

## Some Reflections on Editing *History of Political Economy*

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### History

My editorial career began by accident. In the 1960s there were three of us at Duke who were committed to the history of economics -- as there are today, but now a different three. Then Joe Spengler taught the graduate sequence and conducted research on a wide range of topics; I taught the undergraduate course and was completing a series of studies of economic thought in Commonwealth countries; Robert S. (Bob) Smith introduced a lot of history of thought into his courses on economic history and he did research on the history of economics in Spanish-speaking countries. Like others in the field at the time, we talked often about the decline of interest we perceived throughout the scholarly community, and after a while we determined to resist the trend by the creation of a society -- and perhaps the establishment of a journal. The main problem we perceived was that we were all very busy: Joe was engaged in innumerable projects, international, national, and local; and he was only a few years from retirement. Bob was department chair; and I was vice-provost of the university, with a variety of responsibilities and a sabbatical year coming up in 1967-68.

So we three worked out a strategy and a division of labor. We would attack on all fronts but devote most of our efforts to a journal. Joe would put at our disposal his considerable reputation and be the front man when we needed him. He became chair of the Editorial Board. I contacted the director of the Duke University Press and learned that they would be glad to work with us on a new journal so long as it didn't cost them anything. I agreed to find the money and sound out the academic community during my sabbatical. Bob Smith agreed to become editor when he retired from the chairmanship in 1969.

For a while things went remarkably well. The Rockefeller Foundation, the National Science Foundation, and the Ford Foundation provided generous funding to hold a kick-off conference and to cover the expected losses of the first five years of publication. In Britain, where I spent my sabbatical, I learned of a recent abortive attempt to start a journal in the field and saw some lessons in this failure. I described our own plans to the first U.K. Annual History of Economic Thought Conference held at the University of Sussex in 1968. In general I found the younger scholars in Britain (Black, Blaug, Coats, Hutchison, Meek, O'Brien, Winch, etc.) to be very enthusiastic while the old timers were much less so. A low point occurred at Sussex when Lionel Robbins rose to say he was strongly opposed to a specialist journal on the grounds that this would isolate the historians from the profession and make it easier for general journals to reject manuscripts. Indeed, he would advise his "former pupils" to have nothing whatsoever to do with the project!

In the United States and other parts of the world, we heard no such discouraging words as in the U.K. Virtually all historians of economics that we approached, both young and old, were enthusiastic about the new journal and pledged their support. It was clear from the beginning that if we were to reach critical mass in this small field our community would have to be global, and that is what it became. The membership of the first editorial board gives some indication of the diverse nationalities, scholarly styles, and age groups that we sought.

Joseph J. Spengler, Chairman, Duke University  
William R. Allen, University of California, L.A.  
Robert D.C. Black, Queen's University, Belfast  
Martin Bronfenbrenner, Carnegie-Mellon University  
A. W. Coats, University of Nottingham  
Joseph Dorfman, Columbia University  
Robert V. Eagly, University of Massachusetts  
Frank W. Fetter, Northwestern University, Emeritus  
William Jaffé, Northwestern University  
George J. Stigler, University of Chicago  
Vincent J. Tarascio, University of North Carolina  
Vladimir G. Treml, Duke University  
Robert M. Will, University of British Columbia  
Donald R. Winch, University of Sussex

The inaugural conference in the fall of 1968 went very well and yielded excellent papers for the first issue that came out in 1969. But then troubles began. The turmoil in the United States over the war in Southeast Asia and civil rights boiled over into the colleges and universities and as an administrator I suddenly found myself with full days devoted mainly to crisis management. On one occasion, a group of students took over ("liberated") our administration building and agreed to leave peacefully only if we agreed to a marathon bargaining session over their "non-negotiable demands." Bob Smith and I were both named to the 12-person faculty-administration negotiation team and were locked in the Law School over a snowy weekend with the same number of students and "advisors" from the outside world. It was an extremely stressful time, swinging back and forth from high drama to Gilbert and Sullivan (on the second day, seeking symbolic retribution for years of mistreatment, the students ate the faculty lunch before it could be put on the table by the caterer!). Shortly after the session ended Bob Smith suffered a heart attack and died a few days later.

So, sadly and unexpectedly, the department lost its chair and the journal lost its editor-designate. But happily the new chair John O. Blackburn was highly sympathetic to our plight and supportive of a solution. He said to me, "Find the best young person in the field as Bob Smith's replacement and we will appoint him." Accordingly I wrote around widely and was told that this person was Neil de Marchi, then just finishing his Ph.D. at the Australian National University. I was in Australia on other business in 1969 and persuaded Neil to come to Duke after a research leave in Britain. This was perhaps the most important piece of good luck in *History of Political Economy's* (*HOPE's*) history and he has been associate editor (effectively co-editor) ever since. Both of us have come and gone from Duke several times over the three decades but together we have kept the editorial wheels turning.

When I discovered in 1969 that perforce in addition to my other troubles, I was going to become editor of *HOPE* I read a few manuals on how to be an editor. As I recall they enlightened me a little, but not very much. We have typically proceeded by trial and error and tried to sort out answers to questions as they arose. The following is an account of some of the editorial issues we have addressed, how we dealt with them -- for good or ill -- and some reflections on how our decisions look today in hindsight.

### Location and Identity

We recognized that there were three possible constitutional positions for the new journal. It could be affiliated with, or a part of, a professional society. The obvious candidate was the new History of Economics Society then becoming organized in the 1970s. Second, it could be a publication of the Duke Economics Department. And third, it could become the possession of a journals publisher. With the first two options we feared loss of editorial autonomy and the need to assume responsibility for a multitude of demanding business functions (copy editing, production, fulfillment, warehousing, advertising, etc.). We offered ownership of the journal to the Duke University Press and we have been very pleased with the decision. During the early years we maintained a cooperative relationship with the History of Economics Society, offering discounted subscription rates to the members and collecting membership dues. As the society grew and started its own journal we parted as friends.

The success of the journal over the years has been very close to what was predicted at the start. After about five years, when the subsidy ran out, we broke even, and ever since we have remained roughly on a break-even basis by adjusting the costs (pages printed) to revenues received. We went from two to four issues a year in 1974. The growth in the subscriber base of the journal has been as follows:

| YEAR | PAGE TOTALS | INDIVIDUALS | LIBRARIES | TOTAL<br>SUBSCRIBERS |
|------|-------------|-------------|-----------|----------------------|
| 1976 | 570         | 404         | 944       | 1348                 |
| 1980 | 642         | 370         | 1037      | 1407                 |
| 1985 | 689         | 346         | 1058      | 1404                 |
| 1990 | 775         | 367         | 1169      | 1536                 |
| 1995 | 802         | 328         | 1150      | 1478                 |

We presume that today we reach virtually all individuals and institutions with a serious interest in the field. The number of subscribers crept up gradually until reaching a plateau in the 1980s, and since then vigorous subscription drives succeed mainly in gaining short-term new subscribers who then fall away. Some reduction in subscribers has been experienced with the increasing poverty of university libraries in the 1990s. A total of 3,132 manuscripts have been submitted over the life of the journal to date. At present we receive about 100 per year, of which we can publish about 25. Only in the most exceptional cases do we commission articles. As befits a journal in this field, we retain all records, including rejected manuscripts and all correspondence. These will become available to scholars through the Duke Special Collections Library. A question for *HOPE* in recent years has been whether events in the field worldwide, such as the establishment of new societies and other journals, would affect either the number of subscribers or the quantity and quality of manuscripts submitted, positively or negatively. Neither seems to have been the case.

## Editorial Role

Delicate questions are involved here, and answers are likely to sound self-serving. But that can't be helped! I shall review a few of the questions related to the function of an editor and our answers to them.

First, should the editor dedicate a substantial slice of an academic career to the job? My response has been "no," partly on grounds of principle and partly taste. There are certainly enough tasks related to journal editing to take all of an editor's time and effort if he or she so wishes. But is it good for the editor or the journal to do so? I think not! I have never had released time or special compensation for my editorial tasks, and I think that's the way it should be. It is possible for an editor to put too much time and effort into the job. It can be an escape from scholarship, like any other appointment to academic administration. Moreover, there is a danger that an over-zealous editor will feel the obligation to impose his or her tastes and values too strongly on the journal. My view of *HOPE* has always been that it is, or should be, a reflection of the best scholars in the field -- not of the editors. Therefore one of my main functions is to identify those scholars and persuade them to play their various roles as contributors and referees. This sharing of editorial responsibility not only with the associate editors but with the referees and board members, reduces the demands on the editor substantially.

Another advantage of constraining the demands on the editor is that this person can remain an active teaching and publishing scholar. This, I believe, is a better position from which to make editorial decisions than that of the fully committed editor. It may be that the beleaguered condition of the history of economics, or its essential interdisciplinary character have engendered a mood of generous cooperation among colleagues. But for whatever reasons, unstinting cooperation by generous colleagues has made the part-time editorial role possible. The associate editors, Neil De Marchi, A. W. (Bob) Coats, and E. Roy Weintraub, have labored hard with no reward but the appreciation of our readers. A succession of exceptional Duke graduate students, most of whom have gone on to distinguished scholarly careers, have helped with manuscript review, proof reading, and innumerable other tasks. These have included: John Lodewijks, David J. Hoas, Jane Rossetti, Jin Bang Kim, Michael Lail, Robert J. Leonard, Jeff Roggenbuck, Ted Gayer, Steven Meardon, and Spencer Banzhaf. The decision was made in 1994 to appoint a managing editor for *HOPE* and Beth Eastlick became the first incumbent. This was one of the happiest developments for the other editors who found many of the most irksome editorial responsibilities lifted from their shoulders.

Although we have attempted not to impose our template on the field and to respect on our pages the various styles and traditions selected by the authors, we have tended to give special attention to promising innovations. For this we have been accused of being trendy, but so be it. We have used devices like minisymposia, annual supplements, and review articles to engender controversy and stir up the pot. When we receive angry letters about these efforts (infrequent) we tend to feel we are really doing our job.

A question we have naturally asked from time to time is whether we should shift the editorial office of *HOPE* around to other universities. Obviously the answer thus far has been in the negative. The main reason may be a selfish one. We enjoy the association with the journal and the place it gives us at the pulse of the field. The journal and its related program are complementary to other activities at Duke such as graduate education and our own research. Other reasons for the retention of the office at Duke are the cost of mobility and our fear, perhaps unjustified, that the journal and the field are still sufficiently fragile that they might suffer grievously from any locational mistake.

The question of how to create the individual issues of *HOPE* faced us at the start when we could see three possibilities. The first was to organize issues by themes (e.g. Walras or the Keynesian Revolution). The second was to select the most attractive manuscripts over the interval between issues, reject all others, and start a new selection process afresh every three months. The third was to try to match roughly a flow determined by some quality standard to space availability and publish from an inventory arranged by acceptance date. We determined that the first approach should be kept for special issues; the second approach seemed potentially demoralizing to contributors and likely to contribute to wide fluctuations in quality among issues. So we settled on the third. We send each manuscript received (except those that are patently inappropriate because of wrong field or frivolity) to two referees who we believe are serious and competent on the topic of the paper - sometimes board members but not always. If both reports are strongly positive we accept and put the paper in the queue. If they are negative we decline. If the reports are inconclusive we take one of several courses: go to a third reader, propose a substantial revision, or conclude that the prospect of eventual publication is low and reject. Interestingly, by some sort of law of nature this approach has yielded us just about a steady flow of papers over the three decades at just about the right velocity so that we have a constant inventory of about seven issues waiting to enter the publication pipeline. The benefit of this approach is, we think, equitable and fairly predictable treatment of contributors; the penalty has been a time lag in publication from first submission to printed pages that, excluding revision time (which depends on the author) has typically been 2 1/2 years. By regulating the various valves at our disposal we have tried repeatedly to reduce the inventory and the lag somewhat, but the fecundity of our contributors has stood in our way.

### Use of Board of Advisors and Referees

The *HOPE* Board has never met! Editorial decisions are the responsibility of the editors, which is the way we think it should be. The Board contains colleagues to whom we go often for advice on matters of policy and concerning the decisions on manuscripts. Their only direct reward is a free subscription. In addition, there are innumerable scholars who, seemingly selflessly, advise us on individual papers. Naturally we tend to gravitate toward those referees who demonstrate the most sound judgment that is clear to us and to the author. We observed from the start that referees in the history of economics usually produce more extensive and constructive comments than do those in other subfields with which we are acquainted. We think this is partly because they share a sense of the threat that lies over the field from the discipline as a whole, and so colleagues are seen as allies not competitors and a referee report is perceived as an opportunity to strengthen not skewer the work of a colleague. In any event the referee reports we have received over the years (more than 6000!) have often been quite remarkable -- indeed, occasionally as long as and more interesting than the papers they address! In some cases, where the referee gives up anonymity, they have led to collaboration and lifelong friendship. We have toyed with the use of reviewer forms on which certain standard questions are listed. But we have opted for informality, receiving sometimes a separate statement and sometimes a letter. We think informality encourages spontaneity and yields the best reports.

One controversial decision we made at the start and have kept to ever since, is to pass along to authors all the comments of substance that we receive from referees. Sometimes these are frank, harsh, and very negative. One author called me a sadist for passing along one especially hurtful report. Our reasoning is that these reports are information on which we have based our decisions and all contributors should have the opportunity to "face their

accusers" and rebut them if they can and care to do so. Whenever an author makes a strong case that a reader's report is unsound or unfair we gladly go to another reader.

We have never gone to blind reviewing as a standard practice, for three reasons. (We do so if the author so requests.) Most important, some excellent referees refuse to referee under these conditions. They point out that recycling of old material is one of the most familiar forms of scholarly misbehavior and without the author's name this cannot easily be checked and prevented. Moreover, say some referees, blind reviewing leads to an irritating guessing game that they are unwilling to play. Ironically we sense that blind refereeing would protect just those contributors who it is charged are privileged under an open system. We regularly find that in the open reviews we receive the most prominent authors are given unusually harsh treatment by referees. Many referees rejoice in rejecting a Nobel Prize winner with the curt observation that "He should do better than *this*." By contrast, an assistant professor alone in the field at a small institution is likely to receive a long, encouraging, and thoughtful commentary on a flawed manuscript with numerous suggestions for improvement and offers of further help.

*HOPE* has never charged a submission fee in part because of the financial circumstances of some contributors, in part because of the exchange control faced by others. We also look uneasily at the extra accounting that would be involved. But the consequence of this decision is that we have no funds with which to pay referees and, therefore, no means with which to construct financial incentives for prompt responses. The only hold we have over referees is the critical gaze of the impartial spectator, and our regular wake-up calls. A consequence is that we tend to gravitate toward those referees who are most conscientious. The perverse reward for responsible behavior is more work. This policy may deserve a serious review in the future.

We are confident that the system of refereeing that has been used by *HOPE* and others in the field has contributed to the growth of community in the history of economics. Scholars have been brought together, albeit anonymously in most cases, who otherwise would have had no other occasion for exchange of views. At the same time, we have some residual unease and nagging questions about some other consequences of the system. Do certain individuals, and especially those who perform their refereeing tasks promptly and responsibly, gain too much influence over the field? Is there an inherently conservative bias in an arrangement that places the gatekeeping function predominantly in the hands of those who have already made their reputations? Does the system erect barriers to novelty, to Kuhnian revolutions, to challenges to hard cores of Lakatosian research program, and so on? I don't have the answers but try to remain alert to the dangers implied.

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