OPINION

Science interminable: Blame Ben?

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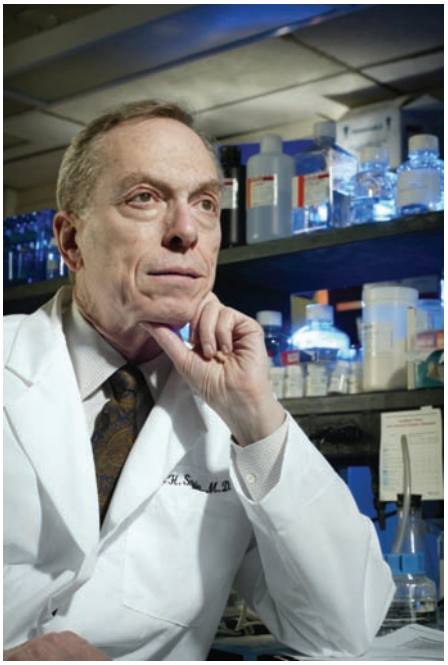

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Science is indeed interminable. Criticizing old ideas, coming up with new ones in an iterative process of creative destruction and reinvention is the spice of life for research. But there is another seemingly interminable aspect of science, at least in the biomedical arena, related to journal publication. When I was a postdoctoral fellow with Julius "Julie" Axelrod at the National Institutes of Health (NIH) circa 1963–1965, a typical postdoctoral fellow might write several papers in a year. While snail mail and other technical limitations slowed progress, the overall research/publication process then was substantially briefer than now. The latency between initiating a project and writing a paper was often under a year, while today the process typically comprises 5 years or more. These discrepancies are perplexing, because today's tools of biomedical research are vastly more powerful than those of 40–50 years ago. The techniques of molecular biology can provide definitive answers rather than mere suggestions.

What Has Changed?

Why does it take so much longer to move from test tube to the printed page? One element is a journal review process that is substantially lengthier, especially in terms of experiments required to address the concerns of referees. To anticipate such referee responses, scientists preemptively carry forward experimentation more exhaustively than is necessary to document their assertions. Yet, we can clone genes in a couple of days. Shouldn't we be able to complete experiments to satisfy reviewers in a few weeks rather than the 7–12 months typically consumed in revision, not to mention the many years devoted to developing the original manuscript? If one spends 5 years accumulating the data for a manuscript and another year revising it to satisfy referees, benefits to the public are delayed for years. The Axelrod laboratory might have been criticized for submitting manuscripts based on a "minimal publishable unit." Perhaps the dialogue of science would have been improved if each paper told a more complete story. On the other hand, this drawback of the old approach must be weighed against limitations of the new paradigm that slow the rate of scientific progress by years.

Besides restricting public access to research advances, the modern mode of biomedical publication hinders the careers of young scientists. A graduate student cannot obtain his or her PhD until completing a "publishable" body of work, which typically means a full-length manuscript. With present-day publication mechanisms, he or she will have to work in the lab for 5 or more years before receiving a PhD. The same concern applies to postdoctoral fellows. Abundant data confirms major increases in the duration of both doctoral and postdoctoral training. When I joined the Johns Hopkins faculty in 1966, graduate students spent their first year taking courses and then rarely more than 3 years doing research for their thesis. Today, 5 years is considered relatively swift for PhD research with 8–9 years of total graduate training not being exceptional. In the late 1960s most biomedical postdoctoral fellowships lasted about 2 years, while today 5 years is considered short.

The present mode of graduate/postdoctoral training can be demoralizing. Authoritative surveys indicate that the mean age for a biomedical researcher obtaining his or her first independent faculty position is close to 40, while the mean age for obtaining NIH grant support is about 43. The only comparable durations for career preparation in medicine or science are for the training of a neuro- or cardiovascular surgeon. However, such physicians can look forward to lives of guaranteed employment with annual incomes of $500,000–1 million or greater. By contrast, PhD biomedical scientists aspire to relatively low-paying academic positions, which are increasingly unattainable. With such disincentives, it is not surprising that few American citizens choose biomedical research careers.

In 1974 Benjamin "Ben" Lewin founded the journal *Cell*. First published by MIT Press, Lewin acquired the journal, created Cell Press in 1986, and then developed a series of new journals such as *Neuron*, *Immunity*, and *Molecular Cell* before selling Cell Press to Elsevier in 1999. A talented molecular biologist himself, Lewin appreciated that the tools of molecular biology enabled an investigator to solve a problem "from soups to nuts," e.g., identifying a novel protein, cloning it, and, by selective genetic deletion, unraveling its biologic function. Accordingly, he conceptualized how *Cell* would differ from existing biomedical journals. He exhorted authors to refrain from publication until they had completed their "stories." He recruited the leading molecular biologists of the day as authors, advocating that they publish in his journal the best of their best research. He encouraged lengthy articles encompassing years of work, providing "the last word." The prestige of each published *Cell* paper would be so great that authors would not mind delaying publication and publishing fewer papers.

Lewin Succeeded Magnificently

From its first issues *Cell* emerged as the top journal in the field with the best sci-

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entists vying for a place in its pages. Soon the *Cell* ethos was adopted by most of the prestige journals, and scientists reasoned, "Why waste our time publishing dozens of papers, when we can write only a few, but each a blockbuster?" With the advent of the internet, even short reports became definitive opuses. With software such as Adobe Illustrator, one could create figures containing a dozen subdivisions so that a short report could document 5 years of work, especially with the assistance of ample supplemental material.

The sad outcome is a mode of publication which is counter-productive to rapid dissemination of important advances.

All of these considerations impacted the process of manuscript review. If publications are supposed to reflect the absolute last word, then they should be close to perfect. Molecular biologic tools permit an experimental approach to almost any question. Hence, manuscript reviews have become progressively more exhaustive, demanding large amounts of new experimentation. When I worked with Axelrod, a third to a half of our papers were accepted with negligible revision. Additional experimentation would typically consume no more than 2 months. Today the lag between submission and acceptance is often more than a year. Most of my colleagues feel that roughly 75% of referee-mandated experimentation involves picayune items that add very little to the scientific importance of the manuscript.

To my mind, Lewin is a giant figure in modern biomedical publication. The journals he created are first rate and provide high ideals to which all of us aspire. However, the process has gone awry. In the early years of *Cell*, Lewin's goal did not seek unreasonably exhaustive documentation nor subject authors to interminable revisions. Indeed, one of Lewin's contributions was a highly expeditious, thoughtful, and incisive evaluation of papers along with speedy publication. As a bonus to authors, Lewin demanded that referees be respectful and shun insulting comments. His own letters to authors were always thoughtful and constructive. In the process of their promulgation over the past almost 4 decades, Lewin's innovations have been distorted. The sad outcome is a mode of publication which is counterproductive to rapid dissemination of important advances and which hinders the careers of young scientists.

What Can We Do?

Journals can change their *modus operandi*. They can inform prospective authors that their goal is to publish important, innovative research, which is well enough documented to establish the major thesis of the authors, but rapidly. They should emphasize that securing these objectives requires adequate but not exhaustive documentation. They should provide expeditious reviewing and reasonable requests for revision. Their cadre of reviewers should be trained in this modified approach.

The ease of Internet publishing has led to a proliferation of new journals, all vying for a place in the sun. One of these, *eLIFE*, launched by Randy Schekman, former editor-in-chief of PNAS, incorporates some of the modifications we have suggested. PNAS has strived to expedite publication while maintaining high standards with working scientists, rather than professional editors, handling editorial chores. The median time from submission through review and revision to publication of directly submitted PNAS articles is now about 5.8 months. Hopefully, these new approaches to publishing will rapidly attract authors and shame editors of established prestige journals into following suit.

We need not "blame Ben." Instead we should emulate his example.