

The Complementarity of Scientometrics and Economics

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Abstract

Economists, especially those of the Chicago school, value systematic empirical evidence to support generalizations concerning human behavior. Hence, when studying the behavior of academic labor markets and the efficiency of academic institutions, they have naturally turned to scientometric measures to understand the phenomena and to test their theories. Economists and scientometricians share epistemic assumptions about the value of measurement and the privileged epistemic status of science. It is therefore reasonable to hope and to expect that scientometricians and economists will find their research programs complementary.

Introduction

Eugene Garfield is the *sine qua non* of the account that follows in more ways than one. Much of the research recounted here would not have been done, or would have been done more poorly or at much greater cost, were it not for the powerful tools that Garfield's intellectual entrepreneurship brought into being.

More concretely, Garfield recommended to Michael Koenig that I be asked to deliver the after-dinner address for the opening banquet of the Fifth Biennial Conference of the International Society for Scientometrics and Informetrics in Chicago in 1995. My paper is a direct descendent of that address.

I believe that Garfield first became aware of my work through my paper (1986b) that estimates the dollar value of a citation. Garfield reprinted this paper in *Current Contents*, and invited me to write two additional papers for *Current Contents* (1989a, 1989b), one ranking economics journals (that is discussed below), and the other analyzing important papers and research

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frontiers in economics. I will always be grateful to Garfield, both for the tools he created, and for his encouragement of my applications of those tools.

One way to address my topic of the relationship of economics and scientometrics would be to discuss it at the methodological level. Except very briefly at the beginning and end, I will refrain from doing so. As Milton Friedman's teacher, the great economist Frank Knight, once said:

"... discussing methodology is like playing the slide trombone. It has to be done extraordinarily well if it is not to be more interesting to the person who does it than to others who listen to it" (as quoted in Merton, Sills, & Stigler, 1984, p. 331).

Instead of emphasizing methodology, I will mainly present several brief examples of the actual research that economists have done that makes use of scientometrics.

But first, a little "slide trombone." In my economics courses, I try to tell my students about the range of positions on the proper role of theory and mathematics in economics. For some, economics has become little more than mathematical modeling. When I first started teaching, I tried to make this point with the following story.ⁱ

An engineer goes into a room. There's a fire in the room. The engineer runs out, gets a pail of water, returns and puts out the fire. There's another room. A physicist goes into that room. There's a fire in the room. The physicist whips out his pocket calculator, calculates exactly how much water is needed to put out the fire, runs out and gets exactly that much water, and puts out the fire. There's a third room. A mathematician goes into the room. There's a fire in the room. The mathematician whips out his pocket calculator, calculates exactly how much water is needed to put out the fire—and walks away.

When I argue for the complementarity of economics and scientometrics, I am arguing most strongly for a complementarity between scientometrics and a certain type of economics; what I would call the "Chicago School of economics." To the educated public, and to most academics, the "Chicago School of economics" mainly calls to mind the *laissez faire* views of Milton Friedman. But an equally important feature of Chicago economics has been the tradition there that empirical evidence matters more than theory, and that the simplicity and tractability of a theory matters more than its mathematical rigor.ⁱⁱ When I use the "Chicago School" in this paper, I will mean those economists who, like the engineer in my story, grab only enough theory to put out the fire.

None exemplified this aspect of the Chicago School better than George Stigler, who for many years co-edited the *Journal of Political Economy* in the Social Science Research Building at the University of Chicago. On the facade of that building is a paraphrase of a famous statement of the British physicist

Lord Kelvin:

"When you cannot measure your knowledge is meager and unsatisfactory."ⁱⁱⁱ

For George Stigler, Kelvin's statement might have been his intellectual manifesto. Or perhaps he preferred a statement of the same sentiment once sung by his friend George P. Shultz:^{iv}

"A fact without a theory
Is like a ship without a sail,
Is like a boat without a rudder,
Is like a kite without a tail.
A fact without a figure
Is a tragic final act.
But one thing worse
In this universe
Is a theory without a fact."

George P. Schultz (1973)

George Stigler was the most famous advocate of scientometrics in economics (Stigler, 1965b, 1965c; Stigler & Friedland, 1982a, 1982b). He advocated measurement and he especially advocated measurement of academic output.^v

In the rest of the paper, I will present a brief smorgasbord of how economists, in the Stigler tradition, have used, and possibly contributed to, scientometrics. The order of topics discussed begins with those that are mainly empirical and then proceeds to those that increasingly emphasize theory. Because of space limitations, I will leave to another occasion the discussion of the growing literature in which scientometricians study economics (e.g., Peritz, 1990; Rau & Hummel, 1990; Spangenberg, Buijink, & Alfenaar 1990; Spangenberg, Breemhaar, & Alfenaar 1990).

Economists Have Applied Scientometrics without Theory

Perhaps the most common applications of scientometrics by economists have been the studies in which economists use scientometric measures to rank scholars, journals or academic departments. The reputation of this sort of work has not been high, since it is sometimes viewed as "navel-gazing" rather than serious scholarship.^{vi} This reputation is unfortunate since tenure, promotion, and other resource allocation issues often depend on measuring the quality of research output. It is inconsistent and misguided for the profession to denigrate the research that underlies such important decisions. And since such work is not highly valued by the profession, probably less time and effort have been invested in it than is optimal.

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In this genre, I once wrote an article for Garfield's *Current Contents* (1989a) ranking economics journals that have become the focus of controversy, especially in the United Kingdom. Researchers there have interpreted the number of articles published in my "core journals" to be one of several outputs of economics departments (Johnes & Johnes, 1992, 1993). They have used a technique akin to linear programming, called "data envelopment analysis" (DEA), to estimate the relative efficiency of economics departments in transforming measured inputs into measured outputs. Some have apparently objected (e.g., Hodgson, 1993) that the use of my ranking in such exercises penalizes those scholars who publish in non-mainstream outlets. In their more limited critique of my ranking, Burton & Phimister (1995) still suggest that:

"Given the continuing assessment of research in the United Kingdom by the Higher Education Funding Council, the extent to which the status of articles from a particular journal are downgraded has important implications for research funding for individuals and institutions" (p. 361).

Burton and Phimister apply the DEA technique to the process of journal ranking itself. My paper had presented multiple rankings of journals according to criteria such as gross citations, citations adjusted by number of articles in the journal ("impact"), and self-citations. Burton and Phimister apply DEA to the three measures with the aim of combining them into a less arbitrary single ranking. One important, and perhaps reassuring, finding of the Burton and Phimister paper is that, in their re-rankings using DEA, "... many of the journals in the Diamond core reappear ..." (p. 372). Of course, some journals do not reappear, and it is also worth noting that some journals that appear in some of the DEA-generated rankings do not appear in other DEA-generated rankings. Such differences matter, especially to those who have published in journals whose ranking is not robust across differences in ranking method. Some of the arbitrariness concerning which criteria to include in a ranking and how to weigh the criteria will always remain. But reducing the role of judgment (also known as "arbitrariness"), perhaps through a well-established (see, e.g., Diamond & Medewitz, 1990) method such as DEA, may represent a laudable advance in method.

Besides Hodgson in the U. K., my journal-ranking article also apparently has its detractors in the United States. Jim Henderson of Valparaiso University (and the 1996 president of the History of Economics Society) has told me that as a result of my journal ranking paper, I am on a lot of people's lists of enemies. He mentioned, in particular, that my article had been used as justification for dropping one of his favorite journals from being indexed in the *Journal of Economic Literature*. I'm sure that many other scientometricians have also been the target of similar irritation. I do not know what Jim Henderson's views on journal rankings were prior to his unhappy encounter with the results of my

ranking. But in general what is ironic, at least among economists, is that many of those who criticize our work, placed little value in it until someone used it to make tough decisions about how to allocate scarce resources. Resources ARE scarce, and economists should know that better than anyone else. Scientometric measures are often better than the subjective whim that often would be used in their absence. I urge the critics to help us improve the measures rather than just criticize them (and in fairness, some are doing that).

Economists Have Applied Economics to Explain Inductive Generalizations Discovered by Scientometricians

For the most part, the work by economists related to scientometrics does not evidence an extensive reading of the work on scientometrics by non-economists. Occasionally, however, there are exceptions. Cox and Chung (1991) claim to be the first economists to examine whether the literature of economics is consistent with Lotka's Law (1926). The Law implies that the frequency distribution of articles per author is highly skewed. The authors find confirmation for the Law in economics, finding, for example, that, out of 13,576 authors, 57.7 percent had published only one article (p. 741). This finding has been generally confirmed in the scientometrics literature and Aloysius Siow's (1991) paper on the importance of first impressions attempts to explain it. His intriguing model implies that if a scientist is unlucky in the quality of her first published paper, other scientists will not read the scientist's later papers because the expected return will be higher for reading new papers by other recent Ph. D.s. Knowing this, the unlucky scientist will give up if the first paper has been a failure.

Economists Holub and his co-authors (1991) have published a paper on "The Iron Law of Important Articles," in which they hypothesize that most publishing is done in order to pass a "publish or perish" screen. Based on citation, and other evidence, they propose an "iron law of articles" which states that the number of "important" articles is equal to the square root of the total number of articles. (They acknowledge a debt to Derek J. de Solla Price (1965) on this.) They conclude that very few published articles provide new and useful information for readers.

Stigler (1982a) also agrees that many articles do not contain substantive new knowledge. He suggests that rather than serving as a screen, the research makes the researcher a better teacher by keeping her up-to-date in the latest methods and substantive developments in the field. (This is also the paper where Stigler says that neutral citations should be counted as positive citations, because they publicize and help keep alive the cited article: this an example of Stigler's dictum that "all publicity is good publicity, except for an obituary.")

Economists Have Used Scientometrics to Document the History of Economic Thought

The earliest, and probably the most important, work in this area is due to George Stigler. Stigler's first major paper in this area is his "Statistical studies in the history of economic thought" (1965c). One of the most valuable aspects of this paper is Stigler's documentation of the nineteenth century professionalization of economics. The last paper on which Stigler was working at the time of his death (Stigler, Stigler, & Friedland [1995]) presents citation evidence that in economics the influence of theoretical work on empirical work has been greater than the influence of empirical work on theoretical work.

In one of Stigler's (1965a) papers on Mill, he concludes that those scientists are best remembered who actively promote their own work and exaggerate its importance. Following Stigler's advice, I next direct your attention to the seminal paper by Diamond (1988) on the effect of Gerard Debreu's general equilibrium theory on applied work. Diamond looked at a random sample of 92 articles that cited Gerard Debreu. Of these, 87 were theoretical and five were empirical (by the most generous definition of empirical). Even in these five papers, the references to Debreu had nothing to do with the empirical sections of the papers.

Economists Have Used Scientometrics in Studies of the Academic Labor Market

Perhaps the largest literature applying scientometrics to the academic labor market examines how scientific output varies with age. One motive for the literature was the concern of some (e.g., Cole, 1979) that the aging of the scientific community might result in a slowdown of scientific progress. Within economics, the literature on this topic has been both theoretical and empirical. Several years ago, I published a couple of theoretical articles in *Scientometrics* (Diamond, 1984, 1987) in which I applied life-cycle human capital investment theory to the case of scientists. This theory says that human capital investment will be greater early in life, because there are more periods in which to reap the rewards, and the opportunity cost of spending time acquiring the human capital are lower. I suggested that the stock of a scientist's citations was a proxy for her knowledge at the frontiers of the discipline, and that a scientist's salary would be a function of her knowledge at the frontiers of the discipline. Hence, citations should be a significant determinant of scientists' salaries. I also suggested in one of those papers (1984) that the main reason that the output of older scientists declines is not the intrinsic decline in mental powers, but rather the increasing demands on the scientist's time in administrative and gate-keeping duties. The empirical studies (both my own [Diamond, 1980, 1986a; Hull, Tessner, & Diamond,

1978] and the more recent work of Stephan & Levin, 1992) of age and scientific productivity have generally shown that age matters in the expected way, but that age has not mattered as much as would have been expected, e.g., by Planck's oft-repeated claim that:

"[a] new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (1950, 33-34).

Holtmann and Bayer (1970) were the first to include citations in earnings functions. A robust finding in all studies, including my own paper, entitled "What is a citation worth?" (1986b), is that citations are a positive and significant determinant of earnings over almost all of the observed range of citation levels. The marginal value of a citation (when the level of citations is zero) varies between \$50 and \$1,300 (in 1984 dollars). Some differences in marginal values may be due to differences in citation practices among disciplines while others may be due to differences among the studies in the control variables included in the salary regressions. Finally, no gain in explanatory power results from the inclusion in the salary regression of the costly non-first-author citation measure.

Robert Frank (and others) have examined another phenomenon of the academic labor market: Why is there greater variance in measures of scientific productivity, such as a scientist's number of publications or number of citations, than the variance of scientists salaries? Frank's answer (1984, 1985) is to use what economists call a "compensating differentials" argument. Scientists who publish the most derive pleasure from knowing that they are the big fish in the pond. Since they thus find their work more pleasant, they do not require as much monetary compensation to remain on the job as they would otherwise. Conversely, those who publish little suffer psychic torment at knowing that they are little fish in the pond. Hence, they have to be paid *more* to remain on the job than their productivity would imply. (Other economists have given other accounts of this phenomena in terms of the university insuring scientists for the possibility that they may turn out to be low-productivity scientists.)

Another important use of scientometrics by economists has been the economic study of academic mobility. Ault and his co-authors (1979, 1982) have shown that scientists who publish more will have higher rates of upward mobility in the quality of school that employs them, but the effect is small. Grimes and Register (1997) have replicated this research for a more recent cohort, finding that quality matters more than quantity. (Here, quality is measured by the rank of journals in which articles are published.) I am not

aware of any studies that have looked at the effect of citation levels on mobility, and I think this is an important topic that needs exploring.

Economists Have Used Economic Analysis to Understand how Institutional Incentives Have Affected the Publication and Citation Behavior of Academics

In one of the foundation papers of this literature, "The market for fame and fortune" (1988), David Levy presents evidence that economists maximize the same utility function that economists assume others maximize.

Anderson and Tollison (1986) argue that new findings are rewarded by citations, whereas rediscovering an old truth in a previous writer will result in citations to the previous writer, not the re-discoverer. Hence, from a socially optimal point of view, there will be under-investment in the history of economic thought and over-investment in "reinventing the wheel."

Dewald, Thursby and Anderson's (1986) paper on replicability surprised the economics profession by showing the large number of empirical errors in the papers that were selected for attempted replication. David Levy & Susan Feigenbaum, in a series of papers (Levy & Feigenbaum, 1990; Feigenbaum & Levy, 1993a, 1993b, 1996a, 1996b), deepened our knowledge of these issues by developing and testing models that seek to explain how careful an economist will be in econometric work.

The evidence on the low rate of replication set off a debate in the economics profession about how institutions could be reformed. Responding to the debate, the *Journal of Political Economy* started a special section devoted to replications, and the National Science Foundation (NSF) started requiring that recipients of NSF grants make their data available to other researchers. The economics program director of the NSF has also sought to encourage journal editors to require that data availability be made a condition for authors to publish articles. The argument against his proposal is that the incentives for original data collection are already too low (as revealed by how little of it economists do). To make a data collector give up property rights in the data so soon after collection will reduce even further the incentive to collect original data. This is a legitimate concern, but a concern for which there might be an answer: why in the social sciences, do we not award co-authorship to those who collect the data in much the same way that biologists sometimes give co-authorship credit to those who develop a cell-line even though they make no other contribution?

Several economists (e.g., Stigler, 1982b) have discussed cases of multiple discovery, citing Merton (1973), and sometimes asking whether multiple discovery represents an efficient use of society's resources. I think one of the most interesting arguments on this issue has come from Don Patinkin (1983) who claims that resources spent on those who come in second or third may not be wasted.

He claims that the also-rans benefit society by being in a position to recognize and help spread a scientific advance more quickly when it occurs. (Patinkin also claims that multiple discovery, properly understood, is much less common than usually thought: to have discovered something is to have made it a "central message," not merely to have presented it briefly, or obscurely.)

Jack High (1987), in his response to Donald McCloskey's (1985) highly cited "Economical writing," has argued that the profession rewards obscurity and penalizes clarity. David Levy and I (Diamond & Levy, 1994) decided to test this claim by using the style program Grammatik to measure the style of the Presidential addresses of the Presidents of the American Economic Association to see if clearer writing was rewarded or punished in terms of citations. We found ambiguous evidence on the effects of word length and sentence length on citations received, but found clear evidence that the use of the passive voice in writing was negatively related to the number of citations received. We also found that the use of the passive voice has decreased over time.

Economists Have Used Scientometrics to Obtain Measures of Knowledge in Models of Technological Progress and Economic Growth

Perhaps the most ambitious recent contribution to this literature is Adams' (1990) paper in the *Journal of Political Economy*. Adams estimates an industry-level production function that includes as inputs the stock of scientific knowledge in each of nine fields of science, with each stock weighted by the number of scientists in the field who are employed by the industry. (The stocks of scientific knowledge are proxied by the stocks of articles published in the area, depreciated by an obsolescence rate.) Adams finds that academic science is a major contributor to productivity growth and that there is approximately a 20-year lag between the publication of scientific research and the exploitation of that research by industry.

Jaffe and others have also recently begun to use patent citations to measure which technological advances have been most fruitful in spawning other advances (Jaffe *et al.*, 1993; Trajtenberg, 1990).

Concluding Obiter Dicta

Traditionally, the social science that scientometricians have felt closest to has been sociology. But scientometrics has been increasingly abandoned by the sociologists of science, as more and more sociologists of science adopt a constructivist methodology. I have attended the meetings of the Society for Social Studies of Science (4S) for many years now. In the beginning, I could count on several of the sessions being of interest to scientometricians. (I

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believe that the same points could be made by doing a content analysis of the pages of the journal *Social Studies of Science*, but I have not done so.)

Over time, there has been a decline in interest in scientometrics at the 4S meetings, and a corresponding increase in interest in "constructivist" analyses of science. I would argue that constructivism and scientometrics are not complementary approaches. To the constructivist, much of scientometrics represents fundamentally misguided activity.

Both scientometricians and economists tend to believe that science is a privileged form of cognitive activity. In contrast, the constructivist believes that since all claims are constructed, none are any truer than the others. Scientometricians and economists believe that if measurement is done well, it can add enormously to our knowledge of human behavior and institutions; constructivists believe that measurement is no different, and hence no better than any other epistemic method.

While scientometricians will understandably continue to work with those sociologists in the tradition of Price, Merton and Hargens, they may find it fruitful to expand the circle of those with whom they work to include a growing number of economists. Of course, it goes without saying that economists would also greatly benefit from increased attention to the work of scientometricians.

The reason why economics (or at least the economics of the Chicago School) is complementary with scientometrics, is largely that we all agree with Kelvin that:

"... when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science, whatever the matter may be" (Lord Kelvin as quoted in Merton *et al.*, 1984, p. 327).

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Endnotes

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annual meetings of the Society for Social Studies of Science; and the annual meetings of the History of Economics Society.

- i I heard this story in the early 1980s, but have no record or recollection of the source.
- ii Klamar and Colander (1990) have documented this difference between Chicago and other major graduate schools in their "The making of an economist."
- iii Some of the founders of the Chicago School of economics who labored in the Social Science Research Building found Kelvin's statement misguided:
"When you can measure it, when you can express it in numbers, your knowledge is still of a meager and unsatisfactory kind." (Jacob Viner as quoted in Merton *et al.*, 1984, p. 330)
"If you cannot measure a thing, go ahead and measure it anyway."
(Frank Knight as quoted in Merton *et al.*, 1984, p. 324)
- iv Shultz was then Director of Management and Budget, formerly Dean of the College of Business Administration at the University of Chicago, and later Secretary of the Treasury and Secretary of State.
- v Stigler's *apologia* for the use of scientometrics in the history of economic thought follows:
"The study of the history of economics has escaped all the forces that have transformed the character of economic research in the twentieth century. Neither foundation nor government is at all interested in intellectual history, so it is perhaps the last unsubsidized research area in economics. It has escaped any serious quantification, and research assistants can seldom be used—why, even committees are scarce in the field! I therefore apologize to my fellow workers in the field for the lapse from old-fashionedness in the previously unpublished essay in this volume, 'Statistical studies in the history of economic thought,' and promise not to sin again, often." (1965b, p. v)
- vi One articulate criticism of the ranking literature is in Colander's (1989, 141-142) and is a very useful, though now somewhat dated, survey article on research on the economics profession.