific Career, *American Sociological Review,* 43 (December 1978) 889–908; D. E. CHUBIN, A. L. PORTER, M. E. BOECKMANN, Career Patterns of Scientists: A Case for Complementary Data, *American Sociological Review,* 46 (August 1981) 488–496.

128. B. C. GRIFFITH, N. C. MULLINS, Coherent Social Groups in Scientific Change, *Science,* 177 (15 September 1972) 959–964; D. E. CHUBIN, K. E. STUDER, Knowledge and Structures of Scientific Growth: Measurement of a Cancer Problem Domain, *Scientometrics,* 1 (January 1979) 171–193.

129. ZUCKERMAN, op. cit., note 45; E. F. KELLER, *A Feeling for the Organism: The Life and Work of Barbara McClintock,* W. H. Freeman, San Francisco, 1983.

130. D. LINDSEY, *The Scientific Publication System in the Social Sciences,* Jossey-Bass, San Francisco, 1978.
131. For example, S. COLE, L. RUBIN, J. R. COLE, *Peer Review in the National Science Foundation,* National Academy of Sciences; Washington, D. C., 1978; D. E. CHUBIN, Competence is not Enough: Essay Review of Cole, et al. 's *Peer Review in the National Science Foundation, Contemporary Sociology,* 9 (May 1980) 204–207; D. E. CHUBIN, Peer Review and the Courts: Notes of a Participant-Scientist, *Bulletin of Science, Technology and Society,* 2 (1982) 423–432. The British case for linking peer review to public policy is made in: J. IRVINE, B. R. MARTIN, Assessing Basic Research: The Case of the Isaac Newton Telescope, *Social Studies of Science,* 13 (1983) 49–86.
132. A. L. PORTER, F. A. ROSSINI, D. E. CHUBIN, T. CONNOLLY, Between Disciplines [letter], *Science,* 109 (29 August 1980) 966.
133. D. T. CAMPBELL, Ethnocentrism of Disciplines and the Fish-scale Model of Omniscience, in: *Interdisciplinary Relationships in the Social Sciences,* M. SHERIF, C. W. SHERIF (Eds), Aldine, Chicago, 1969, p. 327–348.
134. W. LEPENIES, History and Anthropology: A Historical Appraisal of the Current Contact Between the Disciplines, *Social Science Information,* 15 (1976) 287–306. Sociobiology is another obvious locus for disciplinary consolidation.
135. R. T. BARTH, R. STECK (Eds), *Interdisciplinary Research Groups: Their Management and Organization,* University of British Columbia, Vancouver, 1979.
136. P. H. BIRNBAUM, Academic Contexts of Interdisciplinary Research, *Educational Administration Quarterly,* 14 (1978) 80–97; F. A. ROSSINI, A. L. PORTER, Framworks for Integrating Interdiscplinary Research, *Research Policy,* 8 (1979) 70–79.
137. PRICE, op. cit., note 2, 81.
138. Elements of that agenda can be found in D. E. CHUBIN, A. L. PORTER, F. A. ROSSINI, Interdisciplinarity: How Do We Know Thee? , unpublished paper, November 1983.