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Sociology of Scientific Knowledge and Science Education Part 2: Laboratory Life Under the Microscope

PETER SLEZAK

School of Science and Technology Studies, University of New South Wales, Kensington, NSW 2033, Australia

ABSTRACT. This article is the second of two that examine some of the claims of contemporary sociology of scientific knowledge (SSK) and the bearing of these claims upon the rationale and practice of science teaching. In the present article the celebrated work *Laboratory Life* of Latour and Woolgar is critically examined. Its radical, iconoclastic view of science is shown to be not merely without foundation but an extravagant deconstructionist nihilism according to which all science is fiction and the world is said to be socially constructed by negotiation. On this view, the success of a theory is not due to its intellectual merits or explanatory plausibility but to the capacity of its proponents to "extract compliance" from others. If warranted, such views pose a revolutionary challenge to the entire Western tradition of science and the goals of science education which must be misguided and unrealizable in principle. Fortunately, there is little reason to take these views seriously, though their widespread popularity is cause for concern among science educators.

INTRODUCTION

The importance of practical laboratory work for a science education hardly requires emphasis and, accordingly, new insights into the role and significance of experimentation are to be welcomed. Correspondingly, however, there are grave dangers for a science education which may come to take seriously certain doctrines which are self-consciously subversive, having the avowed goal of deflating the pretensions of science both in its knowledge claims and in its claims to the possession of a special "method". The foundational classic of such deflationary laboratory studies is *Laboratory Life* of Latour and Woolgar which is an iconoclastic work, professing to "penetrate the mystique" (1979, p.18), dissolve the appearances and reveal the hidden realities of science-in-the-making at the laboratory workbench and in the tea room. By contrast with our alleged ignorance concerning the details of scientific practice, this study purports to give an exposé of the "internal workings of scientific activity" (1979, p.17). The conclusions of this work are undeniably radical, indeed astonishing, as we will see, and there are indications that these doctrines are having a direct influence in science classrooms.

Sociological constructivism has been described by David Stove (1991) as an illustration of the "fatal affliction" and "corruption of thought" in which people say things which are so bizarre that even they must know them to be false. Accordingly, it is salutary to preface the discussion of

these ideas by noting Jerry Fodor's response to notions of this general sort. In his characteristic manner, Fodor (1986) has wryly remarked upon philosophical "cures for which there is no adequate disease".

> It is a curiosity of the philosophical temperament, this passion for radical solutions. Do you feel a little twinge in your epistemology? Absolute skepticism is the thing to try. Has the logic of confirmation got you down? Probably physics is a fiction. Worried about individual objects? Don't let anything in but sets. Nobody has yet suggested that the way out of the Liar paradox is to give up talking, but I expect it's only a matter of time. Apparently the rule is: if aspirin doesn't work, try cutting off your head (1986, p.1).

Writing with considerable hyperbole, Fodor complains of certain doctrines which he describes as "grotesque proposals" though they are, in the case concerned, rather mild and entirely respectable philosophical views. Although the doctrines in question are not in themselves relevant to our interests here, his comments are worth noting only because what Fodor intends as extravagant parody are precisely the views which have been embraced literally by certain sociologists of scientific knowledge. Sociologists of science, belatedly discovering that scientific knowledge is less than absolutely certain, have concluded that all knowledge must be entirely delusory. Feeling a little twinge in their epistemology, they have found solace in absolute skepticism; noticing that the inference from evidence to theory is not apodictically certain, they have concluded that science is a fiction. Remarkably, we see Fodor's parody expressed in these terms being embraced literally as the principal insight of sociological inquiry into science. Thus, in their celebrated work *Laboratory Life*, Bruno Latour and Steve Woolgar declare that all of science is merely the "construction of fictions" (1979, p.284). The undoubtedly difficult questions concerning the nature of science have led these sociologists to conclude that there is no such thing! Latour explains the profound insights emerging from the new discipline:

> Now that field studies of laboratory practice are starting to pour in, we are beginning to have a better picture of what scientists do inside the walls of these strange places called "laboratories" ... The result, to summarize it in one sentence, was that nothing extraordinary and nothing 'scientific' was happening inside the sacred walls of these temples (1983, p.141).

> ... the moment sociologists walked into laboratories and started checking all these theories about the strength of science, they just disappeared. Nothing special, nothing extraordinary, in fact nothing of any cognitive quality was occurring there (1983, p.160).

Needless to say, the implications of such discoveries for science education must be revolutionary. Undeniably the foundational classic among field

studies of laboratory practice, *Laboratory Life*, (whose subtitle is *The Social Construction of Scientific Facts*) makes the radical claim that scientific facts and/or objects (the distinction is blurred) are socially constructed through "negotiation". An indication of the educational impact of these doctrines is to be seen in the fact that the foregoing remarks have been approvingly quoted in a recent science teachers' journal (Gough 1993) in an article which recommends a radical new vision of "the reality of the scientific process". Accordingly, in this paper I undertake a detailed, critical examination of this new vision of science with a view to exposing its largely unnoticed enormity. The consequences of this conception of science for educators need hardly be drawn out explicitly, for on this view there is no such thing as a distinctive scientific enterprise at all. The pedagogical implications are clear: science is not essentially different from any other social institution and, therefore, science education is presumably only socialization. Rather than learning as a cognitive process involving reasoning, logic and understanding, science education involves merely the observance of arbitrary practices. Although Latour and Woolgar do not explicitly address the questions of most direct interest to science educators as such, their characterization of science clearly suggests the appropriate role of the teacher. They write

> Each text, laboratory, author and discipline strives to establish a world in which its own interpretation is made more likely by virtue of the increasing number of people from whom it extracts compliance (1986, p.285).

By repudiating the role of rational considerations, presumably the function of science teacher is that of principal agent for the extraction of compliance. Education is not a matter of cultivating insight, warranted belief and critical thought but only ideological commitment and political allegiance.

"DERRIDADAISM": READERS AS WRITERS OF THE TEXT.

Such views are undeniably radical and are, indeed, sufficiently unusual for their precise character and significance to have been widely overlooked. A selective perception has led many commentators to miss the quite explicit intentions of Latour and Wooolgar. For example, although clearly aware of the hostility it can evoke, Ian Hacking (1988) has given a sympathetic, even enthusiastic, review of *Laboratory Life*. Berating philosophers for having neglected this work, Hacking wants them "to get away from their idle ambulatory pondering about realism in the contexts of crassly simplified theories, and to attend to some of the realities of the experimental life" (1988, p.278). Howèver, it now seems this has been a mistake. A measure of the perversity of this work is the fact that in the new edition of their book, Latour and Woolgar (1986) tell us that laboratory studies such as their own should, after all, *not* be understood as providing a closer

look at the actual production of science at the workbench, since this view would be "both arrogant and misleading" (1986, p.282), by presuming some "privileged access to the 'real truth' about science" which will emerge from a more detailed observation of the technical practices. This is, of course, precisely the virtue which Hacking has seen in laboratory studies in general and that of Latour and Woolgar in particular. Clearly, Hacking has missed the point. Instead, Latour and Woolgar explain that their work "recognizes itself as the construction of fictions about fiction constructions" (1986, p.282). This is the textualism of Derrida combined with a much-vaunted reflexivity. They continue: " . . . all texts are stories. This applies as much to the facts of our scientists as to the fictions 'through which' we display their work" (1986, p.284). Their own work, then, just like all of science, has no determinate meaning since, as they explain, "It is the reader who writes the text" (1986, p.273).

Quite aside from its consequences for our prior conceptions of science, here we see a notorious Deconstructionist affectation which conveniently serves to protect Latour and Woolgar against any conceivable criticism. This move by Latour and Woolgar is, in fact, a characteristic feature of Deconstructionist writings, and has the effect of showing any critic to have *ipso facto* failed to understand the subtlety and sophistication of the work. In particular, the use of logic and the traditional categories of thought is sufficient evidence of a critic's misunderstanding. This is not only empty and offensive to well-intended criticism, but also suggests something of the subversive educational morals to be drawn from *Laboratory Life*.

Hacking observes *en passant* with apparent equanimity that the notion of "inscriptions" as used by Latour and Woolgar is adopted from Derrida, though Hacking chooses not to focus upon what he describes as merely "out-dated fascination with the sentence so characteristic of Paris intellectuals in the late sixties". Unfortunately, as we will see, the idea of "inscription" for Latour and Woolgar is not merely incidental or unimportant and so readily overlooked. It is, in fact, a central idea of *Laboratory Life* which reveals the peculiarities of the enterprise. Above all, Hacking shows remarkable insouciance in the face of this explicit invocation of notorious Deconstructionist ideas whose contribution to literary criticism are questionable enough, but when applied to science are quite misguided. Sufficiently evident in the work itself, its Deconstructionist character is now explicitly affirmed in the postscript to the second edition of *Laboratory Life* and is the source of its deliberate and avowed subversiveness.

Although disagreeing with what he takes to be their central thesis, Hacking nonetheless offers high praise and suggests that "What is great about Latour and Woolgar is their detailed and articulate attention to experiment, a proper study for philosophers of science" (1988, p.293). Conceding this much however, there are other considerations more relevant to an appropriate assessment. Members of other factions within the enterprise of SSK do not appear to share Hacking's illusions: Thus, Collins

and Yearley (1992) have recently said "Those engaged from day to day with the problem of reflexivity would, if they could achieve their aims, know nothing at all. We might say that SSK has opened up new ways of knowing nothing" (1992, p.302). Hacking's charity can be explained when it is realized that his portrayal of the "irrealism" of Latour and Woolgar is vastly more sensible and innocuous that the thesis they actually propound. Characteristic of Deconstructionist-style writings, the use of metaphor and other literary devices provides rich material for unconstrained interpretation. In case there was any doubt about this Rorschach ink-blot character of their own work, Latour and Woolgar explicitly invite such an approach in the new postscript. Of course, as Lehman (1991) has pointedly observed, Deconstructionists bitterly defend their own authorial intentions in heated polemics notwithstanding their disavowals. Latour and Woolgar, too, drop the affectation on subsequent pages where they defend their view of an issue and "our original use of the term" (1986, p.278). Thus, Hacking's particular "construction" of the text portrays a theory which does not deserve to be controversial and is one which would hardly merit all the fuss. However, Hacking's sympathetic construal makes it clear that Latour and Woolgar rely on the slippery ambiguities to be both scandalous and innocuous at the same time. It is of some importance, therefore, to see the more outrageous reading clearly, for it is the one Latour and Woolgar deliberately rely upon for the radical novelty of their work.

The contrast between the work of Bloor (1976) and that of Latour and Woolgar is interesting: Where Bloor professes to adhere to the usual principles of scientific inquiry, Latour and Woolgar engage in a game which Lehman (1991) has aptly called "Derridadaism". They manage to evade criticism only by adopting deconstructionist double-talk and affecting a posture of nihilistic indifference to the ultimate cogency of their own thesis. In keeping with the principle of reflexivity, they embrace the notion that their own text (like the science they describe) has no "real" meaning, being "an illusory, or at least, infinitely renegotiable concept" (1986, p.273). Rejecting the belief in "the intrinsic existence of accurate and fictitious accounts per se" (1986, p.284), Latour and Woolgar suggest that the only judgement which can be made is a function of the "modalising and demodalising of statements" by its readers who "transform the status of these claims, making them more or less factual, dismembering them into black-boxes for different argumentative purposes, ridiculing them and so on" (1986, p.285). They admit only to a curiosity about how much political transformation of the field might be necessary in order to make their own account more plausible than its alternatives (1986, p.285). That is, its current implausibility is only due to its relative *political* disadvantages rather than the lack of any intellectual merits. Of course, the notion of "plausibility" for Latour and Woolgar is not a matter of assessment on the grounds of rational, cognitive criteria, but only a function of the "balance of forces" (1986, p.285).

ONLY KIDDING!

Latour and Woolgar explain that their own idea of indeterminate meaning is to be seen most clearly in the field of literary criticism. They assert that this idea has "a special relevance for the social study of science" where there have been similar notions involving the "generation of texts whose fate (status, value, utility, facticity) depends on their subsequent interpretation" (1986, p.273). Latour and Woolgar see the "construction of scientific facts" – including their own work – as a generation of "texts" and it is this idea from literary theory which has come "as a welcome relief" from the "spectacle" of traditional analysis and criticism. Reflecting on the controversies surrounding their work, Latour and Woolgar observe that defenders and critics alike have engaged in this futile "spectacle" in which they have debated the presumed intentions of the authors. This "spectacle" is, of course, just the familiar exercise of critical thought which has been the Western Heritage since the presocratics. By conforming to this tradition of criticism, the benighted disputants who have sought to understand the work of Latour and Woolgar have, it turns out, missed the whole point. These authors now reveal that the 'real' meaning of a text must be recognised as illusory and indeterminate. The question of what the authors intended or what is reported to have happened "are now very much up to the reader".

It should be evident that taking seriously the Deconstructionist doctrine that the reader is the writer of the text immediately obviates the need for further discussion. In this Rorschach ink-blot approach no interpretation has any greater claim for validity than any other. The pose adopted by Latour and Woolgar will be deeply offensive to many. A work which is, at best, highly eccentric, paradoxical and obscure, is taken seriously by scholars who try to divine its meaning, whereupon the authors reveal these efforts to have been misguided – because there was no meaning to be found after all! These scholars had been duped into vainly searching for the import of a work, where, in fact, there was none. Latour and Woolgar now declare that the project of the book would have begun to succeed if readers had thought to ask themselves whether or not any of the reported conversations really took place at all, or whether the distinguished scientist, Jonas Salk, really wrote the introduction (1986, p.283–4). Since most normal people would not suspect anything quite so devious, the joke has been on them. At the expense of those scholars who have struggled with the impenetrable, infuriating obscurity of the work, Latour and Woolgar now reveal their little game "as a welcome relief from this spectacle". Their derisive attitude is clear enough towards those misguided scholars who "years after the initial publication of a volume ... continue to argue over 'what was actually intended' by its authors" (1986, p.273). This kind of practical joke is distasteful to anyone who is concerned with the serious underlying import of intellectual life. It is not to be humorless or incapable of appreciating a joke to worry about the

very possibility of being able to distinguish sense from nonsense. Above all, it is sobering to consider how a science class might be conducted in accordance with such a model of scholarship.

One might have had enough cause to suspect that the *Laboratory Life* should not have been taken seriously if only because in many respects it is completely incoherent and unintelligible. For example, some of the diagrams offered as explanatory schemas are impossible to decipher. Such minor problems did not appear to concern those teachers and researchers who have treated the book as among the foundational classics of the field. Presumably their incomprehension was attributed to the impenetrable profundity of the work and their own limitations. Still, the willingness to overlook what surely could not have been understood is a remarkable insight into the sociology of the sociology of science. A wide tolerance for highly paradoxical claims and deep obscurities is a somewhat depressing phenomenon – and of special relevance for those concerned with science education. Of particular significance here is the fact that the doctrines of Latour and Woolgar are evidently having some impact on high school science teachers and their conception of laboratory classes (see Gough 1993).

CONSTRUCTING THE WORLD

The constructivist thesis of Latour and Woolgar is supposed to be a perfectly general claim about the socially negotiated character of science and so the specific details of their particular case study need not concern us here. The focus of *Laboratory Life* is a biochemistry laboratory where scientists were concerned to isolate a certain biologically important compound and discover its structure. Due to technical difficulties with obtaining sufficient quantities of the compound from animals, the biochemical substance of interest was not directly analysed as such, but conjectured to be identical with a synthesized compound whose diagnostic assay behaviour appeared the same. This peculiarity of the investigation provides Latour and Woolgar with the opportunity to exploit the indirectness of the inference as especially suggestive of their constructivist thesis. However, first of all, it is entirely unclear how the special features of this particular case can be generalized to science as a whole, allegedly demonstrating the socially negotiated character of the entire edifice. That is, having to conjecture the structure or even the existence of a substance indirectly by synthesizing it artificially is hardly a model of science in general. Moreover, this case can be fully and clearly understood in literal terms without resorting to elaborate, suggestive, metaphorical re-descriptions. To be sure, one can *say* that the scientists "made" the conjectured naturally occurring substance identical with the synthesized compound and in this sense "constructed a fact". Ordinarily the meaning of such a *façon de parler* would be clear enough. The thesis of "constructing" facts

permits a sensible reading according to which the *theory* or *description* of a substance, including criteria of identity, are settled upon and perhaps even "socially negotiated" in a certain sense. This is an innocuous reading which would not warrant the sensation generated by *Laboratory Life*. Thus, the study in question takes a case whose peculiarities add to the scope for mystification. Playing on the words, one can also perversely choose to construe such banalities as something more paradoxical and seemingly profound – namely that the *substance itself* did not have an independent existence and was socially constructed. Metaphorically, one could say that the phenomena are constituted by the social processes in the laboratory, but on a sane interpretation this would be elliptical for the claim that our *theories* and not the objects themselves are so constituted. In like manner, one might say that Copernicus "removed the earth from the centre of the universe", but asserting this literally would be an attempt at humour or evidence of derangement.

WITCHCRAFT, ORACLES AND MAGIC AMONG THE ACADEMICS

On the face of it, the authors' own description of their project in *Laboratory Life* reads more like a parody than a serious inquiry. Upon entering the Salk Institute for a two-year study "Professor Latour's knowledge of science was non-existent; his mastery of English was very poor; and he was completely unaware of the existence of the social studies of science" (1986, p.273). It is from this auspicious beginning that the revolutionary insights in to science were to emerge. Of course, these apparent liabilities are portrayed as a unique advantage, since "he was thus in the classic position of the ethnographer sent to a completely foreign environment" (ibid.). Much is made of this "anthropological strangeness" or "analytic distance" which is compared to Schutz's conception of the perspective of "the stranger" in an alien culture. However, the idea that the inability to understand one's human subjects is a positive methodological virtue is surely a bizarre conception even for anthropology. For Latour and Woolgar, however, it is intimately connected with their doctrine of "inscriptions". The meaninglessness of the "traces, spots, points" and other recordings is a direct consequence of Latour's admitted scientific illiteracy. Predictably enough, from the perspective of complete ignorance, all these meaningful symbols are indiscriminable and must, therefore, be placed in the category of unintelligible markings – "inscriptions". Avoiding the possibility of understanding their subjects' behaviour is justified on the grounds that, just as the anthropologist does not wish to accept the witch-doctor's own explanations, so one should remain uncommitted to the scientists' rationalizations too. The absurdity of such an attitude follows from the simple failure to appreciate the difference between *understanding* the native and *believing* him. This approach which turns incomprehension into a methodology is an inadvertent parody of the complaints against

'positivist' approaches in the social sciences. If consistently followed, anthropology conducted according to these precepts would be reduced to recording the brute physical movements of its subjects.

DUHEM DÉJÀ VU

In these respects Latour and Woolgar have taken seriously Chomsky's parody of the methods of Behaviourism. If physics were to be conducted according to Behaviourist scruples relying only on externally observable events, Chomsky has noted that physics could be only a "science of meter readings". As it happens, Duhem made a similar point about the unintelligibility of a scientific laboratory precisely on the approach which Latour and Woolgar adopt as their guiding method. *Laboratory Life* inadvertently rehearses a scenario well known to historians and philosophers of science. In his classical work *The Aim and Structure of Physical Theory* (Chapter IV) Duhem (1906/1962) describes the situation in which a naive person enters a physics laboratory and sees only the physical items of the apparatus such as an iron bar carrying a mirror, coils, electric battery and copper wire etc. Duhem suggests that if you ask the physicist what he is doing, he does not say that he is merely studying the oscillations of the piece of iron carrying the mirror, but rather than he is measuring the "electrical resistance" of a coil. Duhem says:

> If you are astonished and ask him what meaning these words have, and what relation they have to the phenomena he has perceived and which you have at the same time perceived, he will reply that your question would require some very long explanations, and he will recommend that you take a course in electricity (1962, p.154).

Given their approach of self-confessed ignorance, it is hardly surprising that Latour and Woolgar arrive at the conclusion that the practices of the laboratory are in reality "a disordered array of observations with which scientists struggle to produce order" (1979, p.36) and the scientists are "routinely confronted by a seething mass of alternative interpretations" (1979, p.36).

POISON ORACLES AND OTHER LABORATORY EXPERIMENTS

From their anthropological field study Latour and Woolgar bring back remarkable insights into the exotic rituals of their natives: Latour cites the fact that "the belief in the 'scientificity' of science has disappeared" (1983, p.142). Though startling enough, this is evidently not all, for he argues that the very boundaries between science and other activities should be expunged. Latour is nothing if not logical here: he recognizes that this insight leads directly to a "naive but nagging question" – namely, "if

nothing scientific is happening in laboratories, why are there laboratories to begin with and why, strangely enough, is the society surrounding them paying for these places where nothing special is produced?" (1983, p.141–2). In the article concerned, I am unable to discern Latour's answer to this question which he describes as appearing "innocent enough" but "actually rather tricky". Indeed. It was the same question he had asked in *Laboratory Life* when explaining how he had come to "make sense" of the laboratory "in terms of a tribe of readers and writers who spend two-thirds of their time working with large inscription devices" (1979, p.69).

At one point, Latour reports his own growing, if still dim, understanding of the scientific ideas and research, but he bravely resists these subversive tendencies because he has not entirely lost sight of his anthropological perspective:

> . . . in the back of his mind there remains a nagging question. How can we account for the fact that in any one year, approximately one and a half million dollars is spent to enable twenty-five people to produce forty papers? (1979, p.70).

This is undoubtedly a deep mystery if one systematically refuses to understand the meaningfulness of the "inscriptions". From this vantage point, Isaac Newton's notebooks would be indiscriminable from fly droppings.

> It is evident that the uncritical acceptance of the concepts and terminology used by some scientists has had the effect of enhancing rather than reducing the mystery which surrounds the doing of science. Paradoxically, our utilisation of the notion of anthropological strangeness is intended to dissolve rather than reaffirm the exoticism with which science is sometimes associated. This approach, together with our desire to avoid adopting the distinction between "technical" and "social", leads us to what might be regarded as a particularly irreverent approach to the analysis of science. We take the apparent superiority of the members of our laboratory in technical matters to be insignificant, in the sense that we do *not* regard prior cognition . . . as a necessary prerequisite for understanding scientists' work. This is similar to an anthropologist's refusal to bow before the knowledge of a primitive sorcerer (1979, p.29).

In other words, Latour and Woolgar refuse here to accept the authority of our best science, though they happily countenance the magical transformation of physical substances into inscriptions.

This affectation of an Evans-Pritchard among the Azande is "anthropological strangeness" in a rather different sense of the term: no anthropologist was ever so strange. Of course, they no sooner articulate this methodological principle than abandon it in practice, as indeed they must. Quite aside from the issue of meaningful action, we may play the role of disbelieving anthropologist in an alien society where our skepticism rests

on our conviction that we have a better theory of the world. However, the option of playing anthropologist to ourselves in this respect is unavailable since our own theories are, ipso facto, the best we've got. In Neurath's figure of the boat we are rebuilding at sea, Latour and Woolgar imagine they can step outside the boat and scuttle it.

The absurdity of proposing to explain human behaviour "without recourse to the explanatory concepts of the inhabitants themselves" (1979, p.41) is not alleviated by any attempt to address the extensive literature on these problems in the philosophy of social sciences. Above all, the influential work of Winch (1958) on these very questions, like all other relevant discussions have been ignored by Latour and Woolgar.

Given his method, predictably enough, Latour finds the activities in the laboratory completely incomprehensible. Undaunted, and unwilling to allow this to become a liability, it becomes, in fact, the deep *insight* of *Laboratory Life*. The behaviour of the scientists not only *appears* meaningless, it *is* meaningless. In their conclusion, Latour and Woolgar reveal that "A laboratory is constantly performing operations on statements . . . " (1979, p.86), and the activities of the laboratory consist in manufacturing "traces, spots and points" with their "inscription devices". The production of papers with such meaningless marks is taken to be the main objective of the participants in essentially the same way that the production of manufactured goods is the goal of any industrial process. This is the view of science as sausage factory.

COMING OF AGE IN THE SALK INSTITUTE

Whatever insight might have been derived from seeing scientists as the Azande or Nuer, the comparision has long been pushed beyond its meaningfulness. Borrowing Evans-Pritchard's own pun, we might see Latour and Woolgar's anthropological posture as a "Nuerosis". Their case is evidently more like the now-notorious scandal of Margaret Mead in Samoa: these ethnographers, too, would have profited from understanding their natives better (see Freeman 1983). In the end, the whole idea has become a labored and somewhat ludicrous conceit. It should hardly need saying that the predicament of the ethnographer in an alien culture is not one deliberately chosen for its virtues as a vantage point from which to understand the natives. Whatever its advantages, anthropological distance is simply inherent in the problem of studying a society other than one's own. Its methods are developed not for their unique advantages but, *faute de mieux*, because of the demands of the situation. In like manner, the historian or archaeologist adopts a stance of *temporal* remoteness from his subject matter, say, the ancient Romans, not by choice but by necessity. But like archaeology, anthropology, too, is partly defined by this kind of predicament. The excavators of Pompeii or Troy would not *choose* to reconstruct the life of a society by digging up fragments of ceramic pots

if they could talk to their makers instead. Nor does the anthropologist choose to be an outsider to the society being studied. Our ethnographers of science, however, by contrast, are choosing to adopt methodological liabilities which they don't have.

The entire approach is misconceived due to a confusion over the goals of inquiry. Through adopting the perspective of the "Outsider" studies in sociology have provided certain insights by drawing attention to "taken-for-granted" practices and by bringing into relief those aspects which seem natural, unquestioned and even invisible to "Insiders". The value of this approach in sociology, however, has been to the scientist who is trying to highlight and explain those features of social life which are unnoticed and not thought to require explanation *by participants themselves*. The very familiarity of our own values, customs and beliefs hides them from our view and, therefore, seeing them through the eyes of a "stranger" reveals their existence and their problematic status. However, this situation is not properly adapted to the case of an ethnography of science because the community of "natives" is not that of the ethnographer in the relevant sense here. The sociologists' ignorance of the technical matters pertaining to the science in question makes him an outsider to begin with and, therefore, hardly needing to make what is familiar strange. Instead, Latour and Woolgar's method contrives to make the strange even stranger.

There is some unintended irony where Latour and Woolgar wish to explain the scientists' activities on the model of their own confusion. They say

> . . . we have every reason to believe that the accomplishment of this kind of task is no mean feat, as is clear from a consideration of the corresponding task faced by the observer [i.e. Latour and Woolgar] when confronted by his field notes (1979, p.37).

In other words, Latour and Woolgar take their own confusion to be typical and assume that the incomprehensibility of their own ill-informed observations must be generalizable. Thus, Latour and Woolgar presumptuously extrapolate their own predicament when faced with a blooming, buzzing confusion and ask "Is there any essential distinction between the nature of our own construction and that used by our subjects?" (1979, p.254). To their rhetorical question they say: "Emphatically, the answer must be no." (1979, p.254). It is not difficult to see why Latour and Woolgar might arrive at the conclusion that science is a more or less arbitrary construction and negotiation with fictions and that "nothing of any cognitive quality was occurring" in scientific laboratories.

ARGUMENT BY SCARE QUOTES

Among their new literary devices, Latour and Woolgar have cultivated into an art form the technique of argument by insinuation. This is best conveyed by a sample from the text:

> It is this document ... which is scrutinised by participants for its "significance" and which is used as "evidence" in part of an argument or in an article. Thus, the main upshot of the prolonged series of transformations is a document which ... is a crucial resource in the construction of a "substance" (1979, p.50).
>
> ... our description of fact construction has left untouched those aspects of scientific activity which have to do with "logic" and "reasoning." ... We focus on the routine exchanges and gestures which pass between scientists and on the way in which such minutiae are seen to give rise to "logical" arguments, the implementation of "proofs", and the operation of so-called "thought processes" (1979. p.151).
>
> ... Chapter 4 encroached on the ground of epistemology in order to demonstrate the microprocesses at work in the constitution of phenomena such as "having ideas," "using logical arguments", and constructing "proofs" (1979, p.187).

This promiscuous use of scare quotes is to convey the impression that the terms in question are, in some unexplained manner, to be regarded as suspicious or unsound. The intention is that the terms are not to be taken literally with their usual significance. Latour and Woolgar use this technique as a substitute for explicit argument. The desired effect is achieved without needing to address the monumental philosophical issues implied. For example, are we to understand that there are no such things as evidence, logical arguments, reasoning, thought processes, having ideas or even substances? Is there no literal sense to the notions of author, theory and nature? The need to actually answer such questions is conveniently avoided.

TURNING COFFEE INTO THEOREMS

Extraordinary significance is placed by Latour and Woolgar and other sociologists on the evident discrepancy between published research reports and the realities of actual scientific work. The complaint is that published accounts in journals are sanitized and tidied in such a way as to hide the vagaries, accidents and irrationalities attending the path to finished results. This is taken to be a profoundly important insight into the way in which the public rhetoric of science conceals, mystifies and misrepresents the realities of science, and is the motivation for exposés such as that of Latour and Woolgar.

There is, of course, no doubt about the fact that published scientific

reports omit most of the actual details of scientific investigation such as the dead-ends, failed experiments and a myriad other aspects of research. The question is whether this undeniable fact can warrant the particular conspiratorial conclusion drawn by sociologists of science. The most obvious alternative seems not to have occurred to them – namely, that published research reports are designed to serve quite specific and quite *different* purposes from that of a narrative, biographical or historical account of the sometimes tortuous path to scientific results. Nonetheless, Latour and Woolgar explain that their 'participant observer' approach is designed to demystify the process and their "anthropological strangeness" is adopted because "the uncritical acceptance of the concepts and terminology used by some scientists has the effect of enhancing rather than reducing the mystery which surrounds the doing of science" (1979, p.29). In other words, the mystery is to be dispelled by watching what the scientists do, but without comprehending their actions or their rationale. Latour and Woolgar can see no alternative to such incomprehension, since reliance on a scientists' own explanation would simply reproduce the technical mystification. Undoubtedly, as they note, "A description of science cast entirely in terms used by scientists would be incomprehensible to outsiders" (1979, p.44). But the possibility that someone who actually understands the technical ideas might be able to explain them in a way which *could* be understood by outsiders, has evidently not occurred to our anthropologists.

Published mathematical results presumably bear the same relation to the actualities of their production as other scientific reports and undoubtedly provide an even better example of way in which "the nature of scientific activity is thoroughly misrepresented by the form of presentation". Publications in mathematics generally consist of sparse, austerely formal, neatly ordered propositions in the form of logically deductive proofs. Conceivably, one might be silly enough to assume that such a sanitized presentation purported to be a public account of the discovery process, since the realities of mathematical practice are completely disguised. That is, there is no evidence of the way in which, according to the joke, a mathematician is a device for turning coffee into theorems. However, it is clear that if Latour and Woolgar had chosen to conduct their field trip in a mathematics department, their method would have yielded precisely this ridiculous conclusion. Following their analysis and their diagram (1979, p.46) we would describe the inputs to the office as caffeine and the outputs as meaningless inscriptions called "theorems". Recall, for Latour and Woolgar, scientific instruments and other kinds of apparatus are precisely such devices which "transform pieces of matter into written documents" (1979, p.51)

> More exactly, an inscription device is any item of apparatus or particular configuration of such items which can transform a material substance into a figure or diagram ... (1979, p.51).

The secret which has been hidden from public scrutiny is this miracle of trans-substantiation. With some self-satisfaction and apparently unintended irony, Latour and Woolgar report that "By remaining steadfastly obstinate, our anthropological observer resisted the temptation to be convinced by the facts" (1979, p.88).

PRE-LOGICAL MENTALITY

Latour and Woolgar choose to describe such things as a refrigerator containing substances as "material dictionaries" (1979, p.48–58) by analogy with a lexicon which contains words. This is part of their overall attempt to re-describe everything in literary terms, that is, as "texts". Likewise they see the scientists as a tribe who spend their time "coding, marking, altering, correcting, reading and writing" (1979, p.49). As we have noted, Latour and Woolgar refuse to accept the accounts of the scientists themselves because they wish to remain appropriately critical and sceptical of the mythologies of these witch-doctors. Nevertheless, they show not the slightest scepticism concerning the magical way in which material substances are supposed to be transformed into written documents, figures and diagrams (1979, p.51). This miracle of trans-substantiation is preferable to the lore of their native informants, and this extraordinary conception is manifest in a flagrant indifference to the distinction between words and things. Thus, they explain that "A substance is obtained by superimposing two sets of inscriptions, one from a recording device known as an assay . . . " (1979, p.58). If it were not the central thesis of the book, one might excuse this as an inadvertent slip intended to assert that the *description* of a substance is obtained from these recording devices. But for Latour and Woolgar, this is no inadvertent slip. Latour and Woolgar wish to suggest that somehow the only realities of the laboratory life are the "inscriptions" which are merely "*taken to be* about a substance" (1979, p.63), though it is implied that this is some kind of mistake or illusion.

Notice that the end product is not the substance, but the inscription. Not the analysis or discovery of a physical substances or processes, but "A particular curve, for example, might constitute a breakthrough; or a sheet of figures can count as clear support for some previously postulated theory" (1979, p.63). Conceivably, this kind of redescription of scientists' behaviour could make sense if it were construed as elliptical and not taken seriously as having deep ontological consequences. However, there can be no doubt about the manner in which Latour and Woolgar wish to construe their words. It is central for their thesis that we take these confusions literally, for it is the texts as products of the inscriptions devices which are the primary reality for Latour and Woolgar:

The central importance of this material arrangement [i.e. the inscription

> devices which transform material substances into diagrams] is that none of the phenomena "about which" participants talk could exist without it. Without a bioassay, for example, a substance could not be said to exist. The bioassay is not merely a means of obtaining some independently given entity; the bioassay constitutes the construction of the substance (1979, p.64).

The idea that none of the *phenomena* could *exist* without the inscriptions is precisely the kind of obnoxious absurdity which leads Baudrillard to deny the reality of the Gulf War (see Norris, 1992). The elaborate play upon words which constitutes their thesis is clearly evident here. Latour and Woolgar indiscriminately talk of words and things as if there were no difference. Thus, they suggest that a centrifuge was responsible, not for discriminating *substances*, but "for creating the *notion* of protein" (1979, p.65). We can see perhaps why Latour and Woolgar did not wish to acknowledge ideas or thought processes in minds: it seems that *centrifuges* are capable of creating notions! As for physical objects or substances "The molecular weight of proteins could hardly be said to exist except by virtue of the ultracentrifuge" (1979, p.65). It is not that we might come to learn of the molecular weight by means of the apparatus, but it is somehow actually brought into existence by the equipment. "Without the material environment of the laboratory none of the objects could be said to exist . . . " (1979, p.69).

It is in these doctrines that we see the kind of extravagance which might be expected to raise at least a sceptical eyebrow among scholars, but which have been received with a degree of credulity and even acclaim that is hard to explain. It is conceivable that the entire book written in these, perhaps metaphorical, terms might be translated into something more sensible, if more banal. But the authors give no reason to believe that they wish their words to be taken in any other way, since their dramatic effect and radical import depends precisely on such outrageousness. It is important to note that my portrayal is not based on secondary or minor aspects of the book which are unrepresentative and chosen in order to ridicule. Latour and Woolgar inform us that, contrary to ordinary expectations about the scientists' purposes, "The production of papers is acknowledged by participants as the main objective of their activity" and, accordingly, "we shall consider papers as objects in much the same way as manufactured goods" (1979, p.71). It is evidently this factory production model which warrants the notion that material substances are supernaturally transformed into diagrams, graphs and other texts. Thus, it is our anthropologists who endorse witch-craft, while scientific instruments are regarded in the magical way that the Azande understand their chicken poison oracle. Evidently, it is the "anthropologists" who show what Lévy-Bruhl notoriously called a pre-logical mentality.

"FACTICITY" AND "ACHIEVING OBJECTS" BY MODALITIES

At one point Latour and Woolgar record a major revelation: They report the way in which Latour began to get some glimmer of the fact "that there must be something in the *content* of papers which would explain how they were evaluated" (1979, p.75). The dawning of this insight is evidently a major turning point in their understanding of the laboratory, but one wonders how these staggering banalities have received such acclaim. They continue: "our observer began to peruse some of the articles in order to ferret out possible reasons for their relative value. Alas it was all Chinese to him! ... he felt entirely unable to grasp the "meaning" of these papers, let alone understand how such meaning sustained an entire culture" (1979, p.75). This admission is frank, if hardly surprising. The question which must be asked is how such utterly predictable conclusions are supposed to provide deep and demystifying insights into science. The answer is, in part, that Latour and Woolgar clearly do not see these revelations as an embarrassment, for they become the basis on which their further "insights" are founded. Unable to understand the content of the papers, Latour finds an alternative, presumably more reliable, way to divine their significance.

> Although he understood little of the content of the papers he was reading, he had developed a simple grammatical technique for distinguishing between types of statements. This, he felt, enabled him to approach the very substance of scientists' statements without having entirely to rely on participants for elucidation or assistance (1979, p.80).

The idea here is so fantastic as to defy comment, though it is perhaps no more fantastic than the other ideas we have already seen. It is from a point of view of ignorance and incomprehension that Latour will rely on a "simple grammatical technique" in order to discern the true significance of the papers. Far from feeling any need to justify the manifest absurdity of this approach, Latour and Woolgar do not even deign to defend its value or address the possibility that it might be misleading. It is on the basis of this method, as throughout, that Latour and Woolgar draw their conclusions about the ways in which facts and/or substances are socially constructed by negotiation. One can at least see how they were able to arrive at this profound result: "Activity in the laboratory had the effect of transforming statements from one type to another" (1979, p.81). Specifically, the rationale of the laboratory activities was the *linguistic* exercise of transforming statements in various ways in order to enhance their "facticity". This term of art is intended to convey the idea that there are no facts as such but only fact-likeness, that is, only statements with various kinds of qualifications or "modalities" purporting to be about something. According to Latour and Woolgar, indeed, there are no facts at all since "a fact is nothing but a statement with no modality ... and no trace of authorship." (1979, p.82). We see here a kind of linguistic

idealism, though any such philosophical label may give the impression of some more substantial or respectable doctrine, whereas we have here only confusion and anarchy. The discussion degenerates to the point where it is unclear whether we have merely ungrammatical constructions or deliberately perverse locutions supposed to convey deep new insights. Thus, Latour and Woolgar speak of the way in which "An "object" was thus achieved (*sic*!) through the superimposition of several statements . . . " (1979, p.84) and "In the laboratory, "objects" were accomplished (*sic*!) . . .". Aside from the anomaly of such tortured locutions, we note again that the term "object" is in scare quotes, presumably to suggest that there really are no such things as objects at all. It is this kind of insight which leads the authors triumphantly to assert "With the notion of operations between (and on) statements in the literature, our observer began to feel more confident in his ability to understand the layout of individual papers" (1979, p.85). Indeed, they go so far as to presume that they can judge the extent to which the scientific claims in the literature are warranted: Their linguistic approach to "modalising" informs them that the inclusion of a mere annotation such as "shown in Fig. 2" can transform the status of statements, and thereby "provide an enhanced reading of an *otherwise unsupported* claim about the results" (1979, p.85; emphasis added). Thus it appears that Latour and Woolgar wish to adjudicate the evidential support, or lack of it, for the scientific claims on the basis of their grammatical technique.

We may perhaps be able to see from the foregoing account how Latour and Woolgar arrive at their celebrated conclusions. They explain "a laboratory is constantly performing operations on statements" (1979, p.86) and it is through this process that "a fact has then been constituted" (87) by social negotiation and construction. In short, the laboratory must be understood "as the organisation of persuasion through literary inscription" (1979, p.88). These are the ground on which we must understand their claims that substances studied in the lab "did not exist" prior to operations on statements (1979, p.110, 121). "An object can be said to exist solely in terms of the difference between two inscriptions" (1979, p.127). Ordinarily, one might sensibly say that theories, concepts and ideas are constructed, but as we noted earlier, the dramatic impact of *Laboratory Life* rests on the equivocation between this notion and the claim that "substances are constructed" (1979, p.128). Latour and Woolgar insist "It is not simply that differences between curves indicate the presence of a substance; rather the substance *is identical with* perceived differences between curves" (1979, p.128; emphasis added); " . . . objects (in this case, substances) are constituted through the artful creativity of scientists" (1979, p.129). "We claim that TRF *is* a thoroughly social construction" (1979, p.152).

I.C. Jarvie (1986) has described Evans-Pritchard's (1937) classic *Witchcraft, Oracles and Magic Among the Azande* as exploring "the problem of how a well ordered social life is possible when so much of it is guided

by magic, oracles and witchcraft, all of which are known to be false" (1986, p.120). This is an irresistably apt question to be asked of the enterprise of SSK. We may inquire analogously how a well-ordered academic communal life is possible when so much of it is guided by doctrines all of which are known to be false. Their anthropological affectation has the unintended consequence of backfiring in this way to reveal Latour and Woolgar rather more like the savages than the ethnographers. Their characteristic 'primitive mentality' may be seen in other members of their tribe. Thus, Jarvie remarks upon Mary Douglas's unenthusiastic introduction to an abridgement of James Frazer's classic *The Golden Bough*:

> Apart from the insults of Durkheim (1912), which Douglas does not quote, it is hard offhand to think of a less enticing essay about Frazer. Perhaps Professor Douglas hopes readers will merely glance at the pictures and not get infected by Frazer's ideas (1986, p.171).

What are these ideas of Frazer's which Douglas finds so dangerously infectious? Jarvie explains: "Frazer takes it for granted that on the basis of western science rain makers do not make rain." (1986, p.171). It would seem that this is the kind of belief which places Frazer beyond the pale in the eyes of sociologists of science. Douglas, like Latour and Woolgar, is willing to be skeptical about the claims of modern science but not about those of tribal magic. It is important to notice that this is not merely an incidental irony but a principled stance which has recently been given explicit endorsement (see Ashmore, 1993, Pinch, 1993). Sociologists have professed a neutrality or impartiality with respect to the merits of scientific theories and have feigned suspending judgement between rival scientific claims on their cognitive or explanatory virtues. In practice, however, this has meant a tacit endorsement of discredited and disreputable theories as being somehow on a par with our best science. Recently this has led to something like a campaign of "affirmative action" for disreputable theories in order to re-instate them as deserving equal time in the class room (see Ashmore 1993). The posture of Douglas or Latour and Woolgar is only a further manifestation of the same spurious and disingenuous attempt to avoid judging the merits of scientific theories. Siding with Frazer and Western science against magical rain making, Jarvie makes a point which deserves to be a platitude but is, in fact, an important lesson for sociologists:

> Whether or not science is invincibly superior, which I must confess I think it is, is not a question for the ethnographer (1986, p.173).

THE THIRD PHASE OF SCIENCE STUDIES: "LITERARY FORMS" WITHOUT LITERARY CONTENT

Plato's 'Theaetetus' dealt with a conundrum which must face any theory of knowledge when it is applied to itself. In some instances a paradox arises because the theory appears to be self-defeating. The epistemological problem has not escaped the notice of sociologists and, indeed, on the contrary, the conundrum has become the source of the new wave in sociology of science, – the "reflexive" third phase. In Woolgar's (1988, p.9) humble estimation "the reflexive project in the social study of science ... is the inevitable next step in – indeed the culmination of – some of the most exciting intellectual work currently being undertaken anywhere." It deserves to be noted that this reflexivity is not entirely as novel as Woolgar suggests. We need not go back as far as Plato, for Merton (1972) notes that "The application of the sociology of knowledge to the special case of sociology itself has ... burgeoned since 1959 ..." (1972, p.99). However, notwithstanding Woolgar's immodesty (or mere self-parody – it's impossible to tell which –), for all the sociological revelling in paradox, some of the new literary forms are not merely paradoxical but pathological. Although encouragement is drawn from Hofstadter's (1979) playful explorations of self-reference, the sociological appropriation of these particular notions has managed to take over only the 'literary *form*' without the literary *content*.

More to the point, perhaps, is the fact that the 'new literary forms' are devoid of *scientific* content, which is odd, to say the least, in work which professes to explain scientific knowledge. For example, Malcolm Ashmore's (1989) book *The Reflexive Thesis* is described by Woolgar as "carefully researched and documented ... an important and serious contribution" (1989, xvii) although the only "science" which is mentioned is the sociology of science and, especially, the reflexive project itself. Presumably, readers are supposed to draw the inference that physics, chemistry and biology may be understood on this model. But it might occur to some readers to inquire how this example of the 'new literary forms' can explain anything *else* in the entire history of science besides itself. The most striking feature of the books on reflexivity by Woolgar (1988) and Ashmore (1989) is the complete absence of anything remotely resembling a discussion of any aspect of any scientific discipline. While philosophers of science have become deeply immersed in the substantive details of physics, psychology, linguistics, biology and other disciplines, the sociologists derive their deep insights into these sciences by narcissistic reflections upon self-reflection.

THE ACCOMPLISHMENT OF GETTING NOWHERE

On the grounds that traditional modes of thought and argument are somehow inadequate to the task of understanding the subtle issues, reflexivists

have resorted to parody, paradox and playfulness as an intrinsic part of their method. Not content to be funny for its own sake, the elaborate attempts at cleverness are supposed to be uniquely illuminating and not merely an incidental stylistic matter. Aside from matters of taste, these extravagant efforts at humour are supposed to have a serious purpose, though in the nature of the case, many readers will inevitably be left perplexed. Others will be dismissive. A predictable reaction will be the following:

> . . . liberally salted as it seems to be with jokes and puns and exagggerations (sic) and word play and flights of fancy and eccentric dialogicians and games with self-reference and lists and so on and so forth. Not that they all come off, of course; some of them are distinctly painful. . . . Is there not a danger that such devices may prove counter-productive, in that they provide for a nonserious reading of your text, which may thus be safely ignored; or if not exactly ignored, at least pigeonholed as merely a species of entertainment to which the proper response is *only* a bellylaugh – or a groan. (Ashmore, 1989, p.208–9).

This quotation is, in fact, from Ashmore's own book. These remarks are presented in a dialogue format as if the words were those of a critical examiner on the work itself. With this device we see not only the somewhat overworked use of dialogues, but a characteristic ploy. There are no illusions among reflexive writers about the reactions they elicit, and the gambit is designed to pre-empt and thereby disarm such criticism. By indicating a full awareness of the annoyance and dismissiveness they provoke, these writers wish to defuse it by anticipating and embracing it. However, the gambit is not *ipso facto* successful in forestalling criticism since it is not exactly to *answer* the charge of nonsense simply by pre-empting it. But, then, of course, asking for answers to criticism is probably to reveal that one has already missed the subtle point. Other sociologists have evidently not failed in this way to understand the point, for as Collins and Yearley approvingly explain, "Subtle reflexivists realize that their work leads nowhere. For example, Woolgar has said that getting nowhere should be seen as an accomplishment, not a failure." (reported in Pickering ed., 1992, p.305). With these tactics Woolgar is undeniably successful in his avowed goal of "transcending" the standard form of debate. However, translated into plain language this means that engaging in debate at all is a disingenuous exercise conceived as merely perpetuating itself rather than in order to explore the merits of argument.

"DANGEROUS SOLIPSISM, INSANITY AND PROBABLY DEATH"

Although Woolgar has proclaimed reflexivity as the third phase of the field, emerging from 'exposition and criticism' of earlier phases in the evolution of social studies of science, I have already noted that a reflexive sociology of knowledge has antecedents in Merton and others. What

Woolgar fails to appreciate is that this reflexivity is not only familiar, but it is applicable to *any* inquiry and not somehow peculiar to social science as he suggests. Any inquiry which includes theories or beliefs in its domain must be subject to the reflexivity which Woolgar has discovered in the social studies of science. The famous 'Cartesian Circle' is essentially a case of 'reflexivity' arising from the self-application of the epistemological criterion of 'clear and distinct ideas'. For all the self-important posturing about the great advances of SSK over traditional epistemologies, Quine's naturalized epistemology is perhaps most familiar as holding that all scientific inquiry must be judged by criteria internal to science itself. This inescapable circularity or reflexivity of Quine's (1960) account (famously illustrated by his epigraph from Neurath) is exactly the same as Woolgar's point that the principles of SSK should be applied to SSK itself. There is nothing new about this, nor anything uniquely relevant to social science.

In particular, nothing requires that only a sociological relativism can be a candidate for a reflexive philosophy of science. *Any* canon can be self applied. Whether such reflexivity is benign or leads to "dangerous solipsism, insanity and probably death" (Latour 1988, p.155) depends crucially on the properties which are to be self-applied. Sociologists appear to be re-discovering the wheel, or at least the kind of circularity which has a large philosophical literature. Better late than never. Nevertheless, it should be noted that, although Ashmore, like Woolgar, alludes to the literature of self-reference including Gödel's Theorem, there is not the slightest attempt to reveal how these particular matters might illuminate, or even be relevant to, the specific sociological claims at stake. This is probably because there is no conceivable connection at all.

Woolgar professes to be concerned with the question of how to resolve the disputes surrounding the sociology of science, but it is entirely unclear how the resort to paradox, irony and "new literary forms" might serve this goal. Above all, there has been no analysis to suggest that conventional approaches to intellectual debate are somehow, in principle, inadequate to the task. Is it self-evident that this strategy of deliberate paradox and contradiction is required? Or do we have to start engaging in elaborate ironic literary games every time there is a disagreement?

Of course, it is not clear why Woolgar should be concerned about consensus at all, because the whole issue does not arise for him, since he appears to think that there is something wrong with consistent points of view (1988, p.5). Woolgar actually suggests that in debate we should "avoid the use of characters with clearly consistent identities and points of view". Apart from the fact that no effort has been made to show any unique need in this domain for radical alternatives to conventional thinking, it is *a priori* hard to credit. After all, if the problem was quantum cosmology and the merits of superstrings as a solution to life, the universe and everything, then one might expect deep problems and a need for innovative approaches. We should recall that we are only inquring after the causes of human behaviour and not the Big Bang. This is not to say

that finding answers in this domain is easy in practice, since it is clear that what makes social sciences or psychology interesting is precisely the difficulty of finding any deep principles. But, of course, that goes for the weather too, and we don't feel the need to resort to "new literary forms" in meteorology.

PASTA AS AN EXPRESSION OF ITALIAN NEO-REALISTIC STARCH: FETTUCINE AS AN INSTRUMENT OF SOCIAL CHANGE. - FABIAN PLOTNICK (1980)

It is salutary to ask how Woolgar's reflexive talking about talking might illuminate any of the questions with which the social studies of science is fundamentally concerned. In particular, one might ask how it supports the claim that scientific knowledge is "the contingent product of various social, cultural and historical processes" (1988, p.1). Woolgar's citation of Hofstadter (1979) in this connection is misleading and mischievous, since there would be little support among Hofstadter's ideas for Woolgar's project. Above all, Hofstadter has been not merely paradoxical; his playful literary devices have been expository vehicles to convey some of the most profound ideas of intellectual history – and there is no perverse mystification about exactly what these ideas are supposed to be. Hofstadter's concern is to clarify and not to obscure. One can't help wondering how Gödel's Theorem is relevant to the social studies of science. But then, perhaps Woolgar's citations are also a clever device as part of the new literary forms, and not to be taken seriously as belonging to any "clearly consistent identities and points of view" (See Sartre 1960, Book 1, Section 3, Part 2(i); and also especially reference to Gödel's Theorem by Fabian Plotnick in Woody Allen, 1980, p.127).

The curious consequence of Woolgar's self-application of relativism is that, in fact, he still has not gone far enough in the direction of a relativized methodology. He has failed to notice that there is, after all, a *fourth* stage to follow his own: Now that social study of science itself has become relativized in order to be true to its own principles, we should not arbitrarily restrict the "new literary forms" to social science. They could be employed with equally profound results in other domains of scientific inquiry. Thus, we can foresee a major revolution in particle physics with "second voices" invoked to explain what the proton said to the neutron before they split. Superstrings are nothing compared to this revolutionary theory.

Though the "new literary forms" invite this kind of Monty Pythonesque parody, Woolgar actually suggests that perceiving such things as nonsense is evidence of our having become victims of a coercive orthodoxy. That is, seeing my ridicule as absurdity is due to the persuasiveness of the "rhetoric of scientific analysis" and only "testimony to the effectiveness of Scientific discourse" (1988, p.23) which somehow "banishes" such pos-

sibilities by means of "the rhetoric of research practices". Where I was only being facetious, Woolgar actually appears to contemplate just such absurdities with complete seriousness: He evidently endorses the possibility that we might admit that "electrons (like physicists) had belief systems, their own theories of interaction and so on" (1988, p.23)! This is delusional thinking bordering on clinical severity. For the sake of discussion, one tends to assume that there is a fundamental seriousness behind all Woolgar's methodological mirth, but this assumption may not be warranted and Woolgar does little to allay such doubts. He provokes a familiar concern with new art forms – namely, the suspicion that we are being swindled by empty pretentiousness masquerading as profundity. With Woolgar's new art forms, life has come to imitate Monty Python farce – except for the fact that with Monty Python one knows when to laugh.

CONCLUSION

The state government of Indiana in the last century considered a bill which would have conveniently legislated the value of the mathematical constant pi (π) to be exactly 3. This case of a crack-pot idea serves to illustrate precisely the idea which is at the heart of the social constructivism. The action of the Indiana State counts as a paradigm, if rather literal, case of legislating the truth. According to the tenets of social constructivism, knowledge claims obtain their force by analogous fiat. According to the sociology of scientific knowledge (SSK), if not actually legislated, the claims of science and mathematics are, nonetheless, social conventions constituted by the 'negotiations' and consensus of the community.

The task of drawing out the pedagogical implications of these doctrines is made easy by the fact that, if they need to be made explicit, then the effort is probably hopeless. It should suffice to say that sociological constructivism exemplifies Feyerabend's (1975a) anarchism in which "anything goes". Feyerabend (1975b) has been explicit in recommending that science curricula should include voodoo and creationism, though his views are considerably more conservative than those we have been considering here. His strategy may be seen as a heuristic device to maintain the novelty and creative vigor of scientific inquiry whereas the doctrines we have seen here simply undermine the very conception of such inquiry which has been developed and refined since the presocratics.

There can be little doubt about the affinities of constructivist doctrines with the Hegelian historicism which Popper so bitterly denounced as "this despicable perversion of everything that is decent". For SSK, as for Hegel, "History is our judge. Since History and Providence have brought the existing powers into being, their might must be right . . . " (1966, p.49). The unmistakable parallel is seen in their essentially similar answers to Popper's fundamental question "who is to judge what is, and what is not objective truth?" He reports Hegel's reply that "The state has, in general

. . . to make up its own mind concerning what is to be considered as objective truth" and adds "With this reply, freedom of thought, and the claims of science to set its own standards, give way, finally, to their opposites" (1966, p.43). Though Hegel's doctrines are expressed in terms of the 'State', the essential idea is that political success is *ipso facto* the criterion of truth. We see precisely this idea resuscitated in Latour and Woolgar, Pinch and Collins and the entire enteprise of contemporary sociology of scientific knowledge. This is a historical relativism according to which truth is dependent on the *zeitgeist* or spirit of the age, and it is the one which Popper charges with helping to destroy the tradition of searching for truth and respecting the truth (see Popper 1966, p.308 fn. 30). This tradition is precisely the one which the sociologists explicitly and scornfully repudiate.

Though implications of social constructivism are not drawn out by the authors, they are close to the surface and not difficult to discern. Thus, once Latour and Woolgar reject "the intrinsic existence of accurate and fictitious accounts per se", the only remaining criterion for judgement is judgement itself. " . . . the degree of accuracy (or fiction) of an account depends on what is subsequently made of the story, not on the story itself" (1979, p.284). There are no grounds for judging the merits of any claim besides the "modalizing and demodalising of statements", a purely political question of persuasion, propaganda and power.

For educators it is salutary to notice that the rejection of intrinsic qualities of theories which might warrant judgements of truth or falsity must, simultaneously, erase the distinction between integrity and charlatanism or honesty and dishonesty. These latter categories only make sense on the assumption of certain standards of judgement, and the assumption that there is an intrinsic underlying difference in the objects to which behaviour might be directed. The notion of scientific fraud, for example, presumes a distinction between data which are in some sense true and those which are false – that is, a distinction between fact and fiction. As we have seen, this is just the distinction which Latour and Woolgar, in common with other sociologists of science, wish to repudiate. This is not ethically neutral. Eradicating the basis for discriminating fact from fantasy must also dissolve the grounds for making ethical discriminations. Questions of honesty and dishonesty can only arise if the categories of truth and falsity are recognized as having application. In the absence of such categories, the scientist, like Cyril Burt, who fraudulently manufactures his evidence cannot be meaningfully distinguished from the honest researcher whose data are also "constructed", albeit in different ways. All science, according to Latour and Woolgar, is the "construction of fictions", and accordingly, there is no underlying substrate of fact or truth upon which the fraud may be distinguished. Notice how the analysis of Latour and Woolgar applies equally to the reputable and disreputable alike: "Each text, laboratory, author and discipline strives to establish a world in which its own interpretation is made more likely by virtue of the increasing

number of people from whom it extracts compliance" (1986, p.285). We have seen that for these sociological theories, success in *extracting compliance* is the only criterion for judging theories. It should perhaps be unnecessary to elucidate any more clearly the insidious implications of these ideas. On this account, Stalinist and Nazi totalitarianism were models of scientific success.

Laudan (1990, x) has recently characterized a "rampant relativism" as "the most prominent and pernicious manifestation of anti-intellectualism in our time". He thereby echoes Popper's (1966) concerns about the same "revolt against reason" in Hegel. Popper suggested that Hegel's argument

> . . . was full of logical mistakes and of tricks, presented with pretentious impressiveness. this undermined and eventually lowered the traditional standards of intellectual responsibility and honesty. It also contributed to the rise of totalitarian philosophizing and, even more serious, to the lack of any determined intellectual resistance to it (1966, p.395).

Plus ça change . . .

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