Fifty Not Out: Reflections on Fifty Years of Publishing in Economics Journals

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Fifty Not Out: Reflections on Fifty Years of Publishing in Economics Journals[1]

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Abstract

This note discusses some aspects of the authors' experience of publishing in economics journals over a fifty-year period. It includes personal reflections on the changing nature of a career in economics, changes over time in the incentives facing economists, and changes in the publishing process itself.

[1] This title is inspired by the paper by Peter Hart (an enthusiastic cricket follower), 'One Hundred Not Out', *University of Reading Discussion Papers in Economics*, Series A, no. 101 (1977). His paper celebrated the production of the first 100 papers in the series that he edited. They were known locally as 'red backs' to distinguish them from the 'green backs' edited by John Dunning. This was a significant milestone, given the size of departments in those days and the 'youth' of the Discussion Paper system in the UK.

[2] I am grateful to Bob Buckle, Peter Earl, and Norman Gemmell for discussions.

Fifty Not Out: Reflections on Fifty Years of Publishing in Economics Journals

'*The cure for boredom is curiosity; there is no cure for curiosity*'
(Dorothy Parker)

Not long ago I suddenly realised that this year (2022) marks fifty years (1973 to 2022) during which I have been publishing research papers in refereed economics journals. During this time there was only one year (1976) in which I did not publish a paper, because of delays between acceptance and appearance in print. In those early years, I still had a lot to learn about writing research papers (though one never really stops learning) and there were drafts of several papers which I foolishly never showed to anyone.[3] I suspect, though I have not attempted to test the suggestion, that continuous publication over such a long period is unusual. This has prompted me to reflect on my experience and to write the following notes suggesting possible factors involved. They also point to some of the changes that have taken place in universities and journal publishing over the years. For obvious reasons, they concentrate only on journal publishing, and therefore ignore books, book chapters, and editing activities, although these have also taken up much time and energy. It should be stressed that these are merely personal reflections, which skim quickly over a number of topics deserving more detailed consideration.

The first and most obvious factor regarding the length of time is that I was able to start as a full-time lecturer in 1972, aged 23, immediately after completing the BPhil in Oxford. And fifty years later in 2022 I remain a full-time academic, long after the normal retirement age.[4] When I started as a lecturer in England, it was not necessary to have a PhD, and the two-year BPhil was widely considered an ideal foundation, consisting of four exam papers and a substantial thesis.[5] These days a PhD is an absolute necessity, and most people are in their late 20s before completion. The ability to continue after

[3] The arithmetic mean over the period is a little over six papers per year. The Appendix below discusses their variability over time.
[4] However, I spent just over two years (from November 2001 to February 2004) as a Principal Advisor in the NZ Treasury, and six years (2011 to 2016) as a half-time Principal Advisor, also in the NZ Treasury.
[5] It would be wrong to describe the BPhil at that time as containing four 'courses', since there was no organised teaching whatsoever and no syllabus or reading list was ever provided. One simply selected, in addition to compulsory Micro and Macro papers, to sit two further papers: I chose 'Applied Econometrics' and 'Statistical Methods of Econometrics' because my first degree in Bristol was in Economics with Statistics. It was probably the confusion regarding the title of the Oxford degree, particularly in other countries, which led to the later name change when it became an MPhil.

‘normal’ retirement age has been helped by obtaining research grants, which have contributed to ‘buying out’ undergraduate teaching.[6]

Without doubt, another contributing feature of my career is that, except for a three-year spell as Head of Department in Durham, I have been fortunate to have had relatively light administrative duties, involving membership of just a few university-level committees. Extensive administrative duties can inevitably take up a great deal of time, and sap energy as well as making continuity in research – such a crucial ingredient – difficult to achieve. In addition, I have not had any serious health problems during the period. It can be said that research is habit forming, perhaps even addictive, and it is not easy for people to return to it after a gap.

I believe that an important contributing factor is the strength of the training I received at Bristol University (1967 to 1970), combined with the vast amount of independent reading in the Bristol library and the library of the Oxford Institute of Economics and Statistics, where I spent very many hours. It could almost be said that I lived in the library in Oxford.[7] This independent reading provided a strong foundation for further research. In my earlier years as an academic in the UK, narrow specialisation in research (or even in teaching) was neither expected nor encouraged. And the major figures in the profession appeared to cover a broad range of topics. This led to a degree of versatility and range of work which I have welcomed, even though (by US and more modern standards) it would now be frowned upon. Focus on a single research area is virtually compulsory. I feel fortunate to have escaped that imperative. For very large projects it is of course necessary to have a clear focus for some time, extending over several years, but it is good to be able to move on to other areas. I was able to pursue work in the history of economic analysis more or less alongside other smaller projects, until it was eventually crowded out completely.[8]

There are many aspects of doing research and writing papers that can be learned only from experience. But it is also true that good advice can go a long way.[9] I have often noticed that people

[6] Most of the credit for obtaining the grants should go to Norman Gemmell, in his role of Chair of Public Finance at Victoria University of Wellington.

[7] I lived in Holywell Manor, across the road from the Oxford Institute of Economics and Statistics. For my later work on the history of economic thought, my own research library was crucial, especially in view of the very low, indeed shameful, quality of the library in Melbourne. On my own library, see the note by Roger Clark, ‘The John Creedy Economics Collection: A Donation of Significance’, *History of Economics Review*, 60, pp. 101-104. I should also mention the wonderful library at Penn State University, where I spent many hours.

[8] I regard the historical work as among the most challenging work I’ve done, and it cannot be tackled half-heartedly. I sometimes say, ‘the best way to get lots of things done is to do one thing at a time’. But what I have in mind is that concentration is the essential ingredient. I do not work on something to which I cannot commit all my attention at the time, otherwise it is wasted time.

[9] Based on my experience of researching and supervising students, I have offered advice on starting and writing research papers in *Research Without Tears: From the First Ideas to Published Output*. Cheltenham: Edward Elgar Publishing (2008).

who, as post-graduates, lacked good supervision, and who did not have the advantage of strong mentors at an early stage, could be slow to 'get going' and build up much-needed momentum. In my case, there is a huge influence which has continued throughout my career: that is the tremendous inspiration provided by J.A.C. (Alan) Brown. I cannot overstate his influence, and my good fortune in the fact that I was able to benefit from his lectures in Bristol (as well as his influence on the degree structure), and then crucially when he became my supervisor in Oxford.[10] Also important is the lasting influence of Peter Hart and Denis O'Brien, who provided such valuable role models and tremendous help and support that is impossible to overstate.[11]

When I started my academic career, there were no research appraisal systems that are now ubiquitous, and which have had such a large impact on university research. In general there was relatively little pressure to publish: the modal number of publications in any year was probably zero. However, I started as a temporary lecturer, with a one-year contract and no hope of the position becoming permanent, so I needed quickly to establish a CV for job applications, at a time when academic positions in the UK were few and far between. So I felt under considerable pressure to publish, which I think had a lasting influence. Furthermore, when I arrived, a year later, at Reading University I felt very lucky to find myself in a strong department where one was left without any doubt – though nothing was actually said explicitly – that an active research profile was expected. This was combined with the much-appreciated encouragement and advice from Peter Hart, whose leadership had such a big influence on the department. His insistence, with clear guidance, on the production of multiple iterations of a paper, before it could possibly be considered of a sufficient standard to be included in the Discussion Paper series, provided excellent mentoring in how to structure papers. Then, in 1978, I became Professor of Economics at Durham University, a few weeks after my 29th birthday. Yet again, I was under pressure to perform, as I must surely have been regarded within the university as a risky appointment. Subsequent moves to the US (to Pennsylvania State University) and then to Melbourne University at the end of 1987 meant that there was no thought of 'resting on one's laurels', while at the same time – and of much more importance – they provided considerable stimulus and excellent research environments.

[10] I have written about Alan in 'J.A.C. Brown (1922-1984): an appreciation', *Melbourne University Department of Economics Research Papers*, no. 1027 (2008). Among his many valuable words of advice, I remember his comment along the lines that 'research is not a sprint', but involves continuous work, day after day: he was stressing the need to take breaks from work and to establish a decent balance.

[11] I have discussed Denis's contributions in 'D.P. O'Brien's contribution to the history of economic analysis', in *Historians of Economics and Economic Thought: The Construction of Disciplinary Memory* (ed. by S.G. Medema and W.J. Samuels), pp. 244-263. New York: Routledge (2001). See also my interview with Denis in 'D.P. O'Brien and the history of economic analysis', *History of Economics Review*, 30, pp. 1-24 (1999). On Peter Hart, see *Industrial Concentration and Economic Inequality: Essays in Honour of Peter Hart* (ed. By M.C. Casson and J. Creedy). Cheltenham: Edward Elgar Publishing (1993).

Fortunately, during my time as a temporary lecturer, I received sympathetic treatment from journal editors, which was fortunate as I clearly had a lot to learn.[12] This is important early in the career, as confidence – a crucial ingredient of research – is easily destroyed at that stage. And the turnaround in those days was on average much quicker than it is today. Dealing with journals, and anonymous and often abusive referees, requires a careful balancing act between an ability to learn from criticism, and confidence needed to keep trying. Peter Hart once commented, with some feeling, that 'to do research, you need to have a thick skin'. Any economist who wishes to publish in journals has to learn to deal with 'hatchet jobs', wilful misinterpretations, strong biases, and plain ignorance: a little sympathy goes a long way.[13]

Other than my work on the history of economic thought, which is no longer possible in the current grants culture, a lot of my research in the area of public economics has been stimulated by policy questions.[14] Here, there seems to be no limit to the range of questions which lead to serious theoretical and empirical research that is suitable for journals, as well as contributing to relevant policy analysis.[15] For some years my concern with policy-related work has involved something of a 'flow against the tide'. Research appraisal systems, and their ludicrous associated journal rankings, provide a strong disincentive for doing work on local issues, resulting in papers which will struggle to be published in 'overseas' journals.[16] Younger researchers, with a keen eye on promotion, are reluctant to get involved with such work, and do not even regard it as one component of a healthy research portfolio. Such work can also often involve research in which few others are actively engaged, and of course this means that it will attract relatively few citations, although it can nevertheless have an

[12] The exception was the Manchester School: the editor (E. Victor Morgan) rejected my submission almost by return of post without any referees' reports. Victor later became a colleague at Reading University. A curious feature of a number of journals at that time is that their editors did not themselves make regular appearances as authors.

[13] My own addition to Peter's valuable advice is to 'kick the filing cabinet and then get on with revisions'. In the days of 'snail mail', when post could accumulate during university closures, I have several times had 4 and 5 rejections in a day. There was a time around 2008-9 when I received so many rejections that I accumulated 17 papers simultaneously submitted to journals, all of which were eventually published. It would have been easy to lose confidence. It is sadly the case that the economics profession is more brutal than others: it would be of interest to try to trace the reasons for this.

[14] My serious research in the history of economic analysis began with an invitation from Denis O'Brien to write a chapter on Edgeworth. He was well aware of my wide reading which provided the foundation for the work. Discussion of the challenges and my experiences of publishing in this area could occupy a separate paper.

[15] Here, my connections with NIESR in London, the Melbourne Institute (particularly during the period when Peter Dawkins was Director) and the New Zealand Treasury have been very important in providing stimulus.

[16] My complaint is not with research appraisal itself, but with the metrics often used, involving arbitrary judgements of quality. Bob Buckle and I have examined research appraisal, particularly the effects of the New Zealand system, in a series of papers: see 'The Performance Based Research Fund in New Zealand: Taking Stock and Looking Forward', *New Zealand Economic Papers* (forthcoming) and references cited therein.

influence.[17] Examples include my work on the UK State Earnings Related Pension Scheme (SERPS), and subsequent modelling of the highly complex Australian compulsory superannuation scheme, along with analyses of the introduction of Goods and Services Tax systems in Australia and New Zealand. It may seem surprising that, while working on income distribution early in my career, there were very few people with similar interests: fashions in economics fluctuate greatly over time.[18] I have been fortunate to be able to respond to policy questions, without worrying about whether it is an optimal strategy to gain promotion.[19] I don't think it is healthy to worry about citations and other indicators.[20]

I have touched briefly above on an active research environment, which is one of the most important factors affecting the ability to carry out research and publish papers. I have been very fortunate to have spent my career in departments which have been free of the interminable squabbles that can easily destroy any semblance of a research-active environment. There is nothing like being in a collegial environment where colleagues are mutually supportive and productive. Such an environment also has an active seminar programme, a regular flow of visiting researchers, and sufficient support services. I have benefited from the presence of colleagues – working in very different areas – who command considerable respect and are model academics. And of course I am fortunate to have been able to carry out joint research with many colleagues.[21] I often say that a joint author needs to be both the most critical and the most sympathetic of readers. So far, I have worked with 86 joint authors. It is interesting how the *modus operandi* varies substantially among authors. These days it is much easier to carry out joint research with people located thousands of miles away, and even with authors one has never met. The increasing frequency of jointly authored papers in economics has many interesting facets and deserves separate consideration.[22]

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[17] These days, papers are judged by such metrics, rather than by actually reading them. Young researchers can no longer take comfort in the thought that people who work in the same area respect their work.

[18] Of course, one could not ignore the work of Tony Atkinson, and I soon came to know and admire the work of Tony Shorrocks and Frank Cowell, at LSE. Yet their focus was quite different from mine. On pensions, Richard Hemming initially introduced me to the topic after he arrived in Reading, yet it was possible to 'count on one hand' the people working on pensions in the UK at that time. The contrast with the US was of course strong, but US economists typically did not, and still do not, read non-US journals.

[19] One piece of advice from Peter Hart, which of course I followed, was not to write books before becoming a professor. Despite the importance I have given to books, I have passed the same advice to many others.

[20] I do not mean to dismiss them entirely: there are good reasons why the 'stars' of the profession have a huge number of citations. I simply argue that they should not be regarded as an objective or ultimate standard of quality.

[21] One joint author, with whom I've been extremely fortunate to work on many papers, has wisely pointed out that authors need to have complementary skills, rather than being perfect substitutes.

[22] In a different type of review of fifty years' experience, Bob Buckle and I looked back over fifty years of contributions to *New Zealand Economic Papers*, and discussed a range of factors involved in the trend towards more joint authorship: see Fifty years of New Zealand Economic Papers: 1966 to 2015. *New Zealand Economic Papers*, 50, pp. 234-260 (2016).

I was fortunate to spend my early career during a time when universities were indeed usually collegial places.[23] Although there was considerably less joint authorship in the early days, it was easy to find colleagues who would freely provide substantial help and feedback. And to be able to 'look up to' more experienced colleagues, and learn from their approach to work, is itself a great stimulus. I fear that much of the collegiality of universities has been destroyed in recent years.

An additional factor which cannot be ignored concerns teaching. I believe the optimal amount of teaching is *not* zero: I have seen too many research-only people with negligible research output. Good students, constantly asking questions and requiring challenges to keep their interest, provide a great stimulus. I have been fortunate in largely having reasonable teaching loads and good students. Indeed, it is a privilege to have had contact with so many bright and energetic young people who are eager to learn.[24] It can also be argued that university teaching is much more effective when carried out by people who are actively engaged in research in the area. I was able to benefit from the beneficial influence on teaching of the lecturer's own research, from my first university experience, thanks to the example set by Alan Brown in Bristol.[25]

While reflecting on fifty years of publishing research papers, it may be worth adding some brief comments about the substantial changes that have taken place in journal publishing over the years (though it is really a subject for full-length treatment). The most obvious change has been the transition to a digital environment in all stages. Even where 'hard copy' is eventually produced, most readers are likely to access papers online.[26] From an author's point of view, the advantage is that, since production is based on a digital file rather than using old-fashioned typesetting, papers contain far fewer 'typos' than formerly. This is particularly important where maths notation is involved. However, some journals can be far behind the times, still insisting on requirements that date from typesetting days. A particular frustration for some years was that many journals were slow to accept Latex files.

The digital revolution has also meant that we all do our own word processing, which has brought costs and benefits. While I might often complain about the time taken to produce decent-looking tables, and the frustrations with some word processors, on balance I find it is much better to have full control

[23] Furthermore, while working in the New Zealand Treasury from 2011 to 2016, I was fortunate to have a number of excellent young colleagues who – without the incentives facing academics – were eager to be involved in producing research papers.
[24] There are exceptions: for example, Durham had very good students but excessive teaching loads.
[25] Evidence of the influence of research on teaching is something that is sought by, for example, accreditation bodies.
[26] It is not merely a convenience. Many library copies were inaccessible for long periods while issues were waiting to be bound, or were being read by other people. Or, especially in the case of Melbourne library, the section containing recent journal issues was generally a 'complete mess'.

from the beginning. An associated feature of work since the late 1980s is the ability, via desktop and later laptop computers, to carry out empirical work much more quickly, and to work from home.[27] This is enhanced by ready access to digital data files. In the early years of reliance on mainframes, using punch cards, getting two computer 'runs' of a program in a day was often the norm, and involved journeys between the computer unit and the department.

Online submission of papers means at least that progress through the system can be traced, although it can be frustrating to see that referees' reports have been submitted, but the editor is taking many weeks or months to make a decision. Many journals seem to be competing to produce the most cumbersome, opaque and frustrating submission process. However, the online system makes it easier for publishers to keep an eye on editors. With the old system, papers would often sit on an editor's desk many weeks before being sent to referees.

These technical aspects are not in fact the most important ones when it comes to getting papers published in journals. There has been a huge increase in the number of authors: there are far more researchers as a result of the vast worldwide growth in the number and size of universities, and the 'publish or perish' environment generated by research appraisal systems. At the same time there has been a phenomenal increase in the number of journals. Yet I firmly believe that it is now much harder to get a paper accepted than it used to be, and the turnaround times are very much longer: it is not unusual to wait a year for the first response (despite a standard request for referees to respond within a stipulated short period). The use of journal ranks in research appraisal has also seriously damaged many journals which were previously well-respected, making them far less attractive to authors.

There is another important change that makes publishing harder these days, at least in economics, and I'm not sure I have a clear explanation for it. That change concerns the typical behaviour of referees, who continue to remain anonymous. Referees now eagerly insist on many trivial changes, which often involve much work but achieve extremely marginal improvements. A revised version is often met with yet more demands, despite all previous ones being satisfied. Hence, more iterations are involved. The time between first submission and acceptance can now often take about three years, which is very frustrating for a young 'tenure track' academic. And this can be made worse when the editor changes in the middle of the process.

My view remains that the refereeing process is important and typically leads to improvements, but too often these days it gets to the stage of diminishing or even negative returns. I have benefited from

[27] The vast changes over time in computing, and in commercial software, are obviously worth much more extensive discussion. Thanks to advice from Alan Brown, himself a pioneer in the application of computers to economic research, I wrote Fortran programs from the very beginning of my research.

many perceptive helpful suggestions and comments from referees over the years. Yet, it is sometimes the rude, dismissive and totally negative remarks of a 'hatchet job' that, annoying as they are, lead to substantial improvements, in a desire to prevent similar comments being made in future, and to clarify the aims, motivation, and indeed main contributions, of a paper. A colleague, on the point of retirement, commented with feeling that one thing he will be very pleased to escape is the frustration and time-wasting created by referees. I can only agree. I used to keep a file containing a collection of the worst referees' reports, but it became far too large. But if you assemble a small group of economists, it does not take long before they start to swap stories of their most ridiculous referees' reports, and treatment by editors.[28]

Based on my own experience, suppose I were to advise a young academic economist as follows:[29]

> 'Select research topics that really interest you, that you believe are worthwhile, and where you feel you can make a contribution to the literature, rather than simply chasing fashions. But read widely, take an interest in what your colleagues and others are doing, and remember that there are different ways to approach a subject, rather than one 'correct' way.'

I feel sure I would be met with a condescending smile, or perhaps a look of frustration, and the thought, if not necessarily the expression, that:

> 'It's ok for you to say that, but I have to work on subjects that will attract research grants, and I have to target only a small range of journals. And I don't have time to read work outside my speciality, or even attend seminars that are in different areas. Also I can't afford to ignore the research metrics imposed by people who will make judgements about the quality of my work, even though they will never read a word of what I write. Don't you know that academic freedom is a thing of the past?'

Of course, I would have to acknowledge their points. Indeed, if I were now at the beginning of my career, rather than nearing the end, I would most probably have to take a similar view. But I am thankful that, despite all the changes that have taken place in university life and publishing over the last fifty years, I have – by and large – been able to follow my own precepts, while having the opportunity to travel widely and get to meet many remarkable people, while continuing to publish.

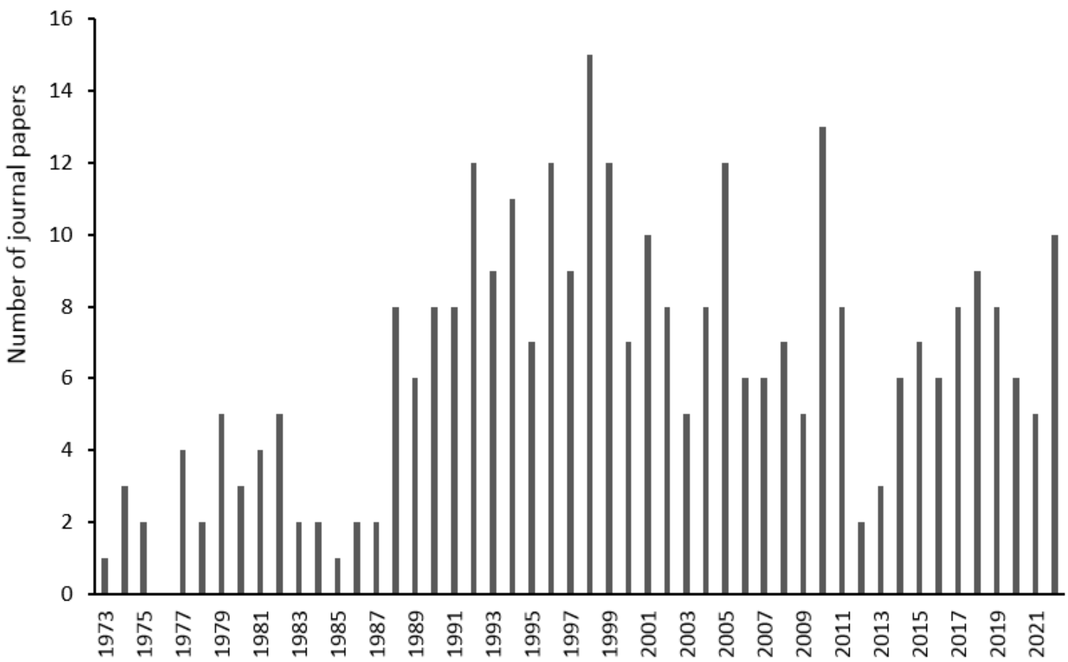
[28] I actually regard this as a healthy activity. It shows us that we are not alone and that there is seldom anything personal in the abusive treatment by referees, who have the benefit of anonymity. The so-called 'double blind' system is, I believe, a farce. It is nearly always possible for a referee to identify the author of a paper, after spending a few moments using Google or other search engines.

[29] While offering advice, I am reminded of the advice given by Denis O'Brien, which I think is particularly good. I abbreviate it to: RM-TM-CL, or 'read more, think more, claim less'.

Appendix: A Time Series

Details of the number of my papers published per year, over the fifty-year period, are shown in Figure 1. This shows a number of features. First there is the inevitable variable start during the first four years. Then there is an increase for a few years after I moved to the chair at Durham in 1978, although much of the initial work on those papers took place while I was full-time at the National Institute of Economic and Social Research in London (1977-78). The following 'dip' arises because I carried the burden of Head of Department during 1980 to 1983, with a full teaching load, and because I was also working on several books. The increase after I arrived in Melbourne in late 1987 is very clear, although again much of the initial work was carried out during a very productive and stimulating year at Penn State University (1986-87). The influence of a very collegial and active department in Melbourne, with light teaching loads compared with the heavy load in Durham, is evident. The 'dip' around 2003 is associated with my two-year full-time appointment at the New Zealand Treasury (late 2001 to early 2004), although I did manage to keep research going with Melbourne Institute colleagues during that period. The move to a half-time appointment at the New Zealand Treasury in 2011, with a half-time position at Victoria University of Wellington, and the associate shift in my research programme, is also evident.

Figure 1 Number of published journal papers per year



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