

Journal Publication in Economics: A View from the Periphery¹

Pranab Bardhan

University of California at Berkeley

Given the supreme importance of publication in journals for advancement in our profession it is not surprising that a fair amount of attention has been paid to analyzing its patterns and trends². The purpose of this short paper is to point to some specific problems faced by economists who write not primarily in the areas of mainstream concern in economics, but in the somewhat peripheral fields (like development, history, urban economics, etc.), and these problems are largely neglected in the general analysis of journal publication. Having myself toiled for many years in the field of development economics, guided many young students in that field, and edited one of the main field journals, I may have acquired over the years some understanding of the problems that economists in such peripheral fields, particularly those who are not yet established or tenured, face in the highly competitive world of journal publication. While many of my examples will be drawn from this field, I understand that some of the other fields have also encountered similar problems. I am writing this in the hope that some general awareness of these problems may enhance the quality of competition and productivity in this market and broaden the range of our professional priorities and concerns.

Let me start with what many young economists in these peripheral fields will describe to you as a Catch-22. When they submit their papers to the mainline journals, they are often sent back (sometimes without a review, or with a cursory one) with the advice that they should try a field journal instead; and yet at tenure time the tenure committee is prone to count the number of papers in mainline journals. I know of cases where tenure has been denied to candidates with a fair

¹ I have benefited from discussion with Alan Auerbach, Angus Deaton, Barry Eichengreen, Glenn Ellison, Maitreesh Ghatak, Karla Hoff, Howard Pack, Gerard Roland, and Christopher Udry on an earlier draft. Usual disclaimers apply.

² Some of the more recent references are: Ellison (2000), Hamermesh(1994), Laband and Piette (1994a and 1994b), and Laband and Wells(1998).

number of splendid articles in field journals but a paucity of articles in the top three or four mainline journals (of course, not for their lack of trying), and at the time of denial they have sometimes been explicitly told so.

Even within the field of development economics in recent years young researchers have got the definite impression that if you carry out cross-country macroeconomic regressions on the basis of easily downloadable aggregative datasets, your papers are more likely to be entertained by the mainline journals than if, for example, you carry out detailed micro-econometric analysis with data sometimes painstakingly collected from field research in Algeria or Zambia — in the latter case you are relegated to the field journals, even though the results could be of general interest and salience. So this is beginning to distort the directions of research toward what I regard as less productive avenues, apart from discouraging otherwise interested bright students from entering peripheral fields. The latter effect can generate a vicious circle, in a way familiar from models of statistical discrimination: if general-interest journals in effect discriminate against some peripheral fields, in equilibrium the pool in these fields may indeed become weak, justifying the initial bias.

It so happens that development economics has undergone a major transformation in the last two decades, and the field is brimming with new and sophisticated theoretical and empirical analyses of, among other things, the complexity of interactions between market and non-market institutions, which, I believe, can be of great interest to economists in general, even those who are only marginally interested in the conditions of the overwhelming majority of the world population who live in poor countries. But you will not see this major transformation in development economics reflected much in the number of publications in mainline journals. According to the estimates of Ellison (2000), the fraction of papers on development in the top five (mainline) journals³ has actually declined substantially in the last two decades, from 3.8% in the 1970's to 1.6% in the 1990's. (In urban economics the decline is from 2.2% to 1.1%, in public finance the

³ *American Economic Review*, *Quarterly Journal of Economics*, *Journal of Political Economy*, *Econometrica*, and *Review of Economic Studies*.

decline is from 6.1% to 5.3%, and in economic history it has remained at about 1%; on the other hand, in macro and finance taken together the fraction has jumped from 22.7% to 29%)⁴.

Of course, some of the best papers in the respective fields are still published in the mainline journals. I recently edited (with Chris Udry) a two-volume *Readings in Development Economics*, and of the 24 articles we selected, as many as 14 were originally published in the top five mainline journals over the last fifteen years. But most (though not all) of these 14 papers are by authors who are also prominent in at least another area of economics. There is a general impression that mainline journals are more receptive to development authors who are also well-known in fields outside development, closer to the mainstream. This adds to the burden of perceived demands on a potential author and raises the threshold of acceptance for young unknown researchers. It is not surprising therefore that many of these young and not yet established economists in the field feel very strongly that they face barriers of entry into the top mainline journals.

Take one of the most distinguished mainline journals, *Quarterly Journal of Economics*. In the 10 years from 1991 to 2000, this journal published a total of 463 articles (including comments, etc.). Of this total a full 30% was from authors in Cambridge, Massachusetts (in the sense of at least one author being currently affiliated to Harvard or MIT)⁵. Of course, we all know this small city is overloaded with some of the world's best economists; and I used to think that charges of local favoritism are mainly generated by the natural frustration of rejected authors. Nevertheless, for a mainline journal, acting wittingly or unwittingly as one of the few gatekeepers of promotion or advancement in the whole profession of academic economists, this high a proportion of local authors cannot but attract attention. Equally noteworthy is the fraction of papers --about one-tenth -
- published in this journal over the last decade which are contributed by at least one author

⁴ Labor economics, which used to be considered a peripheral field some decades back, is doing relatively well. Some suspect this may not be entirely unconnected with the fact that the editorial boards of most of the mainline journals contain some of the major labor economists.

⁵ You may be interested in the fraction of Chicago affiliated authors in the *Journal of Political Economy*. By my calculations, this Journal published a total of 495 articles in the 10 years between 1991 and 2000. Of this total about 13% of the articles were contributed by at least one author affiliated to the University of Chicago.

belonging to its own editorial board (Editors plus Associate Editors⁶). On an average, this amounts to *every* issue of the *Quarterly Journal of Economics* over the last ten years publishing at least one paper by a member of its editorial board. I do not know of any other major journal that matches this.

One standard way of refuting the charge of local favoritism⁷ is to refer to the citation count of articles published in the journal concerned relative to other journals. The top mainline journals, almost by definition, have very high citation counts. I have no doubt this is largely because many of the papers published in these journals are clearly better than those published, on average, in the field journals. But this cannot be the whole story. Ellison (2000) reports that there has been a ‘dramatic decline’ in the number of citations to articles in the top field journals relative to that in the top five mainline journals between 1980 and 1998 (what he calls the *NciteRatio* falling to about half for the top field journals in the latter year of what it was in the former year). It is unlikely that there has been such a precipitous decline in relative quality for the top field journals in less than two decades, particularly when in some fields (like development, as I have mentioned before) there has been, if anything, a revitalization of research activity.

One likely explanation may have to do with the following. With more intensive specialization and the proliferation of field journals as well as of seminars and working papers, most economists do not read more than one or two field journals in his/her own field, apart from reading the top general-interest journals. Thus, for example, a good paper published in a field journal, even though it would have been of potential interest to many readers outside the field, does not come to their attention. This situation is clearly becoming more acute over time, and may have something to do with the dramatic decline in the citation to field journals. There is again a vicious circle here: with mainline journals publishing fewer papers in peripheral fields, and articles in field journals cited less often, economists who work primarily in the peripheral fields (and outside top schools) are in danger of being increasingly marginalized.

⁶ Roughly one-third of these contributions are by the Editors themselves and two-thirds by the Associate Editors.

⁷ See Laband and Piette (1994a).

Let me end with some comments on the refereeing process. It is the experience of most authors, confirmed by the findings of Ellison (2000), that the refereeing lag now is much longer than before, in both general-interest and field journals. This has, of course, added to the nervous waiting and desperation particularly for candidates with the tenure clock ticking. As a harried editor of a field journal, constantly implored by anxious authors and always in hot pursuit of laggard referees, let me point to some special problems field journals face in this respect.

In recent years the proliferation of journals has been particularly rampant in the case of field journals. The marginal cost to publishers of starting another journal is much lower than the "social" cost (among other things, the referees and editors are seldom paid the opportunity cost of their time), and as a result there is "overfishing" in the same pool of referees. The journal editors are now refused by referees with increasing frequency (often with the plea that the referee is involved in the editorial board of another journal which therefore has priority). It is now quite common in the top⁸ field journals for a paper to be refused by 4 to 5 referees before we finally get a referee to agree to do it. It is thus easy to lose 3 months or so after submission in just getting two referees to agree per paper (with increasingly peripatetic referees it sometimes takes even longer). As in the real world of overfishing, this problem will get worse unless there is some way of agreeing on and implementing 'community regulations' on the proliferation of journals.

Of course, with the increasing gatekeeping role of the top general-interest journals in the profession that I have alluded to earlier, the latter face a heavier burden of accelerating submissions. Faced with this these journals sometimes press into service a whole army of local graduate students (and fresh Ph.D.'s) on the refereeing job (it is suspected by some that this is particularly the case when the authors are not established or known to the editors). Graduate students and other younger reviewers are sometimes more meticulous in their refereeing jobs, but

⁸ Of course, the increase in the number of field journals could lighten the load for any particular journal in the field, but for the top field journals this is not much of a relief as most papers get submitted sequentially down a vertical chain of journals at different quality levels. The promise being made in some new online journals of branding different papers with different quality gradation ("gold", "silver", "bronze", etc) may cut down on the refereeing time lost, but many authors may prefer publishing in a third-tier journal, after trying the first two tiers, to being publicly graded as a bronze-quality paper in the same journal in the invidious company of higher-rated papers.

without the experience of general standards in the profession and with their youthful zeal they are sometimes apt to be harsher in their judgment of papers. This may sometimes raise the rejection rates at general-interest journals of papers by relatively unknown authors or articles in peripheral fields.

Several authors have told me that in their comparative experience they have received on an average better-quality referee reports in field journals compared to mainline journals. I used to think they are saying this just to please a field editor, but this has been confirmed to me by authors in many different fields (including one who is currently the editor of a top mainline journal). This may be because mainline journal editors (or co-editors) have to preside over the editing of papers on such a wide variety of topics, on an average they may not have close contact and expertise on who the more appropriate referees are on a particular topic (especially in the peripheral fields), compared to a field journal editor who presides over a much narrower range. It is my own experience (similar experience is confirmed by many of my colleagues) that, compared to field journals, mainline journals have often sent me papers to referee that are somewhat more remote from my current areas of interest. If there is any truth on this issue of relative quality of refereeing in the two kinds of journals, then it is all the more unfortunate that the profession gives such a low valuation in promotion and advancement decisions to articles published in field journals and cites them so much less, compared to articles in general-interest journals.

What is to be done? Some readers of an earlier draft of this paper expressed to me their concerns about the problems discussed here but essentially throw up their hands in despair. At a minimum, I think it is important to raise the awareness of the mainline journal editors and referees - - apart from the departmental evaluators of the research progress of young candidates -- of the special problems that researchers in peripheral fields face. The mainline journals did not ask to be gatekeepers. But their having become that, for various historical reasons in our profession, it is reasonable to ask them to consider their added responsibility and the consequences of their decisions for the fairness of the process. It is also the responsibility of those of us who write both in mainstream and peripheral fields to make deliberate efforts to help in the promotion and

dissemination of good research by young scholars who are sidelined by the mainstream. We can also try to discourage the proliferation of journals by thinking of the social cost of an additional journal that the commercial publishers are prone to ignore when they ask us for our service.

References

- G. Ellison, "The Slowdown of the Economics Publishing Process", unpublished, MIT, 2000.
- D.S.Hamermesh, "Facts and Myths about Refereeing", *Journal of Economic Perspectives*, 8(1), 1994.
- D.N. Laband and M.J.Piette, "Favoritism versus Search for Good Papers: Empirical Evidence Regarding the Behavior of Journal Editors", *Journal of Political Economy*, 102, 1994a.
- D.N. Laband and M.J.Piette, "The Relative Impacts of Economic Journals: 1970-90", *Journal of Economic Literature*, 32, 1994b.
- D.N. Laband and J.M. Wells, "The Scholarly Journal Literature of Economics: A Historical Profile of the AER, JPE and QJE", *The American Economist*, 42, 1998.