

Introduction

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When I was a PhD student at Stanford University, I had a unique experience that gave me some insight into the demand of the economics profession; at least its academic side. By “demand,” I am talking about what academic economists care passionately about; namely, the publication process. What universally captured the attention of some of the profession’s most noted contributors were not new ideas on unemployment, inflation, poverty, environmental issues or how close price is to short-run marginal cost. Instead, it was whether our process of identifying the most valuable of these ideas actually worked. It is this issue that appears to capture the attention of academic economists at all levels. As this volume is a collection of papers of economists looking introspectively at the publication process in the profession I thought it would be useful to recount that experience.

It began when George Shepherd, also a Stanford graduate student, and I surveyed leading economists, asking them to reflect on difficulties they faced in publishing their most influential pieces. The idea for this survey came to us during a lecture by Professor Ken Arrow in his History of Economic Thought class. He had mused about the difficulties his teacher, Harold Hotelling, had in publishing his famous paper on exhaustible resources – one of the first papers to formalise an economic problem using the tools of dynamic optimisation. Keynes at the *Economic Journal* had initially rejected that paper. This story caused us to wonder whether this experience of rejection accompanied other influential works.¹

As it turned out no inquiry into rejection had yet been undertaken and, furthermore, there were plenty of folk legends of such rejection to wet our appetite. So George and I crafted a letter and sent it to 130 leading economists inviting them to tell us any tales of rejection.

The response was immediate and overwhelming. We had clearly struck a nerve. Almost 80 percent replied, many with several pages of tales and their own thoughts about the efficiency of the publication process. It was surprising to have discovered an issue that ignited such fervor. Respondents vividly recounted how their initial work was rejected time and time again.

They recalled precise referee and editor comments. They admitted the misgivings this gave them about their career. And they expressed their concern that little had changed today and how their students faced similar difficulties.

The result of this survey is included as Chapter 3 of this volume.² That article, publishing by the *Journal of Economic Perspectives*,³ reviewed the response to the survey and revealed just how many famous economic papers were initially rejected by some of the leading journals of their time. Hotelling's story was one of many; indeed, the sheer number of important results that faced initial negative reviews was a surprise to many.

The article itself had an intense response after it went to press. It spurred debate as to whether there was a flaw in the way the profession identified important work.⁴ Was it part of a conservatism in economics or a natural consequence of the early novelty of such research? Was it something to do with economics and the application of the scientific method or the difficulties associated with separating values inherent in policy goals from pure economic analysis? Did this happen in other disciplines?⁵ More than one journal editor indicated to me that the article gave them pause to think carefully about negative reviews and their decision to reject.

The article also influenced how researchers considered their own publication experience. On one level, by highlighting the difficulties of the profession's finest, it comforted many people that their own experience of rejection (and it was very rare to find someone without many) was not unusual. On another level, the article gave some people the impression that their work may have been rejected unreasonably and that it was the system and not the substance of their work that was at fault. Thus, there is a tension between empathy and complacency in the attitudes readers took from the stories of rejection. The former is a positive effect but the latter did concern me somewhat.⁶

Nonetheless, the experience of writing the "rejections" article and observing the reaction to it signaled several things to me. First, economists were extremely interested in the practices of their profession. Second, they applied economic logic to those practices. (It was easy to couch the accept/reject decision in terms of the probabilities of Type I and Type II errors.) And, finally, by looking at their own practices, it was possible to gain some insight about the way economic processes worked. In the case of rejection this involved determining the value and hence, demand for ideas in a world of uncertainty. All of these suggested to me that there was some benefit to agglomerating some of the interesting research that economists have embarked on about the publication process in the profession. Whence, this volume.

SUCCESS AND REJECTION IN ECONOMICS

The papers selected for this volume all involve economists applying economic analysis to uncover truths about the publication process. Most of the papers are empirical but there are some theoretical contributions as well. Some are whimsical while others involve a more serious analytical exercise. All have in common that they were published in some of the discipline's leading journals thereby indicating the general interest of such lines of research.

The volume begins with the less serious of these contributions; the "rejections" paper (Chapter 3) included among them. Chapter 1 reprints Axel Leijonhufvud's classic "Life Among the Econ." This article captured the attention of its generation of economists and, in so many ways, its sentiment still rings true today. Leijonhufvud reviews the economics profession as if it were an isolated tribe (the "Econ") in a less technological sophisticated age. The members of the "Econ" are motivated purely by peer acceptance and achieve this by carving ever more elaborate "modls." Upon reading this chapter, one is challenged to consider what value economic modeling has to society and also how the profession values it. How do we determine whether one piece of economic theory is valuable or not; especially when the distance from basic contribution to real world policy analysis can be so far?

The next paper (Chapter 2) offers friendly advice for new lecturers and assistant professors in economics. Daniel Hamermesh runs the gauntlet of taking a PhD dissertation to the journals, amalgamating the wisdom of those that have traveled that path and in the process cautioning young economists against game playing and warning them about the rocky road ahead. The "Guide to Etiquette" has, in many ways, become the graduation gift of choice for new academics.

Turning to more serious academic exercises, in Chapter 4, Sharon Oster and Daniel Hamermesh conduct an empirical analysis of the determinants of research productivity by academic economists (as measured by the quality of the journal they publish in).⁷ They find that research productivity declines with age but that early success does breed later relative success. Moreover, this decline is not because of any difference in rejection rates for older researchers suggesting that the decline is more on the supply-side than any demand-side bias. Like many of the chapters in this volume, when looking for bias and discrimination in the publication process, scant evidence is forthcoming.

REFEREES AND EDITORS

The next set of chapters of this volume deal explicitly with the review process itself; that is, how peer review actually works and what behaviour characterizes editorial decision making. Chapter 5 is a useful starting point in this exercise. There, Daniel Hamermesh again takes a close examination of who referees and what they do. He finds that, in general, referees are more established than those they are refereeing. Moreover, there appears to be no bias in the assignment of “good” referees; that is, such referees are assigned regardless of factors such as age or inexperience. Hamermesh also examines the time lags associated with refereeing. In this regard, there appears to be evidence of “types”; that is, referees either are diligent or they are not. In addition, monetary rewards rarely assist in speeding up the process (an issue we return to in Chapter 9).

Reinforcing Hamermesh’s work is an earlier study by David Laband (Chapter 6). That study, using a survey of editors at several journals, examined whether the referee process was merely about screening the quality of papers or whether the process itself assisted in generating higher quality research. Laband found that screening was not the only factor and that referees did “add value” to papers. Editors, in contrast, did not play this role and assisted the process by matching papers to appropriate referees.

In Chapter 7, Rebecca Blank also informed an old debate about refereeing; namely, does author secrecy alter their chances of rejection? Double blind refereeing is argued to minimize bias from elitism and other forms of discrimination. Against this are arguments that referees can utilize identity to assist in reviewing (that is, expressing confidence about complex mathematical proofs) and besides, they can identify authors anyway. Blank utilized a controlled experiment conducted in the late 1980s at the *American Economic Review*. She found that acceptance rates were lower and reviews more critical when the author’s identity was unknown. There appeared little bias in acceptance rates but papers from near-top institutions or non-academic institutions were more likely to be accepted when reviewing was double blind. Finally, for double blind refereeing, approximately 55 percent of reviewers claimed to be able to identify the author concerned.

Like Hamermesh and Blank, David Laband and Michael Piette (Chapter 8) also examine potential bias in the review process. They are concerned with common accusations that certain journals associated with certain schools and editors in general treated their own students more favourably than others in accepting papers. They utilize citations as an indicator of paper quality and assess whether accepted papers by economists identified closely with editors are of lower quality than other papers. While there is evidence that these editors accept some poor quality papers, it appears that editor connections (controlling for other determinants of paper quality)

improve rather than detract from average paper quality. Laband and Piette conclude that any favouritism that exists may assist in attracting better quality papers with personal connections biasing an economist's decision regarding where to submit their paper. This could be argued to reduce potential transaction costs in the market for economic ideas.

In contrast to the above studies, Maxim Engers and I use economic theory to examine of the question of monetary rewards for referees (Chapter 9). Referees are paid little if at all. With the concerns about reviewing times and referee quality, this was somewhat puzzling. After all, would not journal quality be improved if some money could be spent improving the refereeing process? Against this argument were claims that refereeing activity was conducted out of non-monetary motives such as "service to the profession" and a personal concern for journal quality. However, this did not explain why referees might not be motivated by monetary reward as well. Our theoretical examination took both arguments seriously and found that it was the interaction of the two that explained the almost inelastic supply response of referees to monetary incentives; as alluded to by Hamermesh's (Chapter 5) study. The reason was that while monetary rewards might improve direct incentives for timely review; they do it for all referees. Hence, a referee also concerned about the overall efficiency of the process can rely on the existence of the high monetary reward to participate less often. In equilibrium, the cost of encouraging referees is simply too high given the small improvement in refereeing speed.

The examination of peer review and editorial selection processes is fundamental to the way in which economics ideas are disseminated. These studies indicate the usefulness of economic methods in analysing these but they also suggest the potential fruitfulness of further research using the potential wealth of data in this area.

TO CO-AUTHOR OR NOT TO CO-AUTHOR

Getting papers published is one aspect of the academic reward structure in economics. Another important aspect is the choice of whether to collaborate in your research. Choosing to go it alone means you reap all of the potential glory while collaborating means dividing it up. Of course, collaboration may improve research quality, and hence the chances of publication.

Chapter 10 deals with the division of the rents from co-authored publications. Raymond Sauer looks at how published papers feed into academic salaries. Not surprisingly, he finds that paper quality is important while co-authored papers are "worth" less in monetary terms. Indeed, the monetary value of a paper of given quality is simply divided up among co-authors. To the extent that economists are motivated by monetary concerns

this suggests that co-authored papers are of higher quality than had the same research been embarked upon separately.

John Hudson (Chapter 11) reinforces the value of co-authorship by examining changes in the level of collaboration in economics. Since World War II, the percentage of multi-authored papers in top journals has gone from less than 10 to over 50 percent. Hudson demonstrates a consistent trend towards more multi-authored research; attributing it to the greater opportunities afforded by advances in transport and communications. Once again this suggests the value of collaboration in economic research.

Finally, Maxim Engers, Simon Grant, Stephen King and I (Chapter 12) – as if to prove the value of collaboration (!) – investigate the possible theoretical rationales behind the extensive use of the alphabetical ordering of names in multi-authored papers. In this volume, six of the eight multi-authored papers follow that norm. Economics stands in marked contrast to other disciplines in the social and natural sciences in this regard. The alphabetic norm means that co-authors elect to send no signal to the market regarding who contributed what to the paper. But this is in an environment where the both sides of the academic labour market would value such attribution. The ultimate reason postulated for the lack of a signal is the potential harm an alternative signal would have on co-authors with names “lower” in the alphabet; especially when the market places weight on the fact that an alphabetical ordering signals little. This harm is greater than the potential gain to the other co-author from providing such a signal, and hence they agree to follow the alphabetic convention.

Collaboration in research is a fundamental way in which higher quality research is generated. However, the issues raised in each of these studies indicate the tension between collaboration and attribution. The second feature is fundamental to individual motivations and peer acceptance in scientific fields. Once again this line of inquiry offers potential for so much more, utilizing the data that comes from already published work.

THE INFLUENCE OF ECONOMICS JOURNALS

The final part of the volume takes a selection of papers that try to measure the influence of economics journals. The papers selected are representative of that literature and are not exhaustive. However, all such papers (and associated rankings of economists) always attract wide interest and help establish leading general and field journals. As such, it is useful to include a selection here.

The three chapters take three distinct approaches to examining the influence of particular journals. The first study (Chapter 13) was initiated by the Nobel Laureate, George Stigler, and completed after his death. Using

the *Social Science Citation Index*, that study looks at patterns of citation among journals. Theoretical research tends to be cited by applied researchers much more than the other way around. Core general journals cite each other more than ones in specialized fields that experience more self-citation. Finally, economics has had an important influence on related fields (such as finance).

The second study by David Laband and John Wells (Chapter 14) looks at changes in the types of papers published in the three oldest US journals. This analysis gives some insight into changes in the nature of economic journal publications over the last century. The final study, also by Laband with Michael Piette (Chapter 15) is the most recent comprehensive ranking of journals using data from the *Social Science Citation Index*. It provides an analysis of which journals have had more citations from their papers using various measure of paper quality. While other studies exist, this is a fine example of such quality rankings that are today used in departmental decision making regarding tenure and promotions. For completeness, no volume like this could be without it.

ACKNOWLEDGEMENTS

My colleagues and collaborators including Maxim Engers, Simon Grant, Stephen King and George Shepherd have stimulated my own interest in how economists have come to analyse the publication process in their own profession. But also to be acknowledged are the many participants in coffee room and conference discussions, in particular Kenneth Arrow and Scott Stern, regarding the above issues. It is their interest that motivated this volume. Finally, I wish to thank Robin Carey for her diligent assistance in formatting this volume.

NOTES

1. Moreover, we were fortunate in that we only had to ask Professor Arrow himself whether such a survey had been undertaken to see whether we could fill the gap. After all, if it had been done he would surely have been one of the respondents. This is one case where a sample size of one was enough.
2. George Shepherd has taken our survey responses and put them into a book, *Rejected: Leading Economists Ponder the Publication Process*, Sun Lakes (AZ): Horton, 1995.
3. Somewhat ironically that article had one of the fastest acceptance times ever. George and I walked from class straight to Joe Stiglitz, who was then editor of

the *Journal of Economic Perspectives*, who offered to publish the idea. Furthermore, he would do this even if the article were written poorly; offering to rewrite the article if we had trouble delivering a coherent version!

4. At conferences and coffee rooms for many years since I have been party to discussions arising from the article. More times than not, these debates arose in my presence without the parties knowing that I had co-authored the article. (In the words of an old American Express commercial, “Everyone knew my work but nobody knew my name!”)
5. From discussions I have had it seems that the level of rejection in economics is an order of magnitude above other disciplines; in particular, natural sciences. However, I am not aware of any study identifying any systematic difference.
6. Of course, for me the article totally destroyed any possibility of sympathy from colleagues if I lament that a paper of mine was rejected. All I get is the response, “well, you should know all about that!”
7. In constructing this productivity measure, Oster and Hamermesh utilize Laband and Piette’s (Chapter 15) measures of journal quality and also Sauer’s (Chapter 10) finding the co-authored articles are given less weight in peer evaluation than single-authored articles.