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EUGENE GARFIELD

INSTITUTE FOR SCIENTIFIC INFORMATION[®]
3501 MARKET ST. PHILADELPHIA PA 19104

Stephen P. Lock on "Journalology"

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I don't remember exactly how long I have known Stephen P. Lock. Our first encounter was probably at a meeting of the Council of Biology Editors (CBE) some 20 years ago. It is somewhat unsettling to realize that so much time has gone by. Equally disquieting is the notion that Steve will soon retire as editor of the venerable *British Medical Journal (BMJ)*. But in the near future Steve will indeed step down, having helped train his successor, due to be appointed in the fall of 1990.

It seems odd to refer to Lock as an elder statesman of biomedical editing—however accurate that description might be. The word that comes to mind when I think of Steve is youth. This youthfulness is reflected in his writing and in his approach to problems.

Last spring Lock addressed the CBE's 33d annual meeting in Rochester, Minnesota. His talk on "Journalology," published originally in the newsletter *CBE Views*,¹ is reprinted here. In it, he demonstrates why he has gained the respect of his peers.

While Lock has published scores of editorials and articles, his most visible work is the 1985 book *A Difficult Balance*,² which I discussed in *Current Contents*[®] (*CC*[®]) several years ago as part of an essay on refereeing.³ At that time ISI Press[®] held the publishing rights to this now-classic commentary on peer review. The book originally had a limited distribution under the sponsorship of the Rock Carling Fellowship and the Nuffield Provincial Hospitals Trust. *A Difficult Balance* is still available through Waverly Press, Baltimore, Maryland.

Lock: Editor and Scholar

Stephen P. Lock was born in 1929 and received his education at Queen's College, Cambridge University, UK, and the Medical College of St. Bartholomew's Hospital. Trained as a hematologist, he served on the staffs of various London teaching hospitals, including St. Bartholomew's and the Hospital for Sick Children, before being appointed assistant editor of *BMJ* in 1964. After serving as senior assistant editor and deputy editor, Lock became editor in 1975.

I am amused that Steve opened his talk with the traditional, mythical reminder about the estimates of the number of extant journals. I've commented on this many times before. If there are 15,000 "serious" publications, as he puts it, he is quick to correct the false impression this number creates.

The so-called information "explosion" is an illusion. There has actually been a constant growth of 5 to 7 percent per year since 1665. Although there is good reason to believe that this growth pattern no longer holds, it is a useful rule of thumb to estimate that the number of journals is consistently related to the number of research scientists, or even to the number of physicians. Lock mentions unpublished work by *Annals of Internal Medicine* editor Edward J. Huth, another elder statesman of medical editing who is also approaching retirement. Huth determined that for the last 30 years or so there have been about 17 journals for every 1,000 registered physicians.

But, in spite of all this, it is now clearly established that there is a much smaller number of significant journals in the world. And as the literature grows, the role of these significant journals becomes even more central. Less than a few hundred journals account for the vast majority of significant research. Nevertheless, each edition of *CC* covers approximately 1,000 journals. The combined editions, covering about 5,000 scientific titles in all, give a fairly comprehensive view of worldwide science.

What is more important is that this enormous output demands even higher standards in regard to publication, peer review, and professional performance by editors. Further complexity is added by the huge public interest in the results of research, intensified by a large corps of science journalists eager to report the latest discoveries.

I find particularly interesting Steve's prediction that in the near future editors will be held accountable for their conduct. He forecasts that anonymity in journal operations will disappear and that all authors will receive justice in the form of "much fuller explanations and instructions to authors...and peer reviewers' reports."¹

He also subscribes to the suggestion of yearly self-audits by editors first suggested in *THE SCIENTIST*[®] by Andrew Herxheimer, the editor of the *Drug and Therapeutic Bulletin*.⁴

Steve's talk also refers to meta-analysis, a concept which not all *CC* readers will be familiar with. It is a topic I intend to cover in greater detail in the future because it is so closely related to the notion of the library as a laboratory. The literature becomes a powerful tool when previously unrelated studies are pulled together by an informed reviewer with adequate expertise in probability statistics.

"Journalology" is an appropriate neologism for what has become a widespread human activity. Where it fits on the complex map of information science is of some academic interest but more important is the fact that to my knowledge there exists no course in journalology anywhere. This inadequacy



Stephen P. Lock

is matched by the dearth of curricula on publishing. I've urged my colleagues at several universities to consider the need and opportunities for establishing such a program.

Steve Lock, of course, would make an excellent professor of journalology. Fortunately, his plans after leaving *BMJ* include teaching on this topic. He will be working part-time on the newly established Faculty of the History of Modern Clinical Science at the Wellcome Institute for the History of Medicine, London,⁵ to be established under the directorship of Sir Christopher Booth, currently Harveian Librarian of the Royal College of Physicians of London. Lock hopes to continue teaching about journalology—particularly about fraud and peer review—as well as holding courses in medical writing, of which he has now conducted over 100 throughout the world in the past 20 years.

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"Journalology": Are the Quotes Needed?

Stephen P. Lock

British Medical Journal, Tavistock Square, London WD1H 9JR, United Kingdom

In this talk, Stephen P. Lock considers the continuing evolution in the population and forms of scientific journals and in editorial practices. As disciplines split and grow increasingly specialized, new journals arise to fill the needs of the various subdisciplines. The implications of this, for specialist journals and general journals alike, are examined. Also discussed is the prospect that, in the future, editors will be held increasingly responsible and accountable for article content.

Some current estimates put the total number of scientific journals at 100,000, some 20-25,000 of them biomedical—of which 15,000 or more are "serious" publications (that is, giving data and references, using peer review, and so on). And yet this so-called "explosion" is an illusion; the rate of expansion since the beginning of serious publication in 1665 has been a constant 5 to 7% a year.¹ There are, of course, many more scientists around than there ever have been, and there's a cogent suggestion that the proportion of the number of journals to the number of scientists is constant. This was borne out by some unpublished research by Dr. Edward J. Huth, the editor of the *Annals of Internal Medicine*, who took two undoubtedly accurate figures over the past 30 years: the number of journals received by the National Library of Medicine in Bethesda, Maryland, and the number of registered physicians in the United States. He found that there was a constant ratio of 17 journals per 1000 physicians and that this hadn't changed over the past 30 years.

If this pattern is true, what is the reason? The answer can be found, I think, in the way that scientific disciplines evolve. Derek J. de Solla Price's

rule of thumb is that a scientist who publishes one article every year can take in the contents of more than one other paper a month, but less than one article a day. This leads to a few hundred individuals keeping each other in business. Now a few hundred is the same size as the membership of the early scientific societies, and of today's invisible colleges.²

Disciplines tend to split every 10 years or so, and the new subdisciplines do not necessarily correspond with the organizational and professional structures. These are much more rigid and slower to change than the way in which the pattern of new knowledge changes. These structures will include journals, and thus there is constantly a real need for new journals which will reflect the needs of the new subdisciplines. In this way a specialist journal will be formed to take some of the work which has become too complex for the general journal—and the general journal may truly have been under tremendous pressure for space and unable to publish even all the first-rate material submitted to it. But in time even the specialist journal is seen as too rigid or unable to cope with all the articles, and so the need for another, super-

specialist journal arises. A typical pattern might be: *Philosophical Transactions of the Royal Society*, *British Medical Journal (BMJ)*, *Gut*, *Journal of Gastrointestinal Endoscopy*, *Pancreas*.

In this way you get a hierarchy of journals. Nevertheless, these are not static and their content changes with the needs of the readership and the philosophies of the times. Take my own journal, for example, the *BMJ*. At one time this covered all fields, principally with original articles and a few editorials. Then increasing specialization meant that a number of separate disciplines set up their own journals. Now the load of specialist articles with very little general appeal could be diverted away from the general journal. But then the readers of the general journal wanted to be kept in touch with important developments in all the specialties and to have them explained and put into the context of accepted practice. So general journals started to publish many more review articles, editorials, and other forms of comment in an attempt to keep this fragmented corpus of general medicine together. Today, for example, the *BMJ* may have 8 pages of leading articles and a regular review; 6 pages of a new style of News presentation, with informed expert comment on developments; a Medical Practice section of 16 pages including an ABC of child abuse and an article on new drugs; a large correspondence section almost entirely devoted to comment on previously published articles; and so on.

This process of generalization is now also happening to special journals in the first tier: these are now becoming *general* journals of their discipline. For example, pediatrics is now not only a large specialty but also a very wide one. The pediatric geneticist needs to keep up with pediatric orthopedics and pediatric endocrinology—otherwise he is lost, and so may be his patients. For this reason we get the first tier pediatric journal displacing some of the articles submitted to it to the second tier and filling the space with editorials, review articles, personal practice, and so on. If you look at, say, the *Archives of Disease in Childhood* 30 years ago, you will see nothing but hard science—no editorials, personal practice articles, reviews, opinion pieces, correspondence, and so on. Today the make-up of the journal is quite different. Nevertheless, if you compare the *BMJ* or the *Archives* 30 or 40 years apart, what is also obvious is the consistency of the number of original papers published. This apparent paradox has come about by the professionalization of the editor and his activities, with, of course, the approval of the author and the readers. The

editor has introduced peer review if this did not exist before—or made it more stringent with guidelines and so on if peer review did exist. He has started paying attention to the important aspects of statistics—say, the design or the analysis of a clinical trial. And finally he has started sub-editing the manuscript to be published—not just housestyling but cutting and reshaping the account, so that it can be read with ease by both native readers of that language and others whose language is different. And the result has of course been shorter and more lucid articles.

All this can come about because obviously journals are big business and the revenue from subscriptions and advertisements can pay for this kind of professionalization. But it has also come about for a philosophical reason. Let's go back to the invisible college, which forms a new discipline, which then needs its own journal. At this stage the readership of the journal is the same as the authorship—small groups of workers are writing for each other. Later on, when the discipline has expanded, the situation is different: readers are now in the majority; they don't necessarily understand every article any more, and so they need articles to be clear, relevant, and accurate. Hence the editor now has to change his emphasis to the needs of the reader, and this means the new structure of the journal and the new process of editing which I've described.

Although he's usually a part-time worker, the editor also becomes professionalized, and soon he forms clubs and groups, which then discuss problems and produce guidelines and codes. Many such groups have been formed in the past few years, and they have discussed matters such as ethics, statistics, authorship, and what to do about publications based on fraudulent work, to such an extent that they now talk about "journalology" in only the faintest of ironic terms.

Once you become an organization you develop responsibilities. From within there are factions that want a bigger say in the outside world, whether it's responsibility or money, or both. From without there are calls from national and international bodies for advice on all sorts of topics, as well as demands that the organization should do something. The whole process has become less fun, and more of a job, but that is the way it should be. It's inevitable, for, much as research started with the amateur aristocrat, the Honourable Robert Boyle, paying for it himself, it has ended up as a billion dollar enterprise employing thousands of professionals and supervised by NIH committees and so on.

Accountability for Editors

What all this may mean for editors, I believe, is that there will be a right way of doing things and a wrong way, and eventually I believe that society may even come to condemn the wrong way in the terms that everybody understands: legal and financial. At the extreme, for example, let's suppose that there is a scare story published in a prestigious journal, merely as a debating point. This arouses a lot of public interest; there is identifiable gross distress of individuals or families or a catastrophic loss of earnings to the firm that is concerned in making a product. But the editor of that journal has not submitted that particular article to external peer review, and/or has not checked the circumstances, and/or he or she has not accompanied it with an editorial to put the issue into perspective or labelled it with a "for debate" byline to indicate that the whole concept is speculative. (I emphasize that I'm thinking of hypothetical British examples rather than anything from this side of the Atlantic.) I believe that sometime in the future this editor might be held responsible in law for damages, on the grounds that he was in dereliction of his duty, and that he had behaved irresponsibly by not taking a number of important precautions.

We are in the middle of the evolution of all this, so let me illustrate what I mean by looking at statistics in medicine, rather than the more obvious topic of peer review, the subject of a splendid conference in Chicago [*First International Congress on Peer Review in Biomedical Publication, 10-12 May 1989*]. In other disciplines statistics go back a long way. In agriculture, for example, they were being used routinely in the 1920s. The first mention of probability, I think, was in the *Canadian Medical Association Journal*, in 1934³ and then in the *BMJ* in 1936;⁴ A. Bradford Hill published a series of articles in the *Lancet* in 1937, still updated every couple of years as a book,⁵ and then the great Ronald Fisher piled in the following year.

But statistics didn't really come into their own until after the second world war, any more than peer review did. Then we had massive controlled trials of expensive drugs in short supply, such as cortisone and the antibiotics, and everybody felt that in using statistics they would really come into the scientific era. Nevertheless, 10 years later the first analysis showed that, however impressive they were on the surface,⁶ the actual statistical details were awful, for, let's face it, most of the physicians had no idea what P values, SD, or SE

meant (and some of them still haven't). All too often the design of the trial, the analysis, and the interpretation were all inadequate. It needed similar demonstrations for another 10 to 15 years before anybody did much about it.^{7,8} And then editors began to use statistical referees, and immediately both groups developed new preoccupations. What papers deserved particular statistical attention? How could what the statisticians were talking about be translated for both editors and authors? And where did we get all those necessary statisticians from? And what on earth do editors do when statisticians don't agree?

Is Meta-Analysis Enough?

The next stage in this evolution was to put matters on a more formal basis with indicative rules for which papers needed statistical refereeing and checklists for the referees.⁹ And then, of course, there came another stage, when things got yet more complicated. We began to realize that with a report of a single clinical trial a so-called nonsignificant result was not an end in itself; we needed power estimation and confidence intervals. This led to a consideration of what you do about trials that are inevitably small scale, or produce negative results, or are not published, and, hey presto, you remember the existence of meta-analysis. Is simple meta-analysis enough? Answer: "no," because not all trials are equally valid. And so you develop a checklist for which trials should go into the meta-analysis, and how you get at unpublished work, and work that has produced negative results, and so on.¹⁰

I've chosen statistics as a convenient example, I suppose, because I am still in awe of it and its practitioners. But it is a convenient example, and we could apply this pattern of evolution to several other aspects of the evolving journalology: to definitions of authorship, scientific misconduct, peer review, and duplicate publication, to name only a few. I would like to end with a few personal prejudices. I believe, for example, that the most important development in the last 10 years has been the introduction of the structured abstract, which enables the editor, the referee, the reader (and, increasingly, the author) to see whether a paper is any good.¹¹ And increasingly it will enable a marriage between conventional publication on the one hand and the electronic data base on the other, particularly once this type of abstract has been applied to review articles and to case reports and the like, as I believe it can be.

Anonymity Will Disappear

Next, I believe that increasingly editors will have to remember their political role, with a small "p," and that justice and their explanations will have to be seen to be fair. Within 30 years anonymity, I'm sure, will disappear from all parts of the journal and its operations, and there will be much fuller explanations and instructions to authors and particularly including peer reviewers' reports. Another proposal, a yearly self-audit published by editors, on the lines recently suggested by Andrew Herxheimer¹² will help a lot. The converse, I believe, must be increased harshness

by editors. Half the contributions coming to my office don't conform with simple and necessary requirements; for example, there are not enough copies, or the manuscripts aren't in double spacing. These problems take up a lot of time, which is unfair to those who keep to the rules. In future I believe we should shred these manuscripts and tell the authors to provide us with what we want.

Finally, 40 years later most predictions on journalology turn out to be wrong. We still don't have, for instance, the two types of journals, newspaper and recorder, that were being postulated in the 1950s by J.D. Bernal and Sir Theodore F. Fox.¹³ But predictions are fun, so why not indulge?

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