

EDITORIAL POLICY AS CONVERSATION AID

BY

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The invitation is to discuss my *JHET* editorship, which ran from 2013 to 2018, in light of the once and future challenges of the journal. After accepting it, I gathered my recollections, perused back issues of the journal, and finally reviewed my correspondence and reports to the HES Executive Committee.

Not many in our field are surprised when the documentary evidence does not match the historical subject's recollections. I confess it does surprise me, now that the subject is myself and the history is so recent.

Judging by my reports, beginning with my proposal for the editorship in January 2013, the challenges were how to “maintain the journal’s record of publishing articles in the history of economics with ... broad historiographical embrace, good readability, and relevance to the HES membership”; also how to “extend the journal’s reach, disciplinary and geographic.” That was how I stated my aims. Over the following five years, my reports to the executive were about acceptance rates, length of time of the publication queue, means of disseminating accepted papers before publication, the occasional special issue or similar initiative, communications with our publisher about page counts ... and, invariably, impact factors. Among what may be called the avowed challenges, this last was foremost. The ubiquity of reference during those years to the Clarivate Analytics company’s calculated two-year impact factor as a measure of journals’ scholarly relevance, including in the field of history of economics, where citation practices do not warrant it, was perennial cause for concern. (The interested reader may find fuller expression of the concern in José Edwards and Stephen Meardon [2021] and the related symposium articles.)

Although these challenges stand out in my reports, they do not stand out so distinctly in my memory. This time I will make a case for putting memory above the plainest evidence. I think the case is supported by subtler evidence in the printed issues of the journal. Anyway, what I said at the time only reflected the obvious work of editing the journal. The more challenging work, as I remember it, was different.

As to why publishing articles of broad historiographical embrace, good readability, etc., was not so challenging after all, the reader needs only to be reminded of the state of the field and the journal in 2013. Steve Medema’s *JHET* article “‘The History of

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Economics Is What Historians of Economics Do” (2009), a tribute to Bob Coats that was also a coda to his own editorship, had been published four years before. Its resistance to “an overly rigid definition of what constitutes good history” (p. 387), its accommodation of “the perspectives of rational reconstruction, historical reconstruction, science studies, intellectual history, and biography” (p. 386), its appreciation of archival research, had become commonplace. Although Medema spoke of historiographical “fractionation” (p. 386), implying controversy and even some rancor, by 2013 it was abated in the field and absent from the HES Executive Committee. Historiographical catholicity was implied by the stated scope of the *JHET*. We might call it the Denver consensus, if we allow that its origins and reach were broader. Anyway, my proposal for a broad historiographical embrace by the journal was no big whoop. Achieving it was little challenge. (As for readability, while I put some time into ensuring it, standards were already high and the journal had an outstanding copyeditor in Patricia Sanders.)

The challenge was not how to accommodate submissions with a broad variety of contents and methods (and, underlying them, different historical and social values) so much as how to bring their authors into meaningful conversation with each other.

To highlight the difference, consider first an editorial policy of equal publication opportunity for different factions, or fractions, as it were, in the “fractionation.” How to implement it? Maybe the editor sends a signal of that intention, then stands back to see what lands in the inbox. Receiving it, he gives it a quick read to check that it does not fall short of basic scholarly standards, then sends it to reviewers from the author’s faction. Whether or not the editor belongs to that faction, he lets the referees have their say, averaging somehow their opinion, allowing the paper to proceed to publication when the constructed average rises above a threshold. (The policy is just one way of what is called gatekeeping: a gatekeeper, after all, can choose from several ways of working the gate, even ruling out the entreating and partial.) Fair implementation of this policy requires substantial work and may have good results. The author’s faction has a scholarly literature that is now marginally larger by aid of the journal. The author may advance her career. Members of the faction may feel they are treated fairly—and for that reason fractionation, even if no less fractious, may be less fruitlessly contentious.

Now consider a different policy. What is different is not the principle of equal opportunity for various contents and methods but the implementation and effects. Upon receiving the submission, the editor does a quick read not only to check conformance to basic scholarly standards but also to see that it deliberately joins the society’s conversation. Whatever may be the subject, there is a secondary literature known to the society that is relevant whether for comparison or contrast. If it is not invoked in some way, then the conversation is not being joined. The editor’s job is to tell the author so, reject from the desk, and give reasons. Referees are treated with solicitude, partly by sparing them what is rejected from the desk, partly by personalizing and otherwise tailoring the correspondence. More to the point of this policy, referees are selected with a view less of upholding the particular standards of some faction (although that is still in view) and more of ensuring that anybody in the society can profit from reading and responding. A referee from outside the author’s faction or one who straddles factions is selected. The referee may wonder why; the editor may need to explain. In decision letters, the editor must see whether and how the referees’ advice may be reconciled, and this is likely to be trickier than under the preceding policy. He may have to explain to a referee why she is receiving a resubmitted paper that bypasses conspicuously some points of her advice,

and why, even so, he is soliciting her advice again. Or he may have to explain to the author why the paper is rejected despite garnering the approval of a referee who is apparently from the same faction and the disapproval only of one who is not.

This second policy was my own. The foregoing activities and intentions are what editing the journal meant. One might adopt the policy by disposition, as I did, or better by deliberation. Either way the challenge is forming case by case the right judgment, and finding the appropriate words, to aid a conversation among authors working with different contents, methods, and values. So far as the challenge is met, the good result is the ampler scholarly conversation. Granted, it may come at the expense of good results of other policies like the first.

Whether this kind of editorial policy as a conversation aid also describes what my predecessors or successors have practiced, I am not sure. One needs inside knowledge. As to the current editors, whose practices I have most nearly seen from the inside—Jimena Hurtado as my highly valued associate editor, Pedro Duarte as editor for a complicated submission of mine that he evaluated and rejected with appreciated diligence—I guess so. One could ask why, then, it is worth talking about. I will try to answer. First I will give an example of the policy in practice.

The March 2015 issue (vol. 37, no. 1) includes a lot of fine work. Most prominent is the previous year's HES presidential address by Margaret Schabas. For the present purpose I wish to draw attention to items farther down in the table of contents: first, an article by Steven Kates (2015a), "Mill's Fourth Fundamental Proposition on Capital: A Paradox Explained"; second, a "book forum" comprising a lengthy review by Marcel Boumans (2015) of Kates's book, *Defending the History of Economic Thought* (2013), together with an equally lengthy response by Kates (2015b).

To begin with the book forum, it is worth recalling Kates's purpose in the book. It was to document the controversies over the efforts first by the Australian Bureau of Statistics, then by the European Research Council, to change the categorization of scholarship in the history of economic thought so that it counted as history, not economics. The change had professional consequences that worried many in our field. Kates deplored the consequences, resisted the changes, and gave reasons. In his review, Boumans objects not so much to the worries as the reasons: his title is, "With Such a Friend of the History of Economics, Who Needs Enemies?" He scorns Kates's fashioning of Medema, Schabas, and Roy Weintraub, among others, into adversaries of the field: Medema for his skepticism of historical work directed to some present end, Schabas and Weintraub for going still farther toward separating the history of economics from economics.

Boumans's review originated, as usual, via solicitation by the book review editor, Maria Pia Paganelli. In view of the content and length, about double the norm, I suggested some repurposing. Before any input by me, though, the review did several good things that it maintains as published. One is to point up history's function of revealing the straw men that economists are wont to construct from the past when they do it for present purposes. Adam Smith, John Maynard Keynes, Robert Lucas, and Edward Prescott are cases in point. Scholars with genuine historical sensibility can revisit the figures from the past whom these economists have mistreated—Smith's mercantilists, Keynes's classics, Lucas's Keynes, and so forth—and do better. The argument is familiar, but Boumans gives it a sharp and targeted expression. Indeed, beyond sharp, he goes for cutting, referring to Kates foremost as a "former lobbyist" (Boumans 2015, p. 139). Kates responds at equal length, and ably, first by conceding Boumans's fair-

mindedness, then by owning the lobbying stuff (a good skill to master for battling government bodies), finally by reading closely his adversaries, specifically Schabas and Weintraub, and defending himself against the charge of misrepresenting them.

The editor's marginal contribution to all this was to invite Boumans to put his original review to a larger purpose (easy enough, as this comported with his own idea); to persuade Kates to divert and distill other work of his that was currently under evaluation, so that it would respond specifically to Boumans; to make selective use of the advice of a valued referee, chosen by considerations described previously (here I glance to the h.e.t. firmament in appreciation of this departed colleague and exemplary scholar); and to juxtapose both Boumans's review and Kates's response to an apparently unrelated research article authored by Kates himself (the aforementioned one on "Mill's Fourth Fundamental Proposition on Capital")—which article a patient reader may find to illuminate the forum unexpectedly.

To wit, the same Kates who stakes out a historiographical position against Medema, Schabas, and Weintraub, and who defends it against Boumans, writes a history serving admirably one of the prime functions of history according to his ostensible adversary. Talk about identifying the straw men that economists construct! Kates shows how John Stuart Mill's fourth proposition respecting capital, which is ancillary to Say's Law, became incomprehensible not only because of Keynes's reconstruction of Say's Law in the *General Theory* but even before that, beginning with Alfred Marshall; and how it remained so afterwards, from Friedrich Hayek to Sam Hollander, until now. The argument is impressively substantiated, eloquently written, and, in this reader's view, convincing.

Curiously, what Kates convinces of is not what one would suppose, given his response to Boumans. The gist of the response is that "the history of economic thought should become a center for critiques of mainstream theory" (Kates 2015b, p. 149). The Mill article is no such critique. It makes no argument that, as to the fourth proposition per se, Mill was right and his interpreters wrong. Its argument is about what the proposition is and why you probably did not know. Whatever critique of mainstream theory Kates may sometimes make, this time he has subordinated it to another purpose, namely, to understand historically the thoughts of Mill and his interpreters, period. The upshot? Conversations between scholars as different as Boumans and Kates may be valuable in several ways, including this: they show that their historical interests, methods, and values may coexist more agreeably than one supposes.

The policy of fostering such conversations should be pursued at least as actively, and more deliberately, than this erstwhile editor did. The elevation of social justice as the purpose of higher education was never promising for the scholarly conversation within its institutions. Over the past decade the range of values represented in the conversation has been restricted with gathering pace. The conversation suffers. Although the affliction is not so grave in the history of economics as in higher education at large, maintaining our fortunate condition depends on our appreciation of the variety of methods and values represented in our field. One appreciates it by having conversations that reflect it. They are likeliest where they are cultivated deliberately.

COMPETING INTERESTS

The author declares no competing interests exist.

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