Also see: Introduction on p.232 and Reprint by B.C. Griffith, M.C. Drott, H.G.Small. on p.234, Essays, Vol:3

Why I Am Not a Co-Citationist*

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Some of us make modest use of citation analysis in our work,[1] but remain radically skeptical of the claims of those who devote more prime time and energy to the elaboration of such methods. Why do we not accept the faith? Why can we not do the proper Kuhnian thing and let the "paradigmatic achievements" of the new quantitative methods define the field for us—posing our fundamental problems, laying down agreed techniques, prefiguring acceptable answers, and unrolling a "progressive research program"? I suggest that what is at issue here is essentially a *difference of aim.* My conception of "doing the sociology of science" allows citation analysis, at best, only a very peripheral role. I will try to outline my position as succinctly as possible.

1) Let me first identify and reject a claim that seems to me to lurk, if only implicitly, behind these quantitative methods: essentially, the claim is that, in transcending the "limited, subjective and biased" perspective of individuals, and in giving some "public, aggregated, objective and unbiased" account, these measures have, as it were, "a preferred logical status". They are more "objective", more "reliable"; they can be used to "*correct*" participants' accounts; they can define "what really is (or was) the case", and can *arbitrate* between conflicting accounts; and so on. These quantitative procedures are often labelled "scientific", and the sociology (or history) to which they give rise is "scientific sociology"—as opposed, presumably, to qualitative, individualistic and "biased", "incomplete" sociology. Garfield, Sher and Torpie, for instance, in their pioneer 1964 paper, state:

> The writing of history is subject to much human error in spite of the dedication and relatively rigorous standards held by the professional historian.... Historical description must therefore fall far short of an ideal. We can only strive to develop methods that bring us somewhat closer to the truth.... The historian, in describing the progress of science, is limited by his own experience, memory, and the adequacy of the documentation available. His subjective judgment primarily determines the

*Reprinted from: *Society for Social Studies of Science Newsletter* 2:13-19, Summer 1977.

historical picture of the development of events.[2]

And their paper concludes:

> It is felt that citation analysis has been demonstrated to be a valid and valuable means of creating accurate historical descriptions of scientific fields....[3]

Small, in his most recent paper, makes a similar claim for the preferred status of co-citation analysis in preparing specialty bibliographies:

> The principal difficulty...is that it is almost impossible to establish precise criteria as to what should or should not be included within the boundaries of the subject, and the temptation is to apply present-day criteria to earlier literature. [Co-citation analysis] uses a clustering algorithm to establish these boundaries; it involves no subjective decisions on what is to be included or excluded from the specialty literature.[4]

If this is so, then why bother to "validate" co-citation studies?[5] Differences between the co-citation results and those derived from other sources are only to be expected, and it is implicit in the method that preference, in such cases, should be given to the former over the latter. However, co-citation practitioners lose nerve at this point: not only do they undertake validations, but they allow *errors.* Small, for instance, says, of a paper which was missed by his computer in his study of collagen research:

> The effect of thresholding was, in this case, to exclude an important and relevant item.[6]

Was the decision that this item was "important and relevant" (and therefore, presumably, that it should be included in the specialty bibliography) a "subjective" or "objective" one?

When Griffith, Drott and Small state, with emphasis, that....

> ...the investigator should use all means at his disposal to determine the degree of consensus within the relevant community represented by a citation count and the nature of that consensus...,

they are abandoning the notion of a preferred logical status for citation methods. We would then be *agreed* that citation and co-citation figures are just part of the range of empirical data available to the historian and sociologist—no more, and no less. Where we would still *differ* would be over the relative *weight* to be assigned to citation measures, within the context of the entire range of information.

2) The advent of the co-citation technique has involved a fundamental shift of emphasis which I welcome. Previously, the citing of B by A was taken to reflect an *influence* of B on A. Crane, for instance, claims that:

> Within a research area, frequent citation indicates that a paper contains information that has been useful to other members.[7]

And Meadows lists "two basic assumptions" in citation analyses:

> (I) that the papers selected for citation are those which have been important in an investigation; and (II) that citations are indicative of influence via the literature.[8]

However, in co-citation analysis, which is superficially so similar, the only assumption is that the citing of B and C together by A implies that, *in A's perspective, the work of B and of C are related.* In other words, co-citation "maps", strictly speaking, reflect only the *perceptions of authors.* But those using co-citation analysis act as if co-citations of B and C were evidence that *B and C are related by communication ties,* and the authors "clustered" by co-citation are *intereacting groups.* Small and Griffith, for instance, in one of the pioneer co-citation papers, talk of "the mapping of specialties, to show their internal structures and their relationships to one another", and they continue (reiterating a version of the preferred logical status claim):

> Many of the relationships we have uncovered are, of course, known to the specialists themselves, since they were established by their own citing patterns, but the perspective this method offers is far broader than can be achieved by any individual scientist. This is the crux of the method: the observed relationships are in substance those which have been established by the collective efforts and perceptions of the community of publishing scientists. Our task is to depict these relationships in ways that shed light on the structure of science.[9]

3) I, too, wish to "shed light on the structure of science", and hope to do so via an elucidation of "the collective efforts and perceptions" of the

scientific community. However, it is at this point that essential differences emerge.

One rationale often advanced for developing co-citation (and other quantitative) methods proceeds by analogy: until patient measurement had established the form of the gas laws ($PV = RT$), there was no "problem" for the kinetic theory of gases to "solve"; similarly, it is argued, quantitative studies of science are necessary to define the "problems" which sociological theory has then to solve. I reject this argument. To me, *the behavior of scientists in the conduct of their research* provides an abundance of problems of much more obvious importance than any correlations contained in a computer printout. Whenever a scientist (or a research group) decides to develop a new technique, or to pursue a fresh and unexpected phenomenon, or to adopt a perhaps unfashionable theoretical approach, there is a sociological problem: each decision brings together "cognitive" (intellectual, technical, cultural) and "social" factors, and to me, "to do the sociology of science" is to explicate such decisions, and to explore the "social grounding" of their rationality.[10]

This task starts from the "participants' perspective." Citations *could* be one relevant source of information of participants' perceptions. But every decision is *particular:* citation and co-citation analysis, in striving to *accumulate and average,* destroys the evidence we need of individual variations. It is often because individual scientists and groups *do not* share the consensus view, defined by (*inter alia*) co-citations maps, that crucial innovative decisions are made.

It is worth dwelling on the importance of *particularity* in studies in the sociology of scientific knowledge. Such is the scale of any scientific specialty, there is a limited number of researchers (and even more strikingly, of groups), who might be considered to share (roughly) similar cognitive constraints on their strategic decisions. In the early years of radio astronomy, there were only three comparable groups—Cambridge, Jodrell Bank and Sydney. In *Astronomy Transformed,* we present an analysis of the social structure of the Cambridge and Jodrell Bank groups, and attempt to relate these structures to technical developments at the two centers.[11] In "mapping" the social structures, we compiled a composite picture, melding interview material with quantitative measures—including mutual citation patterns. We found that the citation (and co-authorship) picture agreed closely with that derived from our other sources: in particular, the central influence of Ryle on the Cambridge group was clearly confirmed. Unfortunately, any attempt to repeat this approach on the Sydney group fails; Pawsey, the Sydney group leader, was, by unanimous agreement, an influence of comparable stature to Ryle at Cambridge; but there is no trace of this in the Sydney citation and co-authorship patterns. A citation analyst can brush this aside, as a mere individual variation which is swallowed up in the statistics. But, to sociologists of my ilk, experiences like this merely confirm the unreliability, and very subsidiary status, of citation measures.

4) I mentioned earlier the claim that co-citation analysis can give an "objective" decision on the composition of specialty bibliographies. I am puzzled as to why such "arbitration" is thought to be necessary. I know that the point raises considerable concern among my colleagues,[12] but I remain relatively phlegmatic. To me, the idea of a "specialty" (and of a discipline) is a *social construct*, a concept which allows actors to make transient sense of their experience, and to orient themselves accordingly.[13] I would expect related actors to have broadly similar, but *not* identical, perceptions of their collectivity. So I would expect a wide measure of agreement, but *no* detailed consensus, on the "boundaries" of the specialty. The "correct definition of a specialty" is, to me, a meaningless concept, and I have no need of anyone (computer-aided or otherwise) to provide me with it. I know that this radically sociological perspective on scientific collectivities makes research more difficult than it would be under more simple-minded premises, but then I happen to believe that sociology *is* difficult. And I am comforted by the thought of the great British "middle class": here is a social construct central to any analysis of British society, which undeniably "exists"—but which stubbornly resists the attempts of empirical researchers to "define its boundaries"! I reject any technique which appears to remove from empirical sociology this "constant-triangulation-on-shifting-sands" character, and to substitute some illusory "solid foundations."

5) One final, general objection. Citation (and many other quantitative) methods draw entirely on features of *formal scientific publications*. Griffith, Drott and Small, in their title, refer to "studying scientific achievements and communication"; yet, in their paper, they pose problems and hypotheses in terms of the properties of *literature* and *documents*—not of the *behavior of scientists.* But surely the interesting questions to sociologists of scientific knowledge (and most historians of science) concern the vast "informal" area of scientific behavior (what we might call the "soft underbelly of science"), where interpersonal influences and negotiations lead to intimate choices of theory and technique, and hence determine the precise direction of the development of scientific culture itself. Studies of communication in science emphasize the importance of informal communication, and suggest that the formal and informal areas are *different in kind.*[14] To attempt to use clues from the *formal* area (eventually) to suggest explanations applicable to the *informal* area is, I submit, to reverse the necessary explanatory logic. Explanations

of scientists' behavior in the *informal* domain should surely be extended to include within their scope aspects of "formal behavior"—including the relatively trivial behaviour of adding citations to papers.[15] But, quite apart from "explanatory logic", it is simply my *judgment* that illumination is more likely to accrue "this way round". Certainly, I cannot say that co-citation studies to date have generated any striking insights—even heuristically. But I am willing to be convinced. As Liza Doolittle (who understood epistemology profoundly) put it, in "My Fair Lady": "Show me!"

6) And one final, general point. It seems to be fashionable to say (usually with an air of rather smug satisfaction) that "the sociology of science is a self-exemplifying specialty".[16] Whether you find the insight comforting depends, of course, on the kind of sociology of science you profess: mine is reflexive, but essentially conflict-ridden, and the present debate is a "self-exemplification". To use Mannheim's terminology, the purveyors of quantitative methods seem to me to embody an "enlightenment" (or "natural law") style of thought, while the "participants' perspective" approach is in the "conservative" (or "romantic") style.[17] Placing the respective parties in their social situations, adding positions of relative power and authority (and mutual perceptions of threat), and reflecting on the form of the rhetoric in which this debate is couched,[18] I would venture that our styles are rooted in differences too deep and incommensurable to be bridged by brief academic exchanges—however clear and rational. Since the integrity of 4S is at risk, I find this outlook disturbing. But there is one crumb of comfort: the "participants' perspective" approach generates a self-awareness which allows disputants to live with their differences. What other faith do you need?

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5. For details of some recent such "validations", see *Social Studies of Science*, Vol. 7, No. 2 (May 1977).
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7. Diana Crane, *Invisible Colleges* (Chicago: The University of Chicago Press, 1972), 19.

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9. H. Small and B.C. Griffith, "The Structure of Scientific Literatures. I: Identifying and Graphing Specialties", *Science Studies,* Vol. 4, 39-40, 1974.
10. For examples of such analyses, see D.A. MacKenzie and S.B. Barnes, "Biometrician versus Mendelian: A Controversy and its Explanation", *Kölner Zeitschrift für Soziologie und Sozialpsychologie,* Vol. 18 (1975), 165-96; Jonathan Harwood, "The Race-IQ Controversy: A Sociological Approach, I: Professional factors", *Social Studies of Science,* Vol. 6 (1976), 369-404, and II: 'External' Factors", *ibid.*, Vol. 7 (1977), 1-30. For a related approach, see H.M. Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics", *Sociology,* Vol. 9, 205-24, 1975.
11. Op.cit. note 1, Chapter 9.
12. For an example of such public anguish, see S.W. Woolgar, "The Identification and Definition of Scientific Collectivities", in G. Lemaine *et al.* (eds), *Perspectives on the Emergence of Scientific Disciplines* (Paris and The Hague: Mouton, and Chicago: Aldine, 1976), 233-45.
13. On this point, Warren Hagstrom's remarks on "Sources of Resistance Among Scientists to Their Classification on the Basis of Specialty", *4S Newsletter,* Vol. 2, No. 2 (Spring 1977), 15-16, are relevant and helpful.
14. See, for example, W.D. Garvey and B.C. Griffith, "Scientific Communication: Its Role in the Conduct of Research and Creation of Knowledge", *American Psychologist,* Vol. 26, 349-62, 1971; B.C. Griffith and A.J. Miller, "Networks of Informal Communication among Scientifically Productive Scientists", in C.E. Nelson and D.K. Pollock (eds), *Communication among Scientists and Engineers* (Lexington, MA. Heath Lexington Books, 1970), 125-40; H. Menzel, "Informal Communication in Science: Its Advantages and Its Formal Analogies", in D. Bergen (ed.), *The Foundations of Access to Knowledge* (Syracuse, NY: Syracuse University Press, 1968), 153-67; and F.W. Wolek and B.C. Griffith, "Policy and Informal Communications in Applied Science and Technology", *Science Studies,* Vol. 4:411-20, 1974. (The latter contains many useful further references.)
15. See G. Nigel Gilbert, "Referencing as Persuasion", *Social Studies of Science,* Vol. 7, 113-22, 1977. The need for scientists to have some framework within which to persuade each other may account for the relatively stable and consensual perceptions of specialty boundaries – and hence for the formal properties of co-citation maps.
16. The phrase reappears in the *4S Newsletter,* Vol. 2, No. 1 (Winter 1977), 7.
17. For those unfamiliar with this approach, a succinct account is given by David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976). See also the two papers by J. Harwood cited in note 10.
18. In such disputes, claims about techniques (and, indeed, the techniques themselves) take on an *ideological status* i.e., they are claims, alleging to reflect "objective accounts of the world", which covertly advance and consolidate social interests.