History of Science and Its Sociological Reconstructions

Hist. Sci., xx (1982)

HISTORY OF SCIENCE AND ITS SOCIOLOGICAL RECONSTRUCTIONS

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INTRODUCTION

One can either debate the possibility of the sociology of scientific knowledge or one can do it. If this seems a provocative claim it may be because it appears to contradict current folk-wisdom among many philosophers of science and some historians. This wisdom holds that there are as yet no examples of work in the history of science that show the propriety and value of a sociological approach to scientific knowledge. Over the past decade the communal wisdom has been continually intoned: there is, for example, Ben-David's assertion that a sociology of error and distortion, a sociology of blind-alleys and wrong pathways, is permitted, while "the possibilities for either an interactional or institutional sociology of the conceptual and theoretical contents of science are extremely limited".[1] * In 1974 Rupert Hall judged that "The fruits of the post-Kuhn alliance between sociology and science have yet to be seen".[2] And more recently Professor Laudan has echoed Mannheim's half-century old remark that "the most important task of the sociology of knowledge...is to demonstrate its [explanatory] capacity in actual research in the historico-social realm". In Laudan's view the cognitive sociology of knowledge has lamentably failed in that task. Historians ought to reject sociological temptations and devote their attention primarily to "the rational historiography of ideas" because of its greater "success ratio" compared to cognitive sociology. The fault of what Laudan calls Mannheim's "latter-day disciples" is that they "have tended to assume that one could do sociological history in blissful ignorance of the rational history of ideas". In the absence, as he says, of a "plausible model" for the sociology of knowledge "intellectual historians and others who seek to explain human beliefs in terms of the reasoning processes of agents need make no apologies for not rooting their 'rational explanations' in sociological soil".[3]

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* In the text numbers surrounded by square brackets, e.g. [89], refer to entries in the Bibliography. Superscript numbers, e.g.[12], refer to explanatory references.

0073-2753/82/2003-0157 $2.00

Unfortunately for writers who take this view, it is already too late; the historical sociology of scientific knowledge has gone ahead without them. How this has escaped their notice is an interesting matter, and may, as we shall see, have something to do with superficial and unconstructive characterizations of what sociological explanation is. In the event, if empirical sterility is offered as a reason for rejecting the sociology of scientific knowledge, then a detailed recounting of its considerable empirical achievements will presumably command a change of opinion. This paper will attempt to make as visible as possible the many empirical successes of practical sociological approaches to scientific knowledge.

For obvious reasons some criteria of selection have to be imposed. I will not deal with programmatic statements and will make only brief references to some admirable, and often historically sensitive, theoretical literature in the sociology of knowledge.[4] It would be quite incorrect to regard empirical literature as if it were merely a 'testing' of some theoretical programme; even though empirical work has an important bearing on the validity of theoretical positions, its significance may only be properly appreciated if it is understood on its own terms. In addition, I have attempted to pre-empt some obvious criticisms by discussing work that deals with scientific ideas and practice and largely excluding many admirable studies that treat, for example, images of science, the rhetoric of spokesmen of science, views of scientific method not clearly related to practice, and the sociology of scientists as opposed to the sociology of science. I shall resist any temptation to discuss the social history of science as if it all bore upon the sociology of knowledge; much of it, such as work dealing solely with institutional aspects of science or the career-structure of science, despite an occasional gesture towards sociology of knowledge, is not concerned with knowledge or practice.[5] For my part I see no *danger* of "the history of science losing its science", but much literature in the social history of science has less of a connection with the sociology of knowledge than many apparently traditional exercises in the history of ideas. Thus not all relevant empirical studies come clearly labelled as sociology of knowledge: many of the most significant achievements give little explicit warning that sociological explanation has been perpetrated, and some lay claim to that accomplishment without evident basis. I make no apology, therefore, for attending to what some authors *do,* in occasional (well-meaning) disregard of what they *say* they have done.[6]

I propose to discuss empirical literature from the point of view of a series of related interpretive perspectives. Therefore some justification of the order in which I treat these materials may be required. For many scholars the sociology of scientific knowledge is equated with studies of the role of 'external' macrosociological factors such as social class. Since this is, as I shall show, an inadequate characterization, there is some point in proceeding, as it were, from the inside out. I begin by examining studies which treat structures and processes usually thought to belong to scientific culture and the scientific community. Although much of this work might be readily accepted by historians of ideas, I shall try to establish its relevance to the sociology of knowledge. Only then shall I deal with work which relates scientific knowledge to factors usually thought to belong to the wider society. And I shall conclude with some brief remarks about what sociological explanation of scientific knowledge actually looks like in practice and how it relates to some demarcations (such as those dividing the 'internal' from the 'external' and the 'rational' from the 'irrational') conventionally deployed in present-day scholarship.

I. CONTINGENCY AND THE SOCIOLOGY OF KNOWLEDGE: OBSERVATION AND EXPERIMENT

If scientific representations were simply determined by the nature of reality, then no sociological accounts of the production and evaluation of scientific knowledge could be offered. Perhaps one might attempt to understand why certain features of reality were selectively attended to at different periods and in different social settings, but of the resulting knowledge nothing of sociological interest could be said.[7] It would be pointless to argue against the kind of naive realism and positivism which has few, if any, philosophic proponents at present. The underdetermination of scientific accounts by reality and the 'theory-laden' nature of fact-statements are both quite widely accepted. Nevertheless, the way forward from these basic sensibilities towards a full-blown sociology of scientific knowledge is by no means generally recognized. Even so, this is the best way to proceed: the sociology of knowledge is built upon an appreciation of the contingent circumstances affecting the production and evaluation of scientific accounts.

While it may be banal to *say* that statements of scientific fact may be theory-laden, it is not, apparently, banal to *demonstrate* this empirically and to pin down the specific network of expectations and goals affecting the production and evaluation of statements of fact. Quite simply, there are few such historical studies, and even fewer studies of observation reports. Historians act as if, after all, observed facts count as a 'hard case'; making a fact into a historical product (an artifact) is an exercise which historians of science approach with great caution (even though scientists do it routinely). The small number of historical studies we have are therefore unusually detailed and circumspect. As a foundation upon which one might build a sociology of knowledge they are worth considering.

In the 1860s the English biologist T. H. Huxley undertook a microscopical examination of a number of alcohol-preserved specimens of seabed mud dredged up from the North Atlantic some ten years previously [20; 23]. He discovered a particularly primitive form of naked protoplasmic *Urschleim* that had recently been discussed by the German biologist E. H. Haeckel. These living forms he named *Bathybius haeckelii*, and he produced drawings of what he had seen through the microscope. Subsequently, Huxley's observations were confirmed by Sir Charles Wyville Thomson on the *Challenger* expedition, as well as by a number of American, English and German biologists and geologists. *Bathybius* was a fact. It was also a significant fact; the existence of such a life-form served as crucial evidence supporting a number of scientific theories. It served to establish a link between the nebular hypothesis of planetary evolution and organic evolution much sought after by some Darwinians, as well as by both Huxley and Haeckel. It also served as a datum favouring abiogenesis against the views of Louis Pasteur. Thus it figured in the vitalist-mechanist debates raging at the time. To those who maintained that there was a continuity between living and non-living forms of life and that life might be easily and normally generated out of non-living materials *Bathybius* was not an anomaly; it was a non-contentious fact of nature. It was seen by very many observers. Unfortunately, evidence soon began to appear against the reliability of that perception. Some biologists claimed that *Bathybius* was not a fact but an artifact: it had been created by a combination of observers' imagination and the precipitating effect of alcohol on ooze. *Bathybius* was nothing but calcium sulphate in an amorphous colloidal form, and this is the view taken by present-day scientists. Nevertheless, those who strongly supported *Bathybius*-as-fact continued to fight, disputing the critics' observations. *Bathybius* died a gradual death, assisted by the writings of scientists who opposed the theories which its existence had been used to support.

There are several historical studies which reinforce the general lessons of the *Bathybius* episodes. Baxter and Farley [1] have produced a meticulous account of controversies over cytological observations of meiosis in the period just before and after the re-discovery of Mendel's work. According to whether or not the observer subscribed to Weismann's picture of "reduction division", different accounts were given of chromosome behaviour. There was no agreed interpretive framework within which cytological observations could be unambiguously situated. However, with Mendel's re-discovery, sections of the biological community embraced the chromosome theory of inheritance and, as Baxter and Farley say [1, p. 172], "the cytological work was reinterpreted so as to fit into Mendel's scheme". The microscope has figured significantly in several other historical studies showing the interpretive nature of perception: R. C. Maulitz's study of Theodor Schwann's observation of cell genesis as crystal formation [13], J. V. Pickstone's account of early nineteenth century observations of the "globular structure" of tissues [17], Sandra Black's work on the anatomy of the neural synapse in the 1890s [3], and L. S. Jacyna's examination of Goodsir's cell theory [11]. All these historical studies reject the notion that 'erroneous' perceptions might be sufficiently explained by defects in contemporary observational instruments.

Naked-eye observation reports do not present a radically different picture. One of the finest studies in this area is M. J. S. Rudwick's account of disputes between Darwin and other geologists about the origin of the Parallel Roads of Glen Roy in Scotland [21]. In this episode structures were labelled differently according to whether one held the theory that the Roads were formed by elevation of the land above the sea, a falling sea-level, or by lakes of glacial or non-glacial origin. In some instances, what were 'minor roads' in one version were no roads at all in another. Broadly similar orientations are also available in Rudwick's more recent study of controversies during the 1830s over what came to be known as 'The Devonian System' [22]. This is a detailed account of negotiations among geologists over the classification of strata according to their fossil contents. What proponents of one theory regarded as crucial confirmatory evidence was treated by advocates of another classification as an intolerable anomaly and support for their alternative order of strata. Rudwick displays the fine-structure of negotiations over empirical evidence which eventually culminated in a consensually-accepted order.

Negotiations and conflict over fossil evidence also figure in A. J. Desmond's treatment of nineteenth century controversy over the morphology and physiology of the dinosaur [8]. In the 1830s it was the generally-held view that the Mesozoic saurians were 'monstrous lizards' and their similarity to extant lizards was asserted. Against this position the English anatomist Richard Owen mounted a vigorous campaign. He construed the fragmentary fossil bones as evidence of reptiles so highly developed that they possessed traits associated with pachyderm mammals. On this basis Owen raised the *Dinosauria* to ordinal status, giving them a taxonomic distinctiveness they lacked on existing theories. Desmond shows that this was a strategy Owen adopted to establish the fact of *degeneration,* and thereby to argue decisively against the materialist Lamarckian transmutationism that asserted unabated increase in the complexity of fossil forms through time. Owen and the Lamarckian Robert Edmond Grant disagreed in their readings of the fossil evidence because they diverged in their views on the more fundamental matter of species change. By literally 'designing' or 'inventing' the dinosaurs Owen hoped to counter both materialist intellectual tendencies in the culture as a whole and transmutationism in biology. Negotiations over the correct classification and interpretation of visual evidence also figure in Winsor's sensitive study of nineteenth century work on barnacle larvae [26], and in Burkhardt's excellent paper on the phenomenon of 'telegony' [2; see also 121].

Before one moves from disputes over facts to a full-blooded sociology of scientific knowledge there is one major obstacle to overcome. Suppose, it might be objected, that while scientists disagree over observations and interpretations of observational evidence, they may readily give assent to impersonal criteria for making observations and performing competent experiments: in the end, these 'non-social' criteria will adjudicate disputes of this sort. Recent empirical studies of modern scientific controversies over the reality of certain phenomena do not support this detour around a sociology of knowledge. H. M. Collins has studied controversies in the 1970s over the existence of high fluxes of gravitational radiation [4]. The initial claim by one scientist to have built an 'antenna' which detected such radiation was soon countered by a host of criticisms. Other 'antennae' were devised and put into operation that produced no empirical support for the reality of the phenomena supposedly detected by the original. Those experimenters committed to the reality of the phenomena claimed that 'competent' experiments were those which reliably detected the radiation, while those which failed to do so were judged to have been 'incompetently' performed. Conversely, scientists committed to the opposite view judged that experiments which indicated high fluxes of gravitational radiation were not competent experiments. Different communities' views about what the natural world contained were used, so to speak, to calibrate the experiments. Since 'experimental design' cannot be divorced from the commitments of the communities that frame and evaluate experiments, there is no possibility of avoiding a sociological account of fact-production by appealing to impersonal rules of experimental procedure. As it happens, Collins's work was undertaken when the outcome of the controversy was not known. Since that time, the 'fact' of no high fluxes of gravitational radiation has been established, and Collins has followed the controversy to its resolution: "The existence of (hf) gravity waves is now [in February 1981] literally *incredible*.... Their demise was a social (and political) process" [5, p. 54]. Elsewhere, Collins and Pinch have come to a similar conclusion about competent experiments and the social construction of scientific facts from a study of parapsychological research [7; 18; 142].

A still more detailed account of the contingency of experimental findings is Pickering's study of the recent history of experiments designed to discover whether or not free 'quarks' exist in matter [16]. It has been commonly assumed that experimental results, provided the experiments are competently carried out, can compel assent from scientists. But Pickering offers a particularly striking exemplification of the Duhem-Quine thesis: that all experiments are in principle open to criticism. In the case of the quark work a series of experiments conducted in Italy produced no evidence of free quarks, while others performed at Stanford were regarded as having demonstrated their existence. The Italian physicists calibrated their experimental procedures by their production of credible results — in their case the non-existence of non-zero charge in a version of the Millikan oil-drop experiment. The experiment was deemed to have been competently performed because of its reliable production of no fractionally-charged objects. Since the existence of fractionally-charged objects would be a highly abnormal addition to physicists' natural world, the Italian experiments were not systematically criticized and the experimental findings were accepted. This was not the case with the Stanford work, even though it was carried out with extreme rigour. The Stanford finding of charges of $\pm 1/3e$ has been subjected to intense scrutiny, albeit mainly out of the printed public forum. The Italians have been at pains to identify a number of features of the Stanford experimental system which make its findings less than compelling. Pickering concludes in a similar vein to Collins: "...one cannot separate assessment of whether an experimental system is sufficiently closed from assessment of the phenomena it purports to observe: if one believes in free quarks then the Stanford experiment is sufficiently closed; if not, then it is not" [16, p. 229]. It is the accepted knowledge of the community that adjudicates; reality is filtered through that knowledge and has no unmediated compulsory force. Similar orientations to the relationship between experimental findings and the acceptable options available to a community of practitioners may be found in Pickering's study of magnetic monopoles [15],[10] as well as in papers by Wynne on the detection of J-rays [27; 28], Nye on N-rays [14], Pinch on solar neutrinos [19], Travis on memory-transfer experiments [24; 25], and Harvey on experimental quantum mechanics [9; 10]. (Some of these papers, to be sure, treat phenomena that the modern scientific community regards as 'pathological', but some deal with currently-accepted facts of nature. In any case, what is offered is not a sociology of error or of pseudo-science, but a sociological appreciation of the processes by which statements of fact are accredited or rejected.) Perhaps the most detailed assessment of the social construction of scientific facts is Latour and Woolgar's investigation of a modern neuroendocrinological laboratory [12]; this stresses the role of scientific apparatus and of the 'literary' processes by which facts are stabilized, although these authors do not share Collins's emphasis on the methodological priority of controversy [6].

Such studies serve to demonstrate that neither reality nor logic nor impersonal criteria of 'the experimental method' dictates the accounts that scientists produce or the judgments they make: they open the way to a sociology of scientific knowledge, and for this reason they are invaluable. However, they do not by themselves constitute such a sociology.[11] An empirical sociology of knowledge has to do more than *demonstrate* the underdetermination of scientific accounts and judgments; it has to go on to show why particular accounts were produced and why particular evaluations were rendered; and it has to do this by displaying the historically contingent connections between knowledge and the concerns of various social groups in their intellectual and social settings.[12] In this respect the empirical work discussed up to this point, however particular its historical focus, has a fundamentally philosophical character. Philosophical work sympathetic to a sociology of knowledge ends by displaying the contingency and open-ended nature of scientific knowledge; a fully-developed sociology of knowledge starts with the recognition of historically contingent factors and then proceeds to array and stress their different roles in scientific action. Having said that, we are now in a better position to appreciate the sociological significance of a body of empirical studies that relates divergent bodies of knowledge to the concerns of social groups *within* the culture of science.

II. PROFESSIONAL VESTED INTERESTS AND SOCIOLOGICAL EXPLANATION

Within the scientific community, and within any given specialty or discipline, there will typically exist a distribution of different skills and technical competences. For example, some scientists will be more skilled than others in mathematical demonstration; some biologists will be more adept at morphological studies of animals and others will be highly skilled in biochemical analyses; within a scientific subculture there may also frequently be a division between theoreticians and experimentalists. These technical abilities and competences will have been acquired through processes of socialization; they will have represented a considerable investment on the part of the scientist, and he will naturally tend to deploy them, to show their value in scientific work and to extend the possible range of their application. Such skills and technical competences therefore represent a set of vested social interests *within* the scientific community. There is every reason why a scientist should wish to display the value and scope of what he can do, even to the extent of criticizing the value and scope of others' acquired skills and competences. In the process of defending these professional vested interests conflicts may arise within the scientific community over the nature of phenomena. If nature is constituted in one way, then its investigation may best proceed through the application of one set of competences; if it is constituted differently, then perhaps another set of technical competences are called for. In this way, professional vested interests may form the middle link which connects, on the one hand, controversies about the nature of phenomena and, on the other, conflict over the availability of resources or the securing of credibility for scientists' work. The analysis in terms of socially acquired technical competences may even be extended to encompass scientists' investments in the practical or interpretive line of their previous work. If a group of scientists have accomplished a body of publicly available research in which they argue for a given point of view, theory or interpretation, they may well wish to defend that position from attack and display its value and scope over other positions—even if they are technically able to work from another cognitive or practical orientation. Naturally, there is no coercive force involved and scientists may readily shift their positions, seek to acquire other competences, or see the advisability of terminating a controversy to further shared interests. What is involved is a strategy for defending and furthering interests, based on complex calculations about the consequences of various courses of action.

There is a substantial body of history of science literature that shows the explanatory value of attending to professional vested interests. The significance of this perspective may best be shown by proceeding from the smaller to the larger scale of such interests. In November 1974 two new and unusual elementary particles (named 'J-psi' and 'psi-prime') were discovered by a group of high-energy physicists. Theorists in the community were faced with the problems of explaining the new particles' properties and of situating them within a coherent framework that also dealt with existing particles. Andrew Pickering's study of the controversy between advocates of the "charm" and "colour" models, and the quick resolution of that controversy, is built upon sensitivity to the pre-existing distribution of interests among specialist groups within high-energy physics [40]. Without entering into the technical details of each model, charm's proponents were so successful in vanquishing their colour model rivals that within eight or nine months a solid consensus in favour of charm had developed; within two years colour's advocates had been effectively isolated and the charm model had been solidly established. What was the basis of charm's success and colour's failure? Pickering demonstrates that the charm model intersected with, and could be readily integrated into, a range of existing bodies of theoretical practice in high-energy physics. For example, charm generated a puzzle—to do with rules for interpreting the longevity of certain particles employed by hadrodynamicists; it offered a solution to this puzzle which provided a programme of work for experimenters and hadron spectroscopists; and it gave support to, and generated support from, a group of important 'gauge theorists' who saw ways by which the success of the charm model could give additional credibility to the 'gauge theory revolution' in quantum mechanics. Moreover, the charm model used conceptual resources that were very widely distributed in physics; as Pickering says, "whenever the model encountered mismatches with reality the resources were available to essentially anyone to attempt to fix it up, and for others to appreciate such work" [40, p. 125]. By contrast, very few bodies of practice in theoretical physics incorporated resources associated with the colour model. Charm succeeded insofar as it was successfully insinuated into a range of bodies of practice; the greater and the more consequential the extent of that integration into practice, the more charm appeared as a fact of nature rather than a human contrivance.

In this particular case charm theorists *could have done* colour theorizing and *vice versa*; each group had acquired through their socialization into the high-energy physics subculture the competences to do both charm and colour theorizing. It was not a question of being unable either to do or to see the point of the other's theorizing. Nevertheless, given the pre-existing distribution of theoretical practices, each made the evaluation that gave most promise of validating and extending the range of applicability of its practices. Again, no coercion was evidently involved.

Let us move to consider competences within a scientific community that are not so readily acquired or discarded. One example is John Dean's examination of a series of controversies among twentieth century botanists over the correct classification of plants [31]. One group of practitioners has maintained that species are to be delineated on the basis of their morphology while the other group has claimed that experimental techniques of various kinds (including transplantation studies, cytological and biochemical work, and measures of genetic exchange) are required for a correct classification to result. These disputes have been going on since at least the 1920s and are still unresolved today. Each group is quite capable of generating its own taxonomy employing its preferred techniques. As might be expected, sometimes the taxonomies render given bits of botanical reality differently. In the case of the *Gilia inconspicua*-complex experimentalist taxonomists, using cytological findings, discern five species, while morphological taxonomists identify just one. In another *Gilia* complex (*tenuiflora-latiflora*) the situation is reversed; morphological criteria identify four distinct species while information about gene exchange points to just one. Each set of species criteria, so to speak, works, and each can be used to further the practical concerns of the classifying communities. So each group of scientists construes botanical reality differently. Each group is also distinguished by its members' acquired technical competences and, to a large extent, by the institutions in which it works. The more traditional taxonomists have been trained in morphological techniques deploying the existing Linnaean system of nomenclature and identification. Many of them work in herbaria producing monographs and flora which aim to provide clear-cut means of distinguishing taxonomic groups on grounds of their gross appearance. Botanists who have vigorously criticized the herbaria taxonomists (the experimentalists or "biosystematists") tend to have been trained in genetics, cytology, ecology and related disciplines and to work in university research departments. Thus the criteria each group advances as the basis for a proper classification act to defend and further its investments in socially acquired technical competences. The groups have on occasion competed for resources, but in the main they have worked out a *modus vivendi*, with the result that alternative techniques for classification also stably co-exist in the botanical community. Sometimes differing evaluations of statements of fact hinge upon scientists' differing investments in both experimental practice and theory, and a particularly clear example is provided in Robert Kohler's study of the reaction to the discovery of the enzyme zymase [32; 33]. The explanatory value of attending to the distribution of technical competences and conceptual skills within the scientific community is perhaps best shown in the very well studied 'biometry-Mendelism' and 'Darwinism-Mendelism' controversies of the late nineteenth and early twentieth centuries. Thus Garland Allen notices that the early twentieth century split between Darwinians and Mendelians was paralleled by a dichotomy in the biological community between naturalists and experimentalists [30; also 29].[13] By and large, those biologists who favoured the Darwinian account of evolution by the natural selection of small continuously-varying characters were trained in descriptive and qualitative methods; their research activities concentrated upon the field and the museum. Mendelians, by contrast, tended to have an experimentalist training; they preferred quantitative methods and worked in the laboratory and the experimental garden. In their view, evolution proceeded by large-scale discontinuous variations. In criticizing the Darwinian position, Mendelians claimed, among other things, that their opponents' preferred small-scale variations were not inherited and that selection was of doubtful efficacy. But not all biologists with quantitative methodological preferences were to be found in the Mendelian camp. The 'biometrical' school, led by Karl Pearson and W. F. R. Weldon, married statistical methods in the study of heredity to a strongly Darwinian commitment to the role of small continuous variations. MacKenzie and Barnes press the explanatory role of socially-acquired resources in their examination of the biometry-Mendelism disputes of *c.*1890 to 1906 [36; 35, ch. 6]. Pearson was by training a mathematician; his colleague Weldon was a biologist, although he also studied mathematics for two years at London University. Their joint effort was to mathematize evolutionary biology; as Weldon said "...the problem of animal evolution is essentially a statistical problem" [36, p. 5]. The biometricians' major critic, the morphologist William Bateson, embraced Mendelism as providing strong support for his view that discontinuous variations provided the stuff of evolutionary change; and he disputed the explanatory value of his rivals' methods: "the gross statistical method is a misleading instrument" [36, p. 18]. It appeared to more traditionally-trained biologists that the new statistical methods tended to devalue their skills, in particular morphological assessment of the individual case. If, as Pearson asserted, the future lay with the mathematically competent, then it certainly did not lie with traditionally-trained biologists. MacKenzie and Barnes do not, however, advance the distribution of professional skills as a necessary and sufficient explanation of scientific controversy. Teaching does not *determine* future career-choices or judgments: Pearson possessed the competences to adopt a Mendelian framework; too much should not be made of Weldon's mathematical training; Bateson set aside several important aspects of his technical training [37]. As we shall later see, the controversies between biometricians and Mendelians provide opportunities for deploying other sorts of sociological explanation in addition to those concerned with factors internal to the scientific community.

One could say that the evolutionary disputes among biometricians and Mendelians (or between Darwinians and Mendelians) occurred within the one scientific discipline of biology; alternatively, one could plausibly point to the groups concerned as nascent sub-disciplines. By the early twentieth century the differentiation of scientific specialties had proceeded a long way towards the present condition. If, however, one moves back in time, one reaches a situation in which the demarcations between what we are accustomed to call scientific 'disciplines' were poorly drawn. We may see disputes between 'disciplines', because it may seem to the actors that there is not room and support for more than one approach to a given problem area. Something of this sort is apparent in Dov Ospovat's excellent study of attitudes towards adaptation and teleology among British scientists from the 1830s to the 1850s [39]. Ospovat shows that an important group of British scientists had rejected the explanatory role of teleology and 'final causes' prior to the publication of the *Origin of species*. This group, including the biologists Richard Owen, P. M. Roget, William Carpenter, Martin Barry and Louis Agassiz, accepted the commonly-held view that organisms (both at present and in the geological past) were "perfectly adapted" to their conditions of existence, but they construed adaptation in a way significantly different from other writers. To the biologists adaptation of structure to function was the outcome of specifically biological laws or patterns; it was not the result of environmental determinism; it was not the effect of the Deity's special creation; and it was not regarded as an explanation in itself. Both the adaptation of existing organisms and their succession in the geological record were to be explained without making reference to teleology. In Ospovat's interpretation, teleology ensured the dependence of biological explanation upon geological facts; the rejection of teleology "secured the independence of biological theory from geology" [39, p. 44]. Those writers who insisted upon the explanatory role of teleology in biology tended to be geologists (including steady-state theorists like Lyell, as well as progressionists like Adam Sedgwick and William Buckland). To them changing conditions in the inorganic realm determined the changing forms of plants and animals. In making this argument the geologists regarded external conditions as the product of specifically geological forces and laws; geological change was thought of as primary, with organic change dependent upon it. Thus, by stating a dependency relationship in their objects of study the geologists stipulated a similar dependency relationship in the scientific community of the time; by rejecting that dependency relationship in nature the biologists were making a move for an equality of cultural status.[14]

III. INTERESTS AND THE BOUNDARIES OF THE SCIENTIFIC COMMUNITY

Dov Ospovat's study of differing evaluations of teleological explanation in nineteenth century British science points, as we have seen, to the hierarchical relations between scientific specialties [39]. But his materials also ramify into the relations between the scientific community in Britain and theological concerns among clerics and in the wider culture generally. Theories of the structure and function of organic forms and of their succession in the geological past bore intimately upon religious proofs and demonstrations as well as upon the moral order that religion underpinned. If purpose was regarded as a necessary and sufficient explanation of the accepted fact of perfect adaptation, then the Deity (as the ultimate source of purpose in the world) was implicated in scientific explanation. What happened if one rejected teleology as a satisfactory explanation of natural processes and objects? What if one insisted that the natural world followed its own self-sufficient natural laws and that scientific explanation was the search for those self-sufficient laws and patterns? The effect of such moves was to undermine historically established cultural relationships between natural knowledge and theology. Since the middle of the seventeenth century proofs of the existence and attributes of God had pointed to the evidence of nature as God's creation. As these cultural relations built up, it became one of the accepted functions of natural knowledge to supply evidence relevant to theological concerns. The body of culture which specifically fulfilled these functions was called natural theology, and it acted as a bridge between theology and natural science. Natural theology also established a hierarchical relation between those who studied the natural world and those whose role it was to interpret God's ways to man. Natural knowledge was widely esteemed valuable and accurate insofar as it displayed to mankind the evidence in nature of God's existence, design, power, wisdom and providence. If the practitioners of natural knowledge performed this function, they secured the support of powerful religious institutions in society. If, however, they severed dependency relations linking the natural world to an external source of purpose and spiritual power, then they cut themselves loose from the protection and approval of the clergy.[15]

In recent years some of the most outstanding work in the social history of science has dealt with aspects of the changing relations between the scientific community and the church in Britain during the nineteenth century, and with concomitant changing conceptions of nature and natural knowledge. As we have seen, the geologists discussed in Ospovat's essay were by and large happy to retain teleological explanation in natural science. Teleology performed a number of functions for them: it kept purpose in the natural world and stipulated a dependency relationship subjugating those who studied the organic world to those whose sphere was the inorganic world. But it would be a mistake to treat attitudes towards teleology solely within the scientific setting. A number of the major actors involved were themselves clerics or had strong religious commitments to defend or advance. J. H. Brooke has examined the role of natural theology, and specifically of the argument from design, among British geologists in the 1830s and 1840s [41]. For the Revd Adam Sedgwick and the Revd William Buckland (among others) the argument from design was a central theme in explaining the relationship between geological facts and, for example, the fossil record. To ignore the clear evidence of purpose and design in the natural world would, in their view, be unscientific.

However, as Brooke makes clear, design arguments also served an intrinsically religious purpose. Reference to the evidence of purpose and divinity in nature served the function of uniting Christian factions which more contentious religious tenets were seriously threatening to disrupt. Anglican Broad Churchmen, like many of the Christian geologists of early Victorian England, felt that the most serious danger to the moral order was posed by divisions among believers, such as between Church and Chapel, for such cleavages would open the door to secularizing tendencies.[16] Christian geologists were, therefore, happy to accept ambiguities in their natural theology, for the very doctrinal imprecision of natural theology was the foundation of its irenic function. Pressures on Christian unity were particularly acute during the 1830s and 1840s. By the 1870s these pressures had been considerably relaxed, partly through the removal of civic disabilities from Dissenters and partly through the general opening up of religion to liberal opinions. Correspondingly, many of the social functions that design arguments had been called upon to perform were no longer necessary, and the arguments themselves began to disappear from scientific literature.

Thus the acceptance and propagation of design arguments within scientific culture is shown to be a feature of a society in which the role of the theologian and the role of the scientist were not distinct. In such a society the practitioner of natural knowledge was content that one of his functions should be the provision of evidence to support religion; he accepted a fundamental dependency relationship between himself and the cleric; indeed, there was no clear distinction between the role of the cleric and role of the man of science. This arrangement did not, as we know, survive the nineteenth century. Certainly, by the middle of the century sectors of the British scientific community were making bids for cultural and social independence from clerical concerns and clerical control. The strategy adopted to achieve these ends was scientific naturalism. F. M. Turner and L. S. Jacyna have argued in great detail that scientific naturalism is properly seen as a strategy in the professionalization of science in Victorian Britain [54; 56; 45]. The naturalists' strategy involved the rejection of the existing cosmology which linked theological concerns to natural knowledge and the role of the cleric to the role of the scientific practitioner. In that cosmology matter and spirit were distinct ontological categories; spiritual entities were the ultimate source of power, plan and activity in nature, thereby rendering the material world subservient to immaterial agencies. In its place the scientific naturalists erected a monist mechanism: there was only matter and its states of motion in the world. According to the laws of thermodynamics the world-machine contained a fixed amount of energy that was conserved in all physical transactions; no external source of power was necessary. The naturalist doctrine of psycho-physical parallelism held that mental states were the products of (or were coincident with) corporeal states; it was incorrect to regard mental states as the causes of action, as having power in themselves. Evolution by natural selection was mechanistic; no reference to purpose or design was required to understand organic change.

A natural world so constituted defined the nature of scientific inquiry just as it formed the basis for an autonomous scientific role. If nature was like that, then the old dependency relationship between the man of science and the man of the cloth could no longer be sustained. But freedom from clerical superintendence was only the first step in the attainment of professional status for the scientist. To achieve desired social support and command of resources the applicability of scientific procedures to a wide range of social questions had to be recognized and pressed. Hence scientific naturalists often adopted a more aggressive posture as cultural imperialists: man, society and mind all could be encompassed within a naturalist schema. The development of the social role of the psychiatrist and the social planner were landmarks in the extension of scientific orientations to new cultural domains just as much as social Darwinism and eugenics [42; 43; 44; 48; 51; 53; 35, pp. 52-56]. Ultimately, the issue involved a clash of sources of expertise and credibility in society, and the command of resources which would follow from a recognized position of interpretive authority [55]. Thus studies of the naturalist cosmology rightly situate conflicting evaluations of it in the contest over the professionalization and scope of science. It is also possible to press far beyond the cosmological level to show the strategic nature of scientists' positions on, for example, the fine anatomy of cerebro-spinal ganglia, detailed embryological processes, and the nature of cause and power in physics [45, ch. 4; 46; 47].

Modern scientific representations of the natural world developed in the course of demarcation disputes with traditional sources of authority and intellectual expertise, such as religion. However, demarcation problems faced practitioners of science on many fronts. Securing credibility in society as interpreters of natural phenomena often involved making publicly visible the distinction between the cognitive claims of 'authentic' scientists and those of the general laity. Sometimes this took the form of avowals or disavowals of what sorts of objects existed in the natural world. A particularly instructive instance of this is contained in Ron Westrum's study of attitudes towards meteorites in eighteenth century France [58]. While scientists of the Royal Academy admitted the existence of meteors (or glowing and rapidly moving celestial objects), they did not credit claims that earthy objects fell from the sky or that such alleged "thunderstones" were connected to the appearance of meteors. According to the official scientific establishment, meteorites were not natural objects, not facts of nature. In the eighteenth century French social setting one of the problems with claims that meteorites had indeed fallen from the sky after a meteor display was that such reports tended to come from witnesses whose credibility as observers of nature the official scientific community was committed to denying. It is not the nature of meteorites to fall with great frequency within the precincts of established scientific institutions; more often they fall in rural districts where they are observed by peasants and attested to by local priests and assorted worthies. But it was the credit-worthiness of the laity that the French academic scientists were concerned to dispute. Interestingly, official recognition of the factual status of meteorites followed the Revolution and the changed attitudes that the Revolution encouraged towards the competence of the laity to participate in cultural pursuits. In other studies Westrum has pursued the relationship between social credibility and the reliability of reports with extremely compelling results. The approach he adopts to deal with the eighteenth century French material does not need much modification in order to be applied to modern disputes over the existence of unidentified flying objects, sea-serpents and the like: reports of the existence of phenomena are often evaluated according to their social source [57; 59].

By the late nineteenth and early twentieth century the boundaries between the professional scientific community and mere amateurs had been fairly well defined in most areas of science. However, there were specialties in which the amateur-professional demarcation was not yet as rigid as it had become elsewhere. One such case was observational astronomy. John Lankford has provided an excellent account of a controversy between various sectors of the astronomical community in the 1880s and 1890s [49]. This was a period in which amateurs (that is, persons not employed to do astronomical research) still routinely made significant contributions to stellar and especially to planetary astronomy. Of necessity, amateurs employed telescopes with relatively small apertures compared to the big telescopes used by professionals, particularly in the new American observatories. A controversy erupted when a leading British amateur astronomer asserted that small aperture instruments were actually superior to big telescopes for planetary observations. Specifically, it was claimed that they gave better definition even though they had less light-gathering power. Large refracting telescopes, it was said, suffered from the 'glare' produced from gathering too much light. One consequence of this was that certain planetary details were more crisply seen with small telescopes than with the professionals' large ones, for example the Great Red Spot on Jupiter, canals on Mars and certain spots on Saturn. American professionals, who had used large refractors manufactured by American companies, eventually countered the British amateurs' criticisms. As one American professional said, the sharp borders of planetary structures shown by small telescopes are not genuine features of those structures but are instead artefacts, produced by small instruments' inability to resolve extended detail. Therefore the hazy borders revealed by large refractors are the natural appearance of the objects, not distortions due to defective optical properties. As Lankford concludes, the groups "represented opposing interests, and the scientific knowledge they produced rested on strikingly different perceptions of the natural world" [49, p. 27]. Here, then, is an interesting complement to Collins's point relating evaluations of competency in experimentation to pre-existing views of the reality of the phenomena to be detected by the experiments. In Lankford's material the conflicting groups had no obvious investments in the appearance of planetary spots; instead, they had pre-existing investments in the reliability of the instruments they employed. And, in this case, the type of instrument used served to distinguish two sectors of the astronomical community: professionals and amateurs. Thus, disputes over the appearance of the Great Red Spot were an episode in the professionalization of science. In an extended study of controversies in modern radio astronomy, Edge and Mulkay similarly stress the importance of investments in instrumentation [133].

In a brief and boldly conjectural article written almost thirty years ago Pannekoek weaves together an episode in planetary astronomy and aspects of the professionalizing strategy [50]. After William Herschel's discovery of Uranus in 1781,[17] it became widely noticed that the planet was deviating from its predicted orbit. By the 1830s it was thought that these deviations might be caused by yet another, hitherto unknown, planet lying outside the orbit of Uranus, and it was even suggested that one could find that planet from the pattern of Uranus's perturbations. In the mid-1890s this was duly accomplished, by Adams in England and Leverrier in France. French scientists took the occasion to trumpet the discovery of Neptune to the public as unique proof of the predictive power and certainty of science: a demonstration of the value of science to the nation. This, and the relative lack of such propaganda surrounding the discovery in England, Pannekoek cites as an indication of the particular problems faced by French scientists in subduing the residual authority of the Church. But the real interest in Pannekoek's account arises from his discussion of subsequent events. Shortly after Adams's and Leverrier's discovery, the American Walker calculated a precise orbit for Neptune which differed radically from that constructed by the Englishman and the Frenchman. Walker's compatriot Peirce went so far as to say that this 'Neptune' was in fact a different planet from the planet whose orbit was calculated by Adams and Leverrier. So the relevant scientists were faced with a decision: were the 'Neptunes' the same or were they different? The Americans advanced the view that they were different; the French insisted that they were, after all, the same, pointing to wide limits of error involved in such calculations. Why? Pannekoek conjectures that the French sameness judgment was informed by an interest in saving their previous public display of the predictive power of science, while in America no such public investment had been made and scientists' judgments proceeded on the basis of other criteria. If Pannekoek is right (and much further research would seem to be needed), one of the most fundamental acts of cognitive judgment (are natural objects the same or not the same?) was in this case structured by interests in the professional status and social standing of the scientific community.[18]

IV. SCIENTIFIC KNOWLEDGE AND THE WIDER SOCIETY

Professionalization radically changed the ways in which concerns within the scientific community related to the concerns of the wider society. This historical shift has natural historiographic consequences. The historian of pre-professionalized science will frequently point to different sorts of social factors from those implicated by the historian of professionalized science. However, to many historians of science it is puzzling to speak of a social history of modern scientific knowledge; to them the enhanced degree of autonomy enjoyed by professionalized science spelt an end to the explanatory role of 'social factors'. I have already discussed literature which seems to provide a solution to this puzzle: a solution which explodes an unsatisfactorily restrictive sensibility towards what 'social factors' are and how they function in explanation. (This is a topic to which I shall return later.) Despite this, many scholars regard the social history of scientific knowledge as solely constituted of studies which relate scientific beliefs and practices to social and political concerns in the wider society. A few instances of this sort of work are very well known (or notorious) among historians and philosophers of science, and that is one reason why I shall give them less detailed attention than the studies already discussed.

Nevertheless, there are features of these studies that might profitably be made more accessible.

There are several major reasons why the sociological significance of empirical studies of this type has been widely underestimated or insufficiently appreciated. One reason, undoubtedly, is the impatience of many theorists, including those sympathetic to the sociology of knowledge, when confronted with detailed studies of any sort: 'historical' or modern. When this impatience is allied to a programme, such as that of Lakatos and his followers, that makes empirical history dependent upon philosophical judgments, there is added reason why sociological findings are unlikely to be credited.[19] All the blame cannot, however, be put upon unsympathetic theorists. Historians of science, like most historians, tend to favour particularistic orientations. They generally define their work, not in terms of an interpretive tradition but in terms of a body of empirical materials or in terms of biographical foci. It is frequently counted as a criticism to say of an historian that he has an overriding theoretical commitment: such commitments are felt to get in the way of a properly disinterested engagement with the facts. Or it is felt that a single well-defined interpretive approach to a body of materials causes the loss of the richly textured narrative much valued by historians. Also, many matters of theoretical interest call for comparative perspectives: competence and willingness to master disparate subject matter or materials from different cultural settings. But the specialism and particularism of modern historical training makes the acquisition of these skills difficult and the insecurity of modern academic life tends to make their deployment risky. Immersion in particular empirical materials has obvious advantages (as anyone familiar with cavalier genres of sociological and philosophical theorizing can attest); but there has also been a price to pay. That price has been a 'poverty of theory' among many historians of science. It is sometimes difficult to discern what the 'argument' or 'point' of certain empirical studies may be; in others the stated conclusions bear slight relation to the empirical body of the work; and in general there is little effort at connecting a particular study to its interpretive kin in other empirical domains. Finally, there is a marked lack of rigour in much social history of science; work is often thought to be completed when it can be concluded that 'science is not autonomous', or that 'science is an integral part of culture', or even that there are interesting parallels or homologies between scientific thought and social structures. But these are not conclusions; they are starting points for more searching analyses of scientific knowledge as a social product. All this may reduce to saying that history of science is a largely empirical discipline, and that there are certain problems attendant upon empiricist orientations. Empirical studies relating wider social factors to scientific knowledge can make important contributions to the development of the sociology of knowledge generally. If they are viewed collectively, as they rarely are, they display interesting and valuable similarities in their largely implicit sociological orientations. And, if those orientations can be made somewhat more visible than they usually are, further similarities with the work already discussed should also be apparent.

(a) *The use of cultural resources.* One of the most straightforward approaches to the connections between scientific knowledge and the wider society is found in studies that show scientists taking up intellectual resources associated with other forms of culture. Before briefly discussing some work in this vein a cautionary note is in order. There are two major techniques for addressing the boundaries between 'science' and 'other forms of culture'. One, associated with some philosophers of science and quite a few 'internalist' historians, involves a prejudgment of what counts as science and what does not. Usually that prejudgment is informed by modern scientific conditions and is implicated in a series of evaluations about what properly *ought* to belong within science and what ought to be excluded. Whatever apologetic functions may be performed by such an exercise, strictly speaking it is historical nonsense. The other approach involves trying to ascertain how historical actors *themselves* defined what belonged to science (or 'natural philosophy', or whatever term and cultural domain was indicated) and what did not. This definitely *is* a historically significant project, for historical actors may well treat cultural items differently depending upon what side of their boundary they happen to place them. Thus the matter of so-called 'external influences' upon sciences is interesting insofar as the boundary in question is the actors' boundary and not one imposed willy-nilly upon the past.[20]

There is a rich variety of historical studies convincingly demonstrating that the cultural relations of science in the past were considerably different from what they are at present. For example, the seminal work of scholars such as Alexandre Koyré, Frances Yates and Walter Pagel, along with P. M. Rattansi, J. E. McGuire, E. M. Klaaren and others, has shown the close links between religious and general philosophical currents and developments within natural philosophy. 'Magical', 'neo-Platonic', 'hermetic' and theistic forms of culture, which now would be considered illegitimate if introduced within scientific culture, were important components of the scientific culture of the sixteenth and seventeenth centuries.[21] Interesting as this sort of work is, it is not directly relevant to central questions in the sociology of knowledge. How may we move from recognizing the disparate cultural connections of science to understanding the relations between scientific knowledge and aspects of social structure?

In the making of scientific knowledge *any* perceived pattern or organized system in nature, in culture, or in society may be pressed into service. These patterns serve as resources for understanding the natural phenomena in question. Some of these usages are well known to historians of science and their demonstration has not been regarded as particularly contentious. For example, there are several studies of William Harvey's use of contemporary mechanical pump technology in conceptualizing the workings of the heart [61; 78].[22] And there is an especially well-worked-out instance of the scientific use of technological patterns in Sadi Carnot's idealization of a heat engine in the construction of his thermodynamic theory [65, ch. 7; 70]. There is little disagreement among historians that technology is not part of science proper, and, therefore, that these count as examples of the scientific use of extra-scientific resources.

Historians treat instances of the scientific use of resources from technological culture in a relatively matter-of-fact fashion. Structurally, the same perspective may be adopted in dealing with resources deriving from social thought or social experience. For example, Martin Rudwick has written about the geologist Poulett Scrope's use of concepts from political economy in the understanding of geological time [74], and also about Charles Lyell's use of resources from human history, demography and political economy [75]. However, this matter-of-fact approach comes under threat when the alleged use of social resources concerns a particularly revered scientific production and especially when the social resources are viewed as ideologically suspect. In such cases two historiographic tendencies are in evidence: the first is to treat the use of such resources as an exposé or aspersion on the work in question; the second is to construe the matter in terms of the individual scientist's motivation or state of mind in using these resources. Nowhere are these tendencies more evident than in studies of Darwin's use of the patterns made available by Malthus's social thought [52]. To many writers an 'influence' from Malthus (or from Paley) has not been something to describe and explain, but something to be 'explained away', since, from present perspectives, it would be regarded as an illegitimate inclusion in properly objective scientific thought. Of course, such an individualistic and implicitly evaluative approach is not the only possibility and R. M. Young has shown the way in a series of exemplary papers. He has demonstrated that ideas associated with the early nineteenth century Malthusian debates over the correct distribution of wealth and power in society were also taken up by writers concerned with the scientific understanding of the distribution and succession of organic forms [79; cf. 62; 71]. Rather than identifying this as an instance of 'extra-scientific influences' [69] upon the culture of science, Young points to the existence in that setting of a "common context" in which cultural items routinely deployed in moral and political argumentation were also routinely brought to bear upon problems in the natural sciences [81]. The actors themselves did not regard such usages as illegitimate, although the evolutionary debates later came to involve considerable controversy about what counted as proper scientific discourse and what did not [77].[24]

Valuable as these studies are, for present purposes they do little more than serve as an added reminder that historical actors' conceptions of what counts as 'internal' to scientific culture is likely to vary from one setting to another; there is no reason to expect that present demarcations (and the evaluations they may express) will adequately describe any past context. Nor are patterns of resource-using in science, even when the resources happen to come from social thought, necessarily linked to actors' motivations, particularly to alleged ideological intentions [80, pp. 386-7]. Neither Scrope's uptake of banking metaphors nor Darwin's use of Malthusian conceptions reveals anything in particular about the social purposes of the actors concerned. On the other hand, the availability and comprehensibility of given cultural items will vary for groups differently situated in the social structure and at different times and places. We may echo the judgment of Charles Gillispie who observes that Darwin's use of individualistic and agonistic models makes it "inconceivable that [the *Origin of species*] could have been written by any Frenchman or German or by an Englishman of any other generation".[25]

Young's and Rudwick's work therefore shows that scientists may draw upon the materials provided by social thought; but their studies do not reveal that there were any other reasons why Scrope, Lyell and Darwin should have deployed these resources than that they were familiar with them and regarded them as valuable aids in doing scientific work. There was, that is, no evident purpose in or relating to the wider society that informed Darwin's or Lyell's use of resources deriving from social thought. There is, however, a particularly well-known study which relates the scientific use of such materials to an important purpose in the wider society. Paul Forman has written about the circumstances in which the physical and mathematical communities in Weimar Germany came to adopt acausal modes of scientific explanation [66; also 67]. It is difficult to summarize the arguments of this detailed and complex paper, yet the basic contention is that Weimar physical and mathematical scientists adopted attitudes towards determinism which were prevalent in the wider society arising from Germany's defeat in World War I. The development of quantum mechanics by Heisenberg, Schrödinger and others was in part a consequence of the scientific community's accommodation to powerful currents in the general social and cultural milieu. After the war it became fashionable to attribute the debacle to scientific materialism and determinism, and enormous pressures were brought to bear on scientists to dissociate themselves from these tendencies. One of the most important vehicles for this general rejection of determinism was the social philosopher Oswald Spengler's *Decline of the West*, a work which the historical actors showed no sign of regarding as 'scientific' (even though it did contain the outlines of a sociology of scientific and mathematical knowledge). Yet, as Forman argues, it was the importation into physics and mathematics of attitudes to causality expressed in Spengler's writings and pervasive in the wider society which provided one of the conditions for the production of the quantum mechanical revolution. To adopt, and be seen to adopt, these attitudes was to align Weimar science and its practitioners with increasingly powerful social forces in the milieu, thus defending it from the very real possibility of damaging attacks.[26] Similar processes of accommodation, and their consequences for scientific theorizing, have been made visible in quite different settings. For example, Brown has shown that the English medical community's favourable response to mechanical models of bodily function and disease in the late seventeenth century was informed by an appreciation of the high prestige attached to Newtonianism in that setting, a prestige partly deriving from its deployment as moral and social philosophy in physicians' political disputes with other castes of medical practitioners over professional rights and privileges, and, as it happened, mechanism was abandoned when those particular political considerations were dissipated [63; 64; and for works displaying similar processes 68; 72; 73].

(b) *The social use of nature in the wider society.* The work discussed in the preceding section deals with the deployment in the natural sciences of models, theories and attitudes current in social and political thought. Let us now turn to a body of historical writing that treats the deployment in *society* of conceptions of *nature*.

As we move from the professionalized and highly differentiated science of the present towards the natural knowledge of the seventeenth century we tend to move from a secularized natural order to one which was charged with moral, social and political significance [107, pp. 59-64]. Nature, that is to say, once had a constitutively normative dimension. The normative character of nature was generally thought to derive from the action of a Deity who had created both the natural and the social orders. This Deity might use the normal or abnormal functioning of nature to signal to mankind His overall will, His pleasure or displeasure at particular events. Debauchery, regicide, or insubordination might be punished by plague; good weather might bless conformity. The natural order was, therefore, a pool of moral significances which might be drawn upon as needed to comment upon specific political events or the proper order of society. In the seventeenth and eighteenth centuries such usages were pervasive, and the scientific culture of those periods can hardly be understood without considering the institutionalized moral and social uses to which representations of the natural order were put. To speak of such practices using modern vocabulary, as the 'social use of science', runs the risk of misleading. Seventeenth and eighteenth century moral uses of nature were not the 'scientistic' extrapolation of esoteric natural scientific findings onto social problems; the moral and social uses of nature were essential considerations in the evaluations historical actors made of various theories, models, metaphysics and statements of fact.

A critical overview of some of the most significant recent scholarship dealing with social uses of seventeenth and eighteenth century natural philosophy is available elsewhere and its findings need only be briefly summarized here [104]. For example, J. R. Jacob's studies of Robert Boyle's natural philosophy elucidate the religious and political setting in which English corpuscular philosophy was produced and evaluated [86; 87; 88]. Jacob finds that historical actors in mid-to-late-seventeenth century England regarded matter-theory as highly relevant to social and moral concerns. Thus the correct physical explanation of the behaviour of liquids in the classic Torricellian experiment and in the air-pump experiments at Oxford and Gresham College was treated by the actors as a matter of pressing moral significance. Boyle argued strenuously in favour of explanation in terms of the 'spring of the air', or, as we would now say, the differential pressures of columns of air. In so doing he identified alternative explanations in terms of 'nature abhorring a vacuum' as erroneous, and, interestingly, as morally pernicious and subversive of true Christian religion [86, pp. 114-15; also 104, pp. 99-103, 135-9]. Of what possible relevance to moral concerns was the physics of liquids in a partially evacuated glass tube?

The answer proceeds from the pervasive seventeenth century use of representations of nature to comment upon the social and moral orders. These usages became particularly intense and problematic in the 1640s and 1650s when the dissolution of traditional monarchical and ecclesiastical control in England set loose a deadly contest over the nature of moral and political authority in the state. Of particular interest was the proliferation of extreme religious sects, many of which rejected the notion of priestly intermediaries between God and the individual and abominated a hierarchical order of society. The Digger Gerrard Winstanley, for instance, developed a vigorous and coherent political programme which threatened to make away with established church, universities, legal and medical corporations, and the private ownership of property. His argument was founded upon a vision of God's relationship to the universe in which divinity was immanent in material nature just as it was immanent within each believer. Divine power was thus accessible to all; revelation was democratized and the hierarchical order which made nature dependent upon an external spiritual Deity, the believer dependent upon an external spiritual intermediary, and civil society dependent upon supervision by a divine-right monarch was collapsed and rejected. To the social groups for whom Boyle spoke the radical sectarian threat had to be opposed, and one way of opposing it was to produce and disseminate a philosophy of nature and God which insisted that material entities were "brute and stupid", that God was not immanent in nature, and that, therefore, nature, like a congregation and civil society generally, required for its activity the superintendence of external ordering and animating agencies. The notion that "nature abhorred a vacuum" was morally pernicious because it implied a hylozoism in which activity was essential to matter. It was subversive of institutionalized religion because it threatened the concept of an immortal soul: if there were no immortal soul which survived apart from the body, there could be no eschatological sanctions upon human behaviour [87; also 85]. And if there were no independent volitional soul apart from man's corporeal nature, then determinism, the ultimate liberation from man's responsibility to external moral authority, might be supported.

The overarching task of political and religious writers during the Interregnum was the reconstitution on a sure foundation of the basis of obligation in the state. After the Restoration the work of guaranteeing and securing that basis continued. The natural philosophy of Boyle and the early Royal Society was generated with a view to these social and moral uses; it was evaluated partly on the basis of how well it could be used in those contexts. The moderate and rational spiritual order and the limited monarchical order of the Restoration were soon under threat again. With the Exclusion Crisis, the Glorious Revolution, and, later, the intense uncertainty surrounding the Protestant Succession, the basis of moral and political authority in the state continued to be problematic from the 1680s until the 1710s. And in the political and theological debates inflamed by this long-lasting crisis of authority the social use of the natural order continued to be pervasive and important [89]. Margaret Jacob has studied the moral and social uses of the Newtonian philosophy of nature disseminated from the 1690s by the Boyle Lecturers [90]. Again, it is found that an insistence upon the inert character of matter and its dependence upon external animating causes had important apologetic functions. In particular such representations of the natural order served to secure the moral authority of a Church which was coming under increasing attack from libertine, Hobbist, and freethinking deist circles in the late seventeenth and early eighteenth century. In a more finely-textured account Simon Schaffer has pinned down the particular political and ecclesiastical factions to which many Newtonians gave their allegiance and has shown the social and moral uses which informed the production and evaluation of detailed aspects of contemporary chemistry, physics and astronomy [100]. As Low Church Court Whigs the Newtonians sought simultaneously to celebrate the prerogatives of monarchical power and to show its proper natural limitations. This they did in part by displaying a natural order whose phenomena showed the clear marks of God's supreme and unrestricted will while also manifesting God's perfect wisdom in framing natural laws in accordance with which the cosmos mainly functioned. Depending upon which source of the perceived threat the Newtonians were addressing they might elect to stress either God's will or His wisdom in His relations with the natural world and the 'world politick'. In the extended controversies with Leibniz Newtonian writers like Samuel Clarke laid especially heavy emphasis upon the sovereign power of God's will in nature, explicitly linking this to God's special providence in society and the supremacy of the monarch's will. In this context of use their preference for a voluntarist philosophy of nature proceeded from a perception that intellectualist philosophies linked Leibniz, as the Hanoverian court philosopher of the future royal house, to deistical factions in England which had been arguing vigorously against both the power of king and court and the rights of the Established Church [100, ch. 7; 105; 106]. Elsewhere, Schaffer has argued that the *dramatic* character of natural philosophy in this period is one of the keys to its moral uses [99]. For example, the public display of violent electrical phenomena produced by the Leyden jar served to make visible to every person the power latent in nature and available to God [101]. Much of Schaffer's valuable work on seventeenth and eighteenth century natural philosophy is concerned with the question of *access*: how could God's power be *made manifest* to everyone? Unless God's providence and potency could be made visible to all it was widely felt that the foundations of moral order were unsure. Thus it was part of the eighteenth century natural philosopher's 'job description' to make God's power manifest.

In a setting in which representations of nature are used and evaluated as tools to further wider social interests a network of calculations is likely to be established: contingent associations between particular views of nature and specific constellations of social interest will be recognized and will then provide a basis of calculation and evaluation by other interest groups. In order to oppose the social interests of a group it may seem advisable to discredit and combat the view of nature which that group uses as a social strategy. Such a complex network of calculations involving wider social interests and the use of natural philosophy is evident in eighteenth century Britain. Perceptions that Newtonian natural philosophy was the apologetic resource of a particular party in ecclesiastical and temporal affairs provided the basis for a series of attacks on the adequacy of that philosophy emanating from groups whose social interests conflicted with those of Low Church Court Whig Newtonians. Thus Christopher Wilde has described the Hutchinsonian natural philosophy which many High Church clerics adhered to as a vehicle for combatting the authority and ecclesiastical dominance of the Newtonians [110; also 109]. Margaret Jacob has examined the anti-Newtonianism of the 'Commonwealthmen' for whom John Toland spoke [90, ch. 6; 91; 92]; and John McEvoy has studied the anti-Newtonian rational dissent expressed by the Unitarian natural philosopher Joseph Priestley [93; 94; 95]. All these groups represented social interests which were in conflict with the Newtonian hegemony and all produced natural philosophies which sought to erode key aspects of the Newtonian world-view. And by these processes of opposition the natural orders constructed by Hutchinsonians, by the radical Commonwealthmen, and by the 'rational dissenters' came to share a stress upon the self-sufficiency of nature: but in the cause of different interests. Hutchinsonians advocated a mechanically self-sufficient universe because it was beneath the dignity of a High Church Deity to intermeddle with the material and the mundane; Commonwealthmen rejected voluntarism for Old Whig political reasons (such a God had been the legitimating resource of the Court faction); and Priestley did so in recognition that this God had been used to stifle rational dissent. Thus, shared cosmological representations did not proceed from shared 'social backgrounds', or even from shared social interests, but from interests in attacking positions associated with a common opposition.

The significance of the social use of nature becomes even more visible when *human* nature is at issue. How plastic or rigid is man's constitution? Is there a portion of human nature (how large?) about which one can do nothing, and is there a portion over which one has volitional control or which is subject to modification by environmental forces? These disputes, of course, have a long history and need not take a specifically 'biological' form; thus the general shape of subsequent conflict over the moral significance of human necessity and liberty is prefigured in the celebrated Hobbes-Bramhall debates of the seventeenth century and in Priestley's disagreements with the Scottish philosophers of Common Sense: neither of those episodes involved anything like a 'genetic' theory of human limits. However, by the early nineteenth century a biological theory which seemed to set boundaries on the sphere of human accountability did appear in the form of phrenology. Originally developed in Vienna and Napoleonic Paris as a reaction against Enlightenment meliorism, phrenology in Britain and the United States was forged into an extremely important naturalistic movement—the 'forerunner', if one wants to use the notion, of late Victorian scientific naturalism. The career of phrenology in Edinburgh has been the subject of some quite explicitly sociological study [102; 103; 121]. In these analyses phrenology appears as the strategy of disaffected mercantile groups in early-to-mid-nineteenth century Edinburgh society. Part of their strategy consisted in opposing the academic intellectual elites who, in their view, monopolized access to the universities and mystified proper mental philosophy through their 'method' of introspection. To the Enlightenment environmentalism of the academic philosophers, Edinburgh phrenologists symbolically juxtaposed the 'hereditarianism' of the phrenological system of human nature. But the unmodified assertion that human character was laid down by nature could scarcely further the wider interests of a group which felt itself badly served by the current distribution of rights and resources in British society. Thus phrenology in Britain had another face. Phrenologists claimed that a reliable observation-based (and therefore 'scientific') system of character-diagnosis was a prerequisite to *shifting* human nature in a desired direction; for the size of the thirty-five cerebral organs subserving each distinct mental faculty indicated the traits an individual would come to display, *other things being equal*. Things could be made 'unequal' by a whole array of interventionist environmental techniques: education, public health, even, over generations, what later came to be called 'eugenic' marriages. British (and American) phrenology thus developed into one of the most important naturalistic resources deployed by bourgeois social reformers. Later on we shall see how preferences for or against such changes in the wider society featured in detailed judgments of anatomical fact in the context of cerebral anatomical research.

It was not only phrenology which displayed an interestingly plastic and dynamic view of human heredity in the first part of the nineteenth century. Theories of human heredity shared by medical practitioners and the laity held that heredity was a *process*, extending from the moment of conception through gestation and even weaning. It was also believed that what was inherited was not a trait but a *tendency*, say, a tendency to develop certain chronic diseases. What resulted was a transactional theory of disease in which practitioners could point to nature as the reason why certain of their interventions failed, while also extending their sphere of influence over an individual's patterns of behaviour [97; 98]. Since, in this conception, 'acquired characteristics' could be inherited, what one did with one's own life had a bearing on the constitution of future generations, and was therefore a legitimate area for the concern of society and its medical experts. For example, this dynamic theory of human heredity figured in arguments both for and against female emancipation [108]. Moreover, like the phrenological system, the dynamic view of heredity provided the naturalistic foundations for melioristic social reforms: within natural limits, human nature could be changed for the better. However, by the end of the century, as Rosenberg shows, both medical men and bourgeois social thinkers came to prefer a far harder and more rigid view of human heredity: nature became more unforgiving as social reforming postures *vis-à-vis* the working classes began to seem a less attractive strategy. Hard hereditarianism now manifested itself in the guise of eugenics.

As MacKenzie has shown, eugenics is appropriately viewed as the strategy of the British professional middle classes [35, chs 2, 4].[27] By assuming that the social order was, with some discrepancies, a natural order founded on the biological endowments of individuals, eugenists like Francis Galton found a naturalistic justification for the social claims of 'brain workers' over the hereditary aristocracy, plutocrats and manual labourers. Perhaps more importantly, eugenics fitted into the overall strategy of scientific naturalism, offering both a theory of society and a programme of practical action which made maximum use of the skills and competences of secular intellectuals like scientific medical men and allied professionals. Eugenic views and eugenic programmes, like the other conceptions of human nature we have briefly discussed, are best seen as the strategy of specified social groups using conceptions of human nature as a persuasive resource. The usefulness of this mode of analysis to an understanding of the contemporary debates over racial differences in IQ has been demonstrated in several perceptive papers by Harwood [82; 83; 84]. And Provine has made a related point in his examination of changing evaluations by geneticists of the effects of race-crossing [96]. Both in the case of human nature, and in examples from natural philosophy in the seventeenth and eighteenth centuries, the use of representations of nature in a wider social context formed a basis for scientific judgments.

V. FULL CIRCLE: CONTINGENCY AND WIDER SOCIAL INTERESTS

For purely conventional reasons this paper has so far considered the role of a variety of social interests as if they were distinct and manifested themselves in separate bodies of knowledge. The time has come to correct any such impression and to attempt to put together a number of historiographic orientations which are often seen as incompatible. One traditional source of difficulty in sustaining a sociological approach to scientific knowledge comes from the view that the power and validity attributed to science is guaranteed by its freedom from 'social influences'. In this account social considerations can only work to corrupt proper science; the scholar convinced of the value of science and concerned to defend it from attack must therefore take great care before showing the presence of social interests in scientific activity. Writers in this tradition tend to read sociological accounts of scientific knowledge as aspersions, however great the pains taken by sociological writers to state otherwise. By now this particular battle has been fought so many times that it is pointless to do more than reiterate: sociological accounts have no bearing upon whatever evaluations one may wish to put upon science; indeed, the major reason why such accounts are frequently self-described as 'naturalistic' is simply that they have no evaluative axe to grind.

A related source of misunderstanding seems to stem from within certain strands of sociological thinking, namely a tendency to regard bodies of knowledge as the manifestations of single types of social interest. Knowledge used, for example, to legitimate structures in the wider society is considered to be different in kind from knowledge used for the 'prediction and control' of phenomena.[28] Since these interests are thought to be incompatible, so must the types of culture they produce. In the history of science such an impression may be reinforced by the purely conventional fact that empirical studies of particular bits of science tend to fall into distinct genres: there are studies which treat Newtonian natural philosophy as informed by technical interests in prediction and control and which situate it in a cultural tradition, and there are studies which assess the same body of culture as a legitimating resource deployed in the wider society. The historian may feel he is being asked implicitly to choose between incompatible approaches. Of course, 'choice' is not necessary, and there are already several empirical studies which explicitly make this point [104, pp. 124-31].

In a particularly concise example Lawrence has studied conceptions of the human nervous system and its functioning in eighteenth century Scotland [117]. The major empirical concern of his paper is to show that theories of nervous 'sensibility' and 'sympathy' in that setting were evaluated according to their use in justifying the cultural and social leadership of Lowland intelligentsia and their allies. But Lawrence goes on to argue that these conceptions functioned in both 'scientific' and apologetic contexts; they were, as he says, "multifunctional", and, while he does not himself show their detailed usage in medical and physiological settings, he points to a body of historical work which does display such a role. Similarly, Wynne's account of aetherial and energetic conceptions of matter in late Victorian Cambridge argues the importance of their use as an anti-naturalist, anti-professionalizing strategy, strongly linked to psychical research, without in any way claiming that such ideas did not also inform much technical work in the study of radiation [122]. But to make the point about multifunctionality [103] totally convincing, and to advance our understanding of interests and scientific knowledge generally, it is best to turn to empirical studies which themselves document the role of a variety of interests in the development of knowledge.

Let us return to the cluster of sub-cultures which encompassed the study of evolutionary mechanisms (biometry and Mendelism), the biological understanding and manipulation of social structure (eugenics), and the techniques thought requisite to eugenic theory and practical programmes. In briefly considering some empirical literature dealing with biometry and Mendelism I pointed to the possible explanatory role of the distribution of scientific competences and skills (see Sect. II above). However, some of the writers on these episodes explicitly state that such considerations are insufficient to explain the controversies: Pearson apparently possessed the competences to have embraced Mendelism if he so chose; Bateson's attachment to Mendelism cannot satisfactorily be explained by his particular experimental skills; and proposals to reconcile the two orientations were systematically rejected or ignored for quite some time. MacKenzie and Barnes [36; 37; also 35, ch. 6] thus turn to factors operating in the general cultural, social and political milieu of late nineteenth and early twentieth century Britain, that is to factors generally identified as 'external' to the natural scientific culture of that setting. If one proceeds in a traditional historical manner and concentrates on key individual actors, one can discover interesting differences in their social and political views. Karl Pearson, the major British biometrician, came from a dissenting middle-class background [119; also 35, ch. 4], and had pronounced anti-clerical, anti-*laissez-faire,* social imperialist views close to those of many Fabians. His belief in biological gradualism was paralleled by his strong commitment to progressive and gradualist social change; indeed, he believed in the application of the results of scientific investigation to social problems. The display of progressive continuity in nature underwrote a commitment to progressive and continuous social change. A commitment to continuity thus ran through Pearson's evolutionary views and his highly-developed social philosophy. By contrast, his opponent William Bateson was connected to traditional academic elites and was deeply mistrustful of the effects of industrialization, and the ideologies of utilitarianism and evolutionary social progress. As Coleman has shown [124], Bateson was, in Mannheim's sense, an essentially conservative thinker.[29] He thought that both science and society ought to 'treasure exceptions'; just as evolution depended, in Bateson's view, upon the exceptional discontinuity, so social progress depended upon the uncontrollable appearance of rare individual genius.

So preferences for biometrical *versus* Mendelian explanation appear to proceed from divergent social orientations; preferences for continuity theories versus discontinuity theories in the natural sciences were structured in part by conflicting interests in the wider society. These divergences also manifested themselves in attitudes towards potential courses of practical social action, particularly towards eugenics. Pearson, like many biometricians, was a committed eugenist, while Bateson, like other opponents of biometry, was deeply suspicious of eugenics. This is an association which becomes more understandable when one recognizes the extent to which biometry was developed with a view to coping with the problems posed by a eugenic view of society and by practical eugenic programmes of action [35, chs 5-6; 120]. Insofar as eugenics was the strategy of a particular interest group in British society, the biometry-Mendelism controversy was sustained by conflicting interests in the distribution of rewards, rights and privileges in the wider society. Of course, recognizing these features of the controversy supplements rather than diminishes the significance of professional competences and skills. A range of social interests, including those usually considered 'internal' and 'external' to the scientific culture, needs to be considered in order satisfactorily to explain this particular episode.

This methodological point becomes especially important when one considers some of the mathematical tools developed within this cluster of sub-cultures. For example, Ruth Cowan showed some years ago that Francis Galton's statistical theory was informed by his eugenic commitment [113; also 112; 114; 115]. She took the view that the significance of eugenics was that it provided the motivation which turned Galton towards particular statistical questions the content of which was, presumably, not dependent upon Galton's eugenic purposes. Recently, however, MacKenzie has pressed the point that "the needs of eugenics in large part determined the content of Galton's statistical theory" and has produced detailed demonstrations of this in his account of the differences between Galton's work and that of the error theorists, and in his discussions of Galton's work on regression, correlation, and the bivariate normal distribution [35, ch. 3]. However, given our present concern with the range of social interests involved in sustaining scientific knowledge, it is MacKenzie's study of statistical controversy between Pearson and G. U. Yule which provides better illustrative material [35, ch. 7; 111; 118]. This was a highly esoteric controversy within early twentieth century British statistics dealing with the correct way to measure the association of data arranged in contingency tables. The controversy overlapped with the most intense phase of the biometry-Mendelism conflict and involved some of the same major actors. By 1900 there was general agreement on how to measure the correlation of normally-distributed interval variables, but it was uncertain how best to deal with nominal variables, i.e., those for which no unit of measurement existed such as 'alive' or 'dead', 'vaccinated' or 'unvaccinated'. From 1900 Pearson's approach was to treat nominal variables in the contingency tables *as if* they were produced by an underlying bivariate normal distribution.

Pearson was aware that this was often an untestable assumption, but he nevertheless regarded his measure of correlation as the correct one; others were indeed possible, but these were treated as approximations to the tetrachoric coefficient. By contrast, Yule did not make the assumption of underlying normal distribution, and by 1905 he openly attacked Pearson's work, especially the assumptions underlying the tetrachoric. The controversy continued for a decade, involving a good part of the small British statistical community.

MacKenzie stresses that Pearson (and his followers) and Yule (and his followers) had different goals in statistical theory: the former wishing to maximize the analogy between the treatment of interval variables and nominal variables, while the latter wanted to treat nominal data *sui generis*. These differing goals MacKenzie terms divergent "cognitive interests". The two positions were incommensurable by virtue of Pearson's and Yule's differing goals in statistical theory [111]. But MacKenzie goes on to try to explain why it was that differing cognitive interests were so distributed. MacKenzie's analysis is too subtle to be briefly summarized; however, he connects the two sides' conflicting views on association with their divergent positions on eugenics. Pearson, MacKenzie shows, developed his statistical theory in the manner he did because of the requirements of the eugenic programme to which he was committed. Yule, on the other hand, had no commitment to eugenics and developed his statistical work in this area differently. And, as MacKenzie has already shown, commitment to eugenics is itself to be referred to wider social interests, such as those affecting the professional middle classes, on the one side, and traditional elite groups, on the other.

Thus esoteric work in mathematical statistics is explained by referring different views to divergent purposes within the statistical community, and also to diverging goals in the wider society. Historical work of this sort therefore illustrates two points of relevance here: firstly, it shows beyond any doubt that the explanation of even the most technical and esoteric scientific activities *may* need to be referred to wider social interests. In this respect it has long seemed that the history of mathematics is an unusually tough nut for the sociology of knowledge to crack, but cracks have indeed begun to appear recently.[30] Secondly, MacKenzie's work, and other studies to be discussed shortly, erodes any tendency to think of wider social interests as affecting, as it were, the 'outside' of scientific knowledge (models, metaphysics and metaphors) while the esoteric core is generated solely through disinterested contemplation of reality. Any such 'two-tier' model of the sociology of knowledge gets no support here. And finally, such work reinforces the point made by studies discussed above: that institutionalized bodies of scientific knowledge may typically be sustained by a variety of social interests, and these may cross-cut historians' conventional categories of 'internal' and 'external' considerations.

By connecting interests in the wider society to judgments of the adequacy and validity of esoteric mathematical formulations we have come close to completing the methodological circle. We started by considering historical studies which showed the contingency of scientific judgments about experimental findings and matters of fact, and we have now reached the point at which we can begin to see that such judgments may well be structured by wider social interests. Let us amplify and refine this point by briefly examining two final historical studies: one dealing with the notion of a 'competent experiment' and another concerning observation reports. Farley and Geison have studied nineteenth century French controversies over the spontaneous generation of life [116]. In the late 1850s the Rouen naturalist Felix Pouchet pronounced himself convinced of the reality of spontaneous generation and published what he took to be experimental proof of the phenomenon: micro-organisms appeared in boiled hay infusions under mercury after they had been exposed to artificially generated air or oxygen. These experiments elicited immediate critical comment from Louis Pasteur: he suggested to Pouchet that his experiments had been improperly performed; contaminated air had almost certainly been introduced and this error of procedure, rather than spontaneous generation, was responsible for the appearance of life in the flasks. Immediately thereafter Pasteur undertook his own series of experiments. He took a set of flasks high on a glacier in the French Alps, exposed them to the rarified (and presumably uncontaminated) air and showed that only one developed signs of life. This one Pasteur regarded as an anomalous result—the series definitively proving that competently performed experiments refuted spontaneous generation. Pouchet felt obliged to replicate Pasteur's experiments (although the replication was not exact), and he went to the Pyrenees with his flasks, where all showed signs of life when briefly exposed. This Pouchet took to be proof that competently performed experiments established the fact of spontaneous generation—all that was needed to make life appear in an organic infusion was oxygen. Since Pasteur and Pouchet could not agree between themselves about criteria for a 'competently performed experiment', Pouchet issued a challenge which resulted in the appointment of an adjudicating commission by the Paris *Académie des Sciences.* In the event, that commission was heavily stacked in favour of those already convinced of the impossibility of spontaneous generation; some of the members announced against Pouchet even before the experiments were examined, and so Pouchet withdrew, leaving the prize to Pasteur.

Farley and Geison thus describe a situation already familiar to us from the sociological work of writers like Collins and Pickering [e.g., 4; 6; 7; 16]. There are no transcendent criteria by which the competence of experimental procedures may be judged; prior commitment to the existence or non-existence of the phenomenon in question necessarily enters into a judgment about whether or not relevant experiments have been competently performed. But Farley and Geison do not seek merely to establish the contingency of judgments about experimental procedure; they seek to identify particular contingent social considerations which structured differing judgments. As it happens, those considerations relate scientific judgments to concerns in the wider political and moral setting of mid-to-late nineteenth century France. The issue was materialism, and the consequences perceived to flow from materialism in that context. Belief in spontaneous generation seemed to imply the self-organizing capacities of matter and (echoing seventeenth century hylozoist themes) therefore to threaten the existence and role of the external spiritual agencies upon which the authority of the Church rested. While Pouchet stipulated the religious orthodoxy of his particular version of spontaneous generation, Pasteur's forces insisted upon identifying their opponent's views as heterodox and dangerous to public morality. Farley and Geison conclude that "members of the French scientific community may have chosen Pasteur over Pouchet" (and therefore have judged the competence of their respective experiments) "at least in part for socio-political reasons" [116, p. 183].[31]

In Farley's and Geison's example we see how wider social interests bore upon evaluations of experimental competence, and, therefore, upon the truth-status of experimental findings of fact. The micro-sociological focus of writers like Collins and Pickering is supplemented here by a macro-sociological analysis: the relevant social factors in this case happened to include both features internal to the sub-culture of French science and considerations linking science to religious, moral and, ultimately, political discourse. It is a contingent association: one which there is no reason to expect operates in all controversies over experimental competence. One might, for example, be surprised to find views for or against the authority of religious institutions figuring in the gravity radiation controversies (but perhaps less startled to find that similar considerations might prove relevant to explaining the present-day disputes over species change). The appropriate methodological strategy derives from the historical circumstances, not from the level of culture the historian seeks to explain. This is best demonstrated by bringing our methodological excursion full circle; let us consider an episode in which actors disagreed about visual observations.

The setting is one which we have already introduced: the disputes over the validity of phrenology in early nineteenth century Edinburgh [102; 103]. We have seen that phrenology was the argumentative strategy of groups in that society which were concerned to erode the authority of existing academic and spiritual elites and to substitute for it a proto-naturalist, participatory science of man as natural object. The local controversies thus tended to array disaffected and iconoclastic bourgeois groups against traditional elites and their intellectual spokesmen; it is no exaggeration therefore to see the Edinburgh phrenology disputes in terms of the macro-sociological category of social class. Nevertheless, these controversies also involved a series of esoteric issues in cerebral and neuro-anatomy; there were disputes over the exact contours of the cranial bones, the patterns of the cerebral convolutions, the exact fibrous constitution of the hemispheres, and the fine structure of the cerebellum and the fibres connecting it with other parts of the brain and the spinal cord [121]. Participants in these disputes violently disagreed about what could be seen when one looked at these structures. There might be a temptation to separate the controversies into cosmological and methodological components, on the one hand, and esoteric, technical and 'scientific' matters, on the other. It might be thought that the former could be referred to macro-sociological considerations, while the latter must pertain solely to concerns within the sub-culture of anatomy. This proves, on inspection, not to be the case. Anti-phrenologists' insistence that cranial bones in the region of the frontal sinuses were not parallel was explicitly connected to their claim that phrenological character diagnosis was impossible; phrenologists' assertion that the cerebral convolutions might show standard pattern and morphological differentiation was explicitly related to their view that mental faculties were subserved by distinct cerebral areas. Similarly, disputes over the fibrous nature of the brain mass were closely associated with conflicting views of the differentiation of cerebral organs and of mental function. As the anatomical disputes, so to speak, pushed deeper into the brain and into esoteric anatomical issues, the participants themselves seemed less able convincingly to assign social interests to their opponents' claims. Indeed, it was the public appearance of disinterestedness upon which rested, in that setting as in many others, the credibility of the claims advanced. Nevertheless, close observation of the disputed phenomena did not lead to a convergence of claims, for observation was, apparently, a thoroughly political process and the political ends of the parties involved differed. In this case, as in the micro-sociological studies discussed in the first section, natural reality did not possess the coercive force with which actors' discourse often imbued it. Reality seems capable of sustaining more than one account given of it, depending upon the goals of those who engage with it; and in this instance at least those goals included considerations in the wider society such as the redistribution of rights and resources among social classes.

CONCLUSIONS

If the empirical achievements of the sociology of scientific knowledge are really as impressive as made out here, why have they received so little recognition? And why does the empirical sterility of the sociology of knowledge continue to be cited as a major reason for rejecting the approach? I have already suggested that part of the explanation must reside in historians' interpretive reticence; it is sometimes possible to miss the sociological thrust of certain exercises, and it is not unknown for authors themselves to put a radically anti-sociological label on work which can strongly support a different conclusion. Nor is it beyond the bounds of possibility that the current distribution of reward in the academic history of science affects what some writers *say* about their work more strongly than it affects what they *do*. Too much importance must not, however, be laid upon these considerations; at the end of the day it is practice that is important.

An apparently more significant problem arises from a largely informal model of sociology of knowledge which seems to be prevalent among a number of philosophers and historians of science. For ease of reference this may be called 'the coercive model': its main characteristics can be briefly described: (i) it maintains that sociological explanation consists in claims of the sort: "all (or most) individuals in a specified social situation will believe in a specified intellectual position"; (ii) it treats the social as if one could derive it by aggregating individuals; (iii) it regards the connection between social situation and belief to be one of 'determination', although little is explicitly said about the nature of determinism; (iv) it equates the social and 'irrational'; (v) it equates sociological explanation with the invocation of 'external' macrosociological factors; (vi) it sets sociological explanation *against* the contention that scientific knowledge is empirically grounded in sensory input from natural reality.[32] On these suppositions what would a practical piece of sociological explanation look like? In the first place it would be fundamentally prosopographical: one would search for statistical correlations between the social circumstances of groups and their scientific beliefs; one would worry about 'exceptions' and about the 'level of significance' of the correlations; and individuals would generally be regarded as troublesome, as they would frequently not 'fit' the causal connection being tested. On the other hand, the connection between the social and the cognitive would generally be sought through the use of individualistic orientations, particularly through the category of 'motivation'. Then one would contrast actors' apparently volitional actions with the role of the social; those courses of action that seemed to be purposive (or 'rational') would be excluded from the sociological ambit and be treated as self-explanatory. One would look exclusively to macrosociological categories for one's explanatory tools; factors internal to the scientific community would be viewed as non-social. And on this basis one might claim that there are interpretive and methodological asymmetries between sociology and history of science, or between the study of modern professionalized science and that of past settings. Finally, one would say as little as possible about the fact that scientists conduct experiments, look down microscopes, go on field expeditions, and the like, for wherever 'reality' enters in, there sociological explanation is obliged to stop.

Of course, it should be apparent that the 'coercive model' has, from a certain point of view, two splendid advantages. First, it is a model for the sociology of knowledge that maximizes the chance that no successful instance of its practice will ever be encountered. Second, it portrays the role of the social and of sociological explanation in unpalatable normative light: as if it were said that "no rational person would ever allow himself to be socially determined!" Nevertheless, there is one major problem confronting the 'coercive model': namely, that it is *not* an accurate picture of sociological practice. One could establish this programmatically, or one could proceed in a style more in keeping with the present exercise. by looking at what the empirical literature actually does, and by trying to tease out of various approaches represented there some common sociological sensibilities and explanatory tactics. Naturally enough, there is a variety of sociological perspectives available in these writings, and few historians do anything so vulgar as to advance an explicit model of explanation. However, one would not seriously misrepresent the interpretive thrust of much of this work by discerning in it what may be called an 'instrumental model' of sociological explanation. What are its main characteristics?

For many writers a sociological approach meshes with some routine practices in the history of ideas; for example, the state of knowledge at any given moment is matter-of-factly treated as the base point for cultural change. The cultural heritage is socially transmitted; no man invents his own language, and, just as the speaking of English in seventeenth century England was socially transmitted, so the heritage of, say, natural philosophical concepts and practices was socially transmitted to individual men of science, whatever innovations they then wrought on this legacy. Thus variability in concepts and practices in different settings and among different groups is frequently referred to patterns in the social agencies that transmit knowledge: schools, universities, churches, and the like. Of course, the resources that are available to solve scientific problems may not be the monopoly of formal educational and cultural institutions. People may deploy the resources provided by the experiences of living and operating in society, although, as we have seen, it is an entirely contingent matter whether scientists do so or not. Much of this is generally regarded as uncontentious, although many historians of ideas still treat contributions to culture as if they were generated *in vacuo* by atomistic individuals, and some of them continue to view the source of cultural materials used in science as a matter of moral concern.

In a sociological approach to knowledge-making, people produce knowledge against the background of their culture's inherited knowledge, their collectively situated purposes, and the information they receive from natural reality. Perhaps the most puzzling charge sometimes laid against relativist sociology of knowledge is that it neglects the role played by sensory input.[33] On the contrary, the empirical literature employing this perspective shows scientists making knowledge 'with their eyes wide open to the world'. If anything, writers such as Collins, Farley and Geison, Kohler and Pickering have been more intensely concerned with how scientists conduct experiments, focus on reality, and come to terms with the sensory information channelled by experiment than many more 'traditional' historians and philosophers from whom such criticisms often come.[34] Both in this empirical literature and in the theoretical sociology of knowledge corpus there is no question of denying the causal role of the *unverbalized reality* upon which given scientific beliefs focus.[35] What is perhaps at issue here is whether a specific *verbal formulation* of reality is to be *privileged* in sociological and historical explanation. The historian may indeed have little choice but to 'lay a bet' on the physical reality which impinged on actors, and in 'laying that bet' he might well opt for a modern text-book account.[36] However, he must remain on his guard against using that account as a *sufficient* explanation of beliefs that accord with it. If the historian succumbs to this temptation, he will indeed talk about 'natural reality' as a 'constraint' upon what is said about it. But whatever appeal this procedure may have to rationalist and realist writers, historians ought to be aware of what is involved: for it may be nothing less than the very Whiggism and 'presentism' that historians have so generally agreed to abominate. To reject privileging specific verbal formulations of reality is not to reject the role of sensory input: it is to write more sensitive history. It is the opponent of relativist sociology of knowledge who would make the actor a "judgmental dope".[37] In this case, it is 'reality' which is said to coerce the actor.

This leads us on to what may be the most central aspect of the largely implicit 'instrumental model': the generation and evaluation of knowledge is treated as goal-directed. Knowledge is not regarded in this literature as contemplatively produced by isolated individuals; it is produced and judged to further particular collectively sustained goals. Knowledge, in this perspective, is always tailored to doing things. It is in the course of doing things with knowledge that its meaning is produced; thus, the notions of use and meaning are intertwined. We have seen this instrumentalist perspective at work both in the study of past science and its wider social relations and in the explanation of scientific controversies in present-day science. The purposes for which knowledge is produced and according to which it is evaluated may vary very widely: they may include the legitimation or criticism of tendencies in the wider society, or they may encompass goals generated exclusively within the technical culture of science. And, as we have seen, there are many instances in which both sorts of instrumental goals bear upon the production and evaluation of culture. Typically, usage and meaning will be embedded within a complex social network of calculations, such that possible connections always exist between considerations in all parts of the net.[38] As MacKenzie and Barnes conclude from their interpretive study of the biometrician-Mendelian controversies, "The general point is not that the goal-directed character of scientific judgment implies its relationship to any particular contingency, or to external factors, or political interests; what is implied is that any such contingency *may* have a bearing on judgment and that contingent sociological factors of some kind *must* have" [37, p. 205].

Finally, this brief exposition of a working instrumentalist model in the history of science allows us to reflect back upon certain aspects of the 'coercive model' that have hindered appreciation of the actual role of the social. In that model, the social was routinely contrasted to the 'rational'. However, in the empirical literature we see no such contrast. Actors are all treated as if their 'cognitive wiring' was in proper working order: that is to say, they are all possessed of 'natural rationality'.[39] That rationality is expressed in the instrumental character of their behaviour. Their calculations may, as a matter of fact, take into consideration goals pertaining to the wider society or they may not. Actors' judgments which are informed by wider social interests seem no less intelligible and competent than those which do not. Given that this is so, the only conceivable purpose to be served by equating the social with the 'irrational' is stipulating which sorts of considerations the ideal type of the modern scientist *should* take into account. Few historians will see this as an essential and proper part of their activity. It is this patently normative attitude towards 'rationality' which appears to inform the 'coercive model's' view of determination and the social. We are invited to conceive of 'social determination' as if it were a sort of mugging. But which model is it really that makes out actors as "judgmental dopes"? In an instrumentalist perspective actors are seen to produce and evaluate knowledge against the background of socially transmitted knowledge and according to their goals. The role of the social, in this view, is to prestructure choice, not to preclude choice.

I have attempted to show here that the sociology of knowledge is more than a set of theoretical and programmatic reflections upon what might be done; it is also a body of practical achievements. While there is every reason for satisfaction about the state of the empirical literature, there is no justification for complacency. Empirical writings are attended with problems as well as advantages, and it is highly desirable that practically minded historians should become more aware of the interpretive purport of their achievements: both for their own concerns with the handling of concrete materials and for the clarification of more general issues in the theory of knowledge. Given proper awareness of this work, there might also be no more talk of "the widely acknowledged failure of cognitive sociology to explain any interesting scientific episodes".[40]

A slightly different version of this paper was read to a Colloquium on Ludwik Fleck and the sociology of knowledge at Haus Rissen, Hamburg, in September 1981, and will be published in German in the proceedings of that colloquium. Part of this work was done with the support of a Fellowship from the John Simon Guggenheim Memorial Foundation.

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1. Joseph Ben-David, *The scientist's role in society: A comparative study* (Englewood Cliffs, N.J., 1971), 7-13.
2. A. Rupert Hall, "Microscopic analysis and the general picture", *Times literary supplement* (26 April 1974), 437-8, p. 438.
3. Larry Laudan, *Progress and its problems: Towards a theory of scientific growth* (London, 1977), ch. 7, esp. 204, 209, 219, 243 n. 10. For programmatic criticisms of Laudan's strictures: Barry Barnes, "Vicissitudes of belief", *Social studies of science,* ix (1979), 247-62; David Bloor, "The strengths of the strong programme", *Philosophy of the social sciences,* xi (1981), 199-213.
4. Especially Barry Barnes, *Scientific knowledge and sociological theory* (London, 1974); *idem, Interests and the growth of knowledge* (London, 1977); David Bloor, *Knowledge and social imagery* (London, 1976); Michael Mulkay, *Science and the sociology of knowledge* (London, 1979).
5. Nor will I treat the history of the social sciences, although I do not accept that these materials present an 'easier case' for the sociology of knowledge in anything but a persuasive sense. Perhaps the greatest losses resulting from my selective criteria are: (i) an excellent literature dealing with the cognitive foundations of research schools and disciplines; and (ii) some attempts to operationalize Mary Douglas's 'grid-group' schema. Some selected references are included in the Bibliography, Section VI (c) and (d).
6. Obviously, I cannot and do not claim scholarly competence in all the relevant areas; therefore I cannot 'vouch for' the factual accuracy of much empirical work I treat. Nevertheless, I see no major problem in presenting empirical achievements as, so to speak, 'state of the art'. Very little of this work has been challenged in print, but, where such challenges do exist and may bear upon the adequacy of interpretive perspectives, I shall make every effort to point this out in references. I must also stress that summarizing empirical studies always results in loss of detail and therefore of persuasive power. The brief sketches I provide should be regarded more as *guides* to reading empirical work, than as substitutes for reading it.
7. A positivistic sociology of the *dynamics* of science and its foci of interest appeared in R. K. Merton, *Science, technology and society in seventeenth-century England* (new ed., New York, 1970; orig. publ. in *Osiris,* iv (1938), 360-632).
8. Rudwick is preparing a full-length study of the Devonian controversy.
9. The argument establishing that in principle all experimental conclusions can be challenged was stated by P. Duhem in *The aim and structure of physical theory* (Princeton, 1954), ch. 6. If an experiment produces unexpected results or appears to refute a hypothesis, it is always possible to lay the blame on a subsidiary assumption in the test procedure. Using the usual notation of symbolic logic: if $A \cdot H \longrightarrow O$ and $\sim O$, then all that can be concluded is $\sim H$ *or* $\sim A$ where H = hypothesis; A = background assumption; O = observation. A decisive refutation would require a proof that there does not exist an alternative A, say A*, such that $A^* \cdot H$ produces an 'acceptable' observational outcome. Since proofs of the *non*-existence of a suitable A are never available in practice, neither is a decisive or crucial experiment. These themes have been taken up by W. V. O. Quine, "Two dogmas of empiricism", in his *From a logical point of view* (2nd ed., Cambridge, MA, 1964), esp. p. 43. In Pickering's usage a 'closed' experimental system would be one in which all variables were perfectly understood and controlled, and all findings deriving from such a system would command universal assent. An 'open' system would be one which was imperfectly understood, measurements

upon which would be open to a variety of interpretations. Scientists sometimes behave as if their experimental findings should be incontestable, although Pickering doubts whether such a thing as a 'closed' system exists in reality [16, p. 218].

10. An interesting study of Dirac and the monopole concept, providing background to the episode discussed by Pickering, is Helge Kragh, "The concept of the monopole: A historical and analytic case-study", *Studies in history and philosophy of science,* xii (1981), 141-72.

11. *Cf.* Karin Knorr-Cetina, "Relativism—What now?", *Social studies of science,* xii (1982), 133-6.

12. Quite recently there has appeared a programme devoted solely to analysing scientists' 'discourse': Michael Mulkay, "Action and belief or scientific discourse?", *Philosophy of the social sciences,* xi (1981), 163-71; Nigel Gilbert and Michael Mulkay, "Contexts of scientific discourse: Social accounting in experimental papers", in Knorr *et al.*, eds [9], 269-94; and a series of forthcoming papers by Mulkay and Gilbert. This programme is advanced as a way out of a "current analytic impasse" in the descriptive and explanatory project, viz. most of the empirical work discussed in this paper. We should try to analyse *how* scientists talk rather than *what* their talk is about: "It is simply impossible", according to Mulkay, "to produce definitive versions of scientists' actions and beliefs" (p. 169). There are many problems with this position, not least that relating to the claim that the discourse analyst "is no longer required to go beyond the data". It will be for others to judge whether the 'discourse project' should count as a contribution to the sociology of knowledge.

13. Recently, some aspects of Allen's work have been criticized by Jane Maienschein, Ronald Rainger and Keith Benson in *Journal of the history of biology,* xiv (1981), 83-158. Their diverse objections seem to centre upon (i) the rapidity of the shift to experimental techniques (which is not an issue in the present context), and (ii) the extent of polarization between morphological and experimental methods; the dichotomy is accepted by Allen's critics, although they wish to stress the complexity of the situation.

14. In this connection Morrell and Thackray [38, pp. 461-5] provide valuable institutional background to Rudwick's account of the Devonian controversy [22], pointing to the explanatory role of the control of resources in geology. Their study of the British Association for the Advancement of Science also offers institutional considerations relevant to explaining early nineteenth century controversies over wave versus corpuscular theories of light and differing views of the adequacy of mathematical methods in physics [38, pp. 466-84]. In this instance different evaluations were rooted in contrasted Cambridge and Edinburgh pedagogical traditions, as well as in conflicting English and Scottish conceptions of the social and cultural position of science. At the most vulgar level the disputes involved competition for students and alternative schemata for the social support of the man of science. For analyses (mostly pitched at a far less vulgar level) of these episodes: G. N. Cantor, "The reception of the wave theory of light in Britain: A case study illustrating the role of methodology in scientific debate", *Historical studies in the physical sciences,* vi (1975), 109-32, and David P. Miller, "The Royal Society of London, 1800-1835: A study in the cultural politics of scientific organization" (unpubl. Ph.D. thesis, University of Pennsylvania, 1981), ch. 3.

15. These paragraphs refer to the British setting. The sparser literature dealing with France points to a significantly different pattern of cultural connections and

institutionalization obtaining there; see, for example, Dorinda Outram, "Politics and vocation: French science, 1793-1830", *The British journal for the history of science*, xiii (1980), 27-43, also the studies cited in her note 2.

16. For excellent materials on these subjects, see Morrell and Thackray [38, esp. chs 1, 3, and 5].
17. For a perceptive account of the discovery of Uranus: Simon Schaffer, "Uranus and the establishment of Herschel's astronomy", *Journal for the history of astronomy*, xii (1981), 11-26; and, on a related topic, *idem*, "Herschel in Bedlam: Natural history and stellar astronomy", *The British journal for the history of science*, xiii (1980), 211-39.
18. For a discussion of Pannekoek's work in a theoretical sociology of knowledge context: Barry Barnes, *T. S. Kuhn and social science* (London, 1982), 94-101; and for general treatments of similarity-dissimilarity judgments, *idem*, "On the conventional character of knowledge and cognition", *Philosophy of the social sciences*, xi (1981), 303-35; *idem*, "On the extensions of concepts and the growth of knowledge", *Sociological review* (in the press).
19. Imre Lakatos, "History of science and its rational reconstructions", in Y. Elkana, ed., *The interaction between science and philosophy* (Atlantic Highlands, N.J., 1974), 195-241.
20. This brief discussion of 'internal' and 'external' factors overlaps with a more extended account in Barnes, *Scientific knowledge and sociological theory* (ref. 4), ch. 5, but the point regarding their status as actors' categories is still so often forgotten or missed that repetition may be justified.
21. Historians disagree whether such demonstrations may be said to show 'external' influences upon science. Writers like Koyré appear to regard neo-Platonic philosophy as *part of* rational science. Others seem to think of religion and metaphysics as 'external' to science, while preserving a crucial boundary around the domain of 'the intellect' in general. Again, we may take such boundary-placements purely as expressions of historians' evaluations unless the issue concerns where historical actors themselves placed cultural boundaries.
22. Webster [78] generally accepts Basalla's [61] findings while pointing out certain problems arising from the use of mechanical metaphors in Harvey's overall vitalist orientation. There is some criticism of both Basalla and Webster in Howard B. Burchell, "Mechanical and hydraulic analogies in Harvey's discovery of the circulation", *Journal of the history of medicine*, xxxvi (1981), 260-77; Burchell says that contemporary technology played only an illustrative and expository role in Harvey's work, not a 'triggering' role, but it remains unclear how a distinction is made between the language in which discovery is communicated and 'the discovery itself'. For Harvey's use of conceptions of the social order see Hill [85].
23. In a short note Barry Barnes has pointed out some significant analogies between how historians deal with the science-technology relationship and how they might more constructively treat the connections between science and social context: Barnes, "The science-technology relationship: A model and a query", *Social studies of science*, xii (1982), 167-73.
24. Set alongside the voluminous historical literature on the Darwin-Malthus link it is significant that there is only one paper dealing with Darwin's use of the 'extra-scientific' resources provided by the culture of pigeon-fanciers: Secord [76], even though one could argue that the patterns Darwin observed there were at least as important to his theory of selection as the resources of political economy and natural theology. This historiographical distortion does not escape Secord's notice,

and his paper is in every way a model of how to treat the use of cultural resources in making science.

25. C. C. Gillispie, "Comment on Freeman", in D. Freeman, "The evolutionary theories of Charles Darwin and Herbert Spencer", *Current anthropology,* xv (1974), 224; *cf. idem, The edge of objectivity* (Princeton, N.J., 1960), 311, 343; Bowler [62]; Ospovat [71]; and Shapin and Barnes [52]. The last reference treats some seldom-acknowledged problems associated with individualistic approaches to science and its cultural connections.

26. Forman's paper [66] has been widely criticized by word of mouth, but there has been only one sustained effort to reassess its arguments and the evidence for them: John Hendry, "Weimar culture and quantum causality", *History of science,* xviii (1980), 155-80. The gist of Hendry's criticism appears to be that Forman neglects 'internal influences' on the adoption of acausal perspectives and that he exaggerates the extent to which acausality actually was taken up. Only the specialist can properly assess the weight of Hendry's particular objections to Forman, but it would seem highly desirable that some competent scholar should recover the ground and examine the relations between purposes within the subculture of physics and purposes which connected physical thought to the wider society.

27. The cultural cluster including eugenics, biometry and statistics has been something of a *locus classicus* for social historical study in recent years. Only the most sociologically explicit work is discussed in detail, but also see admirable contributions by Bernard Norton, "Karl Pearson and statistics: The social origins of scientific innovation", *Social studies of science,* viii (1978), 3-34; *idem,* "Karl Pearson and the Galtonian tradition: Studies in the rise of quantitative social biology" (unpubl. Ph.D. thesis, University of London, 1978); William B. Provine, *The origins of theoretical population genetics* (Chicago, 1971); Lyndsay Farrall, "Controversy and conflict in science: A case study—the English biometric school and Mendel's laws", *Social studies of science,* v (1975), 269-301; *idem,* "The origins and growth of the English eugenics movement 1865-1925" (unpubl. Ph.D. thesis, University of Indiana, 1970); Charles E. Rosenberg, "The social environment of scientific innovation: Factors in the development of genetics in the United States", in Rosenberg [51, ch. 12]; Garland Allen, "Genetics, eugenics and society: Internalists and externalists in contemporary history of science", *Social studies of science,* vi (1976), 105-22; and Daniel J. Kevles, "Genetics in the United States and Great Britain, 1890-1930: A review with speculations", *Isis,* lxxi (1980), 441-55.

28. Jürgen Habermas, *Knowledge and human interests* (Boston, 1971). See discussions of this perspective in Barnes, *Interests and the growth of knowledge* (see ref. 4), ch. 1, and Shapin [103, pp. 63-65].

29. The contrast between "conservative" and "natural law" styles of thought is set out in Karl Mannheim, "Conservative thought", in *Essays in sociology and social psychology* (London, 1953), 74-164. For empirical studies utilizing Mannheim's categories, see Bibliography, Section VI (a).

30. See some selected references in Bibliography, Section VI (b).

31. It is true that some of the vocabulary Farley and Geison use in their paper invites a psychological reading of their argument: The "influence" of "external factors" upon Pouchet is made to hinge upon his "sincerity" in insisting upon his orthodoxy (p. 184); we are obliged to choose whether Pasteur "allowed 'external' factors" to "influence" him "consciously" or "unconsciously" (pp. 196-7). It would seem, however, that this individualism and psychologism does not sit easily with the main strands of the paper's argument, which is pitched at a sociological level.

Interestingly, a critical assessment of this paper has picked upon the psychologism and exploited its weakness: Nils Roll-Hansen, "Experimental method and spontaneous generation: The controversy between Pasteur and Pouchet, 1859-64", *Journal of the history of medicine*, xxxiv (1979), 273-92.

32. The 'coercive model' (not so labelled) is most explicitly set forth in Laudan, *Progress and its problems* (see ref. 3), ch. 7, where the empirical failures of this approach are given as reasons for rejecting the sociology of knowledge.
33. There are many sources for this line of attack; perhaps the most explicit is A. G. N. Flew, "Is the scientific enterprise self-refuting?", *Proceedings of the Eighth International Conference on the Unity of the Sciences: Los Angeles, 1979* (New York, 1980), i, 347-60.
34. It is remarkable how little attention the 'Great Tradition' in the history of science has actually paid to experimental practice. Two recent major studies go some way to remedying this neglect; both point out how problematic is the connection between that practice and the theoretical culture that has been the major focus of historical interest: R. G. Frank, Jr, *Harvey and the Oxford physiologists: A study of scientific ideas* (Berkeley, 1980), and, especially, John L. Heilbron, *Electricity in the 17th & 18th centuries: A study of early modern physics* (Berkeley, 1979).
35. See, for example, Barnes, *Scientific knowledge and sociological theory* (ref. 4), esp. ch. 1; *idem, Interests and the growth of knowledge* (ref. 4), esp. ch. 1; Bloor, *Knowledge and social imagery* (ref. 4), chs 2, 8; Barry Barnes and David Bloor, "Relativism, rationalism and the sociology of knowledge", in S. Lukes and M. Hollis, eds, *Relativism and rationality* (Oxford, 1982), in the press, and Barnes papers in ref. 18.
36. See Shapin [121] for the notion of actors 'laying bets' on representations of perceived reality. In this episode the actors themselves privileged their preferred representations and provided psychological and sociological explanations of their opponents' 'erroneous' accounts.
37. Harold Garfinkel, *Studies in ethnomethodology* (Englewood Cliffs, N.J., 1967), 66ff.
38. For the 'network model': Mary Hesse, *The structure of scientific inference* (London, 1974); its sociological significance and implications for history of science have been developed in David Bloor, "Klassifikation und Wissenssoziologie: Durkheim und Mauss neu betrachtet", *Kölner Zeitschrift für Soziologie und Sozialpsychologie,* Sonderheft xxii (1980), 20-51 (an English version will shortly be appearing in *Studies in history and philosophy of science* under the title "Durkheim and Mauss revisited: Classification and the sociology of knowledge").
39. Barry Barnes, "Natural rationality: A neglected concept in the social sciences", *Philosophy of the social sciences,* vi (1976), 115-26.
40. Laudan, *Progress and its problems* (ref. 3), 219.

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This bibliography consists almost entirely of empirical work discussed in the text. It is by no means an exhaustive list of relevant studies, but it is inclusive enough to constitute a working bibliography in the historical sociology of scientific knowledge. Doubtless, I have offended many authors, although perhaps the more profound apologies are owed to writers who will be surprised to see their work treated in a sociological context than to those who may (rightly) feel that they ought to have been included.

The Bibliography is arranged into sections closely connected to corresponding sections in the text. For the most part this is a purely conventional categorization of empirical work. Many studies contain material that relates to more than one sociological theme, and perfunctory indication of these overlaps is given at the foot of several sections in the Bibliography. Some wholesale omissions of sociological foci and interpretive themes are pointed out in Section VI of the Bibliography. I have attempted to make this list as current as possible, but given the healthy state of the empirical sociology of knowledge, I fully expect (and hope) that it will very soon be out of date.

I. *Contingency and the sociology of knowledge: observation and experiment*

1. ALICE BAXTER and JOHN FARLEY, "Mendel and meiosis", *Journal of the history of biology*, xii (1979), 137-73.
2. RICHARD BURKHARDT, Jr, "Closing the door on Lord Morton's mare: The rise and fall of telegony", *Studies in history of biology*, iii (1979), 1-21.
3. SANDRA E. BLACK, "Pseudopods and synapses: The amoeboid theories of neuronal mobility and the early formulation of the synapse concept, 1894-1900", *Bulletin of the history of medicine*, lv (1981), 34-58.
4. H. M. COLLINS, "The seven sexes: A study in the sociology of a phenomenon, or the replication of experiments in physics", *Sociology*, ix (1975), 205-24.
5. H. M. COLLINS, "Son of seven sexes: The social destruction of a physical phenomenon", *Social studies of science*, xi (1981), 33-62.
6. H. M. COLLINS, "The place of the 'core set' in modern science: Social contingency with methodological propriety in science", *History of science*, xix (1981), 6-19.
7. H. M. COLLINS and TREVOR PINCH, "The construction of the paranormal: Nothing unscientific is happening", in Roy Wallis, ed., *On the margins of science: The social construction of rejected knowledge*, Sociological review monograph, xxvii (Keele, Staffs, 1979), 237-70.
8. ADRIAN J. DESMOND, "Designing the dinosaur: Richard Owen's response to Robert Edmond Grant", *Isis*, lxx (1979), 224-34.
9. BILL HARVEY, "The effects of social context on the process of scientific investigation: Experimental tests of quantum mechanics", in K. D. Knorr, R. Krohn and R. Whitley, eds, *The social process of scientific investigation* (Sociology of the sciences, iv; Dordrecht, 1980), 139-63.
10. BILL HARVEY, "Plausibility and the evaluation of knowledge: A case-study of experimental quantum mechanics", *Social studies of science*, xi (1981), 95-130.
11. L. S. JACYNA, "John Goodsir and the making of cellular reality", *Journal of the history of biology*, in the press.
12. BRUNO LATOUR and STEVE WOOLGAR, *Laboratory life: The social construction of scientific facts* (Beverly Hills and London, 1979).
13. RUSSELL C. MAULITZ, "Schwann's way: Cells and crystals", *Journal of the history of medicine*, xxvi (1971), 422-37.
14. MARY JO NYE, "N-rays: An episode in the history and psychology of science", *Historical studies in the physical sciences*, xi (1980), 125-56.
15. ANDREW PICKERING, "Constraints on controversy: The case of the magnetic monopole", *Social studies of science*, xi (1981), 63-93.
16. ANDREW PICKERING, "The hunting of the quark", *Isis*, lxxii (1981), 216-36.
17. JOHN V. PICKSTONE, "Globules and coagula: Concepts of tissue formation in the early nineteenth century", *Journal of the history of medicine*, xxviii (1973), 336-56.
18. TREVOR J. PINCH, "Normal explanations of the paranormal: The demarcation problem in parapsychology", *Social studies of science*, ix (1979), 329-48.

19. TREVOR J. PINCH, "The sun-set: The presentation of certainty in scientific life", *Social studies of science*, xi (1981), 131-58.

20. PHILIP F. REHBOCK, "Huxley, Haeckel, and the oceanographers: The case of *Bathybius haeckelii*", *Isis*, lxvi (1975), 504-33.

21. MARTIN J. S. RUDWICK, "Darwin and Glen Roy: A 'great failure' in scientific method?", *Studies in history and philosophy of science*, v (1974), 99-185.

22. MARTIN J. S. RUDWICK, "The Devonian: A system born from conflict", in *The Devonian system*, Special papers in palaeontology, xxiii (London, The Palaeontological Association, 1979), 9-21.

23. NICOLAAS A. RUPKE, "*Bathybius haeckelii* and the psychology of scientific discovery", *Studies in history and philosophy of science*, vii (1976), 53-62.

24. G. D. L. TRAVIS, "On the construction of creativity: The memory transfer phenomenon and the importance of being earnest", in Knorr *et al.*, eds [9], 165-93.

25. G. D. L. TRAVIS, "Replicating replication? Aspects of the social construction of learning in planarian worms", *Social studies of science*, xi (1981), 11-32.

26. MARY P. WINSOR, "Barnacle larvae in the nineteenth century: A case-study in taxonomic theory", *Journal of the history of medicine*, xxiv (1969), 294-309.

27. BRIAN WYNNE, "C. G. Barkla and the J phenomenon: A case-study in the treatment of deviance in physics", *Social studies of science*, vi (1976), 307-47.

28. BRIAN WYNNE, "Between orthodoxy and oblivion: The normalisation of deviance in science", in Wallis, ed. [7], 67-84.

Also relevant are Farley and Geison [116]; Kohler [32]; Lankford [49]; MacKenzie [35, pp. 120-25]; Shapin [121].

II. *Professional vested interests and sociological explanation*

29. GARLAND E. ALLEN, "Hugo de Vries and the reception of the 'Mutation Theory'", *Journal of the history of biology*, ii (1969), 55-87.

30. GARLAND E. ALLEN, "Naturalists and experimentalists: The genotype and the phenotype", *Studies in history of biology*, iii (1979), 179-209.

31. JOHN DEAN, "Controversy over classification: A case study from the history of botany", in Barry Barnes and Steven Shapin, eds, *Natural order: Historical studies of scientific culture* (Beverly Hills and London, 1979), 211-30.

32. ROBERT E. KOHLER, "The reception of Eduard Buchner's discovery of cell-free fermentation", *Journal of the history of biology*, v (1972), 327-53.

33. ROBERT E. KOHLER, "The enzyme theory and the origin of biochemistry", *Isis*, lxiv (1973), 181-96.

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