

Journal Editing in the History of Economic Thought

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The editing work which I have undertaken has comprised something of a career in and of itself, that is, apart from my own research and writing. I edited the *Journal of Economic Issues* (which had many history-of-thought and methodology pieces) for ten years and have edited (and more recently co-edited with Jeff Biddle) *Research in the History of Economic Thought and Methodology* for sixteen years, each of which included materials both processed through the conventional review process and commissioned by me. I have also edited numerous volumes of commissioned materials, mostly in this field.

In this article I will cover the issues raised by Robert Leeson when he first proposed the minisymposium.

1. Common weaknesses of submitted papers and the modes of overcoming them: I am not sure that there are common weaknesses; most weaknesses are, I think, relatively idiosyncratic. An occasional situation involves the author whose vision is limited and needs to have their sense of deeper issues enlarged. A converse situation is one in which the author is only too aware of the larger questions but neither wants to nor can treat them more fully in a book. A related situation is one in which the author reaches for conclusions which go beyond what can be sustained on the basis of the manuscript (and related work) itself. But all of these situations are relatively rare.

To focus on common weaknesses of submitted papers is not inappropriate. But I would stress a different point: In comparison with the published work on the history of economic thought during the 1950s and 1960s, the period when I entered the field, the work of the last fifteen or so years is, in my view, overall markedly superior. It is far richer: It is much less Whiggish. It is much more self-reflective and hermeneutically sensitive. It pursues diverse topics and approaches largely beyond the imagination and surely the practice of historians of economic thought working back then. In writing this, I do not intend to denigrate the work of Alec Macfie, Lionel Robbins, Joseph Schumpeter, Joseph Spengler and Jacob Viner, to mention only five names; from them we learned a great deal, some of it realized and intended by them and some not. I want rather to celebrate the enormous diversity and penetrating depth of the work published in recent years. The field has never been cut and dry, at least in the twentieth century; but it is far, far less so today than it was thirty or forty years ago. Just how, and why, this transformation came about is a worthy research subject.

2. Non-HET journals: Alas, most, but by no means all, of their editors are not interested in publishing in the field; they do not consider their "general journal" or even "specialized journal" status to warrant history-of-thought work. In support of this, the editors can point to the history-of-thought journals as more reasonable outlets. I wrote "alas" because I would prefer for economists generally to be more interested in historical matters. But is this such a deplorable situation, and if so, why? Getting into non-HET journals would not mean that the journals' regular readers would actually read the history-of-thought pieces. It would mean that these readers would see that the pieces were there, and perhaps get from their presence some degree of respect for such work. But historians of economic thought, in my view, ought not get so enmeshed in the status emulation game to feel bad that they can publish largely only in

the journals in the field. The problem, I know or at least suspect, is that when their work is evaluated for promotion and/or merit raises, not publishing in the so-called "leading journals" in economics can be held against them by myopic colleagues and administrators. The answer is to educate one's colleagues and deans about the role and, yes, putative status of general journals.

3. The refereeing process: Let me say something of my view of the review process. Reviewers are very important to both author and editor in order to produce the strongest piece possible for publication. Authors receive from reviewers (their own and those used by editors) insights into how informed readers react to their work, especially about lacunae and other problems. But the article is the author's, not the reviewers'. It is the task of the editor to mediate between what the author has in mind to do and what the reviewers want done. I frequently write to an author, in transmitting reviewer reports, that it is after all the author's article and that all parties want to publish the strongest possible article; that the author should consider the reactions and suggestions of the readers and revise on the basis of both those suggestions and their own objectives. Authors have to recognize that referees may have something useful to suggest. Referees have to recognize that the paper is the author's and not theirs. And the editor has to give the author their discretion, given what the readers have had to say. All this assumes, of course, a potentially publishable submission.

The review process in the field is somewhat complicated by the fact that many ways of working exist in the history of economic thought (Samuels 1997). Reviewers may have their own prior conceptions and preferences, but they must be open minded when it comes to evaluating others' mode of work. In the case of young authors, the review process must be both gentle and mentoring, constructive and not demeaning.

4. Payment to referees/charges to authors: Given the relatively nominal amounts conventionally involved, and given that many individuals will both pay, as authors, and receive, as referees, monies, I do not think this is a big issue. I know, or suspect, considerable asymmetry exists between payment and receipt distributions. I appreciate that submission charges, especially relatively large ones, can inhibit less confident submissions. Such charges affect younger and older (lower and higher paid) authors differently. I appreciate that services ought to be paid for. But the transaction costs are non-negligible; the amounts are generally not large enough (nor should they be) to effectively dissuade; and professional service as a referee both should be its own reward and does count on merit forms. I am not a fan of such payments/charges.

5. Most reports are good to outstanding, the criterion being helpfulness to author and editor. Casual and/or uninformative reports I find to be rare. On more than one occasion, I received a referee report which was better, including being both more informed and more informative, than the paper under consideration. Yet in one case, the author of such a report, whose work I have published, was himself very sloppy in several ways in his own work!

6. Anonymity: I do not have strong feelings on this, perhaps because in my editing experience in this field I have not recognized any examples of discrimination by referees, and *I have been alert to their possibility*. My practice typically has been single-blind reviewing (referee will know author but author will not know identity of referees unless a referee desires identification) unless author provides for and/or requests nondisclosure of name. In many cases, authorial self-referencing prevents secreting identity. In some cases, where I think author identity and/or affiliation may be a problem, I have made efforts to delete identity

where possible (though not, so far as I can recall, in this field; I have also co-edited the *Journal of Income Distribution*, where I have done so). (In refereeing papers for other journals, I sometimes inform the editor that my anonymity should be lifted—where I think that private communication may be helpful to the author.) I am very attentive, I believe, to issues of fairness—e.g., open discussion and opportunity for heterodox topics and positions to be aired, given the presence of a substantive contribution warranting publication—and that includes identity matters. Perhaps I have been less effective than I might have been and should have adopted double-blind refereeing.

I have received, and eventually published, manuscripts whose authors, in their letter of submittal, indicated concern lest doctrinally or otherwise hostile reviewers be chosen. I do my best to select helpful and non-discriminatory readers in both these and all other cases; in these cases, I have tried to relieve the minds of authors. I should also note that when I was editor of the *Journal of Economic Issues* I regularly—perhaps occasionally would be a better word—employed readers who I expected to be hostile, so as to better enable the author to have the opportunity to prepare an article presenting his or her position in as strong a form as possible and to avoid in advance minor issues. I used as many as seven referees (my usual practice is three or four), in part to gather in advance as much criticism as possible as the basis of constructive revision. This has not been much of a problem in this field, though here too I have used expectably critical readers as referees. The reader can likely trace my sensitivity to fairness issues to both my ideological preference for openness and to my own experience as an Institutionalists in a predominantly Neoclassical world.

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Reference

- Samuels, Warren J. 1997. "The Work of the Historian of Economic Thought," *Research in the History of Economic Thought and Methodology*, vol. 15, pp. 181-197.