



future. The quality of undergraduate education? The future of graduate study? Faculty teaching loads? Funding for higher education? The dignity of teaching? The massive shift to part-time employment is at the center of everything we do.

But for the first time a national movement is taking shape. Meanwhile, too many tenured faculty think the best way to address these issues and win benefits is to wait patiently outside the president's office hoping to press a wet nose into his palm as he heads to or from work. Administrators will grant nothing on their own initiative. You will win what you take.

CW

See *academic freedom, the corporate university, faculty*.

Peer Reviewing (pi(ə)r rəv्यू'ing) Peer reviewing is the system of evaluation that protects academic independence and assures a level of disciplinary consent about scholarly publication. It operates in several contexts. Scholarly journals send out essays submitted for publication to other faculty members for evaluation; the names of the evaluators are kept secret. Academic presses do the same with book manuscripts, and agencies like the National Science Foundation and the National Endowment for the Humanities follow the same procedure with grant applications. Universities use peer reviewing to assess candidates for promotion and tenure. Some journals withhold the names of authors from reviewers, though the names of book authors and promotion candidates are always revealed. Peer reviewing is not a perfect system, since it uses fallible human beings to choose reviewers and equally fallible human beings to make judgments about another faculty member's work, but it is the only system we have. Nonetheless, intellectual bias, prejudice, jealousy, ignorance, and sometimes outright malice can intervene to produce faulty evaluations. The system thus requires considerable oversight if its reliability is to be maximized. Negative reviews should always be carefully investigated for possible bias. Peer reviewing needs to be peer-reviewed.

But peer reviewing also needs to be properly conceptualized. Academics like to think of it as an *objective* process, and that it is surely not. It embodies and speaks for disciplinary consensus, which reflects the historical, social, and political limits of current thought, not transcendently objective truth. Groundbreaking work that challenges the taken-for-granted "truths" of the discipline is notoriously difficult, though not impossible, to



publish. Peer reviewing is not only a mechanism for evaluation but also a mechanism for control. It rewards certain forms of thought and discourages others. If it helps guard against fraudulent work and thus safeguards disciplinary integrity, it also encourages intellectual compliance and safeguards the prestige of dominant opinions and paradigms. As we point out in the entry on *promotion reviews*, it can be a weapon that unscrupulous academics in power can use to destroy people's careers.

The most important scholarly analysis of peer reviewing is surely the study published by Peters and Ceci in 1982. It is an essay every professor in every discipline should read and keep in mind throughout their careers. It is the result of a daring experiment whose results few would have predicted. The authors picked twelve distinguished psychology journals from a variety of subfields. Each journal followed the practice of leaving authors' names and institutions on essays when they were sent out for evaluation. From each journal Peters and Ceci selected one essay published in that journal over the last two or three years.

The investigators then *retyped* the twelve published essays, gave each a new title and new authors, using fake names of the same gender, and invented institutional names that would have no high status identification. So a published essay by a Harvard University faculty member would now be submitted from, say, "Tri-Valley Center for Human Potential." Peters and Ceci then resubmitted each retitled essay to the same journal that had already published it. Of the twelve essays, only three were recognized as resubmissions. Of the nine essays that were then peer-reviewed, *eight* were rejected by the very journals that had already published them. The editors and associate editors of the journals agreed with the reviewers' evaluations.

It is difficult not to conclude that institutional prestige has something to do with successful navigation of the peer reviewing system. The actual content of the essays themselves hardly seems to have been decisive. Indeed the most frequent ground for rejection was "serious methodological flaws," a criterion that certainly *sounds* objective. But these essays had already been found to be methodologically sound. None of the reviewers of the eight rejected essays said anything like "adds nothing to the existing literature," which might suggest the original publication had already entered the discipline's knowledge base. The Peters and Ceci experiment is published with a large number of comments from other scholars, many of which are interesting. But a few revealing notes creep in: of course psychologists are unscientific, but *my* discipline (physics, chemistry, whatever) is objective; or, why shouldn't essays from Harvard count for more



than essays from Kansas State? The sample used in this experiment is small, but the results are still suggestive.

They reinforce our own experience in the humanities, in which we find that acceptance or rejection of essays often depends on whether the approach and conclusions match the peer reviewer's assumptions. Peer reviewing of single-author book manuscripts, as opposed to anthologies, we find more consistently reliable, perhaps because reviewers reading a whole book manuscript are more likely to credit the author's aims and methods, rather than just imposing their own. When one of us several years ago submitted the manuscript of *Marxism and the Interpretation of Culture*, a large collection of essays from a groundbreaking conference, two readers from different theoretical perspectives accepted or rejected essays largely on the basis of their own theoretical commitments. Since one was a traditional Marxist and one a poststructuralist, they had almost no points of agreement, though both did want us to drop what would eventually become the most influential essay in the book. On the other hand, we have both had wonderfully helpful peer reviews on single-author books.

See *promotion reviews, scholarly books*.

Ph.D. in the Side Pocket (pē lāch lāē in thə sīd 'pākət) Active researchers in departments of kinesiology or exercise physiology conduct sophisticated studies of respiration, muscle movement and mass, and so on. Such professors help fund their students through research grants and fellowships, and of course their departments support graduate students through teaching assistantships. What sorts of courses do these students teach? Bowling, canoeing, tennis, even billiards and Ping-Pong (table tennis, sorry). Professors thus need students with formidable backgrounds in science and statistical methods, while departments need instructors with a mean backhand, expertise as a kegler, or the ability to hustle fish out of their paychecks at the local pool hall. Sometimes these two competing needs lead to considerable discrepancies in the abilities of newly admitted graduate students.

It shouldn't come as a big surprise to anyone that many brilliant students in kinesiology wouldn't know a masse shot from a kayak paddle. So, when asked by an admissions committee to list all the sports and games they are competent to teach, many do the only thing they can if they hope for a career in the field: they lie a little. And they hope their impressive transcripts, high GRE scores, and "rave" letters of recommendation will

From January to August of last year, John Bohannon submitted an academic study to 304 peer-reviewed scientific journals. All of the them were open access journals, a newer breed of digital-only academic publications that are free for readers but often charge researchers to publish. Bohannon's study concerned a molecule, extracted from a lichen, that appeared to show promise as a treatment for cancer. It was accepted for publication by 157 of the journals—slightly over half.

There was only one problem. Bohannon isn't a scientist; he's a journalist. And he completely made up the study.

Actually he did more than that. He deliberately inserted unscientific material to test whether or not it would be caught by the journals' peer reviewers. The "cure for cancer" proposition of the study, for instance, should have seriously raised some eyebrows, though in 157 cases, it did not. A bit subtler, though as much a red flag for scientists, was Bohannon's claim that the lichen-based molecule could be used as a treatment for humans, though it hadn't gone through a clinical trial. This, too, was missed by half of the journals. What little feedback Bohannon did receive had more to do with the formatting of his manuscript than the content itself.

Bohannon's sting operation was not the first of its kind. There are many more examples of fake

studies getting published in academic journals. Taken together, they point to serious flaws in academic publishing at large: Journals are too eager to publish surprising studies, and the rigor of peer review is faltering.

Though Bohannon (or his editors at Science, the preeminent American scientific journal where he published the details of his hijinx) blamed open access journals, fake (or at least extremely dubious) studies have also been published in traditional, subscription-based journals.

STING! Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity In 1996, the mathematician Alan Sokal famously published a piece of utter nonsense in the peer-reviewed, postmodern studies journal *Social Text*. His attack was focused more on the humanities than academic publishing overall, but it still makes peer review look pretty weak. Here's an excerpt:

But deep conceptual shifts within twentieth-century science have undermined this Cartesian-Newtonian metaphysics; revisionist studies in the history and philosophy of science have cast further doubt on its credibility; and, most recently, feminist and poststructuralist critiques have demystified the substantive content of mainstream Western scientific practice, revealing the ideology of domination concealed behind the façade of "objectivity."

STING! Independent, Negative, Canonically Turing Arrows of Equations and Problems in Applied Formal PDE

In 2012, the mathematician Marcie Rathke created a study using a tool called Mathgen, which randomly generates math research papers (all of which are complete hogwash). It was accepted for publishing by the open access journal *Advances in Pure Mathematics*. The study was never published, though, because the journal wanted to charge a \$500 fee.

DUBIOUS STUDY! A Bacterium That Can Grow by Using Arsenic Instead of Phosphorus In 2011, the journal *Science* came under fire for publishing a study that, though it wasn't a sting, offered a conclusion so dramatic that it could have been. The claim was that a bacterium was discovered that could use arsenic in its DNA instead of phosphorus, a radical shift in biologists' notion of the very building blocks of life. The paper made it through three peer reviewers, all of whom were uncommonly lenient given how revolutionary its claim was, an investigation would later reveal.