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Taking the Measure of Science: A Review of Citation Theories

I. Introduction

Since the creation of the Science Citation Index (SCI) in 1961 citation analysis has been used as a tool to study numerous aspects of the social structure of science. Garfield, Sher and Torpie (1964) originally proposed that references from recent scientific papers to older ones be used to study intellectual influences. Derek Price (1965) claimed that the fabric of new scientific knowledge was being "knit" through citations, and that social networks ("Invisible Colleges") accounted for networks of papers created by citation ties. Within a few years of its inception, the SCI had also been applied to the study of the stratification system of science. Indeed, a single well-known work on this topic used citations in five different ways, according to its subject index: as measures of diffusion, influence, quality, recognition and utilization (Cole and Cole, 1973).

For at least fifteen years, some sociologists have been issuing warnings that citation data should not be used in research until we understand them better. One well-known early warning came from Norman Kaplan (1965), who felt that "it is all too easy to make quite unwarranted inferences from (citation) analysis". Later, Michael Mulkay (1974) echoed the call for a deeper understanding of this sort of data: "... the use of citation patterns...as an index of lines of intellectual influence clearly involves an implicit theory of citing.... But in fact we know very little about who cites whom in science and why".

Despite these warnings, citation data have come to be more, not less, widely used. Perhaps in the early period of growth of the sociology of science the data were simply too readily available to be ignored, whether or not unproven assumptions were involved in their application. Certainly, as application has increased, the evidence linking citation data to other crucial features of scientific interaction has pointed toward, not away from, their relevance. For instance, citation counts have been found to be highly correlated with more direct measures of scientific achievement (Cole and Cole, 1973). Citation networks have been found to be associated with social networks (Mullins et al., 1977). Consensus has been found on the specific meaning associated with given references in scientific texts (Small and Greenlee, 1980). Histories based on quantitative citation analysis have been found to coincide rather uncannily with qualitative histories constructed from other sources (Sullivan et al., 1979). All the evidence points to the conclusion that citations are a patterned element of interaction among scientists. While the preferences of many sociologists of science lead them to ignore citations in their work, devotees of this kind of research are not on the defensive. Their attention has turned, in large part, to the fine points of applying the data.

In their rush to use citation data, sociologists have neglected the study of citations as a problem in its own right. My objective in this essay is to call attention to the theoretical issues involved in this problem. These issues are important for at least two reasons. First, there is a reifying tendency among citation analysts. They tend to view their measures as direct manifestations of certain social constructs, without visualizing at the same time the scientists who create the citation patterns. More attention to the theory of citing will bring those scientists back into the sociological consciousness. Second, citation analysis as a whole has been criticised by those concerned exclusively with problems of interpretation in social life. An examination of interpretive theories of citing will indicate whether certain applications of these data are possible within that framework.

Toward these ends, I review three major citation theories. This review is intended to be intensive rather than extensive, in two senses. First, I have made no attempt to review everything which has ever been published about citation practices. I have also excluded citation models which are purely mathematical, focusing instead on those which seem to carry the most sociological content. Second, I have not attempted to summarize the articles I will discuss, but have rather explicated their distinctive points. After reviewing those points, I briefly comment on the evidence associated with each position and the research problems it generates. Finally, I present an overview of the three orientations, emphasizing the problems they pose for each other.

II. The Normative Interpretation

The earliest attempt to explain citations sociologically was made by Norman Kaplan. His article, entitled "The Norms of Citation Behavior: Prolegomena to the Footnote," appeared in American Documentation in July, 1965. (Derek Price's "Networks of Scientific Papers" appeared in Science in the same month.) Kaplan's paper is a call for research, and an urgent one at that. On the one hand, he perceived that the SCI, then in its infancy, was about to revolutionize the citation practices of scientists. He wanted sociologists to gather baseline data against which to assess that revolution. At the same time, the application of citation data for evaluative purposes had already begun, and it was spreading fast. Kaplan saw this application as based more on availability than on a genuine understanding of the significance of citations for citers, and he therefore urged immediate attention to the latter topic.

Kaplan's article relates the theory of citations to the central topic of discussion in the sociology of science in 1965, the normative structure of science. Merton (1973 [1942]:267-268) had claimed that the institutional goal of science was the extension of certified knowledge, achieved through a set of practices and attitudes, distinctive to scientists. In particular, he was interested in those general moral imperatives or "norms" which seemed to set science apart from other social institutions. One of these imperatives, according to Merton (1973 [1942]:273), is "communism," the institutional assumption that "the substantive findings of science are a product of social collaboration and are assigned to the community". Kaplan claims that the "rules of the game" concerning citations are a corollary to "communism"; in brief: if you draw on the work of another community member in the course of your work, give credit to that member by citing the work you use.

This formulation of the connection between the Mertonian norms and the practice of referencing had a particular appeal in the era of exchange theories. Both Hagstrom (1965) and Storer (1966) had attempted to articulate the set of mechanisms by which the Mertonian norms could maintain their vitality in ongoing interaction. Kaplan's account of citation rules seemed to describe such a mechanism. Scientists want recognition for their original contributions; this was one of Meron's basic insights. But when they publish their ideas, thus making them available for common use, they take the risk that someone else will use them and claim credit. If an informal agreement exists among scientists that when using an idea they will attach the name of its inventor to it, then the risk is considerably lessened. In addition, the act of citing reinforces the citer's feeling of indebtedness to his or her colleagues. The community of cited and citing authors is thus bound together socially by the exchange, and every reference revitalized the norm of communism.

If the reference serves as one of the community's ways of distributing recognition, then it must be subject to another of the Mertonian norms, universalism, which calls on scientists to judge each other's work on "scientific", not personal or social, grounds. Kaplan seems skeptical of the assumption that scientists would actually be so rationalistic with their footnotes. He suggests that they may actually overcite themselves and their colleagues. Kaplan also recognizes that a perfunctory citation to an unpublished work which one is "scooping" hardly "repays an intellectual debt", and that while reference lists are short, lists of the intellectual influences on scientists are long. The research plan Kaplan suggests largely involves exploring the patterned sources of departures from universalism in references. That is, he asks why credit is not always given where it is due.

Kaplan does not deny that references have functions other than reinforcing communism. After all, a good functional analyst recognizes that a given structure may have different functions for various social units. For instance, Kaplan mentions that citations play a role in scientific communication. He also notes that they can confer "intellectual and scientific respectability on the (citing) paper". Furthermore, they "enhance the visibility of certain papers and this takes on added significance when self-citations or even citations of one's immediate colleagues are involved". Finally, he argues that just as the scientific research report is not a narrative of the research process, the citations included within it are unlikely to represent a true intellectual history of the work reported. "In the case of the paper itself, it may be suspected that the reconstruction of the actual events is considered less important than the adherence to the conventional writing style of the scientific paper. Is this also true of the citation?" (Kaplan, 1965:182). Kaplan does not let his own explanation for the existence of citations, however, rest on any of these secondary functions. For Kaplan, the central function of the reference is not directly observable in the citing act. Its ultimate significance lies in its role in a larger system of social control.

Both Merton (1973 [1942]:267-268) and Garfinkel (1967) have suggested that the existence of many rules of behavior is most easily observable when they are violated. If this is so, then controversies over the etiquette of citations may be taken as an indication that such an etiquette exists. Latour and Woolgar (1979) report on such a controversy in neuroendocrinology, one which was also covered as a news item in <u>Science</u> (Wade, 1978). I have found that a similar controversy exists in opiate receptor research. In this case, some members of the research community have made informal attepts to "heal over" the wound created by undercitation, using letters to the aggrieved party to express their recognition of achievement. In both cases, [redacted] priority disputes are involved, an observation which is consistent with Kaplan's thesis on the linkage between citations and the reward system.

Such anecdotal evidence can be marshalled in support of Kaplan's argument, but it cannot be used effectively against it. The real test of the argument would come from a more systematic and detailed examination of two kinds of informal interaction among scientists: socialization into citation practices and the conduct of citation controversies. Kaplan searched for explicit instructions to citers and could find none. He suggested that citation practices were probably passed on in an oral tradition. This may be true, although given the often-lamented state of training in scientific writing, it is also possible that no explicit instructions are ever given. Rather one may learn by exemplar, absorbing citation patterns in the papers one reads. If this is the case, or even if minor instructions are given, the "norm" is by and large implicit and is only occasionally removed from the realm of the taken-for-granted. If sociologists were to try either to support or negate Kaplan's argument, they would need to turn to those occasions for evidence. When do scientists discuss citations? When do they issue "instructions" to each other on them? When do they argue about them? Who is involved in the discussions? What sorts of things do they say? When do these discussions actually make a difference in what citations appear in print?

III. The Interpretive Account

Although they spring from different theoretical traditions, the approaches of Norman Kaplan and G. Nigel Gilbert (1977) have more in common than might appear at first glance. Gilbert reiterates Kaplan's lament that citations have been used empirically without benefit of a theory of citation. He expresses his respect for Kaplan's "pioneering work", but dismisses it on semantic grounds: Kaplan had labelled citations "a social device for coping with problems of property rights and priority claims". Gilbert points out disanalogies between this use of the term property and its use in economics. He thereby avoids a serious discussion of Kaplan's normative position, which can be stated (as above) without the use of this term. In fact, Kaplan and Gilbert both utilize Mertonian theory in their arguments, accounting for the origin of scientific papers in terms of a system which rewards priority. Gilbert puts it thus: "A scientist is rewarded through recognition for producing results which are seen as new, important and true". As I noted above, Kaplan gave consideration to the role of the reference in scientific texts, but did not develop this theme. Gilbert focuses his attention precisely on this role. Whether results are "new, important and true" is not self evident to the readers of a research paper, he claims. Instead, they must be convinced of these points, and that "convincing" is what the scientific paper is all about. Every reference contributes to the work of convincing (Gilbert, 1977:116):

> However, not all the relevant articles, which might be cited are equally valuable in providing such support... The participants in a mature field will share a belief that some published work is important and correct, some other work is trivial, perhaps some is erroneous, and much is irrelevant to their current interests. Hence authors preparing papers will tend to cite the 'important and correct' papers, may cite 'erroneous' papers in order to challenge them and will avoid citing the 'trivial', and 'irrelevant' ones. Indeed, respected papers may be cited in order to shine in their reflected glory even if they do not seem closely related to the substantive content of the report.

This argument, of course, is very similar to the claim that articles become highly cited because they are judged to be of high quality. Perhaps this argument could be read as being too atomistic: the individual scientist makes a judgement on quality independently of other scientists, then mechanistically reflects that judgement in his citations. Gilbert's argument refines this simplistic account in two ways: Firstly, it is a community of scientists, not the individual, which comes to a consensus on "importance". Second, this original consensus produces the first few citations to an article, after which a process of cumulative advantage sets in, driven by the application of the recognized paper in persuasive efforts. Gilbert thus relates his explanation to "The Matthew Effect" (Merton, 1973 [1968]:439-459). It could also be said to justify Derek Price's (1976:305) claim that in citations "success breeds success":

> In this theory [the general theory of cumulative advantage], it would appear that the course of future citation successes is determined statistically by the past history of the cited paper; and so one is driven to suppose that citations are generated by a pull mechanism from previous citation rather than from a push mechanism of the papers that do the citing.

Even a mathematical model of citations, however, cannot do without a push mechanism, since there must be some way of accounting for the large majority of papers which are cited only once. Gilbert's argument clearly suffers from lack of such a mechanism. Most citations are not to "respected papers", that is, those which either are or will be highly-cited. Most references are therefore idiosyncratic. Gilbert's discussion suggests many reasons why papers would be cited idiosyncratically, but at points in his text, he seems to negate their importance. For instance, in the quote above he claims that authors would "avoid" citing such documents. Later he claims that "authors will tend whenever possible to cite papers which they consider their audience will regard as presenting valid and important arguments and results" (Gilbert, 1977:118; emphasis added). Indeed, if Gilbert's title is indicative of his position, he sees referencing primarily as persuasion, and only secondarily as anything else.

Gilbert's argument has strong appeal in accounting for what I call "knee-jerk references". Certain very highly-cited documents, often quite old, seem to be cited without thought, as a matter of custom. The classic example of such a reference is O.H. Lowry's 1951 paper on protein concentration, which was cited over 10,000 times in 1979. The paper's citations have increased in every annual Science Citation Index. It is this long-term pattern of citation to Lowry's paper which makes it puzzling, since many papers which reach high levels of citation become subject to "obliteration by incorporation" (Merton, 1968:27-29, 35-38). Gilbert (1977:117) discusses this latter phenomenon in his paper:

> In some cases...these exemplary papers may become so widely known and accepted through the field that they no longer need to be cited explicitly. Their contents become a part of that which every competent member of the field can be assumed to know.

The question, of course, is why was Lowry not obliterated? Using Gilbert's scheme, we might hypothesize that the most persuasive references are not subject to this pattern; but then we must posit a grounds for "persuasiveness". Why is Lowry persuasive when the plate tectonics discovery papers, for instance, are not? (See Messeri, 1978 for the discussion of their obliteration.) Why are the highly-cited papers in biochemistry generally methodological, when in entomology they are more often conceptual? In other words, what kinds of success breed success?

If Gilbert's arguments are correct, then we would expect to find that the most perfunctory sorts of references, those which seem least "closely related to the substantive content of the report", would be to the most highly-cited documents, on the average. When less highly-cited documents are used, one would expect them to play a more integral role in the argument. These propositions could be tested using citation context typologies developed by several researchers. Indeed, Chubin and Moitra (1975:435) seem to provide some support for this idea. Gilbert, however, eschews confirmation from these sources, on the grounds that such context analysis is unlikely to categorize references in the same way they would be grouped by the audience to which they were actually directed. The persuasive context, Gilbert claims, is implicit rather than explicit.

Gilbert does not eschew empirical support, however, from another sort of quantitative citation analysis, co-citation clustering.* In fact, he claims to have provided the theoretical explanation for why clustering works (Gilbert, 1977:118-119):

> Their [Small and Griffith's] technique is successful because authors, in choosing references (and thus co-citation pairs) orient to their own perceptions of how the scientific community and its knowledge is structured. They place their work within a field by citing research which their intended audience values. Thus the co-citation analysis reveals the specialty structure by jointly tapping the individual perceptions of all the authors whose work has been examined.

An alternative view of the referencing act has been offered by one of the "discoverers" of co-citation clustering. I turn now to that account.

IV. The Symbolic Perspective

Kaplan's viewpoint was institutional. Gilbert focused on interaction in small groups. Henry Small's (1978) approach to citations can be seen as a retreat to an even more elementary aspect of citation practice. In "Cited Documents as Concept Symbols", he rivets our attention on a point which is so obvious that it was overlooked by previous writers: "The footnote number has the function of pointing to a portion of the text in which it is embedded and at the same time corresponding to a specific document usually given at the bottom of the page or grouped at the end of the article" (Small, 1978:328). Gilbert had explicated the functions of the cited document for its associated text. Small points out that the reference number also works in the other direction. By associating a portion of the text with a cited document, the writer imparts a meaning to the document. Referencing is therefore a process of transforming published documents into symbols.

According to Small, "Most citations are the author's own private symbols for certain ideas he uses". Small thus accounts for items cited only once or twice. Other citations, however, are "standard symbols", having the same meaning for a community or group of scientists. Small demonstrates the existence of such standard symbols with a set of very highly-cited papers from chemistry. He examined the sentences surrounding the reference number in the text and calculated the percent of these contexts which used the same words to refer to the documents. For the highly-cited chemistry documents, the overall "percent uniformity" was 87%. In later work, Small applied this kind of citation context analysis to cited documents in co-citation clusters (Small and Greenlee, 1980) and even to the links between them (Small, 1979), thus providing a detailed description of the intellectual content of the cluster, from the viewpoint of those who created it.

These analyses suggest a different interpretation of co-citation clusters than the one given by Gilbert. Rather than being sets of "valued" documents, the clusters can be seen as sets of symbols. While the context analysis provides the specific meanings attached to documents or links within the clusters, it is also important to ask what the grouping as a whole symbolizes. Does the set of clustered documents stand for a fact, a problem, or for a social identity? Small compares the symbol-making process of citation to Durkheim's notion of "collective representations", and thus suggests that clusters stand for social identities.

The meanings which are imparted to a document are not necessarily the ones the author intended. For instance, a quick analysis of citations to Kaplan's paper, discussed above, reveals that it is cited most often as a critique of citation analysis. Gilbert's association of the paper with the concept of "references as property" is idiosyncratic, although it is probably closer to what Kaplan would have considered his main point than is the standard interpretation. Since the meaning is created by the community and not by the document, it can change over time. Longitudinal analysis of citation contexts should thus prove interesting. I have performed a preliminary analysis of this sort in opiate receptor research, tracing the fate of a "precursor" paper which was included in the co-citation cluster on this topic. The precursor, published three years before the discovery papers, was not highly-cited until after the discovery. In the original discovery papers, it was cited only for its experimental method and its negative results on the existence of the opiate receptor. After priority had been established, however, the paper began to receive flowery accolades, and it eventually became one of five papers which were used synonomously in conjunction with the phrase "the discovery of the opiate receptor".

With the exception of his reference to "colective representations", Small's work seems to move the theory of citations away from sociology, placing it at the doorstep of linguistics. He suggests that references are symbolic resources, embedded in the vocabulary and phrasing used to express scientific knowledge. The research agenda suggested by his work therefore calls for viewing references within the context of larger symbol sets. The examination of obliteration by incorporation becomes, in this view, not a study in "uncitedness", but a portrayal of the substitution of one symbol by another. Likewise, the symbolic approach suggests a new approach to "codification". Zuckerman and Merton (1973 [1972]:497-559) defined this term as "the consolidation of empirical results into succinct and interdependent theoretical formulations", and used it in its past participle form. It might also be used as a gerund, referring to the process of symbolic substitution by which references to research results are replaced by more general symbols. Other traditional research topics in the social history of science can probably also be translated into symbolic terms.

Overview and Discussion

Kaplan, Gilbert and Small have approached the phenomenon of citing from three analytical perspectives, but they have not created competing theories of

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* A co-citation cluster is a set of highly-cited documents which appear together frequently in the reference lists of more recent documents. See Garfield, Malin and Small (1978) for a review and discussion of the technique.

citation. In the course of the actual work of writing a paper, scientists' actions are consistent with all three perspectives. Of course, they use references to persuade each other of the importance of their results. And of course, they are following the general practice of "giving credit where credit is due", according to the current consensus within their reference groups. Instead of urging the adoption of one or another of these perspectives exclusively, therefore, I would like to point out ways that they might complement each other in setting research agendas.

Just as the strength of the normative approach is its broad, institutional viewpoint, its weakness is that it fails to specify just how it is that norms affect everyday interaction. The term norm will not really have a meaning for microsociologists until the mechanisms involved in "orienting action toward" a given norm can be observed and described. This will require participant observation. Earlier, I mentioned two areas to which I thought attention should be directed: socialization into a referencing pattern and controversies over citations. An even more exciting area is the study of the broader pattern to which citation rules contribute, the norm of communism. I think that an explicit comparison between science and other "communitarian" movements (as described for example by Zablocki, 1971) would be useful in this regard. What does the community ask the individual to give up? How does it induce participants to share? What are the payoffs of sharing? Does the demand to "give up the goods" cause some recruits to leave the organization? Merton's description of this norm is based, in part, on scientists' public accounts of their actions. Critics claim that Merton has adopted scientists' ideological defenses and presented them as sociological explanations. A serious field study, performed with sensitivity to these interpretative issues, might provide a different description of scientific communism, assuming it is present in the laboratory. Such studies would lead to a better understanding of citation norms.

Gilbert's approach to referencing, while it focuses on the formulation and deployment of symbolic resources, nonetheless includes a description of the structural conditions under which that process occurs. His more recent work (e.g. Gilbert and Mulkay, 1980:1), which he calls "discourse analysis", seems to be narrowing its focus to the symbolic act alone, on the assumption that the actions of scientists are not accessible to the sociologists except as the "variable context-dependent formulations which scientists produce". This approach is likely to lose the interest of structural sociologists unless it is developed on a comparative basis, for example, on discourse in different organizational settings or in different scientific fields. Discourse analysts may in fact find that accounting practices differ systematically in these various contexts. Whatever the results, comparative data will be illuminating from both interpretive and structural perspectives, in a way that discourse analysis to date has not been.

Finally, both sociological approaches challenge symbolists not to view language as a self-organizing system, but rather to keep citing actors firmly in view. By keeping in mind that scientists choose their symbols, however, we need not forget that those choices can have unanticipated consequences. Indeed, the symbolic approach to citations may provide insight into the role symbols play in shaping social units. Do different sorts of symbols have different effects on the process of consensus formation? A scientific problem may be expressed as a well-formulated model or simply as a set of puzzling research results. Is the community more likely to agree that a "solution" to the problem has been found if the former is the case? Take another example: citations to research results may be obliterated (that is, absorbed symbolically) more quickly than citations to other kinds of contributions. If some fields are more likely to cite research results than others, does the faster obliteration rate have an effect on competition in those fields, by making recognition a scarcer commodity? These are the sorts of questions which must be explored in order for the symbolic analysis of citations to have an impact on sociologists. There is a need for research into citation patterns that cuts across the traditional theoretical orientations of the sociology of science and involves cooperation among people with different methodological inclinations. Participant observation studies of the operation of norms, as well as quantitative and comparative studies of accounting practices and symbolic patterns, need to be carried out. Only by taking each other seriously in this way will our understanding of science expand rather than contract.

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