HISTORY OF SCIENCE

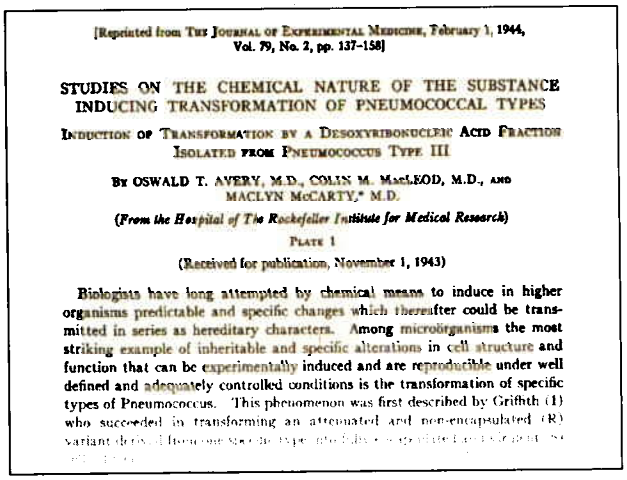
Stepping Forward Too Far?

Naomi Oreskes

In 1968, molecular biologist Gunther Stent declared that molecular biology was dead (*1*). Like Mark Twain's death notice, this announcement was premature, and Stent subsequently became interested in the fate of ideas that appear ahead of their time. That topic is taken up in the volume *Prematurity in Scientific Discovery*, edited by Ernest B. Hook (a professor at the School of Public Health, University of California, Berkeley) and the result of a 1997 conference organized by Hook. The impressive list of contributors includes scientists Glenn Seaborg, Charles Townes, Norton Zinder, and Oliver Sacks along with scholars of science Norriss Hetherington, Mary Jo Nye, Frederic Holmes, Elihu Gerson, and David Hull.

According to Stent, "A discovery is premature if its implications cannot be connected by a series of simple logical steps to contemporary canonical or generally accepted knowledge" (*2*). In addition, he suggested that the scientific community's practice of ignoring premature claims was justified by the need to avoid being overwhelmed by false leads. Scientists have paid some attention to Stent's idea, but historians of science have mostly neglected (or rejected) it. Hook wants to change this. His goals are practical: to help scientists avoid missing important developments, and to get the best possible reception for their own innovative work. Yet it seems unlikely that Stent's concept can help achieve these goals. For Stent, scientists have no choice but to reject a premature discovery until further work creates the simple logical steps that link it to accepted knowledge. But this defines in logical terms a problem that is fundamentally sociological. As Zinder eloquently explains in his essay on the recognition of the three modes of gene transfer in bacteria, "The important part of a scientific discovery in almost any aspect of science is the reception it receives, and this is in large measure a social phenomenon." Whether a theory is accepted, rejected, ignored, attacked, or deferred for later consideration is a question of how people behave—and behavior is not a logical problem.

Looking only at logical connections can blind you to other parts of the story. Consider Hook's own essay on Ida Noddack's 1935 suggestion that Enrico Fermi had not created transuranic elements in his seminal experiments on uranium bombardment but had instead induced nuclear fission, a suggestion that was largely ignored. Hook dismisses the idea that gender may have been a factor: "I find it difficult to believe that a reasonable individual would have been foolish enough to reject or ignore any hypothesis because it emanated from a woman." Difficult to believe or not, there is a voluminous literature on women in science—including physicists such as Marie Curie, Lise Meitner, Herthe Sponer, and Noddack herself—that documents precisely how their work was dismissed or disparaged simply because it emanated from women (*3*). Hook also makes the peculiar claim that it is a good thing that Noddack was ignored, or else the Nazis "might well have developed the atomic bomb." Such a claim disregards the extensive historical scholarship demonstrating that although intellectual understanding of nuclear fission was a necessary condition for the construction of nuclear weaponry, it was nowhere near sufficient (*4*). Ideological blinders seem also to afflict William Glen, whose essay on prematurity in several areas of earth sciences cites none of the extensive historical literature on the rejection of Alfred Wegener's theory of continental drift and the later acceptance of plate tectonics (*5*), nor the most authoritative work on the history of climate research (*6*).



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STUDIES ON THE CHEMICAL NATURE OF THE SUBSTANCE INDUCING TRANSFORMATION OF PNEUMOCOCCAL TYPES

Induction of Transformation by a Desoxyribonucleic Acid Fraction Isolated from Pneumococcus Type III

By OSWALD T. AVERY, M.D., COLIN M. MacLEOD, M.D., and MACLYN McCARTY,* M.D.

(*From the Hospital of The Rockefeller Institute for Medical Research*)

Plate 1

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Biologists have long attempted by chemical means to induce in higher organisms predictable and specific changes which thereafter could be transmitted in series as hereditary characters. Among microörganisms the most striking example of inheritable and specific alterations in cell structure and function that can be experimentally induced and are reproducible under well defined and adequately controlled conditions is the transformation of specific types of Pneumococcus. This phenomenon was first described by Griffith (1) who succeeded in transforming an attenuated and non-encapsulated (R)

Premature proof. Stent's original example of premature discovery was Avery's identification of DNA as the active principle in bacterial transformation.

Prematurity in Scientific Discovery
On Resistance and Neglect
Ernest B. Hook, Ed.

University of California Press, Berkeley, 2002. 398 pp. $80, £55. ISBN 0-520-23106-6.

It is not unreasonable to ask why ideas that are later accepted—even heralded—fail in earlier incarnations, and the volume contains several informative accounts of particular instances. But there is a problem with trying to answer the question generically, for how long should it reasonably take for an idea to take hold? Stent's original exemplar was the 1944 discovery by Oswald Avery, Colin MacLeod, and Maclyn McCarty that DNA (not protein) is the hereditary material, as demonstrated by DNA's ability to transform nonvirulent into virulent forms of pneumococci. Stent claimed (*7*) that this work made "a surprisingly small impact on geneticists" (including himself) until it was reinforced by A. D. Hershey and Martha Chase, who showed that viral DNA enters infected bacterium while viral protein remains largely outside. Meanwhile, eight years had passed.

Stent seems to expect that when an important discovery is made, all scientists in the field should drop what they are doing and immediately attend to it, and he admits this is what he wishes he had done. But he is too hard on himself. With rare exceptions, this is not what scientists do. As Holmes notes in his essay on the dynamics of scientific change, any notion of prematurity presumes some "normal lapse of time between the first presentation of a discovery and its assimilation into a field.…There are, however, no controls against which to measure the deviations."

Moreover, although Stent may have accurately characterized the reaction of Max Delbrück's phage group (of which he was a part), Holmes reminds us that others—including Theodosius Dobzhansky, Hermann Muller, René Dubos, and Erwin Chargaff—took up the question (*8*). Chargaff saw Avery's discovery as crucial to the direction of his own research between 1948 and 1950 (*9*), work that led to his 1950 discovery of base complementarity in DNA. He attributes the lack of fanfare surrounding the pneumococci work to Avery's quiet disposition. (Chargaff also notes that Avery was 67 at the time, making this a nice counter-example to the assertion

The author is in the Department of History, University of California, San Diego, La Jolla, CA 92093–0104, USA. E-mail: noreskes@ucsd.edu

that important scientific discoveries are the province of the young.)

Perhaps Stent's concept is a bad idea that inspires a good discussion. Yet, given the general topic (the failure of scientific progress), I could not help but feel that the focus of *Prematurity in Scientific Discovery* is misplaced. We currently face a plethora of social dilemmas in which science has an important role to play, and we often seem to lack critical information in formulating efficacious solutions (*10*). Why has science failed to keep up? The promise of Vannevar Bush's vision for science in America was that basic science would address—and solve—the problems of our society, just as under his leadership it had addressed the needs of the U.S. military during World War II. Guided by this vision, the United States invested unprecedented amounts of money into scientific research in the second half of the 20th century. Although much has been learned during this time, in many areas our science seems to be lagging. While reading these essays, I found myself inverting the alleged problem. Perhaps the time has come to address the issue of science that comes not too early, but too late.

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POLITICAL SCIENCE

Sizing Up Democratic Systems

Peter R. Orszag

Recent confrontations over the war in Iraq vividly demonstrate differences among the leading democracies of the world. Beyond the immediate contretemps over that war, however, the nations have diverse views regarding economic and social issues. Such differences are the subject of Harold Wilensky's majestic *Rich Democracies*. In 891 pages, Wilensky, an emeritus political scientist at the University of California, Berkeley, examines the sources of convergence and divergence among 19 rich democracies, all of which have populations of three million or more.

Rich Democracies
Political Economy, Public Policy, and Performance
by Harold L. Wilensky
University of California Press, Berkeley, 2002. 923 pp. $85. ISBN 0-520-23176-7. Paper, $45. ISBN 0-520-23279-8.

Wilensky conducted research for the volume over a span of 30 years. As a result, the book's sweeping subject does not generate broad platitudes, which are all too common in cross-country comparisons, but rather produces detailed summaries of the relevant literatures and trenchant observations that have eluded other commentators. The volume is all the more remarkable for the author's disclosure, in the preface, that the 1991 Berkeley firestorm caused the loss of "most of the manuscript, my library, files, notes, and so on. It took another eight years to reconstruct what was lost and complete this book; not even that was enough to update all chapters equally well." Writing one magnum opus is impressive; writing it twice is exceptional.

Wilensky's thesis is that rich democracies, as their income increases, share several tendencies, including reduced fertility rates and higher divorce rates, increased opportunities for minority groups, expanded higher education, and an increasing dominance of media and entertainment in society and politics. These sources of "convergence,"

The author is at the Brookings Institution, 1775 Massachusetts Avenue, NW, Washington, DC 20036, USA. E-mail: porszag@brookings.edu