

UNEXPLOITED SCOPE FOR QUANTITATIVE WORK IN THE
HISTORY OF ECONOMIC THOUGHT

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The kind of scholarship expected of economists (particularly historians of economic thought) these days promotes the copious use of footnotes, huge bibliographies and a more general approach that no stone shall be left unturned. To date, though, historians of economics seem to have been too preoccupied with making their own citations to take time away to study the citation process itself, and consider the significance of observable patterns of citations. Of the few attempts so far made by economists, the work by Eagly (1975) on networks of citations amongst journals is perhaps the best known. Given the limited interest that historians of economics have shown in this area, many may be surprised to hear that work outside economics has been carried far enough to prompt the publication, recently, of a very useful little volume (Cronin, 1984) that presents a review of existing literature on the citation process, covering issues such as the motives and stratagems used in citation and presenting pertinent empirical findings. (Curiously, Cronin does not cite Eagly's work). In this note, I do not intend to attempt to summarise or review Cronin's book; rather, I wish merely to suggest some ways in which historians of economics might engage in empirical work using the Social Sciences Citation Index.

The thinking behind the present note is very much the same as that which I have employed on two previous occasions (Earl, 1983, 1987), and it builds on the premise that it may be useful to think of economists as economic agents operating in a complex, competitive production system, producing outputs (articles, monographs, and so on) which may serve as inputs in the production of other contributions to economic knowledge. The marketability of an economist's outputs, and the success with which they are marketed to potential users, may be significant in determining whether or not the economist succeeds in gaining promotion, in influencing other economists, in winning a Nobel Prize or even a somewhat more lowly place in the economists' hall of fame (for example, in deciding which living economists to include in the first edition of Who's Who in Economics, Blaug and Sturges (1983) chose the 640 authors most frequently cited by their fellow economists). With the aid of the Social Sciences Citation

Index, the historian of economics is in a position to investigate the rise and fall of particular contributions to economics as influential inputs in the production process that some call economic science.

Despite my enthusiasm for reflexive thinking about the behaviour of economists, it had not dawned upon me that there might be scope for depicting the production of economic knowledge explicitly in input-output terms until my attention was drawn to an editorial article by Garfield (1986) in a recent issue of Current Contents. Garfield's piece works at two levels. On the one hand, it consists of an interview with Leontief about his contribution to the literature of economics, and presents an outline of the citation frequency of Leontief's works. On the other hand, it is about the applicability of input-output analysis to the study of citations patterns. Unaware of Leontief's work himself until shortly before the interview, Garfield was not unaccustomed to gathering citations data in matrix form and using appropriate quantitative techniques, such as multi-dimensional scaling, to identify clusters of core sources in particular research areas.¹

A consideration of input-output analysis has prompted Garfield (1986, p. 3) to promise that "we now plan to use Leontief's matrix structure, in an essay next year, to present data on the stocks and flows of citations in and among economics journals". So far, I have yet to see the piece, but the very idea of it must set one thinking about the enormous scope for studies of processes of influence within and between diverse areas of economics, to see who reads (or at least credits) whom. Two main lines of attack come to mind.

First, one can construct matrices to display which particular articles or monographs are used as inputs in the production of other articles and monographs, producing chains of interconnections at one end of which are the latest contributions in the field in question. At the other end of these chains are the inputs which may fall into the category of what, following Sraffa (1960) somewhat loosely, one could call the "basic" sources in the literature: these are the contributions that are used directly or indirectly in the production of all the non-basic contributions in the sample. (These would not be true basics in the Sraffian sense, since a piece cannot be cited in itself in the way that, say, oil can be used in the production of oil.) It may be the case that,

within a particular field, there are no basic sources, no contributions without which the literature as it stands would have got off the ground: there may be a variety of starting points in the production of particular overlapping contributions. In these cases, the sorts of techniques already employed by Garfield could be used to generate some statistical indication of the significance of particular pieces as "core" contributions to the field.

Second, one can investigate the citation experience and behaviour of particular authors, with a view to discovering patterns of authority amongst authors, or at least the citing tendencies of particular scholars. In an ideal world, one might hope that economists who worked in well-defined research areas for a substantial period of time would end up recognizing, through citations, the work of fellow scholars who had been influenced by their own works: for example, one day I might be honoured myself with citations in the work of Herbert Simon or Brian Loasby, whose own, earlier contributions to behavioural economics have influenced me so much. The extent of an established scholar's influence on another's work might similarly be indicated by the frequency of citation of the former in the latter's output. Those who conduct this kind of inquiry may be in for a number of surprises and puzzles. (Why, for example, does Oliver Williamson give so little credit to his early mentors Cyert and March in his work on opportunism and transactions costs and fail to discuss their conceptualization of organizational slack? Why does he treat Leibenstein in a similar manner in respect of X-efficiency, especially given that the latter does relate his concept to contractual incompleteness? Why does Leibenstein likewise give short shrift to Cyert, March and Williamson in his work on X-inefficiency? These are errors of omission that no recently admitted member of the profession would be allowed to get away with in the refereeing process: one wonders whether attempts at academic product differentiation are going on in this kind of situation.)

With this kind of work, the starting point would be to draw up an N by N matrix, entering the name of each economist being studied in the inquiry in a column and in a row. The Index would then provide four simple kinds of entry for this matrix: (1) X cites Y but Y does not cite X; (2) Y cites X but X does not cite Y; (3) X cites Y and Y cites X; and (4) X does not cite Y and Y does not cite X. But we would want to go beyond such basic, all-or-nothing tabulations. In categories

(1) and (2), it would be possible to count the number of times an author who was never cited by a particular second party actually cited the latter's work. For category (4), instead of simply noting that there is a two way relationship between a pair of authors, one can look at the net exchange of ideas between the parties that is reflected by the data; for clearly it would be foolish to class X and Y as mutually influential if X cited Y twenty times, while Y cited X only once. An additional dimension of the question of mutual influence is that concerning the chaining of contributions: in other words, does a piece by Y, which X cites, itself refer to X's earlier work?

In this kind of investigation, one may need to move beyond a mere counting of the number of citations recorded in the Social Sciences Citation Index and do some careful follow-up work on the context of the citations that the Index records; for there is an obvious difference between a work receiving a bibliographic entry by virtue of a single appearance in a footnote, and a work receiving a single bibliographic entry despite multiple in-text referencing. Some kind of weighting system reflecting these issues would need to be introduced in any attempt to process data in this area of inquiry. Such weights would also need to include indications (by their sign?) reflecting whether or not the work is cited with approval or disapproval (I've cited Milton Friedman on a number of occasions, but that doesn't mean he influences my thinking in a positive way).

Precisely how one might manipulate these complex data matrices is something I have yet to fathom: I wish I were more familiar with the literature on factor analysis, multidimensional scaling, principal components analysis, and other, related techniques. If we are trying to assess the relative standing of members of a particular group, then broadly we would want to use a method that scored highly someone (like, say, Keynes) who managed to receive all manner of in-text citations by others on the basis of a minimum of citations of the work of others; at the other extreme we would have the kind of scholar who has so far received no citations or, slightly better, has enjoyed only a single, footnote reference, and yet who has cited practically everybody in the sample (as might be the case for a young scholar who goes to extremes to make his or her work bulletproof in the face of referees²). With this kind of investigation we could restore simplicity somewhat by leaving the reciprocity issue

aside and noting who had cited whom, weighing together the number of cited works and their contexts and frequencies of citation. This would take us back to a somewhat more sophisticated version of the sort of work discussed earlier in respect of Garfield's activities except that, instead of looking at whether individual works are core or peripheral contributions, we are looking at the relative influence exerted by individuals. (The weighting process for citation contexts could also be used to enhance the former kind of study.) On the other hand, if we are interested in membership of particular schools of thought, then the incestuousness of research output is what we should be looking for, so we clearly must grapple with reciprocal citation patterns and indirect patterns of connectedness (for example, if X does not cite Y but does cite Z, who cites Y's work, then we would appear to have greater connectedness than in a situation in which X cites Z but does not cite Y, and Z does not cite Y either).

The investigations thus far tentatively proposed have concerned the cumulative market success and use of economists' outputs in the production of further works. However, historians of thought should note also the scope for quantitative work on the diffusion of economic ideas. One way of modelling this process would be in terms of the characteristics of the products in question in relation to rival products and the wants of economists -- in other words, to relate the history of relative citation frequencies to non-price characteristics of the works, taking inspiration from the work of Lancaster (1966) and Ironmonger (1972). This method would be fraught with difficulties arising from the lack of "objectively measurable characteristics" of economists' works, and from differences in economists' preferences and perceptions. One certainly could attempt to confront the significance of subjective factors head-on by exercises in the critical analysis of reviews or by interviewing samples of economists using the repertory grid technique of Kelly (1955) to discover distributions of different ways of construing particular competing and complementary contributions. However, the notion that it might be possible to model diffusion processes whilst largely avoiding looking at the characteristics of the contributions themselves may be one worth taking seriously. In this context, it could be useful to begin by considering the implications of the following observations by Prais (1973, p. 579):

An alternative approach in considering the diffusion of new commodities is to regard them as formally similar to a disease spread by a process of infection, the number catching the disease in a particular period is proportional to the product of the number of persons who have already caught it, and the number who have so far not caught it. By a blessed dispensation of mathematics (to use a phrase of the late Dennis Robertson), this leads to a sigmoid growth curve of pleasing realism; and this curve has been found useful in applied work in understanding the growth in ownership of cars, television sets, etc.... The virulence of a disease no doubt determines its rate of infection; and the rate at which a new commodity diffuses through the population no doubt depends in some way on relative prices or, perhaps more precisely, on the relative efficiency in meeting certain wants in relation to the costs of the old and new commodities. Rates of diffusion can vary immensely: one is reminded that a careful American study has shown that it took 34 years for the Revised Standard Version of the Bible to be adopted by even 23% of Iowa church members; but an equally careful American study has shown that it took under a year for an improved rat poison to be adopted by as many as 78% of a sample of Ohio farmers.

If sigmoid growth curves can be used to study the diffusion of new versions of bibles in the theological sense, then why not apply them in conjunction with citation data to study the diffusion of economists' "bibles"? One problem is the lack of a well defined market whose proportionate penetration can be inferred from the citation data. Another difficulty is that, in economics, as in the market place for consumer goods, some products flop disastrously at the outset, or never seem to infect more than a tiny minority market, whilst others die off in sales terms only later to come back into fashion.³ These considerations would militate in favour of work on diffusion that is not aimed at fitting sigmoid curves to citations data and yet seeks to avoid the problems in characteristics-oriented work in this area. In looking for suitable projects, it may be useful to bear in mind that the idea lying behind much of the sigmoid curve work on diffusion is that demand is very much the product of social interaction and patterns of experience. Such thinking leads me to the following tentative proposals for quantitative research on diffusion processes in the economics literature.

First, given that some contributions clearly fail to get off the ground and acquire a mass market amongst economists in their subject area, it seems worth exploring whether there is any sign of a critical takeoff speed of citation which commonly has to be reached. A long period of scattered contributions is something one would have thought far less likely ultimately to result in a strong and persistent rate of citations than would an initial bout of intense recognition, for the probability of potential readers discovering the work is so much lower in the former case given the tendency for economists to scan recent literature on the basis that newer is better.

Second, it needs to be asked to what extent particular individual citings can be pinned down as instrumental in enabling a contribution to reach the critical citation rate (if one exists). Some scholars may be exceedingly influential in determining the fate of particular contributions, much as in the way the Austin Mini was given a dramatic boost, after initially slow sales, when members of fashionable London society started to be observed driving it. Related to this is the extent to which the takeoff speed of a citing piece tends to be functionally related to the pieces it cites. Here the causation could run either way: investigations of the cited piece could be encouraged by the fact of its discovery in the citing work's list of references; or discovery of the cited work sometime after its publication could prompt scholars to search for subsequent works that cite it.

Implicit in the previous two paragraphs was the idea that the scholar's search process involves decisions about what could be worth reading with decisions about what to read being prompted by experience of what has already been encountered. To test the hypothesis that choices of works to read may depend upon the strength of prompts, one can make use of data on the network of cross-referencing that is implicit in the Social Sciences Citation Index. Cross-references are the prompts that readers might discover if they peruse particular works. Why not use this information in an attempt to predict the different citation rates of sets of works that are inter-related vertically (in the sense of one being cited by another) and horizontally (in the sense of citing similar works in their lists of references)?

The sort of model one might construct here could be complex, to say the least, if one wished to build in additional hypotheses to catch the significance of the order

of perusal and mode of citation. For example, consider the connectedness of four not-so-hypothetical publications (readers familiar with my own work should be able to infer which works inspired this example, given the initials and dates being used): PWSA (1949), GBR (1960), OEW (1975) and BJJ (1976). Suppose BJJ cites PWSA and GBR quite extensively, but does not refer to OEW, and OEW cites GBR on a couple of occasions without ever referring to PWSA, while PWSA is referred to only in a single footnote in GBR, and cannot make any reference to the three later works himself. It would be useful to be able somehow to capture the possibility that PWSA's chances of being read and hence cited after 1976 can be expected to be worse than those of GBR (other factors equal) because, although both works receive mentions in two other works, GBR enjoys quite extensive citations in two recent works whereas PWSA is not mentioned at all in one of the recent works and only achieves a single footnote entry in an older work. I regret that I presently can offer no thoughts on precisely how such a model might look, but I would suggest that its likely complexity could ensure that it might provoke quite a virulent epidemic of work in the history of economic thought if it could be made to "work" in statistical terms as a means of predicting relative time paths of citations by works in a similar subject area--which seems to bring me nicely to my concluding thoughts.

The kinds of research outlined in this note may, I hope, appeal to historians of economic thought not merely because of the potential they offer for directly enhancing their own contributions; they may also provide useful ammunition in situations where acceptable research topics need to be suggested for honours and postgraduate students. I have no doubt that many historians of economic thought have tended to feel a sense of exclusion in this age in which dissertations at the honours level and beyond are required almost invariably to include quantitative material. Econometrically-oriented colleagues are the ones to whom the role of dissertation supervisor is naturally given, and who subsequently can enhance their research records by polishing up students' efforts into joint publications. Despite the intellectual demands of the traditional style of research in the history of economics, it is not usually considered to be an appropriate vehicle for demonstrating that a student has learnt the tools of the economist's trade: reading books and articles and drawing inferences from them seems to be valued far less than the all too often mindless estimation of variants on well-trying models. It is easy to see why this is so. Number

crunching is an activity that provides something quite definite and "scientific-looking" to examine--new numbers, freshly commented upon--and therein seems to lie its popularity in honours and graduate work. If one takes the view that this state of affairs is going to be difficult to alter and yet believes that the skills normally exercised in work on the history of economic thought are every bit as essential as the ability to run computer programmes and interpret printout according to an econometric bible, then one cannot but recognize the need for quantitative history of thought projects that stand a reasonable chance of getting past the numbers-oriented gatekeepers who decide which research is acceptable.

FOOTNOTES

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1. I found it particularly interesting to see that Garfield cited a piece by Slater (1981) on such techniques, for Slater was a name familiar to me from my ventures into applying the personal construct psychology of Kelly (1955) to economic phenomena: Slater (1977) had pioneered a number of techniques for analysing data matrices that display relationships between the ideas individuals hold in their minds, and I had already realized that such methods might be employed in the study of economists' belief structures without for a moment thinking that precisely the same techniques might be used to process information contained in matrices pertaining to connections between the contributions of individuals economists.
 2. The fledgling economist who has just come through the Ph.D. process would be particularly aware of the need to look carefully at the context in which citations are made, rather than focussing exclusively upon the number of citations; he or she will be aware that many references to the work of others are likely to be made in paying lip service to these works, not because one has found them profoundly influential in one's work. The huge reading

lists of many modern monographs (such as recent doctoral theses) are likely to reflect justifiable paranoia, every bit as much as they reflect the explosion of significant theoretical contributions and empirical findings in recent years. It is not even obvious that the "classics" are as influential as all that despite their frequency of citation; one should remember the old adage about a classic book being one which is often referred to but rarely read.

3. Prais would have done well to think of consumer product parallels with the remarks of Dennis Robertson (1956, p. 81) that "... as I have often pointed out to my students, some of whom have been brought up in sporting circles, high-brow opinion is like a hunted hare; if you stand in the same place, or nearly the same place, it can be relied upon to come round to you in a circle".

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