A Scientific Gold Rush

Polywater. FELIX FRANKS. MIT Press, Cambridge, Mass., 1981. xiv, 208 pp. $15.

"Gold rush" is an image used by Felix Franks in this study of "anomalous water" or "polywater" during the 11-year period from its discovery in 1962 to 1973, when even the hardest-line adherents admitted that the modified water they were studying was only an artifact. Franks's book consists of a description of the scientists who studied polywater and the various professional and nonscientific forces that impelled them.

As in most gold rushes, the original find of anomalous water was made in a remote locale, a provincial Russian institute, where a surface scientist discovered that he could prepare columns of water having anomalous properties in fine glass capillaries. The initial publication was hardly noted outside the Soviet Union, but Boris Deryagin of the Institute of Surface Chemistry in Moscow turned his large research group toward the study of this substance. Deryagin confirmed that "anomalous water" could be condensed in glass or quartz capillaries smaller than 100 micrometers in diameter and was characterized by a lower vapor pressure than ordinary water, a greater density, a reduced freezing point, and an elevated boiling point. Continued Western indifference to the ten or so Russian articles could probably be explained by the fact that they made no mention of studies with modern analytical tools and hardly inspired conviction that the columns in capillaries were truly a modified metastable water, as claimed. What did create notice in the West was Deryagin's lectures in England and the United States in 1966, 1967, and 1968, in which he insisted flatly that he had excluded impurities and was indeed studying a new form of water.

The trigger of the gold rush in the West was the appearance in the 27 June 1969 issue of *Science* of an article entitled "Polywater" from the laboratories of Ellis R. Lippincott of the University of Maryland and Robert Stromberg of the National Bureau of Standards. The authors reported infrared and Raman spectra of the substance in the quartz tubes that were not those of water and concluded that the properties "are no longer anomalous but rather, those of a newly found substance—polymeric water or polywater." Small wonder that these dramatic assertions evoked a cascade of comments and speculations in journals, the scientific press, newspapers, and magazines.

There were more than 160 articles on polywater appearing in 1970 alone. All told some 500 publications related to polywater appeared in the period 1963 through 1974, about half of them in research journals and the other half consisting of comments or journalistic reports. About 400 scientists busied themselves with polywater research. At the height of the excitement a scientist in Washington, D.C., found that the supply of desiccators had run out because so many experimentalists were seeking them as enclosures for condensation of polywater in drawn glass capillaries. Nor were theorists left behind in the rush. Dozens of structures were proposed in some 25 articles, and overenthusiastic quantum chemists went so far as to state that their calculations established the existence of polywater.

Though this history is sketched by Franks, he does not attempt to describe in detail the events or the investigators, nor does he examine closely the scientific issues. His book is essentially a popular sociology of the polywater episode. He is interested in the factors, partly nonscientific, that created the gold rush atmosphere and distorted the normal scientific process. Among the factors blamed by Franks are: the willingness of some scientists to submit for publication incomplete or even shoddy work in order to achieve priority; a breakdown in normal standards of reviewing, particularly in journals such as *Nature* and *Science* that publish short notes on matters perceived to be of wide current interest; a concern among administrators in defense-sponsored research agencies that in the post-Sputnik era it would be unfortunate to allow the Soviets the lead in another field; a fascination on the part of the public, created in part by exaggerated and inaccurate reports in the popular press, with a new form of water; a tendency of investigators to leak results to the press before publication; and a "lack of tolerance" for polywater research by the "scientific elite," which in Franks's view contributed to "the polarization of opinion" and exacerbated the situation.

What is the assay of scientific truth? What allows scientists to place confidence in any given conclusion, particularly when it is based on unexpected and unusual observations? This issue is not directly addressed by Franks but is central to understanding the polywater episode. In the case of polywater, for which chemical analysis was difficult because of the small quantities available, my guess is that most chemists would place heavy reliance on consistency with the great principles of thermodynamics and structural chemistry built up during the past century. This is the idea expressed in the dedication of a famous textbook by G. N. Lewis and M. Randall: "The fascination of a growing science lies in the work of the pioneers at the very borderland of the unknown, but to reach this frontier one must pass over well-traveled roads; of these one of the safest and surest is the broad highway of thermodynamics."

Astonishingly few of the polywater scientists considered the implications of thermodynamics, even though they lead to an immediate negative conclusion about polywater. Since polywater has a lower vapor pressure than ordinary water, with which it is in equilibrium through the vapor, it follows from the first and second laws of thermodynamics that the polymer must be the more stable form. A polywater scientist would be forced to conclude either that billions of years of water waves and water rains washing over silica beaches had not produced the stable form of water or that the laws of thermodynamics are wrong. The correct conclusion—that "polywater" is a solution of impurities in water—is suggested directly by thermodynamics and the observed properties of polywater, such as its lowered vapor pressure and depressed freezing point. The tendency of polywater scientists to ignore well-established thermodynamic principles suggested some "will to believe."

Structural chemistry, in the accumulated results of decades of studies on related compounds, presents a means to test the proposed models for water. That most of the models were inconsistent with firm results of the past was expressed in a lengthy *Science* article by W. Barclay Kamb. This serious paper is singled out by Franks for criticism as "flogging a dead horse" and for being published only because of Kamb's "political muscle."

Franks's book shares characteristics

with the scientific episode it describes. It raises many questions, not always sharply defined, it offers less incisive analysis than one might hope for, and it expresses some questionable judgments. Also like the polywater literature, it makes intriguing reading and here and there yields nuggets of scientific interest or amusement. One is a biological disproof of polywater attributed to the physicist Richard Feynman: "There is no such thing as polywater because if there were, there would also be an animal which didn't need to eat food. It would just drink water and excrete polywater."

Overall the scientific community can find some solace even from the polywater episode. There was no fraud, and it was demonstrated that there is enough flexibility in current science for investigators to exercise imagination and to follow a hunch. Perhaps the main problem was that our scientific system of "organized skepticism" (in the phrase of R. K. Merton) eroded at the start of the gold rush, permitting publication of a series of hasty, incomplete, and poorly thought-out papers. Once these initial papers were in print the standard for publication on polywater had been lowered, and an alarming pattern set in of communication by preliminary notes and press releases. However, it took only four years from the Lippincott-Stromberg paper that named polywater for even Deryagin to admit in print that his anomalous water was merely a solution of impurities. Gradually the self-correcting nature of scientific research took hold and put an end to the polywater gold rush.

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On the Origin of the Principle of Diversity

A Delicate Arrangement. The Strange Case of Charles Darwin and Alfred Russel Wallace. ARNOLD C. BRACKMAN. Times Books, New York, 1980. xii, 370 pp., illus. $14.95.

Charles Darwin wrote of the *Origin of Species* that it was one long argument. The same might be said of Arnold Brackman's *A Delicate Arrangement.* Unlike the *Origin*, Brackman's book fails to establish its thesis: that Darwin perpetrated a "cover-up and conspiracy" against Alfred Russel Wallace. The cover-up is Darwin's alleged delay in transmitting Wallace's famous 1858 paper to Lyell and concomitant lies to both Lyell and Hooker. The conspiracy, generated by Darwin's cover-up but executed primarily by Lyell and Hooker, consists of attempts to deprive Wallace of his priority over Darwin.

What was there to be covered up? Brackman strongly insinuates, but does not openly charge, that Darwin was guilty of unacknowledged borrowing and perhaps outright theft of the "principle of divergence" from Wallace in the years between 1855 and 1858.

Brackman's insinuations can be refuted, but to do so it is necessary to define the principle of divergence and its importance to Darwin and Wallace. Divergence need only mean that taxa can be arranged in a branched—hence diverging—scheme. Let us call this taxonomic divergence. However, once Darwin and Wallace became convinced of evolution, taxonomic divergence became charged with new significance. It became necessary to explain why evolutionary history shows a divergent pattern and to explain how divergence occurs—in other words to formulate a principle of divergence. For both men this was a problem to be solved by natural selection, and for both men implicit in the origin of divergence was the even more fundamental problem of the origin of new species. Thus deriving a unifying principle that would apply natural selection to the origin of species and divergence became a matter of prime theoretical importance. Darwin and Wallace both responded to the explanatory challenge. But, in my opinion, the chronology and the content of their responses differed strikingly.

Darwin recognized the evolutionary implications of taxonomic divergence soon after becoming a transformist in 1837. Initially he tended to explain species formation by geographic isolation. This, however, was never his exclusive explanation (see D. Kohn, "Theories to work by," *Stud. Hist. Biol.* 4, 67–170 [1980]), and ultimately he became committed to the view that new species can form without geographic isolation. This conclusion was important in setting the stage for his later explanation of divergence. In 1838 he first formulated the idea of natural selection to explain adaptation. His attempts in the 1840's to apply selection to divergence were constrained by two leading assumptions: first that variation in nature is severely limited and second that there is a fixed limit to the amount of life the globe can sustain. The first was a common, though not unchallenged, assumption among naturalists. The second, a reformulation of the idea of the balance of nature, was most forcefully argued by Charles Lyell. By the mid 1850's Darwin had revised both assumptions and was able to derive his principle of divergence. He argued that a locality can support more life if occupied by diverse forms partitioning resources. Thus specialization is advantageous to an organism. Hence natural selection, which explains all adaptation, favors the evolution of new varieties, hence of new species. From this first fork of the branching phylogeny it is a matter of reiteration to generate all of classification. Simply put, niche within niche engenders group within group. Darwin's principle, which he regarded as a "keystone" of his work, is itself a set of nested arguments comprising the idea of natural selection, the idea of speciation without isolation, and the view that the relations among organisms create new evolutionary situations.

When and how Darwin came to formulate the principle of divergence is the subject of intense historical research. It is certain that by September 1857 Darwin sent Asa Gray a fully articulated statement of the principle. Subsequently, Darwin wrote and rewrote extensive treatments of divergence for chapters 4 and 6 of *Natural Selection*, the long version of the *Origin*. But these versions do not differ at all in concept from the September 1857 document. However, as we have seen, the principle of divergence was not a unitary idea. Nor was its formulation a single event. Recent work, based on Darwin's abundant notes from the 1840's and 1850's, shows that Darwin repeatedly deployed versions of the principle in extended discussions of classification and embryology (D. Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838–1859*, Cambridge Univ. Press, in press), geographic distribution (J. Browne, "Darwin's botanical arithmetic and the principle of divergence, 1854–1858," *J. Hist. Biol.* **13**, 53–89 [1980]), and ecological relations (S. Schweber, "Darwin and the political economists: divergence of character," *J. Hist. Biol.* **13**, 195–289 [1980]). In the years of massive research leading up to the construction of the *Origin* the theme of divergence is ever present. The case seems to be that Darwin actually applied limited principles of divergence, tailored to the contemporary state of argument and data in a number of different disciplinary domains, before he was able to