

Top priority

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Editorial

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When has an author been scooped? It sounds like a simple question, but in fact, as a recent editorial meeting unfolded, this issue became far more complex than I had thought.

Members of the *JCI* Editorial Board carefully screen each new manuscript submitted for the quality of the science, the appeal of the topic to our readership, and the importance of the findings. I almost wrote “novelty” in place of “importance,” yet I think the former is assumed to be part of the latter; papers will not be deemed important if they merely replicate what has long been known. But what defines “long been known” is open to interpretation.

I think we can safely assume that a paper submitted to the *JCI* in 2009 showing that neutrophils produce superoxide would be uniformly considered old news — namely, 36 years old, since its original description by Babior, Kipnes, and Curnutte (1). Now, 36 years is a long time, but I am similarly sure that a report linking macrophage-related inflammation in adipose tissue with insulin resistance and obesity would be seen as repetitive of papers from the Ferrante and Chen labs only 6 years ago (2, 3). But when exactly does something lose its novelty and become old news? Is 1 year long enough? Or is that even too long?

This very issue of how old is too old prompted one of the longest policy discussions our board has ever had. Like most such discussions, it was prompted by a specific paper. In this instance, a submitted manuscript was sent for external review. Two referees returned very positive comments. A few days later, the third review came in. The third referee liked the work as well, but pointed out that just as the review was being prepared, 2 papers describing very similar studies were released as advance online publications on the website of another high-profile journal. Were the authors of the *JCI* paper scooped? If so, what should we, the Editorial Board, do about the current manuscript under consideration?

One set of editors felt strongly that although it was a regrettable situation, the work now lacked novelty and — although scientifically sound — should be rejected. Most of the editors arguing this side would have agreed to publish the paper (pending appropriate revisions) if

it had been farther along in the editorial process when the other work appeared online (e.g., already in revision), but felt that because no decision had yet been communicated to the authors, all bets were off. Others argued vociferously that this was unreasonable. The authors had submitted their paper before the other work appeared in print (or online), and that gave them equal priority; we should therefore ignore the other work when considering this paper.

One of the most important principles in science is priority — there seems to be little glory in coming in second. Yet how is priority established? Let’s take the example of calculus. Sir Isaac Newton is often credited with inventing calculus first, circa 1665. However, he did not publish his results, but rather reported them to his friends in letters, possibly the seventeenth-century equivalent of a science blog. The first to actually publish a detailed description was Gottfried Leibniz, in 1675. It is not clear whether publication of this type included actual peer review, nor do we know whether accusations that Leibniz had seen early prepublication manuscripts of Newton’s letters (perhaps the seventeenth-century equivalent of taking advantage of a privileged communication such as a manuscript to review?), but what *is* certain is that this ignited controversy regarding invention priority and accusations of plagiarism. The result was a split in the European physics community, with German scientists citing Leibniz, and the English citing Newton. Notably, Leibniz indisputably coined the term *calculus*, whereas Newton called his system *the science of fluxions*: perhaps not a great term for posterity.

To some extent, being cited can retrospectively establish priority. Over the years, people forget which paper was published first and tend to cite papers that were referenced in earlier reviews. So, what of modern citation habits? When multiple papers appear at once, how do later authors choose which one to cite (if, for example, they are limited in references)? Are more senior and better-known scientists favored? Does the impact factor of the journal in

which the paper appeared matter? How about the first or last author’s name — does it help if it was easy to remember? Country of origin — do we favor our local colleagues? Institution of origin — do we give preferential treatment to papers from academia versus industry? What if a paper that appeared somewhat after the first one is more thorough and definitive? Does the submission date matter at all? In theory, it might establish priority, but if papers have undergone multiple revisions and addition of new data prior to publication, this can get very fuzzy.

All these issues brought to mind a group of 3 papers in my field that appeared more than 6 years ago reporting the identification of foxp3 as a key transcription factor controlling regulatory T cells (4–6). The paper from the Sakaguchi laboratory (4) appeared online in *Science* in January 2003, and the other 2 papers, from the Rudensky lab (5) and the Ramsdell lab (6), appeared online in *Nature Immunology* in March 2003. As of this writing, the Sakaguchi and Rudensky papers each have more than 2,500 citations, yet the Ramsdell paper has been cited only about 1,500 times. The more-cited manuscripts were from academic institutions; the less-cited one came from authors with an industrial affiliation. Being from a company, the senior author of that paper publishes less, and therefore could be a less familiar name — is that the reason for the discrepancy? If you were Dr. Ramsdell, would you care that you were less cited? Should Professor Sakaguchi be concerned that his priority isn’t being sufficiently recognized, given that his paper was published first? In this instance, all 3 papers appeared in the same calendar year. It seems likely that the year of publication would make a significant difference for citations; whether papers separated by 3–4 months appear in the same calendar year is really up to serendipity.

Back to our specific manuscript: in this case, we decided that the authors had a claim on priority, since their work was submitted before the other papers were published. Therefore, we returned the paper to the authors for revisions, but mandated that they submit their revised manuscript within 60 days (a time frame that seemed reasonable, given the work requested). I confess that although there was not vocal



dissent about our decision, I don't think there was consensus either, and it seems clear that this issue will continue to arise in other forms.

As editors, we like to think that readers will be more interested in the papers we publish – and will read and cite them more often – if the results describe what is *not* known, rather than confirm what *is* known without extending it significantly. But I worry that we are giving too much weight to our pride, and to an interjournal competitive atmosphere that many on the board bemoan, yet do little to combat. The widespread policy by which journals reserve

the right to cancel publication of papers if authors fail to observe a press embargo is justified by wanting to ensure that the public has full and complete information, but some policies may be more about ego than anything else.

I, and the entire Editorial Board membership, believe that this is a very important issue in scientific publishing. We would like to know what our readers think. Please e-mail us at editors@the-jci.org to let us know.

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Editor in Chief

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