

EDITORIAL

In 2010, I wrote a pair of editorials dealing with issues concerning peer review and the quality of papers appearing in the *JSE*. While I'm not so naïve as to think that my editorials exert any great influence (or even that *JSE* subscribers actually read them), I'm nevertheless a bit surprised to find—five years later—that I still receive a fairly steady stream of complaints about our peer review process. Those complaints fall primarily into two broad categories: (1) charges of rigidity, bias, or tyrannical censorship from authors whose papers were rejected, and (2) complaints from readers who believe that papers appearing in the *JSE* should never have survived peer review.

So I'm thinking it's time to review the issues again. And since I don't believe I can substantially improve on what I wrote five years ago, I offer below, in a spirit of unjustified optimism, my two earlier editorials for your (re)consideration.

From *Journal of Scientific Exploration*, 24(3):393–396, 2010:

There's been a lot of chatter lately on Internet discussion groups to which I subscribe about the virtues (but mostly about the vices) of journal peer review. In substance, the commentary adds little to the ever-growing number of published or online discussions of that subject. And not surprisingly, it resembles the correspondence I receive from authors whose papers have been rejected by the *JSE*. Typically, the negative comments are predictable and familiar complaints about how editors and reviewers tyrannically impose their prejudices on authors who express dissenting or minority opinions, or—even worse—who argue for novel (if not radical) points of view. In this way, we're told, journals reinforce the status quo and keep worthwhile scientific or conceptual innovation at bay.

Of course, censorship of this kind undoubtedly occurs, and some of the incidents recounted in listserves and published articles are horrific and infuriating. But these practices are also nothing new, and I wonder whether it wouldn't help to step back a bit, strive for some perspective, and in particular see if we can find some helpful analogies to the situation regarding peer review. It seems to me that peer review doesn't deserve the battering it often receives.

Ever since Plato's *Republic*, a standard criticism of democracy has been that at best it's inefficient, and at worst it puts important decisions in the hands of people who lack the competence to make those judgments. However, an equally standard rejoinder is, first, that there's no such thing as absolute competence to rule; even equally intelligent and informed people can reasonably disagree. Moreover, the alternatives to democracy are worse in crucial respects. G. B. Shaw once remarked, "Democracy substitutes election by the incompetent many for appointment by the corrupt few." What many want to say about democracy is that non-democratic

systems are inherently brittle, in the sense that a challenge to the ruling authority is (in effect) a challenge to the political system itself, and thus it can undermine the whole political structure. By contrast, democracies are inherently (if inefficiently) self-correcting. Leaders and their policies can be challenged and replaced without having to question or overturn the very system in which they have a place.

Perhaps an analogous series of arguments and counter-arguments can be made about peer review. Is it fallible and vulnerable to abuse? Of course. Can editors and reviewers behave badly or merely exercise poor judgment? Of course. In fact, nothing can be used exclusively for the good, and humans seem to have an inexhaustible supply of disappointing behaviors.

But, as in a democracy, peer review allows for checks and balances, and avenues for appeal. The review process is flexible and potentially self-correcting, and so hasty judgment or instances of outright abuse don't undermine the process itself. Naturally—in fact, clearly—some journals are more editorially myopic, unscrupulous, or cowardly than others. But I can assure our readers that *JSE* editors and reviewers take their responsibility and their commitment to openmindedness very seriously. That's never a guarantee that our biases don't sometimes cloud our judgment, and in fact it's impossible to assess a submitted paper from no point of view whatever. But I can tell you that at the *JSE*, we're particularly alert to this, and in fact rejected papers *are* sometimes reappraised (usually by different readers) and then accepted. Indeed, we recognize that this sort of flexibility is essential in a journal devoted to controversial topics outside the mainstream. But let's not stop with examples from political theory.

In my noble quest for analogies, the following episode from the history of philosophy also occurred to me. In his *Principles of Nature and Grace*, Leibniz famously (though some say, insincerely) claimed that this is the best of all possible worlds. Now as students of modern philosophy know, that claim isn't as optimistic as it sounds. It's rather like saying: If you think this world is bad, you should consider the alternatives. For the case at hand, it's like saying, if you think a world with Steve Braude as *JSE* Editor-in-Chief is bad, imagine it instead with [and then fill in the blank with your favorite tyrant—unless, of course, that would be me].

In fact, Leibniz seemed to think that in the best possible world, some evil is actually inevitable. For Leibniz, the best possible world was one that contained the greatest *surplus* of good over evil. Perhaps a world with no evil is not even a possible world. But even if it is possible, Leibniz wouldn't have considered it as good as the actual world, because it wouldn't contain the greatest *surplus* of good over evil. And that's because, according to Leibniz, some of the greatest goods, such as free will, can't even exist in the absence of certain evils; those goods and evils are necessarily connected. (The necessity here would be stronger than mere empirical necessity: It would be metaphysical or logical necessity.)

For a somewhat down-to-earth example of the sort of relationship Leibniz had in mind, consider the good of satisfying one's hunger. Clearly, the hungrier one is, the greater the good of satisfying that hunger. So the great good of feeding the starving can't occur without the evil of their having suffered great privation. Of course, in the case of free will, the issue is that the great good of human freedom must allow both for the freedom to do good as well as evil, or to act reasonably as well as rashly.

Although this might be stretching it, perhaps there's an analogy here with the

journal peer review process. Perhaps the best possible journal would *not* be one in which editorial prejudice never exists or in which editorial misjudgments never occur. In fact, so long as fallible humans have anything to do with the editorial process, it's plausible that an error- or prejudice-free editorial board and journal are *not* possible (at least not empirically possible). So perhaps the best possible journal, editorially speaking, will be one containing the greatest surplus of fair and reasonable editorial decisions. And perhaps the existence of prejudice and poor judgment is a necessary correlate of having humans do the work. If so, complaining about peer review because the process can be unreasonable or unfair would be analogous to complaining about the existence of free will because it allows for evil. Interestingly (and more or less as an aside), Leibniz seemed to think (or at least he claimed) that his position solved the notorious *problem of evil*: the alleged incompatibility of evil with God's existence. (Roughly, the idea behind the problem is that if God is omnipotent, omniscient, and benevolent, He would anticipate and prevent evil from occurring. Hence, since evil exists, it follows that there is not an omnipotent, omniscient, and benevolent God.)

However, according to the Leibnizian view sketched above, the existence of evil did not count against the existence of God. Quite the contrary; from Leibniz's standpoint it was an indication of God's greatness. Evil would simply be an unavoidable side-effect of God's actualizing the best of all possible worlds. However, as Bertrand Russell once observed, Leibniz's reasoning here is less than compelling. One could just as well claim that this is the worst of all possible worlds, created by an evil demon, and that good things exist only to heighten the evils. So one could argue that the evil demon created us with free will in order to ensure the existence of an excess of sin, and that the demon created good people so that there could be the great evil of their suffering.

Now this might really be stretching it, but I suppose that one could argue that some particular journal is the worst of all possible journals (not the *JSE*, of course), in the sense that it maximizes the amount of editorial abuses over editorial good. *JSE* readers will probably be ready with some likely candidates for that honor. And perhaps the existence of such a journal could even be cited as evidence for the existence of an evil publisher or managing editor who created or uses the journal precisely to suppress or deny certain points of view. One obvious nominee comes immediately to my mind (and I'll wager to those of many readers).

Ironically, however, when it comes to the journal I have in mind, defenders of its editorial policies and practices actually follow Leibniz's lead and claim that what others consider editorial error or abuse is actually a manifestation of editorial greatness. That is, they would say that it's exactly what journal editors heroically must do in order to protect and promote what they consider (or "know" to be) the truth, and strive to shield unwary readers from the subversive and dangerous influence of irrational or stupid ideas.

So let me be clear; I don't endorse that cynical assessment of editorial rigidity and censorship. Granted, the *JSE* does have an agenda—namely, to give a proper airing to scientific data and theory which more mainstream publications ignore or treat shabbily. But the journal doesn't exist to advance or exclude any particular point of view or set of data. What matters to the *JSE* are *conclusion-independent criteria* of scholarly and scientific integrity. In fact, that's why we often publish papers with which my Associate Editors or I disagree. Still, the next time an irate or disappointed

author complains to me about the negative judgment rendered over a submitted paper, perhaps I shouldn't be averse to giving the more cynical position a try.

From *Journal of Scientific Exploration*, 24(4):577–580, 2010:

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.

—STEPHEN E. BRAUDE