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EDITORIAL

I think I now understand why Gene Fowler once said, "An editor should have a pimp for a brother, so he'd have someone to look up to." That unflattering sentiment about editors isn't nearly as uncommon as I'd thought before taking on the job of *JSE* Editor-in-Chief. And I can see why; people in my position have many opportunities for making others unhappy. In fact, because the *JSE* is such an unusual, cutting-edge publication, those opportunities may be especially plentiful. So although I don't want this to become a recurring theme of my editorials, I feel that a few more remarks on editorial business and peer review wouldn't be out of place.

I mentioned in the last issue that my Associate Editors and I occasionally reappraise papers that were previously rejected. That can happen for various reasons. For example, in the case of complex, technical, or less than ideally clear submissions, reviewers can misinterpret what they've read, and authors are quick to point that out. But sometimes it's because the submission's initial review may have been hasty, superficial, or even prejudicial. Now make no mistake: I trust the folks on my editorial team and I don't believe these infrequent cases reveal anything sinister about them or about the review process. As any teacher knows from grading essays, no matter how scrupulous and fair you try to be, sometimes things just rub you the wrong way, and sometimes (probably more often than we'd like to admit) our critical faculties aren't as sharp as we'd like. These lapses can happen to the best of people, and we try to be alert for them and honest about our fallibility. In fact (as I've mentioned before), we are especially alert for the kinds of negative reactions that can all too easily be elicited by works in areas of frontier science.

However, a number of disappointed authors have proposed to me that we make it a policy to re-evaluate submissions, always allowing the author the opportunity for appeal. I haven't yet decided if I oppose that idea in principle, but I must certainly oppose it for practical reasons. It's simply not something we can afford to do as a matter of course. The main problem is that the *JSE* is a very specialized publication, and relatively few people are both technically competent and sufficiently open-minded to referee papers for it. So our pool of potential reviewers is quite limited, and we often have great trouble finding people qualified and available to evaluate submissions. In fact, the *JSE*'s valiant (and unpaid) Associate Editors and reviewers are overloaded as it is. To routinely re-assess papers we reject just because the authors disagree with the judgment would strain our system (and my team) to the breaking point.

I also receive more than occasional complaints from readers who are outraged that a particular article appeared in the *JSE*'s pages. Sometimes they object to the topic of the paper, and sometimes they complain about the way the topic was handled. I'm frequently puzzled about the former sort of complaint. If the reader has such a strong reaction to a topic (s)he considers too disreputable to be covered in the *JSE*, this would seem to be someone who doesn't quite get what the journal is all about. The latter sort of complaint often displays a different kind of shortcoming—namely, a failure to understand the nature and function of peer review. For example, last year a reader was moved to write: "I can't believe a paper with such faulty logic could be published in a peer-reviewed scientific journal. Don't you think saying things like this based on their lame evidence is totally nonsensical?" Then, after quoting a remark which out of context looks much more questionable than when read in context, my correspondent asked: "Why doesn't this demonstrate that your guys' peer review is a joke? How in the world can you possibly justify publishing such a shoddy paper?"

Let's ignore for now whether the criticism of the article is justified. In fact, let's suppose it is justified. Even so, the complaint about peer review misses the point by several miles. I don't know of any journal for which the peer review process is flawless. But more important, peer review never guarantees that only worthy papers and books are published. If that were the case, we'd see far fewer publications across the board. Many journals would go out of business, publishers would probably remainder far fewer books, and many Ph.D. or academic tenure candidates would find their futures jeopardized by painfully skimpy publication lists. And as I mentioned in the previous journal issue, although I don't always concur with the decisions of my Associate Editors and their readers, I'm strongly committed to the view that reasonable and informed people can always disagree. Moreover, the *JSE* doesn't exist merely to promulgate the views of the Editor-in-Chief or some oligarchic body behind the scenes. Among other things, peer review is supposed to guard against editorial tyranny; but it's never been conceived as a guarantee of quality.

In fact, there's a parallel here with what some have said about inductive reasoning. Unlike deductive reasoning, induction doesn't guarantee true conclusions from true premises, no matter how massive our body of evidence may be. But we needn't lapse into Humean skepticism and insist that induction is rationally indefensible. As Herbert Feigl and Hans Reichenbach noted years ago, even if we agree with Hume that induction can't be rationally justified (as providing guaranteed good results), we can at least vindicate induction. Their general idea was that inductive reasoning is better than—or at least as good as—any alternative method of a posteriori reasoning. So if empirical truth is to be attained at all, induction is as likely as any method to get it for us. From this perspective, induction will disappoint only if we're engaged in a quixotic foundationalist quest for final or absolute justifications.

Analogously, and I think plausibly, one could argue for the vindication of peer review. Given the breathtaking varieties of human fallibility, peer review will never guarantee that only the best works, or even just decent works, get accepted for publication. However, if the evaluation process aims to filter out for publication works that deserve attention, peer review is probably better than—or at least as good as—any alternative method of achieving that result. We'll find it unsatisfactory only if we naively look for a surefire reliable method of assessment.

I must emphasize, however, that I'm confident in my superb and hardworking team of Associate Editors, and I believe we have a very loyal, responsible, and thoughtful stable of referees on whom we can rely. In fact, I'm personally pleased and satisfied with the way the *Journal* maintains a high standard in accepting papers for publication, even in cases when my opinion differs from that of my Associate Editors or reviewers. No doubt the quality of *JSE* articles is not uniform. I know of no publication for which that's the case, and in fact I think it would be miraculous if it occurred. What matters is that *JSE* articles are regularly (not uniformly) of high quality.

One more observation on this general topic. Because of the *JSE*'s commitment to providing a forum for speculation and data that more mainstream publications tend reflexively to shun, our editorial team often finds itself in a quandary. For instance, we want to be open-minded about airing novel scientific proposals, but quite a few such submissions nevertheless still lack a reasonable amount of theoretical development, empirical grounding, or engagement with competing points of view. Understandably, the less egregious of these sometimes teeter on the border of acceptability, and editorial decisions in such cases are always tough calls to make. That's why in these borderline cases we may invite the authors to resubmit after substantial revision.

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One last note, on a much different topic. As I hope all readers have noticed, some of our book reviews have been of works that are quite old. For example, our previous issue contained a review by Carlos Alvarado of Charles Richet's 1922 *Traité de Métapsychique.* That's not because Richet's publisher was slow to provide us with a review copy. On the contrary, since at least 2002 the *JSE* has occasionally published reviews of important older books, and this will continue to be a recurring (if not regular) feature of our book review section. David Moncrief (our book review editor) and I share Carlos's hope "that these reviews may bring the old material to the attention of current researchers and to the new generation" (personal communication). It's all too easy to dive into the study of scientific anomalies with little or no awareness of the often sophisticated theoretical and empirical work that's already been done. I've seen this many times in parapsychology, where newcomers to the field think the only qualification for doing first-rate work is competence in some related area of mainstream science.

At the moment, the reviews of older books in our editorial pipeline all concern important theoretical and empirical research in parapsychology. But that's just a temporary and unplanned state of affairs. In the past, reviews of older material have been in areas other than parapsychology. In any case, I want readers to know that this is a general project we will continue to pursue. The *JSE* will continue to publish occasional reviews of older seminal works in various areas of science relating to the study of anomalies.

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