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## An Instructive Case in Referencing, Priority Conflict, and Ethics: The Role of an Editor in a Scholarly Journal

Michael Szenberg

If you torture the data sufficiently, it will confess to anything.

—Ronald H. Coase

*Projection*: A defense mechanism in which one attributes to another person one's own wishes and qualities and thereby does not experience them as one's own.

—Sigmund Freud

*Cognitive dissonance*: A state of conflict and discomfort occurring when existing beliefs are contradicted by new evidence. One way the individual(s) seek to relieve the discomfort is by denying the existence of the evidence.

—L. Festinger

A story is told of a master who was asked by his young disciple how to proceed in his unquenchable thirst for truth. The teacher did not reply but continued to walk in the field with his assistant in silence until they came to a stream. Suddenly, the teacher seized the student and thrust his head beneath the water, where he held it for several moments. Sputtering and surprised, the student asked the meaning of this. The master replied: "When you want the truth as much as you panted for air, you will find it."

It is an editor's role to probe and dissect and to do so in a way that does not encourage distortions or embellishments. To fulfill this role suitably, the editor must adhere to a high standard of ethics. The assessment and especially the scrutiny of the publication process are of vital concern to scientists because scholarly journals are the mainstays of scientific communication, and publication in them by academicians is the primary route to professional advancement.

This chapter reviews a controversy concerning citation and priority of publication that arose between the authors of two different articles on the same subject published in two different journals, and it also

discusses the role played by the editor of a journal. Both authors and editors are essential for the publishing transaction to take place. Yet even under the best of circumstances, the editor has disproportionate power in this relationship because he or she views the job as routine, whereas the writer views it as an emergency.

In 1992, I served as one of four editor panelists at a session called "Publishing in Economic Journals: Selection Criteria, Refereeing, Processes, and procedures" at the Convention of the American Economics Association. During the session, I raised the issue of multiple submissions, and after the session, the eminent economist Gordon Tullock remarked that the time had come for an editor to support the multiple submission procedure. This remark sparked my interest in writing a piece on the subject, and I decided to juxtapose editors' arguments on the pros and cons of multiple submissions. Both articles appeared in the *American Journal of Economics and Sociology* (*AJES*) (Szenberg 1994).

In response to my article, two authors complained to the editor of *AJES* that I had failed to cite one of their articles in favor of multiple submissions. They complained to the editor of *AJES* about what they seemed to feel was plagiarism on my part. As I pointed out to the editor, multiple submissions had been discussed in the journals and in the profession for many years, far antedating anything that these authors published on it. In the early 1970s, I was involved in discussions on multiple submissions at meetings of editors of economic journals, and I also practiced it. For many years, journals and books directed at writers have regularly included discussions of the subject. Furthermore, in the editorial note that accompanied the articles on multiple submissions, the editor of *AJES* wrote that the subject had been frequently discussed by journal editors, authors, and potential authors at academic conferences. Neither the *AJES* editor nor I ever claimed that the idea was original with us.

Although discussions of this topic can be traced to the 1970s (as I have done), the complaining authors do not cite anyone for the point made in their 1982 paper and insist that anyone raising similar points after that date must cite their article. They also claimed that I should have cited their main article because it was published by a journal that I edit. In fact, I do not think that editors are obliged to promote any article that incorrectly claims to be the first contribution in writing on a subject.

Calvin Peters (1976) was the first to have argued in writing for multiple submissions, and following the practice of citing original sources, I

cited his 1976 article (noting that Peters “was the first to raise the issue of multiple submissions”). The Peters article generated a discussion among academics (“Replies to Calvin Peters” 1976) for and against multiple submissions on the pages of the *American Sociologist*. Among the key words included in Peters’s article were “ethics, efficiency, promptness, mobility, security, and restraint of trade” (Peters 1976). The two authors included similar key words in their article but do not cite the Peters article, the “Replies to Calvin Peters,” or any contributions on multiple submissions that were published prior to their piece. In my article, I thank another eminent student of the journal industry, David Laband, for providing me with his unpublished manuscript dealing with the review process, including the subject of multiple submissions. Laband’s key words parallel those of Peters and the two authors in question. Laband also cites Peters and also does not cite the two authors’ article.

In 1971 (eleven years before publication of the complaining authors’ article), I submitted my first lengthy manuscript on the diamond industry (Szenberg 1973) to several publishers at the same time. I continued this practice of multiple submissions with all my other books, one of which (Szenberg, Lombardi, and Lee 1977) was published five years before the piece. So their claim to originality on this point was disproved. (Incidentally, although I advocate eliminating the exclusivity of journal submissions, I have always offered my articles to journals on an exclusive basis.)

Despite my response, the editor of *AJES* published the two authors’ comment, which again ignored the existence of the 1976 Peters article and other earlier contributions and suggested that these authors are convinced that the idea of multiple submissions started with them.

The executive board of the society that appointed me to the editorship of the *American Economist* evaluated all of the materials relevant to this controversy and gave me their unanimous support.

Fortunately, the scientific enterprise has a great capacity for self-correction and is very effective at uncovering mistakes over time. Most editors possess discriminating moral intelligence, dispassionate reflection, and intellectual independence. Editors of scholarly journals, even if encumbered by self-absorption and a sensational bent, must preserve their reputation for truth, candor, common sense, and decency and avoid being hotly partisan for one side instead of a disinterested, detached gatekeeper.

To develop sustainable relationships between author and editor, the critical element of listening must be incorporated into the matrix. Hearing

alone is not sufficient. Learning through talk (and this includes self-inquiry, a form of inner dialogue about what is the right thing to do) can take place only when both parties listen with sensitivity and empathy. Only then can a workable, salutary, and balanced relationship between author and editor exist. In times of conflict, the editor should serve as a bridge between warring factions and strive for magnanimity on both sides. To avoid or diminish erratic reasoning, self-contradictions, and misjudgments, editors must be disposed to listen to others and have the capacity to censure their own actions, however difficult this may be. Otherwise, we diminish ourselves. Editorial character must combine judgment and experience with an attachment to integrity, prudence, and respect for others and an ability to internalize a high degree of moral sensibility so that narrow interest and expediency are recognized, discounted, and shelved. In the case of the article in question above, both the journal editor and the managing editor were replaced within three months, after being forced to publish my very brief rejoinder.

### Concluding Remarks

When it comes to conflicts over priorities, editors sometimes encounter scholars who exhibit “deviant behaviors” and harbor “wish fulfilling beliefs and false memories that we describe as illusions” (Merton 1963a, 81). Eugene Garfield, founder and chair emeritus of the Institute for Scientific Information and editor of *The Scientist*, once referred me to what an eminent social observer writes about cryptomnesia or unconscious plagiarism (Merton 1963b, 273):

Cryptomnesia . . . subjects the scientist to the ever-present possibility that his most cherished original idea may actually be the forgotten residue of what he had once read or heard elsewhere. This fear may give rise to either of two conflicting patterns of behavior: in some cases, it may lie behind the emphatic insistence of any imaginative mind that he is beholden to no one else for his newfound ideas. This pattern of a possibly cryptomnesic scientist who protests-his-originality-too-much, not knowing whether he is right or not, differs of course from the pattern of the-lady-who-doth-protests-too-much, knowing as she does that her act will belie her words. In other cases, the scientist who knows that cryptomnesia can occur may assume that he has unwittingly assimilated an idea which he once believed to have been original with him. This may hold for big ideas or small ones.

In his address given at the 1960 annual meeting of the American Association for the Advancement of Science (AAAS), Sir Charles P. Snow (1960, 257) had this to say about the desire in science to pursue truth:

Without that desire there is no science. It is the driving force of the whole activity. It compels the scientist to have an overriding respect for truth, every stretch of the way. That is, if you are going to find what is there, you mustn't deceive yourself or anyone else. You mustn't lie to yourself. At the crudest level you mustn't fake your experiments.

Unfortunately, the centers that create and disseminate knowledge often become microcosms of scholarly irrationality whose inhabitants are not immune to the corrupting influences of power, close friends, and status. As one physicist observes, "The image of noble and virtuous dedication to truth that scientists have traditionally presented to the public is no longer credible" (Dyson 1995, 33. Politicians, editors, administrators, and other professionals sometimes think that they are unaccountable to others and rely on emotional opinions from the top down rather than on common sense or on facts from the ground up. As they become entangled in a web of their invention, they deny the possibility that their firmest convictions might be mistaken and attempt to arrive at conclusions too quickly.

I like to think that both writers and journal editors can benefit from this comment that John Steinbeck (1962) made in his Nobel Banquet acceptance speech: "The ancient commission of the writer hasn't changed. He is charged with exposing our many grievous faults and failures, with dredging up to the light our dark and dangerous dreams for the purposes of improvement."

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