THE SOCIAL PROCESS OF SCIENTIFIC INVESTIGATION

SOCIOLOGY OF THE SCIENCES

A YEARBOOK

VOLUME IV – 1980

THE SOCIAL PROCESS OF SCIENTIFIC INVESTIGATION

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D. REIDEL PUBLISHING COMPANY

DORDRECHT : HOLLAND / BOSTON : U.S.A.

LONDON : ENGLAND

Library of Congress Cataloging in Publication Data

Main entry under title:

The Social process of scientific investigation.

(Sociology of the sciences ; v. 4)
Includes bibliographical references and index.
1. Science—Methodology—Addresses, essays, lectures.
2. Science—Social aspects—Addresses, essays, lectures. I. Knorr, Karin D., 1944– II. Krohn, Roger G., 1931– III. Whitley, Richard. IV. Series.
QC175.3.S64 507'.2 80–22279
ISBN-13: 978-90-277-1175-5 e-ISBN-13: 978-94-009-9109-5
DOI: 10.1007/978-94-009-9109-5

Published by D. Reidel Publishing Company,
P.O. Box 17, 3300 AA Dordrecht, Holland

Sold and distributed in the U.S.A. and Canada
by Kluwer Boston Inc.,
190 Old Derby Street, Hingham, MA 02043, U.S.A.

In all other countries, sold and distributed
by Kluwer Academic Publishers Group,
P.O. Box 322, 3300 AH Dordrecht, Holland

D. Reidel Publishing Company is a member of the Kluwer Group

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INTRODUCTION:
TOWARD THE EMPIRICAL STUDY OF SCIENTIFIC PRACTICE

The question, "What is science as a human and social enterprise?", has been giving rise to an increasing range and volume of studies in the last 15 to 20 years. This volume marks a new phase of study, the development of the empirical study of the research process itself. From patterns in the long-term growth and change in scientific ideas, to the response of science to its larger social and economic context, to its patterns of institutionalization and professionalization, to processes of the giving and seeking of recognition for unusual contribution, we have only lately finally arrived at the question, "How does scientific work proceed at its core, the research process itself?". Once posed, it seems strange that the question was left until so late. Perhaps that was because research has been seen as being on the one hand self-evident, and on the other mysterious – self-evident in its basic standards of empiricism and logic and a mystery concerning the origin of ideas, how the particular circumstances of research were made to yield universal statements. In either case, self-evident or mysterious, research was not problematic. In studying a respected and even semi-sacred institution, it is not surprising that the sense of the problematic first touched its external and larger features – the economic, class, religious conditions of its countries of origin – and only later its internal and more subtle ones. The former could be done in the institutions' own vocabulary, but the latter has required a new language, a reconception of basic terms in order for there to be any question at all. Given the institutions' own terms and assumptions the reasons for standard practice are self-evident.

The papers in this volume are all in answer to the question "What is scientific practice; how is it done?". They depart from the conventional language

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

of science in different ways and to different degrees, but a major common theme is how the new concerns may be expressed and new observations made. They also move to an array of more specific and more answerable questions: how are problems formulated and selected; how are research opportunities recognized and exploited? How is scientific work presented, received and evaluated by other scientists? And so forth.

The presupposition of these questions is that science is not a category of exception, but elaborated from the same elements as everyday practice, and shares as well as differs in these elements from other specialized cultural activities, whether art, religion, law, journalism, or whatever. The unique features of scientific investigation and communication may then eventually be established. These papers begin to resolve Ravetz's paradox of 1971: how does "the subjective, intensely personal activity of creative science . . . (become) the objective, impersonal knowledge which results from it." (1)

The expansion and shift of focus in this questioning process is illustrated by such papers (2) as Knorr and Woolgar who emphasize that the scientists' later and public account of what was done in his/her laboratory is as constructed, is as much a cultural artifact, as the work itself. Harvey also shows that the reasons for rejecting an anomalous experiment change from the contingent and contextual judgements of credibility during the process of evaluation to a reconstruction in terms of evidence and logic ("the errors argument") after the rejection. (160–161). An earlier and more personal account, as in an interview, will give additional, valuable information, but it will also be a partial and selective construction of "what happened". This, in turn, can be usefully supplemented by an observer's account, if he/she is well informed on a day-to-day basis over a considerable period of time. In this view, the phrasing and revision of the scientists' final published account then becomes the object of attention and interpretation in a way impossible if it is seen as the objective and only possible report of logically organized work.

Thus, the investigation and the communication phases are seen to be in deep interaction, and must both be described as they occur if the "results" are to be understood. In fact, one unique feature of science may well turn out to be the degree to which communication with colleagues enters into the creative process, into the formulation of the work, in its anticipation and in the event. On this view the sciences then become a series of sites of local practice, some of which is translated into the standard forms of public discourse, in publication, and then retranslated by readers and adapted again to local practice at self-selected other sites. Less may be left implicit, and additional personal and contextual information is carried, by the "informal" methods of communication which mediate local projects and international publication. But both methods of communication are screens as well as conduits of information.

History and Background of the Volume

When the planning of this volume began in the spring of 1977, it seemed a natural part of the mandate for the *Yearbook*. There had also been a number of more specific calls for deeper studies of research in social and historical context (3). These calls can be seen as giving permission and legitimacy to ask questions otherwise seen as irrelevant, or even disrespectful, and as attempts to develop new perspectives from which to ask and to answer them. The implied and expressed irreverence toward traditions and institutions of great respect may have prolonged this process of initial apologetics. In any case, in May 1977 the theme of 'The Social Process of Scientific Investigation' was proposed to the Editorial Board for Volume IV as "the heart of the subject." That is, the ethnographic and detailed historical study of actual scientific activity and thinking at or close to the work site. The plan called for one or two conferences of authors presenting papers in response to a "theme paper" to be written by the present author. Papers were also solicited by his trip to most of the known centres of social and historical studies of science in the U.K. and attendance at the ISA World Congress in Upsalla, Sweden in August, 1978. The first conference was held in Bielefield, West Germany, in June 1979. As it turned out, there was more work being done than was anticipated, and it was better developed, especially in England. The response was so strong that the volume was filled by the papers given at the Bielefeld conference.

Naturally, all this work, already underway and separately conceived, could not be fully integrated. However, our hope is that the selection and self-selection of authors, their responding to a theme paper, and their interaction during and after the conference has given the papers greater coherence, integration, and improved quality. The editors believe that this set of papers represents the majority of the empirical work on the conduct of scientific research today.

The Phrasing of Issues and the Revision of Assumptions and Language

As mentioned, the first challenges to established conceptions of science raised issues concerned with some of its external features, its institutional and class location and support, its professionalization, etc. Social factors were asserted as important as technical factors, knowledge was supposed related to social conditions rather than absolute, etc. (4) This is not to outline the first set of issues in the emergence of the sociology of science, but rather to point out that they were phrased in the same terms, and made many of the same assumptions, as the perspective they challenged. Both assumed that social factors were clearly separated from technical ones, for example. The argument about whether technical factors were temporally and logically prior to social ones or vice versa was impossible to solve and not very productive. It constrained formation of genuinely new problems and the new observations needed to resolve them. These traditional issues can be grouped into three assumed dichotomies: data/theory, objective/subjective (includes technical/ social and internal/external), and explicit norms versus implicit coutner-norms of science. (5) These issues and terms are reflected in the following papers, but the significant move is the attempt to effectively bypass them. The focus is not now on the priority of data or theory but on pattern conception and recognition (De Mey) and the utility of analogy and metaphor during research (Knorr). The debate over True/False versus relativity has weakened into whether scientific ideas can be treated "symmetrically" with ideas from other cultural areas (Harvey, Pickering). And the technical versus social aspects of research are now seen to be separated *subsequent* to the research by participants in the course of making persuasive claims for its carefully constructed results (Woolgar, Latour). Nor are norms or counter-norms seen as terms of explanation of behaviour, but rather as selectively used terms of persuasion into which accounts of behaviour are rhetorically cast. (Gilbert and Mulkay)

Some authors have gone another step to invent new language to conceive processes that fall outside or cut across existing concepts. Some of these terms have been borrowed or adapted from economics, as "investment", or "symbolic capital"; a scientist is seen to invest his symbolic capital (credibility, reputation) together with other resources in a local research site. While on the one hand this merely extends the use of economic terms already present in scientific vernacular, as in "making an investment" in a line of work, it also allows the identification of elements otherwise separated into technical versus social at the beginning of the analysis. Rejoining them cannot then appear as a problem in the analysis and as a solution at the end. In the same way, "practical reasoning" has been found useful in describing activities somewhere between formal logic and trial and error, or in finding the implicit reasoning occurring within trial and error. And, "account" has been borrowed from symbolic interactionism and ethnomethodology to replace "theory" or "history", avoiding an assumed reproduction or description of what is "really there" or "really happened"; an "account" is someone's version of what is there or happened, neither irrelevant nor absolute.

More improvization of language is evident in "qualitative logics" or "heterogeneous reasoning" in the attempt to pose the question more precisely of just how relationships and patterns among events are perceived and articulated than "hunch", "insight", and "hypothesis" allow. (Latour, 65–66) In the same way an "idea" becomes a "speech form" in order to avoid the assumption that cognition ever was separate from social context, and in order to be able to say that only later, after the event, are elements isolated and attributed to individuals, times and places. (Woolgar, 247) Finally, I will just mention the metaphor of the ventilation of a building for conceptual change used by De Mey. "With the removal of a 'solid soil', the metaphor of founding scientific theories on the rock bottom of observation becomes empty .... 'ventilation' would emphasize that observation enters our conceptual edifices at several floors simultaneously." (De Mey, 20) Thus, these innovations become fertile sources of new, more specific questions and observations unavailable in the traditional language.

Contributions of this Volume

The first thematic claim of the present authors, taken collectively, is precisely the need to revise the language by which science is discussed: "idea," "logic," "insight," "theory," "problem," "anomaly," are brought under review. There is considerable variation among authors, with perhaps the minimum claim being that traditional categories, especially "social versus technical factors," should be seen as integrated in the investigative process itself, that factors later identified as "technical" were grounded in and made possible by social factors (Harvey, Pickering, Pinch, Barmark and Wallen). As one example, Pinch points out that in his case, because his protagonist needed a new, expensive apparatus of only temporary utility he needed a strong theoretical rationale, which in turn influenced the design of the experiment, his collaboration with an astrophysicist, and the conception of the results. (84–86)

Other authors are more ambitious in their revision of language, and in fact, taken together, they suggest an outline of a potential perspective which assumes that scientists pay as much attention to the selection and construction of their language as they do to the construction of equipment or any other aspect of the conduct of research. To Callon, Gilbert and Mulkay, Knorr, Latour, and Woolgar, scientists are literally constructing their world rather than merely describing it.

The imagery of the "construction of our world" rather than "describing" or "explaining" it marks a more important shift of assumption than may be apparent. Beneath the face assertion that the verbal and symbolic aspects of culture are not that different from material culture lies a more basic revision of the legacy of "Knowledge" being conceived as a separate verbal and symbolic high culture, autonomous and possessing some special force, the power to reveal, order and enlighten. The present visual and spatial imagery locates "Knowledge" outside of people and outside society, somewhere "out there", a heritage of theological and philosophical traditions. Now, "Knowledge" is being brought down to earth, demystified as a human construction, in the natural as well as the social areas. At the same time, the new imagery revises the exceptional status previously given to natural science knowledge as also "out there", but as closer to physical reality ("more objective"), less distorted by its human vehicle.

This shift of imagery has at least three dimensions. A high/distant/formidible image is replaced by a low/close/everyday one. An abstract/spiritual/perfect image is replaced by a physical/imperfect image. And an impersonal/universal/permanent image is replaced by an image of personal work carrying the marks of the craftsman and his time and location of work, which is assumed to be of temporary utility and validity. All of these dimensions were carried in Ravetz's image of science as craft work, which is why his work was so suggestive for many researchers on science (6).

Knowledge is left to be only a part of a craft skill, existing in a person, at a place and time. Its (printed) means of publication may force it to be formally, abstractly stated, or there may be other advantages in such lean statements. But these forms are clearly not all of the skill of doing science, and cannot communicate the entire skill. This skill can only be recreated by people at work at a place and time, whether from master to student or colleague to colleague.

The ability to translate formal, printed statements into locally useful skills and back again may be as formidable as the skill to invent new skills. We could see then at least three phases: (1) The translation and amplification of formally stated "Knowledge" into particular skills; (2) The revision, invention, and improvement of present skills by new particular skills; (3) The translation and reduction of new particular skills into formal, publishable "Knowledge".

What our authors have done, in effect, is to look at these phases as problematic and contingent, rather than as routine, automatic or logical. This brings into view processes left implicit before, and gives rise to a range of questions never posed before. Our authors give most attention to phase (2), the revision and invention of technique, skill, and accounting for what happened at the work site, previously ignored or underplayed, left implicit or even mystified – "genius", "talent," "hunch" serving to divert the question of origins. Scientists seem to have believed, and philosophers have often stated, that scientists could speak directly – citing evidence and logic – to each other in a published form assumed to autonomously convey "Knowledge". This conveyance is now seen as problematic and complex, and to require much other work – "the social construction of reality."

A second consequence of this expanded problematic is that the possibility is raised that the psychological attitudes required of phase (1), the closest approximation to programmed learning, might hinder abilities necessary at phase (2), as in the commonly cited difference between "problem solving" and "problem finding" abilities. Third, the habits and conventions of phase (3) ("writing") might hinder phase (1) ("reading") becoming so elliptical or deepy coded that only very experienced people can make the translation back to what the article means "in practical terms". Some graduate departments have in fact developed special courses in how to read scientific papers (7).

Finally, an issue implicitly raised by the persistently drawn contrast between "practical reasoning", associated with actual research, and formal logic, associated with published argument (Gilbert and Mulkay, Woolgar, this volume, and Knorr (8, 9) is how significant is the manipulation of forms in extension from and in some independence from practical experience, say by theorists or mathematicians? In what ways and when is it productive and when not?

The papers here also provide opportunities for comparative case studies. They are not formally symmetrical case studies allowing precise or conclusive comparisons, but rather each was done for its own purpose and by its own design. Suggestions do emerge for example from the juxtaposition of cases where innovative or non-orthodox research results in open controversy (Travis, Pickering, Harvey) versus private or informal contention and negotiation over the meaning of results (Pinch). A committed protagonist who believes in his own results carries more potential for controversy, which in the latter case the major parties went to great length to avoid, than one with the posture that the theory "at least deserves a test" (Harvey). Another condition is that when an accepted, respected theory is contradicted by a well-conducted experiment, the experiments may be discounted by the experimenter himself (Harvey, Travis) or they may be allowed to stand (Pinch); the particular circumstances by which the latter may be accomplished without public controversy are described by Pinch. A third, more obvious dimension here is that of methods judged "softer" (Travis) versus "harder" experiments (Pinch, Harvey, Pickering). There are here other, complicating contrasts, the depth of challenge to cultural assumptions, as in the relation of animal to human learning, and unorthodox, humourous, non-serious presentation of findings in the former case.

As a third type of contribution to the sociology of science, our authors make more specific observations relevant along the course of the process of investigation. It is the major point of Callon's paper that "the problem" is not the origin of the research, nor purely cognitive, but rather has also to be created in a "core area of suspicion" where previously separated social versus technical factors are fused for re-analysis; the contrasting purposes of rivals gives rise to sharply different research problems and programs, in spite of their having the same apparent technical givens. The presence of an adversary position, in fact, is often useful to an investigator in formulating his views more completely than he would have otherwise done.

The intimacy of the relation of the intellectual state of a field and the social organization of a large-scale project is demonstrated by Barmark and Wallen's case where an administratively centralized project is mismatched to the cognitive dispersal of the various biological fields involved. Realignment then required considerable struggle and reorganization. The social organization of research also gives rise to special social roles. Barmark and Wallen identify the role of "the generalist" in interdisciplinary groups who must understand both cognitive and social problems in order to translate, mediate, and co-ordinate among the specialties involved (231). This is parallel to the "idea man", known locally and consulted frequently although the importance of his contribution is not reflected in co-authorships or even in acknowledgements and footnotes, and to the "super-technician" identified by Latour and Woolgar (10).

Finally, at the writing end of the process, Gilbert and Mulkay in contrasting the formal, published accounts of research with the far more varied and contextual accounts given in interviews, point out that the published accounts do not aim at genuine objectivity (such as a fair statement of an opponent's arguments) as much as seek a subtle, veiled method of persuasion. Gilbert and Mulkay do not speculate upon why scientific style has the particular form that it does, but this reader is tempted to say that "the best offence is a good defense": to put up a hard shell and not offer any unnecessary openings. For example, all opening for *ad hominem* kinds of suspicion are routinely omitted and thus the burden of raising them rests with the potential critic. (277–278). Thus, formal scientific communication would seem to proceed by implication and inuendo – a "high context" type communication and culture in Hall's terms – and nearly the opposite of its self image as having "the mass of the information vested in the explicit code." (11)

Remaining Issues in the Sociology of Science

The older versions of the internal/external factors debate on the causes of the conduct and progress of science seems here to have been safely bypassed, and the technical/social factors dichotomy has been softened to the point that it no longer appears interesting to insist on the priority of one over the other. Still, this social determinist phase of development in the sociology of science veils a residual problem: What constraint does the perceivable environment put on what can be said about it? We have no systematic account of how the perceivable environment enters its investigation, along with intellectual and professional "interests", "opportunities of success", etc.

If the positive programme for science could be said, for present purposes, to have made environmental objects the sole source of the content of knowledge, the sociology of knowledge (insofar as it was extended to natural science), could be said to have assumed that men could and would make of their environment what they wanted. As a third alternative, the "strong programme" of the Edinburgh school brackets the environment for science, saying that the environment is no more or less available to or closely described by science than by any other area of culture. A fourth "interpretive" or "constructivist" school (Latour, Knorr, Woolgar, Gilbert and Mulkay), if we can give them a name for the moment, account for the opposition of social to cognitive-technical facts as an artifact of the research profess itself. They argue that all possible facts are potentially considered by research strategists, resources at hand, career considerations, the credibility of other scientists' techniques, etc., during the research process. At the moment of a convincing construction of a working artifact, they begin to sort "technical", credible from "social", merely situational or personal elements. After this split, the "technical factors" (professionally convincing) are seen as the primary or exclusive causes of their own behavior and of the derived results. Now, presumably, in this latter case, perceivable, inscribable elements of the environment did enter the complex process of construction, and are partial determinants of its final shape, even if not systematically accounted for. So, we still have the question, what was this role and what constraints did it put on what happened and what was produced? A parallel point holds for the "strong programme". After the descriptions have been given of the various interests or other social factors which entered the process of research, the formulation of results, the presentation of arguments, where are the perceptions made along the way? Are defeated theories (colour) potentially as useful, suggestive, etc. as victorious ones (charm)? Could we go back to Lamark, phlogiston, fixed continents, and made those work too? It is one thing to refuse to put theories into true and false boxes before the search for the occasion of their invention, no doubt a useful heuristic. The risk is at the end of the investigation to insist that they bear an equal or underterminable relation to the environment they purport to describe, and to close off another range of answerable questions. To include the perceivable environment as part of the process of research takes nothing away from its social and cultural nature but would add to the realism and credibility of our accounts in the longer term.

In these last pages, I will try to give some perspective on where we are at the moment in the sociology of science and at the same time some personal worries about possible difficulties. We are now well into a second, corrective phase that is saying that science is like everything else: social, built from the same faculties as common sense, also rhetorical in language, etc. As informative as this has been, eventually we will enter a third phase, asking again "How and why is science different?" Why is it accumulative, in the sense that each success opens new opportunities for further achievements? Why is it coherent, in the sense that many achievements in different specialities have been brought into coherence, and places of contradiction have often been informative? Why and how is science able to change itself, in an apparent bootstrap operation where some more certain aspects are brought to bear on and change others which new knowledge then is brought to bear upon and to change the original certainties? Otherwise said, how are new observations of low confidence brought to bear upon and to change "Knowledge" of high confidence? And, of course, why does science have large practical impact and utility? The items behind the fascination and celebration of the reverent phase of science studies will have to find a (rephrased) place in a third phase.

Another aspect of this problem of dialectic type development in science studies is that while we have had changes in the theory of science we have also had changes in science. This problem is activated by the fact that most of the close data of day-to-day scientific practice is available only through current ethnographic type studies. These have naturally first occurred in large-scale, high-technology research in the core of our current national/professional science system. Now, how many of the features currently being read into the nature of science, e.g. careful calculation of risk, direct concern with recognition as "symbolic capital", high attention to the current trends in the direction of research, etc. are actually particular features of our national/professional system? For example, a large part of the support of science, in some areas virtually all, is poured in through its own prestige system. This is done through the peer review system, and the general system of allocation of funds according to the "reputation" of specialities, universities, departments, and individuals. All resources for research, career, and livelihood being channeled through the prestige system, participants are naturally highly conscious of being recognizable in that system, and, may even be somewhat "agonistic". Perhaps further studies of specialities less tightly geared into the national support system, perhaps such as animal ecology, will serve as a partial corrective (12). Another might be renewed interest in detailed historical studies (13) and the attempt to phrase accounts of scientific practice congruent both with historical and current data.

Several authors have used economic metaphor to describe current science. At least one (Pinch, 77) finds the use of economic metaphor frequent among scientists themselves. "Production and re-reproduction," "investment of resources", "symbolic capital" etc. have made insights available into relations among resources, the exchange and conversion from one kind of research resource, such as scientific "credit", to another, research support, or publication in journals, etc. That is, the economic language has allowed the juxtaposition and interpretation of elements otherwise segregated into "intellectual," "professional", "institutional", "cognitive", etc., categories. Less explicitly, economic language has allowed the translation of the semi-mystified or sacred language of institutionalized science into a secular language. Thus, behind apparent noble motives, "love of truth," or "curiosity" can be detected the down-to-earth motives of seeking recognition, prestige, and career advancement.

I hesitate to see this economic language become too well entrenched. From left to right of the political spectrum it is our natural, conventional habit of mind, and the translation of scientific into economic ideom is being readily worked through. But it allows a limited view of human motivation, reduced to various forms of career interest. In the amateur and academic eras of science, "personal interest", "curiosity", and "significant" were used as pervasively as the terms of career interest are now. Even though phrased in the terms of a then current system, did not these terms point to something (just as "interest" does now)? Are the previous sets of motives, although they might be better described, merely missing now? In short, we tend to assume a professional system of science, and hence use extensively the same basic terms as participants. (Properly and persuasively argued against by Latour and Woolgar in *Laboratory Life*) (14) If the system of science is changing at the same time as the framework we use to describe it, what appears as a theoretical advance and more accurate description can also be participation in the adjustment of its language to new circumstances.

On the other hand, our language for cognition and emotion in science seems less developed than that for exchange and negotiation. Only two papers apply analogy and metaphor (Knorr) and Gestalt psychology and the imagery of artificial intelligence (De Mey) to the empirical analysis of cognition. On the side of the emotional life of science we seem still limited to conventional language of little new informative power, "bias", "commitment", "Interesting", etc. And no paper takes up the topic here, nor was one offered. This seems to me a front for future development (15).

These papers are largely sociological in orientation, and their authors tend to see psychological factors as emphemaral, trivial, or methodologically difficult. Perhaps this is a corollary of the advance of a sociological point of view. But, whatever else it is, science is certainly intellectual work with a high degree of personal involvement. The nature and dynamics of that involvement seems a likely influence on the nature, direction and pace of its development. Most obviously, a particular blend of emotional, observational, and cognitive elements in the origin and emotional dynamics of the process of investigation becomes characteristic of a scientist and his style of work. There is the well known urge to interpret, to see connections, to locate phenomena in a larger picture versus the that to pin down and to test, to be "right rather than interesting". Among observationalists, some prefer to survey, to develop an array of observations while others prefer to make a close study of one or a few cases, as in the field versus laboratory sciences. Among theorists there is the difference between those who want to probe for deep structure, for the mechanism behind the appearance versus those who develop a synthesis, a model to explain a wide range of data.

But these direct satisfactions, reflected in the choice of discipline, speciality, problem and research style and not the subject of as much attention as occupational and career motives. Yet, every personal document by an active scientist I have seen witnesses the strength of direct satisfaction in scientific work. How are personal and instituted motives combined in specific pieces of scientific work? (16) Are strong personal motives related to moves to new specialities, to later but greater achievement, to the length of time research is pursued without tangible success, to the kind of intellectual change that is sought? If one sees creativity as ultimately emotional in origin and in drive, the topic takes on basic importance. These or similar questions seem likely prospects for further phases of research.

At the other end of the social scale from psychological dynamics, case studies of research projects, programmes, or specialities can only with difficulty place their events in a larger social and political context. Barmark and Wallen illustrate some of the constraints and limitations of national planning policy on the design of research programmes. And Pinch is apprehensive about the effects of large size and the high cost of experiments on the openness to challenge of modern physics projects. With rising costs of experiments only people of high initial credibility will be able to do them; they will be too expensive to duplicate, and thus not subject to dispute: "... the market for credible information becomes dominated by large and expensive projects which produce almost guaranteed credible information" (103–104). Both Barmark and Wallen and Pinck raise the larger question of what will be the longer term effects of the contemporary concentration and centralization of the support of science. A period of a retrenchment after the growth which put this system into place would seem to accelerate any conservative tendencies of the system. Only at the local level can the subtle effects of such trends be perceived. Other studies here, not explicitly drawing out these implications, might be read in this context.

My final point concerns science as a contradictory process. We may be in some danger of moving from an older straight forward view of correct method producing evidence, and intelligence producing theoretical discoveries, to a new straightforward view of recognition leading to grants leading to purchasing equipment, which produces data, which produces arguments, which produces articles which are read giving recognition, all in a "credibility cycle" (17). But science is historically too late, too rare (one origin point), and too problematic in its discoveries to be produced in such a straightforward way. Science as a contradictory process is brought into view by seeing it as an effort at purposive cultural change. Cultural change is not only sought but also resisted; the sources and occasions of this resistance should be viewed not as irrational deviations but as essential parts of making social life and science work.

Certain of the traditional dilemmas of the interpretation of science can be seen as artificial, as dilemmas of interpretation rather than as inherent to its process. Thus, the division between logic and empiricism stands in the way of a social account of discovery. The problem of the moves between perception and conception has been dealt with through the concepts of "metaphorical innovation" (18) "analogical transfer" (Knorr), "pattern recognition", "ventilation" of observation and conception at several levels, (De Mey, 3), "perspective shifts", and now the current proposal of "metacultural change". These concepts invite attention to changes in terms in which a problem in conceived, to the shift and redefinition of categories as basic events in science. They begin to develop a language for the analysis of scientific advance as not only the addition of items of culture, but also as changes in its basic terms.

A second example is the by-passing of "internalist" (explicit logic and data analysis) versus "externalist" (class, organizational, career) factors for a return to the core topic in science, its cognitive structure, but now in the social and psychological context in which it occurs with concepts which do not force the social/cognitive separation in the first place, such as "opportunities for success", (Knorr), "convergence" of lines of research and "discovery" as an interpretative as well as observational process (19).

Other dilemmas would appear to have a basis in the practice of science and some practical devices or implicit mechanisms have been developed as ways around them. Identifying these dilemmas reveals science as not a natural or easily contrived system, but rather as partly fortunate accident and partly ingenious innovation. Nor need it be seen as automatic once set in place nor necessarily yet complete or perfect.

First, there is a certain tension between the importance attributed to ideas and their being open to empirical challenge. This appears in Lakatos' contrast of the "hard core" of informative ideas and the "protective belt" of auxilliary hypotheses (20). It also appears in Callon's division of a problematic situation into an outer area of unanalysed assumption, an inner "net of certainty", and a core "area of suspicion", where social and technical factors are "fused" and thus open for reconception. In the outer rings the social and technical are separated and thus both are solidified. Concepts felt to be more meaningful, more informative, and interconnected logically are then seen as *barriers* to new perception. This disjunction between the more emotionally significant and the more empirical terms helps to explain the elation people feel on the discovery of terms which both seem significant and are empirically relevant.

There is a parallel tension between emotional involvement in a problem and the ability to obtain perspective on it, to be able to integrate its particulars into one holistic view. Sometimes temporal and spatial distance give also enough emotional distance that another perspective becomes possible and a switch to a more informative image is achieved.

Since the established categories have not solved the problem, this must be a discovered association or invented pattern. One plausible route to a newly recognized association or pattern is a felt or emotionally based similarity. There is dual need for emotional involvement and distanced perspective, the wish to solve the problem and the distance to see the pattern.

The final dilemma to mention is between our need to see in patterns and the need for accuracy. The first images are inevitably vague or incomplete and analogies inexact (21). If initial images are vague and partial, but nevertheless the necessary source of new thought, the problem of resolution between initial image and the empirical and problematic referrent must be great.

These dilemmas or contradictory processes make it difficult for two necessary things to happen at the same time. We can here only briefly suggest how these dilemmas are sometimes bridged in scientific practice. First, one could suggest recurrent phases of conviction in newly perceived relations or patterns followed by phases of criticism and reconciliation of the new pattern with new and older data and data taken as given knowledge from neighboring specialities. Safan-Gerard's discussion of the phases of perception, elaboration, expression, and evaluation in art show how empirical relevance can be brought to bear after meaningful insights are obtained, how distance and perspective is later obtained on ideas conceived in passion, and how refined, well elaborated constructions are developed from initial images (22). More obviously, different types of research – exploratory versus developmental – and research strategies – broadly observational versus precise, restricted, specialist research (23) advance one side or the other of the contradictory demands of innovation. The same is true of different types of people and styles of work, for example the difference between people inclined to be enthusiasts or advocates and those inclined to be critics, or the different cognitive styles apparent in those inclined to "social differentiation" versus "remote association" abilities (problem finding versus problem solving) described by Lodahl and Gordon (24). Finally, some of the organizational features of science would appear to play a role, such as the duplication of research, which allows a pattern to be perceived at one time and place and to be established at another. Ben-David's argument for the key role of decentralization of research and its support and the key role of marginal social roles could be linked to these dilemmas in the same way (25).

It is our hope that this first substantive set of empirical studies in investigative practice renews and significantly advances the question, "What is science?". It is renewed by seeing science not as a category of exception defined in advance, but as a human social construction in its elements like any other. This perspective requires some new language for its developing programme of studies, an "alerting framework" for the observation of events previously ignored, but now seen as highly significant. Some older, much argued issues and their language of phrasing will have been bypassed – such as internal versus external factors, social versus cognitive factors, and accumulative versus revolutionary change. And new themes and issues have emerged, – such as the interpenetration of publication and investigative practice, and how the perceivable environment enters the construction of scientific accounts.

As a third phase we can project the development of and hopefully the incorporation of a psychology of science, an attempt to come to terms with the particularities of the contemporary system of the national support and organization of highly professionalized research, and the impact of larger social structures on the special arrangements of scientific institutions and on the details of local scientific practice. It seems that science studies have now decisively entered a new and rapidly moving phase of development.

Acknowledgements

I would like to gratefully acknowledge the support of the Social Science and Humanities Research Council of Canada, including a sabbatical leave grant and research and travel support, the hospitality, office and other facilities during the summer of 1978, given me by the Department of Liberal Studies of Science, Manchester University, and the continuous support of McGill University for the several trans-Atlantic trips this project has involved.

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Notes

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4. See Krohn, *op. cit.*, 1977 for an historical outline of these issues in the sociology of science.
5. See *Ibid.* for a discussion of this grouping of issues.
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15. See Michael J. Mahoney, 'Psychology of the Scientist: An Evaluative Review', *Social Studies of Science* 9, (1979) 349–376.
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