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Is Science A Case of Wasteful Competition?

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I. INTRODUCTION

Scientists, and economists in particular, have mixed feelings about the use of citations and rankings in evaluating scientific work. In an age of increasing specialization and meritocracy, citations and publications offer a quick-and-dirty measuring rod, which avoids the side effects of nepotism that prevails in the 'old boys'-network. However, there are always two sides to a story and citations can become dysfunctional when they lead policy makers, deans, committees, students and readers to make wrong decisions in hiring and firing, in giving grants, choosing colleges, or in accepting ideas. Most of the worries are reaffirmed once the bibliometricians among us try to measure 'progress' at the aggregate level. Recently, Laband and Tollison (2003) have shown that, in spite of the growth of resources between 1974 and 1996 invested in academic economic research, the percentage of uncited papers in economics has remained more or less constant at 26 percent (in the five years subsequent to their publication). In their view, this is evidence of scientific waste or 'dry holes' as they dub them. 'Scholarly economic research presents many of the characteristics of a rent-seeking game (p. 168),' was their conclusion.

Laband and Tollison are, of course, not the only ones to worry about the scientific research that goes unnoticed. When the popular press gets hold of this fact, it has a field day, exposing how much money is wasted. Quite a few scientists will react with embarrassment when confronted with such popular drubbing of their work, especially when their research belongs to the category of little cited to uncited research. The worry extends to the consequences. Does it mean that, when scientific research is not cited, the money spent on that research has been a waste? Should the number of citations therefore be a

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market test of research? When this informal 'market test' is applied to academic research, most research will fail. In the competition for attention, the large majority of publications goes unnoticed. Scientists know how difficult it is to get their research published, and even more difficult to get it published in a good journal; and they know that even when a paper gets published, the chance of it getting read and cited is pretty slim. Scientists may want to believe that they are making claims to the truth, but the truth is that those claims often go unnoticed. It is an iron law that most articles receive few or no citations, and only a few articles receive a great many citations (Klamer and Van Dalen 2002). This law is the frustration of practicing scientists and may come as a shock to those interested in funding scientific research. But before concluding that scientific research involves a great deal of waste, or that doing science is senseless if the work is not noticed, we may want to reconsider the practice of science to understand why this so-called waste occurs. It may be inevitable. And it may well be that the real waste shows up in a different guise, which citation and publication data will not easily detect.

II. SOME FACTS OF 'WASTE'

So how bad is the state of the scientific publication industry? What are the facts? The outstanding feature of scientific publication and citation behavior is the skewness, not only in publication productivity of scientists, but in particular the citations these publications and their authors receive: most articles receive few or no citations and a few receive a great many citations. The median article in a science journal has a negligible influence on the literature, only the top 5 or 10 percent of the science literature matters, i.e. it gets read and is cited by peers. Generally these top journals represent the core of a discipline; journals which have a wide circulation, extensive peer review and are managed by people who have made their mark and who can recognize high quality or high impact papers (although their choices are, of course, not flawless). The type of skewness may be a sign of competitiveness within a science: the more skewed the distribution of attention is, the larger the pay-off to risky research and, in that respect, *Table 1* gives us a flavor of the differences among sciences.

Competition in the natural sciences is not only revealed by the number of journals and articles appearing, the core journals in the sciences also take up a larger share of the ongoing conversation, even more so if you correct for the impact which the average article in journals has had for the past two years. E.g. the top 10 percent of highly cited medical journals produce 27 percent of all the articles in the medical profession and, if you correct for the impact of these

Table 1

Size and Distribution of Publications in a Number of Sciences and Social Sciences, 2002*

Discipline	# journals	# articles published in journals	% articles published by top-10% of journals[a]	Idem % articles published in top-10% of journals weighted by impact factor
Science	5,876	716,304		
Biology/biotechnology	193	18,279	30.3	51.8
Chemistry	418	85,530	40.4	64.0
Computer Science	338	19,739	30.0	50.6
Mathematics	329	22,998	22.5	33.4
Medicine	183	20,883	26.7	74.5
Physics	267	85,718	46.9	69.6
Social Sciences	1,709	64,039		
Anthropology	53	1,491	28.3	41.4
Economics	166	7,081	9.3	24.8
Educational research	116	3,572	12.0	29.1
Law	102	2,719	12.3	33.5
Political Science	80	2,995	14.5	28.1
Psychiatry	78	4,735	28.3	56.4
Psychology	422	16,759	27.1	45.9
Sociology	93	2,550	13.2	33.7

*There is some overlap between the journals of the sciences and the social sciences and the disciplines within these sciences. Hence the total number of journals of the ISI database is smaller than the sum of science and social science journals.
(a) Selection of the top 10 percent of the journals is based on total number of citations received by a journal in the year 2002.
Source: Institute for Scientific Information, 2003, *Journal Citation Reports*, as reported in Web of Science, Philadelphia.

articles, the top 10 percent of medical articles accounts for 75 percent of the 'conversation' in this discipline.

The differences across disciplines are to a degree the reflection of different citation practices: medical and psychology journals are far more efficient in handling manuscripts than most social science journals (Ellison 2002a, 2002b). By shortening turnaround times from submission to date of publication, one can establish that the immediacy of impact is larger. These differences notwithstanding, the skewness of the distribution of citations is a fact for all of them; and each discipline knows the phenomena of the large percentage of published papers that never get cited.

The conclusion that all the uncited work is a 'waste', and therefore should not be funded, is unwarranted. However, it is the immediate reaction of most observers who use these citation statistics. To cite an extreme example, in 1991

the journal *Science* made the headlines with a bibliometric exercise that suggested the wastefulness of scientific research[1]. It showed that about half the science papers were never cited within the 5 years' time span after publication, a result that spurred *Newsweek* to conclude that 'nearly half the scientific work in this country is worthless' and to depict 'scientists with their belief in their God-given right to tax-payer dollars' as 'welfare queens in white coats.' (April 2, 1991) Later on, the figures were corrected for some anomalies but the blow to the outside world remained[2]. The suspicion of waste in scientific research was confirmed. This conclusion is, however, as misguided as the conclusion that most of the play during a football game is a waste because it does not produce a score. Imagine that the players would have to limit themselves to playing the highlights only! Movie producers cannot limit themselves to the production of blockbusters. Apparently, scores and hits have to occur in the company of many hapless moments and efforts, such as uncited and unsuccessful research.

The discussion may improve if the participants were to understand the skewed distribution of citations[3]. Here is a likely explanation. The extreme skewed distribution of citations, we argue, is part and parcel of what we will call the attention game in science (Klamer and Van Dalen 2002). Science is a creative profession, in which all participants not only have to see to it that their work receives attention, but also need to pay attention to the work of others (to keep up with what is going on). The problem is the excess: there are far too many articles for any scientist to pay attention to, let alone read. The Renaissance scholar, who covered a great variety of fields, is inconceivable nowadays. Everybody has to make a selection and *will usually follow others in doing so*[4]. One scientist reads an article because others cite it; by citing it in his/her own work, others may turn to the article as well. And so the snowball continues to gather momentum, squashing all kinds of other articles on its path. This outcome of the attention game reflects what the sociologist of science, Robert Merton, has called the Matthew effect of science: the accrual of greater increments of recognition for scientific contributions to scientists of considerable repute, and the withholding of such recognition from scientists

1. See Hamilton (1990, 1991).
2. Later on, the ISI, by means of a letter by David Pendlebury in *Science* (March 22, 1991), corrected these figures, as the initial figures included journal marginalia (book reviews, letters and editorials). The corrected uncitedness figures for physical sciences are 22% (initially 47%), social sciences 48% (initially 75%) and the humanities 93% (initially 98%).
3. An extra reason why differences in citation rates and levels of uncitedness between sciences exist is the difference in citation practices. It takes, for instance, many more years before a publication is recognized in the social sciences, such as sociology, demography and economics, than for an idea to be recognized in medicine or chemistry (cf. Hargens 2000; Van Dalen and Henkens 2005).
4. We leave out the screening that takes place before publication; that process only amplifies the skewed outcome.

who have not made their mark. This effect is consistent with what economists depict as a winner-take-all profession (Frank and Cook 1995). As in the attention games in the movie and book publishing industries, as well as in the arts, the amount of attention paid to scientific work and the recognition received is highly skewed towards the 'lucky' few. The superstars receive excessive attention, whereas the starlets and the rank-and-file receive little or none.

In view of the odds, participation in this attention game is a gamble: the risk is great that one's work goes unnoticed and uncited, yet if one article happens to catch attention, the rewards in terms of reputation, invitations to conferences, and possibly promotion, are great. The speculative character of the game continues to affect even the stars. They may continue to publish, but continued attention after a hit is far from guaranteed. You would think that they are guaranteed ample attention, but even they run the risk of having their work ignored. Nobel Prizes are usually awarded for work done early in the career. The critic might argue that funding should be halted after that first success, as the chance of another success is small; the remainder of the work is a waste. Yet, who can tell? Like the dull moments in a game, 'waste' is an inevitable part of creative work. Eliminate waste and you eliminate the possibility of a rare, outstanding piece of work.

Skewness implies the phenomenon of stars. We know from the economics of superstars (Rosen 1981) that superstars are characterized by (1) a close connection between personal reward and the size of one's own market; and (2) a strong tendency for both market size and reward to be skewed toward the most talented people in the activity. Large markets are like science prizes; they stimulate the search for new territory. Social scientists, like sociologists and economists, have to be satisfied with far smaller markets than people working in chemistry and physics, and therefore are engaged to a lesser extent in priority races. In physics and medicine, the priority on every scientist's mind is making a discovery, because the prize of recognition is large.

III. THE COST OF A COMMON GOOD – SCIENCE

The infatuation with citation figures of deans and policy makers, and the concomitant question of why so many articles are never cited, will remain at the forefront of the policy debate, as the flood of publications will increase in the age of electronic publishing and therewith the number of anonymous authors who fail to catch a glimmer of the limelight. Policymakers tend to turn skeptical when they find out about the so-called 'waste' in scientific research. Why should they allocate scarce tax dollars to finance such a waste? But the costs of such a waste are minimal, especially when we consider that scientific research is, in principle, a game without geographical borders: After all, most of

the research gets communicated in international journals with English as the *lingua franca*. Science is therefore not a national but a *global affair*. Stiglitz (1999) has stated more than once that research produced by scientists is a global public good and should also be treated as such. Whether this argument will help to win votes for the budgets of national science foundations is questionable, but it helps us to see the publishing game in the appropriate perspective.

To make our case, let's assume that there is one gigantic decision maker who finances research and who is quite sympathetic to the scientist's fate and who couldn't care less whether the publications produced by the numerous scientists are cited or not. Sooner or later, ideas will pop up, and among those ideas there will be an excellent idea that saves costs, lives or even time to calculate another excellent idea. The only concern this decision maker has is that ideas are produced, communicated and brought out in the open as soon as possible. That is the entire idea of the publishing game in science. It is a tournament, in which being the first to publish a report, and having society acknowledge it in return, is, for the scientist, the only prize worth having. Now what should this publishing game cost the average tax payer: 10 percent of national income, 1 percent, 1/1000 of a percentage point or even less than this small fraction? Applying some back-of-the-envelope accounting, one can easily show that the publishing game is not at all a worrisome institution. For the purpose of making this numerical claim, we will define scientific research as all knowledge codified and disclosed in journals registered by the ISI in the science citation index (SCI) and the Social Science Citation Index (SSCI). The cost of producing one article covers a large number of factors, like the monetary value of time it takes the author to produce a paper, the time editors and referees put into evaluating the merit of publishing the paper and then the costs incurred by the publisher, like printing, copy-editing, marketing and, last but not least, the mark-up to make publishing a profitable business. The direct costs incurred by publishing firms, and reflected in serial prices, attract the most attention in discussions on the serial crisis or the publishing crisis. Estimates of direct costs based on a sample of mathematics and engineering journals fall in the range of $1000 to $8000 per article[5]. But direct costs are, by and large, dwarfed by the indirect costs of which the costs of preparing a paper must surely be the largest category. Tentative estimates arrive at an amount of $32,000 per article, of which $20,000 is attributed to the author's cost of preparing a paper, $8,000 to library costs and $4,000 to editorial and refereeing costs (Odlyzko 1997). The total number of articles published in the 7,500 research journals in the sciences and social sciences amounts to 762,000 multiplied by the all inclusive cost per article of $40,000 – taking the most conservative estimate of the direct costs into account – and one arrives at the grand total bill for codified science of $30.5

5. King and Tenopir (1998) arrive at a cost estimate of $5152 per article, which falls within this range.

billion. To keep in line with the earlier reached principle that science is a global affair, one should relate this number to the world income and, in doing so, the publishing game of science seems to be a cheap affair, as 0.0006 percent of world income for 2002 (48,443 billion US$, source IMF) is allocated to making scientific knowledge public.

Sure enough, the ISI journals are only the tip of the iceberg of science journals: according to *Ulrich's International Serials Database*, there are currently (issue 2004) about 250,000 journals being published, of which 21,000 are refereed. If we take the latter group as the boundary set, and we assume that these journals publish more or less the same amount of articles as the journals registered by ISI, then we should multiply the science cost figure by a factor 2.8. In other words, the corrected price of publishing scientific findings is 0.0018 percent of world GDP. That makes science not an entirely free lunch, but it certainly is a cheap lunch.

IV. COMPETITIVE STRATEGIES OF ATTENTION SEEKERS

Even if the supposed waste of scientific research does not add up to large monetary amounts, practicing scientists continue to have to live with the harsh facts of the attention-getting game in which they are involved. 'How to survive in the game and emerge with some recognition?' seems to be on the mind of every modern-day scholar. Edward Leamer (1981) gives us a hint of what kind of strategies scientists could follow, or actually do follow, to gain attention.

> 'Many of you will conjure up reasons why the number of citations should be ignored. There are fads; there are self-citations; there are conspiracies; there are derogatory citations; there are bribes to editors and referees; there are sycophantic students; and there are subjects capable of direct understanding by only a few. But why didn't your paper start fads; why don't you publish more and cite yourself; why did your conspiracies fail; why don't you become an editor; why don't your students care about your welfare; and why don't you insist on writing about obscure issues?'

The ultimate question is, of course: do these strategies distort the conversation in science in a significant manner, or is this simply the way the world of science works, and it may well be working fine? Let's consider the most important strategies that Leamer cites and evaluate their alleged distortionary nature for a science that is close to home: economics.

1. Starting Fads

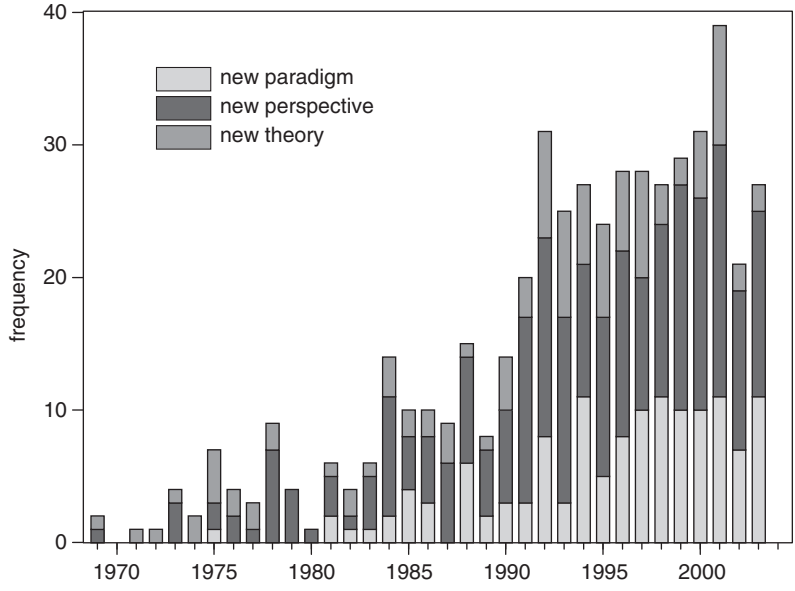
Starting a fad is much frowned upon by academics, but let's face it, it requires a special talent to make a subject the talk of the town. Stigler (1955, p. 6) also considers this possibility, and points out that a fad will only make 'a deep and

lasting impression on the science if the idea meets the durable standards of the science'. To make an idea stick, it is not sufficient to be original; salesmanship has to accompany the process of invention. Stigler takes the case of John Stuart Mill as exemplary for having an original mind. But he did not sell his ideas persuasively, or accompany his written thoughts by salesmanship, and for that reason has not become truly path-breaking.

The most common strategy to gain attention from peers and get the ball rolling is simply by 'advertising'. In writing on the technique of persuasion, George Stigler (1955) states that new ideas are even harder to sell than new products: 'Wares must be shouted – the human mind is not a divining rod that quivers over truth'. General repetition, inflated claims and disproportionate emphases are, according to Stigler, the strategies that accompany the adoption of every new idea in economic theory. The techniques of persuasion have not lost their touch, as *Figure 1* below shows: in trying to attract attention to papers in the economics literature: in trying to rise above the enormity of papers being published, an increasing number of economists grab the browsers' attention by promising new paradigms, new theories or new perspectives in the title of their

Figure 1

Inflated Claims – Attention Grabbing Title Words in Economic Literature, 1969–2003



Source: Econlit (2004)

papers. Over the years, there have been apparently more breakthroughs or more paradigm shifts in economics than philosophers ever could have imagined. Of course, such claims are inflated[6], and scientific practice corrected for such inflation behaves far less progressively than the attention grabbing words suggest.

Salesmanship is just as important in grabbing attention as the scientific quality of a work. In that respect scientists, can learn a bit from ordinary businessmen who value both Research *and* Development, whereas scientists give the impression that the R of research is all that matters. The tenacity of door-to-door salesmen has to be part of the make-up of a scientist, and advertising your own work (by self-citation, by brainwashing your students or by organizing workshops and conferences) are all legitimate. The scholar, who successfully sells his or her ideas, is in the words of Stigler 'more a warrior against ignorance than a scholar among ideas'. According to the Institute for Scientific Information, approximately 20 percent of all citations are self-citations.

2. *Publish, Publish, Publish … or Perish*

One condition of getting recognized is the academic status of the researcher. A hundred years ago, and more, men of practical affairs were almost on an equal footing with academic scholars when it came to publishing research (Stigler et al. 1995). Their articles appeared regularly in the academic journals. Nowadays, academic credentials are a prerequisite for the right to get published in an academic journal. Gentlemen researchers do not stand a chance in the current game for attention. Even academics, who venture out into the non-academic world, like into the government bureaucracy or into the research department of a private organization, see their chances for academic publication seriously diminish. Publications of non-academically occupied economists in star journals, like the *American Economic Review* and the *Journal of Political Economy*, are rare, whereas, in the distant past, the appearance of non-academic authors was a more common phenomenon (see Laband and Wells 1998). The dominant player in the production of ideas is the Academic Professional (Klamer and Colander 1990). Graduate training tells the students that the only chance they have to make it in the academic attention game is an academic job at a top university. As an Academic Professional, their lives will be focused on academic life with its academic conferences, research seminars, socializing with other academics, and endless hours in the office. Such life does not guarantee citations, but it is the only chance.

6. For an earlier evaluation of the use of inflated claims, see Cohen (1999), who notices that 90 percent of the 'new paradigm' papers affect the research world very little.

Late bloomers stand little chance in this game. Educational institutions put a premium on relatively early manifestations of ability. In making decisions on who is to become a member, universities are increasingly relying on publication and citation records (Hargens and Schuman 1990) and indicators of future productivity, such as the time required for completing the doctorate. Merton (1988, p. 614) may warn against the pitfalls of such a practice, but academic institutions do not want to take chances with the late bloomers.

Graduate students adopt the behavior of their teachers. Zuckerman (1977) shows, in her survey on US Nobel laureates, how students of eminent and prolific scientists in general are also prolific writers. Graduate training generally proves to be decisive for the novice academic professional. As Buchmüller et al. (1999) demonstrate, publications and submissions prior to leaving graduate school increase the probability of being employed at a research university where productivity is higher. And so the ball starts rolling. The question, of course, is whether this character trait is acquired, or already apparent and merely cultivated by the supervisor. According to Van Ours and Ridder (2003), who examined the PhD completion records of Dutch graduates in economics, the research productivity of the supervisor is an important determinant of completion and dropout rates of graduates. However, the apparent effect of the research track record of supervisors on the completion rates is due to the selection or attraction of high ability students and not to superior supervision.

3. 'Conspiracy' – Create Your Own Club

The most enduring strategy to make the process of gaining, distributing and sharing attention manageable is to create clusters. Scientists specialize and form clusters in their specialization, each with its own 'discursive practice' or 'conversation', its own journal and association and annual conference. Clustering is a condition for making the process of seeking and getting attention more manageable. The downsizing of clusters has proven to be an effective response to the inflation of research publications. It enables all kinds of selection procedures, without which the world of science would not be able to function. The connection with the phenomenon of 'stars' would seem to be obvious, but is far from trivial in actual practice. We suspect that the larger the cluster, the more space there is, and need, for the creation of stars, charismatic leaders who bring order out of chaos. The reason may be that sustenance of the large cluster (say econometrics or finance) requires a core knowledge that all 'members' share. The sharing will force a highly skewed distribution of attention. Indeed, anyone who has seen his or her share of rankings, knows that people like Clive Granger or Eugene Fama are in a far better position

to generate a huge amount of citations with a single article than economists working in some esoteric sub-discipline. However, stars also have an intermediary role to make diverse clusters stick together. Goyal et al. (2004) show in an impressive paper how the world of economists has become more integrated in the period 1970–1999. In the 1970s, 16 percent of the authors publishing in the economics literature belonged to the most connected group of economists, whereas in the 1990s this percentage has risen to 41 percent, in spite of a rapid increase in papers being published and economists competing for attention. Multi-authored papers may be part of the explanation, but the average author in the 1990s worked with three other authors on average; hence integration by co-authorship seems unlikely. They claim that what makes the world of economists tick is the existence of interlinked stars. Without the 5 percent most connected authors, the entire network of economists would become completely fragmented.

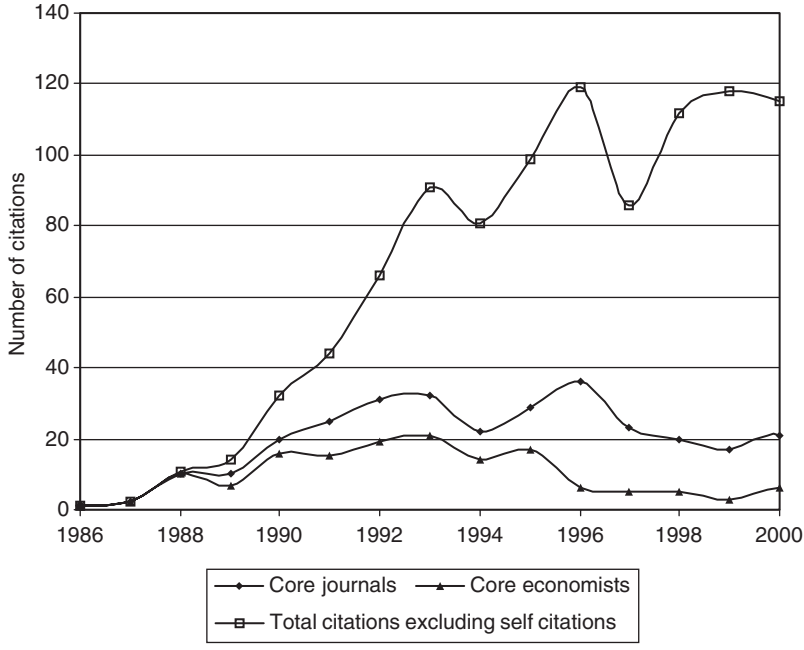
The idea that clusters of star scientists are important for the transmission and generation of ideas goes back to Crane's concept of invisible colleges that dominate the frontiers of science (Crane 1972). Ideas do not consistently come out of the blue, i.e. ideas are not randomly scattered around the world. The geographic proximity of great minds matters in the birth of ideas. This is clearly illustrated in the dominance of a few institutions that have attracted Nobel laureates, the University of Chicago being, of course, the champion attractor of Nobel economists (see Van Dalen 1999). One could say that Nobel laureates are valued for contributions of a long distant past and today's scene is very different. However, in spite of prophets who claim that distance is dead, US institutions still set the tone in economics (Coupé 2003).

The main importance of clusters is that they set the standard of conversation, they frame the questions of interest, select the conversation or sparring partners and they test the robustness of ideas in debate (Klamer 2006 forthcoming). Naturally it is of some importance to win a debate, because influencing one big giant is worth far more at the invention stage than influencing 10 dwarfs, who may perhaps give you their promise that they will cite you, but whose papers rarely get published, and certainly not in core journals. Or look at it in another way: not only do giants generally form a better testing ground; they are also prolific writers of high impact articles in which they cite the proposed idea. To see how this works out in practice, take a look at *Figure 2*, which illustrates the 'applause' generated by Paul Romer's article on 'Increasing Returns and Long-Run Growth' in the *Journal of Political Economy* of 1986[7]. Romer had just

7. The citations made by core economists, or citations stated in core journals, are defined as follows: core journals are journals belonging to the top-20 of the list of journals ranked by Laband and Piette (1994). Core economists are economists belonging to the list of the top-150 economists who were ranked by Meddoff (1989).

Figure 2

Citations Generated by Romer's Increasing Returns Article (JPE 1986)



finished his PhD thesis and this article was his very first article. A striking characteristic of the total number of citations is that increasing returns apply, not only to the content of the article, but very much to the attention generated for a very long period in the lifetime of this particular article. Ten years after the publication date, the number of citations settle at around 118. However, the most striking aspect of the attention generated by Romer is to be found in the role played by economists belonging to the core of their profession and the core journals. The first three to four years after the publication of this JPE article, the core economists and core journals generate attention almost single-handedly. After six or seven years, the economists publishing in second tier journals take over, which is also not so difficult to understand. Not only did these second-tier economists have to forego the first-mover advantage, which the 'invisible college' economists had, they are generally not so prolific and skilled in getting papers published. When the followers catch on, the innovators in the publication process have already moved on and lost interest in the initial paper. In the last three years, a meager 5 percent of the citations to the Romer-paper come from top economists, in contrast to the very beginning of the

paper's career, when 90 percent of the citations were generated by the 'core' economists.

V. SEARCHING FOR THE REAL 'WASTE'

Starting fads, publish or perish, and starting your own club are strategies that are 'all in the game'. Economics teaches us that there is *always* another side to the story. Economics is about trade-offs and seeking attention relentlessly by each and every participant has its price. The strategy of Stigler to advertise with the zest of a used-car salesman has the drawback that words lose their meaning or, to rephrase this for the subject at hand: Inflated claims inflate your reputation and your ideas. In theory, this should not matter, because the theory of rational expectations teaches us that we can (in principle) see through the veil of money and, in a similar fashion, we should also be able to see through inflated claims. However, just as money illusion exists in everyday life, we have to doubt the ability of participants to see through the veil of the academic coin. Inflated claims in science will thereby affect the real behavior of the community of scientists. This is perhaps seen in its most eminent form in the work of new classical economists. A telling anecdote is perhaps the following one made by Robert Lucas. He and his co-author, Leonard Rapping, were joking at a social occasion about a remark made by Edmund Phelps in the introduction to the book *Microeconomic Foundations of Employment and Inflation Theory*. Phelps stated that '… perhaps Lucas and Rapping are 180 degrees to the truth', and Lucas and Rapping, being young academics and elated at being cited for the first time in an important volume, found this an amusing statement. However, Rapping's wife, who was also present when they chuckled over Phelps' remark, was shocked and said:

> 'All you two care about is being cited by a well-known economist, about being famous. It doesn't matter to you whether you are right or 180 degrees off'. (Lucas 2001)

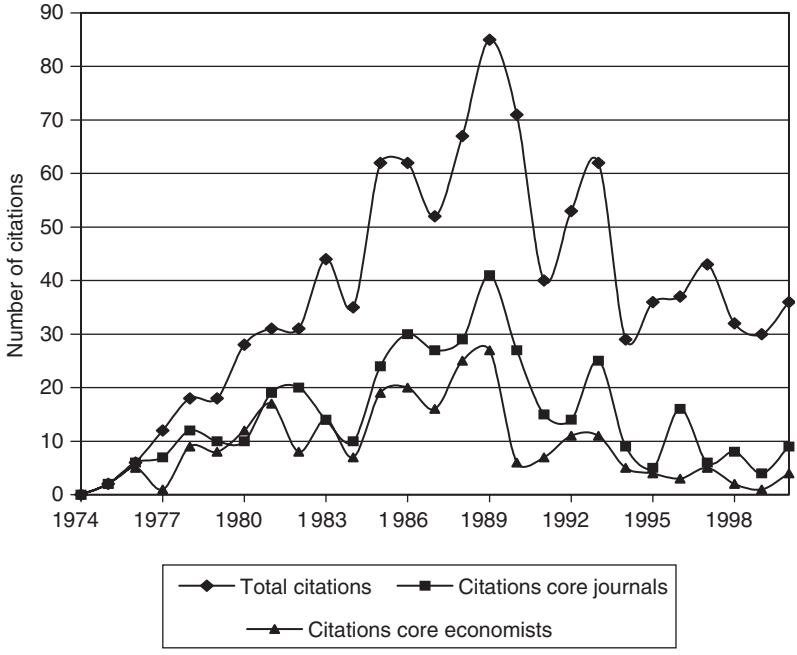
The search for an explanation of the way the world works can easily be substituted by a search for fame, period. The difficulty with using citation statistics is that this distinction is lost in the translation and the strategies which deans and policy makers design. In that respect, not being cited is not necessarily a sign of waste, just as receiving many citations is not necessarily a sign of a scientific breakthrough. It could be more heat than light. If citation studies are to shed light on the practice of economists, then at least the following two strategies may be worth considering.

1. Dig Deeper …

Plain citation data can be just as misleading as macroeconomic statistics about the state of a nation. Micro-studies and/or longitudinal data have to supplement

Figure 3

Citations to the Ricardian Equivalence Paper of Barro (JPE 1974)



the quest for the real waste. For instance, the Laband and Tollison (2003) study, just like the *Science* study of 1991, uses a five-year period to evaluate the state of 'waste'. However, there are numerous studies which show that each discipline has a different lag structure with which ideas are acknowledged and cited (see Hargens 2000)[8]. Furthermore, as Van Dalen and Henkens (2005) show, the state of uncitedness is not necessarily a good predictor of future uncitedness. In other words, negative duration dependence in being cited is not some iron-clad rule.

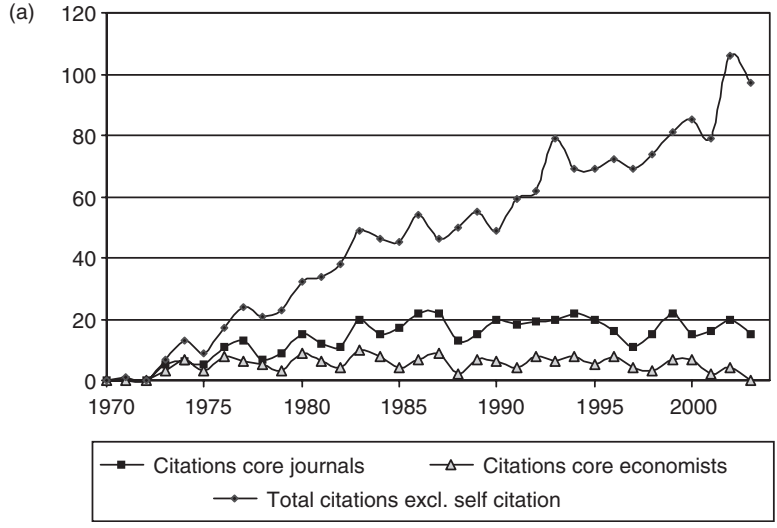
To give another example, the age of rational expectations yielded a host of neutrality theorems, of which the Ricardian equivalence theorem of Robert Barro is perhaps one of the most notable statements of this era in the history of economic thought. *Figure 3* shows how the classical JPE paper on stating the neutrality of public debt has fared over the years.

The 1980s were, in that respect, the high tide; right now the paper is getting citations from by and large second and third tier authors. The scientists

8. Mayer (2004) shows some other pitfalls of citation statistics, as laid out by Laband and Tollison (2003).

Figure 4a

Citations to the 'Lemons' Paper of Akerlof (QJE 1970)

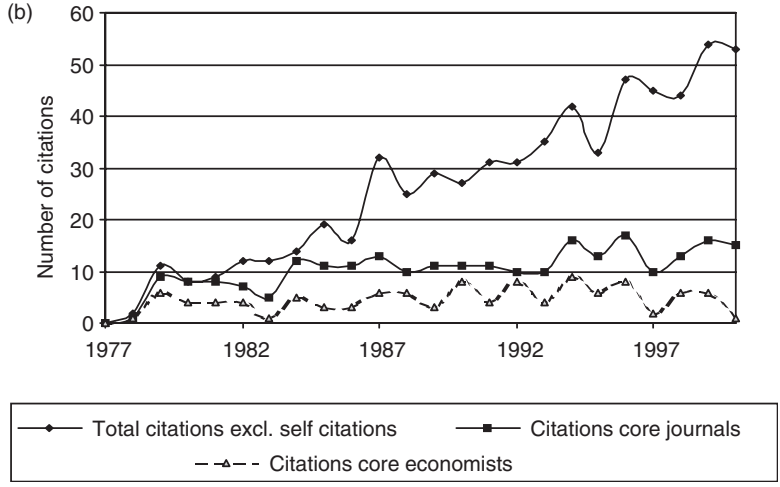


working on the frontiers of knowledge are not that much affected by the Barro-paper. Perhaps this last phenomenon is not that exceptional. 'Obliteration by incorporation' is the Mertonian phrase, and contributions can become so fundamental that only meticulous rookies take pains to cite 'path-breaking' contributions. However, one can have serious doubts about the 'obliteration by incorporation' argument just by looking at three other fundamental papers written about the same time as Barro's classic: the paper by Peter Diamond on Public Debt (1965) – comparable in nature and subject to that of Barro – the Dixit-Stiglitz paper on monopolistic competition (1977), and the Lemons paper by George Akerlof (see *Figures 4a–4c*). Each of these papers displays perhaps the same citation pattern with respect to citations received in core journals or core economists (most core economists have lost interest in this issue), but the patterns diverge when it comes to the total number of citations. Whereas the influence of Barro's paper is clearly declining, the other papers display a steady and increasing citation rate. Apparently, their contribution is so fundamental that it still inspires many economists, or is an essential stepping stone in gaining insights.

Another example of a problem of 'waste', which may be examined by digging deeper, is the problem of plagiarism. The darker sides of the 'publish or perish' culture become visible once scientists are tempted to perform fraudulent acts (like copying research, faking data and statistics, intentionally leaving out erroneous findings) or fall prone to unethical behavior that is permitted by

Figure 4b

Citations to the Dixit-Stiglitz Paper on Monopolistic Competition (AER 1977)



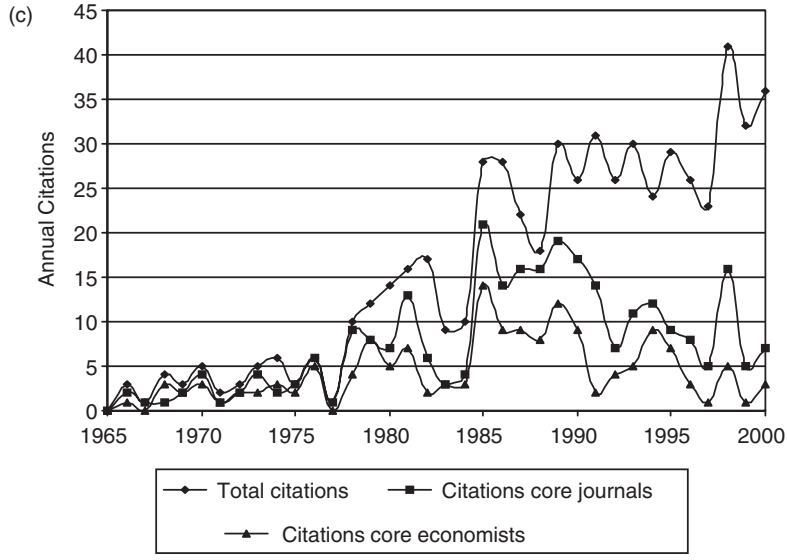
peers because the form of conduct has become 'normal'. Examples of the latter are reflected in citation games, like not citing the 'enemy', the citation of friends and not giving credit when credit is due. But it is also apparent in publication strategies, like slicing up one piece of research in a number of more or less identical papers (the so-called 'salami tactics'), which are submitted simultaneously to journals and edited books, putting your name on the list of authors (preferably first) without having contributed, or leaving out authors (usually assistants or PhD students) in the list of contributors who have made a real contribution etc. List et al. (2001) provide a peak at unethical behavior. They found a significant amount of misconduct among economists, particularly with respect to the expropriation of graduate student research or including an undeserving co-author on a research paper.

2. … And Look Out of Your Window

Another strategy may supplement the information content of statistics and inform policy makers and practitioners to look out of their window and get to know the real world. Economists, taking their cue from Milton Friedman's influential essay on positive economics, are not enthused about asking their economic agents what goes on inside their black box. One should judge an agent by his actions and not by his words is the tacit message economists put

Figure 4c

Citations to the Paper on Public Debt and Growth by Diamond (AER 1965)



across. Preferences do not have to be stated, as they will reveal themselves by the deeds of agents. The funny thing is that the corner stone of every economist – the benefits of the division of labor – was explained by way of recounting the organization of a pin factory. Adam Smith, or – closer to the truth – his teacher, Francis Hutcheson, who already used the example of the pin factory, discovered the use of reality economics (Van Dalen 2005). Economists are not very interested in their own pin factories, and yet it is there that we can really get a feel for what 'productivity' and 'technical progress' really are, how they are brought about and how they are destroyed. Reality economics, or 'learning by asking', as Alan Blinder et al. practice in their book *Asking about Prices* (1998) seems to be going through a revival. Of course, there have always been economists of name and fame who have practiced this art at all times. Alfred Chandler and Ronald Coase are economists who can serve as role models. The basic idea of reality economics, as we would like to call it, is that it not only offers a source of inspiration, but primarily a reality check on the way scientific discovery works. 'Reality economics', as practiced through interviews and surveys, may bring what is going on inside a science more alive than plain citation or publication statistics. In a series of interviews among economists, we (Klamer 1984, Klamer and Colander 1990, Van Dalen and Klamer 1997) have sketched a picture of how the world of economists functions, what triggers their

curiosity, who they respect, what they think makes an economist and what constitutes a persuasive argument. This type of research may help to focus on the essential questions which scholars face, or why the distance between academia and the policy arena diverges over time or by culture.

VI. CONCLUSION AND DISCUSSION

Contrary to the messages brought across by citation and publication statistics, science is not a clear-cut case of wasteful competition. Waste is part and parcel of science and innovation. Based on citation or publication statistics alone, it is therefore hard to say that public expenditure is being squandered, even if most of the research goes unnoticed. Our back-of-the-envelope calculations suggest that, on a global scale, the scientific publication industry costs the world 0.0006 percent of global income. In science and research and development, so-called 'waste' or uncited patents and articles are part and parcel of the act of discovery. For practicing scientists, this harsh 'fact of life' means concretely a scarcity of attention or, as Herbert Simon (1971) once put it: 'An information rich world creates a scarcity of attention'. The chances of your work being attention-catching are slim. The game of attention may be unfair, as only a few get the lion's share of all the attention. The winner in this game takes almost all.

However, our message is mixed: there *may be* 'waste', but citation and publication statistics are simply scratching the surface. Exercises, such as those of Laband and Tollison (2003), trigger a discussion, but the figures they present are not conclusive. Like nineteenth century economist Frederic Bastiat was wont to emphasize: economists should try to 'see what is not seen'. We see a skewed distribution of attention, but what lurks behind these citation statistics? The so-called 'waste' is not the main worry, as it may be the veritable proof of healthy competition and the 'carrot' for inspiring scholars may be bigger than ever. The more worrisome features of competition in academic economics reveal themselves, not through ordinary statistics or the competitive attention seeking strategies, but through the badly designed use of market principles, in which citation statistics have become the sole measuring rod in evaluating and rewarding effort in science. Reward schedules in economic science can fall prone to the classic problem of incentive design: rewarding A (publication) while hoping for B (novel ideas), or badly designed distribution of property rights of ideas (Frey 2003). Perhaps, at this point, academia should pay close attention to how industry deals with innovation. One of the strategies to deal with the innovation in winner-take-all markets is to experiment and let a thousand flowers bloom. As Scherer and Harhoff (2000) point out, for a world of highly skew-distributed outcomes: research programs should not be judged by the numerous failures, but the relatively few big successes should be emphasized.

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SUMMARY

Science is a winner-take-all profession in which only a few contributions get excessive attention and the large majority of papers receive scant or no attention. This so-called 'waste', together with all the competitive strategies of scientists seeking attention, is part and parcel of every creative profession and not a worrisome fact, as the price society pays for human ingenuity is extremely small: 0.0006 percent of world income goes into the publication of scientific research. The more worrisome features of competition in academic economics do not reveal themselves through ordinary citation or publication statistics or competitive attention seeking strategies, like starting fads and networking. Badly designed uses of market principles, in which citations and publications have become the sole measuring rod of scientific 'productivity', deserve more attention instead of the excessive focus on being uncited. To detect the real story of scientific progress, or to judge academic work, 'reality economics' or 'learning by asking and watching' should complement citation and publication statistics.