I have a long story. It’s self-serving but it may be of interest, and possibly cautionary or instructive. For me, telling the story is therapy.

Last year Haggbloom et al (2002) published in the prestigious Review of General Psychology (RGP) a lightweight effort purporting to rank the greatest psychologists of the 20th century. It was amusing nonetheless. My attention was caught by the name of Washburn on the list of the eminent, credited with an eponym for the "Cannon- Washburn experiment". Washburn was a hapless graduate student of the great Walter B. Cannon, and all I knew about him was that he was induced to the disagreeable task of swallowing a balloon to further his advisor's research on hunger and stomach motility (Cannon & Washburn, 1912). I think of this as the first documented instance of exploitation of a graduate student by his advisor, although certainly not the last. So I was surprised but pleased to see his name among the eminent.

Only it turned out it wasn't. This was the wrong Washburn. The one Haggbloom et al meant to honour was instead female, and had the distinction of being the first woman to earn the Ph.D. in psychology. Margaret F. Washburn had many achievements, including serving as president of the APA, but swallowing a balloon was not among them. As a reviewer of my paper later commented, it was most unlikely that Miss Washburn would have submitted herself to such an indignity. Denied her incorrectly-attributed eponymic credit, her placement on the list would sink and she was in danger of being removed from it entirely.

Then I noticed another peculiarity. The high-ranked Cannon listed was not the justly-famous psychologist (actually physiologist) Walter B. Cannon, but the relatively unknown W. Gary Cannon. Once again, it appeared there had been a grievous confusion of names, although how anyone could confuse the superstar Cannon with his obscure namesake escapes me.

So, encouraged by the curious combination of errors both involving Cannon, the need to correct them, and the advantage of a snappy title for the piece ("Cannonical confusions"), I fired off a critique to RGP for publication. I always feel if you have a good title, the rest will take care of itself. Only it didn't.

The new editor of RGP, Douglas Candland, accepted my submission, and sent it off to Haggbloom for comment. He confirmed my discoveries, and both agreed that the errors required correction. It was at this point that things started to fall apart. Because, while they wanted to use the information I had provided them with, to my amazement Candland told me they could not publish my piece, as editorial policy did not allow commentaries. A series of increasingly acrimonious e-mail exchanges took place between the three of us, now also including the previous editor of RGP, Peter Salovey.

Their position was thanks for the info, and we'll publish an erratum. My position was that they had received a privileged communication and had only two choices: publish it under my name as a commentary or reject it and keep your hands off the information, at least until I publish it elsewhere. I quoted the APA Ethical Principles (clear) to no avail; I quoted the guidelines in the APA Publication Manual (even clearer) with no greater effect. I was particularly incensed that Candland would send my work out for review to Haggbloom with no intention of publishing it, and then allow Haggbloom to use the privileged information it contained to write an erratum. They budged only as far as grudgingly offering to thank me in print for tipping them off, nothing more.
The chief editorial advisor for the APA, Lenore Harmon, was asked to resolve the dispute. I thought that surely an impartial observer would see the appalling ethical lapse in what Candland proposed to do. She didn’t. She argued that the decision to publish the erratum without my permission was justified because: a) it was important to publish the corrections as soon as possible; and b) because my ms did not contain "creative theoretical or methodological ideas that I had thought of before anyone else" but only "errors which could have been found by anyone", it was not entitled to protection as a privileged communication. I was astonished at her reasoning.

Getting desperate, I took three steps. First, I sent my submission off to Psychological Reports in the futile hope that it would beat the erratum into print. Second, I filed a formal complaint with the APA Ethics Committee. Third, I contacted two people I had become acquainted with through TIPS, both with impressive expertise in ethical matters and the workings of the APA.

What happened with the Psychological Reports submission is interesting. The exchanges between Candland, Hagbloom, and myself began to circulate to a wider group. To my further astonishment, Hagbloom sent a letter to this group accusing me of unethical behaviour by sending my paper to Psychological Reports while continuing to ask for publication in RGP (the accusation is nonsense, because RGP had rejected my paper). How did he know that I had submitted it to Psychological Reports? Because, he announced, he had just reviewed it as a referee for that journal. Two ethical problems here should be obvious: (i) a reviewer does not agree to review a paper for one journal while planning to make unauthorized use of the information for publication in another, nor while engaged in a dispute with the author; and ii) a reviewer is bound not to disclose that he has received a confidential submission for review.

But Hagbloom’s indiscretion was nevertheless useful. I immediately wrote to the editor of Psychological Reports to point out the undeclared conflict of interest of their reviewer. Sure enough, when I received the reviews, there was one curiously-worded one opposing publication, and it was not difficult to guess its author. Psychological Reports nevertheless accepted my paper for publication.

The APA Ethics Committee also accepted my complaint for review. However, they told me they could not prevent publication of the erratum in RPG. What they could do was censure the editor of RGP, and I did find that option attractive. But given my unhappy experience with the chief editorial advisor of the APA, and the APA’s less-than-sterling reputation in ethical matters (e.g. their actions in the Rind/Lillienfeld cases), I had no confidence that my complaint would be upheld. However, I was encouraged by the two ethicists I consulted, who supported me and expressed concern for the way I was being treated. Moreover, they advised me that I had still another means of appeal, this time to the Executive Committee of Division 1 of the APA. Significantly, none of the other parties to this dispute, including the chief editorial advisor, had informed me of my right to this appeal.

The appeal was dealt with by the President-elect of Division 1, Bruce Overmier. Rather than providing a decision, he tried to mediate. His solution was to allow me an extended erratum (an abbreviation of my ms) in RGP in my own words, together with full publication in Psychological Reports. He implied that otherwise Candland would be allowed to proceed with the Haggbloom erratum. Unable to block publication, I felt this deal was the best I could hope for but I doubted that Psychological Reports would accept such an arrangement. I asked. The editor agreed.

As it happens, the duplication between the two versions was minimized when, with the help of Charles Harris, I discovered further errors in the Hagbloom et al (2002) paper. I withdrew the paper accepted by Psychological Reports and submitted a new, extended critique of the Hagbloom paper to it, this time keeping my mouth shut about the errors to Candland and Hagbloom (I wasn’t going to make the same mistake twice!)

The conclusion is that my erratum, with a response from Hagbloom, was published in RGP in the March issue of 2003. The extended critique (Black, 2003) was just published in Psychological Reports and I’ll be happy to send reprints on request. I withdrew my complaint to the APA Ethics
Committee, reluctantly, because it would have been interesting to see how they dealt with it. And if there's a moral to all of this, it's pretty simple: if you have something to say about a paper published in RGP, don't send it on to them. They just might rip you off.

Stephen

References


Haggbloom, S., et al. (2002). The 100 most eminent psychologists of the 20th century. Review of General Psychology, 6, 139-152.

Relevant ethical clauses dealing with the issue:

From "APA Ethical Principles of Psychologists"

6.22. Plagiarism.

Psychologists do not present substantial portions or elements of another's work or data as their own.

6.23 Publication Credit

(a) Psychologists take responsibility and credit, including authorship credit, only for work...to which they have contributed.


Psychologists who review material submitted for publication...respect the confidentiality of and the proprietary rights in such information of those who submit it.

From the "Publication Manual of the APA" (5th ed., p. 354-5):

"Editors and reviewers may not, without the author’s explicit permission, quote from a manuscript under review of circulate copies of it for any purpose other than that of editorial review...In addition, editors and reviewers may not use the material from an unpublished manuscript to advance their own or others' work without the author's consent."

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Reply from Karl:

What a remarkable story, Stephen. I cannot match it, but can provide a less complicated story about how I was almost ripped off too. After having published a research report about an infrequently studied survival behavior in wild male house mice and deer mice, I attempted a replication using wild female house mice and deer mice. The results were surprising, exactly the opposite of what I had earlier observed with the males. I wrote up another research report and sent it off for review. The
editor responded VERY quickly, rejecting the manuscript without, he said, sending it off for review. He said it was too similar to research that I had already published. Thinking that he must have missed the MAJOR point that the results were significant in the opposite direction of what they had been in the earlier research involving female mice, I pointed this out to him and reminded that I also provided an (a posteriori) explanation of this in terms of species differences in behavioral sexual dimorphism. That angered the editor, who wrote back that the manuscript was rejected because "the data were too easy to collect." Honest, that is what he said -- not that the results were of no scientific importance, rather that I had found an easy way to gather potentially interesting data. Shortly thereafter I received from two of this editors' colleagues at a university that will remain unnamed (it is located in New England) requests for reprints of my earlier research. This immediately suggested to me that these &**%$s intended to steal my easy method of gathering data and use it themselves to produce publishable research. You can be sure that I hurriedly sent off my manuscript to another journal where I was (correctly) quite confident that it would be published with little lag.

Stephen, may I have your permission to post your message on the web site for my students in statistics and research methods. I have them read several articles on the sociology/economics/politics/psychology of scientific publication (including Bradley's articles in Psychonomic), and your story would be a marvelous addition.

Hi Karl:
Yes, I'd be happy to have you post it. And thanks for the interesting story in return.
Regards
Stephen

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Obscure Addendum

I've received a complaint, presumably from a family member, concerning my use of the word "obscure" when I referred to Dr. W. Gary Cannon in my e-mail posted on your website under the heading "Two Case Studies in the Ethics of Scientific Publication". I am happy to clarify that I did not intend by that word choice to disparage Dr. Cannon's distinguished career in psychology.

I only meant that his accomplishments, although substantial, did not reach the level of those of the great scientist with whom he was confused, Walter B. Cannon, and that as a consequence his name is relatively lesser known in psychology. A quick indicator is provided by the results of a Google search on each name in quotation marks. For "W. Gary Cannon", I obtained 59 hits; for "Walter B. Cannon", I obtained 5,000. Other indicators of W.B. Cannon's eminence can be found in the article by Hagbloom referenced in my earlier e-mail (although misattributed to W.G. Cannon). I would appreciate it if you would add this corrective note to your web page.

Regards

Stephen Black
9. June 2005
How curious. How few Google hits does it take to be “obscure.” By comparison, “Karl L. Wuensch” – 12,700 hits – so I guess I am not “obscure” after all. ;-) 

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