THE CONTEXT OF SCIENTIFIC INVESTIGATION

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Introduction

Much of the literature surveyed by Roger Krohn in his draft (1) that served as the theme paper for this volume deals with instances of scientific investigation which resulted in major discoveries. The basic concern of most studies of scientific – and other – forms of creativity has been to understand how certain major advances in understanding come about. Generally the cognitive success, and indeed the nature, of the innovation has not been questioned or seen as socially problematic. Rather, attention has tended to be focussed on the psychological dimensions of invention while the social processes by which discoveries came to be seen as such and culturally legitimated have rarely been examined. Similarly, the pattern of social organisation of scientific work has usually been taken as irrelevant to scientific creativity. Discoveries have been assumed to be self evident and interest restricted to how particular individuals came to find them.

Furthermore, this literature usually only deals with developments that have come to be seen as major discoveries, creativity and research are tacitly assumed to be associated – or at any rate only interestingly so – with great success in the sciences. The study of scientific investigation is here reduced to the study of how a small number of unusual individuals developed major innovations in certain fields, creativity leading to "failure" is rarely explored and the bulk of research conducted by scientists is ignored. By focussing almost entirely on a small number of abnormal and infrequent psychological processes, most of the literature on processes of research and scientific creativity treats the intellectual and institutional context of scientific investigation as irrelevant to its understanding and hence conceives it as being essentially asocial and ahistorical (2).

Karin D. Knorr, Roger Krohn and Richard Whitley (eds.), The Social Process of Scientific Investigation. Sociology of the Sciences, Volume IV, 1980. 297–321.

In direct contrast to this literature, the empirical papers in this volume largely address themselves to what, since Kuhn, we have come to regard as normal science. The relatively routine, everyday activities of people termed scientists are the focus of most of the case studies reported here. The glamorous, Nobel prize winning research is by-passed to deal with the *ad hoc* contingencies faced by journeymen scientists in their pursuit of "profit" and recognition. Additionally, the main interest of many of these studies is on how results come to be accepted as such – or rejected – and how accomplishments are negotiated rather than with how great ideas were developed by particular individuals. It is the social process of making judgements and resolving disputes which claims the attention of most of the authors here. They are concerned to highlight the socially contingent nature of scientific activity and its consequences rather than to assume that knowledge can be taken for granted as the outcome of the work of a few gifted individuals.

This contrast in approaches to the study of scientific development is partly a consequence of changes in assumptions and presuppositions underlying the social study of science as briefly outlined by Woolgar (3). It is also, though, a consequence of major changes in the way science – or what is seen as science – is conducted and organised. Wherever the professionalisation of scientific work is seen as originating (4), there is little doubt that the scientific labour force has expanded considerably since the second world war and that while much of this expansion has occurred in state and business laboratories it has also occurred in what is traditionally termed 'pure' science. Furthermore, the development of applied science on a large scale cannot be treated as irrelevant to the development of scientific knowledge in the universities and academies, partly because it provides a labour market for the expanded Ph.D programmes and also because the priorities institutionalised in such non-academic organisations affect work done in 'pure' fields as has been shown by several recent papers (5). The boundaries between academic science, industrial science and governments have become less clear and obvious than they were which has led some observers to talk of the deinstitutionalisation of science (6). Scientific work has become an activity carried out by large numbers of people in a variety of institutional settings. Each of these has different objectives, but they all share the common property of organising science. Whatever the realities of science before the Second World War, there is little doubt that to view contemporary scientific work as an activity pursued by independent scholars in the interest of truth would be a gross misapprehension. Science today is a highly general umbrella term which covers a vast range of activities conducted by a large number of qualified personnel in a variety of work organisations for a variety of purposes. To see it as the work of a small number of geniuses acting in isolation would be myopic.

It could, of course, be argued, as some philosophers have done, that 'real' science is still that set of activities, and resultant knowledge, carried out by a few elite scientists in relatively autonomous institutions while 'normal' science is not really science at all. Aside from the obvious problem of deciding where the boundaries between real and unreal sciences are to be drawn, and on what basis, it is by no means clear that the day to day activities of the elite are much less mundane than those of other scientists, nor that they are much less likely to occur in highly structured work organisations. The development of large teams of experimental scientists, extensive division of tasks and full time research administrator roles in the highly recondite field of particle physics (7) suggests that "real" science is organised science rather than the work of a few intellectuals putting basic questions to nature's test. To reduce contemporary science to the activities of a small, cohesive and largely self generating scholarly community of equals is to pay nostalgic respect to past ideals rather than to undertake a serious analysis of scientific development.

Given the generality and diffuseness of activities covered by the generic term "science", and the range of organisations in which such activities are carried out, it is not surprising that the question of how scientific investigation differs from other forms of investigation – and indeed what, if any, are the unique qualities of science as a form of understanding – is not directly addressed in the studies published here. Concerned rather to demonstrate the everyday nature of scientific work than to identify its unique character, these case histories highlight the contingent aspects of scientists' judgements and decisions, their dependence upon organisational and resource constraints and the pervasive influence of competition for recognition and rewards in the development of scientific 'careers'. Indeed the occupational label 'scientist' could be reduced to a prestigious title sought by members of that occupation as a means of legitimating organisational autonomy and monopolisation of knowledge production by a privileged group, just as some see the term 'professional' as a disguise for a successful middle class trade union. Science has become desacrelised and returned to the world of profane motives and activities. Or, rather, science as an ideal standard of cognitive worth and basis for making judgements has become so generalised throughout industrial societies that its connections to scientific practices and judgements has attenuated to such an extent that the official myth of science seems irrelevant to contemporary scientific concerns. The very success of science as a means of establishing knowledge about the world has resulted in its extension over so many areas and into so many new concerns that its institutional specificity and distinct character as an activity and as a form of understanding have become less clear cut. The successful establishment of 'the scientific method' as the means of dealing with cognitive problems – whatever their sources and complexity – has so extended the scope of fields amenable to scientific analysis, and hence the range of applications of that method, that its particular character and constitution have become diffuse and unclear. By claiming the preeminence of the method of science as manifested in physics or chemistry, and extending it to other fields of study so that in principle all phenomena are subject to it, the term has become so abstracted from the context of any one activity and generalised across so many fields that it has become reduced to highly abstract and vague injunctions which bear little relation to scientific practices. It is not surprising that many scientists in the more established fields prefer to leave discussion of this topic to philosophers and neophyte sciences whereas in previous periods it was a matter of some considerable moment among natural philosophers.

Insofar as science is today defined by its 'method', this is largely an institutional definition of general norms and presuppositions as reified in textbooks and educational curricula rather than being instantiated in everyday research. Unique qualities of scientific investigation in practice – if there are any – are not located in the very general institutional concept of science. Indeed the variability of research practices in different fields, and of criteria for making collective judgements, is clear from the studies published here, which themselves do not stray much beyond laboratory science. Rather than seeking particular features of scientific investigation, perhaps it should be doubted if there is any coherence to research work in different fields such as would justify the assumption that there is some distinct social activity termed scientific investigation. It is not insignificant that nearly all the case studies in this volume are studies of people who define themselves, and are defined by others, as scientists who are working in large organisations on a range of problems for a range of purposes. Rather than looking for the essential characteristics of scientific research, these authors have accepted the dominant institutional definition of scientists and examined them at work constructing knowledge. Science is seen here as an accomplishment of people accredited as scientists in the course of mutual negotiations and conflicts rather than as the outcome of a distinctive pattern of investigation (9).

However, the rejection of the notion of a distinct type of investigation being scientific – or, at least, the manifest difficulty in ascertaining the particular qualities involved – need not imply that scientific work is disorganised and predominantly structured by *ad hoc* contingencies. Instead it directs our attention away from the search for the essential features of scientific investigation *per se* towards an understanding of how particular investigations are patterned and organised in different sciences. Once it is agreed that research can take a number of forms, and is a social process rather than simply following universal methodological dictates, then the processes governing the conduct of research in different fields and their consequence for the development of scientific knowledge become a legitimate topic for sociological study. The manifest plurality of the sciences in terms of phenomena studied, methods of approach and criteria involved in the assessment of knowledge claims, and the failure of empiricist attempts to formulate general criteria of scientificity for all scientific knowledge, necessitate the development of a framework for analysing how research is organised in different fields and how patterns of organisation change.

In making sense of scientific development in the contemporary sciences, the context of research is obviously a crucial aspect and variations in its structure can be related to variations in research practices and their integration into different fields. This context can be seen as an organised arrangement of the sorts of background factors involved in many of the case studies published in this volume such as Pickering's 'cognitive interests', Knorr's 'resources', Callon's 'aggregation of interests' and the general citing of availability of apparatus and technical staff and other organisational constraints and cultures. These factors affect the everyday judgements of scientists and hence the sort of research which is carried out and the evaluation of its products. Consequently, their organisation and structure constitute the framework within which scientific work is conducted and made sense of. The structure of intellectual and other resources patterns the sort of research scientists both wish, and are able, to conduct and the ways in which results are understood. Different structures of these resources imply different patterns of research and therefore different paths of scientific development. In the rest of this paper I shall discuss what seems to me to constitute the major elements of the research process and this organisation as a way of ordering and understanding the structure of the everyday contingencies of scientific investigation (10).

The Intellectual Context of Research

The intellectual context of research is here considered as that abstracted set of norms and procedures which both govern and constitute what is done to what phenomena, in which cognitive setting, and how it is understood. It consists of the cognitive structures which, on the one hand, represent what is known and, on the other hand, constitute the resources with which to change and develop what is known. A particular scientific field, in this view, is constituted by the procedures which are required to develop knowledge in it so that rather than seeing scientific knowledge as a static structure which is fixed and permanent, it is viewed as a process of acquiring and changing understandings. To do research in a given field is therefore to use the procedures and intellectual resources that constitute it. This use of intellectual tools is itself guided by the particular arrangement or organisation of procedures which characterises the structure of the field and by higher order norms or preferences which order sciences. Particular combinations of available procedures will be preferred to others, and consequently research will develop along certain lines rather than in other directions. However, scientific fields do, of course, vary in their type of organisation and so, too, their patterns of development. The sorts of relations that obtain between the intellectual tools of a particular field – and their own degree of standardisation and formalisation – structure the sorts of scientific investigation that will be conducted and the interpretation and use of their outcomes. Patterns of research, such as those discussed in the case studies in this book, are, I suggest, to be understood in terms of particular arrangements of intellectual resources which order and integrate possibilities and results. Making sense of scientists' activities in different fields involves an understanding of how the intellectual components of that field are structured and mutually ordered such that it seems reasonable for a particular topic to be pursued in a particular way and the outcomes to be understood in certain ways which impinge upon other work so as to alter the collective self understanding of the field.

The ordering of appropriate topics or intellectual concerns to be pursued by scientists in a given field implies that the cognitive space or domain of the field is structured and bounded sufficiently clearly for research problems to be identified as being in it rather than part of another domain. Although the degree of boundedness and clarity of the cognitive space may vary – as between, for example, memory transfer and solar neutrino research – a minimal extent of cognitive structure delimiting the domain of the field is necessitated for it to exist at all. Research issues and foci are 'interesting' or 'relevant' or 'hot' in some intellectual context which makes sense of them in terms of more general concerns and problems which form the basis of identity of a scientific field and so 'contribute' to that field. So one intellectual component or resource involved in any research is the definition and delimitation of a particular domain of phenomena as constituting a field to which the research is oriented. This component can be termed 'domain assumptions'.

These domain assumptions vary in generality, abstraction and scope. The search for interesting and useful analogs of somatostatin described by Latour is clearly a more restricted and specific activity than the search for theoretical models in particular physics as discussed by Pickering. Fields differ similarly as the distinctions between specialisms and disciplines and research networks or areas indicate. Sometimes domains and their constitutive assumptions are seen as hierarchical in that lower order concerns and problems are derivative from more general and abstract ones, as sketched by Callon in his discussion of fuel cell research programmes, but this is not necessarily always the case and a heterarchical model is probably more appropriate (11), not least because the particular structure of conceptual networks can then be empirically investigated rather than assumed. Different sciences exhibit different arrangements of domain assumptions and there seems no good reason to assume, as Shapere (12) does, that 'sophisticated' sciences all exhibit the same structure. Indeed, Shapere requires domains to have a greater degree of conceptual closure for them to be accorded the label 'scientific' than seems necessary. The degree of closure is not simply a matter of 'sophistication' or the 'primitive' state of a science but is a characteristic that varies between fields and changes within fields over time. It is an outcome of social processes and strategies as emphasised by Callon and not simply the instantiation of 'progress'.

Domain assumptions form the background to a particular piece of scientific work which locates it as being 'interesting' within a broader intellectual context. During the course of the research process they may change, in that what seemed interesting in one context no longer is so but now is relevant to other concerns. As Pinch points out, Davis's essential problem domain was the experimental detection of neutrinos but in the course of developing the apparatus for detecting solar neutrinos he had to integrate this concern with theoretical nuclear astrophysics, and indeed could not make sense of the experiment without involving assumptions and models from that domain. Although the experiment was still in the domain of nuclear chemistry in a technical sense, its 'interest' in a broader sense was clearly in the domain of stellar astrophysics.

The specification of a cognitive domain of interest which implies some ordering of research topics or issues sets constraints – however loosely – on how such topics are to be conceptualised and worked on. Certain ways of formulating research problems will be seen as appropriate and others will not. The formulation of research topics in parts of physics, for example, has to follow certain rules if such research is to be accepted as competent contributions to those fields. Harvey's discussion of researchers studying hidden variable theories and their views of what constitutes 'real physics' highlights this point. Nevertheless, some diversity of conceptual approaches to research issues is possible even in highly structured sciences such as physics and it seems sensible to differentiate between intellectual norms distinguishing and constituting scientific fields and those which deal with the formulation of research topics and their conceptualisation. These latter can be termed formative procedures.

The way in which a particular topic is conceptualised will obviously have implications for how it is approached and worked upon. Only certain procedures and technical manipulations will be seen as appropriate or meaningful given a particular formulation of the problem. As Pinch shows in his discussion of the solar neutrino experiment, it was largely Davis' close collaboration with theoreticians in arriving at an 'appropriate' experimental design which ensured general legitimacy for his results and led to their being accepted by theoreticians as contradicting existing models. Although the term 'techniques' tends, in English, to imply some physical system of transformation rather than more general ways of working on materials, it seems a useful way of labelling this set of procedures, especially if its theory infused nature is borne in mind, as in Bachelard's concept of 'phenomeno-technics' (13), and as exemplified by Pinch's account of the solar neutrino experiment. Techniques, then, are viewed here as the sets of procedures which are available for working on topics formulated in particular ways in particular domains in the sciences.

The outcomes of working on research issues are not, of course, totally determined by the particular arrangement of input procedures. Consequently, judgements are required to make sense of results in the light of existing concerns and assumptions. These judgements can be viewed as developments of interpretative norms which are connected – in varying degrees – to dominant conceptions of the field and appropriate conceptual approaches. Again, however, particular interpretative norms are not uniquely determined by any particular arrangement of domain assumptions, formative norms and techniques so that we can usefully distinguish them analytically as a separate intellectual component or resource involved in research. Interpretative procedures are the means by which individual scientists and collectivities decide what the outcome of a particular piece of research is, and how its significance for other work is to be assessed in terms of more general concerns. Many of the case studies in this book provide examples of how such norms are developed in negotiations over the meanings of results in different fields.

The Organization of Intellectual Resources

These four components, or set of procedures, seem to me to form the minimum number of analytically distinguishable elements of scientific investigation although further sub divisions are clearly possible, as I indicated in the case of domain assumptions. Processes of research in different sciences, or in the 'same' field in different periods, can be compared and contrasted in terms of different arrangements and features of these procedures – the intellectual organisation of scientific fields. The particular ways in which research is conducted obviously depends upon the available resources and their interrelationships or organisation which can be characterised on a number of dimensions.

Work procedures in general have been analysed in various ways, although most sociological analyses stem from Weber's discussion of formal bureaucratic rules. Dimensions such as formalisation, standardisation, configuration, centralisation, etc. have been developed and various measurement procedures applied to them, most notably perhaps by the 'Aston' school and its progeny (14). From a different viewpoint, Randall Collins has suggested that scientific disciplines should be viewed as an organisation exhibiting "a regular division of labour and relatively stable forms of influence or control" (15). He further proposes that disciplines can be arranged "along a continuum of forms of organisational control" which seems to be a matter of consensus and coherence of work practices and theories. While I am dubious about the general applicability of the Kuhnian model to all the sciences, and Collins is singularly imprecise in his use of the term 'discipline', this sort of approach to the analysis of science offers considerable sociological scope and is clearly compatible with most of the case studies in this book.

A preliminary way of developing some of the dimensions used by writers on organisations for analysing scientific investigation is to distinguish between fields where the interdependence and interconnectedness of research procedures is high from those where there are a number of alternatives available for use with any particular intellectual resource and work can be validly carried out using a variety of combinations.

Related to this is the clarity of formulation and firmness of characterisation of the intellectual resources of scientific fields. I have already referred to Travis' discussion of the openness of the scientific culture with regard to memory transfer research in planarians. This openness enabled the worm runners to make significant claims without substantial opposition or contradiction. The fluidity of the cognitive space concerning memory transfer in the 1950s was considerable so that, as Travis suggests, the implications of a number of phenomena and approaches were not especially in mutual contradiction or conflict. This fluidity, or relative low degree of clarity and firmness of domain assumptions, enabled a plurality of procedures and formative norms to coexist without much overt disagreement. In contrast, most areas of physics and chemistry exhibit strongly formulated domain assumptions and other norms and these are often mutually implicative to a high degree so that particular approaches and techniques are required by a particular topic and interpretative norms are strongly connected to the initial formulation of the problems. It is usually fairly clear in such fields whether a given topic is 'in' a particular area and whether it has been 'correctly' studied and appropriate conclusions drawn from the outcomes. In other fields where these four sets of norms are not so clearly expressed and mutually connected, both scientists and observers have some difficulty in deciding which field a given piece of research is relevant to, and how its correctness could be assessed. Indeed, the question of its domain may appear peculiar, except in the most general sense (16).

These two related dimensions can be summarised as the degree of formalisation of intellectual norms and procedures, on the one hand, and the degree of their interdependency and integration on the other hand. Formalisation is here meant in a somewhat broader sense than that used by the Aston School in their studies of organisational structures (17). Rather than referring to the use of written procedures and instructions it is intended to encompass the degree of definiteness of intellectual procedures, which may be written down or largely tacit, so that it is clear when mistakes have been made. The more formal are procedures and norms, the easier it is to identify them, differentiate them from other resources and be clear about their appropriateness and correct means of application and use. Generally, highly formal procedures restrict what can be done and how it can be done 'correctly' and so are likely to have relatively strong implications for the use of other procedures. Fields with formal domain assumptions, for instance, will restrict the range of formative norms governing the development of appropriate research topics and the use of technical procedures for working on them. The interdependence of intellectual resources in such areas of scientific research will, therefore, be considerable. In this sense a high degree of formalisation implies a relatively high degree of interdependence. However, not all the resources available in a given field need be equally strongly formalised and not all the possible interconnections between them need be equally direct or restrictive. For example, the way in which a particular domain of scientific research is delineated may be relatively weak and imprecise, in that its boundaries are not especially clear and do not directly specify what topics are part of it, nor how they should be formulated and yet technical procedures may be strongly linked to the way any particular topic or problem is developed and, at the everyday level of research practices, the interpretation of the outcomes may be relatively straightforward even if their implications for the broad domain of, say, cancer, are not so obvious (18).

Where the degree of interdependence is high in a given field so that the development of a particular topic is clearly within or without its boundaries, and it can be assessed in terms of its relevance and importance to the domain of the field, we would expect certain techniques to be clearly appropriate and the results to be relatively easy to interpret in terms of the domain assumptions. Strong interdependence implies high formalisation and in an extreme instance the degree of cognitive closure would be so great as to leave little ambiguity and scientific judgements would be relatively routine. Scientific research in this instance would be so 'normal' as to be programmable as in, for example, routine chemical analysis. In most sciences, though, as indicated by the cases reported in this volume, choices are still required of scientists and decisions have to be made about how to proceed. Nonetheless, those choices are structured by the organisation of cognitive resources in individual fields and by characterising this organisation in terms of the degree of formalisation and interdependence we can begin to understand how scientific investigation varies between various fields.

The Institutional Context of Scientific Investigation

As several papers in this volume emphasise, patterns of everyday research are strongly influenced by organisational and institutional contingencies; the particular ways in which intellectual resources are combined and used reflect what Latour terms the 'material and local circumstances' of the laboratory. An understanding of processes of scientific investigation involves, therefore, an analysis of how resources are organised, reproduced and changed, the institutional context structures possibilities and priorities so that certain patterns of research develop and not others. The 'opportunistic tinkering' mentioned by Latour occurs within distinct institutional orders so that some activities and arrangements of intellectual procedures are more feasible and fruitful than others. In the rest of this paper I shall briefly discuss some of the major aspects of the institutionalisation of research procedures in the contemporary sciences (19).

A fundamental way in which intellectual norms and procedures are institutionalised in research practices is in the form of intellectual commitments and skills. For a given scientific field to reproduce itself as a distinct area of scientific investigation, some commitment to its domain assumptions must be maintained and skills for such investigation inculcated into scientific practitioners who have the resources required for pursuing topics 'in' that field. Just as procedures may vary in their degree of formalisation, so too may the degree and scope of intellectual commitments differ between fields and the specificity of any particular set of research skills to a particular area or range of topics vary. Furthermore, the organisational basis of allocating material resources and scientists' time – i.e. employment opportunities and priorities – may not fully overlap with the domain assumptions of particular fields and, indeed, may constitute alternative criteria for topic selection and conceptualisation. Much research conducted in full time research laboratories, as evidenced by many of the papers here, is not obviously 'in' any particular university based discipline or speciality and, indeed, may deliberately set out to negate such boundaries in favour of different audiences and priorities.

The particular ways in which intellectual resources are institutionalised in a given area structure the sort of research that is carried out, how it is conducted and how it affects other work. Without commitments to topics and approaches in an area and the relevant skills and resources required to work on them, that area will simply disappear as Fisher's analysis of invariant theory illustrates (20). However, commitments may conflict, both among themselves and with the priorities institutionalised in the reproduction of skills and availability of resources, so that scientific work can take a number of directions without any clear development path emerging immediately. Such conflicts are predicated upon the intellectual resources of a field being sufficiently clearly developed – as Travis suggests – and their domain of application being mutually recognised. Where a particular cognitive space is relatively weakly bounded or interests in it are not well defined, overt conflict and competition are not very likely to occur. Scientific investigation in these situations is likely to focus on particular techniques and skills rather than on highly ordered topics and relevant criteria. Domain assumptions will be only weakly connected to research topics and outcomes.

The development and reproduction of intellectual commitments and, skills are results of the scientific education and training system, the structure of job opportunities and 'careers' (21) and the system of publication and recognition. Obviously, commitments to particular norms and procedures develop initially through the system of formal instruction and training in secondary and higher education. It is largely in educational institutions that identities of 'scientist' and, later of belonging to a particular science and discipline are formed. These identities imply general commitments to certain ontological and epistemological beliefs held to be constitutive of 'science' and of particular disciplines as well as to a given domain of interest. They also, as Kuhn and, in a different way Ravetz (22) emphasised, imply the acquisition of skills which are appropriate for the development of research in the discipline. Educational institutions thus form the basic commitments of scientists in nearly all fields, and constitute the fundamental unit of social and cognitive identity in the sciences which is one reason why the term 'discipline' is usually understood to refer to units of organisation in universities (23). Equally, of course, conflict over how these units are formed and bounded is endemic because they affect the supply of new recruits to the 'disciplines' and hence commitments to pursuing particular fields of research and hence the future existence of those fields. Without the opportunities of inculcating interest in a particular field, and the development of basic skills for work in it at an early stage of the research training process, the future of the field remains doubtful. Control over curricula, admissions and assessment in undergraduate departments of universities is thus a crucial part of the reproduction of scientific fields. Equally, the monopolisation of this initial phase of research training by the universities in most industrial countries gives these institutions a dominant role in scientific development (24).

While undergraduate instruction remains an important part of the development of intellectual commitments and skills, it is, of course, by no means the most crucial phase in many sciences. The development of the Ph.D degree as the trade union ticket for obtaining academic posts and access to research positions and facilities, has effectively downgraded the first degree in many countries although this process has occurred at different rates in different fields and in different national academic systems. By insisting on this degree as the basic requirement for conducting research, universities have maintained their control over the development of commitments and skills, although increasingly Ph.D research is being conducted outside university departments in full time research laboratories funded by the State and other bodies. Usually, within the general commitments and skills developed at the undergraduate level the Ph.D degree focusses upon more specific topics and develops more circumscribed skills. Generally, postgraduate research builds upon interests and capacities instilled during the first degree and so ready access to a large pool of competent graduates through control over an undergraduate department is of considerable assistance in ensuring continued recruitment to a particular area (25).

The extent to which a Ph.D implies commitment to a given field within a discipline varies, as Harvey's discussion of the students involved in research on hidden variables shows, and such commitment may be very tenuous in the case of sciences with weakly formalised and integrated norms and procedures. However, the skills acquired during the Ph.D process – and subsequently as a post doctoral fellow in some fields – can be expected to have a major impact upon the directions pursued by scientists. Where these skills are clearly interdependent with particular domain assumptions and other norms, then commitment to working in that field will be greater than in a situation where cognitive boundaries are weakly formalised and integrated. A further point to be noted here is that not all – and perhaps increasingly fewer – Ph.D topics and approaches are clearly located 'within' a particular discipline as institutionalised in undergraduate curricula. The more research is conducted outside university departments, the more likely is this separation of undergraduate identities and boundaries from Ph.D research to become manifest.

The development of certain research skills during Ph.D and post doctoral research implies some commitment to particular ways of formulating problems and ways of working on them, as well as to some means of interpreting outcomes. While these may be very specific and narrow, especially in highly formalised and integrated sciences, they will form the basis for more general commitments to formative, technical and interpretative norms which will direct research foci and approaches. In this sense the research conducted for the Ph.D degree will exercise major influence on future work and the organisation and control of Ph.D topics in a discipline will have major consequences for the development of particular areas. Where Ph.D students are warned off certain problems, or find difficulty in finding supervisors, commitments are unlikely to develop and interest in such areas will decline. The marginality of the work on hidden variable theories emphasised by Harvey in this volume is demonstrated by the determination of most experimentalists to do 'mainstream' physics after their Ph.Ds and not 'crazy' experiments.

The availability of Ph.D topics and encouragement of particular fields of study in a science will depend upon the general structure of the discipline and its institutionalisation in terms of university posts and other resources. The particular skills and interests reproduced during Ph.D training, that is, are a function of priorities and goals in the discipline and how these are manifested in particular departments in terms of staff interests and skills, and the availability of materials, technicians etc. In disciplines where intellectual norms and procedures are relatively highly formalised and integrated, particular areas and topics will be clearly more interesting and important than others and particular ways of approaching, working on them and interpreting the results will be clearly more appropriate than others (26). In this situation, Ph.D students are likely to be channelled into examining certain topics and developing certain skills in working on these and so commitments to particular domains and approaches will be strongly reproduced while others will be largely ignored except by a few 'deviant' students and supervisors such as, perhaps, those comparing hidden variable theories with orthodox quantum mechanics. Specialisation of topics and approaches is likely to become quite high among Ph.D students in such subfields and this in turn will restrict the general applicability of the skills acquired by these students which will reinforce existing hierarchies of topic areas and conceptual approaches. As this process of specialisation and differentiation of tasks and research topics continues, the results of much research will be only tenuously connected to disciplinary goals and objectives. As domains – and associated interpretative procedures – become narrower and more specific, their relations to broader concerns will become attenuated and sub-fields and sub-sub-fields will develop a fair degree of autonomy from the parent discipline so that the precise relevance of much research will be difficult to discern. Equally, the discipline itself will become more fragmented as specialisation proceeds and loyalties become more focussed upon lower levels of intellectual organisation and everyday research concerns are more and more specific. The extent to which the contemporary disciplines of physics and chemistry still direct and integrate research in the same way that they did in the nineteenth century seems limited (27).

On the other hand, where intellectual norms and procedures are less clearly formalised and interdependent the hierarchisation of domains and approaches is less obvious. Here Ph.D students will not be directed into 'hot' areas so much and so commitments and skills will not become so concentrated. To the extent that priorities are less clear cut, and how to pursue them less obvious, Ph.D research will be less highly organised and structured in terms of disciplinary norms and be more susceptible to "local" influences and exigencies. Research will be less clearly "theory driven" and diverse approaches to topic selection and formulation possible. The development of commitments and skills among Ph.D students in such fields will be correspondingly varied and different sets of intellectual procedures manifested in Ph.D theses. While Ph.D topics may be equally specialised in these sciences as in more formalised ones, their formulation will be less clearly specific and a wider range of technical approaches appropriate. Also, the relevance of the results will be less clear for any given set of domain assumptions and may well become seen as broader in scope than any single area. In general, because cognitive boundaries are fairly fluid and not strongly tied to particular approaches and techniques, Ph.D students will develop broader competencies which are more generally applicable, than those working in formalised and integrated sciences. In this sense, Ph.D research in relatively unstructured fields will not be so restrictive in determining future areas of work. Skills will tend to be generalised across a range of phenomena and a multiplicity of different skills will be appropriate in analysing any particular phenomenon (28).

The formal system of education and training develops general commitments to particular fields of research and the skills appropriate for conducting research on certain topics and phenomena. Manifestly, though, it does not determine totally the sort of research that is pursued, nor how that work is made sense of in terms of general concerns. Most of the studies in this volume indicate a variety of social, intellectual and organisational categories affecting scientists' judgements about what to do and how to do it. One of the most obvious of these is simply the provision of employment opportunities and research facilities in particular fields and for particular purposes. While the dominant form of employment remained university based and largely organised around the ideal of the international scholarly community, the specific details of employment structures and resource provision could reasonably be considered subsidiary to the overall organisation and priorities of the scientific field and its 'establishment', at any rate in the more integrated and formalised areas. With the development of scientific research in non-university controlled organisations, which not infrequently have specific purposes set by non-academic institutions, this procedure becomes less justifiable. This is especially so in fields of research which are relatively weakly formalised and integrated so that competing ways of ordering priorities can have considerable impact on the topic pursued and the interpretation of results. Where disciplinary boundaries are not particularly strong and clearly connected to hierarchies of 'interesting' problems and 'correct' conceptual and technical approaches, the establishment of research laboratories providing employment and resources for particular social goals will have greater impact on the sort of work undertaken by scientists than in disciplines with more rigid intellectual structures (29). In general, the multiplicity of employment opportunities for researchers in a variety of organisations, which may be oriented around mutually contradictory and conflicting principles and goals, reduces the degree of integration of intellectual norms in particular disciplines. Patterns of scientific investigation are not so fully structured by disciplinary priorities but rather become the result of a complex set of interrelationships between work organisations, educational structures and cognitive norms. Intellectual fields consequently become less clearly ordered and bounded, the meaning of any particular piece of research for a given area of concern or interest is less clear when potentially competing principles of work organisation are institutionalised in the form of employment opportunities focussed on a plurality of goals. Criteria of relevance and interest become more open and varied than the Kuhnian notion of 'puzzle solving' suggests.

The growing importance of goal directed work organisations in scientific development has, of course, become increasingly recognised (30) although the extent to which they structure commitments and hence research practices has not perhaps been overly emphasised. In some cases they provide intellectual foci and identities which are unlikely to have occurred without formal provision of employment and facilities (31). At the very least they direct attention to particular topics and concern at the expense of others and to the extent that they organise researchers in particular ways and combine or differentiate sets of skills and commitments, they determine how phenomena are conceived and worked upon as Callon's discussion shows. Simply bringing scientists with particular commitments and skills together in a single employing organisation structures orientations and approaches. The importance of these 'local' circumstances is especially notable in relatively decentralised and unintegrated disciplines such as those discussed by Knorr and Latour. In physics they may be less crucial although still not negligible.

The other major aspect of the institutional context of scientific investigation, and the one that has received most attention from sociologists studying scientific communities, is the recognition and reward system. The selection of topics and preferred methods of approach has usually been seen in terms of priorities and criteria institutionalised in particular reward structures. Although reworked from a rather different perspective, many of the papers here adopt a similar focus in their invocation of investment strategies and rates of return to individual scientists. Specific pieces of research are undertaken primarily with a view to gaining recognition or "credit" among the international scientific community in this approach, and hence particular patterns of scientific investigation are to be accounted for in terms of the structure of the intellectual market (32).

These structures vary in their degrees of formalisation and integration between scientific fields and so scientists have varying extents of discretion in seeking audiences for their work and hence in the selection of topics and methods of approach. Those in relatively weakly structured fields, where domain assumptions are not strongly formulated and integrated with particular procedures for delineating and ordering topics and techniques, will have a number of journals available for publicising their work which are not strongly mutually ordered in a prestige hierarchy. The particular topic they choose to work on, and the way they work on it will be related to the outlet selected but that decision in itself will be more open than in sciences where prestige hierarchies are more strongly established, such as physics. Journals can be seen as means of institutionalising intellectual commitments in that they reproduce in their own practices, and in their interrelations, the structure of norms and procedures which characterise particular areas of concern and work in the sciences. Given the 'public' nature of the scientific enterprise in general, and the intimate connections between journal publication and rewards and hence scientific 'careers' in most fields, the operation and organisation of scientific journals are key aspects of the organisation of the sciences in general, and of particular patterns of scientific investigation. Journals signify commitments to particular areas of concern and instantiate procedural norms. They thus both reflect the structure of scientific fields and reproduce them.

By structuring cognitive space and functioning as guardians of procedural norms, scientific journals organise commitments and skills developed in educational systems and employed in research organisations. Research that is unpublished is, on the whole (33), research that has not been done with respect to the public system of science and hence cannot form part of the corpus of public knowledge. Consequently, the existing set of journals in a science constrain and direct research topics and ways of working on them. The development of new domains of investigation and/or novel procedures for working on them frequently, therefore, necessitates the formation of a new journal, especially in highly structured and integrated fields. Commitments and skills, then, are ordered by the priorities and preferences established and reproduced in journal policies and their interrelations.

More generally, we can say that the recognition system as a whole, of which journals constitute a crucial part, directs scientists' attention to particular topics and approaches and hence structures patterns of scientific investigation. This is not by a simple exchange of rewards for valuable commodities, though. Rather, the meaning, significance and consequences of a piece of research are functions of the general structure of norms and commitments that constitute the system of recognition in a particular scientific field or number of overlapping domains. Recognition here involves interpretative norms which make sense of – to varying degrees – research outcomes in terms of domain assumptions and procedural norms. The meaning and importance of knowledge claims are established with reference to some general and appropriate means of working on them – or sets of them – and so recognition locates particular results in particular intellectual contexts. Where this is difficult or impossible, in that relevance criteria cannot link particular descriptions of results to established concerns, recognition cannot be awarded. The recognition system, therefore, involves the establishing and dissolving of links of relevance between cognitive objects in the assessment of individual scientists' contributions to certain fields of concern. It also, of course, orders these fields of concern themselves in terms of more general criteria and domains of interest. By institutionalising relevance criteria and forming the basis for reward allocation, the recognition system manifests and expresses a particular network of connections between intellectual resources which order scientists' priorities and constitute the basis for their judgements. In this sense it directs the commitments and skills developed by the educational system, and combined in various employing organisations, along particular paths of scientific development.

In sciences, where intellectual norms and procedures are highly formalised and interdependent the recognition system locates and evaluates contributions with low degrees of ambiguity. It is usually "obvious" to which area or domain a particular piece of research is a contribution, and the relative importance of it will also be fairly straightforward to assess. However, the increasing specialisation and differentiation of subfields may make the tracing of implications of knowledge claims throughout an extensive network of concerns and understanding difficult, and the integration of distinct domains into a discipline, or some such similar general unit of concern, impossible. Recognition of contributions to the discipline, then, becomes derivative of contributions to the dominant and exemplary sub-discipline or 'speciality' such as particle physics, although the precise implications of such contributions throughout physics may be uncertain. The recognition system here fragments into a number of sub-units which are organisationally linked through the educational system, but whose cognitive interconnections are not always clear.

In less highly structured fields, it is not always obvious to what concern any individual piece of research is relevant, nor how it is to be assessed in terms of 'correct' procedures. With fluid and weakly formulated domain assumptions, which do not have strong implications for particular topics and techniques, the interpretation of results with references to particular intellectual concerns, and assessment of their importance in general, will be tentative and liable to revision. A plurality of possible relevance will be possible and historical revisions of evaluations of previous research more probable than in more structured sciences. Consequently, recognition will be less clearly an outcome of contributions made to an individual area of study and may be frequently revised. Audiences for certain kinds of work, and hence recognition from particular sets of scientists, are unstable and mobile so that reference groups for research topics are different to locate except in general organisational, or largely common sense, terms. In these fields multiple connections between intellectual resources abound and are not strongly ordered so that contributions are recognised in a variety of different ways which need not necessarily be mutually compatible. The changing reception to the work on memory transfer in planarians and in rats discussed by Travis illustrates this point. In general, the contrasts drawn here are illustrated by the accounts of physics given by Harvey, Pickering and Pinch when compared with the work discussed by Knorr, Latour and Travis. By and large, audiences and relevance criteria are clearer and more stable in the former accounts than in the latter cases. Also hierarchies of topics and the evaluation of 'success' are relatively sharply defined in physics.

Conclusions

These papers, and other studies of scientists at work, demonstrate the highly contingent nature of much scientific work and of judgemental processes. What becomes recognised as scientific knowledge is the outcome of complex social processes which are not reducible to an algorithm, and which are themselves subject to change. In fields which are commonly regarded as the most scientific it is quite clear that judgements are made in the context of patterns of social and intellectual organisation and cannot be adequately understood without locating them in that context. These patterns vary in different fields and also differ over historical periods, they constitute the framework for scientific research and provide the basis for constituting scientific knowledge at any particular point. Consequently, their structure forms the basis of the organisation – and its changes – of scientific knowledge. The various ways in which intellectual commitments are organised and institutionalised in the various sciences constitute the resources for research and for making sense of the outcomes of such work. Strongly bounded and integrated norms will lead to certain forms of investigation and not others, their results will be organised in particular ways. The organisation of intellectual commitments constitutes the structure of knowledge in a science and, through the particular way it is socially institutionalised, directs research along particular lines to modify that structure and develop it. It represents – or is – the cognitive order and provides the norms for extending that order through further work which may transform it. Where the principles – or dimensions – of organisations differ in scope and nature, then the directions of future development are open and liable to a variety of influences. The context of scientific investigation, in the sense used here, locates the apparently highly contingent and *ad hoc* nature of much scientific work in a broader framework which enables us to make sense of research practices and their consequences for scientific knowledge. That is, the norms and meanings which organise a scientific field at once represent the structure of knowledge in it and govern processes of developing and modifying it.

Notes and References

1. Roger Krohn, 'The Social Process of Scientific Investigation', unpublished paper, McGill University. Most of the literature discussed by Krohn is written by psychologists and/or famous inventors.
2. An interesting exception to these strictures is Ian Mitroff's, *The Subjective Side of Science*, Elsevier, Amsterdam, 1974.
3. In his paper in this volume. Where no specific reference is given to an author in this paper, I am referring to her/his contribution to the present book.
4. Depending on the definition adopted, this is usually taken to be in the French Academy and later educational institutions or in the reform of the Prussian universities and the rise of the research laboratory. See, for example, the chapters by Crosland, Hahn and Farrar in M. P. Crosland (ed.), *The Emergence of Science in Western Europe*, Macmillan, London, 1975; R. Hahn, *The Anatomy of a Scientific Institution*, University of California Press, 1971; R. Steven Turner, 'The Growth of Professorial Research in Prussia, 1818 to 1848 – Causes and Context', in R. McCormmach (ed.), *Historical Studies in the Physical Sciences* **3**, 137–182 (1971); E. Mendelsohn, 'The Emergence of Science as a Profession in Nineteenth Century Europe', in K. Hill (ed.), *The Management of Scientists*, Beacon Press, Boston, 1964. For a historian's attack on overly sociological notions of the professional scientist see S. F. Cannon, *Science in Culture*, Science History Publications, New York, 1978, Chap. 5.
5. Much of this analysis has been conducted under the rubric of 'goal directed' science as in, for example, W. v. d Daele, W. Krohn and P. Weingart, 'The Political Direction of Scientific Development', in E. Mendelsohn *et al.* (eds.), *The Social Production of Scientific Knowledge*, Sociology of the Sciences **1**, 219–241 (1977), and the papers by Johnston and Jagtenberg, Küppers and by Weingart in W. Krohn *et al.* (eds.), *The Dynamics of Science and Technology*, Sociology of the Sciences, II, 1978.
6. As in, e.g. E. Mendelsohn and P. Weingart, 'The Social Assessment of Science: Issues and Perspectives', in E. Mendelsohn *et al.* (eds.), *The Social Assessment of Science*, USP Bielefeld, 1978.
7. This well known phenomenon is discussed in a number of places including J. Gaston, *Competition and Originality in Science*, Chicago University Press, 1973. See also D. J. Kevles, *The Physicists*, Knopf, New York, 1978 for a historical account.
8. The importance of alternative conceptions of science in the development of a distinct 'discipline' is illustrated in Roy Porter's *The Making of Geology*, Cambridge University Press, 1977.
9. This acceptance of dominant institutional meanings is realistic for those wishing to study scientists' everyday practices in constructing knowledge. It need not, though, and should not, imply an acceptance of those meanings as unproblematic. As an institutional reality the sociological demarcation of science from other meaning systems requires investigation – particularly in its historical context. The apparent collapse of philosophical attempts to formulate absolute criteria for distinguishing scientific knowledge from non-scientific knowledge should encourage sociological accounts of how particular conceptions of science have become entrenched and

reproduced in particular institutional contexts and their relations to research practices accounted as 'scientific'. W. v. d. Daele has sketched part of such an account for 17th century English science in his 'The Social Construction of Science: institutionalisation and definition of positive science in the latter half of the seventeenth century' in E. Mendelsohn *et al.* (eds.), *The Social Production of Scientific Knowledge*, Sociology of the Sciences **1**, Reidel, Dordrecht, 1977.

10. In distinguishing between the intellectual resources for, and the institutional context of, scientific research, I do not intend to reproduce the social/cognitive distinction criticised by Callon and Latour in their papers here. Intellectual structures *per se*, do not constrain research but their institutionalisation in particular organisational arrangements does and these patterns vary between the sciences and in the 'same' science in different periods. Consequently, it seems essential to be able to identify the major components of such intellectual structures so that differences in their organisation can be analysed.
11. As briefly discussed in H. Nowotny, 'Heterarchies, hierarchies and the study of scientific knowledge', in W. Callebaut *et al.* (eds.), *Theory of Knowledge and Science Policy*, Communication and Cognition, Ghent, 1979.
12. D. Shapere, 'Scientific Theories and their Domains', in F. Suppe (ed.), *The Structure of Scientific Theories*, University of Illinois Press, 1974.
13. Although Ph.D topics are by no means always 'within' the cognitive space delineated by a particular undergraduate degree.
14. See D. S. Pugh and D. J. Hickson, *Organisational Structure in its Context*, Saxon House, Farnborough, 1976 and D. S. Pugh and C. R. Hinings (eds.), *Organisational Structure: Extensions and replications*, Saxon House, Farnborough, 1976. Three types of task interdependence – pooled, reciprocal and sequential – with different means of coordination are discussed by James Thompson in his *Organisations in Action*, McGraw Hill, New York, 1967, which focusses rather more on the way work is differentiated into tasks and controlled.
15. Randall Collins, *Conflict Sociology*, Academic Press, New York, 1975, p. 493.
16. Certainly this was the reaction of many scientists in biomedical fields in our study of scientific specialities. Cf. A. Bitz, A. McAlpine and R. Whitley, *The Production. Flow and Use of Information in Research Laboratories in Different Sciences*, British Library Research and Development Division, London, 1975.
17. Pugh and Hickson, *op. cit.* 1976, Note 15, p. 32.
18. Cf. Bitz *et al. op. cit.*, Note 17.
19. Some historical aspects are discussed in R. Whitley, 'The Rise and Decline of University Disciplines in the Sciences'. In R. Jurkovich *et al.* (eds.), *The Nature of Interdisciplinary Research*, Rotterdam, 1980.
20. C. S. Fisher, 'The Death of a Mathematical Theory: A Study in the Sociology of Knowledge', *Archive for History of Exact Sciences* 3, 137–159 (1966).
21. It is not clear what exactly a scientific 'career' consists of; Randall Collins reduces it to the sequence of contacts a scientist has in his professional life, (*Conflict Sociology*, Academie Press, New York, 1975, p. 514) which establishes his fame. Generally, it seems to refer to little more than a set of accomplishments which are only ordered chronologically. Rather similar to the vague yet frequent usages of 'career' in managerial contexts, the term is pervasive without being especially enlightening in science.

22. J. R. Ravetz, *Scientific Knowledge and its Social Problems*, Oxford University Press, 1971, pp. 76–144.
23. Although historians tend to focus more on conceptual consensus and integration as in, e.g. S. F. Cannon, *Science in Culture, op. cit.*, 1978, Note 1, Chap. 4; R. McCormmach, 'Editor's Foreword', in R. McCormmach (ed.), *op. cit.*, 1971; and R. H. Silliman, 'Fresnel and the Emergence of Physics as a Discipline', in R. McCormmach (ed.), *Historical Studies in the Physical Sciences* 6, 137–162 (1974). However the conceptual level at which such integration is supposed to constitute a discipline remains obscure.
24. Some of these points are developed in their historical context in R. Whitley, *op. cit.*, 1980, Note 20.
25. Although Ph.D topics are by no means always 'within' the cognitive space delineated by a particular undergraduate degree.
26. The hierarchy of specialisms in physics is a well known instance of this point. Cf. W. O. Hagstrom, *The Scientific Community*, Basic Books, New York, 1965, and D. J. Kevles, *The Physicists*, Knopf, New York, 1980. The centralisation of resources and facilities characteristic of modern physics is facilitated by the highly formal means of communication, i.e. mathematics, and is by no means universal in the contemporary sciences. The more tacit, fluid and informal are craft procedures – as is arguably the case in 'configurational' fields – the less likely are disciplines to conform to the model of physics. Cf. N. Elias, 'The Sciences: towards a theory' in R. Whitley (ed.), *Social Processes of Scientific Development*, Routledge and Kegan Paul, London, 1974.
27. Indeed, whether they function in the same way at all with reference to current research practices seems dubious. For a discussion of 19th century physics see the literature cited in Note 24, R. Whitley, *op. cit.*, 1980, Note 20.
28. These points are further discussed in the context of a study of laboratories in different sciences in R. Whitley, 'Types of science, organisational strategies and patterns of work in research laboratories in different scientific fields', *Social Science Information* 17, 427–447 (1978). See also, R. Whitley, 'The Sociology of scientific work and the history of scientific developments', in S. S. Blume (ed.), *Perspectives in the Sociology of Science*, Wiley, New York, 1977.
29. An obvious example of this is the field of cancer research in contrast to, say, physics but many biological research organisations offer similar instances. See Bitz *et al., op. cit.*, 1975, Note 17, and B. Latour and S. Woolgar, *Laboratory Life*, Sage, London, 1979 and the papers by Latour and Knorr in this volume.
30. Cf. literature cited in Note 5 and the well known 'finalisation' thesis.
31. As can be argued for the emergence of 'oncology', cf. J. Sadler, *Elites in Science: a study of elites in relation to the cognitive structure and social organisation of cancer research*, unpublished M. Sc., thesis Dept. of Liberal Studies in Science, Manchester University, 1976.
32. This is especially emphasised by P. Bourdieu, 'The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason', *Social Science Information* **14**, 19–47 (1975).
33. Preprints in high energy physics have become accepted as almost as good as papers published in journals provided they come from one of the very small number of internationally recognised centres.