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SCIENCE & TECHNOLOGY STUDIES

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... And Then There Is The Process

One of us was innocently asked in Pittsburgh, "Is *Science & Technology Studies* ever going to publish refereed articles?" The reply was "Certainly, just wait until the next issue." The future is now. And with it comes another of our experiments to sharpen the editorial process that culminates in publication: publishing the names of endorsing referees.

There is a scholarly history to this "innovation." Before we summarize it, however, we wish to report on the process we used to elicit "endorsements" of three articles. A thank-you letter was sent to the referees of those articles including the following paragraphs:

We have decided to publish a version of the manuscript. Attached is that version. We would appreciate your perusing it. If you are confident that its publication represents an addition to the literature that will be valued by the *S&TS* readership, we would like to include your name among those on a list of "endorsing referees" that appears at the end of the published paper. This endorsement does *not* constitute agreement with the author's point of view or even the results of the analysis. Rather, it is a statement of scholarly quality, that standards of rigor and integrity have been observed in the reporting of the research.

If you dissent from this view, or would like to discuss this form of endorsement, please call either of us. If, on the other hand, you are comfortable with our intent and the paper in question, please sign, date, and return (for our records only) the form at the bottom of this page. We realize this is an unorthodox request. We think *S&TS* must be an unorthodox journal at its core — its referee system. Thank you again for your service and support. Regardless of its public acknowledgement in the way we propose, we want you to know that your contribution is valued.

Look for the names of "endorsing referees." Look, too, for "letters to the editors" from referees and others beginning in volume 5. As always, we welcome them, since one of our goals is to increase communication among our contributors and readers.

As for history, Stephen Lock, in his 1985 Rock Carling Fellowship Lectures published as *A Difficult Balance: Editorial Peer Review in Medicine* (ISI Press), devotes a chapter to "Betterment: Improving Peer Review." In it (especially pages 124-25), he discusses the pros and cons of "anonymous" refereeing. The arguments are instructive and we present them here; we urge you to consult the literature he cites. Then decide for yourself (if you can):

... to keep the assessor anonymous is almost as objectionable as his recommending rejection of an article without giving reasons. Open referee-

ing would enable authors to judge how competent a reviewer was, and would promote higher standards of peer review. Nevertheless ... 'editors prefer anonymous refereeing because it is easier to run a journal where exchanges between authors and referees can be controlled and if need be subdued, not because it is necessarily science that benefits from such anonymity.'

... the main reason for preserving anonymity is to avoid introducing more subjectivity into what should be as objective a practice as possible. The other side of the argument is as strongly put by Ziman, who denounces the call for disclosing referees' names as 'populist folly.' Anonymity is better for all concerned: for the referee because he does not have to mix emotional factors with intellectual judgments; for the editor, who gets a more honest guide to his decisions; for the reader, who gets a more reliable and better expressed paper that has been subjected to a higher standard of criticism; and for the author, who when his mistakes are pointed out can vent his chagrin on an impersonal critic.

Lock concludes that:

Another possibility would be to name referees either in a general list acknowledging their help over a year — a practice which several journals already follow — or by printing their names at the end of the articles. ... [T]his might pin some responsibility on the assessors in cases of piracy, plagiarism, or fraud. To my mind, however, it would abrogate editorial responsibility and give far too much apparent power to the referee.

To our minds, endorsements are the stuff of communality. Surely some referees will always prefer anonymity; others routinely sign their referee reports with explicit instructions for transmission to the author; some even copy their reviews and send them directly to authors. We seek a comfort zone; but this may vary not only from referee to referee, but from manuscript to manuscript.

The problem with peers is that they are neither created nor function as "equals." Seniority and world views are precisely the "populist folly" that, to use Lock's phrase again, "mix emotional factors with intellectual judgments." We do not pretend to possess the capacity to detect and separate these on a regular basis. What we do claim is the editorial prerogative to interpret referees' recommendations and sometimes act in contrary fashion. Publishing the names of endorsing referees makes the process of editorial decision a bit more open. We hope it will heighten participation in the scholarly life of *S&TS*. At least we can monitor the experiment together.

DEC/SEC

The Mutual Dependence of Risk Research and Political Context

Arie Rip

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Editor's Note: A version of this paper was presented at the 1985 4S Meeting in Troy, NY. This was the meeting at which S&TS was christened the successor to the 4S Review. It is also where we began to invite submissions to the fledgling publication. The author responded in time for our inaugural issue, but alas, we could not justify the space until now. As both editors and reviewers, we thank him for his patience as well as his "historic" contribution.

If social studies of science are right in claiming that social and political contexts influence not only the rate and direction of scientific developments, but also dominant problem definitions and the interpretation of research results, it must be impossible for science to deliver fully independent scientific advice to the policy process. This will especially be the case when the science is uncertain and/or geared toward decision making. Still, political demands for independent advice have not decreased, and the need seems to be most urgent in areas of "public" and "regulatory" science, where uncertainties, as well as possible socio-political influence are the greatest.¹

The threat of circularity can be countered in different ways. Besides dogmatic assurances that science is, by its nature alone, objective and independent of context, solutions have been offered of an institutional or procedural nature. Independence is created and maintained by separating scientific assessments from socio-political assessments procedurally, for instance in science court proposals (Banks 1980), the Consensus Development Conferences (Asch and Lowe 1984), and in the 1983 U.S. National Academy of Sciences proposal to distinguish between risk assessment and risk management (NAS 1983). Historically, professionalization has given rise to independent, and certified, advisory roles, for instance in radiation protection (Serwer 1978), and such professionalization has often occurred in response to a demand for expert advice that would resolve conflictual issues, e.g. water quality measurements in environmental debates. Increasing pressures on expert advice have changed the rules of the advisory "game" and made the notion of independent, objective, advice less plausible (Rip 1985).

Institutional or procedural independence as such may be a necessary condition, but the real issue is the quality of the scientific advice. In itself, this raises difficult questions, since the production of quality

within science is now recognized to be a bootstrap operation. Advances can often only be made by assuming the validity of at least part of the thesis to be tested. Lavoisier's famous experiments to disprove the phlogiston theory by weighing reactants and products rested on the assumption that any weight deficits were due to the production of oxygen. Although one might argue for interpretive flexibility and thus for some form of relativism, this is not the main issue here. Rather, the controlled production of effects, even if successful only within the protected confines of a laboratory, allows science to make robust claims. As soon as there is "outside" utilization of results—this is at least the aim of regulatory science and policy science—there are additional criteria that break through interpretive flexibility—for instance, "fewer people dying" (although even this is negotiable). Even with these additional criteria, the negotiation—at least the visible negotiation—about results appears to be more, instead of less intensive. This by itself can be explained from the particular criteria (fatality rates, for instance, apply only probabilistically) and/or by the large interests that are involved. The implication, however, is that the production of quality (including its control and improvement) will be more difficult, not less. It also implies that sociological analysis of the processes of quality production cannot be a purely academic exercise; implications for the actual practices of scientific advice should be drawn.

These considerations are generally applicable to scientific advice and the research underlying it. The special case of risk research, and in particular risk analysis, is of interest because some of the dependencies between science and (political) context are particularly striking, and provide a way of addressing the general problem. This paper addresses two issues. First, it traces the evolution of risk research in its social and political contexts to show some of the dynamics of scientific and professional activities of risk research now emerging. Second, the paper discusses some particular cases of risk analysis to show how it is part of a bootstrap operation. In this way, a certain perspective on the mutual dependence of risk research and political context can be developed that allows implications for the possibilities and limitations of the contributions of risk research and researchers to be drawn in the final section of the paper.

Risk Research

Risk research comprises a cluster of research traditions, some well established, others newly emerging. These include risk analysis, safety and reliability engineering, risk perception studies, and risk decision making studies, to name just a few. Since the terminology is not yet definitive (e.g., risk assessment has various meanings), I will use the neutral and comprehensive term "risk research" to indicate a cluster sharing the focus on risk as a potential hazard to be avoided or contained. (Thus, studies of risk taking in business investments are excluded, although there may well be connections, both psychologically and sociologically.)

One could ask whether there is sufficient coherence between the several research traditions to speak of one (emerging) field. In any case, the shared focus on input to and improvement of decision making introduces a shared dynamic.

Take, for example, the definition of risk analysis as:

... the process of identifying potential hazards for individuals and society, and estimating their magnitude and probability of occurrence with the aid of statistics, experiments, analytic methods of various fields of science and engineering, human experience and judgement (Baram 1985: 37).

Risk analysis, thus, is a scientific field that focuses on decision making and its improvement. Other such fields are operations research, systems analysis, and policy analysis. They share a certain dynamic of development which differs from more traditional dynamics in science, like "experimental philosophy" and "natural history," and can therefore be thought of as "strategic sciences." One common feature, for example, is their emergence during and after the Second World War.²

Risk researchers share with other "strategic" scientists the lack of a "laboratory," i.e. no shelter against disturbances within which they can attempt to domesticate their object. They are out in the open and cannot defend themselves in political arenas by referring to their "laboratory" effect. Attempts at fundamentalizing risk research, for instance through impressive methodologies, can never be a solution, since a strategic science must maintain its relations with actual decision making. If a bootstrap operation is possible, it must be part of (and thus reflect) the mutual dependence of risk research and political context.

For strategic sciences, concern about their quality always leads to an ambiguity: is it the research or is it the decisionmaking that falls short?³ By reconstructing how particular decision-making, and thus political, contexts impinge on the direction and substance of risk research, it must be possible to overcome the ambiguity. To achieve such a reconstruction, one must first have an idea of the dynamics of develop-

ment of risk research in general. It is to this topic that I now turn.

Evolution of Risk Research: Five Roots

In retrospect, five different intellectual roots can be distinguished, each with its own social and cognitive make-up: risk analysis, safety engineering, hazard studies, actuarial statistics, and medical risks. The recent psychological, sociological, and decision-analytical studies of risk, still rather fragmented, may become a sixth.

Risk analysis, especially in its recent probabilistic form, is a very visible tradition because of the scientific and political debates surrounding it. Operations research and systems analysis were introduced in military and aerospace sectors to improve design and maintenance for enhanced and cost-effective safety. The tools developed this way could also be used to estimate extent and probability of failures and accidents in the nuclear sector, where a need for such analyses had arisen in the course of the 1960s: the safety of design and operation of nuclear power plants had to be demonstrated. The Reactor Safety Study led by Rasmussen, and its report WASH-1400 (1975) was a landmark in risk analysis (see below). In the 1980s, siting and operation of chemical (and other hazardous) plants and activities have become the main focus of risk analysis.

Less visible in government decisionmaking and public debate, but very important for the design, operation, and maintenance of complex technical installations and systems (e.g. air traffic), is the tradition of safety and reliability engineering. The older traditions of testing and designing "on the safe side" (e.g. in building bridges and other construction work) have incorporated operations research, and sometimes overlap with the risk analysis tradition. The actual development of safety and warning devices and the standard setting and evaluation of safety practices remain more separate. As historical studies show, a major stimulus for the establishment of government research institutes since the late 19th century has been the need to improve and standardize safety (Lundgreen 1985), and safety and reliability engineering is part of this larger development.

If the verbal repertoire of the second intellectual root is centered around the term "safety," the third focuses on "hazard": studies of natural hazards like earthquakes, floods, and tornadoes, and of strategies for coping with them (originating in social geography), now also including general catastrophes and crisis management. Thus, involvement from political scientists and urban and public administration scientists has been added.

A fourth intellectual root is actuarial statistics, both data collection and methods of determining insurance

fee levels. Professional actuaries appear in the insurance sector in the 19th century (Covello and Mum-power 1985: 19). The mathematical problems have interested scientists and mathematicians since the 17th century, and have become part of the field of statistics in general, which also contributes to risk analysis, safety and reliability engineering, and medical epidemiology.⁴

A fifth tradition of research and practice is medical, with toxicology and epidemiology as the central fields. Key terms here are mortality and risk groups, showing the relation with risk problems. The dynamics of these research traditions, especially after the Second World War, are complex and deserve analysis by themselves.

Studies of perception of risk, of acceptability, and of risk communication have increased since the middle 1970s,⁵ and if one adds conferences and comments, there is now a veritable flood of contributions, showing the concern with risk issues. Now that regulation and decisionmaking about risk issues is receiving increasing attention, a whole body of studies and comments may well be the beginning of a sixth research tradition. Until now, only the strand of psychological studies of risk perception, risk decisionmaking, and decision theory has shown clear continuity and some cumulation.

Changing rhetoric. Although the different intellectual streams briefly characterized here were overlapping and interacting to some extent in the 1960s, the pace quickened in the 1970s. "Risk" became the dominant term (before 1975, "hazard" was at least as common),⁶ and served as an umbrella under which a variety of research traditions could come together. The repertoire changed because of substantive, as well as symbolic or rhetorical reasons. There was increasing recognition of the similarities in different sectors of management and decision making, all concerned with estimates and reduction of probability of future hazard. The heterogeneity of different sectors and different scientific approaches is now contained by the use of a single term, and this alone creates an object (even if it was an artefact of decision or management needs at first) for a general field of study: risk research.

The rhetorical aspect introduces an additional dynamic. Authors want to include the term "risk" in their titles, where they expect it to symbolize a range of contemporary concerns and thus attract a larger readership. But to remain credible, such an article has to accommodate itself to some extent to the risk repertoire, and thus reinforce the status of risk as an umbrella term as well as give it some substance. Even if the precise meaning and implications of such a term remain contested, it enables opposing views to be articulated and can be used to define one's own standpoint. In this way it becomes a social reality.⁷

Professionalization. Further insight into the dynamics of the emergence of risk research derive from insti-

tutional and professional developments. As Mazur (in Conrad 1980: 152-53) notes, a "labor force" of risk researchers—aerospace operations researchers and safety engineers—sought jobs by the end of the 1960s, when the aerospace sector in the U.S. was reduced. They found them in the nuclear sector. After the large investments in risk research in the nuclear sector, the expertise (of individuals as well as of institutions) was put to use for other sectors, especially the chemical and process industry, which suffered from credibility problems of its own. Nuclear institutes have broadened their scope and are now interested in risk and safety issues in general.

Professionalization became visible for risk analysis in the second half of the 1970s. Rowe's *Anatomy of Risk* (1977) was the first systematic, book-length presentation of risk analysis. Its appearance marked a veritable explosion of conferences, workshops, and books published on risk assessment. Another indicator was the establishment of special sectors in existing scientific-professional societies (e.g. for operations research), and of an international Society for Risk Analysis publishing its own journal, *Risk Analysis*, since 1981.⁸ One should also note the publication of the *Procedures Guide* for risk analysis, published in the U.S. in the wake of the debate over the Rasmussen report and its parallels elsewhere (e.g. the Dutch *Kansenboek*).

The timing of this professionalization must be explained by both supply and demand factors. The demand side is becoming further articulated, partly through the debates on regulatory policies in the U.S. and the need to assess human health risks scientifically. The supply of risk researchers increases, and the existence of the common umbrella term, "risk," implies that originally separate domains will start to interact. The ensuing territorial conflicts and the need to define (and maintain) control are powerful stimuli for professionalization.

It may be premature to speak of a professional community of risk researchers. Insofar as there is one, it is clearly segmented. Safety and reliability engineers form a "hard" segment, with close ties to industry and utilities, while risk analysts orient themselves to decisionmakers and admit social scientists (in increasing demand) to their ranks. Reputational control is not very strong; much of the risk research being done is published in reports with limited circulation, and some of the material is proprietary. As in engineering communities in general, the important issues are quality control and the regulation of entrance to the (proto-) profession. The characteristic drive for quality control and its justification in terms of clients' and public trust is apparent in the following quotation from the report of the organizational meeting for the European Safety and Reliability Associates (ESRA):

How did the public react to the conclusions of those studies? How credible are the safety and

reliability analysts? How can they improve the quality of their work and become more generally accepted? International cooperation, exchange of the available know-how, better education and training, pooling of basic data, coordinated R&D: these are some of the answers that ESRA could supply (*ESRA Newsletter* 1985: 4).

Critical evaluations of methods and benchmark studies are forms of quality control that have been proposed and sometimes also done, for example, after criticisms of the Rasmussen report. Institutions with a central position in the professional networks (a prime example being the Joint Research Centre of the Commission of the European Community at Ispra, that also publishes the *ESRA Newsletter*) have been particularly active in producing such studies. The outcomes have not always inspired public trust. Risk analysts have come up with a counter-tactic, quoting studies that show that individuals and decision-makers do not change their choices unless there is a difference in risk level of a factor of 100. If this is the case, some risk analysts argue, variations in the results of benchmark studies within this range need not worry us too much.⁹

Insurance Sector. Developments in the insurance sector have been somewhat separate, the most important being the emergence of so-called risk management, i.e. management of insurance portfolios, in the 1970s. (Crockford 1982). There is a long tradition of fee structures related to the riskiness of the client (or his activities or property), which require a limited form of risk analysis (i.e. interpretation of actuarial statistics). On occasion, the promise of reduced insurance fees has been consciously used as an incentive for risk reduction measures. (An example is given in Kunreuther 1982a: 10.) Such measures imply requirements for risk assessment, often just on the level of data gathering, but sometimes also analysis and development of methods. Mazur (in Conrad 1980: 152) notes the similarity between the problem of the insurance rate-setters requiring information about riskiness, and the decisions requiring risk analysis input. In fact, some of the attempts to quantify risks of nuclear energy have been stimulated by the need to set some damage level for insurance companies to base their rates on (see below for a discussion of the Price-Anderson Act in the U.S.), while methodologists of risk analysis have argued that the main (or only) value of probabilistic risk analysis is its setting of limits on damage levels for use in determining insurance fees (Cooke 1982).

Relations between risk analysis and insurance are now becoming stronger, partly because of the emergence of "risk" as an umbrella concept, partly because of increasing litigation and damage claims in the chemical and process industries. Not only the industrial companies themselves face heavy burdens (in-

cluding bankruptcy, as in the case of asbestos manufacturers), but the insurance companies as well incur losses, and now refuse to insure in some cases. This leaves the industrial companies in an awkward situation, and a solution in which the government steps in, as was done for nuclear power plants in the 1950s, is improbable. The insurance sector itself might take the lead and demand risk analysis and risk management practices, in part to be able to continue to cover (and profit from) the large pollution and health insurance market (Baram 1985). There are indications that genetic engineering companies will face comparable insurance problems.¹⁰

In the health context, on the one hand there is a curious split between the internal medical tradition (with its own definition of risk as frequency of hazard, leading to a focus on mortality and risk groups) and assessments of fatality rates versus costs of treatment or prevention (including the recent introduction of decision trees to aid in the assessment of risks and benefits of, e.g., surgery versus other treatments). On the other hand, medical research has made contributions to the study of health effects of hazards, where there is great reluctance to integrate these contributions into the overall risk assessment. For example, in advising on standards-setting for chemicals in the workplace and environment, medical experts generally look only at the protection of individual and public health, while in the end the decision may be based as much on other situational, economic, and political considerations.

The compartmentalized way of handling risk issues is related to maintenance of professional autonomy and the way professionalization has developed in the medical field, with the state having the double role of authority and of client in the field of public health. When there is full professional control, risk aspects are elaborated, and there are incentives to improve both the analysis and the actual safety. When the professional is limited to a service role, he draws back to delivering "packaged" expertise.

Dependence on Political Context

From this brief sketch of the evolution of risk research, it is clear that the problem-definitions used by relevant actors in different contexts have shaped the direction and content of risk research directly. In turn, the expertise shaped by such problem-definitions will produce particular kinds of policy advice. The circularity is brought out nicely in the well-worn quip quoted by Otway: "If you have a hammer in your hand, everything looks like a nail" (Otway and von Winterfeld 1982: 139). There is also more diffuse interaction: the tacit knowledge and accumulated experience carried in the networks of risk practitioners will

pervade risk research, because the roles of researcher and practitioner are not sharply separated.

To illustrate the dependence of risk research on political context, two examples of bootstrap operations in risk research are discussed here: the evolution of risk analysis in the nuclear sector, and present trends to formalize and universalize risk analysis. In the first example, the context is that of the U.S. Atomic Energy Commission, nuclear industry, and insurance companies, and the debates and credibility problems surrounding the rise of nuclear power. In the second example, regulatory agencies, their increasing scope, and their wish to rationalize their decisionmaking are the primary context.

Nuclear Power. The first risk assessment of nuclear power in the U.S. was published in 1957. The report, WASH-740, was prepared by a team from Brookhaven National Laboratory, commissioned by the Atomic Energy Commission (AEC). The U.S. Congress (i.e. the Joint Committee on Atomic Energy) had asked the AEC to estimate in exact terms how many people would be killed and maimed, and what sort of property damage would result from a hypothetical major reactor accident near a large city. This request, in its turn, was motivated by the debate on whether the Federal Government should step in to provide so-called war-risk insurance, since private insurance companies were willing to insure only a token amount of damage. The utilities and nuclear industry refused to go ahead without adequate insurance coverage, a stance which threatened to put a stop to the development of civil nuclear power.¹¹ Congressman Price and Senator Anderson had proposed that there would be a maximum coverage of \$560 million, as much as possible to be obtained through private companies, but the rest to be provided by the Federal Government, up to a maximum of \$500 million. The insurance companies were putting up only \$65 million, and Congress was reluctant to have the Federal Government shoulder such exceptional responsibilities. Some assessment of possible damage was necessary to break through the impasse.

There had been safety studies before, and some of the early accidents (e.g. at the Canadian Chalk River heavy-water reactor on December 12, 1952) had led to increased safety measures in the design of new plants, as well as to attempts to trace possible accident routes and find out how to safeguard against them. Applications for permits with the AEC specified the design for safety, using as a criterion that no credible malfunction or accident could release radioactive products from the reactor. Here, the distinction between "credible" accidents and "hypothetical" accidents (i.e., so improbable that they were considered incredible and irrelevant) became an accepted tool in risk analysis. Accordingly, the Brookhaven team was asked by the AEC to estimate and quantify the damage

resulting from the maximum credible accident. Although there had been hopes that the picture would be reassuring, the Brookhaven figures were not encouraging (3400 deaths, 43,000 cases of severe radiation sickness, property damage of \$7 billion). The report, released in March 1957, did not draw much public attention, and in September the Price-Anderson Act was passed. Among the arguments for going ahead was the need to support development of commercial atomic power, and the conviction that the hazards could be kept low by engineering safeguards, monitoring (and maintenance), discipline in operating the plants, and regular inspection.

1967 revisions. The next phase was initiated by the renewal of the Price-Anderson Act in 1967. Plans for expansion of nuclear power (both in numbers of plants and in their size) made the estimates of WASH-740 obsolete, but many improvements in design and safeguards had been made since 1957. The AEC again commissioned a study from Brookhaven, but difficulties emerged immediately. Although the major task was to estimate maximum damage to people and property, an estimate of the probability of such a disaster happening was also needed. The Brookhaven team refused, arguing that there was too little experience to make rational estimates of probability. When pressure on them continued in later months, they used stronger terms, arguing that it would be "charlatanism" to make predictions about probability and that only "fringe members of the statistical community" would attempt to do this with the meagre data available (Fuller 1976: 141, 146, 148).

The issue had another side. As AEC staff noted repeatedly, the report needed a very strong section on the low probability of accident occurrence to counteract the expected effects of the hypothetical accident calculations (Fuller 1976: 155). In addition, various types of accidents needed to be related to their probabilities, so that liability experts could put dollar estimates on them (Fuller 1976: 149). Although Brookhaven continued to resist, others (no charlatans) made rough calculations. Among them was McLaughlin, an AEC radiation physicist, who projected three accidents in ten years, when the planned 1000 reactors would have been built; others concurred in his estimate. While Brookhaven was producing alarming figures for the damage of a maximum credible accident (many results would be 50 to 100 times worse than in the old report) and was bombarded with offers for help from the Atomic Industrial Forum (along with pressure from AEC to accept), the quest for low probabilities went on. A respected research group, Planning Research Corporation, produced estimates for the probability of a catastrophic accident during one reactor year, lying between a confidently asserted maximum of 1 in 500, and a low of something like 1 in 100 million. To reach the latter estimate they used

"chains of events" that had to occur before a catastrophe would take place, and estimates of relevant probabilities by experts (Fuller 1976: 162). Here, in essence, was probabilistic risk analysis.

Work on the draft report, with chapters on probability by AEC staff and on effects and damage by Brookhaven was started, but no acceptable formula could be found. The consequences of a major accident would loom large, and no assurance could be given that they were impossible, especially if one considered human fallibility (a point brought up by an insurance man) (Fuller 1976: 173). After extended negotiations, including consultation with industry representatives, it was decided to let the matter rest and not produce a report at all. A letter was sent to the Joint Committee, noting the main point that bigger plants, with higher inventories of fission products, would produce larger accidents, but that the chance of such an accident occurring was remote. The insurance hearings on the Price-Anderson Act were held in June 1965. Again, the pressure from utilities and the desire to see atomic energy developed made Congress extend the act without much debate (Fuller 1976: 186).

The history of WASH-740 and its ill-fated update show how elements of present risk analysis have been conceived and articulated as an answer to concrete demands and dilemmas in particular historical contexts. The techniques have then been developed further, more or less independently of these contexts. In the case of probabilistic risk analysis in the nuclear sector, the story continues. Neither credibility problems nor safety issues were resolved, and the Price-Anderson Act comes up for extension every ten years.

Rasmussen report. The AEC resolved to be prepared for the second renewal, in 1977. Besides launching a reassuring report on "The Safety of Nuclear Power Reactors" (WASH-1250), it commissioned a \$3 million probability study of reactor accidents in 1967, to be done under the direction of Norman Rasmussen of MIT. When the study was released in 1975 (WASH-1400), it was hailed as a landmark achievement in risk analysis. Partly because of the praise it received, and partly because of Rasmussen's known advocacy of nuclear energy, the report also attracted much criticism, both technical and political.

What the Rasmussen report did, in essence, was to take the elements of probabilistic risk analysis that had been articulated before and apply them systematically to a population of nuclear reactors. Thus, it used generic failure data for components and systems to calculate probabilities of events in the chains leading to a major accident. The study is considered to be the first modern risk analysis, even though its reception by the scientific community was somewhat turbulent. In 1977, Congress created a special review panel of (external) safety experts, led by Harold Lewis. Its report was fairly critical, although it endorsed the

overall approach as providing a logical framework for the discussion of reactor safety. (A review by the Union of Concerned Scientists rejected this very aspect.) The Lewis panel called information about relative probabilities of various accident sequences one of the achievements of the Rasmussen report, but at the same time criticized the inadequate data base and questionable methodological and statistical procedures, which led to uncertainty bands which greatly understated the actual uncertainty. The Rasmussen report had also been unable to quantify common cause failures (and thus in a sense neglected them). In response to such criticism, the Nuclear Regulatory Commission distanced itself (in January 1979) from the results of WASH-1400, by declaring that it did not regard the numerical estimate of the overall risk of reactor accident reliable.¹²

Lessons. What does this episode in the debate on nuclear energy and its risks teach us about the dynamics of development of risk analysis? First, that demand (from government, from industry, and from interest groups) is important, especially when it is articulated through conflicts and controversies about supply, as happened with the update of WASH-740. In such cases the articulation can focus on perceived achievements and limitations of the risk analysis available, and thus provide definite incentives for quality improvement. The aftermath of the Rasmussen report is an excellent example of work towards quality improvement (including its diffusion to other countries and other sectors). In this case, the existence of public controversy was an important element in the dynamic. Without it, many of the critical points could have been buried in internal memoranda.

Secondly, apart from the improvements in methods per se, there occurs "repertoire learning" (Rip, forthcoming): some outcomes, techniques, and criteria can withstand criticism and are accepted by professionals and decisionmakers and appear in public debate. For example, through the debate around the Rasmussen report, the idea of probabilistic risk analysis as such had become acceptable to decisionmakers by 1979 (even if it took longer for some affected groups like industry). More broadly, criteria for what constitutes an acceptable risk may develop in the course of such episodes. In the debate on acceptable annual individual fatality rates connected with certain activities, the actual numbers and calculations may be contested, but not the fact that one can base decisions on such a criterion. In many cases, such specific debates also reinforce acceptability of the criterion by improving coordination between designers and licensing authorities.¹³ In a sense, a shared culture is created.

Government Regulatory Departments

The second example of the mutual dependence of risk research and political context shows a different

mechanism at play: when demand and supply have similar aims, the dynamic is reinforced. This appears to happen with the risk analysis (and risk assessment philosophy) that is taken up by government regulatory departments. The bureaucratic aims of formalization (e.g. of procedures), of standardization (in contrast to ad hoc negotiations), and universalization (applicability to all, independent of particular local situations) find their counterpart in scientific aims. Formalization enlarges the scope of the analyst; standardization makes exchange and cumulative work possible; while universalization is a general aim of science (Boehme and Van den Daele 1977, Rip 1982), its importance derives from the fact that it makes knowledge transportable and thus enlarges its domain. The development of risk analysis shows such features insofar as it partakes of the general aims of a scientific field. The particular form of the development, however, derives from the political context.

Bureaucratic pressures at EPA. Risk analysis and its role in a risk assessment philosophy have indeed been taken up by regulatory agencies for bureaucratic reasons. The U.S. Environmental Protection Agency (EPA) argues in a recent document (EPA 1984) the importance of a specific risk management strategy as an "integration tool" within the agency:

... the benefits of regulating 'more efficiently across program lines' (can be obtained by taking) risk reduction as the integrating concept for Agency management. . . . Program integration has always been a problem for EPA leadership because of the diverse mandates of the Agency. . . . More consistent risk assessments and improved risk management have thus become attractive as integration tools.

The application of this concept of risk management works best, EPA adds, when we can easily quantify, or express in defensible numerical form, the pollution risks. (It also warns about a false impression of precision.) Both the demand for risk analysis and the motives behind it are thus set out explicitly.

At a higher level, the U.S. Office of Management and Budget (OMB) had been arguing for the risk reduction concept as a way of improving the cost-effectiveness of regulating. In a critical analysis of the earlier EPA strategy of Best Available Technology, for instance, they included tables showing that health gains would have widely divergent costs if the EPA risk management strategy were followed (*Environment Reporter* 1983). The specific argument of cost-effectiveness falls clearly within the scope of OMB, but there is also an element of coordination and rationalization across agencies. OMB continues to make such attempts, and the regulatory agencies themselves have established an Interagency Regulatory Liaison Group.¹⁴ One can consider cost-benefit analysis, and its more recent replacement, regulatory im-

pact analysis, to be the "integration" tools at this higher level (Mogee 1985).

The above quote from the EPA document should not be read as an accusation that the agency sets internal management goals before the protection of the environment and human health. The point is that while doing the latter, it will also serve the former. As Michael Baram has noted, this is a case of the general phenomenon that intellectual scope and socio-political control are enlarged at the same time.

Bureaucratic pressures at VROM. For EPA, the issue is primarily one of a risk management philosophy. In the case of the Dutch government department of Public Housing, Physical Planning and Environmental Management (VROM), such a philosophy is combined with the active development of risk analysis tools that make the desired risk management feasible. On the one hand, the Netherlands has a strong and sophisticated tradition in risk research (related to the concentration of industry, especially big chemical plants, in the Western, urbanized part, and a long experience with site licensing and centralized zoning policies). On the other hand, there has been dissatisfaction within the VROM's section on Environmental Management with ad hoc treatment of licensing and standard setting, and with the long drawn-out processes of negotiation and objections against decisions brought to administrative courts.

Therefore, a two-pronged strategy has been followed. VROM commissioned the development of a computerized risk analysis package (pressing its contractors when they appeared ready to give up), and also prepared a general policy of having one standard for all installations: a risk level of $10E-6$ is acceptable. The actual policy is slightly more differentiated (distinguishing between a lower limit, an upper limit, and a grey zone between), but the thrust is still to have one overall standard. The software is now available as the so-called SAFETI package, and the policy was passed by Parliament early in 1986. Concrete applications have not yet been made public, but Shell Nederland Company has already bought the package. (They can now run their data through the program and predict what the government's answer will be to their applications for licenses.)

The debate surrounding the VROM action, especially in 1985, was both political and scientific-professional. Industry argued strongly against the introduction of one standard, insisting that allowance should be made for the specific circumstances of a particular installation and locale. One of their points, however, was the crudeness of the computerized risk analysis and the unreliability of many of the data to be fed into it. Risk analysts tended to focus on the technical problems, designing some studies to learn about limitations of the package. The routinization of risk analysis is what attracts them, even if this may limit the

exercise of professional judgement.

What we see here is an example of a specific demand — in this case from a bureaucracy — orienting work in risk analysis that leads to generally usable results. The computer package may be used elsewhere. It will become part of the “state of the art” of risk analysis, and thus provide a (relatively) independent input into policymaking elsewhere. In this case, the bootstrap operation has been successful. The example also shows, however, that such a tool carries with it the limitations of its original problem definition, related to the specific bureaucratic demand that drove its development. It may even export its context of origin: since it is especially suited to support routinized decisionmaking, its use will stimulate such routinization. To paraphrase the aphorism with which this section opened: once you have a hammer, you transform everything into nails. But this happens only if the “hammer” is thought to be an effective tool, or one that can be made effective.

Criticisms of Risk Analysis

An important change in the context of risk research, especially during the last decades, has been the emergence (and recognition) of new kinds of risk and the increased size and scope of existing risks (e.g. of traffic). This change has clearly contributed to the growth of risk research and influenced some of its directions. In this paper, while some of the new risks have been discussed (e.g. nuclear), the focus has been on changes in the social and political contexts and the effects of changing demands on risk research. This focus reflects the specific evolution of risk research, in which the problem-definition of providing decision aids is increasingly dominant.

The recognition of this particular focus on decisionmaking has led to criticisms of risk analysis, sometimes from other segments of the profession (e.g., safety and reliability engineers), but mostly from social scientists and social critics. For example, the need for analysis of the actual socio-technical systems and the way they (by themselves, or more often in their interactions) generate risks has been pointed out. The complex problem of social acceptability — itself already a reduction of the issue of “social viability” (Wynne 1983a, 1983b) — is further reduced, critics argue, to the exercise of defining and measuring numerical risk levels:

In this paradigm, theoretical estimates which predict risk levels less than those specified by acceptable risk criteria would be considered to have met both necessary and sufficient conditions for the acceptability of the technology (Otway and von Winterfeldt 1982: 125).

The argument here is not against numbers as such, but against the switch that lies behind their use, from technologies and systems to decision tools.

Another relevant criticism originates with the observation, often made by industrialists, that the identification of hazards is a more important function of risk analysis than the attempt to assign probabilities and quantify effects. Within industrial organizations, special techniques have been developed and are used to meet the diagnostic function of risk analysis (Kletz 1983). In general, one could imagine the improvement of “early warning,” from its present haphazard and often conflictual occurrences, to a tool in the kit of the professional risk analyst. But this possibility is not taken up by the risk analysts, because it would imply a switch to another dynamic of scientific development: from an operations research-type dynamic to that of the diagnostic-semiotic sciences like medical sciences (Ginzburg 1979). Nor are there many incentives from the political context: the demand is for decision tools geared to creating order; early warnings, by their nature, disrupt existing order.

A third criticism of dominant problem-definitions in risk research attacks the assumption that decisionmaking of government (or of other big actors with top-down authority) is the relevant context. While there is some risk analysis that takes workers, community groups, or environmental groups as clients, it still depends on the “big decisionmaking” definition, because it aims at providing counter-expertise to help these groups defend their interests in the larger decisionmaking arenas. There is no attempt to provide tools for their own decisionmaking.

Within risk research in general, there are a number of strands, especially in behavioral and social research, that do not, or do not exclusively, show the focus on authoritative decisionmaking. Laboratory experiments on the psychology of risk decisions of individuals are probably outside the decision focus (even if the results provide ammunition in the debates). Cultural analysis of risk (e.g. Douglas and Wildavsky 1982), is relevant, and retains a pluralistic perspective. Sociological and political science studies of actual decisionmaking, including bottom-up activities, are increasingly done without the specific governmental focus; they may also provide a welcome broadening of the rather specific theoretical approach of the cultural analysis of risk. Since such studies do not respond to a clear demand (or to a powerful patron), there will be little professionalization and also little cumulative progress, unless the work is embedded in disciplinary traditions (as is the case for psychological studies of risk).

Conditions for Reliable Advice

Within the limitations, briefly indicated here, of the actual patterns of risk research, decisionmaking contexts are primary. We may thus ask if bootstrap operations have occurred in this context; and if so, which

conditions have been conducive to the success of such operations.

Controversy as a learning process. There has been a strong tradition arguing that risk analysis should be protected against outside influences in order to develop in a productive way. This view assumes that the rationality of the "scientific" learning process is guaranteed only when it is fully internally controlled. In contrast, I will argue that there may be externally controlled, rational learning processes, and that the absolute rejection of this possibility has more to do with professional autonomy and the protection of relations with important patrons than with the predicted decreases in rationality, which may or may not occur, depending on the particular case.

The case of risk analyses of nuclear energy production shows that the expert input of risk researchers is often not what clinches debates. The political context is dominant; and if this context is one of public debate, there may well be further articulation of data, methods, and interpretation, i.e., a learning process. Indeed, the case material appears to indicate that critical evaluation and improvement of expertise — the traditional responsibility of scientific-professional communities — derives its impetus as well as its focus from the decisionmaking and public controversy contexts. Going one step further, one could argue that debate, perhaps controversy, is necessary to drive the improvement of the quality of risk analysis. Although controversies may induce counterproductive polarization and defensive stances, it is also possible that approaches to risk analysis may be reconsidered and elaborated in a productive ("rational") way. An example would be the aftermath of the Rasmussen report. Although the report is still defended by some as the paradigm of comprehensive risk analysis, with its weak spots patched up or hidden, there is another strand in risk analysis, in which comprehensive analysis leading to definite results for a population of cases (here, nuclear plants) is given up as ideal. The task of the risk analyst is now formulated as the production and presentation of data on hazards and probabilities in a manageable form to decisionmakers and other concerned parties. What is manageable depends on the decisionmakers and on the situation, and work is then directed to concrete improvements.

Note that the learning that occurs in situations where public debate or controversy is a driving force is coupled to the emergence of a repertoire of forceful arguments and findings. For example, Inhaber's (1979) comparison of the risks of different energy sources (nuclear, solar, and coal) suffered, it is now recognized, from both technical problems and bias. But it did create a situation in which opponents of nuclear energy cannot just say "no" and present an alternative as merely an alternative; there now have to be arguments. Inhaber's contribution is that one of the

arguments should be about the risks related to transport and the production of conventional buildings and materials. (The same point is made by Fischhoff et al. (1981: 45).) This kind of "repertoire learning" is a very important aspect of quality improvement, but cannot always easily be captured in simple terms such as those that could be written down in a handbook (Rip forthcoming).

Quality control and bias. The second main implication of the analysis of mutual dependence of risk research and political context is that quality improvement may occur together with bias (e.g. bureaucratic bias as illustrated for EPA and VROM). Such a bias (which need not be necessarily bad) can be sustained by a certain compartmentalization. In order to make room for formalization and universalization, desired by bureaucrats and risk analysts alike, the close links between risk research, its inputs to decisionmaking, and the concrete details of the decision (or management) situation have to be loosened, so that a "niche" is created for internal control of the scientific processes, while they remain lodged in the overall problem definition.

This is exactly what has happened in recent discussions on risk assessment. The NAS 1983 report advocated separation between risk assessment and risk management, and has been very influential. Risk assessment should be done relatively independently, in a "scientific" way, it claimed, while risk managers introduce economic and political considerations afterwards (hopefully, in a rational and/or accountable way). The risk management side may itself be "scientificized," e.g. by applying cost-benefit analysis or other decision rules.¹⁵ EPA's risk reduction philosophy discussed above could lead to such "scientification." Examples from other sectors and other countries, e.g. the Dutch Health Council's new procedures for setting standards for carcinogens (Van Eijndhoven and Groenewegen 1986), show that separation between the "science" and the "socio-political considerations" is sought generally.

Dangers of compartmentalization. Although from the point of view of quality control, there are good reasons to avoid too intimate contacts between the "science" and the "context," there are also dangers. The rationality and quality that is produced this way may become too abstract and difficult to relate to practical situations. (This is an argument often voiced by managers and industrialists.) The rationality is of a specific kind: compartmentalized. Risk assessment products and risk management decisions are optimized according to their own criteria, while there is no check on the adequacy of the division of labor, which is predicated on the specific choice of the "compartments." In addition, it may lead to a false sense of independence for the researchers, and defensive reactions to criticisms. Segmented professionalization further enhances the

growth of compartmentalized rationalities.

The disadvantages of the separation can be illustrated in two ways. One is that a particular form of separation can be used as a socio-political tactic. For example, when the American Industrial Health Council (an active lobby group primarily for the chemical industry) took a stand on carcinogens, it argued for separation. In its proposal to Congress, AIHC stated that in the development of chronic health control policies

... scientific determination should be made separate from regulatory considerations, and that such determinations, assessing the most probable human risk, should be made by the best scientists available following a review of all relevant data (quoted in Dickson 1984).

Since definitions of the "best scientists" and "all relevant data" are always negotiable, the fight against (perhaps unnecessarily) tough controls, if the AIHC separation were adopted, could be fought in the scientific arena, without immediate consideration of the interests and concomitant problem definitions at play.

Not only may the separation between science and decisionmaking hide the choices made in cutting up the issues; it also hides the ways in which individuals, for their own reasons, take context into account when they prepare their expert inputs. Van Eijndhoven and Groenewegen (1986) present two striking examples in which expert advice on environmental standards in the Netherlands produced numbers in which the social and political context must have determined some important choices. In the case of dioxin, the standard was liberalized, although no new data had appeared (there had been a fresh assessment of existing data), and the new standard was just liberal enough to avoid extensive clean-up measures in the case of chemical waste that was the occasion for the second assessment. For formaldehyde, new data appeared, but their evaluation did not lead the experts to propose a change in standards — again, this accorded with their view of the right measures in the situation at hand. Van Eijndhoven and Groenewegen propose that expert's choices, also of a more political nature, should be made explicit, and added to their advisory reports.

Decomartmentalization. Given that compartmentalization has its dangers as well as advantages, some limited decompartmentalization is in order. This could be done in different ways. One route is to follow Ezrahi's (1980) plea for "pragmatic" instead of "puristic" rationalism, provided one adds accountability of experts for the social and political implications of their advice (Rip 1985). The advisor should check the socio-political "landscape" of the decision or management situation and, if necessary, adapt his advice (though not necessarily his science). Concrete versions of this argument can be found in the risk literature, as the following statement by the Assistant Director, British

Radiological Protection Board, illustrates:

Formal advice on standards needs to reflect where the state-of-the-art has reached but must remain stable for a reasonable time. This must be kept separate from research programmes, whether these are to develop more complex techniques for decisions analysis, or to understand better the societal attitudes that these techniques should reflect or incorporate (Webb, in Risk Working Group (ESRC) *Newsletter* 1986: 7).

There are more examples of "pragmatic" arguments to be found. The one cited suffices to show that good reasons not to incorporate all scientific sophistication into advice or decision tools may also be misused (although not in the particular case quoted) to obstruct calls for changes. It will be clear that the assessments of the socio-political landscape required by pragmatic rationalism will not be easy to make, and will increase the responsibility of the expert adviser who has to balance scientific purism against the other extreme of societal paternalism (deciding what is good for the people). While the element of paternalism may be softened when the expert realizes the limitations of his expertise, including its partial dependence on political contexts, structural provisions are necessary to maintain negotiability of pragmatic advice. Accountability of the expert for the social and political implications of the advice is one possibility. Such a "horizontal" responsibility runs against traditions of vertical, hierarchical responsibility, as examples of professional dissent in organizations, including "whistle blowing," show (Chalk and Von Hippel 1979). Here, the argument is not in terms of the public interest, but focused on quality control and improvement of scientific advice. Accountability for implications is not only an antidote to paternalism, but will be a major incentive for decisionmakers to be pragmatically rationalist in the first place; such are the mechanisms of holding people responsible (Rip 1981).

Conclusions and Prescriptions

The route toward decompartmentalization that is advocated here runs counter to some trends in the political context (standardization and universalization that abstract from specific situational features), but may sometimes support other elements, e.g. simplified decision aids for routine decisions. Pragmatic rationalism, coupled with expert accountability, is a way to break through the circularity of a form of advice based on risk research that is itself shaped by the socio-political contexts it addresses. Even if the bootstrap operation has been successful, as is the case for the basic approach of probabilistic risk analysis, it remains the product of its contextual history, and should remain open to criticism. That is, further contextual influence should not be avoided, and risk

professionals should not defend autonomy in terms of some ideal of pure rationalism. If they insist on such a compartmentalization, the bootstrap operation of quality improvement will become the prisoner of a profession on the defense.

Acknowledgments

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Notes

1. Compare the discussion over peer review in public policy, as reflected for instance in a special issue of *Science, Technology, & Human Values*, especially Clark and Majone 1985, Jasanoff 1985, and Salter 1985.

2. "Experimental philosophy" is the programmatic name coined by some of the protagonists of modern (natural) science in the 17th century, and covers the typical dynamic of interaction between experimental observation and controlled speculation geared to reductionist ideals of explanation and a disciplinary hierarchy with physics at the top. (See Boehme et al. 1977 for historical details.) "Natural history" shows a different dynamic: everything that happens is relevant, not just the effects that can be produced under controlled circumstances in a laboratory. Roots of this program can be found in the Aristotelian approach to empirical science and the study of "meteorology" (i.e. changes in nature), in the 18th and 19th century interest in collecting and classifying phenomena, now also with attempts at genetic explanations (as in evolutionary theories). The recent rise of environmental science can be seen as a rebirth of the old "meteorology." Rip et al. (1982: 18-21) from which these ideas are derived, distinguish also other dynamics, of technological fields, of formal sciences, of strategic sciences, of medical (diagnostic/remedial) fields, which have less clear-cut programs. The paradigm of a strategic science is operations research, and the related game theory, with rationalization of decisionmaking the intellectual ideal. Other strategic sciences are cybernetics and system dynamics, human ecology (that is, study of the impact of human intervention), and risk analysis and technology assessment. The specific nature of strategic sciences may be highlighted with an example derived from Barnes (1983: 535). German and French commanders of the opposed battle-lines in the spring of 1940 were involved in analyzing the strong and weak points of their own and the opponent's line. Since the ascription of strong and weak points implied decisions, e.g., to reinforce certain places, the assessment of strength also depended on an estimate of the opponent's ascriptions and strategies. As game theory shows for formalized settings, even the best strategy may then be undermined by the opponent's anticipating it and taking precautions. Barnes continues by discussing the effect of having a low escarpment or a steep hill at one point of the line. For practical purposes, a steep hill may be treated as a point of strength (difficult to climb, etc.), a treatment which is relatively independent of assessments and strategies of the opponent. The example can then be pressed to its two extremes: either the cliff which no man may climb, or the featureless landscape structured by the commanders' fantasies (i.e. ascriptions of strength and weakness). In a sense, strategic sciences are concerned with rational analysis of these ascriptions and their interaction, and whether the domain is a featureless landscape or a sheer cliff is of secondary importance only. I return to this later because risk research, especially risk analysis, is a strategic science that should maintain its concern with actual strengths and weaknesses (the

safety produced by ventilation or by fire-warning systems, the magnitude of toxic hazard of a chemical).

3. The ambiguity is spelled out by Conrad:

Depending on the perspective it is possible to interpret the present situation — and we may again take the example of the LNG-facility siting in California — either by inferring that the current political decisionmaking processes are not (yet) rationalized enough to really enjoy the fruits of risk assessment studies or that risk research has not (yet) reached the maturity and relative autonomy and independence of a productive problem-oriented research field to supply socially useful results (Conrad 1982: 102-103).

4. Covello and Mumpower (1985: 106) suggest that one of the earliest attempts to apply probability theory to a risk problem is the question whether an observed rash of accidents in the Prussian cavalry, of soldiers dying from kicks by horses, was a statistical fluctuation or significant (and thus an occasion for special action). Since the observed numbers could all be fitted to a Poisson distribution, it was considered to be a random fluctuation. Mathematicians still use this example when they want to make the point that politicians do not understand probability and statistics, and thus are wasting public money and human lives (for instance, Ennis 1985).

5. A partial indicator can be constructed from the distribution over time in the literature references in Covello's 1983 review of studies of perception of technological risk. From 1964 to 1974, the average number of references each year is 3 (with maxima of 6 and 5 in 1972 and 1974, respectively), while the number jumps to 22 in 1975, and averages out to 19 a year. Although the phenomenon may be an artefact of the way Covello has collected literature references, there is supporting evidence for the transition around 1975. Covello's review can also be used to distinguish different strands of research and their emergence. Before 1975, four categories of research can already be found: (1) natural hazards and disaster research; (2) individual & small group decision-making studies, behavioral decision research; (3) decision theory and multi-criteria procedures (not visible in the literature, and remaining a minor strand also after 1975); (4) case-studies and analysis of societal decisionmaking. From 1975 onwards there appear many studies of public attitudes and perceptions (36 for the period 1975-1980, out of a total of 112 references for that period), and nearly as many on acceptable risk (25 for the period 1975-1980). The newly emerging strands of research are clearly related to governmental decisionmaking problems and legitimation needs.

6. Inhaber and Norman (1982) give some interesting data for the use of keywords in the titles of articles covered by the *Science Citation Index*. For the period 1966-1982, the total number of entries for "risk" remains at about 500 a year until 1974-1975, and increases rapidly to about 6000 in 1982. To take into account the effects of the generally increasing number of keywords being used in titles, fractions were calculated, which show the same picture (.01% until 1974-1975, then an increase to about .05%). By the end of the 1970s, the use of "hazard" as a keyword had dropped from a 50-50 proportion to 25-75. Although the indicator constructed by Inhaber and Norman has its difficulties (one limitation being the large influence of biomedical articles having "risk group" in their titles), it does appear to cover a feature of the literature which was recognized before in a qualitative way.

7. Conrad (1982: 105) gives a largely negative evaluation of the symbolic function of "risk":

it serves as a means of coming to terms in symbolical terms with the potential dangers of the technology in question which cannot be eliminated in reality; in many cases; it conceals other, more central dimensions of the conflict surrounding large-scale technologies; it allows conflicts to be carried out in an increasingly ritualized form (e.g. in law suits).

Conrad may well be right in identifying these social functions, but this does not diminish the point made in the main text, that the term

"risk" creates a focus for a debate that might otherwise be less productive. The rhetorical function of terms and concepts has both negative and positive aspects, and should be studied, apart from one's evaluations, for its impact on developments, both social and scientific.

8. Journals carrying risk research articles existed already, often belonging to one of the existing traditions, for example, the *Journal of Risk and Insurance* (before 1964, *Journal of Insurance*), the *Journal of Occupational Accidents*, *Accident Analysis and Prevention*, and *Nuclear Safety*. Changes of titles sometimes indicate the developments going on, e.g. the emergence of the notion of "risk management" in the insurance sector in the 1960s. By the end of that period, *The National Insurance Buyer* changed its name to *Risk Management*, as befitted a journal of the professional association of American insurance buyers, which was called the Risk and Insurance Management Society (see also Crockford 1982).

9. This argument has been put forward by the Dutch risk analyst E. F. Blokker, in a paper for a national state-of-the-art conference (January 1985), and reiterated in the discussion, when a participant pointed out the divergence in outcomes of the Ispra benchmark study. Blokker quotes Rowe (1982) to make his point, and assumes these results to be robust and generally applicable in order to defend his own position. There is an interesting comparison with benchmark studies in analytical chemistry, e.g. of lead and cadmium concentrations in food samples (Sherlock et al. 1985). Most of the participating laboratories failed to produce results within an acceptable range. The authors of the article observe that there is room for improvement, but recognize that a high degree of accuracy has to be paid for (i.e. one has to consider if it is worth the trouble). Professional analytical chemists, in their reactions, read the article as a smear on their profession, and argue that the concentration levels in the samples were so low that errors were of the same size as the concentration itself, which explains the wide range of results between laboratories.

10. *New Scientist* (15 May 1986: 30) noted that American biotechnology companies find it increasingly difficult to insure themselves. Underwriters refuse to insure, because there is no history from which to draw risk analyses to set premiums.

11. Most of the information in this and subsequent paragraphs is drawn from Fuller (1976). Although the main focus of the book is the Fermi breeder reactor near Detroit, and its nearly fatal (and "incredible") accident in 1966, the problems of safety, permits and insurance are discussed in detail, and with many quotations. The book also contains a detailed description of the update of WASH-740, based on access to documents and correspondence from AEC and Brookhaven National Laboratory that had never been published. It is for this source material that the book is used here.

12. Materials and insights from Roger Cooke, on which this paragraph builds, are gratefully acknowledged.

13. The chemical industry's view and its initial hesitation are apparent in the report of their International Study Group on Risk Analysis (European Federation of Chemical Engineering 1985). The shift of the British Nuclear Installations Inspectorate towards probabilistic risk analysis, in reaction to initiatives of a licensee, the Central Electricity Generating Board, are documented by Cannell (1986). The Sizewell Inquiry, which made part of this shift public, was also the occasion where the argument of coordination between designers and authorities on the basis of numerical risk levels was voiced.

14. Dickson (1984: Chapter 6) describes how the Office of Science and Technology Policy allies with OMB to create greater coordination over regulation in different agencies (after earlier, but unsuccessful attempts by OMB). OMB is quoted as arguing that tradeoffs between different social and political objectives, such as protecting the environment versus stimulating economic growth, will now be easier to make. The establishment of the Interagency Regulatory Liaison Group to coordinate activities and actions on individual hazards is partly a defense against coordination from above, i.e. through Presidential offices. OMB "interference" with

agency rulemaking has led to a Congressional report (House Subcommittee on Oversight and Investigations, *EPA's Asbestos Regulations*, USGPO, October 1985) and discussions in the literature, e.g. *Harvard Law Review* 99 (1986): 1059, 1075. I am grateful to Michael Baram for drawing my attention to these materials.

15. There is, by now, a plethora of rules and methods, offered by decision analysts, economists, and philosophers (Fischhoff et al. 1981). Stallen and Fischhoff (1985) is interesting in its attempt also to take features of the risk and of the technology into account.

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Social Dimensions of the Mind-Body Problem: Turbulence in the Flow of Scientific Information

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The aim of this paper is to warn my fellow philosophers that the mind-body problem, which may seem distinctively their own, has ineluctable social dimensions. Today, philosophers often reformulate classic issues in the language of scientific theories in order to facilitate their analysis and effective resolution. Thus there are various new forms of the mind-body problem to consider. The scientific literature involved, both psychological and biological, while complex and sophisticated, lends itself to crisp formulation of the issues: e.g., can psychological models of the Pavlovian learning mechanism be explained by cellular/molecular accounts of the functional architecture of that mechanism?

This question is a variant of the problem of inter-theoretic reduction well established in philosophical literature (Hempel, 1952; Nagel, 1960; P. M. Churchland, 1985; 1986; P. S. Churchland, 1986). The issue of reduction has compelling significance for the philosophy of mind and for the mind-body problem. Stephen Stich (1983), on the one hand, and Paul and Patricia Churchland together with Alex Rosenberg (1980), on the other, have argued that the familiar "folk psychology" of beliefs and desires, hopes and fears, in use for millennia as a means of expressing and explaining conscious and deliberate human action, is moribund, unscientific, and ripe for replacement.

The topic of inter-theoretic reduction would present a clear topic for logical and semantic analysis if it were conceptualized in terms of relationships between distinct sets of sentences (e.g., from biology and psychology). But scientific theories are *not* simply sets of sentences whose logical and semantic relations might be regarded as programmable or machine readable. Theories are embedded in investigative and communicative practices, in traditional social frameworks in specific scientific communities. Historians and sociologists of science, therefore, have something to say about the mind-body problem when it is reformulated in terms of such questions as, "Can animal learning theory be reduced to neuroscience?"

The arguments to follow are based on current work in two fields of scientific research: (1) psychological perspectives on Pavlovian conditioning, and (2) cellu-

lar and molecular biology of learning and memory. These fields offer exemplary possibilities for inter-theoretic reduction: the derivation of more powerful analogues of basic regularities of an "old" theory from the fundamental generalizations and models of a "new" theory. Although these two fields are the most likely locus of such reduction in the emerging discipline of cognitive neuroscience, theoretical unification of the requisite sort has not yet occurred.¹ Typically, philosophical discussions of this issue, although they may be garnished with scientific detail, are abstract and indirect analyses of ontological possibilities. Their core arguments often reflect metaphysical methods and agendas rather than the details of scientific practice (Churchland, 1984; Danto, 1983). Nevertheless, the topic of reduction demands careful consideration of scientific work on both sides of the gap it is to bridge. I here concentrate on a small subset of micro-social issues, cognitive factors immediately relevant for effective communication or information flow linking the two fields.

Socializing Epistemology²

Such scientific processes as observation, innovation, development, presentation, and validation all have both social and epistemological aspects. Each of these processes involves both actor ↔ actor and actor ↔ world relationships. Each of these, in turn, is an element in more complex actor ↔ actor ↔ actor ↔ world relationships. For example, actors criticize, reproduce, or use each others' observations and teach each other how to make observations. Assimilation of these skills is a large part of graduate and post-doctoral instruction in science. The migratory behavior of young post-doctoral scientists from one leading laboratory to another, at times across boundaries separating distinct areas of research, is a key factor in the development of a social network legitimating certain forms of observation. Michael Lynch (1985: 60-61) has demonstrated convincingly that graphic formats, instrumental fields and preparatory techniques in histology penetrate both the field of what is visible and the means for

perceiving it. It is as though they operate as elements of an externalized retina, activating the perceptible and schematically processing it.

He concludes that

the resultant data were therefore neither wholly constructed, nor simply a "mirror of nature" arising from an encounter between a rational mind and an inherently orderly nature. Instead, the representational adequacy of the data depended upon a tenuous coherence of actions established in the social environs of the laboratory.

Lynch has studied the work of a contemporary neuroscience laboratory which uses electron photomicrographic techniques to analyze the process of axonal sprouting in response to degenerative lesions of rat hippocampus. His talk about an "external retina" and "resultant data" refers to the process of "observing that" axonal sprouting has occurred and to the results of such observation. Controversy concerning what does and does not constitute appropriate data for determinations of the cellular locus of learning and memory brings different research traditions in neurobiology into conflict and has great impact on the choice of experimental protocols and the selection of organic systems and sub-systems to which such protocols are applied (Alkon, in press).

More generally, such social processes as dialogue, communication, criticism, negotiation and compromise (all the processes leading to consensus and dissent) underlie scientific judgments concerning criteria for well-defined and solvable problems, appropriateness and reliability of available experimental procedures and techniques, clarity and cogency of concepts and conceptual distinctions, general criteria of explanatory adequacy and so on (Gilbert and Mulkay, 1984; Shinn and Whitley, 1985). These social processes are ubiquitous in science. Experts *within* a particular research field disagree (see especially Farley and Alkon, 1985) on such important topics as: (1) what are the important questions in this field? (2) what are the appropriate investigative techniques? (3) what are the basic mechanisms of the system we are studying? (4) what would a good explanation or a good theory or a good solution of this problem look like? So it is not surprising that *inter-field understanding and agreement is even more difficult and less frequently achieved.*

The communication of agreement and disagreement is a social process. The importance and the difficulty of analyzing these phenomena and socializing epistemology is demonstrated by the following possibility. Assume that two scientists agree that *p*, some sentence asserting the results of a particular observation, a methodological prescription, or a theoretical claim, is true. Further assume that they locate *p* in comparable conceptual or inferential networks but are not fully aware of each other's inferences from *p*, or

of premisses from which *p* may be inferred. For example, one scientist thinks *p* implies *q*, and another thinks *p* implies not-*q*; or the first scientist believes *p* implies *r*, while the second believes *p* implies *s*, or the first believes *j* implies *p*, while the second believes *p* is implied by *k*. If neither of *them* is aware of their different beliefs about *p*'s role in a conceptual framework or network of related sentences, careful interpretations of the texts they address to each other must take account of the social facts of such misunderstanding or imperfect communication.

Communication between significant scientific actors is often distorted in this way. Philosophers of science who overlook such socially and historically important details idealize the communicative aspects of science, and their assessments of the epistemological properties or ontological implications of the resulting scientific archive can appear to be exercises in abstract logic having little to do with natural science.

Real information flow in science is turbulent, exhibiting gaps and delays worthy of study and analysis in their own right. Scientific communication benefits from few if any "ideal speech situations" leading to "consensus constrained by nothing but the force of argument." Nevertheless, I propose a concept of effective scientific communication (adapted from Habermas 1984: 99-101, 286-88) as an explicit, critical benchmark for the analysis and appraisal of communication in science. Effective scientific communication is marked by:

- 1) accurate, complete, and reciprocal inter-field exchange of information concerning methods, results, explanatory models, theoretical goals, and ambiguous and tacit aspects of scientific practice;
- 2) reciprocity in the exchange of criticism and appraisal of reported results; and
- 3) reciprocity in requests for reformulation of investigative priorities, research designs, and evaluative criteria.

This notion of effective communication is invoked only as a regulative ideal or critical standard with which to guide the exploration of turbulent information flows.³

A Case Study

One neurobiological version of the mind/body problem can be formulated thus: How can adaptive modifications of behavior taking place during the life history of individual organisms be explained by physical events and processes taking place within the organism? Scientific investigation of one half of this problem by specialists in animal learning theory has gone on for more than a century (Mackintosh, 1974). Comprehensive reviews of the psychological literature are

available in Mackintosh (1983), Dickinson (1980), Rescorla (1980), and Gormezano (1975). Only within the last two decades, however, have advances in cellular and molecular neurobiology and the functional architecture of the nervous system begun to offer hope of explaining the learning mechanism in terms of molecular events taking place within individual cells in an organism's nervous system (Farley and Alkon, 1985; Gluck and Thompson, in press; Hawkins et al., 1986).

The purposes of this paper require simplification of the major episodes in the development of the emerging discipline linking these two research areas. In what follows, I seek such simplification by personifying the two research areas, identifying them with the leaders of research centers who, on the one hand [Cambridge], set the most demanding *psychological* criteria for any putative neurobiological explanation of the Pavlovian learning mechanism, and who, on the other [Columbia], have made the greatest effort to *reduce* fundamental psychological laws of learning to relevant elements of cellular and molecular biology. I argue that communication linking the two research areas has been *ineffective*, failing on the last two of the criteria of *effective* communication listed above. There has not yet been explicit and public reciprocity in the exchange of critical appraisal of published results, nor have there been comparable requests or suggestions concerning the reformulation of investigative priorities, research designs or criteria of scientific adequacy.

What are historians, philosophers or sociologists of science to make of such *non-events*, such *non-communication*? How could a philosopher of science possibly decide that the Mackintosh-Dickinson version of the learning mechanism was reducible to a cellular and molecular model with which it had never explicitly been connected in the scientific literature? How could a philosopher of science determine that Mackintosh's views were continuous with a particular computational model when the only published work directly addressing the problem are Mackintosh's skeptical and even negative remarks on this point? These questions are neither rhetorical nor imponderable. They are raised simply to suggest the necessity of dealing with specific social and historical issues in the course of coming to a decision about the reducibility of theories developed in distinct scientific domains.

The following dialogue⁴ dramatizes this state of affairs and suggests means by which it might be remedied by *simulating* more effective communication in something approaching an "ideal speech situation" (Habermas, 1984: 99-101, 286-88). The dialogue, which is not a report of a real event nor a juxtaposition of hitherto unconnected comments actually in the archival records of the two centers, is based on analysis of their publications, and a full year in residence at the

Columbia Center and participation in its Cold Spring Harbor Summer Course on Cellular and Molecular Biology of Behavior. I have also had the benefit of long conversations with both neurobiologists and psychologists, including senior scientists and graduate students from both fields.

Cambridge: Your claims (Hawkins and Kandel, 1984) to be on the trail of the molecular alphabet of learning are premature. You have not shown that your animals are learning to associate two events or gaining knowledge of the causal contingencies in their environment. You merely show that the sea slug *Aplysia* modulates a family of defensive reflexes as a function of the temporal pairing of stimuli.

Columbia: You presume a distinction between associative learning and modulation of a defensive reflex which we do not find entirely clear and which we fear may beg the question against the most promising strategies for neurobiological reductions of the learning mechanism. Could you please provide further analysis and justification of that distinction?

Cambridge: The tactile stimuli (touch and electric shock applied to sensitive body parts near the gill of the sea slug) you use as conditioned stimuli naturally elicit defensive withdrawal (retraction of the gill within a protective fleshy mantle), albeit at fairly low levels of intensity. You show no more than such intensities can be modulated by certain combinations of the *non-associative* processes of *habituation* and *sensitization*. Within the Pavlovian tradition, *conditioned stimuli* (e.g., a ringing bell or a black square presented to the visual system) are selected precisely because they do not, in naive untrained animals, activate the motor systems implicated in the *unconditioned* response (e.g., salivary glands). Your paradigm does not clearly respond to the objection that you are merely isolating components of a molar unconditioned stimulus (painful stimulation to one region of the body) and manipulating the intensity of the organism's natural response to such components by various non-associative procedures.

Columbia: Now the issue has become the distinction between associative and non-associative procedures. Why is that distinction important for this discussion?

Cambridge: As you well know, non-associative procedures include *habituation*, or repetitive, low intensity presentation of a single stimulus under circumstances leading an organism to reduce or eliminate any natural response, and *sensitization*, intense presentation of a stimulus resulting in significant enhancement of the organism's natural responses to lower levels of stimulation of other elements of the same receptor system.

Associative processes, on the other hand, enable organisms to map a more comprehensive range of the causal contingencies in their environments. These

processes enable the mapping of a relationship between a cause or signal (conditioned stimulus, CS) and a naturally salient environmental event (unconditioned stimulus, US), even if the signal and the naturally salient event stimulate quite distinct receptor systems (hearing and smell).

Columbia. We feel we have more than met that objection by empirically distinguishing, using the most rigorous experimental controls, associative learning from sensitization. We have shown that the response to a *conditioned* (paired, associated) stimulus is more intense, of greater duration, and much more highly specific, than the response to an otherwise strictly equivalent, but non-paired, non-associated, *sensitizing* stimulus. In this protocol, the sensitizing stimulus is never presented to the organism in association with the relevant unconditioned stimulus.

Cambridge. But you have not met the objection that both *associated* and *non-associated* stimuli, in your protocols, are only components of the unconditioned stimulus, since they clearly activate an anatomically related portion of the same receptor system. How can you claim that an organism is learning anything about the *causal contingencies in its environment* by exploiting the details of a preparation which can only reveal the causal contingencies or *internal structure of the organism's own nervous system*?

Columbia. Perhaps you will concede that the theory of evolution provides reasons, not metaphysical fantasies, concerning the pre-established harmony of the properties of all monads, for thinking that the internal structure of an organism's nervous system provides it with a sketch, if not a map, of the major causal contingencies in its environment. The historical tradition linking philosophy and psychology which gives rise to your research area may explain your preoccupation with the distinction of inner experience and outer world. These Cartesian-Kantian scruples seem to have passed through Helmholtz and the German school of physiology to Pavlov himself. But Darwinian biologists operate within an entirely distinct, more thoroughly materialistic and naturalistic tradition. For us, the brain and the nervous system secrete thought just as the liver secretes bile. The brain and the nervous system of the simplest organisms evolve under pressure of natural selection, so that their basic structures come innately to map major contingencies of their environment. Are you still troubled by the archaic dispute between empiricists and nativists to which Darwin wrote the conclusion?

Cambridge. The issue between us is simpler, and has less to do with the history of philosophy. We are not anti-Darwinian. We seek to pose challenging questions to nature concerning the putatively adaptive matching of neural and environmental structures. You risk indulging the Panglossian conceits so effectively parodied by Stephen Gould. Such indulgence

would trivialize your research program.

Our basic objection, however, is pragmatic. It is that your research protocols do not readily translate into the idiom of our field. Higher invertebrates closely related to *Aplysia* have been conditioned in ways which directly respond to our objections. Surely *Aplysia* (an inter-tidal marine slug) has associative abilities as complex as those of *Limax* (the common garden slug studied by Sahley, 1981; 1984). Why not simply concede that your tradition will not be effectively connected with ours until you can demonstrate the cellular and molecular architecture underlying so-called *higher order conditioning*: *secondary conditioning*, *blocking*, and the like?

Columbia. Because we do not accept your division of non-associative and associative forms of learning as theoretically significant. We think it highly implausible that, given the ordinary time course of learning trials, that associative learning involves the development of entirely novel neural pathways, or new connections between old pathways. Our *neo-nativist* position assumes that associative learning processes modify already existing neural pathways and mechanisms by simply raising sub-threshold synaptic connections to threshold levels. Consequently, we do not think that non-associative and associative learning processes involve essentially different types of mechanism. What you see as a difference in kind, we see as a difference in degree. For us, the same cellular and molecular processes are involved in both forms of learning. The activity of one particular metabolic pathway, capable of modulating the quantity of synaptic transmitter released by a neuron, is more highly focussed and intensified by associative learning procedures than by habituation and sensitization. From a cell biological perspective, the difference amounts to no more than that; biologists are not bound by psychologists' catalogues of natural kinds of learning processes. Why don't you take a look at Figure 1?

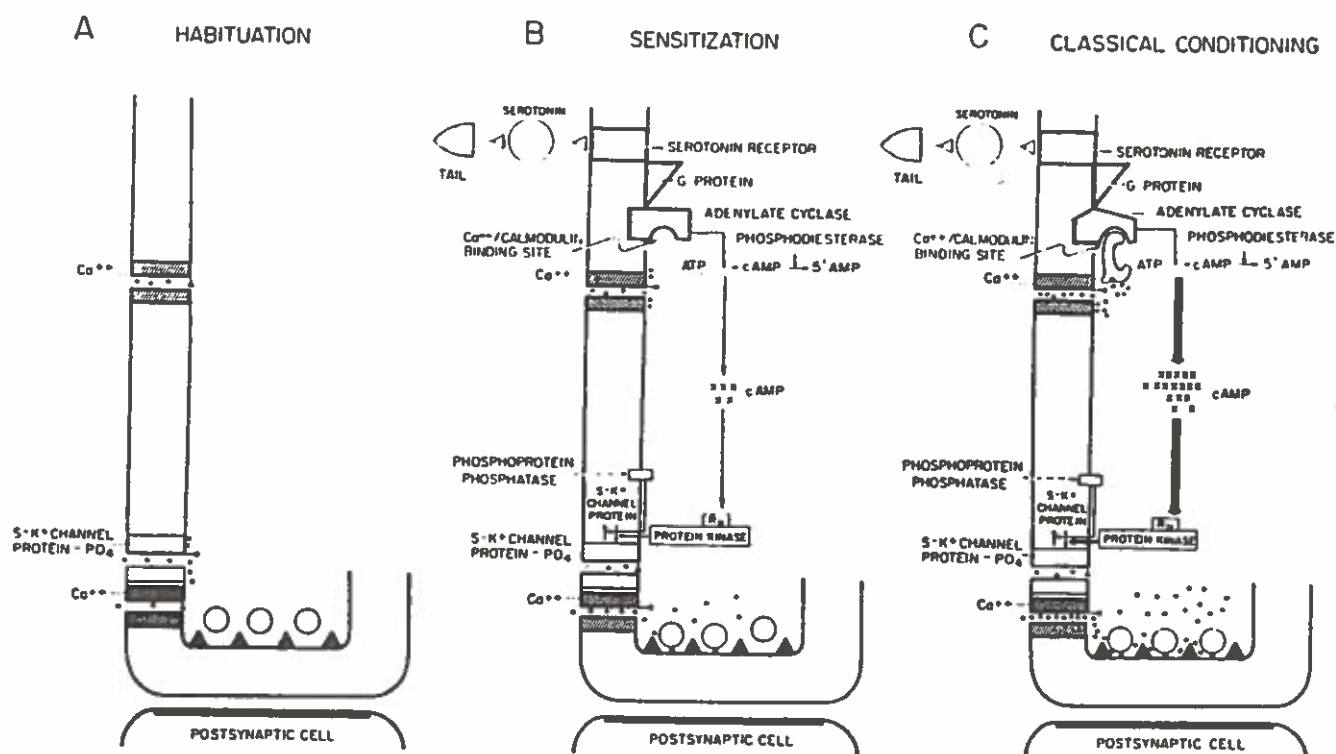
We have shown that the molecular and cellular mechanisms of non-associative learning can be combined to explain the simplest varieties of classical conditioning. We have offered quite plausible theoretical grounds for elaborating these same mechanisms to explain the higher order phenomena of blocking and second-order conditioning. Your criticisms of SR models are addressed to antiquated theories and model structures lacking the information processing capabilities that molecular and cellular biology can now demonstrate in quite simple neuronal circuits. Why not throw out that standard image of the reflex arc which you have in your head from the textbooks of the 40s and 50s, and replace it with the simplified wiring diagram in Figure 2?

Cambridge: We'll think about that. But let us remind you of the salient details of our criticism. The SR account of classical conditioning is based on the con-

cept of "stimulus substitution." Classical conditioning builds upon an innate association of an unconditioned stimulus (US) with an unconditioned response (UR). For the "stimulus substitution model," repeated pairing of a conditioned stimulus (CS) with the US leads the CS to acquire the response-evoking properties of the US. One could *not* infer strict identity of the set of representations activated by the presentation of the US (the reinforcer) and the set activated by presentation of the associated CS *unless* there were an equally strict identity of all aspects of the conditioned and unconditioned responses. "Stimulus substitution" models of classical conditioning fail to the extent that the conditioned response (CR) and UR are not identical. Even more importantly, the putative formation of a

single CS-CR link can not establish that associative learning has taken place. Only the phenomena of *higher order conditioning* provide convincing measures of conditioning.⁵

What the relevant evidence demonstrates is that there are serious questions about the reducibility of the "mental" phenomena of *representation, causal map of the environment, redundancy, surprise* and the like to the functional architecture of any conceivable SR mechanism. How can the phenomenon of cognition be deduced from the properties of neurones and neuronal networks? You have not persuaded us that our study of the ways in which organisms learn about the world will not be seriously impoverished if we drop our categories and accept yours.

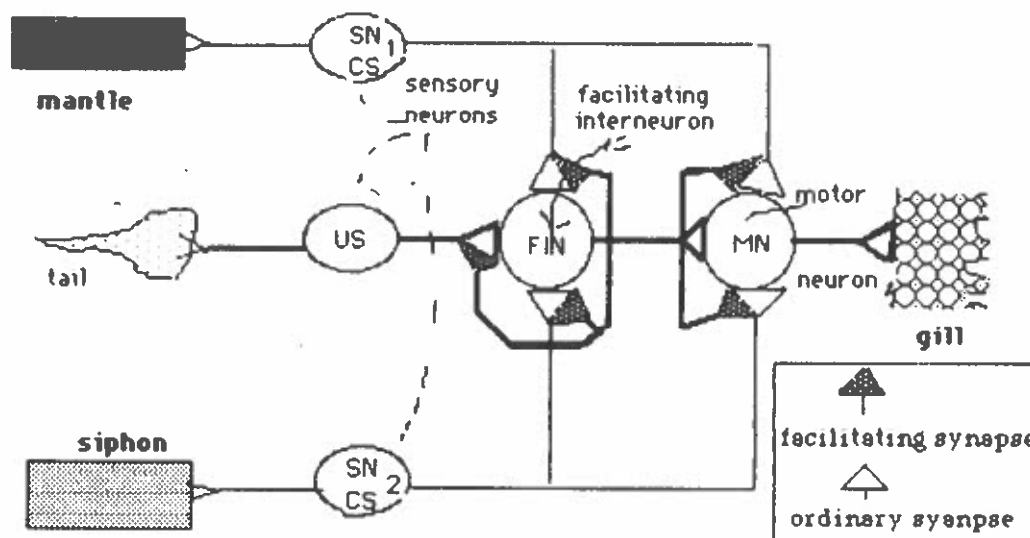


Models of the cellular mechanisms of habituation, sensitization, and classical conditioning in *Aplysia*. (A) Habituation. Repeated stimulation of a siphon sensory neuron (the presynaptic cell in the figure) produces prolonged inactivation of Ca^{2+} channels in that neuron (represented by closed gates), leading to a decrease in Ca^{2+} influx during each action potential and decreased transmitter release. (B) Sensitization. Stimulation of the tail produces prolonged inactivation of K^{+} channels in the siphon sensory neuron through a sequence of steps involving cAMP and protein phosphorylation. Closing these K^{+} channels produces broadening of subsequent action potentials, which in turn produces an increase in Ca^{2+} influx and increased transmitter release. (C) A model of classical conditioning based on the hypothesis that preceding activity leads to an influx of Ca^{2+} in the sensory neurons of the CS pathway. The Ca^{2+} binds the adenylyl cyclase, leading to a conformational change in this enzyme. This change promotes the synthesis of a greater amount of cAMP than would occur in the absence of Ca^{2+} binding.

(From Hawkins and Kandel, 1984)

FIGURE 1

Circuit for secondary conditioning of gill withdrawal reflex



(adapted from Hawkins & Kandel, 1984)

FIGURE 2

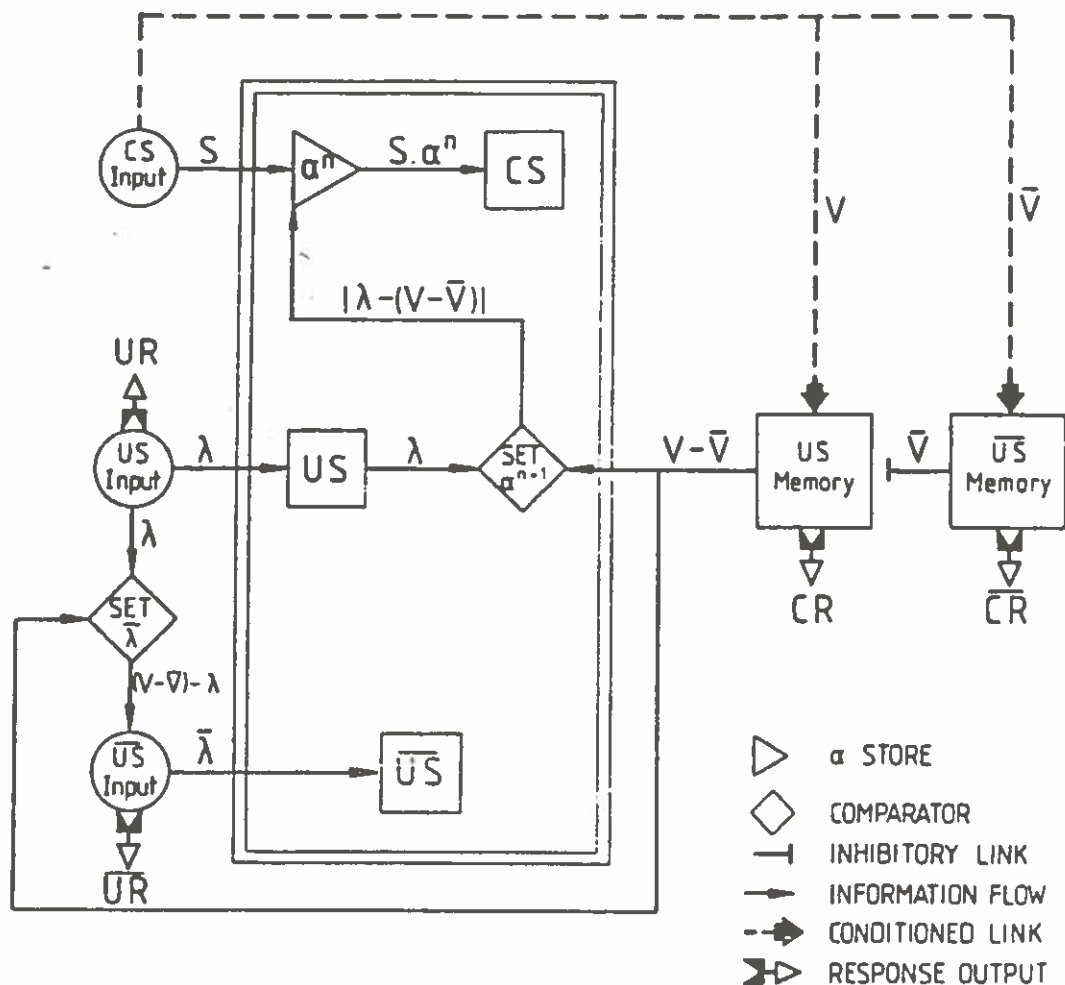
Columbia: Our 1982 and 1984 proposals concerning the cellular alphabet of learning, while speculative, are an entirely new form of the "cellular-connection" hypothesis, rendering obsolete Lashley's (1929-1948) critique of then prevalent views of the reflex arc. The new cellular and molecular models explain "blocking" (i.e., the cessation of the processing of stimuli recognized as redundant predictors of reinforcers), and meet the objection that the temporally contiguous presentation of two stimuli is not sufficient for associative learning. We think the mechanisms displayed in Figures 1 and 2 are immune to your efforts to undermine SR mechanisms as these were understood in the 40s and 50s. We understand the basic thrust of your criticism as follows: SR mechanisms cannot be the whole story. Real organisms in the real world process information in much more complex and subtle ways.

We may be able to bridge our differences through more careful attention to the manner in which very simple SR mechanisms are embedded within the more complex functional architecture of invertebrate and vertebrate nervous systems. For example, the cellular circuitry of our model includes mechanisms for *hierarchical control* by an organizing reinforcer and by competing reinforcers controlling other such systems. Such systems may eventually explain the integration of a complex array of subroutines under major behavioral categories such as feeding, escape, and so on. We are confident, although much work remains to be done, that molecular and cellular-connectionist approaches in neurobiology provide information

about the contents of the "black box" which will explain cognitive phenomena of the sort discussed by Robert Rescorla, Allan Wagner, and your lab.

Cambridge. Recent versions of the Wagner model, as well as one by Pearce and Hall, with their impressive array of decay functions, rehearsal mechanisms, short and long term memory stores, and comparators, present another possible stumbling block for the program of molecular neurobiological learning theories. Pearce and Hall have proposed a computational model of the Pavlovian learning mechanism. Consider the baroque intricacies of Figure 3, if you will. The core of their mechanism is a limited capacity processor that computes changes in the associative strength of a given stimulus with the presence or absence of a particular reinforcer. In their model, changes in the associative strength of signals (CS) and reinforcers (US) vary directly with the computed product of both the intensity and associability of the conditioned stimulus and the difference of the anticipated and received intensities of the reinforcer. If *Aplysia* could not perform such computations, its learning mechanism(s) would be overloaded with *irrelevant* information about its environment.

Columbia. We (1984) have proposed a cell-biological model capable of performing some of the computations Pearce and Hall impute to the neo-Pavlovian learning mechanism, e.g., secondary conditioning enabling an organism to display an elementary form of logical processing: If $CS_1 \leftrightarrow US$, and $CS_2 \leftrightarrow CS_1$, then $CS_2 \leftrightarrow US$. Moreover, our cell biological model theoretically accounts for "blocking", or an



(From Pearce and Hall, 1980)

FIGURE 3

organism's ability to recognize and consequently fail to process information concerning redundant signals of the same reinforcer.

Cambridge. The PH-80 (computational, representational) model accounts for the phenomena just mentioned, plus some distinctive features of simple Pavlovian conditioning not explicitly explained by, and perhaps at odds with yours.⁶ This model *corrects* an important flaw in the Rescorla-Wagner (1972) equation with respect to its predictions concerning the *extinction* of conditioned inhibition.

Columbia. Perhaps recent work linking the cAMP (cyclic adenosine monophosphate) "second messenger" system within a neuronal cell and the expression of particular genes in such individual cells will illustrate the plausibility of molecular biological explanations of simple forms of cognitive processing. Cellular and molecular processes of the sort reviewed

in Kandel and Schwartz (1982) may prove theoretically powerful enough to explain the function of the comparators in the PH-80 model.

Cambridge. Dreadfully sorry, but you just don't seem to get the drift at all. What is at stake is a major or revolutionary paradigm change in our field, not just an extension and elaboration of the 1972 Rescorla-Wagner model. Perhaps a more dramatic expression of this revolution is necessary. Your own invocation of a "cellular alphabet of learning" presupposes a "language of thought" for the Pavlovian learning mechanism (Dickinson, 1980: 116). For us, that expression is not a hollow metaphor. Following a functionalist stratagem (Fodor, 1975), blocking and "surprise" are interpreted as if *inferences* took place within the context of such strategic principles as: "Avoid aversive stimulation." "Anticipatory immobility often avoids aversive stimulation." "Process effective (but not redundant)

predictors of aversive stimulation." Blocking protocols add the following sentences and establish the corresponding inferences:

- | | |
|--|------------------|
| 1. Tone is an effective predictor of shock | <i>Knowledge</i> |
| 2. Tone is on. | <i>Knowledge</i> |
| 3. Suppress feeding behavior. | <i>Action</i> |

-
- | | |
|---|------------------|
| 1'. Combined with tone, light is a redundant predictor of shock | <i>Knowledge</i> |
| 2'. Light is on. | <i>Knowledge</i> |
| 3'. Do not suppress feeding behavior. | <i>Action</i> |

Dickinson explicitly denies that such chains of inference can be mediated by excitatory link (SR) models because they "cannot handle the goal-directed character" of the behavior in question. In particular, the *knowledge-action* translation process involved in these inferences cannot be mediated by excitatory links between event representations. "The control of such instrumental action is determined by the interaction of the animal's knowledge about the consequences of its behaviour with the value it places on these consequences." It's as if Dickinson thought rats and pigeons had sentences in their heads or that the environment pumped truth into them.

Your reluctance to deal with such recent developments in cognitive or computational elaborations of classical conditioning may well put you in the position of those biochemists who attempted to solve the problem of the gene by concentrating upon enzyme chemistry.

Columbia. We certainly may be, but that only suggests that our research program is risky, that it might not succeed, not that it is inherently misconceived. It is plain that you prefer to perform psychological experiments and to construct psychological theories. We prefer developing models connecting the phenomena you study with the rich, broad, and comparatively "hard" scientific procedures and methods of biophysics, biochemistry, and cellular and molecular biology. We hope to find that such phenomena as long-term memory can be explained in terms of modifications of intra-cellular gene expression and morphological changes in presynaptic "active zones" of the sort that have been established with the aid of electron microscopy in our lab.

You seem intent on describing black boxes⁷ so inherently fascinating that no one will ever get around to opening them. Our approach takes advantage of clear connections with other well-established scientific disciplines (the regulation of gene expression and the control of developmental and metabolic processes by inter- and intra-cellular messengers) dealing with phenomena more fundamental (i.e., more general cell biological processes) than those we are attempting to study.

You seem to think of the phenomena we investigate as more or less aberrant special cases of the general

mechanisms you have discovered. *But you fail to understand the overarching significance of the theory of evolution for neurobiology and psychology.* We interpret evolution as implying an underlying molecular conservatism in nature. We think that the mechanisms found to operate in the simplest forms of Pavlovian conditioning that can be demonstrated in invertebrates are unlikely to be aberrant special cases. Instead, these simple cellular and molecular mechanisms are likely to be the building blocks utilized in the gradual evolution of all the more complex forms of learning.

In contrast, functional psychology's insistence upon the beauty of increasingly baroque black boxes is isolating it from other disciplines, particularly from current work in molecular and cellular neurobiology, which might enrich, modify, correct, simplify, or even fundamentally transform the tools you use to identify, describe, and explain those black boxes. After all, genetics was eventually transformed by Linus Pauling's work on the nature of the chemical bond. It is not good for psychology to be alone. Tell us what you want in terms that do not lead us to believe that you really want us to abandon neurobiology and become psychologists.

Cambridge. We want you to open a black box complicated enough to account for the phenomena which current psychological research shows are really distinctive of the basic features of Pavlovian conditioning and associative learning.

Columbia. Every time we do that, you come back with another, more beautifully sophisticated black box for us to crack. You never quit.

Cambridge. We have no more intention of abandoning psychology and becoming neurobiologists than you have of undergoing the same conversion in the other direction. "Reducing" us, an intellectually diverting pastime to which you are understandably addicted since it was so fashionable around *Columbia* in the 50s and 60s, need not result in your "pre-empting" or "eliminating" us (Rosenberg, 1980; Churchland, 1984: 43-49). New theories of conceptual change are replacing old "folk philosophical" models of reduction (Kitcher, 1982; 1984; 1986). As neurobiologists, is it your intention to eliminate cognitive science once and for all? If you do so, you may find that you have destroyed the discipline capable of formulating your most fascinating problems. You might also find that you had destroyed the bridge over which you must travel to probe that new "mystery of mysteries": human learning, emotion, and communicative capacities culminating in language.

You should take notice of the fact that animal learning theorists are pursuing vigorous and fruitful research programs of their own. One of our jobs is to make yours more complicated. We are not just curators of some dusty old museum full of stuffed animals and skeletons. Your sea slugs only learn 1.5 new tricks

per decade. If you can't admit anything else, can't you at least see that we are better animal trainers than you?

Columbia. You really know how to hurt a guy. We are convinced you give insufficient credit to the flexibility and significance of the latest cell-molecular models in neurobiology. You are convinced we fail to understand the significance of current psychological critique, not only of the SR models left over from the 40s, but of the forms of Pavlovian theory that were available in the late 60s and early 70s when we got our project underway.

What can we do to improve communication between our two centers? Obviously we could talk to each other more effectively if we weren't twenty, forty, or sixty years out of date in each other's disciplines. What's the most constructive step we could take?

Cambridge. We would be intrigued by one of two possible outcomes. Gelperin, Sahley and company have taught *Limax*, a not too distant relative of *Aplysia*, tricks which bear interesting analogies with those which vertebrate learning theorists identify as the criteria of true associative learning. It is hard to see how *Aplysia* could have survived and evolved in an environment as complex as that it occupies if its learning mechanisms or black boxes were incapable of handling conditioned inhibition, blocking, and second-order conditioning. Why don't you let one of us try to teach your slugs the tricks *Limax* has learned? If that can be done, and you are able to identify the underlying cellular and molecular mechanisms and show that they meet your current theoretical expectations, we will be well on the way to more fruitful communication and collaboration. Alternatively, Gelperin, Sahley, and company might succeed in independently confirming your results by showing that the mechanisms underlying *Limax* logic are similar to those you have proposed. The key that will open the first door to effective inter-field cooperation will be the rigorous demonstration of the cellular and molecular mechanisms of higher order Pavlovian conditioning.

Columbia. Thanks; we would welcome a post-doc from your lab.⁸

Reflections on the Dialogue

Within a fifteen year interval (1968-83) at least four (and perhaps five) significantly different models have dominated the field of animal learning theory: (1) temporal contiguity, (2) causal contingency, (3) discrepancy (effective prediction of otherwise *unexpected* events), (4) complex information processing (changes in the associability of given stimuli as a function of their role in the individual life-histories of organisms), and (5) an inferential (sentential) processing model. There are important differences in the way these models are articulated. Mackintosh and Dickinson claim to

have *eliminated* a class of biological models ("excitatory link," "stimulus-response," or "reflex chain" models) as candidates for instantiating the relevant functional properties. Rescorla and Wagner agree that the old SR models are of limited generality, and that they must be reinterpreted. They are equally willing to identify their own perspective on Pavlovian conditioning as "cognitive" to distinguish it from the class of explanations they reject. Nevertheless, their insistence upon the complexity and intricacy of the information processing involved in such elementary forms of learning suggests the familiar ontology of "event representations," excitatory and inhibitory links exhibiting decay and retrieval functions. Mackintosh and Dickinson present the new cognitive approaches to Pavlovian conditioning as constituting a marked shift in scientific paradigms, a clear break between distinct scientific research programs with fundamentally different ontologies. For Rescorla and Wagner, the history of Pavlovian conditioning could be written as if one coherent research program had dominated psychology for more than fifty years. On this latter view, the fact that views concerning the basic learning mechanism have varied over time does not raise the spectre of radical ontological inconsistency within the program.

Understandably enough, molecular neurobiologists seeking to establish the cellular architecture of the Pavlovian learning mechanism have not sought to encompass the complex and shifting issues involved in this rapidly developing debate among psychologists. Instead, they have selected one central result from the psychological research program, the Rescorla-Wagner equation, and concentrated their efforts upon its cell biological explanation. From the perspective of the rational division of scientific labor, their move seems unassailably correct. But from the perspective of animal learning theorists convinced of the integrity of their methods and the scientific value of the intricate, subtle detail steadily accumulating in their laboratories, the turn toward deeper and deeper probes into cellular metabolism may seem irrelevant or worse. The purpose of reduction, it should be remembered, is the derivation of *more* powerful analogues of the basic regularities of the "old" theory from the fundamental generalizations of the "new" theory. Such derivations have yet to appear.

Given the centripetal forces driving *Columbia* ever deeper into the molecular/genetic mechanisms of neuronal plasticity and *Cambridge* toward increasingly sophisticated versions of the "language of thought" found in the learning mechanisms of rats and pigeons, the above dialogue is, if anything, overly optimistic about the future of effective communication between these two research areas. The truth, if the activities of scientific laboratories is our means of achieving it, may be less comforting to the philosophical "friends of

reduction" than the dialogue suggests. That is particularly the case if "reduction" is construed as philosophers still commonly construe it, as the deduction from the axioms of a *new* "reducing" theory (e.g., neuroscience) of new theorems "roughly equipotent" to comparable theorems of an *old* theory and offering many additional heuristic advantages in the bargain (P. M. Churchland, 1985: 8-14). This image of youthful vigor overtaking relatively feeble senescence finds no counterpart in the history of recent developments in animal learning theory and molecular neurobiology.

One party may be better financed and better able to attract talented new recruits than the other, but there is no evidence and no philosophical argumentation showing that this social circumstance is the logical outcome of underlying methodological, epistemological, or ontological superiority. Computational and architectural (cellular-molecular) approaches to the animal learning mechanism both flourish and both deserve to do so; they are equally progressive by any available methodological standard. Each field continues to publish challenging new findings together with theoretical accounts of those findings which meet requisite standards of logical rigor and comprehensive scope. Neither field is degenerating into ad-hocery. Neither field levels such critique at the other. No philosopher of science has provided evidence of the methodological or epistemological superiority of one of the two fields over the other.

Consequently, on the assumption that we are looking in the right place, there is absolutely no evidence suggesting that a gallant "new" theory is about to overtake all the explanatory functions of an increasingly impotent "old" theory.

Inter-field links connecting disparate scientific domains may be created by a relatively small number of atypically trained *translators* or *scientific generalists*. Such links may be especially sensitive to contingent features of the cognitive perspectives or social circumstances of these translators. Given the centripetal pressures of scientific specialization, translators' functions are much less likely to be subject to the self-correcting features operative in sectors of the scientific enterprise where the volume of scientific activity is much greater. The study of roles played by such inter-field translators ought to be of particular interest to those interested in the effect of its social, cultural, or economic context upon the content of science.

Before concluding, I should like to note that one of my colleagues has offered a valuable criticism of the concept of "effective communication" used in this paper.

Cambridge and *Columbia* would be the worse for it if they spent too much time in dialogue, reciprocally criticizing and appraising results or reformulating priorities, research designs and

evaluative criteria. Presumably each has good reason to get on with the business of pursuing, without too many distractions, its own research agenda. The "membrane" between disciplines should neither be impermeable nor wholly permeable but semi-permeable. There should be both upper and lower limits on how much communication it is rational to do, or attempt. In the rational division of scientific labor, some account must be taken of the fact that however "good" communication may be, it is not free. It consumes valuable and relatively scarce resources of time, energy, and talent.⁹ Such pragmatic estimates of the *price* of effective communication point to a distinct set of questions concerning the mechanisms which might best insure its availability at appropriate levels.

In the way that both social and market mechanisms work, for example, it would not be surprising to find a *third group* of scientists, not simply identifiable with either party to the dialogue sketched above, taking over the task of synthesizing, unifying and generalizing the significance of the otherwise disparate results generated by molecular biologists and animal learning theorists.

Such a group has begun to form around an effort to build generalizable computer models of animal learning based on elementary properties of the sort established by *Columbia* and cogent phenomenological generalizations of the sort established by *Cambridge*. (See Sutton and Barto, 1981; Gelperin, Hopfield and Tank, 1986; Gluck and Bower, 1985; Gluck and Thompson, in press; Squire, 1986; Thompson, 1986.) As Gardner (1985: 390-92), suggests, such tripartite activity involving psychology, neuroscience, and computer modelling/artificial intelligence are characteristic of efforts in the "strong" version of cognitive science. But discussion of the role of third parties and third theories in the task of inter-theoretic synthesis or reduction must be postponed for appropriately full discussion in another paper.

Conclusions and Summary

The readers of this journal do not require yet another argument for the mutual relevance of the history, philosophy, and sociology of science. The intent of this article has been to assume that relevance and put it to work in the analysis of another case study. I have sought to examine the real diversity within scientific communities party to the putative reduction of one theory to another, and to examine the explicit networks linking centers of research in two different fields with sufficient care to distinguish strong and weak points in patterns of communication between them. My argument is that each of two fields, the molecular neurobiology of learning and memory, on

the one hand, and analysis of the associative learning resulting from classical conditioning protocols, on the other, exhibits a growing technical and conceptual complexity and that neither field has articulated a consensus in support of a single theory. This intra-field situation, not surprisingly, results in significant turbulence in inter-field communication. From this broad finding, I draw two further conclusions:

1. The identification of failure in explicit, public communication on a key issue affecting the convergence of molecular neurobiology and psychological studies of Pavlovian conditioning suggests an important problem for historians and philosophers of science. How is one to discuss the epistemic status of explanations linking disparate domains in the *absence* of objectively effective communication linking their published research results? The carefully trained historian and the carefully trained scientist are aware of the impropriety of creating evidence where none exists. Philosophers, tempted to use and to contribute to science much as logicians use and contribute to mathematics, often attempt, albeit inadvertently, to fill in the blanks. The possibility that significant scientific information might be lost because of such maneuvers (Shapere, 1986) should not be overlooked. Biologists respect organic species; philosophers of science ought to respect significant data bases.

In such contexts, some philosophical discussions of reduction have the status of conjectural histories of the future of science. Participating philosophers tend to change places with the scientists they are studying, or to "go native," and exhibit a weakened sense of the strengths and limits of their own cultural identity. Philosophers who act as participants in rapidly developing and changing scientific activities run a heightened risk of getting everything wrong, of misdescribing and inadequately analyzing the historical events whose cultural significance they must assess. The challenge is simple: philosophers must develop adequate tools for discussing the *reduction, synthesis, or transformation* of scientific theories from fields which are rapidly growing and changing before their eyes.

2. The hypothetical and rational reconstruction of scientific dialogue which has been presented above represents *my* conjectural history of the future of neuroscience. If it proves correct, and does not indisputably convict me of the madness of "going native," what philosophical conclusions ought to be drawn from it?

Cellular and molecular mechanisms capable of explaining animal learning as captured by classical conditioning protocols according to the pattern I have suggested would *not* constitute a "rough" or "eliminative" reduction (Churchland, 1981) similar to the replacement (or elimination) of phlogiston by oxygen theory. Perhaps it ought to be called a "symbiotic," "symmetrical," or "retentive" reduction in which each

field continues to present problems to the other; the influence of each field upon the other transforms the conceptual framework of the other; and neither field eliminates or replaces the other (Danto, 1983).

The term *reduction*, however, naturally suggests theoretical and ontological replacement. The reduction of the Boyle-Charles law suggests that heat just is kinetic energy. Such talk proves awkward in complex biological contexts, where it seems more appropriate to refer to the *conceptual changes*, e.g., associated with the development of our understanding of *gene* during the past century (Kitcher, 1982). Two decades ago, Mary Hesse (1966) put forth the notion of *semantic tension* in discussing the role of scientific metaphors, forms of reference in which talk about both the primary and secondary referent of the metaphor (the lion and King Richard the lion-hearted) is forever transformed because of the metaphor's power and currency. This view has much to recommend it.

Notes

1. Larry R. Squire, "Mechanisms of Memory," *Science* 232 (1986): 1612-19, suggests an intermediate, neuropsychological level of analysis, between cellular and molecular neuroscience and psychological studies of memory. See also Sutton and Barto (1981) and other references cited in the final paragraph of the paper's penultimate section.

2. Hesse 1986. I make no claim to originality concerning a theme which has been developed in quite different ways by such thinkers as D. T. Campbell, M. Hesse, K. Knorr-Cetina, S. Restivo, and R. Rorty. Nor do I wish to commit myself either to critical attack or to alliance and defense of any one of their quite distinct agendas.

3. Concrete realization of this ideal is emphatically *not* at stake here. Its use as an analytic and critical tool is compatible with considerable skepticism concerning the actual occurrence of effective communication in *any* context. Regulative ideals are more like "game plans" and less like standards of physical measurement. That is, they can guide the planning and critical appraisal of action without ever being realized in a state of affairs.

4. Portions of this dialogue appeared in *PSA* 1986 (vol. 1) and are replicated here with permission.

5. To illustrate these points: Pavlov's dogs salivated when presented with a CS (bell) associated with food, but they did not lick, chew, bite or swallow (as they did when presented with food, the US). Secondly, different stimuli (e.g., light and sound) associated with the same reinforcer (food) elicit entirely different responses. Pigeons peck at light sources associated with food, but give no such response when tones are comparably established as signals of food. Finally, so-called opponent responses also indicate the non-identity of the CR and the UR: when a CS is established as a signal of the occurrence of a US, it may elicit an anticipatory response (crouching) opposed to the consummatory response elicited by the US itself (attack or flight).

In higher order conditioning, an organism might be trained to associate a specific tone (CS₁) with a reinforcer (US), and then trained to associate a particular visual pattern (CS₂) with the tone (CS₁). The organism would never experience concurrent presentations of the visual pattern and the reinforcer. The organism effectively exemplifies higher order conditioning if it then responds to the visual pattern as if the pattern were a signal of the reinforcer. A pigeon trained to associate auditory stimuli with food exhibits no behavioral modification when tested by presentations of the auditory stimuli alone. If in the next stage of training, the pigeon is

trained to associate the illumination of a circle in the side of its cage with the auditory stimulus from first stage training, it will respond by pecking at the illuminated circle. It is as if the pigeon learned to associate the tone with the food, but such learning did not express itself in observable behavior until it was experimentally implicated in a second association with the illuminated circle. It follows that what the pigeon has learned about the tone is not expressed in an SR mechanism.

Blocking, like higher order conditioning, results from a two-stage, three-stimulus training process. In the first stage, CS₁ and US are paired and in the second stage a conjunction of CS₁ and CS₂ is followed by the same US. Then solitary test presentations of CS₂ elicit no response and indicate that no association between CS₂ and US has been formed. In these procedures CS₁s block the formation of new associations even if the responses they elicit are quite different than those elicited by the CS₂s in non-blocking protocols. Signalling properties of a CS₁ are processed and stored, explaining its blocking effect. Once the CS₁ has been established as an adequate signal of subsequent reinforcement, redundant signals of that reinforcer are "blocked" and the organism processes them only briefly and ineffectively. The SR link associated with conditioning of the CS₁ is irrelevant to the blocking effect.

6. For example, the PH-80 comparators impute ability to calculate the difference between the level of reinforcement received and that predicted by concurrent (excitatory and inhibitory) stimuli to the learning mechanism. The computed result sets the value of the associability of the signalling stimulus on any ensuing trial, and the value of the no-reinforcer input on the concurrent trial.

7. The slogan, "It's time to open the black box" was frequently reiterated in the recruiting/ pedagogical context of the 1985 Cold Spring Harbor Course on Neurobiology and Behavior. In such courses leading senior neurobiologists (no psychologists) present their views to a select international audience of graduate students, post-docs, and junior scientists active in biomedical and neurobiological research.

8. Ruth Colwill's (formerly of Cambridge) results with *Aplysia* (Columbia) are being readied for publication.

9. Personal communication from Phil Quinn, 3 November 1986.

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The Hidden Agenda of Science Studies for Developing Countries

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Editor's Note: This paper was stimulated by Michael Moravcsik's 1985 4S Review article, "Science in the Developing Countries," and explores the assumptions behind the research program that Moravcsik proposes. It argues that science in developing countries needs to be viewed in full recognition of the relatively marginal role that national science is able to play in the flows of technology and knowledge that power economic development. An alternative research agenda for science studies follows this shift in perspective. Responses to this commentary will appear in a future issue of S&TS.

Introduction: Identifying the Hidden Agenda

Professor Moravcsik, in his recent article, "Science in the Developing Countries," published in 4S Review,¹ has focused attention on developing countries as an unexploited field for research in science studies. He points to two sources of motivation to do such research — one academic, the other practical. As an academic motivation, he proposes that developing countries offer a different domain to test theories and histories derived within advanced country studies. As a practical motivation, Moravcsik proposes that because most of the developing countries are in the process of building their science infrastructures, "factual information on the problems and circumstances that exist" would assist them.

Both of these goals are commendable. But the research program proposal that follows is a distraction from the issues of real importance that developing countries confront. Developing countries are in a situation of increasing disadvantage with respect to their dominance by advanced country-induced knowledge flows. They do not have time to play on the edges of "interesting" academic questions, but for the sake of survival, must focus very scarce science (and all other) resources where they can have maximum impact. The sort of "factual information" that Moravcsik proposes is marginal rather than central.

What is basically wrong with the Moravcsik proposal is that, in spite of claims to the contrary, it assumes a theory about the role of science in the

development process that is patently incorrect for developing countries. His assumed theory is that if a developing country builds science institutions as in the West, then "even a minor amount of improvement in science development would have a large impact."² This assumed theory never quite reaches the surface, but underlies almost all the research questions he proposes concerning, for example, "the nature of science," "motivations and justifications," "requirements for productive scientific work," the popular development of "scientific thinking," "development and maintenance of scientific manpower," and so on. They all focus on building science, and assume more science is good. Such a view can otherwise be described as a "cargo cult of science."³ As with New Guineans building ceremonial landing strips for aircraft during World War II in the false belief that this would attract planes loaded with industrial society's "cargo," the cargo cult of science assumes that by building good research institutions, appropriate development will follow.

However, the reality in developing countries is that science is but a marginal add-on to the knowledge flows that transform their social and economic life. The real thrust of technological progress is likely to follow from where the nation is positioned in relation to access to international technology, and the nation's bargaining power to obtain it on favorable terms. With massively higher expenditures on leading edge technologies in the West, the developing nations can expect to gain a marginal toe-hold on the future through national research, but that's all.⁴ Such a purchase on the future will however only follow if a clear priority focus is developed and the very scarce national resources are concentrated and supported to give this focus a strong power of resolution. Thus, a view of developing countries' science that assumes more science and better quality science per se are good avoids the more fundamental concern about how this research should be targeted to make any difference at all.

Furthermore, in developing countries, science is not bedded into a rich and well-prepared national technological environment like it is in the United States,

Germany, or Japan. So, no matter how “good” the science is, it simply is unlikely to *connect* with the surrounding user environment as it can in these advanced nations. Thus, while good quality “undirected” science in the West *may* produce spin-offs that quickly come to rest in commercial development, “spin-offs” in developing countries whirl out into a vacuum.⁵ There is also a severe danger, indeed an almost universal danger, that *because* the technological environment of developing countries is poor, any research results that have real industrial potential will be swallowed up by international interests simply because local industry will be unable to *translate* this knowledge effectively into commercial practice. Thus, the more that national research concentrates on aping *science-centered* practices of the West to the detriment of developing a different science that first and foremost *links* with a different environment, the more the research is likely to become irrelevant to pressing development issues. Even more insidiously, the model is likely to aid and abet the very forces that hold the national technology and economy into international obeisance in the first place.

While the science-centered model feeds *disconnection* from the local knowledge-flow milieu, it also feeds *connection* with the international centers of science development (both in international legitimation of research topics that mirror advanced nation research concerns, and in career structures that are built on international *discipline* contribution rather than local problem solving). So the model feeds the very fires that are ignited from advanced nation application of science, that burn the center out of the fabric of the developing nation’s own economy. The cargo cult approach may lead to a higher international profile in publications, but not to a central influence on the nation’s own development process.⁶

Such a set of conclusions simply cannot be seen when one stands within a narrowly defined perspective that studies “science.” It only becomes clear when one stands apart and looks at national science as *one* element in a far more pervasive knowledge generation and flow process.

It follows from this shift in perspective that one asks quite different research questions in science studies than those proposed by Moravcsik. First, a very high priority must be given to understanding how national science, whatever its quality, can be effectively *targeted* towards the most critical development contributions in the particular national context. Second, an equally high priority must be accorded to understanding how to construct *knowledge bridges* that link indigenous research with its social and economic environment. However, while these are priority questions in their own right, they also *frame* any other science studies questions about any aspect of the research process, and its comparison with practices in the

West. This is so because, along with the priority accorded to targeting and linkages, one moves from a position (such as Moravcsik’s) that assumes external power to be irrelevant to internal science direction and use, to one that assumes that external commercial and international power commands, transforms, and filters the way science is conducted and used. We can choose in our science studies to conduct research as if such an environment does not exist, or we can choose to address our questions towards what can be done *within* an international power frame to foster greater power for national science. The ideology is written into the questions.

Examples of More Appropriate Science Studies Questions

Some examples might assist the reader to see the sort of science studies questions that follow from the shift in perspective proposed in this critique. The examples relate to the highest priorities — “targeting,” “linkages,” and the “research process.” As will become quite obvious, the questions are not just about science, but treat science as one component in an overall knowledge-flow system; without knowledge of the rest of the system, knowledge about science is irrelevant.

Targeting

1. What are key links in the technological system of the national economy that have been broken by the introduction of modernizing technologies? What are the key “gaps” in the national technological system that only national science is likely to fill?
2. What areas of scientific research can be selected and fostered to reintegrate higher and lower technological systems within the economy?
3. What areas of fundamental research need to receive greatest financial support with respect to:
 - potential contribution to the development of critical national physical and social resources;
 - “generic” technologies that could have the greatest multiplier effect through the total economy?

Linkages

1. What can case studies of successful vs. unsuccessful technology transfer from government laboratories tell us about the conditions that determine success and failure in terms of:
 - laboratory liaison with users in formulating research questions;
 - the linkage of ongoing research with user interest;

- the role of fundamental research in solving practical problems;
 - the influence of laboratory patterns of expectations and rewards on the encouragement or discouragement of effective targeting of research towards user needs, and involvement of scientists in the full transfer process;
 - the role in laboratory/user liaison of alternative types of institutional mechanisms within the laboratory structure, such as commercial or liaison units, advisory committees, and joint researcher-user projects;
 - the technological "receptivity" of users to the technological changes introduced by laboratory research;
 - the levels of associated technical support provided during technology transfer;
 - the levels of associated managerial, capital, and other support provided during technology transfer?
2. How can "success" vs. "failure" in the application of national scientific effort be defined and indicated in, for example:
 - national economic contribution, impact on redistribution of social and economic advantage, technological multiplier effect within the economy, and contribution to national resource ownership and exploitation;
 - impact on upgrading the technological sophistication of the user environment;
 - contribution to the national scientific stock of knowledge; contribution to the international scientific stock of knowledge?

Research Process

1. How does the mode of *formulation* of research projects relate to subsequent application success in terms of:
 - disciplinary vs. problem centered/multidisciplinary focus;
 - organizational strategies that encourage single vs. multidisciplinary approaches to research;
 - modes and efficiency of access to international scientific literature and data base sources;
 - modes and efficiency of access to national and international sources of information on the kind of problem being researched?
2. In what ways is the national scientific capability transformed from a Western "universalistic" science model according to:
 - organizational, bureaucratic, planning, reward, and status systems;
 - local cultural influences on, for example, the meaning of research, the actions that are appropriate in dealing with the organization's user environment, conflicts between modernizing and traditional cultures;

- international vs. local connectedness of the national scientific enterprise, as demonstrated in, for example, publication patterns, international flows of scientists, and patterns of training?

3. What are the effects of such transformations on local research and application effectiveness?

Concluding Remarks

The alternative perspective presented in this critique does not deny that the particular conditions of science in developing countries are worthy of study. Rather, it suggests that behind the approach presented by Moravcsik lies a hidden agenda that more accurately reflects the ideological location of science in advanced nations, and seriously distracts from an understanding of science in developing nations. In particular, the "cargo cult" perspective on science studies reifies national science above the total pattern of knowledge flows that create technological and economic development. The perspective assumes that what is good for science in America is good for science in the Third World, and that the radically different power of science within these two contexts is irrelevant; the perspective further assumes that generalized rather than targeted growth of science is an appropriate strategy for a nation where scientific resources are extremely scarce. As soon as one looks outside the laboratory window in the Third World, it becomes very clear that these assumptions are simply incorrect. Meanwhile, there is a desperate need for information as a platform for Third World science policy, so that distraction in science studies towards the ideological assumptions of the West could be dangerously misleading to the science policy that evolves within the Third World. It could indeed contribute to retaining the Third World in the position of relative powerlessness they presently are seeking to escape through national scientific effort.

It is for this reason that the intention of this paper was to demonstrate that when one commences with the alternative assumptions that science is for the knowledge-poor, and is set within an internationally dominated power environment, a quite different agenda of science studies questions emerges.

Footnotes

1. Michael J. Moravcsik, "Science in the Developing Countries: An Unexplored and Fruitful Area for Research in Science Studies", *45 Review*, 3, 2-13 (1985).

2. *Ibid.* p. 3.

3. For a detailed presentation of the way that such a "cargo cult" of science can distort research practice in a developing country, see Stephen Hill, "Contrary Meanings of Science - Interactions between Cultural and Personal Meanings of Research in a Developing Country Research Institution." Pp. 195-230 in Stuart S. Blume (ed.), *Perspectives in the Sociology of Science*, London: John Wiley, 1977.

4. The present critique is intentionally a perspective' paper. The assertions here about the role of science in Third World countries are supported by a detailed analysis of science in the Asian and Pacific region. This evidence is presented in A. Rahman and S. Hill, "Science, Technology and Development in Asia and the Pacific - Progress Report, 1968-1980", UNESCO: SC82/CASTASIA II/ Ref 1, (1982).

5. Even within the most advanced countries, the incidence of significant spin-offs from fundamental research is severely restricted. The assumed centrality of such a concept derived from the rather more spectacular advances that have followed in technologically-rich environments of the American space program massively funded electronics research, and so on. When looked at the other way around, i.e., in terms of the relative contributions to the pattern of technological changes that have occurred in general industrial development, the significance of "spin-offs" is radically reduced; "spin-offs" only come to rest when there is a highly receptive technological environment awaiting them. See R. M. Bell and S. C. Hill, "Research on Technology Transfer and Innovation."

Pp. 225-274 in F. Bradbury et al. (eds.), "Transfer Processes in Technical Change." The Netherlands: Sijthoff and Noordhoff, 1978.

6. This "other view" of the role of science in developing countries is now being recognized, not only within some academic circles, but also by policy decision-makers in the Third World. To take but one example, since UNESCO's 1982 CASTASIA II Conference (on Science, Technology and Development within Asia) — where Government Ministers from the 26 nations of the region participated — there has been a growing realization within government circles in the region that there is a need to switch gears to a "demand" or environment-centered approach to science from a "cargo cult" perspective. This has emerged particularly in High Level Regional Conferences and Training Programs that the Center for Technology and Social Change has been organizing over the last two years.

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Frontier Science and Textbook Science

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Shapere, Carroll, and Turner¹ agree on some important aspects of science: that the reliability and coherence of modern science are striking, and that external as well as internal factors influence science. But the degree of agreement could have been more evident had the distinction been recognized explicitly between well-established science and science-in-the-making.

At the frontiers of science, (almost) anything goes. Working scientists display degrees of competence that range over the human spectrum and therefore—if for no other reason—what they do is influenced by many things that are not norms or ideals of science. Individuals, groups, and institutions seek to have science serve their ideological ends, and they lobby among their peers toward acceptance of some part or corollary of their particular sets of beliefs. Even were there the ideal scientist, he would still make mistakes when trying to do or to understand something quite new, and his mistakes would tend to be in the direction of his wishes—his choice of hypotheses would be influenced by what he regards as desirable, and he would tend to see the data that support his ideas and to miss those that conflict. Science-in-the-making, or frontier science, is heavily subject to external factors.

But science-in-the-making is not all there is to science. As Polanyi² and Ziman³ in particular have cogently argued, there is a “republic of science” in which theories and data and paradigms are subjected to mutual criticism; a thing becomes an actual part of science only when consensually accepted by the appropriate scientific community or sub-community.

The requirement of consensuality inevitably filters out some of the external influences—those stemming from the idiosyncracies of the people who have carried out the work, since those idiosyncracies are not likely to be shared by all the referees, editors, and other critics to whose judgment the work is subject. Only consensually accepted work is eventually incorporated into textbooks; and it is the coherence and reliability not of frontier, but of *textbook* science that we find striking. Textbook science generates few arguments, whereas matters on the frontiers are almost invariably matters for argumentation.

Shapere, of course, was talking chiefly about textbook science. For example, when he says, “the situation in modern science is radically different from

what it was in early periods” because of the progress made in “learning how to learn about Nature,” we can only agree — provided we are concerned with those parts of modern science that pertain to well-developed disciplines or parts of disciplines. In not-so-well-established specialties, we are still very much in the process of learning how to learn; even in those parts of physics that have to do with gravity waves, say, or with magnetic monopoles.

Again, on the matter of “real flesh-and-blood humans,” Shapere is clearly concerned with those parts of science that have already run and survived a considerable gauntlet of competition and criticism; this means that ideology and wishfulness have been largely filtered out to leave things about which wide agreement is possible.

By contrast, Carroll is talking chiefly about frontier science, about science-in-the-making, when he draws attention to the fact that scientists are flesh-and-blood humans; and so is Turner when he speaks of “*prospective* judgments and expectations” — research *strategies* (emphasis added) influenced by individual sub-beliefs. In talking about cosmology, Turner chooses a subject that will always remain frontier science; and in talking about stream-crossing, Turner focuses only on the path across that happens to be taken, which is again science-in-the-making (even when viewed in retrospect, be it noted). It is the solidity of the other bank, when reached, that constitutes textbook science. There may be many ways across the stream, but there are only two banks, and neither is influenced by the different ways across that different people take.

Thus much of the apparent disagreement among Shapere, Carroll, and Turner results from their implicit concentration on different aspects of science: Shapere focuses more on textbook science, Carroll and Turner on frontier science. This is not, of course, a sharp distinction of matters of kind: frontier and textbook science are the extremes of a continuum. What Shapere says is merely more true toward one end than toward the other, just as what Carroll and Turner say is more true toward the other end than toward the first; sharp distinctions can rarely, if ever, be made in science studies (in contrast to within science, see below). Nevertheless, I suggest that the distinction between frontier science, science-in-the-making, and

textbook science, well-established and widely accepted, is a significant one. That distinction is not the same as (but overlaps with) the distinction between the contexts of discovery and of justification. It is not the same as that between normal and revolutionary science—much science-in-the-making is perfectly normal science. Nor is all contemporaneous science necessarily frontier science; many scientists practice textbook science, seeking to apply or to refine or to amplify, not to generate new or wider understanding (again, of course, a matter of degrees and not either-or). And “textbook” science is not necessarily correct or true, of course, though it is much more likely than frontier science not to be untrue.⁴

Shapere’s description of the piecemeal approach and its success, and corollaries of that, I found very useful; so, too, is Turner’s concise illustration of the work of sociologists and their need when looking at science to *create* contrast-spaces. It may be that the piecemeal approach, successful within science, cannot be so successful in science studies. The striking successes of science have come in fields where distinct categories could be discovered and used; science studies deals with matters of degree and not of kind: the continuum of influencing factors, internal to external; a continuum of normal to revolutionary bits of science; disciplines and sub-disciplines that span the range of young to mature; the variability I have discussed above, frontier to textbook science; and so forth. Chemists (say) have the luxury of dealing with a finite number of discrete elements, and a very small number of forces that can rationalize all the interactions of atoms and of molecules; moreover, it turns out that the magnitudes, the values, of most properties can be calculated by simply additive means. But students of the activity of science cannot do anything analogous. For example, we cannot aim to evolve a formula by which the degree of external as opposed to internal influence on a bit of science can be estimated from knowledge of where into the structure of scientific knowledge that bit fits, and when it was discovered, in what country, by man or woman, in a large or a small laboratory, a well-known or an obscure one, by atheist or believer . . . and so on. Yet we have to admit that those and many other factors probably do influence the degree to which external factors played a role in the particular discovery.

In one sense, then, Shapere’s call for a piecemeal approach in science studies is very well taken. Surely we know enough about science to recognize that sweeping statements about the whole of science are unlikely to be widely accepted, let alone to be true. Indeed, the burden of this comment has been to suggest that a piecemeal approach, differentiating between frontier science and textbook science, would have made the exchange among Shapere, Carroll, and Turner more immediately productive.

At the same time, in applying such piecemeal distinctions, we need to remember that the distinctions are not inherently sharp ones. It can hardly be productive, then, to argue on the one hand for the decisiveness (say) of external factors in science, and on the other hand for the decisiveness of internal ones, when the degree of influence varies for different bits of different sorts of science. Rather, the task is to elucidate increasingly the mix of factors that might tend (and only *tend*) to strengthen the effects of external in contrast to internal factors, though that mix of factors cannot be expressed in a meaningfully additive way. It must be a process of continually adding and refining nuances, and defining more and more clearly under what other conditions any given factor is most likely to express itself strongly.

Much discussion has consisted not of attempts to refine or to add, but flatly to contradict sweeping statements with other equally sweeping ones. For example, Kuhn’s distinction between normal and revolutionary science immediately rang true for many practicing scientists, as did his notion of paradigm. Naturally both the distinction and the concept needed refinement, the adding of nuances and qualifications; yet much of the criticism, especially at first, seemed to be attempts to argue in sweeping terms against the very distinction itself and against the very possibility of defining rigorously and usefully the concepts underlying Kuhn’s uses of “paradigm.” In other words, the critics seized on what might be wrong rather than on what might be right — or the difference between destructive and constructive criticism. Science studies needs to build understanding through the cumulation of nuances and qualifications to distinctions that can never be true in more than qualified ways; and it needs to build by a piecemeal approach—at least until someone has shown how human beings can come to understand a complex matter through some other approach. In such a process, arguments over sweeping generalities are unlikely to take us much further—as indeed Shapere, Carroll, and Turner have all agreed.

NOTES

1. *Science & Technology Studies* 4 (1986): 1-23.
2. Michael Polanyi, “The Republic of Science,” *Minerva* 1 (1962): 54-73.
3. John Ziman, *Reliable Knowledge* (London, New York, & Melbourne: Cambridge University Press, 1978).
4. Errors in textbook science are discovered periodically; and textbook science typically ignores anomalies or lacunae for which no immediate possibility of solution is seen. On the ignoring of certain advances, see Gunther S. Stent, “Prematurity and Uniqueness in Scientific Discovery,” *Scientific American*, 1972 (December): 84-93; for an example of the prolonged use of a “constant” that is not constant, see Henry H. Bauer, “The Electrochemical Transfer-Coefficient,” *Journal of Electroanalytical Chemistry* 16 (1968): 419-432.

Research Management at the University Department

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Background

What encourages academic research quality and productivity? The question is of more than theoretical interest. If research productivity is a function of factors other than individual intellectual talent, this has important implications for the development of research systems in the nation.

The study summarized in this paper was carried out while the authors were members of the Productivity Improvement Research Section at the National Science Foundation. The general mandate of the section was to uncover the processes contributing to technological change and, by extension, the enhancement of U. S. industrial competitiveness. Studies undertaken by the section included a review of the literature on innovation (Tornatzky et al., 1983) and evaluations of NSF programs such as university-industry cooperative research projects (Johnson and Tornatzky, 1984), university-industry cooperative research centers (Eveland and Hetzner, 1982), and the NSF Small Business Innovation Research Program (Tornatzky et al., 1982).

Previous efforts to understand the research process and the circumstances that promote or retard research have taken a number of different approaches. Some studies have focused upon the characteristics, work habits, and attitudes of individual scientists. Others have been concerned with the management of research groups, and with issues such as "project stop-

ping" rules (Balachandra, 1983) or the internal decision making patterns of research teams (Allen, 1979). Still other studies have emphasized the development of fields of scientific inquiry, examining how new specialties emerge and grow.

These lines of inquiry, although informative, have left one important question largely unanswered: what causes particular academic departments — as organizational entities — to be more productive than others? In studies of individual faculty members, variations in research productivity far exceed variations in ability or personality traits. In studies of emerging specialties, the reasons why a field expands rapidly at particular institutions are often unclear. Studies of R&D management have been limited in the types of organizations studied, with a major emphasis on profit-oriented industrial laboratories. In short, current research on scientific productivity has not carefully examined the individual/organizational interactions involved in such activities, particularly within the administrative context of the university.

Objectives and Methodology

The study was concerned with factors having the greatest influence on university research at the departmental level (particularly industry-sponsored research), the nature of the incentives that affect such research, and how academic organizations structure themselves to encourage research activity.

To meet these objectives, a national survey of academic departments was conducted. The sample for the study consisted of all chemistry, economics, and electrical engineering departments at the top 100 research universities in the United States. The rationale for focusing on these three disciplines was to study how different epistemological orientations might translate into different research management practices. By gathering data about a physical science, a behavioral science, and an engineering department, potential departmental variations within a university

The study described in this paper was completed while the authors were members of the Division of Industrial Science and Technological Innovation, National Science Foundation. Any opinions, findings, and conclusions or recommendations expressed in this paper are those of the authors and do not necessarily represent the views of the National Science Foundation or of the authors' current institutional affiliations.

The authors wish to express their sincere thanks to all those who assisted in this project. Particular thanks are due to Dr. Janice Beyer and Dr. John Lounsbury for preparing some of the indices, and to Andrew Wyckoff, Marjorie Myers, and Brenda Smiley for effective and patient research services.

would be maximized. Some universities did not have one or more of the identified departments, so the effective sample was 292 academic units. Of these, complete data were received from 259 departments, which was an 89% response rate for the mailed questionnaire.

The questionnaire itself was 17 pages in length, covered 35 topic areas, and was coded into 154 specific variables. Some items were written as Likert scales; others were open-ended or direct inquiries. The questions covered six categories of data: 1) faculty composition, as measured by number of faculty, rank, and previous industry experience; 2) teaching load; 3) departmental policies on tenure, travel, allocation of research assistance and equipment; 4) research policies of the institution regarding consulting, financial support, release time, and overhead; 5) university-level administrative practices; and 6) several indices of research support received, including amount, sources, and changes over time.

In addition to the questionnaire data, two indices were computed from archival sources: a measure of departmental citation rates, and an index of faculty prestige. The latter was computed as the proportion of faculty in a department who had received their terminal degrees from top-ranked schools. The publication index for each department was based upon total number of citations to each faculty member's work during the most recent year; data were then averaged across all department members.

Summary and Policy Implications

The results of the study may be captured in the following summary observations:

1. Academic research is a "mature industry".

The organizational context in which university research is carried out exhibits many of the characteristics associated with well-developed sectors of private industry. Established institutions tend to dominate, and the size distribution (in terms of total research funding) is sharply skewed in favor of a few large organizations. A relatively large portion of the employees (faculty members) are in the upper seniority ranks: over half the faculty members in the study sample were full professors, and approximately three-quarters were tenured.

To cope with this top-heavy structure, there is considerable reliance on a sizeable "secondary labor market" of nontenured personnel to support the upper, more favored, ranks. Two questions on the survey explored the use of non-tenure-track, part-time staff: these might include joint appointments, visiting faculty, part-timers, research associates, and post-doctoral scientists. Across the sample, such special faculty categories added 53% more individuals to a department; however, this differed significantly

across discipline and institution. For example, in chemistry departments at private universities, individuals in special categories outnumbered regular faculty members by almost 25%. In all, private institutions tended to use such special personnel 40% more than public universities.

The use of non-faculty personnel correlates strongly with productivity, as measured by the citation rate for the department ($r = .61$). Clearly, such personnel do affect the rate of publication of the faculty. It is unclear whether such non-faculty personnel contribute by directly assisting with the research, or by taking over teaching, thus freeing regular faculty for research. The study data suggest the latter interpretation: as the percentage of non-faculty personnel increases, the teaching load declines.²

To the degree that academia is "mature," some problems may be posed for the future. Clearly this study is not the only one to note this phenomenon; there have been many analyses that discuss what the increasingly top-heavy structure of academic science means for the future development of trained productive personnel. It is evident from these data that such senior departments are "productive" in the sense that they get funding and get cited by others.³ How long this state of affairs will continue, and what it bodes for the future, have been subjects more of speculation than empirical research.

2. Academic departments, even within the same university, differ markedly by discipline.

This finding is not quite as obvious as it may first appear. While it has been contended by many that different fields of science operate with rather different social patterns, empirical evidence has been less than overwhelming. In this study, statistically significant differences across discipline were found for many variables relating to departmental structure and incentives, as well as several dimensions of research activity. Funding levels, both internal (direct university funding) and external, varied significantly. So did citation rates, proportions of faculty at senior levels, use of nonfaculty supernumerary personnel, use of research in tenure decision processes (electrical engineering departments placed the least and economics departments the most weight upon research), and the proportion of faculty with industrial experience.

There are implications of these differences for research policy. The data suggest that the department is very much the appropriate level of analysis for understanding university research. If social and institutional patterns differ across fields of science, application of similar policies aimed at increasing research activity can be expected to have rather different effects in different disciplinary contexts. This study found rather wide differences among three disciplines; it can only be speculated what the true range of variation across all fields might be. Setting academic research

policy, either at the university or at the Federal level, is likely to be rather more complex than policy makers might like to believe.

3. Public and private universities also differ, but along different dimensions.

Reflecting their different mandates, public universities tend to place greater emphasis on teaching. While there were only limited differences in course load, as measured by the number of courses taught in an academic year, the number of student credit hours varied dramatically. On a per FTE (full-time equivalent) basis, faculty in public universities taught over 600 student credit hours, as opposed to roughly 400 for their colleagues at private institutions. This trend was paralleled by the number of undergraduate majors per FTE, with a significantly greater load on public institution faculty.

Not surprisingly, these *de facto* incentives tended to be formalized in promotion criteria. The vast majority of departments in the study (88%) used criteria that were formalized in written by-laws, memoranda, or other documents. Overall, more than half the weight in tenure decisions was given to research, although there were important differences between disciplines and between institutions. Private institutions placed significantly more emphasis on research than did public universities.

Other differences were the greater research productivity of private institutions (faculty at private universities were cited more often) and their tendency to hire more prestigious faculty, provide more financial support for faculty research, and to make greater use of non-tenure line research personnel. For example, across the three disciplines in the study, private universities had 63% of their faculty members trained at the top 15 departments in the field, while only 49% of the faculty at public institutions came from top-15 schools. Departments at private universities also had significantly lower rates of faculty unionization, higher indirect cost rates and somewhat more relative control of decisions than faculty at public universities.

"More highly cited" does not necessarily imply "better." Universities appropriately fulfill a variety of roles, including teaching and community service as well as research. Public universities are a heterogeneous mix, and the findings of this study may imply that they are suitably configured to their mission. However, to the extent that public universities aspire to be centers of research excellence, they may need to emulate some of the organizational practices of their private sector peers.

4. Incentives affect research output.

Research output is affected by the incentive/reward structure, but these effects are definitely moderated by individual and system characteristics. The study included information on incentive and disincentive variables (research support, availability of research

support and travel money, teaching load, students per faculty member, emphasis on research in the tenure process); characteristics of the individual faculty members (rank, industrial experience, rank of Ph.D. granting institution) and the academic organization (discipline, department size, public or private university, use of part-time and joint faculty, distribution and amount of control, formal tenure criteria and consulting policies, unionization, indirect cost ratios); research outcomes (publications, citations, and outside funding); and research rewards (formal recognition for research contributions, allocations of overhead funds, release time, and research assistance based on previous research). Although incentives and rewards were both hypothesized to influence faculty behavior, rewards were contingent upon research by faculty, whereas incentives were not.

Canonical correlation analysis⁴ was used to examine the relationships among incentives, participant characteristics, research outcomes, and rewards. As the concepts were defined in the study, incentives tend to lead to research (as moderated by participant characteristics), but research does not in general tend to lead to rewards. Incentives have a strong relationship to research output; the incentive variables developed for the study explained over half the variance in the research outcome variables. The relationship between system characteristics and research measures is also strong, the former accounting for nearly half the variance in the latter.

Rewards have relatively little relationship to research output: the correlation between rewards and research measures is the lowest correlation between any two categories, and suggests that "research is its own reward." Rewards are, however, closely related to incentives (canonical $r = .768$), which suggests that these two categories may in fact be measuring the same concept.

Institutions seeking to increase their volume of research are apparently more likely to be successful by upgrading their personnel and changing their incentive structure than by tinkering with the reward system. It is worth noting that the awards that matter most — released time and allocation of resources for research — are the more direct and personal rather than institutional-level rewards.

5. Industry funding and government funding are different animals.

The relationship between receipt of funding from government and receipt of funding from industry is not large. (The correlation between government and industry funding is $+ .30$). As indicated in the next two sections, the institutional factors that are correlated with government funding are also completely different from those that affect industry funding.

There are no strong indications that government or industry prefer particular types of institutions, such as

private industry favoring private institutions. Both sources tend to fund the richer institutions more heavily. It should also be noted that in the sampled departments, roughly 83% of research funding came from government. For all practical purposes, research in the American university is a Federal activity.

6. We have a fairly clear picture of what is related to government funding.

A stepwise regression⁵ of the incentive, reward, and structural variables on government and industry funding levels provided some extremely interesting results. Six variables were significant, and accounted for 28% of the variance in government funding, a relatively effective level in cross-sectional data. The most important variables in the prediction of government funding are faculty prestige, as measured by the proportion of faculty from top-ranked schools (this variable alone accounts for 21% of the variance); faculty seniority; and the indirect cost ratio of the university. Government funding seems to go to the "senior stars" at well-heeled research organizations.

As interesting as what factors enter the prediction equation is the list of what factors do not predict research funding. For example, the weight given to research in the tenure process, which one might suppose to be a significant research incentive, is not related to funding. Neither are the degree of faculty control of decisions nor the amount of non-financial university assistance provided to potential researchers.

7. We have no good idea as to what leads to industry funding.

When the stepwise regression for industry funding was examined, the variables accounted for only about 6% of the variance. Only three variables were significant (a low teaching load, lack of a faculty union, and a tendency not to return overhead funds to the department), and these variables do not represent a coherent picture. The lack of relationship between industrial funding levels and the number of faculty in the department with industrial experience was also a surprise. In short, these prediction equations raise as many questions as they answer.

Even so, the results of this study should be of interest to departments attempting to influence their research output. For although individual characteristics are important, the organizational environment and incentive structure provided by the university also has an influence — a big one — on the output of research at academic institutions. The findings should also stimulate some concern on the part of research funders regarding the criteria by which they allocate their resources, and what the long-term effects of those criteria might be.

Notes

1. Many of these studies are summarized in NSF (1983).
2. The zero-order correlation is $-.36$; its relative attenuation is accounted for by the significant number of departments low on both scores.
3. One problem with this inference of productivity is that citations are typically for work completed at some time in the past. Whether current citation rates are then an appropriate reflection of current research productivity is open to some debate.
4. This technique measures relationships among sets of variables rather than individual variables per se. It uses the information provided by the many individual variables to develop one or more linear combinations, known as "canonical variates". The number of canonical variates utilized depends on the number of dimensions which appear independently in the original set of data. A separate set of canonical variates was developed for each set of variables — incentives, rewards, research output, individual and system characteristics. Canonical correlation measures only correlation, not causation.
5. The regression used a forward-selection algorithm with an inclusion alpha-level of $.15$. Intercorrelations among the predictor variables were not sufficiently large to pose serious multicollinearity problems (none higher than $.3$).

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Endorsing Referees:

Helen Hofer Gee
Edward J. Hackett

1986 John Desmond Bernal Prize

David Edge
Science Studies Unit
University of Edinburgh, UK

This citation was delivered by David Edge, President of the Society for Social Studies of Science, at the 45 Banquet, October 15, 1986.

The winner of the 1986 John Desmond Bernal Prize is Professor Michael Mulkay, of the University of York. The award is given in recognition of his outstanding contributions to social studies of science over the past 20 years.

Mike Mulkay left school at 16, and re-entered academic life as what we now call "a mature student." He graduated in Sociology at the London School of Economics in 1965. He then spent three years in Canada, teaching at the Simon Fraser University in British Columbia; two years in Scotland, at the University of Aberdeen (which awarded him his doctorate); and three years in East Anglia, at the University of Cambridge—before, in 1973, Mike completed his pilgrimage around the British Commonwealth by taking a post at one of its spiritual centers, York, in the Department of Sociology. He was promoted to Reader in 1975, and to a Personal Chair in 1981. For the past two years, he has been Department Head. (I understand that these events have no connection with the recent fire in York Minster, which is believed to have been caused by a controversial promotion at *Durham*.)

This impressive academic career has been (and this will be no surprise to certain other sociologists now present) closely correlated with an equally impressive publication record. Indeed, one might say that, in just 20 years, Mike has achieved more than one normal "careersworth" of influential contributions—and, at the ripe young age of 50, he shows promise of at least one more careersworth to come.

Some years ago, Mike told me about the recorder band in which he and his family performed. Mike played the bass recorder—and as he said, "I look on myself as someone who plays the bass line while others elaborate the melody and the harmony." It is a compelling, if modest, metaphor: *compelling*, for Mike's work has, indeed, underscored and consolidated much innovative research in the past 20 years; but *modest*, for Mike's bass recorder has often (and effectively) taken over the solo lead.

Mike's work has been characterized by two consistent themes: one is the study of natural science and natural scientists—physicists, radio astronomers, bio-

chemists, and their work and writings—with careful and detailed consideration of complex empirical data; the other is an insistence on rigor and clarity in theoretical argumentation and methodology. These virtues are embodied in a notably lucid writing style. Anything written by Mike is always a pleasure to read: his bass recorder emits a strong, firm line.

Mike's theoretical and methodological interests were signalled by his first book (his doctoral thesis), in 1971—*Functionalism, Exchange and Theoretical Strategy*—and subsequent books have developed his two themes in fruitful tandem—*The Social Process of Innovation* (1972), *Science and the Sociology of Knowledge* (1979), and *The Word and the World* (1985). Add to those his co-authorship of *Astronomy Transformed* (1976) and of *Opening Pandora's Box* (1984); his co-editorship of *The Emergence of Scientific Disciplines* (1976) and of *Science Observed* (1979); his two magisterial review papers ("Sociology of the Scientific Community" 1977 and the "West" part of "Sociology of Science in East and West" 1980)—and the magnitude of Mike's achievement becomes clearer.

But his *c.v.* lists a further 57 other papers. (Why 57? Why not 42? "What is the Ultimate Question?" indeed! Has the Secret of the Universe something to do with *baked beans*?) A few of the earlier titles on Mike's list will ring bells, and recall again his consistent impact: "Some Aspects of Cultural Growth in the Natural Sciences" (first drafted just 20 years ago); "A Sociological Study of a Physics Department"; "Conformity and Innovation in Science"; "Three Models of Scientific Development"; "Norms and Ideology in Science"; "Knowledge and Utility"; "Interpretation and the Use of Rules" . . . It may be that the later Mike now looks back at some of these works of the earlier Mike and, consistent as ever, argues that these should not now count as a legitimate basis for the award of a Prize. But I am sure that I can speak for the Society in saying that, if he does believe that, then he is just *wrong*.

But I must now take off my Presidential hat and speak, not for the-Society-and-me, but for myself solo. I am personally delighted that circumstances have so conspired that it should fall to me to deliver this citation. (Note, by the way, that "citation" is a technical term: a "citation" should never be confused with a "reference.") Early in 1970, Barry Barnes wandered

into my office: he had just been reading *Social Research*. "Do you know," he said, "there's a guy in *Aberdeen* who actually *understands Kulm*." As an Aberdonian myself, I was not quite sure how to take Barry's remark! But we sent Mike an invitation. On 22 April 1970, he led for us a splendid seminar on "Paradigms and Cognitive Norms." It was a Red Letter Day for me (and I guess for him; too). For, over the post-seminar supper, we casually asked Mike what his future plans were: he told us that he was moving in the summer to Cambridge—where he hoped to interview with an interesting group of pioneer radio astronomers. I must confess that my first reaction to this was to echo Captain Mainwaring's immortal line in the BBC TV series, "Dad's Army"—"Keep your hands off my Privates!" But reason soon prevailed. In a masterly display of self-exemplification, it took us only a few

minutes to decide that collaboration was the obvious strategy. And so began a relationship that has lasted over the years, and has been, to me, both a pleasure and an education.

And now, in asking Mike to accept the 1986 Bernal Prize, I am asking him to embark on another kind of exercise in self-exemplification. One of Mike's recent papers is entitled, "The Ultimate Compliment: A Sociological Analysis of Ceremonial Discourse." There, and in *The Word and the World*, Mike discusses, with characteristic wit and insight, the rituals and rhetoric of scientific awards and prizes. So, in awarding him its 1986 Bernal Prize, the Society has set Michael Mulkay an appropriate challenge to his prodigious reflexive talents. We look forward to your response, Mike, with anticipation and relish!

A Black Day For the 4S

Michael Mulkey
University of York, UK

Thank you very much, David, for those kind and generous remarks. In a moment, I will tell the *true* story. But first I want to say very clearly that I'm greatly honored by this prize. I feel that particularly when I consider the names of those who have received it in previous years. I am proud to accept the John Desmond Bernal Prize for 1986.

What I'm going to do this evening is to perform a text that reflects the style adopted in my most recent work. Some of you will know that my recent work has moved towards what Steve Woolgar has called "new literary forms." It seemed to be appropriate, therefore, in responding to this award, for me to try out a new textual form in this acceptance speech. I will offer a kind of playful fantasy. I hope that you will come with me into this realm of the imagination.

So what I'll do, to begin with, is to show that my accepting the prize generates a self-referential paradox. This paradox will lead to the appearance of a second voice who will raise the discourse to a higher, more reflexive level. This voice will resolve the paradox. But in doing so, it will have to engage in some vicious textual deconstruction. However, in the end, you will be relieved to hear, viciousness will give way to charity, differences of opinion will be reconciled, and the discourse will close with a reaffirmation of our collective wisdom. On the way there may even be a touch of magic. You've heard of conjurors producing a rabbit out of a hat. I will try to produce a professor out of thin air. I hope that sounds OK. What is to be said should not, of course, be taken entirely seriously. Nevertheless, there is, as the occasion demands, an underlying, serious message. First of all, then, the paradox: which I call the paradox of personal inadequacy — the P P I.

The paradox arises, in part, because the award of the prize has produced in me a strong feeling of personal inadequacy. I feel that I have not done enough to deserve the honor. This feeling is normal amongst prize-winners. But in my case it leads to a rather special paradox because, as David mentioned a few moments ago, I have actually done some analysis of prize-giving ceremonies. Like all good paradoxes, therefore, the P P I is closely related to the problem of self-reference. You will remember that the statement "All Cretans are liars" causes no problems until it is uttered by a Cretan and thereby becomes self-referential. Similarly, "All Bath relativists tell the

truth" only causes us to laugh derisively when it's said by a Bath relativist.

I have to admit that I'm worse off than a Cretan or a Bath relativist. My position is rather like that of a Bath relativist living in Crete. I can neither lie, nor tell the truth, without falling into a paradox. Let me explain. In a paper written several years ago, I examined the structure of prize-giving ceremonies and I claimed that this structure requires recipients of prizes to downgrade, even deny, their own achievements and to attribute any apparent accomplishments, in large measure, to other people. Of course, this must be done in such a way that it does not suggest that those awarding the prize have made a mistake. This kind of response to prizes, to compliments and to other forms of direct praise is probably due to a fairly widespread prohibition against self-praise. The basic problem for prize-winners is that, if they accept the prize without proper humility, they will seem to be guilty of the sin of self-praise. Some "subtle, rhetorical footwork" is needed.

Now it is clear that this interpretative dilemma sometimes gives rise to utterly conventional and insincere self-abasement. If you have ever watched the Oscar awards, you will know what I mean. However, I think that, on the whole, prize-winners' feelings of inadequacy are quite genuine. It is not that people merely *say* that they don't deserve the prize. They really believe it. In the first instance, therefore, my own deep sense of inadequacy in response to the award of the Bernal Prize seemed to confirm the adequacy of my own analysis. For I fitted the pattern proclaimed in that analysis. Once I'd come to realize this, I began to feel more confident. Maybe my work wasn't so bad after all. At least this analysis of the structure of prize-giving seemed to be correct. My feeling of inadequacy began to disappear.

However, as soon as I began to believe in my analysis and to feel that my work was reasonably adequate, I began to feel like saying that I *did* deserve the prize. But as soon as I started to think and talk like this, I became a *living refutation* of my own predictions. This seems to be another illustration of a golden rule of sociological method, which says: never apply your theories to yourself. I looked in Durkheim's *Rules* and I couldn't find this rule. I couldn't even find it in Tony Giddens. But you disobey it at your peril, as my own case shows.

My situation was that I could only feel comfortable accepting the prize if I believed that my work was at least adequate and reasonably valid. But if my work on prize-giving *was* valid, it required me to feel inadequate and *not* to believe in its validity. I was in great distress. My work seemed to involve a massive logical flaw, which had only been revealed because circumstances had forced me to apply my own analysis to myself. Too late had I come to realize the true danger of reflexivity. What had seemed to be my moment of greatest triumph was being transformed by reflexivity into a dismal failure.

It was at this stage that signs of paranoia first appeared. I became anxious about the possibility that others would spot the basic inadequacy of my work. I began to wonder whether I might attend this banquet only to find that the President, instead of offering me the prize, would instead announce that "a massive logical flaw has been discovered in the foundation of Mulkay's analytical edifice and he has been 'denominated' pending a close inspection of his writings." My so-called "acceptance speech" would only confirm his accusation. That hasn't happened yet. But I still fear that David may have unchained one of the "wild men of Edinburgh" and brought him over surreptitiously, to be set loose during my talk. It may still happen. Any moment now, a strong programmer may lurch ponderously through the door, like the stone statue at the end of *Don Giovanni*, to denounce me with the words: Are you now, or have you ever been, a member of the discourse analysts' workshop?

Well, you can see what fears and anxieties the award of the prize has caused me. But I think there may be a way out: new literary forms, of course. The use of new literary forms has shown that when you have problems of reflexivity, you can introduce another voice into the discourse and let this other voice deal with them. Some of you may be familiar with this idea from *The Word and The World*. However, I've recently seen the sales figures for the first six months since publication and I know there's only a handful of you out there. So, for the benefit of those who have not yet enjoyed the peculiarities of *The Word and The World*, I must explain that in the last chapter there is a quasi-fictional account of an occasion very similar to the one we are engaged in tonight. The chapter contains a representation of a prize-giving banquet. In that text, the prize-winner is a Prof. Purple whose celebratory discourse is deconstructed by a Prof. Black.

What I'm going to do now is to hand over to another incarnation of Prof. Black. There's some risk in doing this, because in *The Word and The World*, Prof. Black's intervention causes only trouble and discord. But as you have seen, I have little choice. The only way I can avoid the problem of self-reference and defuse the Paradox of Personal Inadequacy is to pass the discourse to another speaker and to ask this independent

speaker to give a brief, objective appraisal of my work and of the award of the prize. From here on, I will retire from the scene. But before I go, I must ask you to suspend disbelief. If I am to succeed in escaping from the paradox, you must help me. You must believe that the person before you is not Michael Mulkay, but the utterly different character of Prof. Black. Before your very eyes, I am undergoing a transformation. Mulkay has gone now and has been replaced by Black's distinctive figure. Try and imagine that the speaker now is a grey haired man of middle age. He peers at you through a pair of bifocals. He's very tall and thin. He also has an English accent and is wearing a dark suit and a white bow tie. I'm sure that you see him now before you. The power of the word has brought him into being. Prof. Black speaks.

Thank you very much for allowing me to speak here tonight. I've spent the last year confined within the covers of Mulkay's book and it's refreshing to get out into the real world, to see so many new faces and to hear some new ideas. I am, of course, intimately familiar with Mulkay's work. Let me consider it systematically. It won't take very long.

What can we say of his early writings? Well, even if we are determined to be generous, the best we can say is that he tried to climb "onto the shoulders of giants." But, unfortunately, he slipped off. In those early writings, as some of you will know, he drew heavily on the ideas of such major figures as Robert Merton, Derek de Solla Price, Thomas Kuhn, and Joseph Ben-David. That's O.K. Everybody in social studies of science has done that. But *he* claimed to know better than *they* did. He had the effrontery to try to tell them all where they had gone wrong. Well, I need say nothing more. We must be charitable and put these errors of judgement down to the excesses of youth. However, there's nothing here to his credit. At best we can disregard this early work.

It took him a long time to settle down and do some proper empirical research. But eventually he had the good fortune to collaborate with David Edge in the study of radio astronomy. That, I think, is a thorough, careful piece of historical sociology. Its one notable defect is that it's much *too* thorough and careful. In fact, it is so long and packed with material that nobody has ever been known to read the whole book. A clear sign of its limitations is that only *one* copy has been reported stolen from a university library in a period of ten years. Pretty poor stuff! And this is the *best* of his work.

Despite the failure of his efforts with radio astronomy, Mulkay has continued to try to do empirical work during the 1970s and early 1980s. But he also devoted a lot of energy to writing reviews and criticising other people. Just a few examples. We find him:

- a. Telling Tom Gieryn that he's made the wrong theory choice;

- b. Telling Harry Collins that his findings can't be replicated;
- c. Telling Gus Brannigan that what he thinks he's discovered is actually a social construct;
- d. Telling Barry Barnes that his work is "lacking in interest."

I won't go on. The puns only get worse. But these illustrative examples show that we seem to have somebody who is out of step with the rest of social studies of science. It seems *very* unlikely that *he* is right and everybody else is wrong. We can see this clearly in Jerry Gaston's devastating critique of one of Mulkay's arguments. Jerry shows that Mulkay's faulty logic and poor conceptualization means that he has, in Jerry's elegant phrase, "painted himself into a corner." As an objective commentator, I can see that the same thing happens in every debate in which Mulkay takes part. Given that he claims to believe in multiple realities, it's not surprising that he has painted himself into *multiple* corners — all at the same time.

Is there anything more positive to be said for Mulkay's more recent efforts in discourse analysis and new literary forms? I would like to be able to say "Yes." After all, I owe my existence to Mulkay's experiments in analytical form. But I'm afraid that even I have to say "No."

The problem with this new stuff is that it's so abstruse and removed from the real world. For example, look what it's done to Mulkay tonight. It's made it impossible for him to give an acceptance speech without seeking outside help. Yet this help is provided by a character, me, Prof. Black, who only exists in his imagination. My conclusion is that he has become paralyzed by the poison of reflexivity and bemused by the complexity of his new literary forms. We can only hope that his current phase in his work is but a temporary aberration and that he will in due course recover his intellectual balance and get down to some more case studies, in the true British tradition, of the social production of *other peoples'* knowledge. There's some hope in the fact that Mulkay has recently been joined at York by Trevor Pinch, who has properly trained at Bath and who may be able to lead his older colleague in his declining years out of the fictional realm of plays and paradoxes, and back to the *real world* of quantum mechanics, elementary particles, and paranormal phenomena.

My objective appraisal has, unfortunately, been uniformly negative. It is evident that Mulkay has done nothing which justifies the award of the Bernal Prize. Indeed, in my judgement, he has not been right even once throughout the whole course of his career. He certainly doesn't deserve the prize. Does this mean that he should not have been given it? Fortunately for us all, my answer is: No! Not at all! To the contrary! Indeed, this is where the *true wisdom* of the 4S is revealed. For the one thing that cannot be denied is

that Mulkay does keep on trying. True — he hasn't got it right yet. But he may do one day. Such dogged determination deserves its reward. Moreover, Harriet Zuckerman's work in this connection cannot be ignored. Her studies tell us that the award of honors to outstanding figures serves particularly to motivate those who are highly gifted among the younger generation. It follows, in the light of my appraisal of Mulkay's work, that this year's Bernal Prize will motivate *everyone* in social studies of science. For this year's award shows that recognition can come despite persistent error, as long as you don't give up. The 4S has, I think, shown true wisdom this year in awarding the prize to Mulkay, *before* he has made a major contribution to the field. In this way, the society has encouraged us all to continue to try hard and to do our best with the confident expectation that the 4S will respond more than generously to our honest efforts, as they have done on this occasion.

The tasks set by the previous speaker are now completed. The initial paradox has been avoided, although perhaps not entirely resolved. A new form of acceptance speech has been attempted, though how successfully is uncertain. And the terrors of self-reference have been tamed, at least for the time being. All that remains to be done, it would seem, is for me to give way once more to this year's prize-winner. However, I'm reluctant to do that for *his* sake. For tonight is undoubtedly the pinnacle of his career. After this, he can only go downhill. Better to leave him for the moment in the golden world that most prize-winners occupy so briefly. On his behalf, therefore, I thank you sincerely for the award of the Bernal Prize and for your kind attention this evening.

Michael Mulkay

STOP PRESS: On the morning following the award of the prize, the following memorandum was delivered to Michael Mulkay by Harry Collins. No reply has yet been received from the prize-winner.

MEMORANDUM TO: Michael Mulkay
FROM: Prof. Black
TIME: 4.30 a.m.
DATE: Sat. 25th Oct. 1986
RE: J. D. Bernal Prize

I thought your acceptance speech was your best work to date, amply demonstrating to the whole audience your eminent suitability for the award. Therefore, since you can no longer represent a model for the less accomplished you should not have received the award.

Please return it!

W.V.O.B. (Prof)

Averting Catastrophe, by Joseph G. Morone and Edward J. Woodhouse, University of California Press, 1986.

Morone and Woodhouse bear good tidings, competent analysis, but little practical advice in their timely look at how U.S. society and government have dealt with one class of technological risk, what they call "catastrophic risk." Catastrophic risk — defined by Slovic as entailing potentially large-scale, dread, and often irreversible impacts — poses particular difficulties for decisionmakers. Morone and Woodhouse (M & W) look at five large-scale risks: toxic chemicals, nuclear power, recombinant DNA research, ozone depletion, and atmospheric greenhouse warming. The good tidings are that (so far), through trial and error and without even knowing that we were doing it, we have managed to avoid a serious catastrophe. Despite media hand-wringing, there have been no large nuclear or DNA accidents, no cancer epidemics from synthetic chemicals. Regulation has been surprisingly intelligent and sensible.

There are dark clouds on the horizon, however: risky technologies are proliferating and muddling through won't keep us safe in the future. The Morone and Woodhouse strategy for minimizing catastrophic risk has five commandments:

- Protect against the worst possible causes of error and the most severe risks, not just the most likely ones. Available tactics include prohibiting or limiting use of a technology, and preventing, containing, and mitigating potential impacts.
- Err on the side of caution.
- Set priorities among risks to focus on the gravest ones.
- Test risks to reduce uncertainty.
- Learn from experience and error. (M&W criticize "the contentious nature of the current U.S. regulatory environment, which can actually create disincentives to learning" [p. 169].)

These strategies are pulled together from the five case studies. The necessarily brief treatment of the case studies has little new to offer for those who have followed them, but the systematic comparison of regulatory histories is appealing. The discussion reviews how the above strategies did or did not come into play in each of the cases, reaching back into environmental ancient history (i.e., pre-Silent Spring). They laud rDNA research regulation as the closest thing yet to a

complete catastrophe avoidance system. The key elements of rDNA risk management that evolved from Asilomar into the NIH RAC Guidelines are protection through prohibiting the most risky experiments, caution through different containment levels, and reducing scientific uncertainty and learning from deliberate laboratory tests, including worst-case scenarios.

Society's favored approach to risky technologies has been trial and error — try out the technology, gather information, and adjust regulation according to the results and public judgment. This has more or less worked in the past for pesticides, toxic chemicals, nuclear power, and rDNA. However, M&W emphasize that trial and error is too slow to deal with the accelerating pace of technological application and inappropriate to deal with catastrophic technologies. Trial and error only works when the impacts of the trial are mild and feedback is prompt.

What do analytic approaches to risk management have to offer? Not much, according to the authors. They briefly review and dismiss analytic techniques such as cost-benefit and risk-benefit analysis, decision and default trees, and technology assessment. Technological risks are too uncertain, too varied, and too political to be pigeonholed into such methodologies. M&W nod instead to the classic literature on environmentalism, risk, history of technology, and decision theory — Commoner, Meadows, Mumford, Ellul, Steinbruner, Slovic and Fischhoff.

The last chapter outlines a strategy for managing risky technologies. Here the authors get muddled. They discuss the problems of ad hoc and analytic methods in dealing with risky technologies, then present strategies which depend in the end on judgment, analysis, and dealing with public perception of risk. Their strategy sounds suspiciously like organized trial and error.

Take, for instance, priority-setting. M & W decry the "inconsistent" (read irrational) regulatory attention to radioactive rather than chemical and hazardous wastes, to nuclear reactors rather than highway safety. They argue for more systematic management through priority-setting, but neglect to explain just how labeling priority-setting as part of a strategy will make regulation rational and effective. Societal treatment of risky technologies differs from individual or institutional practices;¹ how do we decide whether greenhouse warming or recombinant DNA is a graver risk?

What differences would the M & W strategy make in dealing with emerging risks such as environmental release of genetically engineered microorganisms? So far the U.S. has proceeded with great caution. But, as

1. Miller B. Spangler discusses areas where individual and societal attitudes toward risk clash, for example, the "put it in Texas" syndrome, in "Syndromes of Risk and Environmental Protection: The Conflict of Individual and Societal Values," *The Environmental Professional* 2 (3-4):274-291, September 1981.

the legal and media posturings on proposed field tests of engineered microorganisms show, it is still a societal judgment call whether caution means prohibiting a technology altogether or proceeding along an agreed-upon progression of field testing. A Rifikinesque wrench in the works can toss out any carefully crafted regulatory strategy. Caution as part of a deliberate strategy or as part of an ad hoc response is still value laden and political. On the battlefield of regulatory decisionmaking what we need is not strategy, but better tactics for communication, negotiation, and arbitration. Evolving institutional mechanisms such as issues management and technology assessment, although by no means well-defined panaceas, offer a means to air and consider different points of view.

The M & W analysis is after the fact — how to regulate and manage after a risk has emerged. An important and neglected part of risk management strategy is foresight. Changing values and technologies affect how public policy issues emerge and are settled.² Solid wastes were the “egregious risk” of the 1960s; toxic wastes did not enter the public consciousness until the 1970s.

2. For example, Joseph F. Coates, *Issues Management: How You Can Plan, Organize, & Manage for the Future*. Mt. Airy, MD: Lomond Publications, 1986.

Lessons from the issues management literature, which stresses the role of actors in public policy, would broaden the work of Morone and Woodhouse. Their treatment of the roles of different actors in risk management is weak, the cast of characters simplistic. They portray industry as beleaguered, defensive, ever wary of regulation; regulators as bureaucrats adrift in a sea of information without a set of priorities to steer by; scientists as quiet laborers who produce information only on demand. The media are noted as watchdogs alert to risk, but their potent role in policy is ignored.

Averting Catastrophe is well written and strikes a good balance between reputable and readable. It is short and simple, and manages to avoid sensationalism. The case study analysis is fresh and informative. In the end, Morone and Woodhouse offer well-packaged common sense: be more cautious in the face of larger, more uncertain risks; be more rational in assigning priority to risks; reduce uncertainty through research. They fall disappointingly short only in examining how these commandments might be put into practice.

Lisa C. Heinz
Office of Technology
Assessment

Minutes of 4S Council Meeting

Pittsburgh, October 23, 1986

Present: David Edge (President); Thomas F. Gieryn (Secretary/Treasurer)

Council Members: Marcel LaFollette, Marc DeMey, Bruno Latour, Ruth Cowan, Susan Cozzens

Past Presidents: Bernard Barber, Nicholas Mullins

Committee Representatives: Ned Woodhouse, John Wilkes, Loet Leydesdorff

President Edge called the meeting to order at 4:00 P.M.

Old Business

1. Minutes of the 4S Council meetings at Troy on October 24 and October 26, 1985, were approved with one dissenting vote.

2. Tom Gieryn circulated a revised 4S Charter showing amendments approved in a referendum of the general membership on February 28, 1986. At that time, members also approved increases in membership dues from \$15 to \$30 for professional members, from \$5 to \$10 for student members and from \$30 to \$40 for institutional members. [Note: The revised charter is available upon request from the Secretary.]

Secretary/Treasurer's Report

3. The Treasurer's Report was approved by the Council, showing a balance of \$12,659.85 September 30, 1986, an increase from \$10,162.01 on January 1, 1986. The increase is due to leftover funds from the 1985 meeting at Troy and from higher membership dues. It was noted that the Society was not likely to receive any additional funds from the registration fees of the 1986 Pittsburgh Four-Society meeting. [Note: The 1986 end-of-year Treasurer's Report will be published in Volume 5, Number 1 of *Science & Technology Studies*.]

4. As Secretary, Tom Gieryn described a decline in 4S membership from about 544 members in January 1985 to about 330 currently paid members in October 1986. The non-renewal rate in 1985 (with lower membership dues in effect) was about the same as in 1986 (higher dues), suggesting perhaps that the dues increase is not the primary reason why people are leaving 4S. [Note: Membership statistics are available upon request from the Secretary.]

5. The Council created an ad-hoc Committee on Membership, chaired by Susan Cozzens with Ned Woodhouse and Tom Gieryn as members. The Committee is charged with carrying out a membership drive. The Council authorized funds for production of a flyer, acquisition of mailing lists, and postage to be used in enlisting new members and re-enlisting old ones.

6. The Council approved a provisional budget for 1987, showing income and expenses of \$14,850 for the calendar year, leaving the Society with a balance of about \$10,500 on December 31, 1987. [Note: The provisional 1987 budget is available upon request from the Secretary.]

7. Tom Gieryn appealed to all past and present officials to send files of Society business to the Secretariat for possible inclusion in the 4S Archives. Tom Gieryn was asked to pursue (with the help of Arnold Thackray) the possibility of storing 4S archives at the American Philosophical Society.

8. The Council thanked Scott Long for a fine job during his tenure as 4S Secretary.

Publications Committee

9. Marcel LaFollette (Chairwoman) expressed the pleasure of the Committee with *Science & Technology Studies*, and the Council thanked Daryl Chubin and Susan Cozzens for their efforts as Co-editors.

10. Marcel LaFollette reported on efforts by the Committee and the Co-editors to find a commercial publisher to assume responsibility for production of *S&TS*. Four issues were discussed: ownership of the journal, photoreproduction of the contents of *S&TS* for educational purposes (a matter of copyright), advertising in the journal, and sale of our subscription list. The Council affirmed the desire to retain ownership of the title, to control selection of editors and other procedures, and to make all material in the journal available free for educational purposes. The issue of copyright was left for negotiations with the publisher. The distinction between "advertising" (including that from corporate sources) and "news items" (about books or activities of other scholarly societies) was discussed at length.

11. The provisional 1987 budget allowed \$12,000 for publication of four issues of *S&TS*.

12. The price for back issues of Society publications was reduced to \$4 per volume plus postage and handling; an advertisement will soon be placed in *S&TS*.

13. The Council discussed the possibility of reducing the "run" for each issue of *S&TS* from 750 to 600 copies.

14. The Council thanked Marcel LaFollette and the Publication Committee for their efforts.

1986 Program Committee

15. The Council noted the high quality and interesting diversity of this year's program, and thanked Ruth Cowan, Stephen Cole, Elizabeth Garber and their Committee for a job well done.

16. Ruth Cowan noted that her Committee's task was complicated by the lack of information of what was to be done when. This raised a general question of how better "organizational memory" could facilitate 4S Committee work. It was suggested that the Secretary maintain copies of all Committee files (with reports from previous years, and calendars of deadlines), and distribute these files to the Chairs of Committees at the start of their tenure. It was suggested that Program Committees be appointed two years in advance of a meeting.

17. In particular, the process of sending off letters of invitation/acceptance must be centralized so that each participant is informed as early as possible of his or her spot of the program. Also, it was advised that the Program Committee in future years work more closely with the Local Arrangements Committee, so that the relationship between number of sessions and number of available rooms can be considered. The problem of achieving a balance between a high quality program and a catholicity tolerant of a wide variety of papers was also discussed.

18. The Council expressed its wish that curriculum vitae not be used in the evaluation of papers submitted for the 4S program. The Council reminded itself of its oft-forgotten and hard-to-enforce policy: one must be a 4S member in order to be on the program of its annual meeting (except for some categories of invited guests).

19. The Council affirmed its desire to continue Four-Society meetings on a regular basis.

Local Arrangements Committee Report

20. The Council thanked Peter Machamer and his Pittsburgh crew for their successful efforts.

21. The Council thanked Bernard Barber for his shepherding of the Chinese contingent.

Nominations Committee Report

22. Ron Westrum reported the slate of candidates to be placed on the ballot for a November 1986 election. For

Future Meetings Committee Report

President: Jerry Gaston and Arie Rip. For Council (three to be elected): Mary Frank Fox, Rob Hagendyk, John Irvine, Ron Johnston, Helga Nowotny, Larry Rhoades and Ryan Tweeney. The Council thanked Ron Westrum and his Committee for their work.

23. President Edge suggested that the President-elect, rather than the President, be placed in charge of the Nominations Committee.

24. John Ziman's report was read in his absence. The possibility of a meeting in Israel fell through. Two sites were proposed for the 1988 meetings: Amsterdam and Bielefeld. It was decided that President Edge would approach the Amsterdam group (and Arie Rip specifically) with a formal request to host the 1988 meetings in conjunction with EASST. In response to Loet Leydesdorff's concerns, the Council expressed its enthusiasm for a meeting in Amsterdam, and hopes the Science Dynamics people will take on the task. Bielefeld remains a back-up location.

1987 Meeting at Worcester

25. John Wilkes reported on plans for next year's meeting, including ideas for a satellite meeting of representatives of STS teaching programs. It was decided that the Local Arrangements Committee should not plan to publish the proceedings of the 1987 meeting, and that registration fees be modestly increased.

26. At the Society's business meeting, a vote was taken to hold the meeting on November 20-22, 1987.

Bernal Award Committee

27. Michael Mulkay was announced as the 1986 recipient of the John Desmond Bernal Award. The Council thanked Robert Merton and his Committee for their fine choice. The Council also thanked Eugene Garfield and the Institute for Science Information for their continuing support of this important Society activity.

New Charter Amendments

28. Council voted to poll the full membership on a set of Charter amendments. Some of these changes were proposed during the Council meeting at Blacksburg in 1983, although they were never put to the full mem-

bership in a referendum. Other changes were proposed in Pittsburgh in order to bring the Charter into greater consistency with Society practice. [Note: The wording of these amendments was approved during the Society Business meeting at Pittsburgh on October 25, 1986, and the referendum went out to members in November 1986.]

- . Add to Section II.A. "Every effort will be made to have as broad a disciplinary representation as possible in the membership."

Add following Section II.B.4 "There shall be a standing committee on future annual meetings."

"There shall be a standing committee responsible for the program of annual meetings."

"There shall be a standing committee responsible for local arrangements of annual meetings."

"There shall be a standing committee on liaisons with other scholarly organizations."

Change in Section III.A from ". . .designated by him. . ." to ". . .designated by the President. . ."

Change in Section III.C from "The accounts shall be subject each year to an audit by two members chosen by the President of the Society or by a certified public accountant. At the end of each three-year term of the Treasurer the accounts must be audited by a certified public accountant." to "The accounts shall be subject annually to an audit by two members of the Society or by a certified public accountant or by its equivalent, and at other times if requested by the President or by a majority of the Council."

Other New Business

29. The Council accepted in principle Ron Giere's proposal for a Committee of representatives from the Four Societies (4S, HSS, PSA and SHOT). It was suggested that implementation be carried forth by the President working with the Liaison Committee (chaired by Ned Woodhouse).

30. Ned Woodhouse reported progress in preparation of a 4S Membership Directory, and the Council thanked him for his work.

The Council adjourned at 7:00 P.M

Science Policy Support Group

The following description is excerpted from a paper circulated by the staff of the United Kingdom's Science Policy Support Group (SPSG).

SPSG has been set up by the Economic and Social Research Council (ESRC), with core support from other United Kingdom Research Councils. SPSG's Chairman is Professor John Ziman, FRS, who has moved academically from theoretical physics into science studies and who works for the group on a part-time basis. The full-time program manager is Peter Healey, a sociologist who until recently worked on the administration of science policy research as a staff member of ESRC.

The Program Committee which advises SPSG on its work also advises the Council of the ESRC on the funding of research in the related fields of science studies and science policy. The membership of this Committee includes SPSG's core funders, and is also broadly representative of academic and policy interests in the field. To insure that work on science policy is undertaken within a comparative framework, some of the members of the Program Committee are drawn from outside the United Kingdom.

Plans and Methods

SPSG's purpose is to promote studies which will help us understand the workings of the R&D system, and increase our ability to manage it effectively to meet policy requirements. We do not see science and technology policies as being made solely in a top-down manner by such bodies as ABRC [the Advisory Board to the Research Councils, a board which makes recommendations to the government on the science budget]. Science policy issues also arise when a laboratory director assembles resources for research, or when an industrial R&D manager decides how to deal with shortening product cycles, or when MoD [the Ministry of Defense] has to try to respond to demands that specifications for military systems should lead to generic technology applicable in the civil sector.

SPSG will not itself undertake research, but will facilitate, coordinate, and organize research work, primarily through seminars and critical review, to

- improve UK research capacity in science studies and science policy, both by strengthening the inter-institutional network between scholars, and by providing more opportunities for exchanges between researchers and policymakers
- develop an agenda of policy issues thought likely to be salient within UK and European

science over the next 10-15 years and foster discussion of these issues

- evaluate research (and commission new research) and insure that relevant policymakers both know about it and appreciate what it can or cannot do
- encourage better training for science policy analysts.

SPSG's methods of working will include:

- advising on existing work and/or new specific studies and research projects to fulfill this agenda. [Although concerned primarily with the capacity of UK research to address UK problems, SPSG will draw as necessary on research and consultancy from anywhere in the world.]
- organizing workshops and seminars, bringing researchers and policymakers together to discuss potential, current, and completed research
- fostering new research to fill gaps in our knowledge
- helping to manage research for customers or ad hoc consortia
- organizing the critical evaluation of research and the dissemination of its results to potential users
- contributing to training through
 - monitoring and advising on the doctoral and post-doctoral support available within science studies/science policy
 - organizing training for research administrators on such issues as methods of research evaluation
 - advising on the longer-term training needs of future science policy analysts

We expect the seminar/workshop to be the most important of our working methods. Most seminars will be organized thematically within the agreed program, but we intend also to seize the opportunity of overseas visitors passing through London who may have something to say to a wider audience on their research or on current policy issues.

SPSG is aware of much useful policy work in science studies, and established seminar series. It does not intend in any way to duplicate these or to interfere with fruitful existing links between customers and contractors. It sees itself with a contributory role and a limited life—no more than six years—to help develop the network of researchers and improve links with policymaking.

The SPSPG Agenda

The whole of SPSPG's activity is concerned with increasing our understanding of the functioning of the R&D system, and the potential reach and effectiveness of policies for science and technology. That general aim can only be achieved in relation to specific current policy issues; and we believe that generating a priority list of such issues in itself can be a useful exercise.

SPSPG therefore intends to develop a coordinated national agenda of issues of science and technology policy thought likely to be prominent over the next 10-15 years and to work systematically within this agenda. This agenda is necessarily tentative and is open for revision. The program put forward here will be revised in light of responses to it.

This program cannot be tackled all at once, since it will depend on the available funds and research expertise. Research is already underway on a number of these topics, and research projects are being defined on a number of others.

Our activity will therefore concentrate on the following issues:

I. The Economic and Industrial Aspects of Science Policy

A. *The overall scale of national expenditure on science and its relationship with expenditure in development, marketing, and production.* What are the connections with economic prosperity and what are the general implications of "steady state" funding for science?

B. *The relationship between national, European, and world science.* What criteria for selectivity should be used at different levels, and across different areas, of the resource allocation system? Can there be a general framework for European selectivity and collaboration? What is the minimum scale of scientific activity that a nation has to maintain to keep a watch on technological opportunities in a particular field?

C. *The specific contributions of research to strategic science and generic technology.* What do we most need to know about the way in which science relates to innovation to aid policy initiatives designed to improve the stock of exploitable research? Can defense R&D make a bigger contribution to other aspects of national welfare and security?

II. Models of the Research Process and Means of Monitoring its Development and Change

A. *Internationally comparative work on the understanding of science.* This includes a number of topics on which research is being funded by the ESRC under

SPSPG management. On what models of science do people base their attitudes and action on technological and scientific issues, and how do they relate to their education and experience? Are there particular things that we can learn about the development of such models of science in the young, and what are the implications for science education? What is the role of the media as institutions reflecting attitudes toward science, as molders of opinion, and as a means of communication between scientists outside their specialist areas?

B. *Indicators of science and technology.* Can we start to develop process or management indicators which link our understanding of indicators of research inputs and outputs? Can those developing indicators learn anything from conceptual models of science being developed by sociologists, economists, and historians; can they also learn more from the needs of decisionmakers? How can and should such indicators be used in the decisionmaking process and how do they relate to peer review? What is the UK role in helping build more coherent and consistent international databases for R&D indicators?

C. *The organizational structure of the R&D system.* What can we learn from R&D structures in the other EEC countries, the USA, the USSR, Japan, and the Commonwealth, in order to improve our ability to cover multidisciplinary problems, avoid duplication of research, and improve our management of science and technology transfer? Together with the work under I.B., do such comparisons suggest organizational changes in the UK, or new policies for the coordination of R&D in a European framework?

III. Management of the R&D Process in the Various Sectors of the System.

A. *Manpower and career problems.* How can we foster personal mobility and adaptability to change and provide against a net loss of scientists and engineers abroad within a constrained system?

B. *Selectivity and quality control in scientific institutions.* What are the managerial problems of applying performance criteria, and of establishing or reorganizing centers of excellence? Given the increasing cost and complexity of scientific equipment and research facilities, what are the trade-offs between equipment and manpower, and between scientific programs?

C. *Information transfer.* What are the implications of the changing scientific information system for scientific practice, and for publishing and storing scientific information? What are the policy implications of the contrast between the apparent openness offered by new technical developments, and increased sensitivity to national and commercial confidentiality?

4S News

Call for Papers, 1987 4S Annual Meeting. The Twelfth Annual Meeting of the Society for Social Studies of Science will be held in Worcester, Massachusetts, November 19-22, 1987. The host institutions for the meeting will be Worcester Polytechnic Institute and the Environment, Technology, and Society Program at Clark University. John Wilkes (Department of Social Science and Policy Studies, Worcester Polytechnic Institute) will chair the local arrangements committee. The Program Committee will be co-chaired by Sal Restivo (coordinator), Leigh Star, John Wilkes, and Susan Cozzens.

The Program Committee welcomes proposals for papers from scholars interested in the social, human, and policy dimensions of science and technology. The proposal should be in the form of an extended abstract (approximately 900 words) or a paper. Please send three copies of your proposal or paper to Sal Restivo, Program Committee Coordinator, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12181, USA.

If you would like to organize a session, send us a letter (three copies, please) stating the proposed topic and participants; and ask the suggested participants to send us extended abstracts. In addition to the regular sessions, we are planning to organize several one hour roundtable discussions. If you are interested in leading a roundtable, send a one page abstract (three copies, please) of your proposed discussion topic. The deadline for receipt of paper, session, and roundtable proposals is 1 March 1987. Abstracts (250-300 words) of accepted papers will be due 1 June 1987, for publication in the annual meeting issue of *Science & Technology Studies*. If you have any questions about the 1987 program, please write to Sal Restivo at the above address, or call him at (518) 266-8504.

Call for Papers, 1988 AAAS Meeting, 11-16 February 1988. 4S efforts to arrange panels at AAAS meetings have been too little and too late in recent years. Henceforth, we want to do better, so we're starting early for 1988.

Four AAAS sections are potentially open to us: H (Anthropology), K (Social, Economic, and Political Sciences), L (History and Philosophy of Science), and X (Societal Impacts of Science and Engineering). Among them, we ought to be able to formulate panels that represent a good cross-section of the STS community.

In the past, 4S typically has sought only proposals for complete panels. We now also welcome proposals for individual papers, which we'll match up wherever possible to form complete panels. Discussants, panel chairs, and participants for the 4S-AAAS program committee also are needed.

To discuss potential proposals, phone or write: Ned Woodhouse, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12180-3590, USA. Telephone: (518) 266-8506.

Sale of 4S Publications. The Secretariat has accumulated complete sets of back issues of all Society publications. We have *The Sociologist of Science*, *4S Newsletter*, and *4S Review* from 1975 through 1986. The prices are \$5.00 per annual volume (usually four issues) or \$1.50 per issue (mailing costs are included). These prices are good for members of 4S *only*; other individuals or libraries (or those interested in bulk sales) should contact the Secretariat. Please include payment with your order, and indicate your needs by date (e.g., Winter 1980) and/or by volume and issue number. Send orders to: Thomas F. Gieryn, 4S Secretariat, Department of Sociology, Ballantine Hall 744, Indiana University, Bloomington, IN 47405, USA.

4S Archives. The Secretary/Treasurer requests that all present and former Society officials send files containing Society business to him, for archiving and institutional memory. The address is in the preceding announcement.

Positions

The Interfaculty Group Wetenschapsdynamica invites applications for a two year post-doctoral fellowship which will become available on 1 March 1987. The fellowship has been created to give young researchers the chance to continue their intellectual work and at the same time to introduce new perspectives into the work of the science dynamics group. Applicants should have completed a PhD within the field of Social Studies of Science and Technology. The successful candidate will be expected to formulate and carry out a research project within the area of Science Dynamics (defined as "the study of the factors governing the development and steerability of the sciences"). He or she will also be expected to contribute to the general development of the group and to undertake a limited amount of educational and administrative work. Further information on the current work of the group may be obtained from Mrs. Tini Bakker, Wetenschapsdynamica, Universiteit van Amsterdam, Nieuwe Achtergracht 166, 1018 WV Amsterdam, The Netherlands. Telephone: 522-3595. Applicants are invited to

submit a 3-5 page outline of the research they would wish to do (a fully developed proposal is not required), together with a *curriculum vitae* and the names of two references, to Professor Stuart Blume at the above address. Applications should be received within three weeks after the publication of this advertisement.

Sloan Videohistory Project Assistant. Immediate opening available at the Smithsonian Institution, Washington, DC, for an historian to act as general assistant to the Program Manager for a four-year exploratory program to produce a set of videohistory studies now under development by members of the Smithsonian staff. The incumbent is expected to have knowledge of 20th century history of science and technology, scientific institutions, intellectual history or anthropology, as well as knowledge of videohistory and oral history techniques and general archival practices. This is a research assistant position in what is primarily an archival activity, in support of ongoing Smithsonian studies in the history of recent science and technology. Salary range: \$14,390-\$23,170 per year. In addition to a Personnel Qualifications Statement SF-171, submit a supplemental sheet giving explicit examples of research experience and training to: Smithsonian Institution, Office of Personnel Administration, Arts and Industries Building, Room 1410, 900 Jefferson Drive SW, Washington, DC 20560, USA. To request a copy of the vacancy announcement, call (202) 357-2465, and ask for announcement 86-406-T. For further information, call or write David H. DeVorkin, Room 3557, National Air and Space Museum, Smithsonian Institution, Washington, DC 20560 USA. Telephone (202) 357-2828. The Smithsonian is an equal opportunity employer.

Beginning with Volume 5, Number 1, *Science & Technology Studies* will begin to charge for publishing announcements of job openings. The charge is \$25 per listing for up to 1000 characters, and \$.50 for each additional 50 characters or portion thereof. *S&TS* expects institutions in the U.S. to adhere to Affirmative Action/Equal Opportunity Employment practices, and institutions everywhere to consider applicants for positions without regard to extra-scholarly characteristics. *S&TS* reserves the right to edit all copy and refuse advertisements. Although every effort will be made to ensure error-free publication, *S&TS* liability for listing errors is limited to refund of the listing fee paid and/or placement of a corrected advertisement in the next issue at no charge, with decisions about appropriate remedy for errors to be made at the discretion of the editors.

Publications

The Indiana University Press is pleased to announce a new publication series entitled *Science, Technology, and Society*. This series will be devoted to works exploring the nature of science, technology, or their social relations. Efforts will be made to include a variety of perspectives within history, philosophy, sociology, and other disciplines contributing to Science and Technology Studies. The series will seek a diverse audience including specialists in one or another discipline, those working in various interdisciplinary frameworks, as well as interested scientists and the general reader. The Press and General Editors welcome book-length manuscripts and detailed book proposals for contract consideration. Inquiries should be addressed to either of the Editors or the Press, at the following addresses: Ronald N. Giere, History & Philosophy of Science, 130 Goodbody Hall; Thomas F. Gieryn, Sociology, 754 Ballantine Hall; or Robert Sloan, Indiana University Press, Tenth and Morton Streets; all at Indiana University, Bloomington, IN 47405 USA.

The Center for the Study of Ethics in the Professions at the Illinois Institute of Technology has published six modules for teaching applied and professional ethics courses, especially those in engineering ethics and technology in society. The modules were produced under a grant from the Exxon Education Foundation. The titles are: Professional Responsibility for Harmful Actions, The Moral Status of Loyalty, Technology Assessment: a Historical Approach, A Critical Examination of Risk/Benefit Analysis, Conflicts of Interest in Engineering, and Whistleblowing. They cost \$4.95 each, except the conflicts of interest module which is \$5.95. They are available from the Center. Write Modules, CSEP, IIT Center, Chicago, IL 60616, USA. Telephone: (312) 567-3017.

Dealing with the problem of unemployment in the 1980s, *Future Employment & Technological Change* rejects the commonplace assumption that renewed economic activity will in itself bring lower levels of unemployment. This new book shows that job loss from industry is the common experience of western industrialized nations and that there is no automatic employment growth likely from the service sector. The role of technology in generating economic growth is analyzed and it is concluded that growth led by information technology would not produce the necessary number and diversity of jobs. By Donald Leach (Queen Margaret College, Edinburgh) and Howard Wagstaff (Edinburgh University), the book is available from Kogan Page Ltd, 120 Pentonville Road, London N1 9JN, England, UK, for £9.95 paperback.

Meetings/Calls for Papers

"The Value of Many Voices", 11-13 February 1987, sponsored by the Center for Applied Biomedical Ethics and AMI/Presbyterian/Saint Luke's in Denver, Colorado, explores how cultural, religious, and social value differences complicate ethical decisionmaking. Faculty includes: physicians, nurses, mental health specialists, religious, and cultural leaders. This conference also presents a practical process for developing ethical solutions to these dilemmas. For information, please contact the Center for Applied Biomedical Ethics, 4567 East Ninth Avenue, Denver, CA 80220, USA. Telephone: (303) 320-2895.

North Central Sociological Association, 1987 Annual Meeting, 2-4 April 1987, Westin Hotel, Cincinnati, Ohio. The meeting will include a session on the sociology of science and technology. Persons interested in presenting a paper at this session should communicate with Richard Senter, Department of Sociology, Central Michigan University, Mt. Pleasant, MI 48859 USA.

Cheiron: The International Society for the History of the Behavioral and Social Sciences invites papers, symposia, and workshops for its nineteenth annual meeting to be held at Bowdoin College, Brunswick, Maine, 10-13 June 1987. Inquiries should be directed to the Program Chair, Professor Laurel Furumoto, Department of Psychology, Wellesley College, Wellesley, MA 02181. Deadline for submissions is 15 January 1987.

Fifth Biennial Student Pugwash USA International Conference, "Choices for Our Generation: Ethics and Values at the Cutting Edge of Technology," 28 June - 4 July 1987 at Stanford University. The Student Pugwash International Conference assembles 100 students from around the world, each of them selected for their creative thinking on science and technology issues. Undergraduate, graduate, and professional students from all disciplines will gather for an intensive week of discussion in a working group focusing on one of the following issues: reproductive technologies; dilemmas for the future of computing; roles for the biotechnologies in international development; water: politics, pollution, and supply; science and technology in the media; nuclear proliferation and the international control of atomic energy. For each topic, small groups of student delegates and accomplished senior participants from government, industry, and academia will meet throughout the week. Application to the conference consists of an abstract and outline of the student's working group paper, a transcript, recommendation, and a brief essay. Student Pugwash will cover participant's food and lodging at the conference. Domestic travel costs are the responsibility of the student, though Student Pugwash will make every effort to subsidize international travel costs. Application due date: 20 March 1987. For information,

contact Benjamin Austin, Conference Director, Student Pugwash USA, 505-B 2nd St., NE, Washington, DC 20002 (phone: 202/544-1784).

Relations between Qualitative Theory and Scientometric Methods in Science & Technology Studies workshop, Science Dynamics Department, University of Amsterdam, 12-13 December 1987. The political demand for science and technology indicators, together with theoretical developments in social studies of science, have challenged researchers in the field of S&T studies to integrate the qualitative and the quantitative. Given the recent emergence of some new centers for scientometric studies in Europe, the organizers hope to be able to hold an interesting workshop focusing on the potentials for combining qualitative theorizing with scientometric methods. The workshop is planned as the first in a series of two, of which the latter, on implementation of problems of S&T indicators, is to be held in Paris in 1988. Both workshops are organized under the aegis of EASST, the European Association for the Study of Science and Technology. The deadline for proposals is 1 August 1987. Proposals must include a one page abstract of an envisaged contribution. For further information contact Loet Leydesdorff, Wetenschapsdynamica, Universiteit van Amsterdam, Nieuwe Achtergracht 166, 1018 WV Amsterdam, The Netherlands. Telephone (020) 522-3698 or 3595.

The sixth *Annual Luncheon of the Planning History Group* will be held Saturday, 4 April 1987, at noon in the Wyndham Franklin Plaza Hotel in Philadelphia. The luncheon is being held in conjunction with the meeting of the Organization of American Historians. Theodore Hershberg, University of Pennsylvania, will present a paper entitled, "Planning for a Region: The Political Problems." Sam Bass Warner, Jr., Boston University, will chair the session. Tickets will be available as part of the pre-registration package for the OAH meeting or at OAH Registration. As the number of tickets is limited, purchase through pre-registration is encouraged. For additional information, contact: Blaine A. Brownell, College of Social and Behavioral Sciences, University of Alabama at Birmingham, Birmingham, AL 35294, USA; or Mark H. Rose, The Program in Science, Technology, and Society, Michigan Technological University, Houghton, MI 49931, USA. Telephone: (906) 487-2115.

Other News

Northern Illinois University announces a new program of graduate study in Politics and the Life Sciences. This area of study, also referred to as "biopolitics," will equip students for society's growing need for specialists trained to work professionally at the interdisciplinary intersection of the life sciences and political science. It is an integral component of the M.A. and Ph.D. programs in political science. Because

of its interdisciplinary character, students can construct individualized programs which can include bio-social course work in other departments such as anthropology, sociology, philosophy, and biological sciences. For further information contact Thomas C. Wiegele, Program for Biosocial Research, Northern Illinois University, DeKalb, Illinois 60115-2854, USA. Telephone: (815) 753-9674.

Science, Technology, & Human Values, a journal cosponsored by the Writing Program and STS Program at MIT and the Kennedy School at Harvard, offers a reduced rate exclusively to members of 4S. Members may subscribe for \$26.00. (The regular rate is \$32.00 for individual subscriptions.) Members outside the U.S. may subscribe for \$26.00 (surface rate) or \$48.00 (air mail). Write: Periodicals Division, 4S87, John Wiley & Sons, P.O. Box 836, Boundbrook, NJ 08805, USA.

The Alfred P. Sloan Foundation of New York City has awarded a grant to the Smithsonian Institution for a videohistory project which will explore the potentials inherent in adding a visual component to research projects already underway in the history of recent science. Scholars from a number of SI bureaus will conduct interviews documenting the history of astrophysics, biology, mathematics, and physics. David DeVorkin of the National Air and Space Museum is chair of the advisory committee, and the project staff will be located in Smithsonian Archives, A&I 2135, Washington, DC 20560, USA.

ABOUT THE AUTHORS

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J. D. EVELAND is Director of Technology Applications Research for Cognos Associates, a research and consulting firm in Los Altos, California. He has a B.A. in history from Reed College, an M.P.I.A. from the University of Pittsburgh, and a Ph.D. in administration and organization behavior from the University of Michigan.

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A. L. WALTON manages the Environmental Technology Program for the Jet Propulsion Laboratory, California Institute of Technology, Mail Stop 79-6, Oak Grove Drive, Pasadena, CA 91109. Dr. Walton received a B.A. from California State University, Northridge, and an M.A. and Ph.D. from Princeton University.

MEETINGS CALENDAR

2-4 April 1987. Cincinnati, Ohio. North Central Sociological Association 1987 Annual Meeting, session on Sociology of Science and Technology. Contact Richard Senter, Department of Sociology, Central Michigan University, Mt. Pleasant, MI 48849, USA. (See announcement this issue.)

4 April 1987. Philadelphia, Pennsylvania. Sixth annual luncheon of the Planning History Group of the Organization of American Historians. Contact Mark H. Rose, The Program in Science, Technology, and Society, Michigan Technological University, Houghton, MI 49931, USA. (See announcement this issue.)

13-24 April 1987. Dubrovnik. Course/workshop on "Ethics and Sociology of Science." Contact Henk Verhoog, Institute of Theoretical Biology, University of Leiden, Groenhovenstraat 5, 2311 BT Leiden, The Netherlands.

10-13 June 1987. Brunswick, Maine. Nineteenth annual meeting of Cheiron: the International Society for the History of the Behavioral and Social Sciences. Contact Laurel Furumoto, Department of Psychology, Wellesley College, Wellesley, MA 02181. (See announcement this issue.)

16-20 June 1987. Blacksburg, Virginia. Summer Conference on History, Philosophy, and Social Studies of Biology. Contact Richard M. Burian, 1987 Summer Conference, Department of Philosophy, VPISU, Blacksburg, VA 24061, USA.

28 June - 4 July 1987. Stanford, California. Fifth Biennial Student Pugwash USA International Conference. For applications or additional information, contact Benjamin Austin, Conference Director, Student Pugwash USA, 505-B 2nd St., NE, Washington, DC 20002. (See announcement this issue.)

17-21 August 1987. Chicago, Illinois. Annual Meeting of the American Sociological Association. For information on the session on Sociology of Science, contact Susan E. Cozzens, Department of Social Sciences, Illinois Institute of Technology, Chicago, Illinois 60616.

24-28 August 1987. Diepenbeek, Belgium. Conference on bibliometrics and theoretical aspects of information retrieval. Contact Limburgs Universitair Centrum, Algemeen Secretariat, International Conference, Universitaire Campus, B-3610 Diepenbeek, Belgium.

19-22 November 1987. Worcester, Massachusetts. Twelfth Annual Meeting of the Society for Social Studies of Science. Contact Sal Restivo, Program Coordinator, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12181, USA. (See call for papers this issue.)

12-13 December 1987. Amsterdam, The Netherlands. Workshop on the Relations between Qualitative Theory and Scientometric Methods in S&T Studies. Contact Loet Leydesdorff, Wetenschapsdynamica, Universiteit van Amsterdam, Nieuwe Achtergracht 166, 1018 WV Amsterdam, The Netherlands. (See announcement this issue.)

11-16 February 1988. AAAS Annual Meeting. For information on 4S sessions contact Ned Woodhouse, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12180-3590.

Science & Technology Studies welcomes information about forthcoming events of interest to its readership. Deadline for submission of news for the Spring 1987 issue is 27 February 1987.

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The editors urge contributors not to be constrained by the format of traditional scholarly research articles in their submissions. Contributions may also take one of the following forms: letter to the editors, commentary on a published article, opinion column, or synthetic review essay.

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Paul Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927," in *Historical Studies in the Physical Sciences* ed. R. McCormach, Vol. 3 (Philadelphia: University of Pennsylvania Press, 1971), 1-115.

In reference style, examples are:

Latour, Bruno and Steve Woolgar. *Laboratory Life: The Social Construction of Scientific Facts*. Beverly Hills: Sage, 1979.

Roll-Hansen, Nils. "The Death of Spontaneous Generation and the Birth of the Gene: Two Case Studies of Relativism." *Social Studies of Science* 13 (1983): 481-519.

MacKenzie, D. and B. Barnes. "Scientific Judgement: The Biometry-Mendelism Controversy." In B. Barnes and S. Shapin, eds., *Natural Order*. Beverly Hills: Sage, 1979, 191-210.

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