Journal of Scientific Exploration, Vol. 16, No. 2, pp. 265–273, 2002 0892-3310/02

EDITORIAL ESSAY

What's an Editor to Do?

More particularly, what should the editor of a scientific journal do? And even closer to home, what should the editor of a *Journal of Scientific Exploration* do?

Consider first this context in which the issue arises: Not infrequently, scientific results and theories are refused publication in scientific periodicals even when those results or theories are later—sometimes much later—vindicated and incorporated into the accepted paradigm. Those whose work had been thus rejected have grounds to believe that science is not working in the manner that it should; they are inclined to call for science to be reformed.

Science and Consensus

Among the host of suggestions for how science is best described, for how science should be defined, I believe John Ziman (1968) has it most nearly right: Science seeks to attain, about Nature's phenomena, a consensus of rational opinion over the widest possible field. That takes into account some essential points:

Nature is not directly knowable. Observations and experiments do not yield self-evidently objective facts. Results must be interpreted and judgments made about them.
- Opinions are not always rational.
- Consensus is for science what a jury's verdict is for law and what a creed is for religion. People sometimes call that—inappropriately, misguidedly—"the truth".

Like any human activity, science has its institutions. Human institutions are at the same time bureaucracies. Therefore rules and decisions inevitably suffer from inadequacies, infelicities, and downright mistakes. How best to ensure the progress of sound scientific knowledge despite those deficiencies?

The Society for Scientific Exploration had to come into being because a sufficiently burdensome array of interesting topics was not being attended to by the scientific community: "cryptid" animals, "psychic" phenomena, UFOs. Those and similar matters were being investigated and discussed, to be sure, but outside the scientific community and often in ways that lacked science's disciplined interplay of experiment and theory, of proposition and test, of claims made by individuals and assessment and counter-claims by competent others. Anyone seeking to learn about these subjects had to wade through selfpublished monographs and pamphlets, non-refereed and often short-lived and hard-to-find magazines, and media coverage catering to the lowest common denominators of sensationalism, "entertainment", or gossipy "human interest". The *Journal of Scientific Exploration* has the task of bringing disciplined consideration to bear on these matters.

The Society and its *Journal* have acquired an additional role that was not, I believe, foreseen at their founding: to promote consideration of heterodox views about matters already broached within the established scientific disciplines. Astronomy has been unwilling to consider seriously the indications that redshifts may be somehow quantized, or the accumulating evidence that the redshifts of important classes of objects are not measures of their speed (and thereby not measures of distance either). Electrochemistry and nuclear physics have pronounced the evidence for low-energy nuclear interactions ("cold fusion") to be a matter of "pathological science". Mainstream work on AIDS is restricted to those who accept the dogma that HIV is its sole, necessary and sufficient cause. Plate tectonics became accepted after 40 years of resistance to Wegener's notion that continents could move, and has subsequently become a dogma in its own right, not to be assailed in mainstream publications.

That last example illustrates a general point made by Bernard Barber in his classic discussion (1961) of resistance by scientists to scientific discovery: In science, "objectivity is greater than it is in other social areas, resistance less.... Nevertheless, some resistance remains... As men in society, scientists are sometimes the agents, sometimes the objects, of resistance to their own discoveries".

Referees and Editors

Disciplined consideration means calling on competent experts to judge submitted manuscripts. But all experts have their own beliefs, strongly influenced by the prevailing paradigms. Inevitably, novel contrarian claims will often be judged by the experts to be unworthy of publication.

Those remarks apply to the *Journal of Scientific Exploration* as to any other refereed periodical. As reviewers the *Journal* does try to choose people whose minds are open—relatively speaking!—to the possibility that unorthodoxies might be valid or useful. But as the quote from Barber recognizes, none of us can be entirely free of bias toward what we believe to know. Moreover, as the popular aphorism has it, a totally open mind would let the brain fall out; or, as G. K. Chesterton (1936) understood, an open mind has the same function as an open mouth, namely to shut itself again on something solid. Every potential reviewer who knows *anything* will thereby have a bias against *something*. Recently I was led to suggest to an author that the prime *raison d'être* for our Society and its *Journal* could be interpreted as providing a forum for *scholarly* anomalistics—what might be called *mainstream* cryptozoology, ufology, and parapsychology. There may then be unorthodoxies in those fields to which—

history may later show—we gave too short shrift.

So, what's an editor to do?

Reject It or Send It out for Review?

The Editor reads each submission and has to decide whether or not it should be sent out for review. On what grounds might the Editor decide to reject without further review?

Over the two years that I have edited the *Journal of Scientific Exploration,* the ratio of rejections to acceptances has been roughly 2 to 1. Among the rejections, about 60% were by the Editor and the remainder upon advice of referees. (In some cases, the rejection was not an outright one but rather we asked for revisions before bringing referees in; typically, however, those revisions were never made.)

"But how", asked one disappointed author, "could the Editor use his personal judgment when the range of topics covered by the journal is so vast?"

Because the *Journal* is intended to be *read* by its subscribers, who on the whole do not expect to find in it material that is so arcane, or so confusingly presented, that only a few individuals (at most...) could make head or tail of it[1]. One good suggestion for how referees should be chosen, made to me as I was becoming Editor, is that there should be at least one specialist referee and one generalist; the first to ensure the competence and quality of the contribution, the second to ensure that the piece would be understandable by a high proportion of the *Journal's* readers. In practice, the Editor functions as the generalist reviewer (except, of course, in the rare event that a manuscript is in his area of technical competence).

Now of course I do not claim to be able, any more than anyone else, to judge a manuscript without bias. But I am at least aware of my fallibility. Over the years I have told many people of perhaps my greatest discovery as a result of participating in the Society for Scientific Exploration: that highly competent and intelligent people can hold views that seem to me ill-founded, and that *therefore my own opinion on those matters may be misguided.*

So as far as is humanly possible, I don't judge a manuscript before I've read it—difficult though that may be at times, given the titles that some authors choose for their submissions. I also don't prejudge manuscripts according to who the author is (unless, of course, I am already familiar with the work that author produces). For example, I found fault with Velikovsky's science on its (de)merits (Bauer, 1984), not because Velikovsky happened to be a psychiatrist writing about planetary events (instead of being an astronomer or at least a scientist). On an Internet discussion-group featuring geology, the article about plate tectonics we had published *(Journal of Scientific Exploration, 14,* 2000, 307–52), by David Pratt, was criticized because of Pratt's views about Theosophy as revealed on a Web-site; my invitation to participants in that news group was that they send, for publication in the *Journal,* critiques of *the contents of the article* instead of *ad hominem* remarks. To date, no such critique has been received. (Pratt's manuscript had received mixed reviews, and I solicited more than the usual number of informed opinions before accepting it for publication. Those who had advised against publication were invited to have some or all of their comments published together with the article itself, but declined the opportunity.)

In other cases too I've followed my belief that this Journal should bend in the direction of publishing controversial material so long as the evidence and logic and literature citations seem sound. As I argue in my recently published book, *Science or Pseudoscience* (2001), accomplishments in science or other personal credentials of those who make anomalous claims are not a good guide to the possible validity of those claims. People new to a discipline sometimes make great advances; on the other hand, people long versed and highly accomplished in a field sometimes go sorely wrong, as with N-rays. For my part I expect others not to reject my opinions on other subjects just because I hold the belief, to them absurd, that Loch Ness Monsters are real animals. Similarly, I don't reject a manuscript just because its author is a Theosophist, a creation scientist, or holds any other beliefs that I happen not to share.

In summary: I reject a manuscript if it does not make a plausible case about something interesting within the purview of the *Journal,* or if it adds nothing to already available discussions. For example:

- Some rejected manuscripts describe the author's overarching, all-inclusive, all-explaining world-view, an ontology and its set of relationships about the material, mental, and spiritual aspects of all that exists.

  While that may be genuinely useful and satisfying to the author, it is quite unlikely to be so to others; especially in absence of discussion as to how this world-view relates to what wise people over the ages have had to say on these matters. Such a discussion would need to be a large book, not an article.
- Some rejected manuscripts have offered unorthodox explanations for accepted facts without demonstrating that those explanations are at least as useful as the current paradigms, and without exposing a logical chain of reasoning by which that explanation could be arrived at.

  Long ago and in another country, one of my scientific mentors was presented by an acquaintance with the revelation that the smallest units of matter are vortices in the shapes of prehistoric fish. The author of this insight was able to explain very many chemical facts in this way—but none that then-current atomic theory could not also explain. As to how he came to the insight in the first place, he said he had dreamed it just as Kekulé had dreamed that benzene molecules are rings. But Kekulé's vision, it should be remembered, was accepted only after material support for its validity hadbeenadduced.
- Some rejected manuscripts offer novel results whose experimental or observational provenance is obscure or incomplete. The experimental approach may be described in considerable detail while its rationale remains mysterious.

To construct a fictional example: If one takes 6 red, 5 blue, and 4 green sticks and arranges them in a particular manner, the temperature in the space so enclosed will rise. The manuscript includes tables of temperature rises obtained at various times, accurate to one-hundredth of a degree, and a statistical analysis showing that this difference from ambient temperature is significant at $p > 10^{-6}$.

However, no experiments are reported with different numbers of sticks, or differently colored ones, or with different geometries. There is no explanation of how the number and nature and type of arrangement of sticks was arrived at.

- Some rejected manuscripts combine novel results of doubtful provenance with unorthodox explanations that lack logical provenance. Or, a variety of existing unorthodoxies are brought simultaneously to bear: for example, Kirlian photography might be used in support of self-reports of UFO abduction and explained on the basis of Velikovsky's electromagnetic *Cosmos Without Gravitation.*

It Is Not Easy

If there is any unifying thread among these classes of rejections, it is that the submitted manuscript is the result of insufficient work, be it in gathering data or in working out its implications or in taking into account the relevant published results or theories of others. The fact of the matter is that creating new knowledge is in no way easy. When it comes to scientific anomalies, it may well be even more difficult to produce useful work than it is in mainstream science. In the latter case one has much to build on and many colleagues to call on for help; both are largely lacking in anomalistics.

At our 20th Annual Meeting, a panel of people with quite varied intellectual backgrounds discussed "creativity". It struck me as worth noting that all the speakers agreed that it is not the having of an idea that represents success, it is the development of that idea that is crucial. One illustration of that is Stigler's Law (1980): "eponymy is always wrong"; or, "a discovery is named after the *last* person to discover it, because once a discovery has been named, no one else claims it as a discovery" (Cohen, 1992). Those who merely mentioned or suggested something are often forgotten whereas those who worked out its applications and implications and connections are assigned credit by posterity.

Some illustrations of the lack of impact of mere ideas are the "partly baked ideas" (PBIs) that I. J. Good gathered in columns of the *Mensa Journal* and *Mensa Bulletin* between 1968 and 1980 (Good, 1994), for instance:

- What would be the nature of a discipline that would do for logic what logic seems to be doing for mathematics?
- **A** tautology can be misleading and a logical contradiction can be enlightening.
- Consciousness and matter are equally metaphysical.

These are intriguing, provocative ideas. They raise deep questions. But without being taken any further than that, they are frustrating more than enlightening; they hardly advance knowledge or understanding. (Somewhat more developed PBIs can be found in Good's 1962 collection, *A Scientist Speculates*.)

So far as the *Journal of Scientific Exploration* is concerned, some PBIs might perhaps warrant publication in the form of brief letters, but certainly not as full articles or essays.

Reject or Accept the Referees' Judgments?

An editor's responsibility to exercise judgment does not cease when a manuscript is sent to reviewers. There is no law that the reviewers' judgments must be accepted. Many members of the Society for Scientific Exploration will have in their files copious illustrations that editors should have overruled reviewers who got their facts wrong, offered their own interpretations as the only legitimate ones, vented personal spleen, and so on. One of my early papers in electrochemistry was turned down because I suggested that a certain parameter had a certain value and the referee refused to believe that possible; he overlooked that I had cited the value from the published work of a highly respected researcher, work that had never been contradicted. One of my most recent papers was welcomed by the editor if only I would shorten it by a third and meet the many comments from two referees; I did so conscientiously, whereupon a third referee voted it unequivocally down and the editor accepted that despite his earlier encouragement.

So I am fully aware of the need for editors to be as critical in reading referees' comments as the referees are (or should be) in reading the manuscripts. However, referees are a valuable asset to a journal, which is the chief reason for sending for review only those manuscripts that seem likely to be publishable; I don't want to waste the good will of our referees by asking them to spend time on unworthy material. For the same reason, an editor goes severely against referees' recommendations at his peril. But since in disputed cases I always offer the referees to publish their dissenting comments simultaneously with the article, I am able to be somewhat diplomatic even when I reject their rejection of a manuscript. (My standing offer is that dissenting comments can be published verbatim as coming from an anonymous referee; or disguised by paraphrase, as an editorial comment; or, of course and ideally, under the referee's name.)

In summary: I regard the interplay between author, referees and editor to be a conversation, with the editor as the inevitable final arbiter of which side has made the more convincing case. No matter how felicitous the choice of reviewers has been, their decision is not the final one. It is similar to the use of expert outside consultants when universities are considering someone for promotion or tenure: The consultants' opinions are valuable information for those who must make the decision, but they should not in themselves constitute that decision.

The Larger Context: On Reforming Science

This editorial essay is intended to inform readers and authors about my policies and practices, and it is an invitation to comment on them (be it for publication or for the Editor's private enlightenment). The impetus to make this an essay rather than an editorial came as I was reading a call for the reforming of science, made urgently and convincingly by one whose competent but unorthodox articles have consistently been resisted by specialist mainstream scientific journals. Such calls for reform are not infrequent; yet I think they are usually misguided, because:

- Hard cases make bad laws. I can conceive no system of making judgments that could work well for both the mass of journeyman science and also for the occasional genuinely revolutionary stuff.
- No matter what institutions science evolves, mistakes will sometimes be made. No matter what institutions unorthodox science or anomalistics evolves, mistakes will sometimes be made.
  "The consensus of rational opinion" is the best approach to objectivity, imperfect as that approach may be. Consensus is not the same as unanimity.
- It is highly desirable that science be reliable; and the reliability of science is greater when its mistakes err on the side of conservatism. (That is perhaps most obviously true in medical science, where drugs approved too readily have killed some people even as proof is lacking that others' lives have actually been prolonged. Statistics are available but not certainty.)
- The occasional call to allow publication without refereeing is misguided because there are too many incompetent would-be authors. Such journals as practice non-refereed publication deservedly enjoy a lesser reputation. Free publication via the Internet will, in due course, demonstrate whether it advances science, or whether it retards it by producing an indigestible mass of inferior material that researchers must sift individually instead of with the aid of referees.

I am not arguing, of course, that science now is functioning in the best possible manner and that no reforms are needed. Indeed, as I noted in one of my books (Bauer, 1992: 83–84), the state of science nowadays is comparable to that of the Church as the Reformation was getting under way:

> It seems to me not farfetched to compare the current state of science (and more generally that of academe) to the situation of the Church at the time of the Reformation, which has been described in the following way by De Lamar Jensen: "Until the middle years... the actual number of clergy [read scientists] increased, but then a decline set in. Even before the outbreak of the... revolt, their prestige and influence were already waning. Whether justified or not, the general population's growing disrespect for the clergy [scientists], especially the monks [researcher-scholars], tended to weaken some of the bonds of the Christian [scientific] community and make the church [scientific institutions] as a whole more vulnerable to criticism and attack. It had not been above criticism in earlier ages, but now it was becoming the practice rather than the exception to

blame the institution as a whole, along with individual members of it, for infractions... of law and... ethics. As... abuses increased, the recognition and condemnation of those abuses mounted proportionally. To compensate for their declining prestige, many clergymen [scientists] became even more avaricious [asking for ever lower teaching loads, higher salaries, freedom to consult and to found business enterprises; ignoring conflicts of interest], and the growing chasm between the priesthood [scientists] and the laity, and between the higher and lower clergy [administrators and practising scientists], widened."

But the Reformation did not reform the Church: it led to schisms and smaller competing entities where before there had been a single authority. Perhaps we are now seeing an analogous development in science. Medicine is attending increasingly to "alternative" treatments and theories. Radically extreme feminists and other social activists are asserting that personal, political, and professional activities are inextricable (Bem, 1998: ix), thus ditching the very hallmark of traditional science, its objectivity. Social constructivists taking a similar view have gained hegemony in "cultural studies", "science studies", and much of the social sciences. At the same time, mainstream science arrogantly ignores qualified members of its own community who have unorthodox insights to offer on central issues—cold fusion, redshifts, HIV/AIDS. There are grounds here for agreeing with Jacques Barzun's description of Western culture as decadent: we accept futility and absurdity as normal (Barzun, 2000: 11).

Perhaps the most basic question about possible reform of science is, "How rapidly could human understanding progress under the most favorable conceivable conditions?"

The history of science offers ample illustration that the time needs to be ripe for any given advance to carry the day. It may be that more data are needed, or a new means for obtaining data, or a new way of looking at the data, but none of those alone is likely to bring a great breakthrough, and so change in any of them cannot go too far without waiting for consonant change in the others. Somehow these three aspects of science need to advance in a somewhat coordinated manner. As I've suggested elsewhere (Bauer, 2001: 9–11), most scientific work adds detail without upsetting the existing body of data, methods, and theories. Scientific revolutions involve something strikingly contrarian in only one of those three aspects. When two of these aspects are brought simultaneously into question, as Mendel or Wegener did, the rest of the scientific community cannot assimilate it or catch up with it for decades. And when all three aspects are brought into question at the same time, we have questions of the kind that this Society was founded to grapple with.

I think that the Society for Scientific Exploration is an embodied illustration of the only feasible way in which science can in practice be reformed: through providing forums in which disciplined discussion takes place of the issues that are given short shrift in the mainstream disciplines because of the hidebound but necessary conservatism of mainstream institutions. I emphasize *disciplined* discussion, which is surely what we mean by *scientific* exploration. The paradox cannot be avoided, it seems to me. Disciplined discussion means gatekeepers: peers, referees, editors. Sometimes they will make mistakes; most frequently in the hardest cases. My understanding of this cannot prevent me from making mistakes. As Editor, the most I will ever be able to do is to apologize for my mistakes. But that could only be after the fact, when some individual has already been damaged without redress. In the meantime, I hope this recognition and admission of fallibility may serve.

Note

[1]However, I believe that the *Journal of Scientific Exploration* should also serve as a publication avenue of last resort for apparently competent work unable to find mainstream publication, so occasionally it publishes articles of a highly technical or mathematical nature.

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Ziman, J. (1968). *Public Knowledge: An Essay Concerning the Social Dimension of Science.* Cambridge: Cambridge University Press; especially Chapter 1, "What is science?"