8. THE DETERMINANTS OF A SCIENTIST'S CHOICE OF RESEARCH PROJECTS*

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Scientists may differ in their choice of research projects due to differences in tastes (e.g., degree of risk aversion) and due to differences in constraints (e.g., ability, time, laboratory resources, information on likelihood of success, etc.). Although Stigler in his Nobel Lecture (1983) has suggested that conservative scientific behavior is due to risk aversion and to a desire to protect human capital already invested in current theory, economic explanations of the behavior of scientists are at a fairly early stage of development. The aim of the current research is partly to develop the economic theory of scientific behavior, but mainly to learn what the stylized facts are upon which future theory should focus.

For the present study, limited data have been collected on approximately 200 scientists in the West who either wrote on polywater or else were members of the population of scientists who could have written on polywater. "Polywater," condensed in very small capillaries, was thought to be a new form of water that had a lower freezing temperature and a higher boiling temperature than ordinary water. In the West the surge of articles began in 1969. By 1973 when everyone admitted that the polywater phenomena were due to impurities in normal water, over 100 academic papers had been written on the subject.

More detailed biographical and professional data have been obtained on a subsample of about 100 scientists from the larger sample. In addition, a survey was sent to all of those in the larger sample for whom any potential mailing address was available. Using the evidence from the polywater episode, I examine several hypotheses concerning alleged determinants of a scientist's choice of research projects. In particular, I briefly examine risk aversion, age, number of children, income and academic tenure, funding and journal demand, religion, geographical location, characteristics of family of origin, and number of citations.

1. THE PROBLEM

Since Kuhn's *The Structure of Scientific Revolutions* (1970), considerable attention in philosophy and sociology of science has focused on how much of scientists' decisions on theories can be explained in terms of the objective criteria emphasized in textbooks ("internal" factors) and how much needs

to be attributed to prejudice, ideology, age and economic interest ("external" factors).[1] Accepting the common, but disputed interpretation of Kuhn's book that external explanations of decision making undermine science's claim to objectivity and progress,[2] many philosophers (e.g., Toulmin 1972; Laudan 1977; Hull 1978; Diamond 1978, 1988b), have attempted to reconcile the internal and external accounts in a way that does justice to the history of science while still defending something of science's claim to special epistemic status.

While economists have attempted to model the labor market for scientists, historians of science and scientists themselves have continued to author accounts of scientific decision making that place varying emphasis on internal *versus* external factors.[3] For example, since Watson's (1968) now classic account, some scholars have discussed the DNA episode with an emphasis on external explanations (see, e.g., Toulmin 1972, Fleming 1968), while others have emphasized internal explanations (see, e.g., McCarty 1980, Diamond 1982). Case studies of this sort are often useful and sometimes provide the best evidence that one can reasonably hope to obtain about the reasons for the acceptance or rejection of historically important scientific theories. But sometimes it is possible to do better. In particular, certain sorts of external factors allegedly important for theory acceptance are quantifiable. In such cases rigorous social science statistical techniques allow us to determine how much of the differences in acceptance of a theory are explainable in terms of differences in the quantifiable factors. The larger the residual variance in acceptance after the quantifiable external factors have been taken into account, the larger the potential role for purely internal factors to play. More sophisticated inferences are also possible. Failure of the quantifiable external factors to be important might lead one to look with renewed interest at the nonquantifiable external factors. Alternatively, even if quantifiable external factors turn out to be statistically important, it can be argued that through institutional natural selection or some other process, at least some "external" quantifiable factors are correlated with objective "internal" theory selection procedures (see, e.g., Hull 1978, Diamond 1978, 1988b). An example might be high salaries (an external factor) being correlated with skill at judging a new theory's explanatory power (an internal factor). Although not eliminating all disputes in metascience, rigorous testing of the effects of quantifiable external factors thus has the positive effect of improving our knowledge of the phenomena that any plausible metascientific position must explain.

Work in the Merton tradition of sociology of science has in the last twenty years vastly increased our systematic knowledge of the workings of scientific institutions (Cole and Cole 1973, Zuckerman 1977, Crane 1972, Goffman and Warren 1980, Hargens 1988; Mullins *et al.* 1977, Studer 1980). But although much has been learned about the determinants and corre-

lates of success in paper-writing, citations, and professional advancement, little has yet been done to systematically increase our knowledge of the determinants and correlates of theory acceptance. Instead, discussions of theory acceptance usually have taken the form either of abstract generalization or else of detailed narrative case studies. Zuckerman's article (1979) provides a good survey of much of this work.

2. AN ECONOMIC APPROACH TO THE PROBLEM

Assume that the objective of a scientist is to advance the frontier of knowledge either because this objective is a direct source of utility for the scientist or because it is a productive activity for which the scientist is compensated. Assume further that the choice variable for a scientist is the research project. Research projects may differ along several dimensions, most notably the probability of success, the time to completion and the magnitude of the contribution if successful. Broadly speaking, differences in choice of research projects will depend either on differences in values or on differences in constraints (or on some combination of the two). Under the heading of differences in values, one frequently hears that scientists differ in the extent to which they are risk averse.

Under the heading of constraints, the following would presumably be important:

1. The scientist's belief about whether a project can be successfully completed.
2. The scientist's belief about whether *she* can successfully complete the project.
3. The scientist's belief about how important the contribution to science will be if the project is successfully completed.
4. The scientist's beliefs about all of the above for other research projects.

The above beliefs would themselves depend on the objective state of scientific knowledge, the information that the scientist has on the state of scientific knowledge, the scientist's ability, the scientist's resources and the scientist's beliefs about her ability and resources. Although economists are apt to deny it, other social scientists frequently have concluded that persons with the same ability and the same resources may nonetheless differ in their estimate of the probability of their own success. Such differences would be attributed to differences in "self-confidence." My discussion assumes the subjectivist interpretation of probability. The assumption is increasingly supported by current work on the frontiers of statistics indicating that "randomness" is not a feature of the objective world (Kolata 1986).

Although considerable exploratory research has been done on the behavior of scientists, the subject is still in the early stages of development. Much additional research will be required before we can say with confidence which constraints matter most in determining the choice of research

projects and whether there is a residual of choice behavior that is due to differences in risk aversion.

The objective of the current research is mainly to provide additional information on the characteristics of those who chose and those who did not choose a particular well-defined research project. The hypotheses to be examined are suggested by many sources both within economics and outside of economics. But all the hypotheses can be viewed as fitting into the framework just sketched, that is, they are related to the scientist's ability, to the scientist's other resources, to the scientist's knowledge of herself, to the scientist's knowledge of science and to the scientist's taste for risk.

The well-defined research project that I will examine is the polywater episode. I first briefly summarize the episode and describe the data set. Then I examine some hypotheses about what characteristics we would expect to be correlated with the choice of an ex ante promising, but ex post mistaken, research project. I will locate the hypotheses within the choice model sketched above. Finally, I will examine how the evidence from the polywater episode bears on the truth of the hypotheses.

3. THE POLYWATER EPISODE

Polywater, also known as 'anomalous water,' was first discovered in 1961 by an obscure Russian scientist named Fedyakin who found that in very small capillaries a form of water could be condensed that has a lower freezing temperature and a higher boiling temperature than ordinary water. The discovery was taken up by the prestigious Russian scientist Deryagin who brought his research to the attention of English-speaking scientists in a series of lectures from 1966 to 1968. In the West the surge of articles on polywater began in 1969. By 1973 when Deryagin admitted that the polywater phenomena were probably due to impurities in normal water, over 400 publications had appeared that referred to the subject (see Bennion and Neuton 1976).

Among mistaken research projects, some are only mistaken *ex post*, while others are mistaken both *ex post* and *ex ante*. Classifying a particular project is often difficult, but the polywater case to be studied here, as well as the N-Ray episode (Klotz 1980), phrenology (Cantor 1975) and some of the cases discussed by Langmuir (1989) are probably best thought of as *ex post* mistakes. Good examples of research projects that were mistaken both ex post and ex ante can be found in some of the chapters of Wallis's *On the Margins of Science* (1979).

Franks's *Polywater* (1981) is by far the most extensive account of the episode but other useful accounts are also available (Hasted 1971, Howell 1971, Gingold 1973, Gingold 1974). The polywater episode has several advantages over other mistakes that could be studied: (a) a consensus

exists that, at least *ex post*, research on polywater was a mistake, (b) the episode occurred recently enough for data to be available in the *Science Citation Index* on citations and number of articles, (c) the episode occurred long enough ago to permit observation of career consequences for the polywater scientists and (d) the number of identifiable scientists involved was larger than for most other mistakes that could be studied.

Kollman and Allen (1972) estimate that by January 1972 about 25 theoretical articles had been written on polywater. The development of theoretical models of anomalous water before the existence of the phenomena was well established is one of the aspects of the episode that has been criticized as being in violation of accepted standards of scientific practice. Allen and Kollman, to the contrary, in a theoretical article coauthored with others, aggressively defended early theorizing by saying that:

> Many scientists question the existence of such a new and well-defined form of water and it is therefore of obvious importance to investigate the problem theoretically—preferably by *ab initio* techniques. (Sabin *et al.*, 1970, 235)

What was obvious to Allen and Kollman in 1970 was apparently no longer so obvious in 1972 after the scientific community was close to a consensus that the polywater phenomena were due to impurities in normal water:

> It is not unexpected that a number of those theoreticians active in the theory of ordinary hydrogen bonds participated in the polywater problem, but the manner of their participation has been unusual—it is only rarely that both theoreticians and experimentalists have simultaneously been involved during that phase of research when the existence as well as the properties of a new material have been in question. (Kollman and Allen 1972, 288)

Franks has gone so far as to suggest that one methodological lesson of the episode is, quoting Sherlock Holmes, that "it is a capital mistake to theorize before one has data" (1981, 87). (In the early stages of the controversy, however, Franks had been more sympathetic to the pro-polywater researchers [1968, 48-49]).

Some current commentators are ready to label the polywater episode as bad science (see Eisenberg 1981; and Franks 1981, 180-182). Zuckerman, for instance, discusses the polywater episode as a "classic" case of a "disreputable error" because polywater's proponents failed "...to live up to the cognitive norms prescribing technical procedures designed to rule out even the most favored hypotheses if they are in fact unsound" (1977, 111 and 112). But other scientists (e.g., Deryagin 1983; Gould 1981, 15; and Pethica 1982) seem to view polywater more generously as one of those "...mistakes of good men earnestly seeking the truth" (Metzger 1970, 9).

Certainly there must be a continuum of mistakes graded by severity with plagiarism and data falsification on one extreme and contextually plausible, but ultimately unfruitful, hypothesis on the other. In another place, Diamond (1986a), I discuss in considerable detail the question of whether the pro-polywater theoreticians and experimentalists were doing work

that was consistent with the then-accepted standards for doing good science. My conclusion in that paper is that the better of the experimentalists and probably the better of the theoreticians were indeed doing work that met the then-accepted standards. If I am correct, the implication is that polywater was an ex post, but not an ex ante mistake.

However, for the purposes of this paper I need not precisely locate pro-polywater research on the continuum of mistakes. No matter how severe a mistake polywater is ultimately judged to have been, it is still important to understand why some scientists chose polywater as a research topic.

4. THE DATA

The data set compiled for this study is 'longitudinal,' which means that information is obtained on individuals over an extended period of time. The advantages of longitudinal data in studying scientists are discussed by Long (1978) and Diamond (1986b).

Biographical and career information was obtained for a sample of scientists who wrote on polywater (call them the 'polywater scientists') and a comparable sample of scientists who did not write on polywater (call them the 'non-polywater scientists'). Gingold's (1973) nearly exhaustive bibliography on polywater was the basic source for my sample of polywater scientists. Gingold's aim was apparently to include all publications, regardless of language, that mention polywater in any way. Most of the bibliography consists of publications that had appeared before 30 October 1972, although a few entries were added on the final page while Gingold's article was in proof.

As a partial check of the exhaustiveness of the Gingold bibliography, I compiled a list of entries that appeared in the *Science Citation Index (S.C.I.)* either under the heading 'anomalous water' or under the heading 'polywater' for the years 1968 through 1972, inclusive. I found that of the 97 entries, 81 were on the Gingold list. Although most of the remaining entries have both 'water' and 'anomalous' in the title, they do not appear in any sense to be related to the polywater research. I interpret this as evidence that Gingold's bibliography is a reasonably complete listing of the important research articles on polywater from 1968-1972.

I also eliminated monograph and textbook entries from my sample on the grounds that in modern times current research in science is reported in articles (see Kuhn 1970, 19-20). A few articles that appeared in edited books were included. Of the 437 total entries on the bibliography, I examined copies of 325.[4] Books, working paper technical reports and articles in foreign language journals account for most of the entries for which photocopies were not obtained. Of the 325 entries obtained, I excluded from further analysis those publications that were judged not to be research articles on polywater. The excluded publications consisted mainly of either

letters, review essays, articles for the popular press, or articles written in some language besides English.

Some journals' letters sections contained contributions that represented more substantive research than other journals' notes sections. So, although in general, notes were included in the sample and letters were excluded, a few exceptions were made. I also excluded research articles on subjects other than polywater that mention polywater only as an aside without bringing forward new evidence or argument. More specifically, if an article assumes the existence of polywater in order to make a substantive argument on another issue (e.g., Gingold # 171), then it was included in the sample (and classified as "pro"). If it briefly and vaguely mentions polywater as possibly relevant to the issue under discussion (e.g., Gingold # 421), then the article was excluded from further analysis. After the elimination of all excluded entries, my final sample of research publications on polywater consisted of 112 entries.

These remaining entries were examined in order to judge whether their attitude toward polywater was pro, con or neutral. An article was judged as being pro-polywater if on balance it seemed to support the position that the anomalous phenomena were due to some different structure of water. An article was judged as being con-polywater if on balance it seemed to support the position that the anomalous phenomena were due to impurities in the water. Articles that were so heavily qualified that they did not seem to lean toward either position were judged as being neutral. The process of judging an article's position is made more difficult by standards of scientific style that encourage qualification and discourage speculation or direct criticism of the work of others (see Diamond 1982, and MacRoberts and MacRoberts 1984).

A scientist was judged as having been pro-polywater if she had ever authored or coauthored a research article that was judged to have been pro-polywater. Thus Allen and Kollman who wrote an important pro article in 1970 and an important con article in 1971 were classified as 'pro.' A scientist was judged as having been con-polywater if she had never written a pro research article but had authored a con research article. A scientist was judged to have been neutral on polywater if she had never authored either a pro or con research article, but had authored an article that was judged to have been neutral.

The 437 entries in the Gingold bibliography were written by 415 different authors. The numbers differ because many entries had more than one author and some authors wrote more than one entry. Of the 415 authors, I was able to identify 115 authors whose only contribution to the bibliography consisted of publications that were excluded from the article sample for any of the reasons mentioned above. The 115 were excluded from the sample analyzed later in the paper.[5]

I also excluded from the data analyses 61 non-Western scientists (mainly Soviets) on the grounds that the citation process and reward structure in the East may have differed from those in the West. A second reason for excluding Soviets from the sample is that the plethora of Western transliterations of Soviet names substantially increases the noise in citation counts for Soviet scientists. I excluded as Soviets those (1) who were identified as Soviets by Franks or (2) who wrote only in Soviet sources *and* were not identified in any of my biographical sources as having been employed in the West from 1968-1983. Thus George K. Swinzow who coauthored with Deryagin in a Soviet journal and who grew up in the Soviet Union is *not* counted as a Soviet by us because he was employed by United States laboratories throughout the period.

Finally, another 44 authors were excluded because I had not been able to obtain any of the publications by them. A couple of scientists, for example, S. Levine and Joe L. White, were excluded because their last names and first initials were the same as those of other scientists in related fields, implying that distinguishing their citations from the citations of the like-named scientists would be prohibitively difficult (the *S.C.I.* does not give full first names in the citation volumes).

Using the article evaluations, I was able to code the remaining 195 scientists as being either pro, con or neutral. Of the 112 scientists classified as pro, 101 wrote at least one article that was pro and none that were con while 11 wrote at least one article that was pro and at least one article that was con. The 78 scientists classified as con each wrote at least one article that was con and none that were pro. The 5 scientists classified as neutral each had written only articles that were neutral. The classification of the 415 authors on the Gingold list into pro, con, neutral and various excluded categories is summarized in Table A-1. All tables in the paper are grouped together at the end of the text, just before the footnotes.

A control group was selected of scientists who presumably could have chosen to write on polywater but did not. The identification of those in the control group is difficult in part because the polywater topic stimulated considerable interdisciplinary interest. Bennion and Neuton have suggested that:

> Because of its startling implications, the possibility of a polymeric form of water caught the fancy of scientists worldwide in many disciplines, especially biology, chemistry, physics and the bio-medical areas. A conservative estimate would place the number at no less than 10^5 for those having the immediate potential of becoming involved in polywater research. (Bennion and Neuton 1976 54)

The distribution of polywater scientists by discipline is presented in Table A-2. Note that of those identifiable by discipline, over 60% had some affiliation with chemistry. The next most important discipline, physics, had only roughly 10% exclusively affiliated with it. I decided to restrict the control group to chemists in part because chemists had been the dominant

group in polywater research and in part because chemists document themselves (through their *Directory of Graduate Research*) better than do physicists. I restricted the control group to those who had published in *Science* because that was the most important vehicle for Western polywater research and because I wanted to insure that the control group consisted (like the polywater group) of those who were active in publishing. More concretely, the control group consists of the 37 authors of Research Notes (also sometimes called "Research Reports") in *Science* from 2 January 1970 through 19 March 1971 who, from their listed affiliations, appeared to be chemists. Some evidence that the control group does indeed contain those who could have written on polywater but chose not to, is provided by the three scientists who would have qualified for the control group but were excluded because they were also polywater scientists. The three excluded scientists were Kamb, Pimentel and Turkevich.

For all scientists in the sample, first-authored citation counts were obtained from the *S.C.I.* for the years 1968, 1969, 1974 and 1981. The *S.C.I.* only lists citations under the first author of an article. As a result, full citation counts can only be obtained by learning the first authors' names of all coauthored papers that a scientist has written and then obtaining a citation count for each paper from the *S.C.I.* under the first author's name. The process is time-consuming and laborious, the more so where, as in chemistry, the average number of articles is large and coauthorship is common. Some evidence (Diamond 1986b) suggests that first author citation counts may be a reasonably good proxy for full citation counts. For the present only first author counts have been used.

Values for biographical variables were obtained either from the various bi-annual editions of the *Directory of Graduate Research* (1977), from the first edition (1984) of *Chemical Research Faculties: An International Directory*, from the various editions of *American Men and Women of Science* or from responses to the survey. Data on number of publications were obtained from the annual source volumes of the *S.C.I.* Data on number of citations per year to the scientist's work were obtained from the annual citation volumes of the *S.C.I.*

Descriptive statistics for the various subsamples appear in Tables A-3 through A-5. Table A-6 identifies the scientists who contributed 6 or more publications to the polywater literature. Table A-7 provides variable definitions and Table A-8 presents simple correlations for many of the key variables in the study. In each cell in Table A-8, the first number is the simple correlation coefficient; the second number is the significance probability for use in testing the hypothesis that the correlation coefficient has a statistically significant difference from zero; and the third number is the number of observations used in calculating the correlation coefficient. The correlation coefficient is conventionally judged as "statistically significant" if the significance probability is less than 0.05.

Henry Small of the Institute for Scientific Information has graciously provided me with citation cluster data that were calculated by the Institute (see Garfield 1983, Mullins *et al.* 1977, and Small and Griffith 1974). The data identify those polywater scientists who were most visible in their acceptance of polywater and, hence, whose careers might have been most damaged. The articles that are included in the pro and con clusters are listed in Tables A-9 and A-11, respectively. The names and some descriptive statistics for the authors of the articles in the pro and con clusters are given in Tables A-10 and A-12, respectively.

A survey was conducted of the scientists in this study. The initial mailing was sent to the most recent known address of all those authors living in the United States who either had written publications listed in the Gingold bibliography or else were members of the control group. In the course of the paper, I will quote remarks made by the scientists in their answers to the survey. Since the scientists were assured confidentiality, I will refer to them only by their general position (i.e., pro, con, etc.). The survey response rates for all of the subsamples are given in Table A-13.

Most of the survey questions that will be utilized here were of the sort that asked the respondent for factual information about past or present demographic, family background and income variables. As such, they were similar in kind to questions asked in surveys routinely used by labor economists such as the National Longitudinal Surveys. A couple of the questions, however, were of the sort that economists are generally skeptical toward. In particular, I asked the scientists if they had viewed polywater research as risky when they made their decision to choose it as a research topic. McCloskey (1985, 9-10) discusses the usual story that has been told to discredit such questions, but goes on to note that the story might be given a new moral: Some questions about motives and beliefs are more useful than others. Even Friedman, who is one of those most opposed to the use of such questions for testing, argues that they often are useful as a source of hypotheses (1953, 31).

In my survey, I asked the polywater scientists whether their papers had supported the existence of an anomalous form of water. As mentioned earlier, I also examined the papers independently in order to judge from the published evidence whether the scientists had supported the existence of an anomalous form of water. Three of those whom I had judged to have been pro, considered themselves con. None of those whom I had judged con considered themselves pro. Also worth noting is that several of the others who were pro made a point of minimizing the extent of their involvement (in time and effort) in polywater research.

5. THE HYPOTHESES

I discuss several broad classes of hypotheses that are relevant to the account of research project choice that was sketched above. The discussion

will include a description of the hypothesis and remarks on how it relates to the general account of research project choice. The discussion of each hypothesis will conclude with evidence from the polywater episode on the truth of the hypothesis. The hypotheses are divided into two broad groups. First, I consider those that are mainly related to how scientists differ in the extent of their risk aversion. This group includes attitudes toward risk, age, number of children, income and academic tenure. Second, I consider those that are mainly related to their information, ability and other constraints. This group includes funding and journal demand, religion, geographical location, and characteristics of family of origin and number of citations (as a measure of knowledge at the frontiers of science). Some of the hypotheses could be included in either group, depending on which version of the hypothesis is emphasized. Age, for instance, may affect risk aversion, but may also affect information and ability.

6. ATTITUDES TOWARD RISK

Stigler in his Nobel Lecture (1983, 538) has suggested that different attitudes toward risk can partially explain why some scientists accept a new theory while others reject it. The few scientific innovators are not risk averse, according to Stigler. The vast majority of scientists, including most of the young, are risk averse, and hence tend toward scientific conservatism.

Lester Telser has argued that variation in product quality will be less for well-advertised products than for other products, since consumers will be more apt to recall and retell bad experiences with a well-advertised product (Telser 1974, 32). Similarly, one might argue that variation in article quality would be less for a well-cited scientist than for others since scientists are more likely to recall and retell having read a bad article by a well-cited scientist. The well-cited scientist thus acts more risk averse than the less-cited because the well-cited scientist has more to lose from a mistake and less to gain from a success.

Psychologist David C. McClelland has argued that persons with a high need to achieve (as measured by his battery of test questions) have greater general self-confidence (1976, 222-223). Thus in situations in which they have limited prior information they will estimate that their chances of success are greater than will otherwise similar persons with a low need to achieve. When good information on past performance on similar tasks is available, however, both those with a high need to achieve and those with a low need to achieve are equally accurate in estimating their chances of success. Thus under conditions of uncertainty about personal performance, persons with a high need to succeed will take greater risks than persons with a low need to succeed.

The pro polywater scientists seem to have differed in how they perceived the riskiness of the polywater episode. I asked the scientists whether they

considered polywater a very risky topic, greater than average riskiness, about average in riskiness or safe. The responses to this question, broken down by subsample, are presented in Table A-14. One notable feature of the answers is that a majority of the pro scientists report having perceived the polywater project to have been above average in riskiness.

In retrospect, several more precise questions might have been more informative than the broad one actually asked. For example, one question might have been how high the scientists thought that the probability was that the research might not be fruitful. A separate question might have been whether the scientists thought that, if unsuccessful, the research would damage their careers. Several of the scientists in the sample worked on the project because they were post-doctoral fellows or research assistants for a more distinguished colleague. One of these in fact told Franks (1981, as quoted on p. 190) that "...as a graduate student I don't stand to lose as much if I turn out completely wrong."

An additional ambiguity in the original question applies solely to the scientists in the con and *Science* subsamples. For them the question could have been interpreted as asking about the risk of writing a pro article on polywater or, alternatively, of writing on polywater at all (pro or con). Writing con on polywater in 1973, for instance, would have been a very low risk activity.

The ambiguity of the question on risk for the con and *Science* subsamples is reduced, however, by considering the responses of the *Science* subsample to the question: If you were aware of polywater research at the time, why did you not choose polywater as a research topic? All of the responses for the *Science* sample are reproduced below:

(500) I had better things to do.

(506) Too high a probability it wasn't a real phenomenon.

(511) Other, more interesting things to do.

(516) Not close to my field.

(526) Couldn't think of any good experiments or novel approaches.

(529) 1. The reports as presented were not credible.

2. In my opinion, the definitive experiments required very sophisticated equipment, not available to me at that time.

3. My research group was fully occupied with more significant problems.

(534) Good luck.

7. AGE

According to Edge, sociologists expect that greater risks will be taken by younger scientists than older scientists (Edge 1977, 336).

The gene-maximization hypothesis from sociobiology has been used in a couple of papers by economists in order to make inferences about optimal risk aversion in various circumstances (Rubin and Paul 1979, Diamond and Locay 1989). Rubin and Paul, for instance, argued that societies would have a survival advantage in which (holding wealth constant) mature males had greater risk aversion than adolescents.

Max Planck has claimed that: "...a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (Planck 1949, 33-34). Nearly all those who quote Planck's Principle accept it as true. The usual inference drawn from the Principle is that science is an irrational enterprise, "rationality" here being understood as the adherence to objective standards in judging theories.

Although the inference is *usually* drawn, I have shown elsewhere (Diamond 1988) that, even if older scientists do in fact accept new theories more slowly, this phenomenon is compatible with all scientists evaluating theories on the basis of standards such as elegance, rigor, and explanatory scope. Whether or not such an argument succeeds in making Planck's Principle compatible with the rationality of science, the first question is: Was Planck right? Until several years ago, the truth of his principle was merely assumed, but recently a few studies have begun to test it.

Finding important scientists who are counterexamples to Planck's Principle is one way to test the truth of the principle (Brush 1976, 94; Hagstrom 1965, 291; and Blackmore 1978, 347-349). But such counterexamples can never serve as conclusive refutation of the principle since they leave open the possibility that the Principle still holds for scientists most of the time. More rigorous and systematic tests of the importance of a scientist's age as a determinant of theory acceptance can be found in Hull *et al.* 1978, Diamond 1980, and Gieryn and Hirsh 1983. All three studies found a statistically significant effect of age in the direction predicted by Planck, although the first two found that the magnitude of the effect was small.

We are interested here in learning the effect of age on the acceptance of polywater. I have data on year of Ph.D. for many more scientists than I have data for year of birth. So instead of looking at the effect of age on the acceptance of polywater, I look at the effect of experience (which is highly correlated to age and could be alternatively labelled as "professional age"). The operational definition of "experience" is 1971 minus the year of receipt of the Ph.D degree. Even apart from data considerations, experience may be the more appropriate concept if I accept the 'costs of retooling' explanation for Planck's Principle.

In results reported in Diamond 1988a, I find that experience was not a statistically significant determinant of the probability that a scientist would accept polywater.

8. NUMBER OF CHILDREN

The number of children that a scientist has may effect her scientific decisions in a number of ways. One is that having dependent children may make a scientist more risk-averse (see Rubin and Paul). Another is that children and advancing the frontier of knowledge may be substitute technologies for producing the 'impact on future generations' z-good.

Evidence on the number of children reported by respondents to the survey is reported in Table A-16. No systematic differences between the different subsamples in average number of children are suggested by the table.

9. INCOME

Two measures of risk aversion have become standard in the economics profession: absolute-risk aversion and relative-risk aversion. Where utility is a twice differentiable function of wealth with positive first derivative and negative second derivative, absolute-risk aversion is defined by:

$$8.1 \quad \frac{U''(W)}{U'(W)}$$

and relative-risk aversion is defined by:

$$8.2 \quad \frac{WU''(W)}{U'(W)}$$

where U is utility and W is wealth. Economists routinely assume that absolute-risk aversion is a decreasing function of wealth. A less routine assumption is that relative-risk aversion is an increasing function of wealth (Arrow 1971, 96).

Apart from the just-mentioned hypotheses, economists have seldom ventured to hypothesize how persons can be expected to differ in their extent of risk aversion. Holding wealth constant, differences in risk aversion between persons are usually attributed to differences in tastes. As with other tastes, economists generally adopt an agnostic position, assuming (again holding wealth constant) either that risk aversion is constant among persons or else that it varies stochastically.

One notable exception to this generalization is a 1983 paper by Morin and Suarez. Using Canadian data on household assets, they find that relative-risk aversion is a *decreasing* function of wealth. In addition, they find that when wealth is controlled for, relative-risk aversion increases with age. A second exception to the economist's usual agnosticism on risk aversion is Reuven Brenner who concludes that "...when one's relative position in the distribution of wealth is worsened, one will gamble more frequently on new ideas..." (1983, 27).

Income in 1971 for scientists in each of the subsamples is reported in Table A-16. The accuracy of the data may be limited to some extent by the fact that the scientists are being asked to remember their income over 10 years earlier. No systematic differences in income are apparent for the different groups.

10. TENURE

A few labor economists in recent years have attempted to model the labor market experience of academic scientists (Freeman 1977, Siow 1979, Harris and Weiss 1981, Weiss and Lillard 1982).

For Siow, productivity is exogenous although, unlike Freeman, all scientists draw from the same productivity distribution. All scientists in the first period receive less than their expected value of marginal product and all scientists in the second period receive more than their expected value of marginal product. Siow's formal model has nothing to say about a scientist's choice of research topics although he writes some suggestive obiter dicta near the end of the paper. In particular, Siow hypothesizes that younger pre-tenure scientists will take fewer risks than older post-tenure scientists (Siow 1979, 20). Remarks by Weiss and Lillard (1982) seem to support the Siow hypothesis.

More recently, other papers which have attempted to explain the existence of tenure have been written by Ito and Kahn (1986), Carmichael (1988), and Kahn and Huberman (1988).

I have no data on the year of receipt of tenure for any of the members of my sample. However, I do have longitudinal data on academic rank for many scientists in the sample. Since year of promotion to the rank of associate professor usually corresponds closely to the year of tenure, such information should be useful in evaluating the effect of tenure on a scientist's position on polywater. Table A-17 reports the rank of scientists in various subsamples as of the beginning of the polywater episode in the West (i.e., 1969). Most of the scientists in the sample had obtained the rank of associate or full professor by the beginning of the polywater episode. It appears that slightly more scientists in the con and *Science* subsamples did not have tenure than in the pro subsample. This would be consistent with Siow's hypothesis that after the achievement of tenure, scientists choose riskier research projects.

11. FUNDING AND JOURNAL DEMAND

We would expect that, ceteris paribus, scientists would choose research projects that could be funded and would result in publishable articles. Several of the survey respondents suggested that funding and an interest by prestigious journals in the polywater episode explains why many scientists chose polywater as a research topic. One of the pro scientists (41) said in the questionnaire that:

> Most people writing about polywater or who sought research grants did so simply because the funds were available, not because they may or may not have believed in the existence of polywater.

12. RELIGION

Merton ([1936] 1970) in his doctoral dissertation, adapting earlier work by Weber ([1930] 1958), suggested that the Protestant ethic encouraged good science. Hardy (1974) has suggested that certain denominations seem to be over-represented among scientists relative to their numbers in the general population. The interpretation of simple correlations between religious denomination and measures of scientific achievement must be undertaken with great care because denomination may be a proxy for a wide range of other variables including, for example, income and family size.

Information on the religious affiliation of the scientists in the sample is presented in Table A-18. No systematic differences in religious affiliation appear to be present across the subsamples.

13. GEOGRAPHICAL LOCATION

Geographical location may be important to a scientist's choice of research projects for several reasons. A scientist at a major research center will likely have access to important resources required for successfully carrying out a project, not the least of which is access to able colleagues. Stigler has emphasized that access to colleagues is important because their conversation suggests fruitful topics, their criticism improves the quality of the research and their knowledge of the work increases the likelihood that it will have an impact on the larger community of scientists (Stigler 1986).

Pledge (1959) has claimed that the birthplace of scientists tends to have been clustered in certain locations. His suggestion, however, did not take into account general population density.

The distribution of Western polywater scientists (whether pro, con or neutral) by country is presented in Table A-19. The distribution of United States polywater scientists by state is presented in Table A-20.

The number of scientists for whom polywater had been suggested as a promising topic is reported in Table A-21. The importance of location in a research group in the choice of research project is highlighted by the following comments by scientists on who had influenced them in their choice of polywater as a project. The following comments are all those that were made on this issue.

Pro Sample

(2) Senior scientists with my organization. I was a junior scientist at the time.

(29) Ordered by W. A. Zisman. Zisman was Chemistry Department Superintendent at Naval Research Laboratory. I was two levels below him as a research chemist.

(185) Sponsor-Federal Agency.

(384) Co-worker in research at Xerox suggested concept of polywater vulnerable.

(241) Senior doing senior research project suggested we look into it. I knew how to use the equipment.

(41) Dr. James Kassner, Jr., was my faculty advisor.

(98) I first learned of it in the ONR newsletter and thought it looked interesting.

(107) I accepted their results (the Russians) as a challenge on a theoretical basis. Just as there are variations on the structure of ice, certainly I thought there could be variations in the structure of water. Maybe there are different structures. Certainly the structure of water is different inside of cells. But what causes this change? Perhaps it is the network of protein and polysaccharide chains inside the cell. Nevertheless, there *is* a difference in properties inside a cell as compared to outside that cell.

(202) Research advisor.

Neutral Sample

(122) Deryagin's publications suggested the topic was interesting. I have known him and respected his outstanding work for years.

Con Sample

(322) I was his postdoctoral fellow.

(386) Scientific Colleague interested in aqueous solutions at high pressure.

(154) I was a post doctoral associate at the time.

(172) I was a postdoc working for people who had money from the Office of Navel Research to attempt to prepare polywater and measure its properties if possible. Drs. Kassner and Zung obtained the grants.

(200) Supervisor

14. FAMILY OF ORIGIN

A long tradition (e.g., Cattell and Brimhall 1921) suggests that scientific achievement may be related to the characteristics of the family of origin of the scientist. Some versions of this approach have been criticized by Stigler (1982b) and Coase (1984) using Mill and Marshall respectively as examples.

Becker (1981) has emphasized the trade-off between quantity and quality of children. I would expect, ceteris paribus, that children with few siblings would have a greater parental investment in their human capital, including (but not limited to) the kinds of human capital that would be useful to a productive scientist. Following up on a long-noted empirical regularity (e.g., Galton 1875), Zajonc and Marcus (1975) have argued that the allocation of time in the household will result in greater parental investment in the first born and, to a lesser extent, in the last born.

On the basis of these considerations, I would expect that scientific ability

would be inversely related to the number of the scientist's siblings and would also be inversely related to the scientist having been a 'middle' child.

In Table A-23 I present data on the number of children (including the scientist himself) in the scientist's family of origin. Pro scientists seem to have come from larger families than con scientists. In Table A-24 I present data on the birth order of the scientists. No important differences in birth order appear between the various subsamples.

15. NUMBER OF CITATIONS

If one scientist cites the work of another scientist, then the first scientist is usually saying that the work of the first scientist (no matter how old) is still relevant to the current research. Thus, the number of citations that a scientist receives from her fellow scientists in a given year can be interpreted as a proxy for her stock of knowledge at the frontiers of the discipline (Diamond 1984). One interesting question is whether those with a larger stock of knowledge at the frontier will be more or less likely to choose polywater as a research project. Choosing a successful research project is partly due to good judgement and partly due to luck. If high citations are mainly due to good judgement, then we might expect that those with higher citations would be *less* likely to choose an ultimately-unsuccessful project such as polywater. On the other hand, if high citations are mainly due to lower levels of risk aversion, then we might expect that those with higher citations would be *more* likely to choose an ultimately-unsuccessful project such as polywater.

Looking at Table A-8, we find that citations in 1968 (AUTH68) is positively correlated: with having written on polywater (*either* pro *or* con), with having written a pro article on polywater, and with having written a highly visible pro article. This lends some credence to the view that those with higher citations have lower levels of risk aversion.

16. CONCLUSIONS

We presented a maximization-under-constraints framework that suggests that differences among scientists in their choice of research projects will depend on differences in tastes (most notably in risk aversion), and in differences in constraints (most notably the scientist's ability, time, information and research resources). Many of the hypotheses that have been commonly suggested to account for the behavior of scientists can be interpreted within this framework.

We examine several of these hypotheses using data from a particular episode in the recent history of science.

Several of the hypotheses have been discussed as though various characteristics of the scientist were exogenous. Some of these characteristics, at least partially, may be endogenous. Religious denomination, for in-

stance, might in part be chosen for its compatibility with a life in science, rather than the other way around. The number of children that a scientist decides to have may be influenced by the intellectual (and therefore career) risks that she intends to take. Dealing with the potential partial endogeneity of many of these characteristics is an important objective of future work.

Some arbitrariness has been involved in the choice of hypotheses to consider. In retrospect others might equally well have been discussed. One would be the extent to which scientists consider the potential for practical applicability when choosing a research project. Polywater was perceived by many as having practical consequences in explaining the unusual behavior of water in a wide variety of contexts, including water in soil and in biological organisms.

Another hypothesis is that scientists sometimes choose a research project because they view it as being "fun." Several respondents mentioned this as a partial motive for the polywater research. Munevar (1981), pursuing a suggestion of Lorenz's, has suggested that much important scientific research is pursued as a form of play (1981, 66-69).

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TABLE A-1

Classification of Authors of Entries on Gingold's List

Classification	Number of Authors Falling Under Classification
Excluded because non-Western	61
Excluded for having written only letters, review essays, articles for the popular press, or articles that only mention polywater in a trivial aside	115
Excluded for having written only articles written in some language besides English (number only includes those not already excluded because non-Western)	9
Excluded because I was unable to obtain any publications by them (most of those publications not obtained appear to be in a foreign language, so most of the scientists excluded in this category would have been excluded for falling in the previous category even if all publications had been obtained	52
Scientists who wrote only articles that were neutral	5
Scientists who wrote at least one article that was con and none that was pro	77
Scientists who wrote at least one article that was pro and none that was con	85
Scientists who wrote at least one article that was pro and at least one article that was con	11
Total Number of Authors in Gingold (1973) (sum of the above)	415

TABLE A-2

Field of Employment	Number of Scientists	
Chemistry (academic)	87	(42.0)
Chemistry-Physics (academic)[b]	11	(5.3)
Chemistry-Specialized Applications[c] (academic)	7	(3.4)
Physics (academic)	12	(5.8)
Engineering (academic)	3	(1.4)
Biology and Biophysics (academic)	2	(1.0)
Other academic[d]	11	(5.3)
Government or private lab	58	(28.0)
Other	14	(6.8)
Missing	2	(1.0)
TOTAL	207	

[a]Indicates that both "physics" and "chemistry" were in the title of the unit of affiliation.

[b]Category includes: crystallography (3), chemical engineering (2), geochemistry (1), polymers (1), biochemistry and physics (1), and dental materials and surface chemistry (1).

[c]Category includes both: (a) specific academic affiliations other than those listed above and (b) academic affiliations too vague to classify. Note that under (b) are included some scientists who would with fuller information, be classed in one of the first four categories.

[d]Most of the information was obtained from the affiliation as listed on the article on polywater. Percentages for the nonmissing observations are given in parentheses beside the frequencies.

Field of Employment for Polywater Scientists at Time of Polywater Article[d]

TABLE A-3

Variable	N	Mean	Standard Deviation	Minimum	Maximum
Age in 1971	41	40.1	10.3	23	79
Year of BS	33	1954.5	9.4	1923	1970
Year of Ph.D.	46	1959.3	9.5	1926	1979
Citations in 1968	97	35.5	131.1	0	1000
Citations in 1969	96	35.3	131.9	0	1087
Citations in 1974	97	44.1	165.4	0	1546
Citations in 1981	96	40.6	118.1	0	1016
Articles in 1970	97	3.1	3.9	0	20
Articles in 1971	97	3.2	3.6	0	20
Articles in 1972	97	2.3	3.2	0	17
Articles in 1973	97	2.0	2.8	0	14
Articles in 1974	97	2.0	3.0	0	10

Description Statistics for Pro Subsample (N=98)

TABLE A-4

Variable	N	Mean	Standard Deviation	Minimum	Maximum
Age in 1971	20	40.9	10.0	22	60
Year of BS	19	1951.9	10.6	1931	1970
Year of Ph.D.	26	1960.5	10.3	1935	1978
Citations in 1968	38	20.3	31.1	0	100
Citations in 1969	38	26.9	41.6	0	151
Citations in 1974	38	34.1	46.9	0	176
Citations in 1981	38	43.2	56.6	0	245
Articles in 1970	38	4.4	4.9	0	26
Articles in 1971	38	3.2	3.6	0	14
Articles in 1972	38	2.5	3.3	0	14
Articles in 1973	38	3.7	4.4	0	15
Articles in 1974	38	2.8	3.5	0	15

Descriptive Statistics for Science Subsample (N=38)

TABLE A-5

Variable	N	Mean	Standard Deviation	Minimum	Maximum
Age in 1971	24	40.3	9.3	26	64
Year of BS	23	1953.1	9.5	1928	1967
Year of Ph.D.	33	1962.1	8.1	1934	1977
Citations in 1968	76	15.2	29.0	0	128
Citations in 1969	76	16.7	31.2	0	135
Citations in 1974	76	18.1	34.1	0	198
Citations in 1981	76	25.5	47.1	0	277
Articles in 1970	76	2.4	3.5	0	20
Articles in 1971	76	2.5	3.5	0	21
Articles in 1972	76	2.1	3.8	0	21
Articles in 1973	76	1.7	3.3	0	20
Articles in 1974	76	2.2	3.9	0	28

Descriptive Statistics for Con Subsample (N = 76)

TABLE A-6

Number of Publications on Polywater per Scientist	Number of Scientists in Sample Who Wrote Number of Publications in First Column	Names of Scientists in Sample Who Wrote Number of Publications in First Column
1	102	
2	33	
3	13	
4	12	
5	6	Davis, R. E.; Drost-Hansen, W.; Jakobsen, R. J.; Lingertat, H.; Rao, C. N. R.; Rousseau, D. L.
6	4	Bradspies, J. I.; Everett, D. H.; Finney, J. C.; Howell, B. F.
7	3	Entine, G.; Horne, R. A.; Killman, P. A.
8	2	Brummer, S. B.; Lippincott, E. R.
9	1	Adlfinger, K. H.
10	1	Allen, L. C.
11	2	Abendroth, R. P.; Peschel, G.
	179	

[a]"Publications" here may include letters to the editor, review articles and articles for the popular press. To be included in the dample, however, a scientist had to have written at least one article on polywater.

Number of Publications[a] on Polywater Written by Polywater Scientists

TABLE A-7

POLY	1 = scientist wrote on polywater; 0 = scientist did not write on polywater
SPEED	year of Ph.D. minus year of BS
RANK69 (RANK75)	0 = lecturer in 1969 (1975); 1 = instructor in 1969 (1975); 2 = assistant professor in 1969 (1975); 3 = associate professor in 1969 (1975); 4 = full professor in 1969 (1975)
NUMARTPW	number of publications on polywater
PRO	1 = pro on polywater 0 = con, neutral or did not write on polywater
PROCLUST	1 = highly visible pro-polywater 0 = everyone else
AUTH68 (AUTH74)	citations received by scientist in 1968 (1974)
POST69AR	number of non-polywater articles written from 1970 through 1974
AGEIN71	1971-(year of birth)

Definition of Variables Used in Correlation Table

TABLE A-8

	POLY	SPEED	RANK69	RANK75	NUMARTPW	AGEIN71
POLY	1.000 0.00 216	0.120 0.29 78	0.086 0.54 53	0.048 0.74 49	0.415 0.00 216	-0.023 0.84 87
SPEED	0.120 0.29 78	1.000 0.00 78	-0.403 0.01 46	0.062 0.69 45	0.136 0.24 78	0.197 0.08 78
RANK69	0.086 0.54 53	-0.403 0.01 46	1.000 0.00 53	0.905 0.00 43	0.086 0.54 53	0.253 0.08 50
RANK75	0.048 0.74 49	0.062 0.69 45	0.905 0.00 43	1.000 0.00 49	0.022 0.88 49	0.293 0.04 49
NUMARTPW	0.415 0.00 216	0.136 0.24 78	0.086 0.54 53	0.022 0.88 49	1.000 0.00 216	0.021 0.85 87
AGEIN71	-0.023 0.84 87	0.197 0.08 78	0.253 0.08 50	0.293 0.04 49	0.021 0.85 87	1.000 0.00 87
PRO	0.417 0.00 216	0.017 0.88 78	0.209 0.13 53	0.124 0.39 49	0.378 0.00 216	-0.043 0.69 87
PROCLUST	0.156 0.02 216	-0.016 0.89 78	0.159 0.25 53	-0.146 0.32 49	0.210 0.00 216	-0.081 0.46 87
AUTH68	0.025 0.71 216	-0.094 0.41 78	0.183 0.19 53	0.139 0.34 49	0.035 0.61 216	0.143 0.19 87
AUTH74	-0.006 0.93 216	-0.090 0.43 78	0.163 0.24 53	0.098 0.50 49	0.019 0.79 216	0.092 0.40 87
POST69AR	-0.166 0.01 216	-0.194 0.09 78	0.124 0.38 53	0.041 0.78 49	0.015 0.82 216	0.016 0.88 87
PROFAGE	0.014 0.88 107	-0.201 0.08 78	0.480 0.00 52	0.389 0.01 49	0.078 0.43 107	0.878 0.00 83

[a]Correlation Coefficients/Prob > |R| Under HO:RHO=0/Number of Observations.

Correlation Coeffieients for Selected Variables[a]

TABLE A-8 (continued)

	PRO	PROCLUST	AUTH68	AUTH74	POST69AR	PROFAGE
POLY	0.417 0.00 216	0.156 0.02 216	0.025 0.71 216	-0.006 0.93 216	-0.166 0.01 216	0.014 0.88 107
SPEED	0.017 0.88 78	-0.016 0.89 78	-0.094 0.41 78	-0.090 0.43 78	-0.194 0.09 78	-0.201 0.08 78
RANK69	0.209 0.13 53	0.159 0.25 53	0.183 0.19 53	0.163 0.24 53	0.124 0.38 53	0.480 0.00 52
RANK75	0.124 .039 49	-0.146 0.32 49	0.139 0.34 49	0.098 0.50 49	0.041 0.78 49	0.389 0.01 49
NUMARTPW	0.378 0.00 216	0.210 0.00 216	0.035 0.61 216	0.019 0.79 216	0.015 0.82 215	0.078 0.43 107
AGEIN71	-0.043 0.69 87	-0.081 0.46 87	0.143 0.19 87	0.092 0.40 87	0.016 0.88 87	0.878 0.00 83
PRO	1.000 0.00 216	0.281 0.00 216	0.102 0.13 216	0.090 0.19 216	-0.031 0.65 216	0.089 0.36 107
PROCLUST	0.281 0.00 216	1.000 0.00 216	0.099 0.15 216	0.035 0.60 216	-0.060 0.38 216	0.018 0.85 107
AUTH68	0.102 0.13 216	0.099 0.15 216	1.000 0.00 216	0.909 0.00 216	0.341 0.00 216	0.224 0.02 107
AUTH74	0.090 0.19 216	0.035 0.60 216	0.909 0.00 216	1.000 0.00 216	0.427 0.00 216	0.161 0.10 107
POST69AR	-0.031 0.65 216	-0.060 0.38 216	0.341 0.00 216	0.427 0.00 216	1.000 0.00 216	0.136 0.16 107
PROFAGE	0.089 0.36 107	0.018 0.85 107	0.224 0.02 107	0.161 0.10 107	0.136 0.16 107	1.000 0.00 107

Correlation Coefficients for Selected Variables (continued)

TABLE A-9

Journal, Year and First page of Article	Total Citations in 1970 to Cocited Article (if 16 or more)	Total Citations in 1971 to Cocited Article (if 16 or more)	Scientists Who Coauthored Article	Pages of Frank's Judgment that Article Was Pro
Science, 1970, p. 1443	—	27	Allen, L.C. Kollman, P.A.	p. 90
Chem. Ind. London, 1969, p. 686	24	17	Bellamy, L.J. Osborn, A.R. Lippincott, E.R. Brady, A.R.	pp. 68-69
Nature, 1969, p. 865	17	—	Bolander, R.W. Kassner, I.L. Zung, J.T.	p. 65
Science, 1970, p. 865	—	16	Castellion, G.A. Grabar, D.G. Hession, J. Burkhard, H.	—
Science, 1969, p. 1482	58	46	Lippincott, E.R. Stromberg, R.R. Grant, W.H. Cessac, G.L.	p. 70
Science, 1970, p. 51	—	32	Page, T.F. Jakobsen, R.J. Lippincott, E.R.	—
Science, 1970, p. 171	—	17	Petsko, G.A.	—
Nature, 1969, p. 159	24	21	Willis, E. Rennie, G.K. Smart, C. Pethica, B.A.	pp. 61-62

[a]An article was judged to be highly visible if it was a member of a group of articles that were frequently cited together. Articles not mentioned in the Frank's book (1981), were examined directly to judge whether they were pro, con, or neutral.

Data on Highly Visible Pro Articles on Polywater[a]

TABLE A-10

Name	Age in 1971	Year of Ph.D.	Citations in 1968	Citations in 1974	Citations in 1981	Location in 1969-71	Location in 1979-83
Allen, L.C.	45	1957	49	66	37	Princeton	Princeton
Bandy, A.R.	31	1968	0	5	2	Old Dom. Univ.	Drexel
Bellamy, L.J.	—	—	809	488	469	—	1981-Univ. of Birmingham (England)
Bolander, R.	31	1969	0	4	2	General Motors Institute	Same
Burkhard, H.	—	—	0	1	3	—	1982-Deutsch Bundespost FT2 (Fed. Rep. Ger.)
Castellion, G.A.	—	1956	2	0	2	—	—
Cessac, G.L.	—	—	0	2	3	—	—
Grabaer, D.G.	—	—	1	2	0	—	—
Grant, W.H.	38	1968	3	3	12	Nat. Bureau Standards	Same
Hessian, J.	—	—	0	0	0	—	—
Jakobsen, R.J.	42	—	37	27	79	Battelle Cols. Labs	Same
Kassner, I.L.	40	1957	6	5	15	Univ. of Missouri-Rolla	Same
Kollman, P.A.	27	1970	0	160	116	Univ. of Cal. SF	Same
Lippincott, E.R.	51	1947	120	112	74	Univ. Maryland	Same
Osborn, A.R.	—	—	6	4	2	—	—
Page, T.F.	—	—	14	21	45	Univ. of IL-Urbana	Same
Pethica, B.A.	45	1949	44	36	32	—	1981-Clarkson Col. Technology, NY
Petsko, G.A.	23	1973	0	2	21	Wayne State Univ.	MIT(81)
Rennie, G.K.	—	—	1	1	3	—	—
Smart, C.	—	—	17	5	17	—	1983-under Smart, C.C. Univ. of Edinburgh (Scotland)
Stromberg, R.R.	—	1951	24	40	18	—	—
Willis, E.	—	—	3	2	3	—	1981-City Hospital Copenhagen (Denmark)
Zung, J.T.	41	1960	8	2	2	Univ. Missouri-Rolla	Same

Age, Citation and Location Data on Pro Polywater Scientists

TABLE A-11

Journal, Year and First Page of Article	Total Citations in 1970 to Co-cited Article (if 16 or more)	Total Citations in 1971 to Co-cited Article (if 16 or more)	Scientists Who Coauthored Article	Pages of Frank's Judgment That Article Was Con
Nature, 1970, p. 1033	—	19	Everett, D.H. Haynes, J.M. McElroy, P.J.	pp. 85-86
Science, 1970, p. 48	—	22	Rabideau, S.W. Florin, A.E.	p. 130
Science, 1970, p. 1715	—	38	Rousseau, D.L. Porto, S.P.S.	pp. 101, 135

[a]An article was judged to be highly visible if it was a member of a group of articles that were frequently cited together.

Data on Highly Visible Con Articles on Polywater[a]

TABLE A-12

Name	Age in 1971	Year of Ph.D.	Citations in 1968	Citations in 1974	Citations in 1981	Location in 1969-71	Location in 1979-83
Everett, D.H.	—	—	148	119	155	—	—
Florin, A.E.	—	—	3	1	3	—	—
Haynes, J.M.	—	—	7	3	9	—	—
McElroy, P.J.	—	—	0	0	3	—	—
Porto, S.P.S.	—	—	51	37	26	—	—
Rabideau, S.W.	—	—	36	18	15	—	—
Rousseau, D.L.	31	1967	5	15	0	Univ. of Southern Cal.	Bell Labs (1981)

Age, Citation and Location Data on Con Polywater Scientists

TABLE A-13

Subsample	Number in Subsample	Number Responded in Subsample	Percentage of Subsample Who Responded
Con	77	11	14.3
Neutral	5	1	20.0
Pro	96	12	12.5
Wrote on Polywater	178	24	13.5
Highly Visible Pro	23	4	17.4
Science	38	9	23.7

Questionnaire Response Rates for Subsamples

TABLE A-14

Responses	Con	Neutral	Pro	Wrote on Polywater	Highly Visible
A Very Risky Topic	3 (30.0)	—	3 (25.0)	6 (26.1)	2 (50.0)
A Topic of Greater Than Average Riskiness	2 (20.0)	—	2 (16.7)	4 (17.4)	1 (25.0)
A Topic About Average in Riskiness	1 (10.0)	—	3 (25.0)	4 (17.4)	—
A Safe Topic	3 (30.0)	—	1 (8.3)	4 (17.4)	—
Did Not Respond	1 (100.0)	1 (10.0)	3 (25.0)	5 (21.7)	1 (25.0)

Consideration of Polywater as a Risky Topic

TABLE A-15

Responses	Con	Neutral	Pro	Wrote on Polywater	Highly Visible	*Science*
Zero	—	—	1 (7.1)	1 (4.5)	—	—
One	—	—	1 (7.1)	1 (4.5)	—	—
Two	1 (14.3)	—	4 (28.6)	5 (22.7)	—	3 (60.0)
Three	6 (85.7)	—	3 (21.4)	9 (40.9)	—	2 (40.0)
Four	—	1 (100.0)	3 (21.4)	4 (18.2)	2 (66.7)	—
Five	—	—	1 (7.1)	1 (4.5)	1 (33.3)	—
More Than Five	—	—	1 (7.1)	1 (4.5)	—	—

Number of Children

TABLE A-16

Responses	Con	Neutral	Pro	Wrote on Polywater	Highly Visible Pro	*Science*
less than $10,000	2 (18.2)	—	2 (16.7)	4 (16.7)	1 (25.0)	1 (11.1)
$10,000 - $15,000	1 (9.1)	—	1 (8.3)	2 (8.3)	1 (25.0)	1 (11.1)
$15,000 - $20,000	—	—	4 (33.3)	4 (16.7)	1 (25.0)	1 (11.1)
$20,000 - $25,000	2 (18.2)	—	3 (25.0)	5 (20.8)	1 (25.0)	1 (11.1)
$25,000 - $30,000	1 (9.1)	1 (100.0)	1 (8.3)	3 (12.5)	—	1 (11.1)
$30,000 - $35,000	1 (9.1)	—	1 (8.9)	2 (8.3)	—	—
Did Not Respond	4 (36.44)	—	—	4 (16.7)	—	4 (44.4)

Monetary Income in 1971

TABLE A-17

Subsample	Lecturer-Instructor	Assistant Professor	Associate Professor	Full Professor	Missing
Con	2 (18.2)	2 (18.2)	2 (18.2)	5 (46.5)	65
Neutral	0	0	1 (50.0)	1 (50.0)	3
Pro	0	3 (12.5)	8 (33.3)	13 (54.2)	74
Wrote on Polywater	2 (5.4)	5 (13.5)	11 (29.7)	19 (51.4)	142
Highly Visible Pro-Polywater	0	0	2 (33.3)	4 (66.7)	17
Science Non-Polywater	2 (12.5)	3 (18.7)	2 (12.5)	9 (56.2)	22

[a]Percentages for the nonmissing observations are given in parentheses underneath the frequencies.

Academic Rank in 1969[a]

TABLE A-18

Responses	Con	Neutral	Pro	Wrote on Polywater	Highly Visible Pro	*Science*
Agnostic or Atheist	2 (18.2)	—	3 (25.0)	5 (20.8)	1 (25.0)	2 (22.2)
Catholic	1 (9.1)	—	1 (8.3)	2 (8.3)	—	1 (11.1)
Jewish	1 (9.1)	—	—	1 (4.2)	—	—
Protestant	4 (36.4)	1 (100.0)	5 (41.7)	10 (41.7)	2 (50.0)	2 (22.2)
Other	1 (9.1)	—	3 (25.0)	4 (16.7)	1 (25.0)	2 (22.2)
Did Not Respond	2 (18.2)	—	—	2 (8.3)	—	2 (22.2)

Current Religious Affiliation

TABLE A-19

Country	Number of Scientists
Australia	9
Belguim	2
Canada	10
England	19
France	1
India	1
Israel	1
Italy	6
Scotland	2
USA	101
West Germany	2
TOTAL	159

Geographical Distribution by Country of Polywater Scientists at Time of Publication of Polywater Articles

TABLE A-20

State	Number of Scientists
California	11
Connecticut	4
Florida	1
Illinois	2
Indiana	3
Louisiana	5
Massachusetts	8
Maryland	4
Michigan	1
Missouri	11
New Hampshire	3
New Jersey	7
New Mexico	2
New York	7
North Carolina	2
North Dakota	3
Ohio	8
Pennsylvania	9
Tennessee	2
Texas	2
Washington, D.C.	5
Wisconsin	1
TOTAL	101

Geographical Distribution of U.S. Polywater Scientists at Time of Publication of Polywater Articles

TABLE A-21

Responses	Con	Neutral	Pro	Wrote on Polywater	Highly Visible Pro
Yes	5 (45.5)	1 (100.0)	3 (25.0)	9 (37.5)	2 (50.0)
No	5 (45.5)	—	9 (75.0)	14 (58.3)	2 (50.0)
I Don't Remember	1 (9.1)	—	—	1 (4.2)	—

Polywater Suggested as a Promising Topic of Research

TABLE A-22

Responses	Con	Neutral	Pro	Wrote On Polywater	Highly Visible Pro	*Science*
One	1 (11.1)	—	2 (12.5)	3 (11.5)	—	1 (14.3)
Two	5 (55.6)	1 (100.0)	3 (18.8)	9 (34.6)	1 (25.0)	3 (42.9)
Three	2 (22.2)	—	6 (37.5)	8 (30.8)	1 (25.0)	—
Four	1 (11.1)	—	3 (18.8)	4 (15.4)	1 (25.0)	1 (14.3)
Five	—	—	1 (6.3)	1 (3.8)	—	2 (28.6)
. . Eight	—	—	1 (6.3)	1 (3.8)	1 (25.0)	—

Number of Children in Family of Origin

TABLE A-23

Responses	Con	Neutral	Pro	Wrote on Polywater	Highly Visible Pro	*Science*
First-born	7 (77.8)	1 (100.0)	8 (50.0)	16 (61.5)	1 (25.0)	5 (71.4)
Second-born	1 (11.1)	—	5 (31.3)	6 (23.1)	1 (25.0)	1 (14.3)
Third-born	1 (11.1)	—	—	1 (3.8)	1 (25.0)	—
Fourth-born	—	—	1 (6.3)	1 (3.8)	—	1 (14.3)
Fifth-born	—	—	1 (6.3)	1 (3.8)	—	—
Sixth-born or Later	—	—	1 (6.3)	1 (3.8)	1 (25.0)	—

Birth Order of Scientist

Notes

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1. The layperson's externalist view of the scientist has never been more enthusiastically expressed than by Adam Smith in the *Theory of Moral Sentiments*:

> Mathematicians and natural philosophers, from their independency upon the public opinion, have little temptation to form themselves into factions and cabals, either for the support of their own reputation, or for the depression of that of their rivals. They are almost always men of the most amiable simplicity of manners, who live in good harmony with one another, are the friends of one another's reputation, enter into no intrigue in order to secure the public applause, but are pleased when their works are approved of, without being either much vexed or very angry when they are neglected. (Smith 1976, 125)

2. For a defense of the claim that the usual interpretation of Kuhn is closer to the text than is Kuhn and Merton's interpretation, see Diamond 1978, 5-7.

3. A useful general discussion of the internal-external debate can be found in Barnes 1974.

4. Gingold lists 438 total entries, but his entry 240 is identical to his entry 30. Hence he actually has only 437 total entries.

5. Several commentators on polywater have overestimated the number of scientists actively involved in the polywater episode. Bennion and Neuton, for instance, claim that "[a] conservative estimate would place the number at no less than 10^5 for those having the immediate potential of becoming involved in polywater research. Only 430 of these, or less than 0.5 percent, actually became infected, i.e., published results of their own investigations" (1976, 54). Grove, for another instance, writes that "...by 1972 some 400 scientists had published results about this exciting new substance" (Grove 1985, 133). The Bennion and Neuton claim is based on the Gingold list. The source of the Grove claim is unclear. A casual reading of the Gingold bibliography leads to an overestimate of the quantity of research on polywater because the bibliography includes letters, articles in the popular press, review essays, and articles that, although they mention polywater (albeit in a very casual way), are written on another topic.

References

American Chemical Society. (1977), *Directory of Graduate Research 1977*. Washington, DC: American Chemical Society.

Arrow, K. J. (1971), *Essays in the Theory of Risk-Bearing*. Chicago: Markham.

Barnes, B. (1974), *Scientific Knowledge and Sociological Theory*. London: Routledge & Kegan Paul.

Becker, G. S. (1981), *A Treatise on the Family*. Cambridge, MA: Harvard University Press.

Bennion, B. C. and Newton, L. A. (1976), "The Epidemology of Research on 'Anomalous Water,'" *Journal of the American Society for Information Science* 27: 53-56.

Blackmore, J. T. (1978), "Is Planck's 'Principle' True?," *British Journal for the Philosophy of Science* 29: 347-349.

Brenner, R. (1983), *History—The Human Gamble*. Chicago: University of Chicago Press.

Brush, S. G. (1976), *The Kind of Motion I Call Heat*, Book 1, in E. W. Montroll and J. L. Lebowitz (eds.), *Studies in Statistical Mechanics*, vol. 6. New York: North-Holland.

Cantor, G. N. (1975), "The Edinburgh Phrenology Debate: 1803-1823," *Annals of Science 32*: 195-218.

Carmichael, H. L. (1988), "Incentives in Academics: Why Is There Tenure?," *Journal of Political Economy* 96 (no. 3): 453-472.

Cattell, J. M. and Brimhall, D. R. (1921), "Families of American Men of Science," in *American Men of Science*. Garrison, NY: The Science Press, pp. 781-808.

Coase, R. H. (1984), "Alfred Marshall's Mother and Father," *History of Political Economy* 16: 519-527.

Cole, J. R. and Cole, S. (1973), *Social Stratification in Science*. Chicago: University of Chicago Press.

Crane, D. (1972), *Invisible Colleges*. Chicago: University of Chicago Press.

Deryagin, B. (1983), "Polywater Reviewed," *Nature* 301: 9-10.

Diamond, A. M., Jr. (1978), "Science as a Rational Enterprise," Ph.D. dissertation, (University of Chicago Department of Philosophy.)

_____ . (1980), "Age and the Acceptance of Cliometrics," *Journal of Economic History* 40: 838-841.

_____ . (1982), "Avery's 'Neurotic Reluctance,'" *Perspectives in Biology and Medicine* 26: 132-136.

_____ . (1984), "An Economic Model of the Life-Cycle Research Productivity of Scientists," *Scientometrics* 6 (no. 3): 189-196.

_____ . (1988a), "The Polywater Episode and the Appraisal of Theories," in A. Donovan, L. Laudan and R. Laudan (eds.), *Scrutinizing Science: Empirical Studies of Scientific Change*. Dordrecht: Kluwer Academic, pp. 181-98.

_____ . (1988b), "Science as a Rational Enterprise," *Theory and Decision* 24: 147-167.

Diamond, A. M., Jr. and Locay, L. (1989), "Investment in Sister's Children as Behavior Towards Risk," *Economic Inquiry* 27 (no. 4): 719-735.

_____ . (1986), "What is a Citation Worth?," *The Journal of Human Resources* 21: 200-215.

Edge, D. (1977), "The Sociology of Innovation in Modern Astronomy," *Quarterly Journal of the Royal Astronomical Society* 18: 326-339.

Eisenberg, D. (1981), "A Scientific Gold Rush," *Science* 213 (September 4): 1104-1105.

Fleming, D. (1968), "Emigre Physicists and the Biological Revolution," *Perspectives in American History* 2: 152-189.

Franks, F. (1968), "Effects of Solutes and Surfaces on the Structural Properties of Liquid Water," in L. Bolis and B. A. Pethica (eds.), *Membrane Models and Formation of Biological Membranes*. Proceedings of the 1967 meeting of the International Conference on Biological Membranes. New York: Wiley & Sons, pp. 43-51.

_____ . (1981), *Polywater*. Cambridge, MA: The M.I.T. Press.

Freeman, S. (1977), "Wage Trends as Performance Displays Productive Potential: A Model and Application to Academic Early Retirement," *Bell Journal of Economics* 8: 419-443.

Friedman, M. (1953), "The Methodology of Positive Economics," in *Essays in Positive Economics*. Chicago: University of Chicago Press, pp. 3-43.

Galton, F. (1875), *English Men of Science*. New York: Macmillan.

Garfield, E. (chair), (1980), *Science Citation Index 1979*. Philadelphia: Institute for Scientific Information.

_____ . (1983), "Computer-Aided Historiography—How ISI Uses Cluster Tracking to Monitor the 'Vital Signs' of Science," in *Essays of an Information Scientist*, vol. 5, Philadelphia: I.S.I. Press, pp. 473-83.

Gieryn, T. F. and Hirsh, R. F. (sic) (1983), "Marginality and Innovation in Science," *Social Studies of Science* 13: 87-106.

Gingold, M. P. (1973), "Anomalous Water: General Review," *Societe Chimique de France, Bulletin*, Pt. 1: 1629-1644.

_____ . (1974), "L'eau Anomale: Histoire d'un Artefact," *La Recherche* 5: 390-393.

Goffman, W. and Warren, K. S. (1980), *Scientific Information Systems and the Principle of Selectivity*. New York: Praeger.

Gould, S. J. (1981), "Ice Nine, Russian-Style," *The New York Times Book Review* 130 (no. 45,056): 7, 15.

Grove, J. W. (1985), "Rationality at Risk: Science against Pseudoscience," *Minerva* 23: 216-240.

Hagstrom, W. O. (1965), *The Scientific Community*. New York: Basic Books.

Hardy, K. R. (1974), "Social Origins of American Scientists and Scholars," *Science* 185: 497-506.

Hargens, L. L. (1988), "Scholarly Consensus and Journal Rejection Rates," *American Sociological Review* 53: 139-151.

Harris, M. and Weiss, Y. (1984), "Job Matching with Finite Horizon and Risk Aversion," *Journal of Political Economy* 92: 758-779.

Hasted, J. B. (1971), "Water and 'Polywater,'" *Contemporary Physics* 12: 133-152.

Howell, B. F. (1971), "Anomalous Water: Fact or Figment," *Journal of Chemical Education* 48: 663-667.

Hull, D. L. (1978), "Altruism in Science: A Sociobiological Model of Co-operative Behaviour Among Scientists," *Animal Behavior 26*: 685-697.

Hull, D. L., Tessner, P. D. and Diamond, A. M. (1978), "Planck's Principle," *Science* 202: 717-723.

Ito, T. and Kahn, C. (1986), "Why is There Tenure?" Discussion Paper No. 228, Center for Economic Research, University of Minnesota.

Jacques Cattell Press (ed.), (1979), *American Men and Women of Science*, 14th ed. New York: Bowker.

Kahn, C. and Huberman, G. (1988), "Two-sided Uncertainty and 'Up-or-Out' Contracts," *Journal of Labor Economics* 6: 423-444.

Klotz, I. M. (1980), "The N-Ray Affair," *Scientific American 242*: 168-175.

Kolata, G. (1984), "Surprise Proof of an Old Conjecture," *Science* 225: 1006-1007.

_____. (1986), "What Does It Mean to Be Random?" *Science* 231: 1068-1070.

Kollman, P. A. and Allen, L. C. (1972), "The Theory of the Hydrogen Bond," *Chemical Reviews* 72: 283-303.

Kuhn, T. S. (1970), *The Structure of Scientific Revolutions*, 2d ed. Chicago: University of Chicago Press.

Langmuir, I. (1989), "Pathological Science," *Physics Today 42*: 36-48.

Laudan, L. (1977), *Progress and Its Problems*. Berkeley and Los Angeles: University of California Press.

Long, J. S. (1978), "Productivity and Academic Position in the Scientific Career," *American Sociological Review* 43: 889-908.

MacRoberts, M. H. and MacRoberts, B. R. (1984), "The Negational Reference: or the Art of Dissembling," *Social Studies of Science* 14: 91-94.

McCarty, M. (1980), "Reminiscences of the Early Days of Transformation," *Annual Review of Genetics* 14: 1-15.

McClelland, D. (1976), *The Achieving Society*. New York: Irvington Press.

McCloskey, D. N. (1985), *The Rhetoric of Economics*. Madison: University of Wisconsin Press.

Merton, R. K. ([1936] 1970), *Science, Technology and Society in Seventeenth-Century England*. New York: Fertig.

Metzger, N. (1970), "Polywater Boils," *Chemical and Engineering News* (November 9): 9.

Morin, R. A. and Suarez, A. F. (1983), "Risk Aversion Revisited," *The Journal of Finance* 38: 1201-1216.

Mulkay, M. J., Gilbert, G. N. and Woolgar, S. (1975), "Problem Areas and Research Networks in Science," *Sociology* 9: 187-203.

Mullins, N. C.; Hargens, L. L.; Hecht, P. K.; and Kick, E. L. (1977), "The Group Structure of Cocitation Clusters: A Comparative Study," *American Sociological Review* 42: 552-556.

Munevar, G. (1981), *Radical Knowledge*. Indianapolis: Hackett.

Pethica, B. A. (1982), "Review of *Polywater*," *Journal of Colloid and Interface Science* 88: 607.

Planck, M. (1949), *Scientific Autobiography and Other Papers*. New York: Philosophical Library, pp. 33-34.

Pledge, H. T. ([1939] 1959), *Science Since 1500*. New York: Harper & Brothers.

Pollock, G. L. (Project Editor) (1984), *Chemical Research Faculties: An International Directory*. Washington, DC: American Chemistry Society.

Rubin, P. H. and Paul II, C. W. (1979), "An Evolutionary Model of the Taste for Risk," *Economic Inquiry* 17: 585-595.

Sabin, J. R.; Harris, R. E.; Archibald, T. W.; Kollman, P. A.; Allen, L. C. (1970), *Ab Initio* MO-SCF Calculation on a Model of Anomalous Water, *Theoretica Chimica Acta* (Berl.) 18: 235-238.

Siow, A. (1979), "A Model of Academic Tenure." Preliminary draft presented at Applications of Economics Workshop, The University of Chicago.

Small, H. G. and Griffith, B. C. (1974), "The Structure of Scientific Literatures, I: Identifying and Graphing Specialties," *Science Studies* 4: 17-40.

Smith, A. (1976), *The Theory of Moral Sentiments*. London: Oxford University Press.

Stigler, G. J. (1982a), "The Literature of Economics: The Case of the Kinked Oligopoly Demand Curve," in *The Economist as Preacher and Other Essays*. Chicago: University of Chicago Press, pp. 223-43.

_____. (1982b), "The Scientific Uses of Scientific Biography, with Special Reference to J. S. Mill," in *The Economist as Preacher and Other Essays*. Chicago: University of Chicago Press, pp. 86-97.

_____. (1983), "Nobel Lecture: The Process and Progress of Economics," *Journal of Political Economy* 91: 529-545.

_____. (1986), "George J. Stigler," in W. Breit and R. W. Spencer, (eds.), *Lives of the Laureates: Seven Nobel Economists*. Cambridge, MA: MIT Press, pp. 93-111.

Studer, K. E. and Chubin, D. E. (1980), *The Cancer Mission: Social Contexts of Biomedical Research*. Sage Library of Social Research 103. Beverly Hills: Sage.

Telser, L. G. (1974), "Advertising and the Consumer," in Y. Brozen (ed.), *Advertising and Society*. New York: New York University Press, 25-42.

Toulmin, S. (1972), *Human Understanding*. Princeton: Princeton University Press.

Wallis, R. (ed.), (1979), *On the Margins of Science: The Social Construction of Rejected Knowledge*. Sociological Review Monograph 27. Keele, Staffordshire: University of Keele.

Watson, J. D. (1968), *The Double Helix*. New York: The New American Library.

Weber, M. ([1930] 1958), *The Protestant Ethic and the Spirit of Capitalism*. New York: Charles Scribner's Sons.

Weiss, Y. and Lillard, L. (1982), "Output Variability, Academic Labor Contracts and Waiting Time for Promotion," in R. G. Ehrenberg (ed.), *Research in Labor Economics 5*: 157-188.

Zajonc, R. B. and Markus, G. B. (1975), "Birth Order and Intellectual Development," *Psychological Review* 82: 74-88.

Zuckerman, H. (1977), "Deviant Behavior and Social Control in Science," in Edward Sagarin (ed.), *Deviance and Social Change*. London: Sage, pp. 87-138.

_____ . (1979), "Theory Choice and Problem Choice in Science," in J. Gaston (ed.), *Sociological Perspectives on Science*. San Francisco: Jossey-Bass, pp. 65-95.