Contextual Knowledge: A Model for the Overthrow of the Internal/External Dichotomy in Science[1]

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Introduction

Although the scientific enterprise has been examined from a wide variety of perspectives, the body of research can be divided into two categories on the basis of their assumptions with respect to the production of scientific knowledge. The first category includes those studies which take as a central question the production of scientific knowledge (a question which can be studied in isolation) and are concerned to elucidate the history and conditions of its production, and the basis of its validity. This category, which assumes the processes of production and validation of knowledge to be, by and large, isolated from external influences, includes most of the research carried out under the rubric of the history and/or philosophy of science.

The second category of studies accepts the production of scientific knowledge as largely unproblematic, a somewhat mysterious and ineffable process, and concentrates on the operation of the scientific institution; questions of interest include the appropriate forms of education for scientists, the application of scientific knowledge, and the appropriate relationships with government necessary to ensure adequate financial support. This category comprises, in general, a more recent tradition, and includes such diverse and uncertainly identified areas of study as 'science and government', 'science policy', the 'economics of research', the 'relationship between science and technology', 'social responsibility in science' and 'communication processes in science'.

Not only may these two categories of research on science be distinguished, but work in each proceeds, by and large, independently of the other. In other words, the findings of those studying the production of scientific knowledge are not normally reflected in studies of the social operation and impact of scientific institutions, and *vice versa*.

I wish to argue that this division is both artificial and unproductive, and that its continuance represents a major barrier to progress in understanding what has become one of the major forces shaping industrial society. The concepts and modes of analysis used in the study of scientific institutions are themselves called into question by theoretical advances in fields concerned with the production of scientific knowledge. On the other hand, research which is addressed to the production of scientific knowledge as an isolated process is unable to grapple with such contemporary problems as the scale of national resources which should be devoted to the scientific enterprise, the responsibility of the scientist (as an individual and member of a specialist community) for research results, and the control of both intended and unintended consequences of research.

In order to develop this argument, it will be necessary first to demonstrate that the classification outlined above rests on an analytical dichotomy which pervades the study of science—that between 'internal' and 'external' explanations.

The Internal/External Dichotomy in Science

The origins of the internal/external dichotomy, or at least the general awareness of it, can be traced to a long-standing debate within the history of science between protagonists who have claimed exclusive explanatory authority for internal, or external factors, respectively. This debate has been particularly concerned with explaining the emergence of modern science which, according to the 'internalists', resulted from the struggle between novelty, tradition, alteration and evolution which characterises the emergence of revolutionary ideas in any intellectual enterprise, and which, for the 'externalists', resulted from the operation of special social, economic and religious forces.

The externalist argument is associated with Hessen (1971: 151-176) and Needham (1943: 144-145), the latter claiming that—

> We cannot dissociate scientific advances from the technical needs and processes of the time, and the economic structure in which all are embedded. . . . The history of science is not a mere succession of inexplicable geniuses, direct Promethean ambassadors to man from heaven. Whether a given fact would have got itself discovered by some other person than the historical discoverer had he not lived it is certainly profitless and probably meaningless to enquire. But scientific men, as Bukharin said, do not live in a vacuum; on the contrary, the directions of their interests are ever conditioned by the structure of the world they live in.

The externalist approach was taken up by Robert Merton in his study of the links between Puritanism and the form of science in seventeenth-century England (1970: 155), in which he noted that—

> the relation between science and economic needs is seen to be twofold: direct, in the sense that certain scientific research is advisedly and deliberately pursued for utililitarian ends, and indirect, in so far as certain subjects, because of their technological importance, are sufficiently emphasized to be selected for study though the scientists are not necessarily cognizant of their practical significance.

This view of science subsequently attracted relatively few adherents among historians, though it became enshrined in the 'Planned Science' movement of the 1940's[2] and has enjoyed a recent revival.[3]

The externalist position draws its theoretical justification from Marxist historical materialism, in which intellectual work in science is seen as a direct and relatively straightforward superstructural expression of the socio-economic base in a given society. In other words, the explanation of scientific advances can be traced to the technical needs and processes of the time, and the economic structure in which all are embedded.

A statement of the internalist position with regard to the scientific revolution is offered by Nef (1958: 63):

> During these times of decisive change in rational procedures it was the mind itself, not economic institutions nor economic development, which called the new tunes and composed most of the variations which the greatest scientists were playing on them. The revolutionary scientific discoveries by Gilbert, Harvey, Galileo and Kepler, like the new mathematics of Descartes, Desargues, Fermat and Pascal, were of no immediate practical use. Freedom, rather than necessity, was the principal power behind the scientific revolution.

In the context of an assessment of the Mertonian thesis,[4] Hall (1963) allows that science does not exist in a cultural vacuum, that its activities may be facilitated by technological resources, and that its problems may be drawn from the general context of the surrounding culture, but argues that this has no significance. Science is practised within a social and economic setting, but that setting influences neither the rate nor the direction of scientific thought:

> Social and economic relations are rather concerned with the scientific movement than with science as a system of knowledge of nature (theoretical and practical); they help to understand the public face of science and the public reaction to scientists; to evaluate the propaganda that scientists distribute about themselves, and occasionally—but only occasionally—to see why the subject of scientific discussion takes a new turn. But to understand the true contemporary significance of some piece of work in science, to explore its antecedents and effects, in other words to recreate critically the true historical situation, for this we must treat science as intellectual history, even experimental science (Hall, 1963: 13-14).

The theoretical justification for the internalist argument rests on a view of the nature of science which derives from the philosophy of science. Agassi (1963: 1) has shown how particular stances in the philosophy of science have generated distinctive kinds of history of science. One of these approaches he characterises as 'inductivism' according to which nature, when investigated with a properly open mind, reveals an ever-increasing body of hard facts, which can be progressively better understood by means of cautious and tentative generalisations and theories. With this view of science, the historian's task becomes quite obviously one of reconstructing the 'discovery' of the hard facts. By and large, among historians, the inductivist view of science has been replaced by a 'conventionalist' argument, associated with Popper (1959) and Lakatos

(1970: 91; 1971: 91), according to which certain canons of rationality can be recognised even in long out-moded conceptual systems so that rational reconstruction can be carried out by a process of 'thinking men's thoughts after them'. However, this somewhat more sophisticated view of science still presupposes that the resulting history of science is relatively autonomous, i.e., that it can be described independently of the social and political environment of science and of the social histories of particular scientific researchers.

An intermediate position commonly encountered among historians acknowledges that both internal and external factors must be taken into account in the explanation of scientific development,[5] but argues that the different factors explain distinct elements of the scientific process. Thus, the form and content of scientific knowledge are seen as a strict consequence of theoretical development which has as its goal the mirroring of reality, but it is accepted that external factors may have a 'directive' influence (Barnes, 1974: 102), by stimulating, retarding, or to some extent shaping the direction of scientific change. Thus the level of funding and the availability of instrumentation are accepted, in part, as determinants of the rate of 'exploitation' of a particular field. Also, public interest or economic need can lead to more attention being directed to one field than another, though this is seen as having no effect on the overall progress of science.

This 'directive' model of external influence rests on a 'spatial' metaphor of scientific development. Problem areas are implicitly considered to be finitely fillable spaces with an essentially pre-determined content of theories and observations.[6] Thus the level of funding support can only advance or retard the 'uncovering' of a particular area. Likewise, external factors can direct the interest of scientists to the uncovering of one area of knowledge rather than another.

It is not my intention to continue this distinction by classifying all research on science into an internalist or externalist camp, since this would be a sterile exercise. Rather, I wish to argue that this dichotomy has pervaded the study of science by effectively defining the range of questions which may be asked, the lines of demarcation between fields, and the appropriate methodology of research. This situation is reflected in Kuhn's comment that '. . . the internal-external debate is one which historians have lived with rather than studied' (Kuhn, 1971: 271), but it applies to all the disciplines which have been focused on science, not simply historical enquiry.

Numerous examples can be offered. Thus, excellent studies of the growing politicisation of science (Haberer, 1969; Ezrahi, 1971: 117) examine a wide range of social consequences of this process, but do not see this process as having more than a 'directive' effect on the knowledge produced, i.e., it leads only to either a more or less rapid 'uncovering' of particular areas of knowledge. Similarly the various prescriptions for science policy[7] have been based on the assumption that by channeling resources, both financial and manpower, into selected areas, the ineffable scientific process will automatically produce the desired results, or at least resolve the problems of interest. Studies of research from an economic viewpoint have regarded the production of knowledge as an exogenous process, a 'black box' which can be unproblematically incorporated into input-output analyses. The possibility that economic forces might actually determine the knowledge content has been disregarded.

The Internal/External Dichotomy in the Sociology of Science

Two figures have been particularly influential in the sociology of science—Robert K. Merton and Thomas S. Kuhn. Merton laid the foundation for the investigation of science as a social institution, and until this decade his normative approach focused research on reward allocation procedures among scientists, communication structures of scientific groups and the social organisation of science.[8]

Since that time some sociologists, predominantly European, have seized on the possibility offered by Kuhn's (1970) model of scientific innovation to develop a distinct approach to the analysis of the production and validation of scientific knowledge.[9] Thus it is instructive to examine the theories of Merton and Kuhn for the assumptions they make with respect to the autonomy of science.

Mertonian Sociology of Science—As we have already noted, Merton has been the subject of attack from Hall (1963) on the grounds of his acceptance of the economic determination of scientific theories. If we accepted Hall's criticism at face-value, we might expect Mertonian sociology of science to be concerned with an analysis of the cultural factors determining the form of scientific knowledge. In practice however, it has concerned itself almost exclusively with the social analysis of the scientific institution, accepting that the production of knowledge is not itself susceptible to sociological analysis. How can this be explained, when Merton regards his own work to be '. . . in the tradition of the sociology of knowledge, concerned with the relations between the social origins, the substance and the role of ideas' (Morton, 1968: 585)?

The clue lies in the implicit epistemological assumptions about the states of scientific knowledge. As King (1971: 3) has argued so forcibly, Merton's sociology of science rests on an extended analogy between scientific research and economic activity which assumes a 'positivistic' view of science:

> thus it treats science as 'work', scientists as workers, and scientific ideas as commodities or as 'products' of scientific research. Accordingly, it sees the social system of science as a system for the production and dissemination of scientific ideas, and it postulates the virtual bifurcation of scientific 'product' and the processes of scientific 'production', of scientific ideas and the concrete practices which give rise to them. By thus treating as extrinsic the connection between the practice of science and the knowledge it produces, sociologists have been able to undertake a sociological interpretation of the former without concerning themselves with the question of the social determination of the latter (King, 1971:4-5).

Indeed, under a positivist view, there is nothing about scientific knowledge for the sociologist to explain since:

> If in practice scientists advance more than one answer to a question, then the choice between them is a fully rational matter, for the relative merits of the rival solutions can be reasoned against fixed, impersonal standards of judgment. So, from this view, scientific authority is simply the authority of truth over error. Scientific choice is choice between theories in terms of their ascertainable truth-content. Scientific consensus forms automatically, as scientists ally themselves to the theories that are demonstrably 'truer' than their rivals (King, 1971: 15).

Thus Hall's charge that '. . . social and economic relations are rather concerned with the scientific movement than with science as a system of knowledge of nature' (1963: 12), is substantiated in the case of the Mertonian sociology of science. Knowledge is generated by an autonomous intellectual tradition unproblematically reflecting reality, with external factors limited to a directive role, i.e., externally imposed goals are regarded as either supporting, or undermining, internal norms. This view has led to the well-known 'value-clash' hypothesis[10] and to the argument concerning the congeniality of liberal democracies to scientific progress.[11]

Kuhnian Sociology of Science—Kuhn, as a historian, has shown himself well aware of the problems raised by the internal/external dichotomy in the history of science (1968, 1971). However, following this lucid analysis of the problems raised by the dichotomy, his theory of scientific change (1970) returns to the traditional form of the debate and stresses the pre-eminence of internal over external factors. For Kuhn, it is the accumulation of anomalies, in the context of the internal logic of the scientific community, which is the decisive factor in paradigm shift. Only in the early stages of development of a new field are 'cultural' external factors, 'the ambient intellectual milieu' (1971: 280) allowed a role, i.e., in the acquisition of the first paradigm. Thereafter,

> The practitioners of a mature science are men trained in a sophisticated body of traditional theory and of instrumental, mathematical and verbal technique. As a result they constitute a special subculture, one whose members are the exclusive audience for, and judges of, each others work. The problems on which such specialists work are no longer presented by the external society but by an internal challenge to increase the scope and precision of the fit between existing theory and nature. And the concepts used to resolve these problems are normally close relatives of those supplied by prior training for the speciality. In short, compared with other professional and creative pursuits, the practitioners of a mature science are effectively insulated from the cultural milieu in which they live their extra-professional lives (Kuhn, 1968:81).

This insulation operates primarily with respect to concepts, and secondarily with respect to problem structure, but other aspects of scientific advance are considered to depend on factors emphasised by the externalist approach. Thus:

> Both the attraction of science as a career and the differential appeal of different fields are, for example, significantly conditioned by factors external to science. Furthermore, since progress in one field is sometimes dependent on the prior development of another, differential growth rates may affect an entire evolutionary pattern. . . . In addition a new technology or some other change in the conditions of society may selectively

alter the felt importance of a specialty's problems or even create new ones for it. By doing so they may sometimes accelerate the discovery of areas in which an established theory ought to work but does not, thereby hastening its rejection and replacement by a new one. Occasionally, they may even shape the substance of that new theory by ensuring that the crisis to which it responds occurs in one problem area rather than another (Kuhn, 1968:81).

In this passage Kuhn comes closest to transcending the internal/external dichotomy altogether. However, having developed a role for the social milieu in scientific development, he retreats by somewhat arbitrarily defining the community of scientists as effectively constituting that milieu.

The assumption that internal and external factors account for different aspects of scientific development is embedded in the neo-Kuhnian sociology of science, which seeks to link the development and operation of social institutions within science with that of the cognitive structures produced by them.[12] Considerable progress has been made in explaining the emergence of a variety of new specialties in terms of the interaction of social and cognitive processes,[13] where cognitive structures are viewed as sources of authority which direct the production and control of what is to be accepted as scientific knowledge.[14] However, these explanations have been confined within the apparent boundaries of the emergent science, with the occasional implicit acknowledgement of the directive effect of factors such as the level of student recruitment and availability of instrumentation.

Thus the sociology of science, whether based on the theories of Merton, or those of Kuhn, has been essentially limited by the internal/external dichotomy. By allowing (though for different reasons) external factors to have only a directive role, the distinction between the 'areas of competence' of the internalist and externalist approaches has been upheld. There has been no consideration of the possibility of mutual interpenetration between the two approaches and their subject matter, let alone of the possibility that they may actually be constitutive of each other.

The Validity of the Internal/External Dichotomy

In view of the pervasiveness of the internal/external dichotomy, it is obviously of some importance to examine its validity. An explanation could be made in sociological terms on the basis of the prior establishment, and authority, of the history and philosophy of science. In other words, the assumptions enshrined in these two fields have unquestionably been adopted by all subsequent analysts of science. While there may be some truth in such an account, it is necessary to penetrate rather deeper to examine the assumptions behind, the strengths and the weaknesses of, and the consequences of accepting, the dichotomy.

In examining the origins of the dichotomy among historians of science, and Merton's adoption of it in the sociology of science, I have already suggested that it is rooted in an essentially positivist conception of scientific knowledge as offering a true and direct account of reality. A consequence of this view is that the existence and distribution of scientific knowledge is readily explained, or, to be more precise, is not in need of explanation at all. What counts as knowledge is what is true. Under these assumptions, a sociology of knowledge which seeks to account for the content of knowledge by social causation is, *a priori*, irrelevant to an understanding of scientific knowledge systems.

However, this view of scientific knowledge has been seriously challenged. The range of arguments involved is extensive, but a list of some of the more important objections will suffice for our purposes. Firstly, on epistemological grounds, findings in the psychology of perception and in anthropology cast doubt on the unmediated nature of sensory inputs.[15] Since Hume, philosophers have been unable to demonstrate a logical basis for the inductive process of generalisation from empirical particulars. If factual statements are to be regarded as providing a universally acceptable base for the construction of theories, there must exist a special, theory-independent set of categories of experience. However, as Hesse (1970) has shown, there is no independent observation language, and hence to some extent all facts are theory-laden. Indeed, most terms used in science have a decidedly theoretical, non-empirical character,[16] and most knowledge can be shown to derive from theories rather than being the product of pure experience. This non-empirical character of knowledge is emphasised by the demonstration of the role of metaphor and analogy in explanation.[17]

Secondly, studies of the everyday practice of scientists do not support a positivist conception of knowledge. The sociological tradition initiated by Kuhn (1970) places great emphasis on a consensual basis of scientific knowledge. According to this tradition, scientists employ socially defined cognitive tools, ranging from personal theoretical constructs to the theories generated by previous phases of the scientific work process and objectified in the form of technical apparatus and publications, to produce theoretical representations of reality acceptable to their peers.[18]

Thirdly, there does not appear to be an unproblematic boundary separating science from other elements of a culture. For example, in a detailed examination of Darwin's development and defence of his theories, Robert Young (1969: 109; 1971: 442; 1973: 344) argues that Darwin's own position is unintelligible without incorporating his dogmatic belief in uniformitarianism. This argument casts doubt on the standard internalist account which emphasises the distinction between the genuine science of Darwin and the less reputable thought of others. Young emphasises the common context of nineteenth-century thought about man's place in nature, and the inter-connectedness of science, social thought and natural theology at this particular time. More generally, Barnes (1974: 121) notes that 'As one passes back into the history of science one finds it (or, if preferred, its cultural antecedents) less and less differentiated from the general culture'.

Fourthly, there is some doubt about the adequacy of describing the goal of the pursuit of truth as covering all activities that are carried out under the name of science. Authors such as Sklair (1973), Cotgrove and Box (1970), Ellis (1969) and Blume (1974) have criticised research programmes that deal only with academic scientists (whose goals might be considered the pursuit of truth), on the grounds that they comprise a relatively small and a-typically autonomous element in the total scientific community. Today, most scientists work in industrial or government settings. In addition, much research within universities is directed to applied or utilitarian goals. For example, an increasing number of specialties are explicitly oriented to a particular externally-determined goal.[19] Examples include cancer research, industrial research areas such as tribology and terotechnology, urban planning and environmental science. Furthermore, much research in the traditional 'pure' disciplines such as physics and chemistry is sponsored by industrial, political and military sources. In this situation, the validity of the view of science as the isolated pursuit of truth, and hence of the internal/external dichotomy, is seriously challenged.

In the face of the weight of argument and evidence disputing the basis of the internal/external dichotomy, its continued hold is in need of some explanation. Firstly, philosophical arguments notwithstanding, the apparent 'solidity' of scientific knowledge in practical application has precluded its consideration as a socially mediated product. As Collins argues:

> it is as though epistemologists (and sociologists!) are (and for the sake of argument, 'were') concerned with the characteristics of ships (knowledge) in bottles (validity) while living in a world where all ships are already in bottles with the pine dried and the strings cut. A ship *within* a bottle is a natural object in this world, and because there is no way to reverse the process, it is not easy to accept that the ship was ever just a bundle of sticks (Collins, 1975:1).

Secondly, and I would argue more importantly, the assumptions underlying the internal/external dichotomy have played an important role in maintaining the public image of the scientific institution as an autonomous producer of objective knowledge. The very power of scientific knowledge and the authority of the community of producers has rested on their ability to maintain a consensus and produce an exclusive form of knowledge, apparently dissociated from the interests of any particular power group in society. Thus, as Ezrahi has argued:

> scientists have tended to be much more sympathetic to those theories in the sociology and history of science which perceived scientists as obeying the inherent imperatives of scientific ideas and the logic of inquiry. The theory that scientists follow only the internal rules of science would seem to reinforce their effort to prevent the subordination of their work to standards extrinsic to science and to protect themselves from external political influence (1971: 117).

By the same token, scientists have been unwilling to accept that the knowledge they produce may be determined by its social context because such an admission could threaten their claim to political neutrality and freedom from external control. Indeed, the typical response when any such argument has been advanced is an outcry against 'Lysenkoism'[20] and the embodied dangers of censorship and political direction.

In the light of the various advances which dispute the theoretical grounds of the internal/external dichotomy, and of the political basis of its appeal to scientists, it would appear necessary to challenge the abstract opposition between internal analysis, which recreates the logic by which specific problems are solved within science, and external analysis, which relates those problems to the social conditions of their attaining 'visibility'.

It is not a question of replacing the internalist, with or without directive effects from outside, with a more complete externalist approach. The need is to develop an approach which will transcend this dichotomy completely and provide new insight into the process by which scientific knowledge is shaped.

A Contextual Model of Scientific Development

In this section I wish to develop a preliminary sketch of a contextual model of scientific development which may succeed in transcending the restriction of intellectual and social closure by taking full account of the extent to which scientific knowledge is a social construct.

All modern scientific practice can be regarded as directed activity, i.e., science is oriented towards goals which may be directly or indirectly perceived, which are expressed at varying levels of generality, which are developed and transformed by social processes and which are dynamically linked to scientific practice and the state of knowledge in such a way that the goals and science are only analytically separable.[21] Although the current research on specialty emergence reveals an awareness of the need to demonstrate the ways in which social processes operate through and upon cognitive concerns, explanations in terms of perceived relative potential for exploitation, based on the information - recognition exchange model,[22] fail to account adequately for the 'purposive' nature of scientific work.[23] There is a need to explore the full range of socially mediated forces which determine the form and timing of emergence of a particular specialty.

It is important that the notion of goal-direction is not misconstrued as representing yet another example of external factors constraining scientific progress. According to this model, science is considered to have no independently defined boundaries and hence no 'outside' or 'inside' at all. To the extent that goals which guide and condition scientific work are themselves determined by the state of cognitive development and orientation, they are part of the research process themselves; labelling these goals as external would be as inappropriate as describing the consensual standards whereby scientific work is validated as 'outside' science.

Of course, all work is directed in some way, but socio-economic direction does not necessarily follow from the inherent purposiveness of research consequent on the social basis of construction of scientific knowledge. To develop this notion of goal-directed scientific activity, there is a need for a theory of mediation between socio-economic and cognitive aspects of the social reality which constitutes and conditions science. Such a theory must allow for a dynamic process of goal formulation which can be conceived of as occurring in the medium of interpenetrating systems of scientific and social production such that the goals are, potentially, continually redefinable in terms of existing theory, technique, socio-economic condition and congeniality with prevailing social thought.

Young has attempted to develop an analysis along these lines. He dismisses the Kuhnian approach as, ultimately, a mystification (Young, 1973: 411), but at the same time he rejects the Marxist 'base-superstructure' model because of its easy vulgarisation into a crude form of economic determinism. Instead, he argues that science should be treated in a fully contextualist way with understanding being sought initially from the point of view of the actors involved. The subject of examination, be they theories or debates, can then be related to the economic base, not in any one-to-one way, but by a subtle and complex theory of mediations. Such a contextualist theory will replace 'the crude polarity of idealism *versus* materialism (with) a genuinely totalising, relational theory of man, nature and society' (Young, 1973: 436). Unfortunately, little explicit indication is given of the form such a theory might take.[24]

Barnes also argues for a contextual analysis based on the perceptions of the actors involved (Barnes, 1974: 116), and an example of the operation of contextual mediation is offered by Bourdieu:

> For example, the present-day struggle between different specialists for research grants and facilities can never be reduced to a simple struggle for strictly 'political' power: in the social sciences, those who in the USA have reached the top of the great scientific bureaucracies (such as the Columbia Bureau of Applied Social Research) cannot force others to recognise their victory as the victory of science unless they are also capable of imposing a definition of science implying that

genuine science requires the use of a great scientific bureaucracy and abundant manpower; and they present the procedures of large-sample surveys, the operations of statistical analysis of data, and formalisation of the results, as universal and eternal methodology, thereby setting up as the measure of all scientific practice the standard most favourable to their personal or institutional capacities (1975:21).

Other illustrations of this approach include Forman's (1971: 1) demonstration of the relation between the emergence of quantum physics and the hostility to strict determinism in Weimar culture, Young's (1969: 109; 1971: 442; 1973: 344) work on the social basis of Darwin's evolutionary theory and Cowan's (1972: 509) tracing of the mathematical and biological theories and techniques of Francis Galton to his eugenic concerns. With regard to more contemporary science, Slack (1972: 202) has argued that the concentration on the atypical single-product high yield chemical reaction in academic organic chemistry is a response to the need for profitability of chemically-based industries, and Robbins and Johnston (1976) have analysed the role of occupational values in determining the development of theory and technique.

Though Barnes proposes a fully contextual analysis of scientific practice, he argues that it is precisely on the basis of the individual scientist's perceptions that boundaries may be drawn between science and society and between the specialties which constitute science. While one can concede that scientists **do** perceive that there are boundaries to their specialties, and to science, Barnes' ready acceptance of the existence of specialties as 'clearly demarcated sub-cultures' (1974: 121) appears to rest on the normative tradition within the sociology of science which takes as given a high degree of cognitive, and consequently social, consensus.

However, if we accept that knowledge is socially constructed, then the degree of conceptual coherence in any particular field is not *a priori* established; it may vary from low to high, and is a matter for empirical investigation. By the same token, the existence and 'height' of boundaries between individual specialties and between specialties and the wider society is in no way predetermined. Considering specialty formation, a useful model of the first stages rests on the 'discovery' by scientists of a new 'area of ignorance' (Mulkay, 1975: 509) which, for a variety of reasons, appears worth the investment of personal resources. The 'interest' of any specialty will depend on an array of factors, including its relation to the present field of the researcher, its apparent exploitability with current techniques, prospects of funding, and the social and political visibility of the problem involved. The nature and extent of this 'interest' is another issue to be settled by appeal to empirical evidence.

Success in exploiting a new area depends on the emergence of a recognised and accepted cognitive order, including elements of theory, techniques, instrumentation and problem definition. In the process of negotiating this order, the scientists, either explicitly or implicitly, define 'in' certain areas of socially mediated reality and at the same time define other areas 'out'. In other words, a process of definition of cognitive interests determines the demarcation of the new area from other specialties. The erection of boundaries also serves the function of demonstrating cognitive authority over a range of phenomena. Hill (1973), for example, has shown that the social legitimation of any particular scientific field rests on the maintenance of internal mechanical solidarity built around a shared meaning system. Johnston and Robbins (1977) have questioned the maintenance of such mechanical solidarity under conditions when the goals of the specialty are largely determined outside the scientific community. Thus while the erection of an appropriate set of boundaries contributes to the cognitive authority of the specialty, these boundaries are not by any means static. They are in a continual process of negotiation as new problem areas are exploited, developments in other fields impinge, and competition emerges between specialties for explanatory authority over a particular phenomenon.

At every stage, scientists are attempting to demarcate their specialty, and in doing so, they are engaged in a continual and changing process of negotiation with mediated physical and social reality. It is hardly surprising that the crude internalist/externalist dichotomy, and the studies of science which accepted its limitations, have not been able to develop major explanatory programmes. The development of adequate models of the mediation process will require much more detailed empirical investigation unhindered by assumptions based on the dichotomy.

Conclusion

The potential impact of the overthrow of the internal/external dichotomy on the history and sociology of science is obviously considerable. However, the overthrow may have more immediate importance for the formation of policy governing science and technology, the research and practice of which have largely assumed an autonomous 'dichotomy' model of science.

The underlying model for science policy organisation is based on the 'transaction' concept drawn from political science; its aim is the establishment of effective institutionalised transaction processes between an independent science institution and society, via society's representatives in government. In this model there is no mechanism permitting society's interest to operate on the scientific institution, and analysis, planning and one might add, responsibility, is limited to the areas of **application** of scientific knowledge. By the same token, the coupling of the scientist's motivation with wider goals, as in an industrial or governmental environment, or in special circumstances in a university, is assumed to hold no implications for the quality and integrity of science.

Adoption of a thoroughly contextualist model of science will allow a much broader scope for examination of important issues in science policy. For example, it may be possible to demonstrate that different sorts of policy initiatives are appropriate for sciences differing in type of phenomena selected for study, as in the distinction between 'restricted' and 'unrestricted' sciences (Pantin, 1968). The programme of research on 'finalisation',[25] examining the process through which external goals for science become the guidelines of the development of scientific theory, may indicate another fruitful line of development.

It is apparent that policy issues can be construed as leading to various different research approaches and that up to now the decision has rested on the scientific-political authority of a particular field. Research based on the contextual model should indicate, on the one hand, the range of assumptions built in to any particular existing field (and hence its ability to respond to specific goals), and on the other, a potential orientation for a new field.

FOOTNOTES

1. Some of the ideas contained in this paper were developed in a series of seminars during summer, 1975. I would like to acknowledge the valuable contributions of Angela Ellis, Chummer Farina, Brendan Gillespie and Tom Jagtenberg.
2. The clearest statements of the Planned Science movement are by Bernal (1949;1960): the programme of the opposing 'Freedom in Science' movement is provided by Baker (1945) and Polanyi (1951).
3. Robert Young (1973:344) has been campaigning for the development of a more sophisticated Marxist history of science; for a valuable assessment of Marxist developments in science studies see **Radical Science Journal Collective** (1975:65).
4. The Mertonian thesis has been the subject of considerable debate among historians of science. See for example, Basalla (1968), for extracts and an annotated bibliography on the subject.
5. See for example Lilley (1953:59) and Thackray (1970:112) who both argue for a combination of the two approaches which will avoid the extremes of both pure history of ideas and vulgar Marxism.
6. Law and Barnes (1976:115) similarly criticise Mulkay's (1975:509) reliance on this metaphor in describing models of scientific development.
7. See for example Polanyi (1962:54), OECD (1964), Brooks (1968).
8. The Mertonian tradition of research and its limitations have been extensively analysed in Barnes and Dolby (1970:3), King (1971:3), Martins (1972), and Whitley (1972:61).
9. By denying the inevitability of scientific progress through application of the scientific method, and hence allowing for a degree of epistemological irrationalism, Kuhn opened up the possibility of a sociological study of scientific ideas; furthermore his concept of the paradigm provided sociologists with a framework for linking cognitive and social aspects in the analysis of the development of knowledge.
10. The argument that the norms of science are such that scientists will encounter disorientation in an industrial environment was developed by Kornhauser (1963) and Marcson (1960), and attacked by Cotgrove and Box (1970) and Ellis (1969:33).
11. Merton, and at greater length his student Barber (1962), argue that only in a democratic society can science operate properly, for it is only in such a society that there will be sufficient freedom for the scientific norms to be held.
12. This approach has been developed in Whitley (1972), and Barnes (1974); the progress can be assessed from the Proceedings of a conference on the Sociology of Science, held at York University, in September 1975; a number of the papers have appeared in **Social Studies in Science**, Vol. 6, 1976, No's. 3/4.
13. Progress in the analysis of the emergence of specialties is reviewed in Edge and Mulkay (1975), Mulkay **et al.** (1975:187) and Griffith and Mullins (1972:959).
14. An analysis of the role of cognitive structures as sources of authority has been made by King (1971:3) and Bourdieu (1975:19).
15. Arguments and evidence demonstrating the mediated nature of sensory inputs include, for example, Hanson (1958) and Kuhn (1970) in a scientific context and, more generally Gregory (1966), and Segall (1966).
16. The non-empirical character of terms used in science is illustrated by Barnes (1974:10): 'Take the term "light" for example. It is clear that to ask the question "what is light?" is to expect a non-empirical answer for it is impossible to see light, on present common-sense or scientific uses of the term. As to light "travelling", this is even more obviously a move into the realms of theory'.
17. The metaphorical basis of scientific explanation is outlined in Hesse (1966), Schon (1963), and Bloor (1976).
18. It is important to recognise that the recognition of science as a socially mediated activity in no way devalues the efficacy of the knowledge thereby produced. Rather it