

Comments on Publishing in Economics

Women in Economics (WinE) Workshop on Successful Publishing

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Introduction

- May be editors are not the right people to ask about successful publishing. A successful editor is not necessarily a successful author: Artists vs. art critics. Editors are selected for their standing in the profession but in particular for their judgement and for being reliable. Their role is mostly passive in that they process what comes along. Successful authors are probably very focused in the activity of publishing, more so than editors. We should also ask them.

How to choose a journal

Top journals

- Publishing in top journals is very hard. What is needed?
 - Highly professional research that thoroughly looks at all aspects of the problem at hand. Professional writing is also a requisite. Doing research in this way is a lot of work.
 - Making a substantial original contribution to the literature: The value added hurdle.
 - That the editor likes it, i.e. that he/she thinks the paper is exciting and interesting.

- The table below shows the number of submissions to two generalist journals that publish both theory and econometrics. Acceptance rates are low, but some submissions to the two journals will be the same papers, so if we aggregate the picture is not so bleak.

Submissions and Publications
(annual averages)

Period	<i>REStud</i>	<i>Ec'trica</i>
Number of submissions		
1981 – 1999	450	488
2002 – 2005	735	593
change	63%	22%
Number of published papers		
1981 – 1985	54	93
1986 – 1990	46	71
1991 – 1995	44	60
1996 – 2000	41	65
2001 – 2005	41	69

- The number of submissions has increased but the number of articles published has not, so it has become increasingly difficult to publish in top journals.
- Where is the increase in submissions coming from? Probably from the incorporation of researchers in newer areas (Continental Europe and Asia) more so than from an increase in submissions of more traditional areas (UK and US).

Choosing a journal

- There are papers of many kinds. Each journal specializes in a certain kind of paper. A good journal aims at publishing the best papers of its kind.
- If you can get good advice from your supervisor or somebody else you should not waste time sending the paper to an inappropriate journal. Trouble is if one is uncertain. Real uncertainty is editor-taste uncertainty. In that case it may be worth trying a top journal.
- Reading published articles is a good way of getting to know what kind of paper is appropriate for a journal.
- Curiously enough if the paper is clearly inappropriate this is not very costly because editors tend to operate quick screening devices.
- The number of journals has increased a lot. This can easily happen if it is profitable for publishers and there are papers that cannot make it to existing journals.
- However, the credit from publications in non well-established journals can be very small, especially at leading departments. Publication in top generalist journals or top field journals continues to give immensely more credit than in lower rated journals.
- Special issues? Some journals do not do special issues (Econometrica), others occasionally do a symposia edited by regular editors (REStud), yet others may do special issues with guest editors (Journal of Econometrics) by invitation, often linked to a conference, or may open a call for papers, in which case you would hear about it.

Role of editors

- Choice of referees. I always read enough of the paper to form a view of its contents before choosing referees. If I think the paper is inappropriate for the journal I ask an associate editor if he/she agrees with me and if so to write a quick report for the author. In that case the whole process should happen within a month. Otherwise I assign referees. Choice of referees is very important for the quality of the process. Ideally you would like an experienced, quick referee that has a good understanding of the paper and the literature but is not necessarily too close to it.
- There are two basic models of operation. In one model (typically generalist journals) power of decision is concentrated in a small number of co-editors who assign referees and make decisions. Among them there is an overall editor or coordinator who distributes the papers. Then there is a board of associate editors that act as committed referees (Econometrica or REStud work like this).
- In another model, co-editors distribute papers among associate editors who assign referees and write an anonymous report, which is the basis of the co-editor's decision letter (Journal of Econometrics and other field journals operate like this). The second model means less work for co-editors but less of an editorial policy, some dilution of responsibility, and more variance of standards. It is better suited to a field journal that intends to publish all papers above a certain hurdle, as opposed to a generalist journal that intends to follow a publication policy.

- As an editor I do not expect a detailed submission letter. I prefer this information to appear in the abstract and the introduction to the paper.
- Acknowledgments and citations affect the choice of referees because they contain relevant information about the paper. To what extent there is significant scope for manipulation that matters to referee choice is not clear to me.

Revision process

- Most journal web sites provide information on processing times.
- I do not know why the refereeing process in economics is so long. May be we are just in a bad equilibrium. Writing a report seldom takes longer than one or two days, but many referees only start thinking about them after the deadline is passed.
- The pool of reliable referees is limited and increasing submissions strain the resources.
- Referees often feel they need to provide comments on the papers for the benefit of the authors, even if they recommend rejection. This is a kind of service that probably journals should not do. These expectations contribute to delaying the whole system and create more cost than benefits even to authors. The critical referee input to editors is a well argued editorial recommendation in the covering letter (or in the report).
- When the referee's recommendation is not to reject, it almost never is a straight acceptance or something close to it. The referees often feel obliged to plot a reshaping of the paper. This is also in our culture and probably we should move to a situation where this is done less. Authors should be allowed to write the paper the way they like to.
- Nevertheless, my impression is that there has been some progress in recent years. The electronic management systems have cut on administrative delays and implemented simple ways of keeping pressure on referees and editors.
- In my experience most rejected papers are turned down on the first round.

- Referees are selected by editors. As an editor I like the fact that a referee has published in the journal (specially if recently) because I feel it may help the referee to have a good understanding of what an appropriate paper for the journal is, but this is not a precondition. Again, electronic databases of referees have facilitated the selection of referees.

Formal issues

- I do not think editors care much about journal-specific formal requirements of papers (like mode of citation) when submitting for the first time.
- My feeling is that playing strategically with choice of citations seldom pays in terms of improving the chances of publication, at least in good journals.

General questions

Empirical papers and empirical content

- Empirical papers have become more central to economics than they used to. This reflects the new possibilities afforded by technical change in research and is a sign of scientific maturity of economics. The following table taken from Han Kim, Adair Morse, and Luigi Zingales (“What has mattered to economics since 1970”, NBER Working Paper 12526, 2006, p. 23) gives an impression of this change.
- I guess nowadays is not as easy as it used to be in the past to publish in a generalist journal a theoretical paper just because the analysis is internally consistent and mathematically elegant, in the absence of some evidence on the model’s relevance to explain some real life phenomenon.
- In an empirical paper the econometric strategy is often paramount, i.e. what aspects of data to look at and how to interpret them. This typically requires a good understanding of both relevant theory and sources of variation in data. Once this is done there is usually a more or less obvious estimation method available and ways of assessing statistical error.
- Statistical issues like quality of large sample approximations or measurement error may or may not matter much in a particular problem, but a characteristic of a good empirical paper is the ability to focus on the econometric problems that matter for the question at hand.

Panel B

This panel presents the percentage of highly-cited articles whose primary contribution is in econometric methodology, empirical results, survey, theory and experiments by half-decade. The grouping is based on the authors' collective familiarity with the articles or a quick reading of articles. The counts of articles for each contribution type and half-decade are in parentheses.

<i>Main Contribution</i>	<i>1970- 1974</i>	<i>1975- 1979</i>	<i>1980- 1984</i>	<i>1985- 1989</i>	<i>1990- 1994</i>	<i>1995- 1999</i>	<i>Total</i>
Methodological	6.7% (2)	28.2% (11)	28.6% (10)	28.6% (10)	22.9% (8)	22.9% (8)	23.4% (49)
Empirical	13.3 (4)	10.3 (4)	11.4 (4)	25.7 (9)	31.4 (11)	60.0 (21)	25.4 (53)
Survey	3.3 (1)	--	2.9 (1)	--	2.9 (1)	5.7 (2)	2.4 (5)
Theoretical	76.7 (23)	61.5 (24)	57.1 (20)	42.9 (15)	40.0 (14)	11.4 (4)	48.7 (100)
Experimental	--	--	--	2.9 (1)	2.9 (1)	-	1.0 (2)
Observations Total	30	39	35	35	35	35	209

Han Kim, Adair Morse, and Luigi Zingales (2006): "What has mattered to economics since 1970", NBER Working Paper 12526, p. 23.

Exciting topics in two or three years time

- I wish I knew what will be the trend four years from now. The process by which some topics become very fashionable over some period is not difficult to understand. A new idea carrying force comes into play, this may be from within economics or perhaps imported from some other discipline. The idea is then pursued in the research agendas of various people; if it proves fruitful, more people enter the area until more or less everything that could be done is done.
- In applied microeconomics there has been a lot of excitement in recent years in empirically establishing causal impacts of interventions (from field and natural experiments and the like). This is understandable because in principle causal impacts are more useful for policy than correlations.
- However, there is an increasing awareness of the limitations due to heterogeneity of responses and interactions and dynamic feedback. Addressing these matters require more theory. A good thing of the treatment effect literature is that it has substantially raised the empirical credibility hurdle.
- A challenge for the coming years is to have more theory-based or structural empirical models that are structural not just because the author has written down the model as derived from an utility function but because he/she has been able to establish empirically invariance to a particular class of interventions, which therefore lends credibility to the model for ex ante policy evaluation within this class.

Journal ratings

- I would say that the more established ratings are the ones based on citations. Despite their shortcomings ISI-JCR impact factors are widely used.
- I particularly like the axiomatic method proposed in Ignacio Palacios-Huerta and Oscar Volij (2004): “The Measurement of Intellectual Influence”, *Econometrica*, 72, 963-977.

Concluding remarks

- Do not think too much about the mechanics of publication at the time of starting a research project. Papers that are engineered to suit current fashion often look dull and are hard to publish. Even if they are not, there is no substitute for the excitement of scientific pursuit of discovery. If we are to do research as if we were selling toothpaste, why taking the trouble?