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From Citation Indexes to Informetrics: Is the Tail Now Wagging the Dog?

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This article provides a synoptic review and history of citation indexes and their evolution into research evaluation tools including a discussion of the use of bibliometric data for evaluating U.S. institutions (academic departments) by the National Research Council (NRC). The review covers the origin and uses of journal impact factors, validation studies of citation analysis, information retrieval and dissemination (current awareness), citation consciousness, historiography and science mapping, Citation Classics,® and the history of contemporary science. Retrieval of information by cited reference searching is illustrated, especially as it applies to avoiding duplicated research. The fifteen-year cumulative impacts of journals and the percentage of uncitedness, the emergence of scientometrics, old boy networks, and citation frequency distributions are discussed. The paper concludes with observations about the future of citation indexing.

The primary purpose of this article is to provide a synoptic review of citation indexes for information retrieval, information dissemination, and writing the history of contemporary science. In that context scientometrics and bibliometrics are treated simply as a by-product of *Science Citation Index®* production. However, in a classic example of the tail wagging the dog, we now observe and explore here the transformation of that by-product into the new field of informetrics.

As many know, there is now a substantial literature involving citation studies. Many of these studies involve the selection and deselection of journals by research libraries, journal evaluation and ranking by editors and publishers, or tracing the lifetime impact of individual scholars.

There is considerable emotion when citation analysis is discussed in connection with research evaluation. That is why the subject of validation is so important. Therefore, at the outset I quote from the recent huge report published by the U.S. National Academy of Sciences titled *Research Doctorate Programs in the United States – Continuity and Change* (Goldberger et al. 1995). Based on an extensive questionnaire technique addressed to most academic research institutions in the USA, the tabulated results were correlated with citation and publication analyses and concluded that:

> "The clearest relationship between ratings of the 'scholarly quality of program faculty' and these productivity measures occurred with respect to 'citation' – with faculty in top-rated programs cited much more often than faculty in lower-rated programs who published."

Questionnaire surveys, however, are but one of many different subjective approaches to research evaluation.

Cornelius Le Pair has stated succinctly the approach to citation analysis that I have always supported: "Citation Analysis is a fair evaluation tool for those scientific sub-fields where publication in the serial literature is the main vehicle of communication" (Le Pair 1995).

It is important to recognise the ambiguity of the term "research evaluation." Sometimes it refers to faculty evaluation, other times to graduate

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research programs. Others, like granting agencies, are doing research evaluation of particular areas of science. In all these studies, methodologies for the proper identification of specialties and sub-specialties (invisible colleges) is crucial. The work of Henry Small, Callon, Van Raan, and others on co-citation and co-word clustering is important to note. Any citation analysis for research evaluation must take advantage of such methods to provide an informed decision for funding or award purposes.

The idea of a citation index for science was the culmination of the author's investigation into the linguistic and indexing characteristics of scientific review articles and a serendipitous encounter with *Shepard's Citations*. Both these inspirations resulted from interaction with established scholars. My initial interest, which soon became a preoccupation, was aroused by pharmacologist/historian Chauncey D. Leake (Garfield 1970, Garfield 1978). My introduction to the U.S. legal citation system came from a retired vice president of *Shepard's Citation*, W. C. Adair (Adair 1955), who wrote to me in March 1953, towards the close of the Johns Hopkins Welch Medical Indexing Project, of which I was a member.

When the Project closed in June 1953, I enrolled in the Columbia University School of Library Service. There, early in 1954, I wrote a term paper proposing the creation of citation indexes. After much revision and help from Johns Hopkins biologist Bentley Glass, it was published in *Science* (Garfield 1955) in 1955. Its primary aim was to improve the retrieval of science information. That the putative *Science Citation Index*® (*SCI*®) should be unified, that is multi-disciplinary, and each journal indexed cover-to-cover was further reiterated in a paper I presented at the 1958 International Conference on Scientific Information (Garfield 1959).

At that time, there was widespread dissatisfaction with the array of traditional discipline-oriented indexing and abstracting services. They were all inordinately late. Indexing was inconsistent and uncoordinated. Selection policies left major gaps in coverage.

The impact factor

Only a few lines of the 1955 *Science* paper referred to the "impact factor" of individual research papers. The idea of average citation frequencies, that is, journal impact factors, now so widely used for evaluation analyses, did not develop for more than a decade (Garfield 1972, Garfield 1976b). Ironically, these impact measures have received much greater attention in the literature than the proposed use of citation indexes to retrieve information. This is undoubtedly due to the frequent use and misuse of citations for the evaluation of individual research performance – a field which suffers from inadequate tools for objective assessment. While there are countless legitimate applications of citation data, in the hands of uninformed users, unfortunately, there is the definite potential for abuse.

It is safe to say that journal impact factors, as reported each year since 1979 in the *SCI* and *Social Sciences Citation Index*® (*SSCI*®) *Journal Citation Reports*® have been the most widely used *derivative* metric of citation analysis. They are extensively used by libraries for journal selection and weeding and by faculty selection committees as part of the evaluation of individual performance.

A recent study from Copenhagen by Hansen and Henriksen is an illustration of the use of impact data. They found "good agreement between journal impact factor and overall citation frequency of papers in clinical physiology and nuclear medicine"(Hansen 1997).

Validating studies of citation analysis

There is a huge literature on citation analysis. But there are only a few studies that could be called "validating," in that they confirm its value in literature searching or evaluation research. This lack of extensive validating studies has not affected its pragmatic utilisation for these purposes. The work of Julie Virgo in 1977 (Virgo 1977) demonstrated a high correlation between citation analysis and peer judgement in identifying research-front leaders in cancer:

> "The purpose of this study was to develop, using objective criteria, a statistical procedure to evaluate the importance of scientific journal articles. Two approaches were taken using articles from the field of medicine.
>
> "The [We] first tested the specific hypothesis that journal articles rated important by subject experts would be cited more frequently in the journal literature than would articles judged to be less important. The hypothesis was

tested by determining the extent to which a measure based on citation frequency could predict the subject experts' opinion on the importance of papers presented to them in pairs (one pair member was an infrequently cited paper and the other a frequently cited paper). The experiment showed that citation frequency was able to consistently predict the more important paper.

"To determine which other factors were associated with articles judged important, a stepwise regression analysis was made. Although ten variables were considered, only two were significantly related to the differences between articles that had been rated on a scale of one to five of importance. While citation frequency had been a strong predictor of pair-wise judgment about the importance of articles, the regression equation performed even better in agreeing with judges' ratings.

"The design of this study called for judgements to be made on pairs of articles, one pair member being an infrequently cited paper and the other a frequently cited paper, since using extremes maximized the chance of detecting small effects.

"It is suggested that a potentially fruitful area for further research would be to obtain judge ratings on sets of articles coming from a variety of citation frequencies, not just extremes. Using the regression equation obtained in the present study, importance predictions about each of the articles could be made and compared with the subject experts' opinions about the articles. In addition, similar studies in other areas of science should be carried out to determine the applicability of the approach used in the present study, to subject areas other than that of medicine."

The same year, Henry Small performed a similar study for the field of collagen research (Small 1977). Through a longitudinal study of co-citation linkages, he identified the most important advances in collagen research over a five-year period. The results were validated by questionnaires. The survey "demonstrated that the clustered, highly-cited documents were significant in the eyes of the specialists, and that the authors of these papers were, by and large, the leading researchers identified as such by their peers."

In 1983 Michael Koenig published bibliometric analyses of pharmaceutical Research (Koening 1983a, Koenig 1983b). The second of this series involved a comparison between bibliometric indicators of expert opinions in assessing the research performance of 19 pharmaceutical companies. He concluded *inter alia* that expert judgements were very predictable from the bibliometric measures, while the converse relationships were not.

But wider-ranging validation studies based on much larger populations across most academic disciplines had already been conducted earlier by the sociologist Warren Hagstrom (Hagstrom 1971). In his paper he used citation counts from the 1966 *Science Citation Index*, just two years after the service was launched. Publications and citations were determined to be the two leading determinants in an analysis of quality indicators among 125 university departments.

These researchers compared the results found through questionnaire surveys of U.S. faculties against the results of citation analyses. And several subsequent NRC studies have combined publication productivity and citation data with peer surveys including the most recent highly publicised NAS report, (1) which was quoted earlier.

During the past two decades, dozens of papers have been published that use bibliometric data to identify leaders in various specialties by measuring article productivity, citation impact, and most-cited papers. When used in combination with peer judgements, the overall validity of these studies is rarely questioned. A recent example is the work of Nicolini et al (Nicolini et al. 1995). Nicolini describes an evaluation of 76 candidates for university chairs in biophysics and related disciplines. He correctly points out that the evaluation of an individual person by scientometric methods is complex and needs more precautions than bibliometric analyses on countries, institutions, or groups. Since this work is so recent and well known to scholars in the field, I will not repeat his conclusion (p. 106) about the relevance of such studies provided that the proper normalisation procedures are followed.

Correlations between citation impact and peer review have also been reported by Charles Oppenheim (Oppenheim 1995, Oppenheim 1997). Comparative validation studies in information retrieval have also been limited (Spencer 1967, Pao 1993). Apart from retrieval and research evaluation, there have been hundreds of applications of citation data in studies designed to test various hypotheses or conjectures or to identify key people, papers, journals, and institutions in various scientific and scholarly specialties. Not surprisingly, fields like economics and psychology, where quantitative measures of human behaviour are the norm, have produced a large fraction of such studies. Various correction factors have been devised to improve identified discrepancies between quantitative citation studies and human peer judgements. The need to account for age and other differences was predicted by Normal Kaplan (Kaplan 1967), Margolis (Margolis 1967), and others.

Information retrieval

At its official launch in 1964, and for another decade (Garfield 1964, Steinbach 1967), the utility of the *Science Citation Index* as a retrieval and dissemination device was hotly debated in library circles, but it is rarely questioned today. We do not know the precise extent of its current use for information retrieval. But we do know that it is frequently used in most major research libraries of the world, in print, CD-ROM, or online. Nevertheless, it is a sobering commentary on the conservative nature of education in science, medicine, and the humanities that only a fraction of scientists, physicians, or scholars ever receive formal instruction in the use of citation indexes. With rare exceptions, researchers do not encounter *SCI*, or *SSCI*,® or *Arts and Humanities Citation Index*® (*AH&CI*®) until they enter graduate school. A few U.S. liberal arts institutions have incorporated such training into undergraduate instruction, but the work of Evan Farber at Earlham College (Richmond, Indiana) is the exception rather than the rule (personal communication). At some large U.S. research universities, such as Purdue University (West Lafayette, Indiana) or Michigan State University (East Lansing, Michigan), where chemical literature searching has been taught for some time, use of *SCI* is covered routinely, especially as a means of augmenting searches begun with other indexing services.

A key advantage of citation indexing from the outset was its capacity to bypass the use of normal linguistic forms such as title words, keywords, or subject headings. In 1978, Henry Small described the symbolic role played by the citation in representing the content of papers (Small 1978). In combination with various natural language expressions, citation indexes greatly ve comprehensive literature searches. I have often described the *SCI* as a tool for navigating the literature (Garfield 1983, Garfield 1988) but the fundamental retrieval function of the citation index is to enable the searcher to locate the subsequent and especially the current descendants of particular papers or books. Each starting paper is used to symbolise a unique concept, from the simplest to the most complex of ideas of procedures. The *Citation Index* user frequently wants to focus initially on retrieving only those papers that have cited that primordial work. Once the citing papers are retrieved, the scope of the search can be expanded by using the papers they cite or other related papers as entry points to the *Citation Index*. These starting references can also be supplemented by using the *Permuterm*® *Subject Index* section of the *SCI* (Garfield 1976a) or other keyword indexes.

Citation consciousness

In principle, all scholars and editors ought to be asking about any publication of interest to them, "Has it been cited elsewhere?" Such citation consciousness is especially important when reviewing what is cited in newly submitted manuscripts. The routine exercise of the cited reference search in the citation index would help prevent much unwitting duplication and the alleged widespread failure to cite relevant literature (Garfield 1994). John Martyn's classic study in 1963 indicated that there was 25% inadvertent duplication in research (Martyn 1964). In any event, routine checks of citation and other indexes would help reduce such duplications. The attention recently devoted to misconduct in science has given greater impetus to the notion that authors should explicitly declare that they have searched the literature (La Follette 1994). However, the issue of "when to cite" is complex (Garfield 1964, Garfield 1996).

A fundamental dilemma arises when routine searches of citation indexes are based on what is cited in new manuscripts. How will the search identify highly relevant references that are not known to the author or the referees and were not found by traditional keyword or *KeyWords Plus* (Garfield & Sher 1993) searches? Experience tells us that relevant material is frequently missed. An idea can be expressed in many different ways that defy the normal search procedure. The advantage of citation indexing in overcoming these linguistic barriers has been extensively discussed and documented; Henry Small's review of the citation as symbol, mentioned above, is only one example (Small 1978).

The answer to the dilemma is found in the natural redundancy of reference lists. Each new research manuscript, depending upon the field, normally contains from 15 to 35 references. What is the chance that a "missing" relevant reference will not be found by searching citation indexes to determine whether any of the papers cited by the author are cited elsewhere? Such procedures have sometimes been called "cycling" (Garfield 1979).

Routine citation checks in refereeing

One of the earliest examples I used to demonstrate cycling (Garfield 1964, Garfield 1971) involved an apology published in *Analytical Chemistry*. The authors (Mazur et al. 1962) had not known about another paper (Schwartz et al. 1958) that had *anticipated* their work. Upon examining the two papers in question, I observed that half of the references they cited had also been cited by the earlier authors. This co-citation pattern is quite common, indeed pervasive, in scholarship. Its prevalence enabled Henry Small to use co-citation as a reliable means of tracing and mapping specialty literatures (Small 1973). Referees and editors ought to be asking authors not only whether a traditional key-word search has been performed, but also whether the author's own bibliography has been subjected to citation searches.

Current awareness

Apart from its widespread use for information retrieval and in evaluation research, citation indexing has two other uses that deserve special mention. The first is in selective dissemination of information (SDI). Over 25 years ago, Irving Sher and I published the first papers on *ASCA* (Automatic Subject Citation Alert) (Garfield & Sher 1967a, Garfield & Sher 1967b) now called *Research Alert*®. This system of SDI involves matching combined reference, keyword, and author profiles of each new paper with a user's search profile of references, terms, and authors. The system enables thousands of searchers to be alerted to new papers that have cited any one or more of the terms in their personal profiles, including their own or related authors' work. It is hard to understand why this type of scientific clipping service is not more widely used. However, variants of SDI profiling have been adopted in many online search systems, mainly using keywords, descriptors, or subject headings, as in MEDLINE. The comparable *SCI*-based system is called *SciSearch*® and is available on several search systems including Dialog, STN, etc.

Figure 1: SCI Search for Duplicated Research

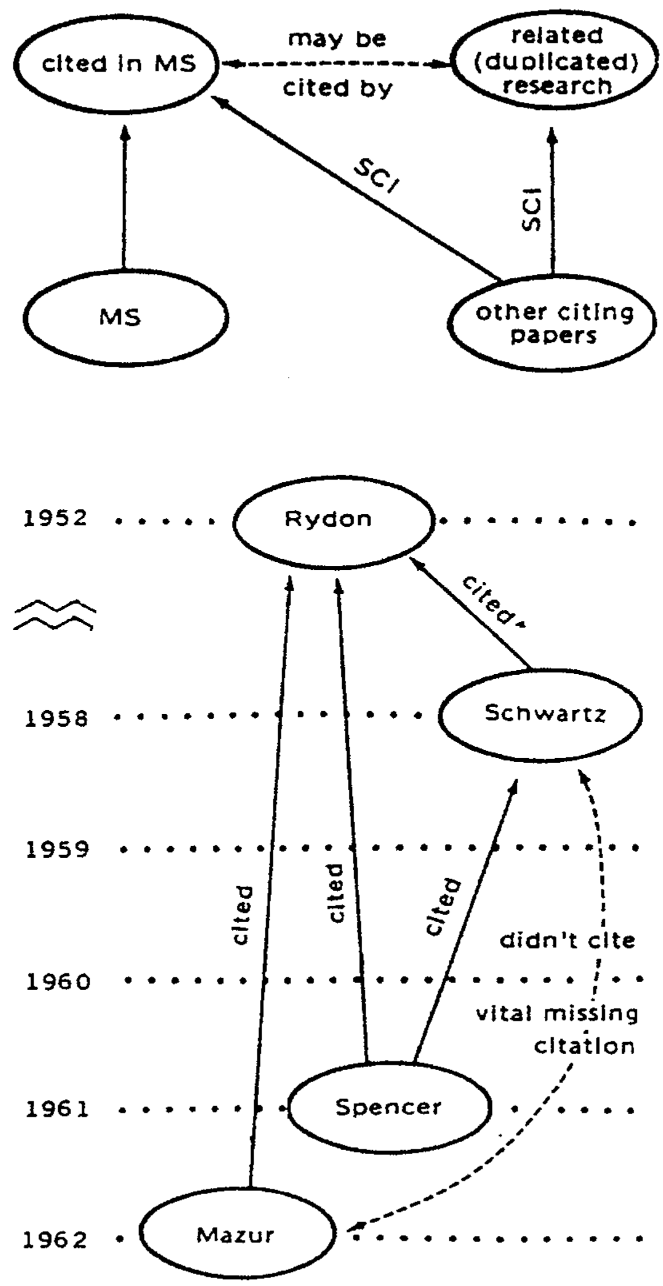

Figure 2: Skou Citation Classic

This Week's Citation Classic

CC/NUMBER 20
MAY 18, 1981

Skou J C. Enzymatic basis for active transport of Na^+ and K^+ across cell membrane. *Physiol. Rev.* 45:596-617, 1965. [Inst. Biophysics, Univ. Aarhus, Aarhus, Denmark]

The paper is a review on the characteristics of a membrane bound $Na^+ + K^+$ activated ATPase which shows that the system is responsible for the energy requiring transport of Na^+ and K^+ across the cell membrane. [The *SCI*® indicates that this paper has been cited over 1,500 times since 1965.]

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DK-8000 Aarhus C
Denmark

April 13, 1981

"In the beginning of the 1950s I was interested in the effect of local anaesthetics (l.a.) and had found that the increase in surface pressure by penetration of local anaesthetics into a monolayer of lipids extracted from nerves correlated to their local anaesthetic effect. This raised the question: Can an increase in surface pressure from penetration of l.a. into the lipid part of a nerve membrane influence the configuration of proteins in the membrane and thereby block the trigger mechanism in the Na^+-channels? Experiments showed that the activity of surface spread enzymes (catalase and acetylcholin esterase) was surface pressure dependent which was taken as an indication of an effect of surface pressure on protein configuration. I then needed a monolayer of a lipoprotein with enzyme activity to test the effect of penetration of l.a. Libet[1] had shown that there is an ATPase in the sheath part of giant axons, and being membrane bound it was likely that it was a lipoprotein. I had no access to giant axons but looked for and found an Mg-ATPase in the microsomal fraction from a homogenate of crab nerves. However, activity varied from preparation to preparation and with no explanation. Finally after three months of work it was observed that K^+ in the test solution increased activity. I went on a summer holiday to forget about ATPases and crab nerves. After returning, the experiment was repeated but no effect of K^+ was found. However, addition of Na^+, which had little or no effect in the presence of Na-ATP, increased the activity when K-ATP was used, i.e., the activity was $Na^+ + K^+$ dependent. This explained the varying results. Sometimes Na^+ and sometimes K^+ had been used for ionic strength effect in the buffer and as counter ion for ATP. The characteristics of the system suggested that it was involved in active transport of Na^+ and K^+ across the cell membrane. This shifted my interest to active transport of cations. The results were published in 1957.[2]

"Crab nerves were a lucky choice and later experiments showed that it is one of the few tissues where the Na,K-ATPase activity is revealed without use of detergents. A problem was, however, to kill the crabs. 25,000 shore crabs—200,000 nerves. The only usable way was to put them in boiling water immediately after having cut the legs—but the smell! 'Couldn't you use another tissue,' was a standing remark in the department.

"The often cited paper is a review on the following eight years of research on the Na,K-ATPase by many different authors which gave the evidence that the enzyme is found in the membrane of most cells and is responsible for the active transport of Na^+ and K^+ across the cell membrane. It appeared at a time when membranes had come into focus and there was a lot of interest in cations. For more recent information and references see, 'Isolation and characterization of the components of the Na^+ pump' and *Na,K-ATPase, Structure and Kinetics*."[3,4]

1. Libet B. Adenosinetriphosphatase (ATP-ase) in nerve. *Fed. Proc.* 7:72-3, 1948.
2. Skou J C. The influence of some cations on the adenosine triphosphatase from peripheral nerves. *Biochim. Biophys. Acta* 23:394-401, 1957.
3. Jørgensen P L. Isolation and characterization of the components of the Na^+ pump. *Quart. Rev. Biophys.* 7:239-74, 1975.
4. Skou J C & Nørby J G, eds. *Na,K-ATPase, structure and kinetics.* London: Academic Press, 1979. 549 p.

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History of contemporary science

A second further use of citation indexing is in writing the history of contemporary science. The *Science Citation Index*® source material now covers over fifty years of the literature from 1945 to the present and thus provides a major tool for the contemporary history of science. There have been about 25 million papers published since the end of World War II, containing at least 250 million cited references. In spite of the huge number of these reference links, the complete citation network can be stored in about 20 gigabytes of computer memory. Such a complete file is not yet available electronically but could be created from ISI's master tapes.

The *SCI* print edition covers 1945 to the present while 1980 to the present is also available on CD-ROM. The on-line version *SciSearch* is available on DIALOG and STN from 1974 onward. The *Social Sciences Citation Index*® (SSCI®) is available from 1955 onward in similar print, CD-ROM and on-line editions. The *Arts and Humanities Citation Index*® begins with the 1975 literature.

The availability of the ISI Web of Science adds a new dimension to these possibilities. The ease with which we can navigate these files going back over decades will surely affect the contemporary history of arts and sciences.

Citation Classics®

Using the full *SCI/SSCI/AHCI*® files one can trace an uninterrupted path covering fifty years for almost any designated paper. Nevertheless, there is little evidence that scholars conduct such searches. I myself have regularly used a derivative of this huge file to identify putative *Citation Classics*. This internal ISI file can be used to find the papers most cited for 1945–95 in the *SCI/SSCI* above a specified threshold. The file is sorted by author or journal.

As you may know, I've often stated that it is a rare Nobelist who has not published a *Citation Classic*. I'm happy to report that Professor Jens Skou is no exception (see Figure 2). However, it is also of special interest that 25 other Danish authors have published *Citation Classic* commentaries.

Over a fifteen-year period, authors of about 10,000 candidate *Citation Classics* were asked to comment on their highly cited papers or books (Garfield 1993). About 50% responded and their responses were published in *Current Contents*®; about 2,000 of these commentaries were reprinted in 1984 in a seven-volume series titled *Contemporary Classics in Science*® (Garfield 1985). Companario recently used a "sample" of these "most-cited" papers to examine a number of issues, e.g., patterns of initial rejection by leading journals for papers published elsewhere (Companario 1993). A major study of women in science also relied on this database (Astin 1991). It has been somewhat disappointing to see how few journal editors have chosen to use these specialised ISI files to identify their landmark papers (Lundberg 1984, Garfield 1987).

Perhaps this reluctance by editors is connected to a more general hostility toward and distrust of quantitative methods in peer review. The point of examining lists of most-cited papers is not to claim dogmatically that they are the "best," but rather to make certain that various evaluation processes do not overlook high-impact work that otherwise might be ignored by members of awards or other evaluation committees. It is equally important for such groups to determine why certain papers deemed to be seminal to a field have not been cited at levels one might have expected. Was this due to obliteration by incorporation (Merton 1968, Garfield 1975) or to other factors in the history of these relatively uncited landmark papers?

Emergence of Scientometrics

The new sub-specialty known as "scientometrics" has developed because of the availability of the ISI citation indexes. Derek de Solla Price was one of the first to recognise the potential of citation analysis for science-policy related studies, and thus helped found scientometrics. His mentor, J.D. Bernal, referred to such studies as the science of science. Countless scientometric analyses have been published as separate papers while many others are simply incorporated into papers that support scientific, scholarly or policy arguments of one kind or another.

There has been much ado about the applicability of citation data, but this varies at the level of aggregation chosen. The national and regional indicators reported by the NSF over a twenty-year period have used *SCI* data to identify trends of one kind or another (National Science Board 1993). Such statistical reports by country or discipline also appear regularly in publications like *Science Watch*® or *Scientometrics*. Citation analysis becomes controversial mainly when it is used as a tool in making decisions about funding or the tenure of individuals or groups, especially when it is perceived to be an uninformed use of citation data. Many of these unpublished citation analyses, like most unrefereed work, may, in fact, involve the abuse of *SCI* data and rightly evoke hostility or unease. After all, some highly published authors are little more than bureaucrats who attach their names to every paper they can. Unless such details are known to the evaluators, citation data could be used to perpetuate unjust distribution of resources. Various forms of inappropriate authorship appear in a recent discussion of scientific misconduct (Journal of Information Ethics 1994).

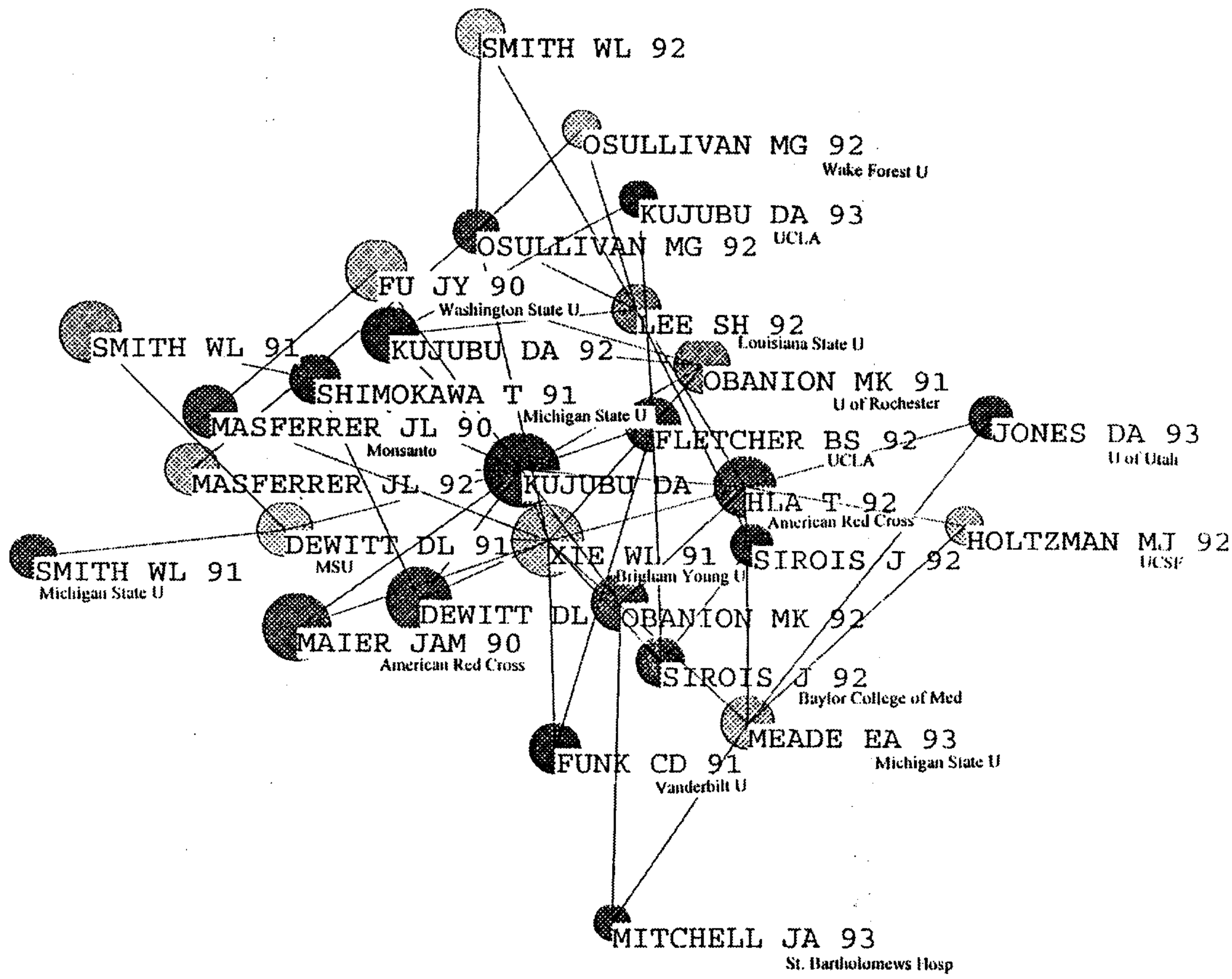
But the opposite may also be true. In several countries where research funding is often highly political, many of the most deserving researchers receive a small fraction of research funds in contrast to parasites who hadn't published a paper for a decade or more. Many well-funded clinical researchers publish in obscure national journals in the local language to hide their lack of international significance. In contrast, younger researchers not only publish in the international journals but are also well cited. Their impact on their scientific fields becomes clearly visible through citation analysis.

Old boy network

In science, as in other areas of life, "awards" and elections to academies are usually made by committees, sometimes described as "old boy networks". Unless they are regularly refreshed with new members, they tend to rely on biased human memory in making their selections. Such fossilized groups rarely ask for outside nominations or subject their choices to informed confirmation. The routine use of citation analysis in the award process can ameliorate such situations and should include consideration of appropriate cohort candidates for the innumerable awards in science. The routine use of citation analysis should uncover those individuals who have been inadvertently or otherwise overlooked in the nomination process. And, in some cases, where certain pioneers are selected late for awards, the citation history should demonstrate that their basic work was premature (Stent 1972) – that many years had elapsed before it was widely recognised.

It is often asserted that citation analysis does not adequately recognise small fields. It is rare indeed for a Nobel Prize or a Lasker, Wolf, or Gairdner award to be given in fields so small that the citation impact of the candidates is not above average or otherwise recognisable. To avoid injustice to smaller fields, the *SCI* files should be sub-divided or categorised as was the case, for example, in radio astronomy (Garfield 1980, Garfield & Welljams-Dorof 1992). Papers and authors

Figure 3: SCIMAP of Prostaglandin Synthase



with above-average impact will stand out from others in the cohort. Arno Penzias was not among the 1,000 most-cited scientists but he was among the most-cited radio astronomers.

Identifying research fronts

To use citation data properly, one needs a procedure for identifying the fields, especially small fields. From the earliest days of our work in indexing and classification, it was recognised that the process of field identification is highly problematic. With the pioneering work in 1973 of Small, and Marshakova (Marshakova 1973), we entered the era of algorithmic classification. The use of co-citation analysis for the identification of research fronts made it possible to systematically identify small and large fields. While a research front can emerge from a single seminal work, normally two or more core papers are involved in the identification of new fronts.

While co-citation analysis has been used systematically by ISI to identify *en masse* thousands of research fronts each year, a similar procedure called co-citation mapping can also be used to create *ad hoc* clusters maps. In short, one can construct the map of core papers for any specialty by establishing the citation linkages between groups of papers associated with any individual or group under consideration. Indeed, it might be argued that unless one has actually or implicitly created the map of an individual's specialty, one cannot say with assurance whose role was primordial. Such maps can be created to cover short or long periods. It would be an anomalous situation if a deserving scholar's work did not to turn up as one of the key nodes on the map of his or her field.

Indeed, such key links need not be based on high citation frequency. As Sher, Torpie, and the author have demonstrated over 30 years ago, careful citation mapping leads to the uncovering of small but important historical links overlooked by even the most diligent scholars (Garfield et al. 1964). Henry Small's SciMap software is now routinely used to create these maps for small databases extracted from the ISI indicators files for 1981–95.

Uncitedness

A frequent topic, given prominence in 1990 by David P. Hamilton a reporter for *Science* (Hamilton 1990), is that of uncitedness; that is, failure of publications to be cited at all, or rarely. The truth is that we know too little about uncitedness. Hamilton garbled some unpublished data he was given without recognising the details to be worked out. He used these data to support his preconceived notions about the alleged lack of utility of large areas of scholarly research. Pendlebury published a note in *Science* which attempted to correct the false impression created (Pendlebury 1991), but like so many other published errors, Hamilton's report continues to be cited while Pendlebury's "correction" is mainly overlooked.

Regardless of what is done about the selective funding of research, there will always be skewed distribution in citation frequency. As shown in Table 1, the vast majority of published papers will always remain infrequently cited. These Pareto- or Lotka-type distributions of citation "wealth" are inherent in the communication process so well described by Derek J. deSolla Price (1986). As a result, there are inevitable discontinuities in scholarship. A small percentage of the large mass of poorly cited material may include some work that can be described as "premature" in the sense of being valid and important but not recognised (Stent 1972). It should be possible to systematically re-examine such uncited works even decades later. Presumably some have served as useful stepping stones in the evolution of a particular field. Editors can easily identify such papers and reconsider them in light of changes in the field. While it would be a daunting task to re-evaluate all previously published manuscripts for this purpose, serious thought should be given to this task. What would happen to these papers were they resubmitted in light of the changes in the past decade or two?

Table 1: Citation Frequency Distribution of Papers in the SCI, 1945–1988

A	B	C
> 10,000	20	*
5,000–9,999	47	*
4,000–4,999	23	*
3,000–3,999	54	*
2,000–2,999	181	*
1,000–1,999	1,051	*
900–999	325	*
800–899	438	*
700–799	727	*
600–699	1,073	*
500–599	1,828	*
400–499	3,406	0.01
300–399	7,736	0.02
200–299	21,952	0.07
100–199	112,299	0.34
50–99	348,537	1.06
25–49	842,950	2.58
15–24	1,089,731	3.33
10–14	1,207,577	3.69
5–9	2,955,984	9.03
2–4	7,877,213	24.07
1	18,255,577	55.78
Total	32,728,729	100.00

A = number of citations
B = number of items receiving that number of citations
C = percent of entire SCI file
* = less than 0.01 percent of the SCI file, 1945–1988

More than likely, many uncited papers involve supersedure. Publication of research is a cumulative process. Each new laboratory report by established investigators builds on and/or supersedes their own earlier work. As the work progresses, it is not necessary to cite all the earlier reports. It is not unusual to observe that after a decade of research, the entire corpus is superseded by a "review" which is preferentially cited by subsequent investigators. Hamilton also ignored the more prominent fact about uncitedness; among the leading research journals of the world uncitedness is, to all intents and purposes, nonexistent.

The following tables (see Table 2–4) provide data on cumulative citation counts for the average paper in each of the thirty-three most influential scientific journals. For each journal we

Table 2: Cumulative Impact of 1981 Articles and Uncitedness

Rank	Journal	# Items	1981 Articles Avg Cum Cites 1981–93 (All)	1981 Cited Items (Cited)	1981 % Uncited	1981 Total Cites
1	Cell	393	126	126	0	49,307
2	NEJM	378	116	117	1.06	43,784
3	J Exp Med	343	89	90	0.29	30,630
4	PNAS (Biol)	1550	86	86	0.13	133,135
5	J Cell Biol	367	81	81	0	29,629
6	Arch Gen Psych	152	79	80	1.31	11,970
7	J Clin Invest	418	77	78	0.48	32,226
8	Nature	1375	71	73	2.76	96,881
9	J Neurosci	106	70	70	0	7,432
10	Science	1077	61	64	4.36	65,831
11	J Mol Biol	307	61	61	0.99	18,629
12	J Immunol	989	55	55	0.2	54,380
13	Circulation	416	54	55	1.92	22,601
14	Circulation Res	267	54	54	0	14,439
15	Ann Int Med	290	54	55	2.1	15,528
16	Blood	360	53	53	0	18,983
17	Lancet	641[a]	52	69	25.6	33056
18	JBC	2220	49	49	0.59	108,107
19	Gastroenterology	325	47	48	1.23	15,408
20	Mol Cell Biol	122	47	47	0.81	5,713
21	Phys Rev L	992	43	43	0.4	42,463
22	Syst Zool	34	43	44	2.86	1,455
23	Am J Pathol	167	42	43	1.19	7,053
24	Eur J Immunol	171	42	43	2.33	7,156
25	Cancer Res	851	37	37	0.59	31,245
26	Ann Neurol	222	37	38	2.69	8,131
27	Lab Invest	139	36	36	0.71	4,952
28	J Virology	483	32	33	0.83	15,558
29	J Natl Canc I	306	31	31	0.65	9375
30	Arthritis Rheum	204	27	28	2.44	5,577
31	Am J Hum Genet	78	24	25	1.27	1,896
32	Angew Chem	413	21	22	2.66	8,664
33	JAMA	551	21	23	9.2	11,382

a – Includes „Notes"

have determined the percentage of uncited papers. As we see, well over 90–95% of this literature is well cited. The cumulative citation frequency for these journals is indeed startling. By definition, as one reaches down into the many smaller and lower impact journals, the percentage of uncitedness increases.

Other important facets of low-frequency citation would be the consequence of obliteration by incorporation (OBI) into review articles (Merton 1968, Garfield 1975). There are hundreds of review journals which incorporate into their coverage thousands of stepping-stone papers which form the building blocks of scientific knowledge. Consider, for example, that the *Annual Review of Biochemistry* published approximately 250 papers over the decade 1981–90. These reviews contained about 40,000 cited references, but more importantly, each review itself has been cited on average in over 300 subsequent papers. This is detailed in Table 3. Not all review journals are cited this often as shown in Table 4. These data must be updated each year by using ISI's *Journal Performance Indicators*.

After the 1979 publication of *Citation Indexing* (Garfield 1979), an excellent review of the literature was published in 1981 by Linda Smith (Smith 1981). Then in 1984 Blaise Cronin published *The Citation Process*, an excellent account of citation behaviour and related issues (Cronin 1984). Sub-

Table 3: Cumulative Impact for *Annual Review of Biochemistry*

Year	Cumulative impact per paper
1981	361
1982	273
1983	442
1984	425
1985	373
1986	361
1987	650
1988	333
1989	265
1990	166
1991	197

sequently, White and McCain (White & McCain 1989), and Cozzens (Cozzens 1989) provided additional reviews. However, it is important to state that while a putative theory of citation is implied by the many works published to date, there is no complete theory that accounts for the myriad uses that can occur.

Conclusions

It is commonplace to speak about the isolation of the cultures of science and social science. Most practising scientists seem completely oblivious to the large literature of citation and bibliometric studies. This synoptic review has only touched the highlights. From the perspective of the social scientist or humanities scholar, the failure to include monographs as sources in the ISI citation indexes may be a drawback in making conclusions about the impact of certain work (Biddle 1996). Nevertheless, the inclusion of books as cited references in ISI's citation indexes has permitted studies of most-cited books to be accepted as reasonable surrogates for more comprehensive studies that might have included books as sources. Undoubtedly, the creation of a Book Citation Index is a major challenge for the future and would be an expected by-product of the new electronic media with hypertext capability!

Future citation index databases will include the all-author feature so frequently missed by those who use the present generation of citation indexes. This capability is already built into the ISI Research Indicators database and the *Web of Science*. Back in 1963 Michael Kessler introduced the notion of bibliographic coupling (Kessler 1963), that is, retrieval of related papers, by ex-

Table 4: Cumulative Impact for 1981–6 Review Journals Articles from 1981–94

Journal	6-Year Average	Cites	Source
Ann R Bioch	320	60219	188
Ann R Cell	231	7866	34
Adv Protein	205	4311	21
Ann R Immun	195	16929	87
Physiol Rev	177	22066	125
Rev M Phys	168	21821	130
Adv Immunol	159	6686	42
Microbiol R	145	18015	124
Ann R Neur	142	13877	98
Pharm Rev	138	10646	77
Ann R Plant	138	17139	124
Ann R Genet	132	13199	100
Endocr Rev	129	17731	138
Adv Physics	109	6318	58
Ann R Pharm	108	15493	144
Adv Carb C	97	3388	35
Ann R Astro	94	8782	93
Adv O Rg M et	91	3534	39
Chem Rev	91	13853	153
Brain Res R	90	9602	107
Ann R Ph Ch	86	10277	120
Immunol Rev	80	22753	284
Q Rev Bioph	77	3946	51
Rev Phys B	77	3847	50
Ann R Physl	75	20722	275
Prog Nucl	74	2373	32
Psychol Rev	74	11646	158
Crc C R Bi	73	7754	107
An N R Eco L	70	8239	117
Rec Prog H	70	4273	61
Ann R Micro	69	9829	143
Acc Chem Re	68	23887	349
Ann R Bioph	65	7148	110
Adv Enzym	57	2963	52
Epidemiol Rev	53	3039	57
Phsy Report	52	24259	466
Progr Neurob	50	5789	115

amining the cited references they share in common. This capability awaited the implementation of large computer memories. It is now available in the form of "related records" in the *SCI* CD-ROM editions and the *Web of Science*. This feature enables researchers to navigate (hyperlink) from one document to another in real time. Combined with access to full texts of papers online in the near future, the navigational and retrieval capabilities of citation links will finally come into full bloom. Citation analysts will then have at their disposal citations in context so that quantitative data can be augmented in real time with qualitative statements about the works being cited.

Let me conclude by quoting a recent paper by Jeff Biddle (Biddle 1996) at Michigan State University:

> "Citation analysis is one potentially useful tool for the researcher interested in the history of twentieth-century economics and will be most useful when used in conjunction with the more traditional methods relied upon by historians.... [citation analysis can be compared to] playing the role of a fallible witness. The historian who relies only on the testimony of the citation record is risking serious error, but the historian who fails to make use of it may be bypassing a valuable source of information."

Over the years a number of studies have attempted to use citation analysis to prove that there is a sex bias in science. The culmination of these efforts, however, was the recent publication by Christine Wenneras and Agnes Wold of Gothenberg University in Sweden (Wenneras & World 1997). The subtitle in *Nature* states that it was "the first-ever analysis of peer-review scores for postdoctoral fellowship applications ... and the system is revealed as being riddled with prejudice." It further recommends that "The policy of secrecy in evaluation must be abandoned."

Further, in the November 13th issue of *Nature* (Sachs 1997), Frederick Sachs of SUNY Buffalo states that

> "The shortage of funds has its greatest effect on those who are not 'in', as shown by the ground-breaking study done in Sweden (Wenneras and Wold), showing that women needed vastly superior credentials, equivalent to two or three extra publications in *Nature* or *Science*, to have the same chance as men of getting a postdoctoral fellowship. The study showed that men who were not associated with members of the peer-review committee were also treated unfairly."

Does this sound like the Old Boys' Network?

I have taken a great deal of satisfaction in my life as a citation analyst, but I can think of no other study that has given me greater satisfaction, even though few of the commentators on this study mentioned that it was based on the use of citation data, with the exception of Alison Motluk, in the *New Scientist* for 24 May 1997. She correctly pointed out that Wenneras and Wold tested their hypothesis by using citation data and found "That women and men with the same publication impact were awarded vastly different scores for scientific competence by their peers" (Motluk 1997).

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