An Examination of the Reliability of Prestigious Scholarly Journals: Evidence and Implications for Decision-Makers

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October 2006
To appear in Economica

Abstract

Scientific-funding bodies are increasingly under pressure to use journal rankings to measure research quality. Hiring and promotion committees routinely hear an equivalent argument: "this is important work because it is to be published in prestigious journal X". But how persuasive is such an argument? This paper examines data on citations to articles published 25 years ago. It finds that it is better to write the best article in an issue of a medium-quality journal such as the OBES than the worst-4 articles in an issue of an elite journal like the AER. Decision-makers need to understand this.

Acknowledgments: For their helpful comments, I thank Danny Blanchflower, Gordon D.A. Brown, David De Meza, Amanda Goodall, David Laband, Steve Nickell, Mark Stewart, Radu Vranceanu, Ian Walker, Ken Wallis, Michael Waterson, and an anonymous referee and editor. I would like to record that my thoughts on retrospective reward structures have been heavily influenced by the excellent suggestions of, and electronic conversations with, David N. Laband, and that Michael Waterson's ideas first suggested to me that the order of articles within a journal might be correlated with their eventual citations.
JEL codes: A11, O3
Keywords: Research Assessment Exercise; metrics; citations; refereeing.
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"The results … will be expressed as quality profiles of research in each department submitted to the 2008 Research Assessment Exercise. They will determine the annual distribution of more than £8 billion for research in UK higher education institutions over a six-year period." www.rae.ac.uk. *"The Government's firm presumption is that after … 2008 … the system for assessing research quality … will be mainly metrics-based."* www.hm-treasury.gov.uk/media/20E/EA/bud06_ch3_192

Introduction

The United Kingdom is currently a useful test-bed for a worldwide problem -- how to allocate resources to scientific research. Its forthcoming Research Assessment Exercise will determine how much money goes to each department in more than 100 UK universities. To do this, a panel of experts will assess the quality of every department in every university. A small selection of each department's scholarly articles and books is to be given by the appropriate panel a quality rating. These will run from 4* down to 1*, where 4* corresponds to the highest world-class standard, and 1* corresponds to a national standard of research excellence. On such assessments will turn 8 billions of taxpayers' pounds. It appears that nations like Italy and Australia are soon to follow the UK's example and introduce a form of state-run university assessment exercise.

Partly because of the size of the undertaking, there will be pressure, if only covertly, on members of these peer review panels to use journal labels (X is a 4* journal, Y a 2* journal, and so on) in a mechanical way to decide on the quality of articles. Rumours of this, and guesstimates of the key list of journals, are currently circulating. Similar forces are discernible in other places. Seglen (1997), for instance, notes the rising blanket use of journal prestige ratings as part of funding decisions in medical research. In the world of economics research, a distinguished research institute in Amsterdam publishes a list of starred journals, ranked into categories of quality, to emphasise to its researchers that papers in certain journals should be viewed as of quality 'A' while others are of quality 'B'.

It might seem natural that, despite UK government-backed formal disclaimers, the RAE expert panels should behave in this way. An obvious argument could go: these papers have already been anonymously refereed, so the quality of a journal paper will be accurately captured by the prestige of the journal in which it has been published.

Thanks to sources such as the ISI Web of Science database, journal standing can be judged fairly objectively, by, for example, 'impact factors'. Journal labels, and 'impact factors', are thus becoming a kind of measuring rod, and thereby taking on a life of their own. This is not because of governments per se. Similar forces can be seen elsewhere. In universities around the globe, including the leading private United States universities, hiring committees regularly listen to the argument: "this is important work because it is about to appear in prestigious journal X".

But how persuasive are such arguments? There appears to have been little research directed at that question. As in most areas of life, prestige ratings in academia have their uses, and it is unlikely that any scholar would argue that labels are meaningless. Yet this does not mean that journal names are genuinely a sufficient statistic for quality.

This paper is an attempt to explore the reliability of prestige labels. It might be viewed as closely related to papers such as Laband and Tollison (2003), and newspaper articles such as Monastersky (2005), which emphasize that where a modern scientist publishes appears to be in some danger of becoming more important than what he or she actually says. It is also potentially complementary to work such as Laband (1990), Oswald (1991), Laband and Piette (1994), Johnson (1997), Kalaitzidakis et al (1999), Frey (2003), Seglen (1997), Coupe (2003), and Starbuck (2003, 2005), and is a small contribution to the field of scientometrics (van Dalen and Henkens 2005, Sussmuth et al 2006). There is also a natural link to the work of information-science researchers such as Oppenheim (1995), who have shown that, in the U.K., the departmental rankings that come out of the Research Assessment Exercise are closely correlated to ones that would have emerged from a citations-based departmental ranking.

The paper collects data on the accumulated lifetime citations to papers published a quarter of a century ago. It uses these to construct a simple test. The data come from issues of six economics journals of varying levels of reputation. These data show the expected ranking. However, and more interestingly, they also reveal that the best article in a good-to-medium quality journal routinely goes on to have a much greater citations impact than the 'poor' articles published in issues of more famous journals.

This fact may not be known to all of the people who sit on funding councils, or perhaps even to many economists.

1. Data collection and analysis

Assume that after some decades the quality of a journal article is approximately known. Perhaps the most usual measure is that of impact as captured by the total citations the article has received (that is, the number of times the article has been quoted in later researchers' bibliographies).

There is a considerable line of work that uses citations to assess intellectual output and productivity, and it has long been known that professorial salaries are correlated with researchers' lifetime citations, and that these citation counts are a good predictor of Nobel and other prizes. Moreover, better universities are led by more highly-cited individuals. See, for example, Hamermesh et al (1982), Laband (1990), Garfield and Welljams-Dorof (1992), Toutkoushian (1994), Moore et al (1998), Van Raan (1998), Thursby (2000), Bayers (2005) and Goodall (2006). As is also well known, citations are a noisy signal of quality -- survey articles tend to garner citations more easily than regular papers, there may be some pro-US bias in citations, citation numbers are more open to manipulation than are publications figures, for some individuals self-citations can cause problems, and so on -- but a common view is that citations are the most persuasive single measure of scholarly productivity. If the impact factors of journals become distorted over time, of course, as may happen if citations attract greater publicity and editors opportunistically try to manipulate their journals' citations totals, then the signal-to-noise ratio of citations may decline in the future.

For this paper, a selection of economics journals was taken from the year 1981 (namely, a quarter of a century earlier, to allow a long lag for the 'true' quality of a journal paper to be revealed). The winter issue of the year was examined for the American Economic Review, Econometrica, the Journal of Public Economics, the Economic Journal, the Journal of Industrial Economics, and the Oxford Bulletin of Economics and Statistics.

The AER and Econometrica are routinely viewed as two of the most prestigious journals in economics; in rankings they often appear near or at number 1 and number 2 out of approximately 200 economics journals. The Journal of Public Economics and the Economic Journal are usually viewed as good journals -- routinely in the world's top-20. The Journal of Industrial Economics and the Oxford Bulletin of Economics and Statistics are typically put a little lower again in prestige. They often appear around number 40-50 in journal rankings, and sometimes very far below the position of the top journals in informally distributed 'rankings' for the UK's RAE.

At the time of writing, for example, the Web of Science total-citations rankings in the Economics category put the AER and Econometrica at #1 and #2, the EJ at #9, Journal of Public Economics at #16, Journal of Industrial Economics at #47, and Oxford Bulletin of Economics and Statistics at #51.

Data on total lifetime citations were collected on each article. The raw data are summarized in the appendix. Table 1 lays out a summary of the data. As is known, the skewness of citation numbers implies that the mean values lie far above the median values. A small group of papers accounts for the majority of citations.

The remarkable variation in the number of times these journals' approximately one hundred articles have been cited by other researchers is clear from the raw data. The single most-cited paper is the famous theoretical analysis of trade unions by Ian McDonald and Robert Solow. Published in the American Economic Review, this paper has garnered 401 cites to date. The next most influential article is the Hausman-Taylor econometric estimator paper published in Econometrica; it has been cited 355 times.

However, many of these papers attracted very small numbers of cites. For instance, over a quarter of a century, 15 of the articles have been cited either zero times or on only one occasion. Judged from the perspective of the time elapsed, it might be argued that these articles' contribution to intellectual output has been and probably will continue to be zero. In a sense, their publication might now be viewed as having been an error (with the benefit of hindsight, needless to say).

The mean lifetime cites across these six journals follow the broad pattern that might be expected. The prestige labels are, in a sense, correct: AER 68 cites; Econometrica 63 cites; JPubEcon 22; EJ 30; JIE 9; OBES 7. The top journals thus dominate. Similarly, median lifetime cites are: AER 23 cites; Econometrica 22 cites; JPubEcon 9; EJ 11; JIE 3; OBES 2.

The variation of true quality -- as measured by cites -- is strikingly large. Because of this high variance, the less highly-cited articles in the top journals are easily bettered by good articles in less prestigious outlets. For instance, the 4th most-cited article in the entire sample (199 cites) is that by Mansfield et al, which appeared in the Economic Journal, and not in one of the top-two journals. As another example, in the American Economic Review, which is perhaps the most famous journal in the discipline, in its winter issue in 1981 more than one third of the issue's articles had after a quarter of a century each been cited fewer than 20 times. The very best papers in the other lower quality journals had by then garnered far more mentions in others' bibliographies -- respectively 88 cites (Sandmo in the Journal of Public Economics), 199 cites (Mansfield et al in the EJ), 43 cites (Teece in the Journal of Industrial Economics), and 50 cites (Sen in the OBES).

Consider, as a benchmark, the median number of cites. In the two top journals here, it is approximately 22. A natural question is then: how many of the articles published in the other four journals turned out to exceed that level? These 'should', in principle, have appeared in the top journals. The answer is approximately 16% of the articles. In the Journal of Public Economics, 1 out of 6 does. In the EJ, 4 out of 15 do. In the Journal of Industrial Economics, 2 articles out of 17 do. In the OBES, 1 out of 11 does.

One way to make this point more vividly is to take the mean value of cites among the 4 least-cited articles in each of the six journals. As shown in Table 1, those totals are respectively 6 cites; 5 cites; 23 cites; 3 cites; 4 cites; and 1 cite. Compared to the best article published in the lesser journals, these are of the order of one-tenth as cited.

Ex post, therefore, labels cannot be relied upon to be free of significant error. It appears that the journal system often allocates high-quality papers into medium-

quality journals, and vice versa. The data of this paper are consistent with the theoretical argument of Starbuck (2005), who points out, using simple statistical parameterizations, that an error-ridden system would generate empirical patterns of the sort documented here.

Although the implication of these data is that labels work too imperfectly to be taken as a sufficient statistic for the quality of an article, this does not automatically mean that peer reviewers can ex ante improve upon the journal labels. Perhaps the label is the best that can be done without waiting for 25 years?

Nevertheless, simple evidence against such a view comes out of the raw data. There are signs that the journal editors had an idea which would be the best papers in that issue of their journal. In the way they assigned the order of publication, those editors turned out, ex post, to have what now, in 2006, looks like prior insight. This can be seen informally by looking at the raw data. If we regress total cites, y, on publication-order in the journal, x, (that is whether the paper was first, second, third…eighteenth), we get a more formal sense for the pattern. [Notes and Comments, it should perhaps be emphasized, were omitted from the data; the criterion was whether the papers had these words in their titles]. Summarizing as regression lines:

Econometrica Cites = 133.14 – 7.36Order
AER Cites = 119.43 – 5.41Order
EJ Cites = 66.68 – 4.57Order
JPubEcon Cites = 58.93 – 10.60Order
JIndEcon Cites = 13.15 – 0.44Order
OBES Cites = 19.42 – 2.05Order

Individually, the sample sizes here are too small to give well-determined results (the six results vary in statistical significance from approximately the 5% significance level to approximately the 30% level), but, as can be checked by pooling the data with journal-dummy intercepts, as a group they are more persuasive. Hudson (2006), which was not available at the time the first draft of the paper was written, finds equivalent results on the statistically significant role of the order of journal papers within an econometric equation explaining citations. What editors know, and exactly

how, seems worth exploring in future research, because of the importance of peer review in the allocation of research funding in western society. It is possible that it can be conveyed to the experts who sit on funding bodies.

Perhaps it is appropriate to record that the point of this paper is not to argue that journal-quality rankings should, in all instances, be eschewed. When large samples of data are used, as in the assessment of a university department over a long period such as a decade or two, the error in judging articles in a mechanical way by simply assigning to each of them a journal-quality weight may turn out to be fairly small.

A referee has asked the related question: how then, in a practical way, should RAE peer review actually operate? One approach might be to assume that the anticipated intellectual value, v, of an article can be approximated by

$v = w(y)c + [1-w(y)]\, e(j, r, i)$

in which w is a weight between zero and unity, y is the number of years since publication, w(.) is an increasing function, c is the flow of citations per-unit-of-time since publication of the article, e(…) is a function that describes the a priori expected long-run number of citations to an article, j is the known modal or mean level of citations to all articles in the particular journal, r is the ordinal ranking of the article within the particular journal issue in which it appeared, and i is some quality factor, which might be termed academic 'instinct', assigned by an independent assessor or assessors, such as the considered view of a peer review panel [In some disciplines, it might not be appropriate to rely at all on the within-journal ordinal ranking factor, r.]

Therefore, in the short-run, emphasis would be given to the modal or mean level of citations to the journal. The sufficient-statistic nature of a prestige label would (temporarily) be dominant in an assessment of the particular article's likely quality. But, after a number of years -- and in some scholarly disciplines only one or two years -- considerable weight would be given to the actual number of citations acquired by the particular article. The quality of the actual article and the quality of the journal would gradually de-couple. In the long-run, virtually all the weight would be put upon the total citations acquired by an article, and the identity of the journal -- the label itself -- would cease to be important. This approach has a Bayesian spirit, of course. The exact way in which the weighting function w(y) is decided would have

to be determined by, among other things, the probability distribution of refereeing mistakes.

2. Potential objections

Several issues deserve consideration.

One objection is that the data set used here is fairly small. However, examination of the Social Science Citations Index suggests that these characteristics are found repeatedly. The same kinds of patterns occur, for example, in the winter American Economic Review issues for the later years of 1982, 1983, 1984 and 1985. Looking at the 'worst' 4 articles in each issue, none of these articles reaches 10 citations after a quarter of a century. Moreover, if we work through the 1980s issues of the Oxford Bulletin of Economics and Statistics, we find that the top article in each year attracts a considerable but variable number of cites: 23 cites in 1982 (Siddarthan-Lall); 57 in 1983 (Caves et al); 24 in 1984 (Evans-Gulamani); 99 in 1985 (Nickell); 483 in 1986 (Granger); 54 in 1987 (Nickell); 79 in 1988 (Osborn et al); 48 in 1989 (Jackman et al). While it might be useful for other reasons to extend the sample size, the paper's broad conclusions seem unlikely to change.

Second, could this phenomenon have disappeared over the last quarter of a century, as journals potentially improved their ability to sort papers into different quality classes? One check is to examine the winter issues of the Oxford Bulletin of Economics and Statistics and American Economic Review just 5 years ago rather than 25 years ago. Doing so reproduces the principal conclusion. For example, the most-cited paper in the December 2001 issue of OBES (by Lee-Strazicich) has to date acquired 12 cites. The four least-cited papers in that issue of the AER sum to a total of 8 cites. They are Bianchi et al (0), Deck (2), Bennett-LaManna (3), and Banerjee-Eckard (3).

A third objection is that citations -- some will say -- should be weighted by the importance of the journal doing the citing. Being mentioned in the American Economic Review is perhaps better, in some sense, than being mentioned in other journals.

Opinions differ on the case for this. Some commentators argue that it is an undesirable form of double-counting. Another objection is that because of their prominence the most elite journals' articles tend to be over-cited relative to their true worth. Perhaps the most sensible view is that it is only in the short run that a citation in a top journal matters more -- because in the long run the issue is instead the stock of intellectual influence across the discipline as measured by acquired cites in the year the article ceases to be mentioned. For the purposes of the present paper, however, the key point seems to be that the broad ideas are not going to be altered by weighting the cites totals. The reason is that the papers in AER and Econometrica garnering very few cites are not -- it is straightforwardly checked -- getting them disproportionately in the top journals.

Fourth, no adjustment has been made for the number of authors on each paper (as Hudson 1996 shows, the proportion of single-authored economics papers steadily declines through time). Nevertheless, as the focus here is the impact of individual articles rather than the productivity of particular researchers or collaborations (as in Kalaitzidakis et al 1999, Laband and Tollison 2000), it seems reasonable not to weight according to author numbers.

Fifth, it could be argued that self-citations are best removed from the data sample, as is often done in rankings of top economists and university departments. On balance, however, it seems appropriate not to do so here. It does not alter the conclusions of the paper (because self-cites are insignificant for important articles' total cites), and, for some of these highly influential researchers, there seems a logical case for leaving in own-mentions to those authors' important earlier papers.

3. Conclusions

This paper is a simple one. It provides evidence that it is dangerous to believe that a publication in famous journal X is more important than one published in medium-quality journal Y. This does not mean that young scholars ought to ignore top journals, nor that research funders should. Nevertheless, the publication system routinely pushes high-quality papers into medium-quality journals, and vice versa. Unless funding bodies are aware of this fact, they may make bad choices about how

to allocate resources. It is likely that some senior scholars understand the general point made in this paper, but young researchers and funding agencies may not.

By definition, scholarly articles in better journals go on, on average, to be more highly cited. This is not at issue. Importantly for decision-makers, however, there is a highly imperfect match between the quality of the journal and the lifetime cites of the individual articles. Approximately 16% of articles in the four lesser journals studied here ended the period with more citations than the median cites of an article in the two elite journals, the AER or Econometrica. To make the point in a different way, if the criterion is intellectual impact measured by citations, in this sample it was better to publish the top article in an issue of the Oxford Bulletin of Economics and Statistics than to publish all four of the bottom-4 papers in an issue of the American Economic Review. If peer reviewers, and those who sit on funding panels, have expert insight that allows them to judge quality, then the results in this paper suggest that there is a case for them to do so. They should not rely simply on mechanical rules based on journal labels.

It might be objected that perhaps such reviewers have no extra information that would allow them to rank journal papers (beyond the prestige of the journal itself). This possibility deserves to be taken seriously and needs further study. Nevertheless, one counter argument is to look at the citation levels of the journal papers by order of where the paper appeared in the issue of the journal. The early-position papers, such as the Cooley-Leroy and Rosen papers in the 1981 AER, are more highly cited than articles lower down the order of appearance. This suggests that editors had some ability to forecast which would turn out, 25 years later, to be the best papers. The individuals on peer-review panels may be able to do the same.

Finally, although it must be left to future work, there are interesting dynamic effects to be considered. The fundamental difficulty here is that there can be a large discrepancy between the 'market' forecast of the stream of citations from an article and the actual time-pattern of realized citations. As this paper shows, a journal-prestige metric is noisy. Therefore funding agencies and university employers in promotion cases might, in principle, be able to improve the efficiency of the reward structure by developing some sort of ex post settling-up mechanism. Such a

mechanism would reward ex post accumulated citations on a paper rather than merely the ex ante mean citation rate of the publishing journal.

Whether the future will see these retrospective reward structures in scientific research is an open, but interesting, question.

Raw data on the total cites to each 1981 article (in the order they appeared in the journal issue)

American Economic Review
Cooley-Leroy 118
Rosen 123
Kohn 23
Howe-Roemer 8
McDonald-Solow 401
Hendershott 16
Spulber 19
Bresnahan 156
Azariadis 16
Jonung 23
Startz 3
Darity 3
Caves et al 147
Akerlof-Main 45
Walker 0
Mussa 70
Conybeare 0
Boland 53

Econometrica
Malinvaud 28
Hausman-Taylor 355
Mundlak-Yahav 1
Nickell 258
Geweke 40
Godfrey 21
Anderson 17
Bourguignon 11
Harris-Raviv 97
Edlefsen 21
Deaton-Muellbauer 32
Pollak-Wales 142
Balk 1
Helpman 7
King 23
Nakamura-Nakamura 80
Bell 2
Rob 1

Journal of Public Economics
Sandmo 88
Courant-Rubinfeld 9
Hey-Mavromaras 9
Weymark 5
Bennett 0
Berglas 20

Economic Journal
Harris-Purvis 12
Malcomson 44
Bingswanger 77
Dervis et al 7
Mansfield et al 199
Hughes-McCormick 54
Spencer 4
Von Ungernsternburg 15
Skott 0
Chiplin 6
Hughes et al 0
Shah-Desai 11
Masuda-Newman 3

Formby et al 20
Shea 0

Journal of Industrial Economics
Williams-Laumas 13
Lynn 2
Aaranovitch-Sawyer 3
Levine-Aaronovitch 7
Teece 43
Thompson 21
Dries 2
Feinberg 2
White 3
Smith 23
Likierman 0
Hirschey-Pappas 2
Highton-Webb 3
Lamm 15
Bartlett 6
Baye 3
Link-Long 7

Oxford Bulletin of Economics and Statistics
Sen 50
Banerjee 8
Boltho 0
Stromback 0
Winters 0
Mayhew-Rosewell 5
Lye-Silbertson 1
Metwally-Tamaschke 2
Tsegaye 0
Brundell et al 9
King 3

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Table 1

Data on the Accumulated Lifetime Citations to Articles Published a Quarter of a Century Ago in Six Economic Journals

Data on articles published in 1981

	American Economic Review	**Econometrica**	**Journal of Public Economics**	**Economic Journal**	**Journal of Industrial Economics**	**Oxford Bulletin of Economics and Statistics**
Mean cites per article in that issue	68	63	22	30	9	7
Median cites per article in that issue	23	22	9	11	3	2
Combined cites to the 4 least-cited articles in that issue	6	5	23	3	4	1
Cites to the single most-cited article in that issue	401	355	88	199	43	50

Notes: These are taken, for each journal, from the winter issue of the year 1981. The data measure the number of times over the ensuing 25 years that the articles were cited by others. The source is the Web of Science's Social Sciences Citations Index, in late-March 2006. The data include short papers, partly because some of them are highly cited, and partly because it was not possible to draw a dividing line between those and full papers, but exclude articles denoted Notes, Book Reviews and Comments (where it was possible to assign these categories unambiguously).