Independence of Scientific Publishing

Reaffirming the Principle

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Historically, the American Psychological Association (APA), as part of its contract with the editors of APA journals, has granted to the editors sole responsibility to accept or reject manuscripts for publication, without such actions being open for review by APA. Such a policy fosters the freedom of inquiry and expression so necessary for a healthy science. It also serves to protect the Association because publication of an article in an APA journal does not represent endorsement by APA itself. The lessons of history are clear: When political or other pressures interfere with the autonomy of science, the societal and scientific consequences are grim. APA should reaffirm the principle that a healthy science of psychology requires an open exchange of ideas and findings.

In July of 1993, with some fear and trepidation, I sat down to read the standard contract between the American Psychological Association (APA) and its editors, the signing of which would commit me to a six-year term as editor of the *Psychological Review.* As I read though the contract, I was struck by the unequivocal wording of one particular clause:

> MANUSCRIPTS. As editor, you will have complete authority to accept or reject manuscripts. Your decisions in this area are not subject to review by any office or employee of the APA nor by any member of its governance structure.

I was pleased, of course, to see such an unambiguous affirmation of what I assumed was the Association's policy—namely, that the editors of the Association's journals were free to exercise their best judgment without interference from the Association. At the same time, however, I was somewhat taken aback by the strength and clarity of that affirmation. It emphasized, in cold black-and-white print, the frightening responsibility I was about to assume. It said, in effect, that however much I might rely on reviewers and associate editors, the actual responsibility for what did or did not appear in the *Psychological Review* would be mine.

Aside from that sobering implication, however, the clause in question captured my attention simply because its wording was so clear and blunt. What incidents and events, I wondered, had led APA across its 100-year history to arrive at such a clear hands-off policy? By that time, I had studied the history of the *Psychological Review,* which was founded in 1894—only a couple of years after the founding of APA itself. The findings, issues, and questions that have motivated and defined psychology across essentially its entire history as an empirical science are documented in the pages of the *Review.* I wondered what specific events, controversies, or challenges had served to alert the Association to the importance of protecting the independence of its editors and journals.

Protecting the Independence and Integrity of Scientific Journals

Whatever the specific events and lessons learned that might have shaped the wording of APA's policy of noninterference,[1] the wisdom of such a policy is clear. It serves not only to protect the independence and integrity of the Association's editors and journals—and, more broadly, freedom of scientific inquiry—but also to protect the Association itself. To the extent that publication decisions are solely a consequence of peer review, augmented by an editor's best judgment, the acceptance of an article cannot legitimately be viewed as endorsement by the Association, nor can the rejection of an article be viewed as censorship. In the interests of a healthy science of psychology, that is the way it *must* be.

Given the nature of psychology as a field of inquiry, it is especially important that the appearance of an article in one of the Association's 40 journals not imply endorsement of that article's findings or arguments by the Association itself. Collectively, APA's journals report findings, theories, and commentary that span the entire range of psychological science and practice—from brain mechanisms to cognitive processes to social/group processes, from infancy

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[1] According to Wade E. Pickren, APA's archivist, and Gary R. VandenBos, APA's Executive Director of Communications, the current hands-off policy has a long history and actually grew out of a concern that some editors had too much power. Earlier in APA's history there was a relatively small number of editors, some of whom controlled their positions for a very long time. Limited editorial terms of six years were created to limit the power of the editors—but only in time. A policy of strong editorial control was developed to ensure that editors, during their time in office, were free to select manuscripts on the basis of scientific criteria, not political criteria.

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to adolescence to old age, and from normal to abnormal functioning of individuals, families, and societal institutions, such as schools. As a consequence, articles in those journals touch on issues that are of pervasive and often intense public interest. Those issues are often also incredibly complex from a scientific standpoint. E. O. Wilson (1998) has argued that the social sciences are "hypercomplex," that they "are inherently far more difficult than physics and chemistry, and as a result, they, not physics and chemistry, should be called the hard sciences" (p. 183).

Given the complexity and public-interest relevance of the questions and issues that are the focus of psychological research, it is a foregone conclusion that articles appearing in APA journals will occasionally report findings or arguments that are preliminary, controversial, or even inconsistent with the public-policy positions of the APA. Whether such findings or arguments are ultimately supported or refuted by subsequent research, it is in the Association's interests to let science take its course. The appearance of an article in an APA journal cannot and should not imply endorsement by the Association.[2]

Given the autonomy the Association has granted and should continue to grant to its editors, it is incumbent on the Association to select its editors in an open and exceptionally careful way. An editor's decisions have very important consequences for the findings that are communicated to the community of scholars in a given domain and for the impact of the work of individual authors. Historically, though, APA cannot be faulted on those grounds. Selection committees, the members of which are themselves carefully selected, typically work for more than a year to select a new editor, starting with a pool of nominees that can number as many as 100 names or more. Individuals who are selected must then also be approved by the Publications and Communications Board of the Association. Procedures also exist to remove editors from office for violations of APA's editorial standards, and an appeals procedure is open to authors who think that some standard of publication practice was violated in the handling of their manuscript (see American Psychological Association, 1994).

Lessons From the History of Science

Historically, major scientific advances have frequently been accompanied by initial findings that were threatening and upsetting to the established scientific, religious, cultural, or governmental order. Even individuals who are now giants in the history of science, such as Galileo and Darwin, paid a price in their time for ideas that threatened dominant views—whether religious, political, or scientific—of humankind's place in the world.

A case that involved a lesser light in the history of science, the 19th-century Hungarian physician Ignaz Semmelweis, is particularly instructive (for a more detailed account, see Broad & Wade, 1982). Semmelweis found that childbed (puerperal) fever, which was then causing a 10%–30% mortality rate in the maternity hospitals across Europe, could be eliminated if physicians would only wash their hands in a chlorine solution before examining the mother. In Semmelweis's obstetric clinic in Vienna, where the procedure was first implemented, the mortality rate dropped from 18% to 1%. By 1848, not a single mother in Semmelweis's clinic was being lost to childbed fever.

Semmelweis's findings, however, were hardly welcomed by his colleagues or by his hospital superiors. Owing to his vocal criticism of other physicians for letting women and infants die unnecessarily and his involvement in a liberal political revolution then sweeping Europe, Semmelweis was dismissed from his Vienna clinic. He returned to Hungary, established an obstetrics practice, and accumulated an additional decade's worth of findings, which he published in a book (Semmelweis, 1861/1983). His book was ignored by the medical profession, however, even though puerperal fever continued to reap a horrible toll in the death of mothers and infants. In Stockholm, in 1860, 40% of mothers contracted the fever, and 16% died. In Vienna, in the very same clinic where Semmelweis had demonstrated that the fever could be eliminated, 35 of 101 women died in the fall of 1860.

In frustration, Semmelweis began to write open letters that were desperate in their tone and viewed as hysterical by his medical colleagues. He became more and more distraught mentally and increasingly erratic in his behavior, which led his friends, on a pretext, to lure him to a mental sanatorium in 1865. He was forcibly restrained, put in a straightjacket, and confined to a dark room, where he died two weeks later.

There are multiple lessons to be learned from the Semmelweis case and similar tragic cases across the history of science. One is that it is not in our nature, individually or institutionally, to welcome findings and ideas that pose a threat to our images of ourselves, our societies, our religions, or our cultures. Semmelweis had the misfortune to propose a theory that was threatening on several levels. His medical colleagues apparently found it hard to welcome the idea that they personally, with their unwashed hands, had caused the deaths of mothers and infants in their care. Semmelweis's empirical findings could hardly have been more convincing, but accepting those findings required entertaining a responsibility that the medical profession was loath to accept.

More broadly, Semmelweis's findings suggested a theory of disease that threatened the nobility of humankind and its place in the grand scheme of life and faith. His results suggested that the source of diseases might have little to do with one's goodness or evilness or one's relationship to a divine being and more to do with something carried around on dirty hands. It took the work of Lister and Pasteur, some 30 years after Semmelweis's discovery, to persuade the medical profession that germs were that something.

A final lesson from the Semmelweis case is that commitment to a political, cultural, or religious viewpoint is not

[2] *Editor's note.* An example of the separation between Association policy and publications is the article by Rind, Tromovitch, and Bauserman (1998).

the only type of commitment that can bias one toward wanting to suppress findings or ideas that threaten one's beliefs. The scientific community is itself vulnerable to wanting to suppress or censor findings or ideas that threaten established theories or viewpoints. Often, in the history of science, the scientific community has not stood tall in defense of the freedom of scientific inquiry.

The case of Trofim Denisovich Lysenko in Stalin's Soviet Union, for example, is often cited as the ultimate example of the dire consequences for science and society when politicians intervene to destroy the autonomy of science, but a compliant scientific establishment also contributed to those consequences. Lysenko—on the basis of minimal empirical evidence and questionable theoretical assumptions—claimed that *vernalization,* a system of soaking and chilling winter wheat, which could then be planted in the spring, would enhance the harvest of wheat. For a combination of reasons, including the eagerness of Soviet politicians to find quick answers to the sorry state of Soviet argriculture, which vernalization and related techniques seemed to offer, Lysenko and his followers gained a favored status that lasted for decades—until 1965, when Lysenko was finally dismissed from his post as director of the Soviet Academy's Institute of Genetics. The whole Lysenkoite period was a disaster for Soviet agriculture and for basic scientific research in physiology, botany, and genetics.

One lesson from the Lysenko era is that favoritism can be as unhealthy for science as suppression. Another is that scientists need to be vigilant and stalwart in defending basic principles and standards. As Broad and Wade (1982) pointed out, however, a major factor in the "long lunacy of the Lysenkoite era" (p. 191) was the pliancy of the scientific establishment in the then Soviet Union:

> The basic fault should of course be sought in the pathology of the society in which Lysenko flourished. But the point is that the scientific community within that society was unable to preserve either its principles or a central part of its beliefs under pressure from outside. (p. 191)

The Need for Vigilance and Reaffirmation of the Principle

In looking back at key events in the history of science, one can make the error of assuming that all or most findings and ideas that are new and controversial are eventually vindicated. Were there some kind of statistical record from across the entire history of science, however, I have little doubt that it would reveal that controversial findings and theories have been more often rejected than confirmed by subsequent research. Ideas that are new and controversial but wrong—such as cold fusion, to pick an example from physics (see Close, 1991)—do not earn the same kind of permanent place in the scientific literature or in our memories as do ideas that are vindicated.

Such rejections, though, serve to remind us of the self-correcting nature of science. The ultimate arbiter of a new or controversial theory or finding is continued research and discussion, not endorsement or censorship based on the compatibility of that finding with particular political, religious, philosophical, or cultural beliefs and attitudes.

Protecting the independence and integrity of scientific inquiry and publication requires vigilance and periodic reaffirmation of that basic principle. There will always be new challenges to the freedom of scientific expression and new reasons why people believe that manuscripts addressing controversial topics should be suppressed or censored. In each case, the advocates of such interference will feel that their beliefs and concerns truly warrant curtailing the open publication of controversial ideas or findings.

Toward the goal of reaffirming APA's commitment to freedom of inquiry and the independence of its journals, the Council of Editors and the Publications and Communications Board have joined in proposing that the Association adopt the following Statement of Principle:[3]

> The American Psychological Association is committed to fostering a vigorous science of psychology through the open exchange of ideas and data. A productive and healthy science requires freedom of inquiry and freedom of expression. Researchers must be free to pursue their scientific investigations within the constraints of the ethical principles, scientific principles, and guidelines of the discipline. Editors, too, after seeking appropriate peer review, must be free to publish that science in their journals even when findings are surprising, disappointing, or controversial.
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> The publication of a scientific article by a journal of the American Psychological Association does not constitute its endorsement. The Association will not condone any attempt to censor the reporting or discussion of science within its journals so long as it has been conducted ethically and meets the scientific standards of the profession. Further, the Association will neither retract a published paper nor censure authors or editors for ethical scientific activities that yield potentially controversial findings. Scientific investigation is an evolving process: The ultimate evaluation of scientific results depends on a continuous exchange of ideas and reexamination of ideas and findings.

Concluding Comment

Editing an APA journal is a complex mixture of rewards and punishments. It is a gratifying and important job, but one that entails daunting complexities, formidable difficulties, and an unrelenting workload. Serving as an editor is an honor but also a sobering lesson in the limits of one's wisdom, scholarship, and administrative abilities. Looking back to 1993, the fear and trepidation I felt in signing the contract with APA was fully justified.

In am confident, however, that I speak for all APA editors, past and present, when I say that the job of editing is made easier and more rewarding by the knowledge that one has the support and trust of the Association. It is important to feel that no one is looking over our shoulders or our associate editors' shoulders—that we are free to accept or reject manuscripts solely on the basis of our and our reviewers' assessments of their scientific merit. Across its long history, and through the efforts of thousands of individuals, APA has developed an extraordinary journals program that is a worldwide resource for the field. The

[3] The APA Board of Directors authorized a vote on the Statement of Principle by the Council of Representatives. It was approved by Council on August 6, 2000.

Association's policies on how editors are selected, supported, trusted, and reviewed, which have evolved over many years, are key components of that success story. Of those policies, none is more important than the effort to ensure the independence and integrity of its journals. It is time to reaffirm that principle.

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