

On Peer Review

by

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two posts from his [blog](#):

Drop Errors and the Trouble with Peer Review

and

The Ethics and Epistemology of Peer Review

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Drop Errors and the Trouble with Peer Review

In product development there are two kinds of errors. A “go” error occurs when the green light is given to a product that eventually fails. The Edsel, a \$250 million write-off by the Ford Motor Company in 1959, is one example. The “drop” error occurs when an idea that could have been highly profitable is eliminated from further consideration. How do we know that the idea could have been profitable? In a free market dropped ideas have the habit of being picked up by someone else. Chester Carlson’s invention was dropped by such notables as General Electric, 3M, Kodak, RCA, and IBM, but picked up by the small Haloid Company. In 1961 Haloid changed its name to Xerox. Even go errors in a free market often get corrected; just a few years after the Edsel fiasco, Ford rolled out a better idea called the Mustang.

Peer review is the process by which millions of dollars of government money are handed out to researchers in medicine and the physical sciences; the process by which recognition, promotion, and tenure are determined for professors, especially those in the “softer” sciences who do not need or use grants for their research; and one of the criteria—numbers of peer-reviewed journal articles, for example—used to determine accreditation for universities.

Peer review, a “blind” process in which the names of author and evaluator are concealed from each other, requires two or three so-called peers to read a paper or proposal to judge the quality of actual or proposed research before acceptance. As such, peer review is a product development process that protects only against go errors. It is at best quality control that insures accuracy and reliability of research done. At worst it holds back innovation through drop errors. Since there is no free market in scholarly research—today’s government-university-science complex is a severely hampered market—dropped ideas either never get a hearing or take many more years than they otherwise should to surface.

Medical researcher and long-time critic of peer review, David Horrobin, argued that the peer-review process, which developed in its current form largely as a screening device after World War II, has perhaps improved the accuracy and reliability of conventional research published in medicine, but it has done so at the price of innovation. Prior to World War II, unknown researchers could submit papers to journals only with the endorsement of a published author. The editor would then decide whether or not to publish. Peer review was ad hoc and not common. It was the growth of government involvement in education and, especially, the government's lavishing of money on research that called for the blind-review screening process.

In a [paper](#) titled "The Philosophical Basis of Peer Review and the Suppression of Innovation" Horrobin urged that more unconventional and innovative research be encouraged by journal editors. When a reviewer questioned the need for such a statement, Horrobin produced eighteen incidences of medical innovations rejected by the peer-review process. In 1995 Horrobin's paper was cited by the [US Supreme Court](#) as support for the argument that some "well-grounded but innovative theories" may not be published in peer-reviewed outlets.

Horrobin's solution to divvying up grant money was to give funds equally to all researchers and let each work on whatever his or her interests indicated. Prior to 1960, said Horrobin, this interest-as-guide process was essentially how funding was distributed in the UK and more innovation in medicine resulted in those years than in the years since 1960. Horrobin approved of government involvement in and funding of research, but the analogy to free markets in his solution—bottom-up, self-interested choice by researchers—versus central planning—top-down, "expert" direction by peer reviewers—cannot be escaped.

Never mind that Socrates and Galileo were badly treated by their peer reviewers or that frauds and hoaxes sometimes dodge the quality controllers or, further, that you may want to cite Ayn Rand and Ludwig von Mises but can't figure out how to get past your peer-review gatekeepers,

the real problem of peer review is the severely hampered market in scholarly research. What would a truly free market in scholarly research be like?

First, publishers of journals and scholarly books would have to earn a profit from their buyers and not live off the donations of their authors or other benefactors. Some university presses, for example, are now publishing what are called "supported books," which means someone, usually the author's department, must contribute one or two thousand dollars to the publication of the author's book. And at least one commercial press requires authors to do their own copy edit and provide camera-ready typeset text; this can add up to two thousand dollars or more that authors must fund. Twenty-five dollars per page, charged to authors or their departments, has long been the going rate for published papers in some fields. (In some quarters today this method of getting into print would be called subsidy or vanity publishing.)

In addition, the so-called nonprofits, which finance a portion of today's research and journals, are in fact creatures of the tax system and must, despite their descriptive name, show an excess of donations over expenses lest their organization become some philanthropist's very expensive hobby. Under laissez-faire, in the absence of tax write-offs and the guilt and ignorance of economics that wealthy business people tend to exhibit, there probably would be far fewer such organizations than exist today.

Second, there would be no government money to dangle in front of researchers and no government-owned or -regulated universities filled with bureaucratized product lines (curricula designed by committee), bureaucratized sales reps (the professors), or bureaucratized performance evaluations (those mounds of paper, which include lists of published research, that must be produced for promotion, tenure, and most every other consideration). All of this distorts the market and probably encourages the overabundance of pretentious minutia that fills today's overabundance of academic journals.

Under laissez-faire, only the market would decide who produces what and who gets what in scholarly output. Indeed, the market for this research might not differ much from the product development market in automobiles. Private, profit-making firms, both traditional businesses and universities, would finance the work and effectively and efficiently produce market-satisfying results. Portions of the results might be published in profit-making journals and books, much of it perhaps not.

Yes, there might be some Edsels created by this free-market development process and there might still be some delayed acceptances of Xeroxes, but there also might be a lot more Mustangs! Absent the government-encouraged gatekeepers and other hurdles that must be jumped in order to get into the market, researchers who cannot find an outlet will be free to start their own journals, publishing companies, businesses, or even universities. The hampered market today, which includes the “golden handcuffs” of tenure, makes this quite difficult.

See related posts on the Mises blog: [1](#), [2](#).

Wednesday, October 24, 2007

The Ethics and Epistemology of Peer Review

In a previous [post](#), I argued that academic peer review is a gatekeeping process brought about by the post-World War II growth of government involvement in research and scholarship. Though it may control quality in a narrow, conventional sense, one significant consequence of this process is the suppression of innovation. The present post takes a look at the underlying ethics and epistemology of peer review.

Medical researcher [David Horrobin](#), whom I quoted in the previous post, says that critics of peer review "are almost always dismissed in pejorative terms such as 'maverick,' 'failure,' and 'driven by failure.'" Lest those epithets be ascribed to me, I hasten to say that I have had some success in the process and that I am not denigrating anyone who uses it to advance his or her career. The process nonetheless does have serious flaws.

Most significant of its flaws is the view that peer review must be blind in order to maintain objectivity, that is, to prevent bias from entering the process. However, as the [British Medical Journal](#), which has not used blind peer review since 1999, points out, "A court with an unidentified judge makes us think immediately of totalitarian states and the world of Franz Kafka." Objectivity is the fallacy-free perception and communication of what the object of cognition is, and bias means that some other factor, such as irrelevant preconceived notions, whether formed by emotion or by reason, interferes with this perception and communication. Lack of objectivity stems from a failure to perceive reality accurately.

Neither blind nor open peer review can guarantee this accuracy. Indeed, anonymity removes the need for care and responsibility when commenting on someone else's work. How many ill-mannered or ill-thought-out remarks would be made about submitted papers if reviewers knew that the papers' authors will know their names and how to contact them? Being

allowed to hide behind anonymity is an invitation to scurrilous behavior. This is why the objectivity of legal systems in free societies demands that witnesses, whether supporters, accusers, or expert testifiers, be identified. Contrary to the conventional wisdom of peer review, objectivity requires at minimum that the process be open.

Objectivity, at root, is an epistemological concept and the failure to perceive and communicate accurately is a function of how one uses one's mind in the processes of perceiving and communicating. Neither anonymity nor openness will improve this. The most important requirement of objectivity while reviewing someone else's work is a constant awareness of one's preconceived notions. The most significant one to watch out for is "This is not how I would have written the paper; it should therefore be changed to . . ."

As one journal editor said, no doubt with some exaggeration, all of his reviewers of so-called empirical papers recommend rejection and those of theoretical papers insist that the papers be "recreated in the reviewers' own images." And another [editor](#) complained that reviewers have turned into wannabe co-authors, requiring extensive revisions and writing comments that are sometimes as long or longer than the original articles. Clearly, decentering, to use [Piaget's](#) term, meaning the ability to consider other points of view or to appreciate the perspectives of others, is needed by some, perhaps many, reviewers.

Once it has been established that a paper meets a journal's editorial guidelines and philosophy, that is, that the topic of the paper is appropriate for the journal, then it is the author's objective that should guide evaluation. Decentering in reviewing, or editing or criticism, means accepting the premises of the author and recommending improvements in execution. The reviewer's personal preferences on the topic, including agreement or disagreement with the author's basic premises, should be set aside. The author's paper is the reality to be adhered to in the reviewing process; interference from irrelevant, previously formed emotional associations and intellectual beliefs destroys the objectivity of the process.

A reviewer, of course, may strongly disagree with the editorial guidelines and philosophy of a journal or with the objective of a paper, but then such a reviewer should either decline to be a reviewer or come to terms with the principle of objectivity. Much suppression of innovation in the peer review process probably stems from the failure of reviewers to distinguish their personal philosophies and preferences from those of the authors they are reviewing. When reviewer and author disagree, the reviewer either demands conformity or recommends rejection.

The issue of objectivity in reviewing (or editing or criticizing) is similar to the so-called problem of taste in art. Is this work of art bad art or is my reaction to it just my taste? Artists have an aim for their art and their execution of that aim makes it either good or bad art. Whether one likes a particular work of art, though, depends on many other factors, including emotional associations and intellectual beliefs. Therefore, as [Ayn Rand](#) points out, it is not a contradiction to say "This is a good work of art, but I don't like it," and vice versa. The same can be said in reviewing scholarly work, namely, "I don't like or agree with this paper, but it is well done."

The reviewer, editor, or critic who can make this last statement is one who exhibits objectivity. When looked at from the standpoint of epistemology, whether the process is blind or open is beside the point.