Psychology From the Other Side of the Line: Editorial Processes and Publication Trends at *JPSP*

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The experience of editorial responsibility can produce many surprises, both pleasant and unpleasant, and some of these are outlined. The author also responds to Reis and Stiller (this issue), who carefully document how the length and complexity of articles published in the Journal of Personality and Social Psychology have increased over the past three decades and attribute this growth to the increasing sophistication and depth of psychological research. Although this characterization is surely correct about much modern research in psychology, some articles may be getting longer for the wrong reasons, including obsessiveness and pseudosophistication. The reviewers and editors of our journals need to distinguish between articles that are truly sophisticated or are breaking new ground and those that are obsessively detailed reports of trivial, albeit complicated, variations on the same old theme. To make this distinction wisely, we may need to read and evaluate research within a broader frame of reference than is customary.

For the past few years (although it seems much longer), I have had the privilege and burden of spending many of my working hours on the other side of the invisible but important line that separates "us," the psychologists who do research and write it up, from "them," the editors who decide whether it gets published.[1]

Among the consequences of this role is that I have found myself, more than a few times, in the previously unaccustomed position of defending editors in general, the *Journal of Personality and Social Psychology* (*JPSP*) in particular, and specific editorial decisions that, in retrospect, may not have been as wise as they seemed at the time. As a loyal former student of Daryl Bem (1972), I know enough self-perception theory to predict that (in the absence of large material incentives, which indeed were not forthcoming) my general attitude after these experiences would be more favorable toward journal policies and editorial practices than it was before. And so it is, though my optimism pales in comparison with that expressed by Reis and Stiller (more on that anon).

PLEASANT SURPRISES

Reviewers

It has been my experience that the process of peer review works rather well, and perhaps surprisingly well given the obvious potential pitfalls of ignorance, apathy, and bias. All three are quite rare, though none, of course, is nonexistent. By and large, the individuals who evaluated manuscripts provided thorough, knowledgeable, conscientious reviews. Most worked hard to avoid *ad hominem* comments and bent over backward to be fair and to give authors the benefit of the doubt. Criticisms were almost always constructive, and some reviews were so insightful that I found myself wishing I could publish them instead of the manuscript under review. And please do not forget that these reviewers worked *free*. Their only rewards, apart from an occasional mention on small-print Acknowledgments pages, were the opportunity to shape their field in some small manner and whatever intrinsic rewards inhere in being among the first to see the work of one's colleagues and in being able to help improve the final product.

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Authors

A second pleasant surprise concerned the authors. The rejection rate at *JPSP* has run, historically, at a fairly steady 80%. That means I have written an awful lot of rejection letters—I have never counted exactly, but a couple of hundred, at least. As Mel Manis once said about his tenure as a *JPSP* editor, "I have blood on my hands!"

Submission of an article to *JPSP* represents a large investment of time, energy, intellect, and emotion. Important outcomes often hinge on acceptance—tenure, raises, and prestige, to name a few. To wait several months for a reply and then be told, "Thanks, but no thanks," can be devastating. Given all that, I found the degree of dignity and—for lack of a better term—plain good sportsmanship displayed by most rejected authors to be impressive. I braced myself for a steady onslaught of telephone calls and letters impugning my ancestry, or at least my judgment, demanding re-review, appealing to a higher authority, and so forth. Such actions were, in fact, exceedingly rare. Fewer than half a dozen rejected authors gave this editor any real grief, and most of those were individuals who seemed to be dispositionally inclined in that direction, so I tried not to take their behavior personally.

UNPLEASANT SURPRISES

Other surprises were less pleasant.

Reviewers

I was astonished by how difficult it often was to get any reviews of a manuscript at all. I cannot tell you how many manuscripts were returned, unreviewed, a couple of months after they were sent out with the scrawled notation "Sorry—I'm too busy." This kind of response, though understandable in a way, always disturbed me, because it seems to imply that those psychologists who *do* review are somehow *not* busy. The real meaning of the comment just quoted, of course, is "Sorry—reviewing is not one of my high priorities, and I am content to leave the job to those who think it's important." Many individuals who refuse to provide reviews still enjoy participating in the publication process as authors and show no discernible tendency to be especially understanding when their own reviewers are dilatory or unconscientious.

Particularly disturbing was the fact, which I have verified with other editors, that especially eminent psychologists seem in general the *least* willing to review. Brand-new Ph.D.s almost never turn you down, by contrast, and usually provide excellent reviews besides. A possible explanation is that famous psychologists are "too busy" to review. To which one could reply: What, then, *are* they doing with their time, that newly minted Ph.D.s still seeking tenure are not or should not be doing?

Editorial Freedom

A second less-than-pleasant surprise was that the role of editor entails much less freedom of action than I had somehow expected. Like everybody else, I had heard numerous suggestions for how journal editors could impose major reforms on the field (Effect sizes must always be reported! All researchers must state their statistical power! No one-shot studies! Always use converging operations! A significant contribution to theory must be present! or even: Every paper must be interesting!). Such ambitions, I found, quickly bite the dust in the face of the first few submissions and rounds of review. When dealing with actual research and actual authors, abstractions either force one into intolerable rigidity or go out the window. Publishability remains a judgment that must be made, by reviewers and editors, one real manuscript at a time.

A second limitation was equally surprising. Again, like everybody else, I had heard that editors can determine the content of reviews, and therefore the outcome of the editorial process, simply by adept choice of reviewers. But that theory presupposes reviewers to be highly predictable. It turns out that they are not, in part because even "obviously" biased reviewers often return surprisingly objective reviews.

Finally, like all authors, I have long held a simmering anger at the unconscionably long editorial lags that characterize our leading journals. A big surprise was that my own editorial lag, once I had the job, was not one *jnd* shorter than anybody else's. Authors should know this: Your submitted manuscript spends nearly all its time "under editorial review," languishing beneath mounds of papers on the desks of potential reviewers scattered across North America and the world. And although all editors send out numerous reminder letters (my office had a computerized system for this), the reviewers work free, remember? In the final analysis, there is not a lot an editor can do.[2]

WHY ARE ARTICLES GETTING LONGER?

The preceding comments pertain to the nuts and bolts of the editorial process. The provocative article by Reis and Stiller focuses, instead, on the output of this process, the published content of our journals. With exemplary care, Reis and Stiller document that over the past 20 years articles in *JPSP* have increased substantially in length (i.e., the average article has more than doubled) and also have come to include longer titles, more tables, more studies, more subjects, more authors, more references, and more complex statistics (while enjoying *less* grant support).

Reason 1: Because Some Articles Are Getting Better

Reis and Stiller's interpretation of their findings is extremely optimistic. Indeed, in this era of pessimism, their point of view is downright unexpected and rather refreshing. They interpret every finding (except the one about grant support) as reflecting the enhanced development and sophistication of personality and social psychology. Even the frequently heard complaint that *JPSP* is boring (Deaux, 1988, cited by Reis & Stiller) is treated as a positive sign—specifically, as reflecting that the journal's articles are "increasingly specific or subtle . . . [or] are based on more elaborate studies with more complex procedures, and . . . require more extensive description of prior literature, methodology, and findings."

Reis and Stiller's conclusion is original, provocative, and even rather amazing. To some degree it is surely correct. There are many encouraging factors that have led articles to grow longer in recent years. Reis and Stiller discuss paradigmatic maturity as one of these, and other reasons are no doubt important as well. For instance, it is the impression of this reader that an increasing number of studies:

(a) are *interdisciplinary*, combining, for instance, personality psychology with social and/or developmental psychology or even with evolutionary biology. Such interdisciplinary work requires the researcher to master and to summarize at least *two* areas of prior research and to explain them to a readership that may be familiar with one but not the other.

(b) are using or developing *new methods* that must be documented, explained, and defended. These include techniques of cross-cultural research with which many psychologists are (or were until recently) unfamiliar, new kinds of statistics, a renaissance of long-neglected narrative and qualitative methodologies, more frequent field studies in applied settings, and an increasing use of novel and complex psychophysiological measures. To use and explain any of these methods will, of necessity, consume a lot of journal pages, at least until we all get used to them.

(c) are being done in *new areas* of research that, because they are still being invented, require a large amount of space to explain. A few examples are health psychology, the study of individual lives, and the accuracy of social judgment. Each of these areas raises issues that are not (yet) routine to the field as a whole, and so research in these areas requires a different and more lengthy kind of exposition than will suffice in areas where more understanding (or agreement) among the readership can be assumed.

Reason 2: Because Other Articles Are Getting Worse

Some of the reasons that articles are getting longer are not so encouraging, however. There is an important degree of heterogeneity to the studies that lie behind the numbers Reis and Stiller report. Some of the studies of the 1980s *are* better—more sophisticated, more thorough, more original—than their predecessors in the 1960s. But others are worse. Many factors can make an article longer and more complex. Not all inspire optimism.

About paradigms. There is a sense to the word *maturity*, applied to paradigms, that is not so positive. The sense is "senility." Paradigms not only grow up, they grow old, and not always gracefully. Intellectual veins can peter out, and investigators sometimes run out of ideas before they run out of ambition. Research will then be characterized not so much by thorough analysis as by nitpicking, as each new study tries, sometimes rather desperately, to establish that its findings go beyond those that have been published 200 times already.

Or sometimes, as a paradigm ages, it begins to fall apart. Findings start to accumulate that are inconsistent and confusing. Measures of key constructs get refined and redefined to the point that key variables no longer resemble what they started out to be and in some cases are no longer relevant to the phenomena that made the variables interesting in the first place. (To make matters even more confusing, the *names* of these variables rarely change and indeed may be the only constant thing about them over time. Apparently, it is easier to redefine a variable than to rename it.)

Or, as a paradigm grows older, it can become increasingly closed off from other areas of research. In some cases, it becomes the nearly exclusive property of a small club of investigators who exchange technical details of their research among themselves with enthusiasm and thereby enhance one another's citation counts but who have less and less to say to the psychological community at large. On occasion, these clubs have even been known to work actively to exclude researchers whose work fails their litmus test of what they see as proper. More frequently, they develop a special jargon and self-referential style of analysis that has the same practical effect.

As paradigms degenerate in any of these ways, the articles they produce may well grow longer and have bigger reference sections, but it won't be a sign of progress.

About statistical "sophistication." Some of the increasingly sophisticated statistics used in more recent studies indeed represent methodological progress. But mixed in there, too, are statistically complicated articles that are merely *pseudo*sophisticated. On more than a few occasions, a trendy new statistical technique, usually fearsomely complex, has suddenly emerged and begun to pop up all over the literature, in all of sorts of investigations. Of course, sometimes its use is appropriate. But sometimes the new technique is used even when it adds nothing to what could have been found with simpler methods. Sometimes the technique is used even when it *obscures* what would have been clear with simpler methods. And sometimes the technique is used even when the author of the article does not really understand it. When these things happen, and they frequently do, statistical complexity and the numerous tables that go along with it are not signs of progress.

Problems with the review process. The occasional publication of incestuous, ingrown, or pseudosophisticated articles such as I have described is to some degree a side effect of the same review process that usually works so well. The natural tendency of an editor is to seek "expert" advice—that is, advice from a specialist in the same subfield of research as the paper under review. The (unintended) effect can be that the reviewer may then be one of the precisely four persons on earth who could conceivably muster any interest in the paper! At worst, the result may be a positive review for a publication that nobody else will ever read.

As I gained experience, I tried increasingly to seek reviews from "nonexperts," reputable researchers in a field a few degrees away from the paper under review. This practice proved useful, but I was surprised to see how forgiving such reviewers could be—perhaps they have come to expect uninteresting articles in their premier journal. On more than a few occasions, I received reviews (or cover letters) that said something like this: "This article strikes me as trivial, uninteresting, and utterly unimportant, but it might matter to a few specialists in this area, so probably you should accept it."

Even more disturbing were the cases where my attempts to gain some sort of true third-party opinion proved to be in vain, as reviewer after reviewer returned an article without comment, on the grounds that it was "outside my area." To some extent this was understandable; I made some mistakes in reviewer selection that the reviewers themselves were quick to correct. But I was also rather shaken by my discovery of how narrowly many psychologists define their "area." In an actual and not really unusual case, one reviewer replied: "This article is about emotion. My own area is stress and coping, so I couldn't possibly evaluate it."[3]

The underlying issue here goes beyond the standard editor's lament that good reviewers are hard to find. If very many psychologists define their areas of interest and expertise extremely narrowly, then their research will become increasingly ingrown and self-referential, as described above. The increasing lack of connection of their research to the field as a whole and intellectual life in general could carry a cost that severely attenuates the effect of the increase in sophistication and depth that Reis and Stiller celebrate. And if these same psychologists refuse to review articles outside their own, narrow fields, then too many such articles will receive reviews (when they are reviewed at all) that fail to subject research to the toughest and most important question of all: So what?

THE STAKES OF THE GAME

A final comment: Too much is riding on the actions of journal editors and reviewers. As Reis and Stiller point out, "Publication in prominent journals is an important unit of worth . . . and helps control access to significant resources (such as grant funds and tenure)." I never liked this tendency, but after serving in an editorial role I find it more alarming than ever. To hear a colleague described as somebody who "must be good because he (or she) has three publications in *JPSP*" (or, for that matter, "must *not* be good because he (or she) does not publish in *JPSP*") rings false, somehow, when you are spending much of your own time deciding which articles get accepted.

The feeling I and (I think) most other editors have is that it is tough enough to edit a journal so that each issue appears with a reasonable content—please do not give us the job of making personnel decisions for the whole field, as well. Nobody is in a better position than a journal editor to know how fallible the process is, how decisions must be made on insufficient grounds, with inadequate deliberation, and with excessive bias. We all do the best we can, of course, but I think we all wish our decisions were not so consequential.

So here is the exhortation: Please do not abdicate the responsibility of evaluating your colleagues and assign it to journal editors, who have sufficient problems of their own. Evaluating our colleagues and their work is a necessary, if sometimes disagreeable, part of academic life. To do this job responsibly, you must do more than count an individual's articles. You must read them. So often, we have all heard colleagues say, "I cannot evaluate the work of Dr. X [being considered for tenure] myself; it's somewhat outside my area." This comment sounds especially familiar to a journal editor, who can only think, "Here we go again!"

CONCLUSION

The analysis by Reis and Stiller is empirically compelling, and much of their interpretation is on target. However, we should not forget that although articles can get longer for good reasons, they can get longer for bad reasons, too. Some long articles are truly sophisticated, break new ground, or integrate wide literatures. But others are obsessively detailed reports of trivial, albeit complicated, variations on the same old theme. We cannot tell which is which by counting pages, authors, references, or tables. We will have to read the articles, even if —especially if—they are outside our immediate research area. That practice might lead us, occasionally, to attempt the even more difficult and important task of evaluating our *own* research in terms of the contribution it makes to psychology in general and to do what we can to increase the breadth and relevance of our own contribution.

NOTES

1. To be exact, I served as one of two associate editors of the Personality Processes and Individual Differences section of the *Journal of Personality and Social Psychology*. Under the policy of the editor, Irwin Sarason, each associate editor had the delegated authority to make final accept/reject decisions. My own manuscript load ranged from about 100 to 150 per year. In the interests of full disclosure, I should reveal that although my editorial duties began *after* the period considered by Reis and Stiller, I did publish an article in *JPSP* in 1988 that doubtlessly contributed to the trends they reported. It had detailed tables, a long reference section, and a verbose title (which included a colon).

2. An impatient and disgruntled author once suggested to me that reviewers who are tardy be banned from future review. I had to reply that such a policy would leave a very small pool of potential reviewers and that editors will always be more sympathetic to suggestions on how to find *more* reviewers than how to weed out those they already have.

3. To the extent that this is true, then the field of "stress and coping," or at least this nonreviewer's representation of it, has developed not a mature depth but a senile narrowing of vision that fails to see the obvious relevance of neighboring areas of research. But I hasten to add that the field of stress and coping is in general a truly mature field, filled with creative, broad-minded investigators to whom the individual just described is a rare exception. After my editorial term, I do not need any more enemies.

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