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Low acceptance rates, commercial publishing, and the future of scholarly communication

John P. Conley
Vanderbilt University

Abstract

This letter calls attention a recent trend in economics publishing that seems to have slipped under the radar: large increases in submissions rates across a wide range of economics journals and steeply declining acceptance rates as a consequence. It is argued that this is bad for scholarly communication, bad for economics as a science, and imposes significant and wasteful costs on editors, referees, authors, and especially young people trying to establish themselves in the profession. It is further argued that the new "Big Deal" business model used by commercial publishers is primarily responsible for this situation. Finally it is argued that this represents a compelling reason to take advantage of new technologies to take control of certifying and distributing research away from commercial publishers and return it to scholarly community.

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Introduction

The purpose of academic journals is to facilitate scholarly communication, filter for errors, and maintain the record of scientific advance. The purpose of this letter is to argue that at least in economics, the current system of publication is doing an increasingly poor job at accomplishing this mission, and in turn, to propose possible solutions.

The Problem of Declining Acceptance Rates in Economics Journals

I share the view of Ted Bergstrom and many others that commercial publishers have largely outlived their usefulness to the research enterprise. It irritates me that scholars and research scientists write, edit, and referee papers, mostly for free, and then give them to commercial publishers (sometimes even paying submission and publication fees) only to buy them back at huge expense through their libraries. Clearly the pay-wall that publishers establish to protect their revenue stream limits access to knowledge and is contrary to the interests of both individual researchers and the research community as a whole. Moreover, besides the historical accident that many of the best journals happen to be owned by commercial publishers, there seems to be little that stands in the way of the academy taking back control of the quality certification process entirely.[1]

I had to acknowledge, however, that I was typically able to find a version of almost any paper I needed somewhere on-line. People, especially the more productive people, are fairly good at posting working papers. I usually know most of the individuals working in the areas in which I have interests. As a result, the name attached to a paper is a fairly good indicator of its likely relevance, in any event, at least as good an indicator as the fact of publication in any particular journal. Moreover, I seldom actually read journals any more. I research topics using Google Scholar, RePEc, SSRN, and so on. It is inconvenient to sign up with publishers to get tables of contents emailed to me or to login to my university's library web portal to search a journal issue by issue. I find it adds very little value over a more general search in any event. In short, certification remains important to help people gain tenure and promotion and to get a sense of the quality and centrality of individual scholars. However, neither certification by a journal, nor the collection of similar papers within the bound or even electronic pages of a specific journal has very much meaning to me when I am trying to understand where the debate in a subfield is at any given moment. As a result, I was beginning to come to the conclusion that while they are irritating, commercial publishers are "mostly harmless" to the research enterprise itself as publishing itself is becoming mostly irrelevant.

I attended a meeting of journal editors at the ASSA meeting in Chicago this year and at the end, found myself coming to exactly the opposite conclusion. I now think that commercial publishers and the business models they use are in fact very harmful to the research enterprise and it is important that the scholarly community takes steps to address the problem.

What changed my mind is that most of the editors at the table reported significant increases in submission rates (on the scale of 50 to 100 percent over the last two or three years). Their response was to sharply decrease acceptance rates (most editors reported that current acceptance rates were in the 5 to 15 percent range). As an example, at the *Journal of Public Economic Theory*, we have seen about a 50% increase in submissions over the last three years, and our acceptance rate has dropped from about 20% five years

1 See Conley and Wooders (2010) for more discussion of the technology and practicality of running journals without commercial publishers.

ago to close to 10% today. At the *Economics Bulletin*, submissions are running 30% ahead of last year's although the acceptance rate has remained fairly constant at 25% to 30%[2]. While it is difficult to document this trend in a formal statistical way, more will be said on this below.

What is driving this increase in submission rates? The profession as a whole does not seem to be getting appreciably larger. For example, in Conley, Crucini, Diskill, and Onder (2011), we document that the number of new Ph.D's produced each year by North American universities has remained fairly constant since 1986 at about 1000 per year. There have been increases from Europe and Asia, but in general graduates from these universities do not end up getting jobs in the top 100 research universities[3]. The study above also documents that research output in economics is highly skewed. We find that, on average, graduates of a top 30 US department publish more than three times as many quality-normed papers as graduates of non-top 30 US departments, that the top 1% of scholars publishes 14% of all quality-normed papers, and that the top 20% publishes about 80%. Thus, while there probably has been a slow and steady increase in Ph.D's from non-North American universities and increasing pressure from scholars in teaching and less highly ranked departments to publish more research, the increase are unlikely to be on the scale of 30% over three or four years. As such, we cannot explain the rise in submission rates as resulting from a similar rise in the amount of research being produced.

It turns out, however, that increased submission rates can occur even without much of an increase in research production. A simple way to see how this is possible is to note that there is a strong similarity between the publishing dynamics and the Federal Reserve System. Think of a newly written paper as being like a newly printed dollar bill. One is submitted to a journal and the other is deposited at a bank. The paper is accepted with some probability a. If it is not published it becomes a submission at a second journal (making $(1-a)$ new submissions in expectation). The bank is required to retain r percent of the new dollar just deposited but then is allowed to lend the rest. Thus, $(1-r)$ new dollars become a deposit in a second bank. In other words, new papers are like new dollar bills, the equivalent of M1. The acceptance rate is just like the fractional reserve requirement. The result is that a unit of M1 can generate many units of demand deposits, M2. In the same way, one paper can generate many submissions if acceptance rates are low.

At least in this simple version of the story, what we notice is that anything can be a steady-state equilibrium, and in particular, low acceptance rates are sustainable without any increase in the number of papers produced each year. Suppose that W manuscripts are written each year. If all journals decide to have a 5% as opposed to a 25% acceptance rate, it does not change the number of papers that are published in the steady-state. Eventually every manuscript written finds a home (at least with a probability approaching 1) and an average of W are published each year. The only change is the number of times that each paper ends up being a new submission at a different journal.

I will develop a somewhat more realistic model below, but first, consider the costs of being in the low acceptance rate equilibrium.

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2 This is because EB is a purely on-line, non-commercial, open-access journal. As such, we do not have a page budget forced on us by a publisher and so are free to choose our own acceptance rate. The Economics Bulletin can be found at this link www.economicsbulletin.com

3 For example, data used in Conley et al. (2011) taken largely from Hasselback (2003) shows that there were a total of 48 graduates of non-North American universities on faculty at all North-American economics departments in 2003 out of a total professoriate of about 7200.

The most obvious problem in the low acceptance rate/high turnover equilibrium is that it is simply antithetical to goal of advancing the scholarly conversation. If one accepts that refereed journals play any positive role at all in communicating new scientific ideas, then forcing the average paper to go through ten referee processes before publication both limits and delays access to new research. Hiding new work in a series of long[4] editorial reviews hurts both individual scholars (especially young scholars trying to get tenure) and the research enterprise generally.

The process of repeatedly reviewing papers at different journals also increases the quantity of editorial and referee effort needed to deal with any given manuscript. A paper might have to go through ten peer-review processes instead of two or three before publication. The increasing submission numbers at journals places severe strains on editors and also on the networks of friends and colleagues that they use for refereeing. Unless there are meaningful improvements to the significance or quality of the research produced as a result of each of these rounds of revision, this is wasted effort, and effort that participants in the peer-review process could have spent writing new research of their own instead of rejecting papers. Low acceptance rates seem to be a welfare dominated state.

To me, however, the most troubling implication of the low acceptance rate equilibrium is that it requires editors to try to distinguish the top 5% or 10% of submissions from the next 5% or 10%. I may be unusually modest, but I, personally, don't think that I am capable do doing this with very much accuracy. The wide variation in the number of citations received by published papers (even those published top journals) suggests that it may be that editors in general have much to be modest about. With luck and help from good referees I can generally tell when a paper is wrong, poorly executed, or extremely derivative. I have opinions about which topics are most interesting, but I doubt that all my views are universally held. My experience from JPET and EB is that 20% to 30% of what is submitted is credible and probably should be published. The next 20% or so is more boring in my personal view, but correct, and perhaps interesting to groups of researchers with whom I might be less familiar. The bottom 50% should be rejected with good reason. Picking 10% or 5% puts editors in the position of either attempting to make Delphic predictions of which of the 20% to 30% of acceptable papers will end up being more important, or simply expressing his or her own biases about topics or people. This seems to me to give editors far too much power. Rather than simply being gatekeepers to prevent false, plagiarized or trivial results from appearing in the scholarly record, editors can both push favored topics and individuals while closing off other topics or limiting debate. Now I firmly believe that editors generally do the best they can and try to be as even-handed as possible, but forcing editors to choose which 7% of submissions to publish places them in a almost impossible position. Even editors who struggle to be objective will succumb to some degree to confirmation bias.

Finally consider the combined effect of the doubling of publication lags documented by Ellison (2001) and the lower acceptance rates discussed in his note on the evaluation of junior faculty. Just from a mathematical standpoint, establishing a tenurable CV in six years with two year editorial lags and 10% acceptance rates is tremendously harder than it was twenty years ago with nine month editorial lags and 20% acceptance rates. Conley, et al. (2011) explore this further and document the phenomenon empirically. We find, for example, that graduates of the top 30 Ph.D. programs from the 1986 cohort were about 65% more productive than those from the 2000 cohort. If institutions do not internalize the effect of the new publishing environment, then fewer junior faculty will receive tenure than in the past. At an individual level, the cost of not gaining tenure is obviously significant. However, the costs are also

4 See Ellison (2001) for documentation of the increasing length of the referee process.

significant to the profession at large. Failure to promote qualified scholars leads to more frequent, costly searches by departments for new faculty and the discouragement and exit of qualified scholars who would otherwise enrich the stock of economic research.

A Model

While the similarity between monetary economics and submission dynamics is instructive, it ignores some key differences. In the case of money, the fractional reserve requirement is an exogenous policy choice of the Federal Reserve bank. In the case of submission dynamics, the acceptance rate is endogenously determined as a function of the submission rate and the exogenously set page budget allotted to a journal. In addition, the only way that dollar bills "leave" the system is to be held as reserves. In contrast, manuscripts have two ways of leaving the submission system: being published or having the author finally give up on having a paper published after too many failed attempts. Eventually the author becomes discouraged as the results becomes more dated and the paper ultimately "dies" without being published. Incorporating these differences pins down the steady state acceptance rates as a function of the ratio of newly written papers to publication slots (implied by page budgets) and the death rate of papers. Although it is no longer the case that any acceptance rate could be a steady state equilibrium, it turns out that the qualitative story is the same.

Notation:

W – papers written every period
P – papers published every period
U – stock of unpublished papers at the beginning of each period.
a – acceptance rate of papers submitted in a period
d – death rate at which unaccepted papers fail to appear in the next period.

To find the steady state, note that the acceptance rate must adjust so that the number of papers accepted equals the number of papers published. Thus:

$$aU = P \quad \rightarrow \quad U = \frac{P}{a}$$

In addition to this, at a steady state, the number of new papers written must equal the number of papers taken out of the unpublished stock either though publication or death. Thus:

$$W = Ua + U(1-a)d$$

A little algebra shows that:

$$a = \frac{Pd}{W - P - Pd}$$

To get a sense of what this means suppose that 100 papers are published each year. The columns indicate the number of papers written each year while the rows indicate different death rates.

Death rate (d)	Paper writen per year (W)			
	125	**150**	**200**	**300**
.05	.167	.091	.048	.025
.1	.29	.167	.091	.048
.2	.44	.29	.167	.091
.3	.55	.375	.23	.13

***Table 1**: Implied acceptance rates with different numbers of newly written paper and death rates.*

What this shows is that even if the ratio of papers written to papers published is only two to one, the ratio of submissions to papers published can be four, ten or even twenty to one, depending upon the death rate.

Given this, what might be reasonable numbers for papers written and published? It is difficult or impossible to get a firm figure on the number of papers written overall, however ReREc added approximately 150,000 papers to its database in 2011. Of course, RePEc is neither a comprehensive record of all written papers nor can we be sure that there are no redundancies in these uploads, but at least this gives some notion of scale. JEL reports a total of 19,722 papers published in 715 journals published in 2006 and 4,796 published in the top 80 economics journals in 2011. The conclusion is that a ratio of papers written to publication slots on the order of three or five to one are not unreasonable and may even be on the low side.

What might be a reasonable death rate? This is also hard to measure empirically. However, even though revising rejected papers has some opportunity costs, it is lower than writing new papers. Ironically, one of the tools that editors are now using to address this new flood of submissions may make the problem even worse. The significant fraction of the editors at the meeting reported that they have started to make heavy use of bench rejections. The numbers seem to be about 20% to 40% and this is mostly something started in the last two or three years. While this reduces the strain of referees, it increases the strain on editors. As an aside, one has to wonder if this is good for science in general since it creates an additional barrier to publication based on a quick and possibly biased look by a single person, but that's as may be. More to the point is that rejecting papers quickly increases their velocity. Bench rejected papers are not as dated, don't require as much revision (since a meaningful review is not provided) and are more likely to incense the author than to discourage him. Bench rejections makes it possible for one paper to be a submission to six journals in one year instead of only one or two. Thus, bench rejections are likely to lower the death rate and thus create the negative externality of additional (presumably, less desirable) submissions at other journals. Given this, death rates of .2 or below do not seem unreasonable. We conclude that even with this more sophisticated model, acceptance rates on the order of 10% are quite supportable as a steady state and may even be optimistic.

Discussion

The obvious question is why haven't publication slots increased at a rate sufficient to keep up with increasing rates of submission? If a 20% acceptance rate seemed correct or optimal for leading field journals or even general journals ten or twenty years ago, it is hard to understand why rates of 5% to 10% would suddenly be optimal today. I argue that most of the blame can be placed squarely at the feet of commercial publishers, and to a lesser extent, society publishers. This extends from two causes. First are the habits that remain from the papyrocentric era of scholarly communication. When the only means to disseminate new research was to send people bound collections of printed pages by mail (something called a "journal") the marginal cost of publishing another manuscript was high. Each additional paper would have to be physically reproduced hundreds or thousands of times and in addition to being physically distributed. There was also the cost of typesetting which used to require highly skilled labor and involved setting up complex equations and diagrams by hand in physical type. This resulted in publishers quite reasonably giving page budgets to journal editors for reasons of economic necessity. Publishers were willing, often eager, to increase these page budgets year to year since the subscription price of individual journals could be increased to reflect the additional content.

The new business model is quite different. Paper journals have essentially outlived their usefulness. While a large number of journals still have paper versions, it is doubtful that these continue to contribute much to the dissemination of knowledge. Many new journals, especially in fields other than economics, are completely on-line and this does not seem to have harmed their impact (see The Public Library of Science journals, or the "express" publications of the Optical Society of America for example). To the extent that costs of producing paper versions induces commercial publishers to limit page budget, they are culpable for not fully embracing available new technologies. In addition typesetting is now wholly electronic and usually just modifies a file provided by the author. Thus, while not costless, typesetting is a much less significant expense than it used to be. So why would publishers not be willing to increase page budgets when the costs of doing so are so reduced? The answer is that most journals are sold under "big deal" contracts in which publishers in effect give libraries all or nothing offers to subscribe to their whole catalog in a given field rather than allowing librarians or consortia to chose their subscriptions journal by journal. See the work of Ted Bergstrom and Preston McAfee on this for more details. What the exact incentives of the publishers are here is not entirely clear. They seem to care weakly about the over-all quality of the bundle, but they get little or no extra revenue from increasing page counts. Thus, the decision to starve journals for pages in light of strongly increasing submission rates is driven by the commercial interests of the publisher and is entirely contrary to the mission of fostering scholarly communication. This is a new, and I think compelling, reason to try to reclaim scholarly communication back from commercial publishers and into the community of scholars, and is the main point I would like to be taken from this letter.

Society publishers are somewhat better than commercial publishers, but are not entirely innocent. Like most journals, many society journals continue to be published in paper and this makes it somewhat more costly to increase page counts. More to the point, many journal editors have taken increased submission rates as an opportunity to increase the apparent quality of their journals. Surely a journal with an acceptance rate of 5% is higher quality and more prestigious than one with a rate of 20%. Prestige is probable, quality is possible, but I argue this comes directly at the expense of the scholarly mission. Take the case of a field journal, for example. Its purpose should be to promote the field, facility communication and build a community of scholars working is the area. Quickly publishing all papers that make a reasonable contribution to the field is the best way to do this. What purpose is served by taking only 5% of papers? It is hard to believe that 95% of the papers written in an area are uninteresting, incorrect or

unoriginal. It certainly is not the case in theoretical public economics. The only point to this artificial scarcity of publication slots is to build the reputation of the journal. Thus, the question must be asked, which is more important: the journal's or editor's interest in prestige or serving the community of scholars to whom the journal is addressed? It is not enough to say that rejected papers can always find a home elsewhere. Even if editors and referees were correct in choosing the top 5% of submissions, the cost of maintaining this low acceptance rate is delay in getting good papers (the top 10, 20 or 30 percent) published, increased burdens placed on editors and referees of other journals, and a more chancy and difficult tenure process for young economists in the area.

Conclusion

The point of this note is to call attention to the recent trend in economics publishing: increasing submission rates, lower acceptance rates, and increasing use of bench rejections. I would like to suggest the following:

- That this new low acceptance rate equilibrium be recognized as something that is undesirable to the economics research enterprise. It hinders the communication of new results, wastes editors', referees' and authors' time, and places special burdens on new Ph.D's trying to earn their way into the profession.

- That it also be recognized that journals owned by commercial publishers have financial incentives to keep page budgets where they are and that this provides a strong additional reason for the profession to retake control of the publication and certification process.

- That the solution is to restructure journals in economics to take full advantage of the new technological and commercial realities. We simply do not need commercial publishers to make journals economically viable, and since we no longer need paper, we can choose acceptance rates with an eye to maximizing the scholarly mission (which I argue should be between 20% and 30%) rather than having then imposed on us by commercial publishers whose interests are very different from our own. There are several ways to go about this. First is to start new entirely electronic, non-commercial journals. *Economics Bulletin*, and *Theoretical Economics* are examples, We should also note the *Berkeley Electronic Press Journals* which take advantage of the technology, but in a more commercial way. Second, if societies do not feel comfortable expanding page counts of their existing journals, they should create new on-line journals linked to the existing ones. These new journals could serve the need for scholarly communication while the old journals would be places where papers that the editors thought were the best would receive additional honor and attention. We should credit the *American Economic Association* and the *Econometric Society* for having already done just this with their new four new field journals and *Theoretical Economics*, respectively. I would encourage other societies to do the the same. Third, editors of commercial journals can and should follow the example of AEA and ES and set up new electronic journals on their own if they find their page counts force them to reject too many submissions. The commercial side could publish 5% or 10% of submissions, while the non-commercial, hopefully open-access, sister journal would use the same editors and referees and publish the remaining 10% or 20% of worthwhile submissions. This would enrich the field, serve the interests of both scientific advance and individual authors, and could be accomplished with very little additional burden on editors and referees.

The *Economics Bulletin* has been publishing research in economics for eleven years now without any commercial help at all. The platform that EB runs on (AccessEcon.com) is ready and willing to support other editors, societies or groups of researchers break free of commercial publishers without charge. Editorial Express provides similar back-end services to edit and publish journals at very reasonable cost. In short, we have both the tools and compelling reasons reestablish control over our intellectual products. If we are serious about our scientific mission as scholars, it shameful if we fail to do so.

Appendix

Hard data about acceptance rates is hard to come by. Not only are they are to calculate, but many editors and especially their publishers are reluctant to reveal them. My own private conversation with many editors seem to confirm the trend to lower acceptance rates. Daniel S. Hamermesh of the University of Texas at Austin has also been interested in this topic. The table below represents data from a survey he conducted and published in "How to Publish in a Top Journal (I wish that I knew!) " and is used with his kind permission. The complete paper may be found here:
*https://webspace.utexas.edu/hamermes/www/**HowtoPublish**.pdf*

Acceptance Rates at Various Journals Year 2008 or Shortly Before

Journal	Acceptance Rate
*American Economic Review**	0.07
*Econometrica**	0.09
Journal of Political Economy	0.05
Quarterly Journal of Economics	0.04
BEJ Applied Economics (All 4 Levels)*	0.51
Canadian Journal of Economics	0.18
Economica	0.11
Economics Letters	0.17
European Economic Review	0.09
Industrial and Labor Relations Review	0.18
Journal of Human Resources	0.10
Journal of Labor Economics	0.08
Journal of Monetary Economics	0.20
Journal of Population Economics	0.21
Journal of Public Economics	0.10
*Labour Economics**	0.15
RAND Journal of Economics	0.11
Review of Economics and Statistics	0.12
American Political Science Review	0.08
American Sociological Review	0.08

*Based on email exchanges with Editors, except where *, which is from a report printed in the journal or displayed on its website.

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