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PROBLEMS OF CITATION ANALYSIS

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Introduction

We have approached the subject of this paper – problems of citation analysis – in two ways. In the first section, we list the traditionally recognized problems of citation analysis and briefly summarize empirical research on each. In the second section, we contrast two views of science. One is the traditional view of science which explains why it was originally believed possible to use citation counts as evaluative data. We contrast this view with the constructivist view of science which explains paper writing and citing behavior quite differently from the traditional view and makes it highly unlikely that citations can be used as quality indicators.

Citation counts: problems

During the past thirty years, citations have become a primary analytical tool among sociologists of science and, more recently, among science administrators. The motive behind citation analysis is the evaluation of the performance of scientists.

"The starting point," says *Wade*,[1] "of all citation analysis studies is to count the number of times an article or author is cited in the scientific literature...on the general assumption that the number of citations reflects an article's influence, and therefore quality; this measure can be made to serve as the fundamental yardstick for quantifying many aspects of the cognitive and social structure of science." According to *Bavelas*,[2] the promise of an "objective alternative" to subjective evaluation is its greatest promise. "Citation analysis is concrete – not anecdotal;" it is an objective measure of quality.[3] Citation analysis rests on the assumption that authors cite their influences and that they do so because they are motivated to give credit where credit is due; that is, to acknowledge their influences. This is clearly testable.

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1) *Not citing influences*. While citation analysts claim that scientists cite their influences, we found that no citation analyst had ever performed the simple test of reading scientific papers and comparing the influence evident in the text with what was contained in the bibliography. In other words, while there was a clearly testable assumption upon which citation analysis was based, no one had ever tested it! Consequently, we read papers in fields that we know and recorded whether or not influences were cited. In one field, the history of genetics, we carefully studied fifteen randomly selected papers and found that from zero (paper had no footnotes or references) to 64 percent influence was captured in references and footnotes. That is, after reconstructing bibliographies, we estimated that these fifteen papers taken together required some 719 references at a minimum to cover the influence evident in them, when in fact they contain only 216 references. This is a coverage of 30 percent for the entire sample.[4] We have found that this figure – thirty percent – typifies all fields with which we are familiar, for example, botany, zoology, ethology, sociology, and psychology.

2) *Biased citing*. While it is clear from the above that authors cite only a fraction of their influences, it is possible that in the aggregate citing may be proportional to use. To test this possibility, we conducted another experiment. We selected 13 facts and traced them through 23 subsequent papers on the same subject. Knowing the history of these facts, we read papers and followed specific ideas that originated with specific individuals in specific papers through a series of later related papers and noted credit allocation. In the 23 papers, the 13 facts were used 93 times. Of these, 34 (37%) were correctly credited, 38 (41%) were not credited at all, and 21 (22%) were credited to a secondary source. Breaking these figures down further, 37 percent of the sample were correctly cited and 63 percent were not cited or were credited to a secondary source. But what is of particular interest here is the disproportional credit allocation among influences. Some influences were almost always credited correctly while others were either not credited or credited to someone else. The 13 facts ranged from never being credited to being credited 88 percent of the time used. Some research is cited essentially every time it is used while other research is never cited, even though it may be used more than the highly cited work. Thus, citing is highly biased.[5]

3) *Secondary sources preferred*. One of the findings of the above study is that secondary sources played a large role in citing. Of the 55 citations made, 21 (38%) were to secondary sources; that is, over one-third of the "credit" given was taken from the discoverer and given to someone who had nothing to do with the discovery. Review papers are the main source of these citations. That review papers receive a disproportional number of citations has been noted by others.[4–5]

4) *Informal influences not cited*. *Edge*[6] first brought attention to the fact that a great deal of scientific information is transmitted informally, not via publications, and

Collins[7] showed that informal interaction is a necessity to scientific work. Conferences, meetings, phone calls, letters, and so on, transmit information long before publication. In addition, publications do not contain even the same information that is contained in the informal interactions. But science has never evolved an adequate institutional means for crediting these informal sources. Clearly, the few pers. comms. and acknowledgments in published papers do not do this.[4] Nor do pers. comms. or acknowledgments get into the citation data base.[8] While there are no figures on the proportion of influence that is informal, *Edge*[6] analogizes the situation to that of an iceberg, with "formal communication" (published papers) representing the tip.

5) *Citer motivation*. Citation analysts originally assumed a relatively simple motive for citing: to credit previous influences. *Brooks*[9-10] questioned authors about their motivation for citing in their own papers and found that they cite for many reasons, giving credit being the least important. He found that about 70 percent of references were multiply motivated. *Brooks*[9] concluded: "No longer can we naively assume that authors cite only noteworthy pieces in a positive manner. Authors are revealed to be advocates of their own points of view who utilize previous literature in a calculated attempt to self-justify."

Liu[11] provides an excellent detailed review of context/contents/ motivation analysis, the summary of which states that citing – and not citing[12] – is a complex social-psychological behavior that cannot be overlooked if citations are to be used as data. In other words, authors are not simply citing their influences.

6) *Variation in citation rates with disciplines, nationality, time period, and size and type of speciality*. There is a great deal of information on these subjects and we do not have space here to summarize it. It is known that different specialities have different citation rates, the same specialities in different countries have different citation rates, different types of papers have different citation rates, and so on.[13-14] Particularly interesting here is a recent review by *Peritz*.[15] It points to the complexities of research design if citations are to be used as data: basically, every individual would have to be assigned a handicap or normalization factor.

7) *Self-citation*. While it is agreed that self-citing is excessive, the reasons why authors do so are not clear. *Bonzi & Snyder*[16] found that the motivation for self-citing was basically the same as for citation of others. But what is important here is that it is not clear how to handle self-citing within the framework of citations as evaluators.[13]

8) *Size of audience*. One implicit assumption that must be accepted for citation analysis to work is that all papers have the same probability of being cited, the same potential citing audience. Clearly a moments reflection will show that papers are not equally citable, not because they are of unequal quality but because of differences in the size of their audiences. To determine if a paper (note, editorial, report, review,

book) has received the proper number of citations, one would first have to know its potential audience. This is never known. Again, the normalization issue must be addressed.

9) *Traditional non-citing*. *Tainer*[17] recognized that there are some areas of research that simply do not get cited, even though the work is used. Protein crystallography is apparently one such area. Papers in it are not frequently cited, but information contained in them is used by researchers in the field. Botany is also notorious in this regard. Floras, while indispensable, are seldom cited. Nor are range extension records often cited. For such fields, citation analysts are faced with an unresolved problem: determine if a person works in a "citation field" or a "non-citation field."

10) *Ignorance of the literature*. In order for citation analysis to work, authors would have to know the literature. Research on the reading habits of scientists is not extensive, but wherever it has been conducted indications are that scientists do not burn the midnight oil. All evidence suggests that scientists seldom know the scientific literature at all well and that what they do know generally comes from secondary sources (review papers) and informal communications.[4] It is highly possible that authors would be more familiar with the primary "first-tier journals" than the secondary journals. If so, this makes scientific ignorance even more profound since it would indicate that researchers feel no need to consult anything but a few major journals.

11) *Data biased*. The SCI and SSCI monitor the "top 10% of the scientific journals.[18-19] But what does "top 10%" mean? Who selected these journals and on what grounds? If one wanted to sample scientific journals to see who was contributing to science, would one select the "Top 10%" or a random sample? Unexplained sampling methods are involved here, which, to our knowledge, have not been addressed.[13]

12) *Technical problems*. The problem of multiple authorship is repeatedly raised, especially in those disciplines in which papers regularly have ten to thirty authors. Additionally, there are problems with homonyms and clerical errors. Most recent estimates indicate that "it is not unreasonable to assume that 20% of the records in ISI's citation indexes are erroneous.[8] All citation analysts recognize these problems, but there is no consensus on how to handle them.

One paper: two philosophies

Behind citation counting is a philosophy of science. In this section, we briefly outline that philosophy – traditional scientific view – and contrast it with another – social constructivism. In these two philosophies, citations are viewed very differently. Accept the traditional account and citations are unproblematically accessible as data for

evaluative purposes. Accept social constructivism and citations are not unproblematically accessible as data for evaluative purposes.

Traditional scientific paradigm. The assumptions of citation analysis can only be understood against the background of the traditional view of science. This view goes back to Francis Bacon. According to it, natural reality (the one real world out there) explains scientists' beliefs.

Science consists of a unique set of institutional arrangements, behavioral norms, and methods that enable it to exist and function effectively. Ultimate answers are Nature's; man is only a mediator and passive observer. Scientists combine the methods of science with objectivity, disinterestedness, humility, universality, and skepticism, and stand back to let Nature tell her tale unimpeded;[20] they are trained to perform the task of objectively observing Nature "as she is." Scientific disputes are settled on the basis of evidence and rational discussion. The content of science is independent of personal and social forces and is, therefore, outside the realm of sociology and psychology.

In this view, the scientific paper is value free. Nature writes papers, not human beings. The scientific paper is a blueprint of what is "out there", including the contributions of other scientists whose work has influenced its development. The scientist's purpose is plain and simple: to convey information. Paper writing is unproblematic: it is objective and rational. Likewise, citer motive is unproblematic. Citing is free of personal bias and social persuasion. Citations are made for chaste reasons. The scientist indifferently builds on the shoulders of predecessors – giants and dwarfs alike – and accurately portrays their contributions in his citations.

The early sociologists of science accepted this view of science and consequently did not study the content of science but only its structural features, for example, the reward system and scientific stratification.[21] It is within this framework that, in the early 1970's, citations were first used to study ranking among scientists and to determine whether the research of average scientists contributes to scientific advancement and whether science is a meritrocacy.[22–23]

Social constructivism. During the 1970's, stimulated by the views of Thomas Kuhn, Ludwig Wittgenstein, and others, a number of scientists made the unusual step of going straight to the center of science to study its knowledge claims.[24–25] This new breed –all of whom were trained as scientists and knew science first hand – found that traditional claims about science were false.[25–26] The "storybook" scientist – the objective, disinterested, humble, universal, and skeptical seeker of truth – simply does not exist.[27–29] Science was found to be subjective, contingent, social, and historical.[30] It is not independent of personal and social forces, nor are its disputes settled entirely by evidence and rational discussion.[31] Science is not disembodied interaction between non-social automata and a single independent world but is closely enmeshed with

prevailing cultural history and beliefs.[32] These researchers discovered that scientific history is constantly being rewritten, sanitized, and fictionalized to make it appear autonomous and rational.[33-34] Scientific knowledge is socially negotiated, not given by nature.[31] They found that fact construction is a collective process: "great men" do not build science; it is built by teams of hundreds and thousands, even though scientific historians, the media, and scientists themselves emphasize the heroic feats of individuals.[35-36] Scientists' beliefs explain natural reality, natural reality does not explain scientists' beliefs.[33] Scientific theories are underdetermined by evidence and observation is theory-laden.[30] Closure of scientific disputes is not the simple result of one set of results matching nature and thus being "right" and the other side's data not matching nature and thus being "wrong" but instead is a complicated business involving consensual judgments and interpretations emerging from argument and negotiation.[31] Nature has ceased to have a capital "N"; in fact, even the idea of a single, orderly independent world so long a part of western scientific tradition is recognized to be a social construction. "Nature" emerges only when consensus is reached among scientists.[35] One of the main interests of the constructivists has been scientific communication, of which papers and citations are a part.[29,35,37,38] They found that informal interaction constituted the major form of coummunication in science,[6] and that when scientists write scientific papers, they reconstitute what they have done and rewrite scientific history.[34] Research papers are written as if the researcher had worked inductively. The author's personal involvement is suppressed; controversy is toned down and often eliminated altogether, findings are construed as logically situated in a rational scientific history, and the scientific method is portrayed as central.[38] Results and conclusions are represented as leading inevitably from methods, logic, and history, and as such, take on an appearance of objectivity: the account could be anyone's account. What happened at the lab bench is cleaned up, and nature is depicted as telling her story without intermediary.

As *Mulkay*[29] has pointed out, the formal language of the scientific paper is a language of certainty, mastery, and domination. It is a language that denies its social origins and its human limitations. It is a language that actively misleads non-scientists and, ultimately, misleads the scientists themselves about their own role in the creation of facts and theories. In their examination of scientific communication, the constructivists found that paper writing is basically a rewriting of recent history to give the discovered object its ontological priority and to deny the cultural nature of the enterprise.[36] To this creation, citations are added: the names of recognized and respected individuals are prominently displayed at traditonal places to persuade an audience.[39] The cumulative effect of citing more and more people who similarly agree with the author is to concretize the universality of the knowledge claim.[36] An author's

main objective is not to cite their influences but to present as authoritative an argument as possible.[10,11,39]

Latour[35] likens paper construction to Machiavellian or Byzantine politics. Authors are literally lining-up the literature (he could have said stacking the deck): "A given paper may be cited by others for completely different reasons in a manner far from its own interests. It may be cited without being read ... or to support a claim which is exactly the opposite of what its author intended; or for technical details so minute that they escaped their author's attention; or because of intentions attributed to the authors but not explicitly stated in the text ...."[35]

As *Latour*[35] further indicates, citations are not put in papers to indicate to others who has influenced the production of the work but to display the "black boxed" (established) knowledge. If one does not agree with a referenced statement, one must, in essence, dispute it with the cited definitive authority.

Papers are meant to sell a product, which is wrapped up with "the passionate pursuit of research grants and professional success."[40] The scientific paper as *Knorr-Cetina*, *Latour*, *Woolgar*, and many others[36,41] have demonstated does not reflect what happens at the lab bench nor, as others have shown, is it a blueprint, or is it even supposed to be a blueprint, of the author's intellectual history.[4-13] It is, rather, only the last in a series of often dozens of laborious and painful and continuously changing drafts in which authors and co-authors construct, reconstruct, and negotiate knowledge and in which outsiders, notably referees, colleagues, and editors, add their two-bits, often including references (usually their own) the author has never seen.

With these discoveries about the nature of science, the scientific paper itself becomes a part or phase of ongoing science, not its end point. Paper writing is but one act in the creative process to be followed by other published reworkings of the basic theme. Indeed, as *Gilbert* and *Mulkay*[38] found, not only do scientists' accounts vary between the lab, over beers at a bar, in letters, lab notes, conferences, and within a single recorded interview, but between successive published versions of the same data!

The formal paper presents a story, a nicé rational story, but not the story, and the citations present an array, but not the only array possible.

Given the results of this research, it appears very strange that this highly ritualized bit of the creative process - the scientific paper, which many years ago *Medawar*[42] called a "fraud" and more recently *Latour*[35] called a "fiction" and *Knorr-Cetina*[37] a "perversion"- has been so heavily used as data in philosophical, historical, and sociological accounts of science. And it is absurdly incongruous that citations, one of the most artful parts of this highly contrived production, have been chosen as "objective" data by which to measure the quality of a scientist's work and hence their worth.

Conclusion

While thousands of papers have been produced by hundreds of workers using citations as data, none of the problems has been exhaustively studied and some have been ignored almost entirely. This conclusion contrasts sharply with the repeated assurances of citation advocates that they recognize the limitations of the method, take the problems into account, have satisfactorily dealt with their critics, or have examined the problems and have found that error without correction is small or unimportant.[13,19] In fact, as we pointed out in an earlier review, there was a real dearth of papers critical of citation analysis; in other words, while the citation analysis literature has grown dramatically, there has been no proportional growth in papers critical of it.[13] Today, in spite of an overwhelming body of evidence to the contrary, citation analysts continue to accept the traditional view of science as a privileged enterprise free of cultural bias and self-interest and accordingly continue to treat citations as if they were culture free measures.

The basic assumptions of citation analysis are clearly false. The assumption "that the research cited by scientists in their own papers represents a roughly valid indicator of influence on their work"[22,43] has been falsified repeatedly, as has the assumption on which it is based: that scientists are motivated to cite their influences or to give credit where credit is due.

Neither of these assumptions is supported by the evidence. Rather, scientists do not cite most of their influences; citing is biased, secondary sources replace primary sources, and informal sources, which are the lion's share, are not credited. And authors definitely are not motivated to bestow credit. Unfortunately, none of the caveats about citation analysis is ever made known to the audience of the popular science literature.[3]

If one wants to know what influence has gone into a particular bit of research, there is only one way to proceed: head for the lab bench, stick close to the scientist as he works and interacts with colleagues, examine his lab notebooks, pay close attention to what he reads, and consider carefully his cultural milieu. The Nobel laureate Sir Peter *Medawar*[44] made this point years ago when he said: "It is no use looking to scientific 'papers', for they not merely conceal but actively misrepresent the reasoning that goes into the work they describe. Only unstudied evidence will do – and that means listening at the keyhole."

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