

ARTHUR M. DIAMOND, JR.

THE POLYWATER EPISODE AND THE APPRAISAL OF
THEORIES*

1. BACKGROUND AND DATA

Instead of only examining how often a thesis about scientific change is confirmed in cases of scientific advance, it would be better to examine both how often the thesis is true in cases of scientific advances *and* how often it is true in cases of scientific mistakes. (For the purpose of this paper, a 'mistake' is defined as a scientific claim that is initially accepted by a sizable proportion of specialists, but is shown to be false before being integrated into the prevailing interpretation of the research area in question.) One could then conclude that the higher the success to failure ratio (out of the total number of successful and mistaken episodes in which the thesis was true), the greater the warrant for affirming the thesis as a normative standard for how good science should be done. The main criterion for choosing an episode for this study was that the episode be an exemplary mistake. For the purposes of this study, the polywater episode has several advantages over other mistakes that could be studied: (a) the episode occurred recently enough for biographical and citation data to be available, and (b) the number of identifiable scientists involved was larger than for most other mistakes that could be studied.

Some current commentators are ready to label the polywater episode as mistaken *ex ante* (see Eisenberg, 1981; Franks, 1981, pp. 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, pp. 111, 112). But other scientists (e.g., Deryagin, 1983; Gould, 1981, p. 15; and Pethica, 1982) seem to view polywater more generously as one of those "mistakes of good men earnestly seeking the truth" (Metzger, 1970, p. 9).

Franks' *Polywater* (1981) is by far the most extensive account of the episode, but other useful accounts are also available (Everett, Haynes and McElroy, 1971; Hasted, 1971; Howell, 1971; Gingold, 1973; Gingold,

1974; Klotz, 1986, pp. 67–95). Anomalous water was first discovered in 1961 by a Russian scientist named Fedyakin who found that in very small capillaries he could condense a form of water that has a lower freezing temperature and a higher boiling temperature than ordinary water. The discovery was taken up by the prestigious Russian scientist Deryagin¹ 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 anomalous water began in 1969. The term 'polywater' was introduced by Lippincott and his co-authors to refer to a new polymeric form of water that they claimed exhibited the anomalous phenomena. In what follows we sometimes adopt the convention of subsequent writers by using the word 'polywater' more broadly to refer to anomalous water regardless of whether it is due to impurities in ordinary water or to a new polymeric structure. By 1973, when Deryagin admitted that the polywater phenomena were probably due to impurities in ordinary water, over 400 publications had appeared that referred to the subject (see Bennion and Neuton, 1976).

I began my study of the polywater episode by collecting biographical and career information for a sample of scientists from the West who wrote on polywater and a comparable sample of scientists from the West who did not write on polywater (the control group). Gingold's (1973) nearly exhaustive bibliography on polywater was the basic source for the sample of scientists who wrote on polywater. For more details on the sample selection criteria and on the sources of biographical and career information, consult Diamond, (1987a, b). After the elimination of all excluded entries, the final sample of research publications on polywater consisted of 112 entries.

These remaining entries were examined in order to judge whether their author's attitude toward polywater was pro, con or neutral. An article was judged to be pro-polywater if on balance it seemed to support the position that the anomalous phenomena were due to some different structure of water, while an article was judged to be 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 to be neutral. For the polywater case, I evaluated 66 entries as pro, 4 entries as neutral and 42 entries as con.

A scientist was judged as having been pro-polywater if he had ever authored or co-authored a research article that was judged to be

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 he 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 he had never authored either a pro or con research article, but had authored an article that was judged to have been neutral.

2. RELEVANCE OF POLYWATER TO THE THESIS CLUSTERS

Which of the canonical set of theses is the polywater episode most relevant to? The episode has some of the sociological characteristics that have been associated with revolutions, so one might be tempted to focus on the theses that refer to revolutions. One revolution-like feature of the episode, for instance, was the enormous popular controversy that was generated.

For the purposes of this volume, a 'revolution' has been defined as "the replacement, abrupt or gradual, of one set of guiding assumptions by another". In the polywater episode most of the participating scientists accepted one or the other of two mutually inconsistent guiding assumptions. The first was that the polywater phenomena were due to some new structure of water, while the second was that the polywater phenomena were due to some impurity in ordinary water. Some of those who accepted the first guiding assumption attempted to develop theories of a new structure of water that would explain the properties of polywater in a manner consistent with the laws of physics and chemistry. Some also attempted experimentally to produce polywater under conditions in which every precaution against contamination had been taken.

Some of those who accepted the guiding assumption that the polywater phenomena were due to impurities in ordinary water developed theories concerning which impurities were producing the phenomena. Some of them also attempted to show how mixtures of the suggested impurities with water would produce the phenomena. Finally, some of them attempted to demonstrate that such impurities would in fact be produced by following the experimental procedures of those who accepted the competing guiding assumption.

Although some aspects of the polywater episode can be interpreted in terms of divergent guiding assumptions, the scientists on both sides of

the controversy shared, from beginning to end, far more assumptions than did scientists in many of the examples of revolutions that are traditionally discussed. We will, therefore, consider first, and focus most of our attention upon, the theses on the appraisal of theories. In conclusion we will consider the relevance of the polywater episode to one thesis from the set of theses on scientific revolutions, viz., the thesis on age and the acceptance of theories.

Since most of our attention will be focused on theses concerning theory appraisal, we should note that we reject a common view that theory appraisal constitutes a phase of the scientific decision-making process distinct from problem choice. Ample evidence exists that appraisal is an on-going process, intimately involved in a scientist's choice of problems for research.

3. THESIS (T2.8)

Thesis (T2.8) states that "the appraisal of a theory is relative to prevailing doctrines of theory assessment and to rival theories in the field". One question of interpretation that must be answered before the thesis can be evaluated is whether the thesis applies only to the ultimate appraisal or also to the on-going appraisal that occurs while the issue is still in doubt. Here we focus mainly on the ultimate appraisal. Ultimately the theories that proposed new structures for polywater were rejected in favor of the theories that proposed that the phenomena were due to impurities.

In what follows we also give disproportionate attention to the appraisals made by the 'elite' where that term is operationally defined as those scientists who either play a substantial role in the accounts of the episode given by authorities such as Franks and Klotz or else who were important by some independent, objective measure such as by the Institute for Scientific Information's co-citation clusters. Much work, largely in the sociology of science, could be cited to justify the decision to focus on the elite (cf. Cole and Cole (1973), *passim*; and Mulkey (1980), pp. 28-42).

Using citations made during 1970 and 1971, the cluster method identifies 11 distinct articles as having been important.² Of the 11 (all published in either 1969 or 1970) 8 are pro polywater and 3 are con. Of the 8 pro articles, 4 are focused on reporting experimental results, 2 are

focused on presenting new theoretical structures of water and 2 incorporate both experimental results and a significant discussion of new theoretical structures. Of the 3 con articles, all are focused on reporting experimental results.

The theories that proposed new structures of water in order to explain the polywater phenomena can be divided into two broad categories. One group consisted of structures derived largely in analogy with other known chemical structures. Allen and Kollman characterize this first group of theories by saying that the theories employ “intuitive physical arguments” (1971a, p. 461). The second, and much smaller, group consisted of structures derived largely from computer models based on quantum mechanics. The former group of theories can be called more ‘casual’ than the latter both in the sense that less time and effort went into the construction of such theories, and also in the sense that such theories tended to be given less probative weight. Referring to this group of theories, Pimental and McClellan claim that “‘back-of-the-envelope’ proposals for novel H-bonded structures to account for the properties became a popular luncheon sport” (1971, p. 367). First-hand testimony to the casualness of these theories is provided in the admission of Jerry Donohue (not to be confused with the F. J. Donahoe who proposed in *Nature* that polywater might end life on earth) that his own theory was “concocted one evening while watching TV, between commercials, with tongue in cheek”.³

For the most part, those scientists appraising the theories of a new structure of water did not make much effort to distinguish the various theoretical accounts from one another. Some rough evidence of this is that only one purely theoretical paper, that of Allen and Kollman, was heavily enough cited to qualify for the 10-article polywater citation cluster for 1971. One reason for this might have been that the constraints imposed by the known regularities of chemistry (such as Gibb’s law) combined with the constraints imposed by the then-current observations may have been thought to underdetermine theory choice in the straightforward sense that more than one theory might be consistent with the corpus of chemical knowledge and the specific evidence on the polywater phenomena. Another possible reason might have been that although relative assessments would have been possible, the greater part of the interested scientific community did not consider such assessments worth the time and effort required until the broader issue of the presence of impurities was resolved.

Allen and Kollman, at the June 1970 Lehigh symposium, held near the end of the polywater episode, made a systematic attempt to assess the relative merits of several of the most prominent pro theories on the basis of quantum mechanical calculations of their stabilities (1971a). The authors found that their own earlier structure is clearly superior to the others. *Then*, in a companion article in the same issue of the same journal (1971b), Allen and Kollman report that on the basis of new calculations, again making use of quantum mechanics, they no longer believe that their own earlier model is stable. They finally conclude that, since their theory is the best one available and since their theory is unstable, polywater does not exist. Near the end of this article, the authors express the fear that their complete reversal on the stability issue “will cause many chemists to laugh and some to discount the efficacy of theoretical chemistry” (1971b, p. 430).

Another noteworthy reversal by a pro theorist in the small group using calculations from quantum mechanics was that of O’Konski. O’Konski had claimed that his theory was superior to various particular alternative theories in that it accounted for (1) how polywater could be in equilibrium with ordinary water and (2) how polywater could be stable. (O’Konski, 1970, pp. 1089–1090) He argued that polymeric theories are superior to allotropic theories because they account for (1), while his particular polymeric theory is superior to other polymeric theories because it accounts for (2).

The following year, using sophisticated (and at the time, computationally expensive) techniques, O’Konski concluded that his own theory cannot account for (2). On the basis of this result “together with”, reports of numerous impurities found in samples of anomalous water O’Konski reached a negative appraisal of his own theory and expressed doubts that any polymer theory would turn out to explain the phenomena (O’Konski, 1971, p. 551).

It is not clear whether the theoreticians changed their stand because they did their calculations more carefully, as is implied by some accounts, or rather, because they saw how the experimental work was going and changed the interpretation of their results so as to be in line with the expected scientific outcome. In their thorough, 30-page, 1971 survey article on polywater, Everett *et al.* (1970) devote only slightly more than a page to the theories of a new structure for water. They conclude that:

It is now clear that present theoretical techniques are quite inadequate to assess reliably the stability, relative to monomeric water, of postulated polymer species. Whether polywater is eventually proved to exist or not there is no doubt that by an appropriate selection of models and parameters, a theoretical basis could be found for whichever turns out to be the experimentally established situation: theoretical calculations cannot have any reliable predictive value in the present case (p. 301).

Lest these strong claims be discounted because their source was a group of con scientists, it should be noted that one of the co-authors of one of the main pro theoretical articles has retrospectively remarked of the evidence from the theoretical calculation that "one could weight the evidence to either support or not support the existence of polywater". He goes on to say: "Early on we chose to weight the positive evidence more, since one could construct such a seemingly self-consistent picture of the phenomena" (Kollman as quoted by Franks (1981), pp. 94–95).

Apparently the plethora of theories concerning which impurities caused polywater did not prevent the formation of a consensus that the phenomena were due to impurities of some sort. A turning point in the development of a consensus (see Franks (1981), pp. 103–104) was the paper by Lippincott presented at the Lehigh symposium on polywater in June 1970, in which he reported that he had been able to reproduce the polywater spectra from ordinary water contaminated with impurities, but was unable to reproduce it when the impurities were rigorously excluded. Lippincott's reversal carried more weight than papers by other researchers because he had the reputation of being a careful experimentalist (see Franks (1981), p. 68) and because his early research in favor of the existence of a new structure of water had been a key in the growth of polywater research in the West (see Klotz (1986), p. 85).

Although the Lippincott reversal may have been a turning point, the accumulation of additional evidence of impurities was necessary before the consensus was complete. In order to understand why the theories that proposed a new structure ultimately received a negative appraisal, it may be worthwhile to focus on the three con articles identified as most influential by the Institute for Scientific Information's co-citation cluster method. The three con articles identified by the cluster method as important all appeared in 1970 and were authored by Everett, Haynes and McElroy; Rabideau and Florin; and Rousseau and Porto.

One of the striking features of the three key con articles is that they propose what appear to be mutually inconsistent impurity theories of the polywater phenomenon. Rousseau and Porto conclude that “in this complex mixture of impurities only trace amounts of silicon have been found, ruling out the speculation that the anomalous properties result from a silica gel” (p. 1718). Everett *et al.* conclude to the contrary that “as the properties outlined above appear to be consistent with those of a silicic acid sol, it is not necessary on the basis of the above evidence to seek an explanation in terms of ‘polywater’” (p. 1035).

Everett *et al.* claim to have shown that spectroscopic and other evidence indicates that the polywater phenomena are what one would expect from a mixture of ordinary water and silica. They do not, however, present the details of the precautions they took to avoid contamination by other impurities (such as those reported by Rousseau and Porto) nor do they report spectroscopic or other tests indicating that such impurities were absent.

At one point in their paper (p. 1036) they refer explicitly (and positively) to Occam’s razor, a reference that is instructive about the structure of their argument. They do not claim to have shown that other explanations (such as the polymer theories) are inconsistent with the phenomena. Rather they claim that we should not assert a new structure unless the phenomena cannot be explained by mixtures of already known chemical structures. Further, they appear to be totally unconcerned with appraising their silica impurity theory relative to other impurity theories. In fact, near the conclusion of the paper they offer conciliatory remarks to those accepting other impurity theories (p. 1037).

Consider finally the paper by Rabideau and Florin in which they find that the main impurities are sodium and boron. Everett *et al.* had also found significant quantities of sodium, but only a trace of boron – a trace that only appeared using one of the four analytical techniques reported. Rabideau and Florin mention the Everett and Porto study (which had appeared a few months earlier) only as a source of a potential mechanism for the contamination, but they do not speculate on why the earlier study had not turned up significant quantities of boron. Perhaps even more surprising is the absence of any mention, let alone appraisal, of the rival impurity theory that emphasized silicon as the key impurity. Instead, the authors only mention in passing that “chlorine and silicon were found, but these were small weight fractions of the residue” (p. 50).

Some of the pro-polywater scientists argued that the diversity of

POLYWATER

impurities theories undermined the credibility of the guiding assumption that the phenomena were caused by impurities. But this would be only to the extent that the characteristics of the substance obtained from different samples by the pro-polywater experimentalists had been consistent. Although there was considerable consistency in some basic qualitative characteristics, such as high viscosity, other characteristics varied considerably (see Howell (1971), p. 666; Hasted (1971) p. 1). The pro-polywater experimentalists explained the diversity of reported characteristics in two ways: (1) some experimentalists may have insufficiently guarded against impurities, and (2) all samples contain mixtures of polywater and ordinary water in differing relative proportions. On the other hand, the diversity of characteristics reported may explain why the conventional experimentalists frequently did not appraise their theories: what impurity was important relative to other theories of what impurity was important. The inconsistency in the characteristics reported from sample to sample was interpreted by many as indicating that different impurities were present in different samples. Apparently those characteristics that were consistently reported from sample to sample were sufficient to distinguish between several different impurity explanations. For example, Everett *et al.* (in a useful summary article published in the year following the article we have just discussed) summarize the physical properties of anomalous water (1971, pp. 295–297) and suggest that

Many of the properties of 'anomalous water' have now been shown not to be specific to dilute hydrosols of a variety of materials: it seems highly probable that many 'anomalous waters' described in the literature represent but a few of the possible forms of this phenomenon, and deliberate attempts to prepare anomalous water in contact with other solids have in some cases been successful (pp. 298–299).

The initial disagreement among scientists about whether to give a positive or a negative appraisal to the theories of a new structure of water occurred largely because standard precautions against contamination turned out not to be sufficient in the polywater case and because quantities of the anomalous water produced were so small that the usual techniques for detecting impurities were taxed to the limits of their sensitivity. Many, though perhaps not all, of the pro-polywater experimentalists had taken considerable precautions to avoid contamination of their water with impurities. Very early on, for instance, Derya substituted quartz capillaries for Fedyaikin's glass capillaries because quartz was known to be less susceptible than glass to surface contamination. In addition, rinsing the capillaries with distilled water was routine. Other precautions against contamination varied from study to study.

were often quite elaborate. Finally, however, the cleaning techniques that were previously considered sufficient were judged *not* to be sufficient in the polywater case. Rousseau notes that “new cleaning techniques must be devised to eliminate all surface contaminants” (p. 1718).

In addition to the unusual difficulty of preventing contamination, a second reason for the initial disagreement over how to appraise the polywater theories was the difficulty in overcoming the limitations of standard analytic techniques. On this issue Franks (1981) notes that:

With never more than few micrograms available, ingenious analytical methods had to be devised if definitive and quantitative results were to be obtained that would themselves stand up to scrutiny. The instrumentation required for much of this work was of the most advanced kind, only available at a few centers in the United States. In the end the analysts carried the day, because they showed to their own and most other scientists' satisfaction, that polywater really contained little water . . . (p. 87).

If we are to assign a date to the time by which the scientific community had reached its ultimate negative appraisal of the theories of a new structure of water, no better candidate can be found than 5 March 1971 when “Polywater Drains Away” appeared in the unsigned “News and Views” column of *Nature*. Howell (p. 666) calls this editorial an ‘obituary’ of polywater, although in tone it reads more like a self-satisfied homily. Deryagin, however, was not quite ready to concede his theory’s demise. Five months later, in the same journal, in a note co-authored with Churaev, Deryagin attempted to respond to the claim that the phenomena were due to impurities. If due to impurities, he argued, then liquid water introduced into the capillaries should absorb the same impurities as the vapor condensate associated with the polywater phenomena. He found that it did not, from which he concluded that the impurities theories were “at odds with the facts” (1971, p. 131).

After the passage of two more years, however, Deryagin conceded (again in a note in *Nature* co-authored by Churaev) that the polywater phenomena were due to impurities in ordinary water. In the end Deryagin concluded that something in the process of vapor condensation increased the likelihood of dissolving impurities from the surface of the capillaries. He found that in samples demonstrably free of impurities, the polywater phenomena did not occur and in samples where the polywater phenomena did occur, impurities could always be detected. In justification of his earlier scepticism toward the impurities theories, he again mentioned the lack of agreement about what the impurities

were and what the mechanism was for producing the contamination. In his final sentence he suggested that the mechanism is a subject requiring further research, a 'requirement' that apparently has never been filled (1973, pp. 430–431).

The temporary disagreement over the appraisal of the polywater theories consisted, not so much in disagreement over the broadly defined doctrines of theory assessment, but rather in matters of judgement over two more concrete issues. The first was the extent to which the various theories had satisfied the prevailing standards, while the second was the extent to which the standards had to be met before the theories should be appraised positively. Based on the experiments that had been performed, the pro-polywater scientists (for a time) attached a higher probability to the thesis that the polywater phenomena could be produced without impurities. Also, the pro-polywater scientists seemed to believe that the level of probability required to justify a positive appraisal was lower than the level that the con scientists believed was required. Eventually, with the accumulation of sufficient sophisticated evidence of impurities, the appraisals of the scientific community converged on the conclusion that the polywater phenomena were due to impurities rather than to a new structure of water.

All of the scientists on both sides of the polywater issue accepted, in broad terms, the prevailing standards for theory appraisal. In particular they agreed that the theories should be consistent with the laws of physics and the current knowledge of molecular chemistry. They agreed that empirical support for the theories would require that the polywater be produced under conditions meticulously excluding impurities and that application of the techniques of analytic chemistry should show no impurities sufficient to explain the phenomena. I conclude that, in broad terms, (T2.8) is consistent with what occurred in the polywater episode.

4. THESES (T2.5) AND (T2.6)

Now that we have a general account of theory appraisal in the polywater episode before us, we are able to succinctly evaluate (T2.5) and (T2.6), which treat the role of empirical evidence in theory appraisal. Thesis (T2.5) states that "the appraisal of a theory is based on phenomena which can be detected and measured without using assumptions drawn from the theory under evaluation". In the case of polywater this thesis is clearly true. The phenomena were observed first, the theories came

later. The original discoverer of the phenomena was Fedyakin, an experimentalist who was interested in the behavior of water condensed in small capillaries. Unfortunately, as Klotz notes (p. 79), Fedyakin is an elusive figure. But from Deryagin's account we can infer that Fedyakin did not have a new structure of water in mind when he performed his initial experiment. Deryagin reports (1969, p. 64) that: "Fedyakin had set himself the task of studying how the properties of liquids change when they are closed in extremely narrow glass capillaries".

The basic phenomena on which the theory was based consisted of the creation under certain experimental conditions of water that had certain properties. The basic apparatus had not been designed with any theory of polywater in mind (although it was refined during the course of the episode so as to reduce the likelihood of contamination, increase the quantity produced and decrease the time required to produce it). In addition, the equipment used to determine the physical and chemical properties of the substance (most notably spectroscopy) was not designed specifically for use in the polywater episode and was used by those on both sides of the fundamental issue of whether the polywater phenomena were due to a new structure of water.

Thesis (T2.6) states that "the appraisal of a theory is usually based on only a very few experiments, even when those experiments become the grounds for abandoning the theory". Two related, yet distinguishable types of experiments were performed during the polywater episode. One was designed to see whether polywater could be created using techniques that scrupulously avoided contamination by other substances. The second was designed to see if spectra and physical properties corresponding to those found in the first kind of experiment could be replicated using mixtures of known substances with ordinary water. Neither experiment could be interpreted as a crucial test because the results of both (no matter how they turned out) could be interpreted in ways consistent with either a pro or con position on polywater. I conclude that (T2.6) is not consistent with what occurred in the polywater episode.

5. THESIS (GA4.5)

Thesis (GA4.5) states that "during a change in guiding assumptions, younger scientists are the first to shift and then conversion proceeds rapidly until only a few elderly holdouts remain". The view that age matters in acceptance of new theories is not unusual. Perhaps the most

extreme and oft-quoted articulation of that view was given by Max Planck: “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”⁴. Kuhn (1962, p. 151) explains Planck’s principle by noting the costs of intellectual retooling when a new theory is adopted. Those who are older will, *ceteris paribus*, have more human capital invested in the old theories and, hence, will have more to lose by the adoption of a new theory.

We are interested here in learning the effect of age on the acceptance of polywater. We have data on the year of receipt of the PhD for more of the scientists involved than we have data on year of birth. So in addition to looking at the effect of biological age on the acceptance of polywater, we also look at the effect of professional age (which is equivalent to the economist’s operational definition of “experience”). Even apart from data considerations, professional age may be the more appropriate concept if we accept the ‘costs of retooling’ explanation for (GA4.5).

Since the dependent variable is equal to one when the scientist accepted polywater and equal to zero when he did not, an ordinary least squares regression would have several undesirable features, inefficiency most notable among them (see Judge *et al.* (1980), pp. 586–587). So instead of ordinary least squares a binary logit is estimated.⁵

Polywater scientists differ in the extent to which their papers were mistaken. According to Franks (1981, p. 72), Lippincott’s *Science* paper “is one of the rare examples in the chronicle of polywater where a group of authors took a definite stand based on their reading of the experimental evidence”. Those who added more qualifications, it can be argued, turned out to have been less mistaken when polywater was shown to be due to impurities. Stigler (1982, p. 234) persuasively argues, however, that:

Unless an author explicitly sets out to refute a theory, one should characterize his attitude toward that theory as favorable, or at worst neutral, if he actually refers to the theory. For he is reviving its currency and advertising its existence.

Since the effects of professional age on the acceptance of polywater might differ depending on the form of ‘acceptance’, we estimate separate logit regressions using three different definitions of what it means to have accepted polywater.

The relationship between a scientist’s age and his position on polywa-

TABLE I.

Logit estimates of the effect of biological and professional age on the probability that a scientist was 'pro' on polywater

Definition of 'pro-polywater' used	Regression number					
	(1)	(2)	(3)	(4)	(5)	(6)
	1=highly visible pro; 0=control group or wrote con		1=wrote on polywater; 0=contro- group		1=wrote pro; 0=control group or wrote con or wrote neutral	
Biological age in 1968	-0.036 (0.87)	—	-0.005 (0.04)	—	-0.008 (0.14)	—
Professional age in 1968 (i.e., 1968 minus year of Ph.D.)	—	0.011 (0.11)	—	0.003 (0.02)	—	0.019 (0.79)
Constant	-0.083 (0.00)	-1.674 (16.85)	1.403 (1.94)	1.111 (14.61)	0.198 (0.05)	-0.428 (2.79)
Sample size	55	71	87	107	87	107
No. of obs. where pro poly. = 1	11	12	67	81	41	46
No. of obs. where pro poly. = 0	44	59	20	26	46	61
- 2 log likelihood	55.04	44.50	93.81	118.66	120.18	146.22

The absolute value of the asymptotic *t*-statistic is reported in parenthesis. A value of 1.96 or more would indicate statistical significance at the usually used 0.05 level. As a measure of goodness-of-fit for the logit regression as a whole, we follow the convention of reporting minus two times the log likelihood. The statistic cannot easily be given a straightforward interpretation for the uninitiated, but we should at least note that, *ceteris paribus*, the higher the statistic's value, the better the fit.

ter is explored in the logit regressions presented in Table I. The dependent variable (i.e., the variable to be explained) has a value of 1 if the scientist is 'pro' polywater and a value of 0 if the scientist is not 'pro'. We will define the various senses of 'pro' shortly. Following the convention in the statistics literature, a variable that has only two possible values is called a 'dummy' variable.

In the sentences that follow 'favorable' will mean believing that the polywater phenomena are due to a new structure of water, while 'unfavorable' will mean believing that the polywater phenomena are

due to impurities in ordinary water. In logit regressions 1 and 2 the pro-polywater dummy variable was equal to 1 if the scientist had written a highly visible favorable article on polywater and was equal to zero if the scientist either had not written on polywater or else had written an unfavorable article. Scientists who had written neutral or less visible favorable articles were excluded. In logit regressions 3 and 4 the pro-polywater dummy variable was equal to 1 if the scientist had written any research article on polywater, whether favorable, unfavorable, or neutral. The dummy was equal to 0 if the scientist had not written on polywater. In logit regressions 5 and 6 the pro-polywater dummy variable was equal to 1 if the scientist had written a favorable article (whether highly visible or not) and was equal to 0 if the scientist had either not written on polywater or had written a neutral or unfavorable article.

Regressions 1, 3, and 5 use biological age as the measure of age, while regressions 2, 4, and 6 use professional age. A negative sign on the coefficients on the age variables would indicate that older scientists are less likely to be 'pro' polywater while a positive sign would indicate that older scientists are more likely to be 'pro'. The signs of the biological age coefficients in regressions 1, 3 and 5 are all negative, while the signs of the professional age coefficients are all positive. However the asymptotic *t*-statistics (given in parentheses underneath each coefficient) are never even close to being statistically significant for any of the age variables in any of the regressions. The appropriate conclusion is that neither biological age nor professional age seems to have mattered in predicting whether a scientist would be 'pro' polywater in any of the three senses of 'pro'. In other regressions estimated, but not reported in the table, this conclusion is further confirmed when scientists' pre-polywater quality is controlled for by including as a variable in the regression the number of citations that a scientist received in 1968. I conclude that (GA4.5) is not consistent with what occurred in the polywater episode.

NOTES

*The research reported in this paper was partially supported by a grant from the National Science Foundation and a grant-in-aid from the College of Social and Behavioral Sciences of the Ohio State University. I am grateful to Gregory Armotrading, Mark Chapinski, Jack Julian, Lisa Knazek, Bret Mizer, Steven Oetken, Maureen Ogle, Chris-

topher Smith, James Thomas, William Tisch, Ann Wertz and Kathryn Williams for able research assistance. Henry Small of the Institute for Scientific Information graciously provided me with 1970 and 1971 co-citation cluster data. Irving Klotz suggested to me the appropriateness of the polywater episode as an example of a mistake. I have had useful conversations on the subject of this paper with my colleague Gonzalo Munevar. I also appreciate comments on an earlier version by the participants of the VPI conference and by an anonymous referee. Finally, I am grateful to many of the participants in the polywater episode for sharing with me their accounts of what happened.

¹We have adopted a uniform transliteration for Deryagin's name. The most common alternative in the literature on polywater is 'Derjaguin'. When we quote authors who use an alternative transliteration, we change the spelling to 'Deryagin' without adding 'sic' or other scholarly distractions.

²For more details on the co-citation cluster method and illustrations of its use, see, e.g., Garfield (1983) and Small and Griffith (1974). For more details on the particular articles in the polywater clusters, including some biographical and career data on the authors, see Diamond, (1987a).

³As quoted in Franks, p. 95. Of course, a cynic might note that after the ultimate negative appraisal of polywater, those who had written theories of a new structure might have an interest in downplaying the resources they had invested in the mistaken research.

⁴Planck (1949), pp. 33–34. Nearly all those who quote Planck's principle accept it as true. For extensive bibliographies listing sources that have referred to Planck's principle, see footnotes 1 and 3 in Diamond (1980) and, for more recent sources, Diamond (1987b). For systematic tests of the importance of a scientist's age as a determinant of theory acceptance see: 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.

⁵A justification for using the logit cumulative distribution function can be found in: Judge *et al.* (1980) p. 591. The estimates of the constants may be subject to choice-based sample bias as discussed by Manski and Lerman. But since the values of the constants are irrelevant to the test of (GA4.5), the potential problem need not concern us.

*Department of Economics,
University of Nebraska at Omaha,
Omaha, NB 68182,
U.S.A.*

REFERENCES

- Allen, Leland C. and Kollman, Peter A. (1970), 'A Theory of Anomalous Water', *Science* **167**, 1443–1454.
 Allen, Leland C. and Kollman, Peter A. (1971a), 'Comparison of Theoretical Models for Anomalous Water', *J. Colloid and Interface Science* **36**, 461–468.
 Allen, Leland C. and Kollman, Peter A. (1971b), 'What Can Theory Say about the

- Existence and Properties of Anomalous Water?', *J. Colloid and Interface Science* **36**, 469–482.
- Bennion, Bruce C. and Neuton, Laurence A. (1976), 'The Epidemiology of Research on "Anomalous Water"', *J. Amer. Society for Information Science* **27**, 53–56.
- Cole, Jonathan R. and Cole, Stephen (1973), *Social Stratification in Science*, Chicago: University of Chicago Press.
- Deryagin, Boris (1969), 'The Equivocal Standard', *Saturday Review* (Sept. 6, 1969), pp. 54–55.
- Deryagin, Boris (1970), 'Superdense Water', *Scientific American* **223**, 52–71.
- Deryagin, Boris (1983), 'Polywater Reviewed', *Nature* **301**, 9–10.
- Deryagin, Boris and Churaev, N. (1971), 'Anomalous Water', *Nature* **232**, 131.
- Deryagin, Boris and Churaev, N. (1973), 'Nature of "Anomalous Water"', *Nature* **244**, 430–431.
- Diamond, Arthur M., Jr. (1980), 'Age and the Acceptance of Cliometrics', *J. Economic History* **40**, 838–841.
- Diamond, Arthur M., Jr. (1987a), 'The Career Consequences for a Scientist of a Mistaken Research Project', (preliminary draft).
- Diamond, Arthur M., Jr. (1987b), 'The Determinants of a Scientist's Choice of Research Projects', (preliminary draft).
- Eisenberg, David (1981), 'A Scientific Gold Rush', *Science* **213**, 1104–1105.
- Everett, D. H., Haynes, J. M. and McElroy, P. J., (1970), 'Celligative Properties of Anomalous Water', *Nature* **226**, 1033–1037.
- Everett, D. H., Haynes, J. M. and McElroy, P. J. (1971), 'The Story of Anomalous Water', *Science Progress, (Oxford)* **59**, 279–308.
- Franks, Felix (1981), *Polywater*, Cambridge, Mass.: MIT Press.
- Garfield, Eugene (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: ISI Press. pp. 473–483.
- Gieryn, Thomas F. and Hirsh, Richard F. (1983), 'Marginality and Innovation in Science', *Social Studies of Science* **13**, 87–106.
- Gingold, Marcel Pierre (1973), 'Anomalous Water: General Review', *Société Chimique de France, Bulletin*, Pt. 1, 1629–1644.
- Gingold, Marcel Pierre (1974), 'L'eau Anomale: Histoire d'un Artéfact', *La Recherche* **5**, 390–393.
- Gould, Stephen Jay (1981), 'Ice Nine, Russian-Style', *The New York Times Book Review* (30 August 1981), pp. 7 & 15.
- Hasted, J.B. (1971), 'Water and "Polywater"', *Contemporary Physics* **12**, 133–152.
- Howell, Barbara F. (1971), 'Anomalous Water: Fact or Figment', *J. Chemical Education* **48**, 663–667.
- Hull, David L., Tessner, Peter D., and Diamond, Arthur M. (1978), 'Planck's Principle', *Science* **202**, 717–723.
- Judge, George G., Griffiths, William E., Carter Hill, R., and Lee, Tsoung-Chao (1980), *The Theory and Practice of Econometrics*, New York: John Wiley.
- Klotz, Irving M. (1986), *Diamond Dealers and Feather Merchants*, Boston: Birkhauser.
- Kollman, Peter A. and Allen, Leland C. (1972), 'The Theory of the Hydrogen Bond', *Chemical Reviews* **72**, 283–303.

- Kuhn, Thomas S. (1970), *The Structure of Scientific Revolutions* (2nd edn., enlarged), Chicago: University of Chicago Press. (1st edn. published 1962).
- Manski, Charles F. and Lerman, Steven R. (1977), 'The Estimation of Choice Probabilities from Choice Based Samples', *Econometrica* **45**, 1977-1988.
- Metzger, Norman (1970), 'Polywater Boils', *Chemical and Engineering News* (9 Nov. 1970), p. 9.
- Mulkay, Michael (1980), 'Sociology of Science in the West', *Current Sociology* **28**, 1-116, 133-184.
- O'Konski, Chester T. (1970), 'Covalent Polymers of Water', *Science* **168**, 1089-1091.
- O'Konski, Chester T. and Levine, S. (1971), 'Instability of Water Polymers', *J. Colloid and Interface Science* **36**, 547-551.
- Pethica, Brian A. (1982), 'Review of *Polywater*', *J. Colloid and Interface Science* **88**, 607.
- Pimentel, George C. and McClellan, A. L. (1971), 'Hydrogen Bonding', *Ann. Rev. Physical Chemistry* **22**, 347-385.
- Planck, Max (1949), *Scientific Autobiography and Other Papers*, New York, pp. 33-34.
- Rabideau, S. W. and Florin, A. E. (1970), 'Anomalous Water: Characterization by Physical Methods', *Science* **169**, 48-52.
- Rousseau, D. L. and Porto, S. P. S. (1970), 'Polywater: Polywater or Artifact?' *Science* **167**, 1715-1719.
- Small, H. G. and Griffith, B. C. (1974), 'The Structure of Scientific Literatures, I: Identifying and Graphing Specialties', *Science Studies* **4**, 17-40.
- Stigler, George J. (1982), '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. 223-243.
- Zuckerman, Harriet (1977), 'Deviant Behavior and Social Control in Science', in Edward Sagarin (ed.), *Deviance and Social Change*, London: Sage Publications, pp. 87-138.