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A Former Editor Views the Editorial Process

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Introduction

It was at the AGU Fall Meeting in San Francisco in December 1976, shortly after my appointment as coeditor of *Water Resources Research* that I first began to realize the strong emotional ties that exist between a scientific community and its journals. Feelings run high, regardless of whether they come from readers, contributors, reviewers, active scientists, or scientific administrators. Opinions are often positive, sometimes negative, usually a mixture of the two; but regardless of their tenor they are delivered to the editor, in person, usually fortissimo. From that day until this, conference life has never been dull. When I meet a colleague in the halls there is never a loss for words, no need to search for a topic of mutual interest; WRR is always there at the ready.

Over these years I have listened to suggestions, compliments, opinions, proposals, questions, complaints, secrets, and curses. But when stripped of the specifics, most of my colleagues were asking a variant of one of the following seven questions:

1. Why did it take so long for my paper to appear?
2. How could you possibly have rejected my recent submission (especially in light of the enthusiastic support of reviewer D and the obvious incompetence of reviewers A, B, and C)?
3. How could you possibly have accepted the paper by Smith and Jones (especially after the scathing review I sent you)?
4. What is the policy of WRR toward multiple-part papers?
5. What is AGU's page charge policy with respect to WRR?
6. Why does WRR publish so many theoretical papers and so few applied papers?
7. Is the review process really needed at all?

I am writing this article in the hope that it will provide some answers to these questions and that it may help to clarify the murky workings of the editorial process. Of course, as with all clarifications, there is a hitch. My term as editor expired on January 1, 1981, and philosophies of editing are notoriously personal. My successor as coeditor for the physical sciences side of WRR is Steve Burges of the Department of Civil Engineering at the University of Washington in Seattle. He has read this article and on the reviewer appraisal form, he recommended 'publication with minor revision.' This response suggests either that our personal philosophies are not all that far apart or that this is the hydrologic equivalent of the Nixon pardon.

The editorial board of *Water Resources Research* consists of two coeditors and a slate of associate editors. During my tenure, I was fortunate to work first with Dave Major and then with Jerry Cohon as coeditor for the social sciences side of WRR. At various times, 25 different scientists (see box) served as associate editors, and all were involved in both the day-to-day processing of manuscripts and the long-term development of policy. They deserve a great deal of credit for the success of the journal.

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The hours they put in on behalf of the journal are long; the thanks they get is embarrassingly meager.

The WRR Editorial Process

The WRR editorial process is outlined in Figure 1. Authors submit their papers to one of the two coeditors, who in turn select an associate editor to process the paper. Associate editors are responsible for selecting reviewers and ensuring that reviews are completed within a reasonable time. After analyzing the reviews, the associate editor may return the paper directly to the editor, either for rejection or because no revisions are needed, or he may return it to the author for revision. Authors are instructed to send their revised manuscript back to the associate editor so that he can check to see that the requested revisions have been carried out. If so, the manuscript comes back to the editor and thence to AGU for publication. One copy of the typescript and the glossy prints of the figures are kept on file at the editor's office during the entire editorial process. Final notification of acceptance or rejection comes to the author from the editor's office. In rare instances, the editor may choose to reject a paper without sending it through the full review process.

Figure 1 also shows the range of elapsed times that one might expect for each step of the editorial process. The total processing time is controlled in large part by the time taken by reviewers during the review stage and by authors during the revision stage. With mailing times now running between ½ and 1½ weeks, even if reviewers and authors respond quickly, total processing time takes 2½ months. A more usual period would be 5 months; and if reviewers, authors, and the mails are all slow, the editorial process can take 8 months. Statistics kept by the AGU Publications Division confirm this analysis. In 1979, for example, 10% of the submissions were sent to AGU within 14 weeks and 50% within 28 weeks. There were 10% that took longer than 1 year. An analysis by the editor's office of those manuscripts that took longer than 8 months to process revealed that 75% of the cases resulted from lengthy author revision periods. Of the remaining 25%, about half were the result of unavoidably lengthy or multiple interactions between author and associate editor on difficult or marginal papers, and about half can be chalked up to inefficiency on the part of the editorial board.

Once the papers go to AGU there is a further processing time of 5 to 8 months for copy editing and galley-proof preparation and review. The AGU Publications Division recently committed itself to improving its average performance from the 25–27-week average for 1979–81 to 20 weeks for 1981–82.

In summary, authors who do not take undue time with revisions should anticipate that the total time from submission to publication will run between 8 and 13 months. As this statement indicates, total processing time follows a statistical distribution with a fairly large standard deviation. Undoubtedly, most authors realize this, but the realization doesn't lessen the frustration of those authors whose papers seem to be progressing at a rate designed solely to satisfy the laws of statistics in the 95% tail.

It is clear that processing times could be reduced by a simplification of the scheme outlined in Figure 1. As Alex Dessler has pointed out for the blue JGR [*Dessler,* 1972], the editor could take on a greater role in the selection of reviewers or in making decisions without the aid of reviewers. This approach would minimize the role of the associate editors. In a field as diverse as water resources has become, I personally doubt whether an editor operating without heavy dependence on associate editors could properly maintain the quality of the journal. I think that the current system is a good one and that processing times are best minimized by administrative vigilance from the editor's office and constant pressure on authors and referees to review and revise quickly.

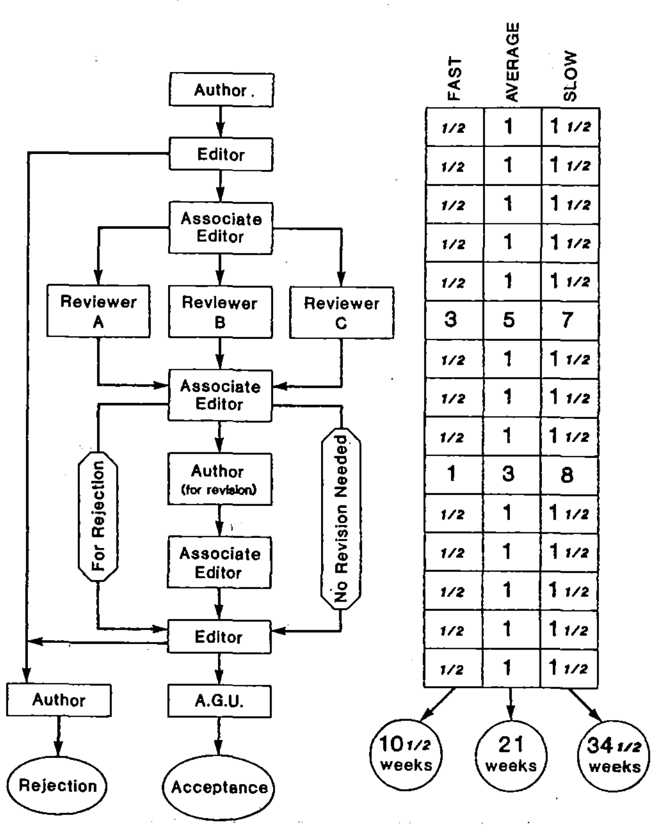

Fig. 1. Editorial review process.

Reviews, Rejection, and Type II Errors

Gamesmanship, as Stephen Potter has made clear, pervades all of life. It should come as no surprise then to find that the reviewing process can be viewed as a game. As described by *Chambers and Herzberg* [1968]:

> Play opens with submission of the paper by the author. At this point the editor of the journal intervenes to select the opposing player(s). The next move is by the referee. Without loss of generality, we call this move the refusal. This may be followed by a further submission, a further refusal, and so on, until one or [the] other player concedes defeat.

Chambers and Herzberg then outline a series of tactics for the author and for the referee. Among those listed for the author is the 'Anticipation tactic:'

> Here the author attempts to disarm criticism either (a) by inserting flattering references to the work of all the more likely potential referees, or (b) by writing papers jointly with all the experts in the field, thus making it impossible to find a referee.

Among the tactics for the referee is the 'unsuitable-for-publication-in-this-journal tactic:'

> This tactic is also known as the 'shirking-of-duty tactic.' As a last resort the referee says that the paper is unsuitable for publication in the journal in question and makes a suggestion that it be submitted to another journal, which is suitably insulting to the author. This then ends the game between these two particular opponents. The referee then hopes that the suitably insulting journal does not ask him to referee the paper.

Apart from the obvious pleasures of gamesmanship, the purpose of the reviewing process is presumably twofold: (1) to provide authors with information to improve their presentation, and (2) to provide editors with information to aid them in their decision to accept or reject. Reviews may be positive or negative, and they may be useful or useless. A positive review recommends acceptance; a negative review recommends rejection. A useful review is one that provides helpful suggestions to the author in support of a positive recommendation or one that provides well-articulated documentation in support of a negative recommendation; a useless review is one that recommends acceptance or, worse yet, rejection, but provides no specific reasons.

If two or more reviews are received by the associate editor on a given paper, a unanimous recommendation for rejection or for acceptance, with or without revision, is usually accepted. In the case of mixed reviews, it has been WRR policy not to go out for a second round of reviews. A decision is made by the editorial board by implicitly assigning weights to the conflicting reviews and by exploiting the expertise of the board itself. Reviewers may vary widely in their suitability to the assigned review task. They may vary in technical competence, in scientific experience, in experience in the reviewing process, and in their known predilections for favorable or unfavorable response to the work of others. Reviewers must recognize that their reviews are recommendations only; the decision rests in the hands of the editors. Reviewers can be assured that all negative reviews are passed on to the authors, even if the negative recommendation has not been accepted. Once a decision has been reached to allow an author to revise his paper toward eventual publication, however, reviewers and editors alike must realize that it is the author's paper. If the author is going to have to lie in the bed, he ought to be allowed to make it.

Editors, like statisticians, are subject to type I and type II errors. We occasionally reject papers we ought to publish; we occasionally publish papers we ought to reject. An editor's goal is simply to reduce the number of such occurrences to delta (which mathematics students will recall is always smaller than epsilon, which is itself very small). An unworthy acceptance is thought by most editors to be a much lesser evil than an unwarranted rejection. It is hoped that peer response will identify the incorrectly published paper in due course. The unfairly rejected material, on the other hand, may never appear, to the detriment of the author and the scientific community; or worse yet (in the eyes of the editor), it may be acclaimed after publication by the competition, to the detriment of the journal.

When a paper appears in WRR, no matter what you may think of it, it presumably received reviewer support from some quarter. The only exception is when the editor invokes what I like to call the Langbein doctrine. As Walter Langbein explained to me during his tenure as the first editor of WRR, there are some papers that are so original or so provocative that they deserve publication on those grounds alone, perhaps without review, or perhaps despite negative reviews. During my tenure, I invoked the Langbein doctrine on very few occasions and have not yet regretted any of those decisions.

During the period 1977–1980, the rejection rate for WRR ranged between 25% and 30% on first submissions. The effective rate is somewhat lower in that material originally rejected sometimes reappears in a totally revised resubmission that proves acceptable. The WRR rejection rate is in keeping with other AGU publications, with other earth science publications, and, indeed, across the broader spectrum of scientific journals in general. Much higher rejection rates are common in the humanities but not in the sciences.

Multiple-Part Papers

During my editorial tenure, I generally tried to avoid hard-and-fast policy rules, preferring instead a more flexible approach that allowed leeway for decisions on an individual basis. In this spirit I did not have a fixed policy about multiple-part papers. Papers that were submitted by authors in multiple parts were usually reviewed in that form. In cases where reviewers or editors felt that the readers would be better served by a single paper, authors were requested or instructed to carry out a major revision to that end. I did not have then, nor do I have now, any personal objection, either as an editor or a reader, to the appearance of multiple-part papers. I believe there are many scientific studies that are best reported in this form. I believe that decisions about format should be left in the author's hands, unless reviewers identify the format as a weakness in the presentation. Editorial decisions on multiple-part papers ought to rest entirely on the technical merits. Journal editors have no obligation to take into account how institutions treat multiple-part papers in their publish-or-perish assessment of individuals. On the one hand, then, authors should be allowed (although perhaps not encouraged) to separate their work into parts when there is good reason to do so; on the other hand, the editorial board must remain vigilant to discourage abuse.

I have seen no evidence to suggest that authors who submit their work to WRR are familiar with the LPU strategy outlined by *Broad* [1981] in a recent issue of *Science*. An LPU is the 'least publishable unit' of an ongoing research project, and Broad holds that the trickling forth of LPU's into the literature is in large part responsible for the massive explosion in journals, papers, and journal pages in recent years.

Page Charges

Many authors fail to submit good work to WRR because they feel that they would be unable to pay the page charges. This is a mistake; AGU recognizes that all scientists do not enjoy sufficient support to pay page charges, and it is AGU policy that all accepted papers are published in WRR, regardless of whether the page charges are honored. In this sense, WRR page charges are voluntary. Having said this, I must emphasize that the financial health of WRR is dependent on the payment of page charges by those with sufficient research support. It is an abrogation of scientific responsibility if available grant funds are diverted to other purposes while page charges go unpaid.

Correspondence about page charges takes place directly between the author and AGU. The editorial process is carried out independently of the page charge decision; in fact, without knowledge of it.

If the percentage of unpaid pages in WRR were to become very large, AGU reserves the right to offer priority publication to papers on which the page charges have been paid and to delay those on which the page charges have not been paid. During my 4 years as editor, however, there was no delay at any time in the publication schedule of any paper, and there is currently no such delay.

Theory and Practice

Apparently the hydrologic community carries two strong perceptions about WRR. First, it is perceived as the leading journal in the field; and second, it is thought to favor theoretical papers at the expense of applied papers. As editor, I was always pleased with the first view, less so with the second.

As noted on the inside front cover of the journal, 'the editors of WRR invite original contributions in hydrology.' Clearly, 'original contributions' may come in the form of improvements to scientific theory and methodology, or they may come in the form of advancements to engineering practice and policy analysis. I have occasionally noticed that authors who publish the theoretical derivation of a new methodology in WRR will publish its initial application in another journal. This may be done simply to gain a wider readership; but if it is done with the thought that WRR would not be interested in the practical paper, then that perception is incorrect. The journal is very interested in publishing papers that emphasize field applications, engineering design, instrument development, or policy analysis.

The fact that there are relatively few such papers reflects upon lower submission rates, not upon higher rejection rates. It is not necessary that a paper have a strong mathematical component. The editors would like to see more papers that report the results of careful field measurement programs, especially ones that lead to an original or creative hydrologic message.

The type of applied or practical paper that is not likely to be accepted is one that utilizes a well-known technique in a field application that has no particular uniqueness. (Of course, theoretical papers of this type are not likely to be accepted either). This is not to say that papers of this type are not useful to the water resources community. The purpose of such papers, which is to build up documentation of engineering precedent and case histories of policy analysis, is a valid one, but WRR has chosen not be the outlet for this type of work.

One last comment: while the perception of WRR as a theoretical journal has some basis in fact, the reality is not nearly as clear as the perception. Any reader who thumbs through the issues of the past few years, will find a healthy percentage of papers that emphasize field measurements and practical applications.

Sociology of the Reviewing Process

The most fundamental question that can be asked about all this is: 'Is the review process really necessary?' A negative response would probably be treading on the rather thin line that exists between the review process and censorship, and on the question of bias.

Surprisingly perhaps, there has been a good deal of sociological study of these questions. Ever since Derek de Solla Price first turned the methods of science on science itself [*Price*, 1964], there have been numerous statistical studies designed to measure the efficiency of the review process in terms of its stated goals and to uncover evidence of bias. Most of the studies have used the physics literature as their statistical sample, but I expect that their conclusions can be carried over to the earth sciences.

With regard to the bias question, *Gordon* [1979] discovered statistically significant relationships between referees' evaluations and the national and institutional affiliations of the referee-author pairings. For example, reviewers from 'major' universities were harder on authors from 'minor' universities than on those from major universities. In this case, of course, there may be a deterministic as well as a stochastic component to the finding. Less easily dismissed is his evidence that British referees provided more favorable reviews of British authors than of North American authors, and vice versa.

Zuckerman and Merton [1971] report more encouraging results with respect to bias. They investigated the effect of the relative ranks of author and referee on the referee's decision. The first rank was a small group of award-winning physicists; the second rank was a larger group, whose biographies were widely available in scientific who's-who listings; and the third rank was the very large group that didn't qualify for either of the first two ranks. Six possible forms of bias were investigated. If authors outrank referees, either status deference or status envy could be important. If referees outrank authors, bias might take the form of status patronage or status subordination. If author and referee come from the same rank, the referee could feel status competition or status solidarity. The statistical studies did not lead to the acceptance of any of these six hypotheses.

Zuckerman and Merton did uncover a correlation between rank and acceptance rate but not between age and acceptance rate. In fact, 'the youngest group of third-rank physicists had as high an acceptance rate as the oldest group of high-rank physicists whose work, we suppose is no longer as good as it once was.' Zuckerman and Merton concluded that the reviewing system apparently does exactly what it is supposed to do, sift out the good papers from the bad.

The question of censorship must surely stand or fall on whether partisan judgements or harsh reviews have created (in the words of *Ziman* [1968]) 'a hidden treasure of rejected works of genius which would have revolutionized our view of Nature had they been published.' Ziman thinks not, and I think not, too. I agree with *Manheim* [1973] and *Broad* [1981] that a more likely cause for the failure of a good idea to take root would be its burial in the flood of publication that overwhelms scientists every day. Manheim makes the case for higher journal standards as a protection against this flood. I suppose it is every editor's prerogative to judge for himself the balance point he wishes to occupy on the tightrope between the maintenance of journal standards on the one hand and the reduction of type I errors on the other.

Lastly, there is the question of whether a review system that manages to reject only one quarter of its submissions is superfluous on that ground alone. This view neglects the fact that the remaining 75% may be strengthened. In addition, as Zuckerman and Merton have noted, the very existence of a reviewing system serves as a form of quality control. Knowing that their papers will be reviewed, authors take care in preparing them, and often the journal's high standards become their own.

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