

## Letters to the Editor

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### IS CITATION ANALYSIS A LEGITIMATE EVALUATION TOOL?\*

Sir:

While I write to commend Eugene Garfield on his May 1979 *Scientometrics* article, a comment is in order. The breadth of Dr. Garfield's perspective on the citation analysis literature no doubt exceeds mine, but seems to omit several critical pieces that emanate from a scholarly segment of the science studies community. In other words, the omissions are neither trivial nor deserve to go uncited, particularly in what is otherwise a comprehensive review by an acknowledged expert. Aside from some of my own research,<sup>1</sup> noteworthy contributions include those of Edge,<sup>2</sup> Gilbert,<sup>3</sup> Kaplan,<sup>4</sup> Moravcsik,<sup>5</sup> and Porter.<sup>6</sup>

The problem, as we well know, is that responsible use of the *SCI* and the *SSCI* is not the norm, that counting is a fallible exercise, that indeed, "citation analysis is not meant to replace judgement" (p. 364), but to augment it. Resorting, as Garfield does, to arguments about validation by "peers" and "the scientific community", however, just begs the questions of who is a peer and what is the relevant scientific community (i.e., how is it defined)? Because these ambiguities in level and interpretation of measurement exist, many critics have rejected citation analysis as meaningless numerology. (Oh the sins of positivism!) Proponents, however, have been equally cavalier in ignoring or dismissing critics as uninformed, misguided "non-believers." A responsible review must address the concerns of *both* these audiences. The citation research of both is needed, just as is the dialogue that too seldom ensues from evaluation of that research.

If there were only more researchers who related citation findings to *other* kinds of social data on science and scientists,<sup>7</sup> perhaps reviews would be simpler to write and citation analysis a less abused, more discerning tool in our repertoire. But until then, it is the authors' and the editors' joint responsibility to exercise their bibliographic prerogative by *including*, not *excluding*, literature. If errors on the side of overkill result, they are apt to be construed as overzealousness, not as faulty scholarship.

### References

1. D. E. CHUBIN, The Conceptualization of Scientific Specialties, *Sociological Quarterly*, 17 (1976) 448-476; D. E. CHUBIN, S. MOITRA, Content Analysis of References: Adjunct or Alternative to Citation Counting? *Social Studies of Science*, 5 (1975) 423-441.
2. D. O. EDGE, M. J. MULKAY, *Astronomy Transformed: The Emergence of Radio Astronomy in Britain*, New York, Wiley, 1976; D. O. EDGE, Why I am Not a Co-citationist, *Newsletter of the Society for Social Studies of Science*, 2 (1977) 13-19.

\*E. GARFIELD, *Scientometrics*, 1 (1979) 359.

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3. G. N. GILBERT, S. WOOLGAR, The Quantitative Study of Science: An Examination, *Science Studies*, 4 (1974) 279–294; G. N. GILBERT, Referencing as Persuasion, *Social Studies of Science*, 7 (1977) 113–122; G. N. GILBERT, Measuring the Growth of Science: A Review of Indicators of Scientific Growth, *Scientometrics*, 1 (1978) 9–34.
4. N. KAPLAN, The Norms of Citation Behaviour: Prolegomena to the Footnote, *American Documentation*, 16 (1965) 179–184.
5. M. J. MORAVCSIK, P. MURUGESAN, Some Results on the Function and Quality of Citations, *Social Studies of Science*, 5 (1975) 86–92.
6. A. L. PORTER, Citation Analysis: Queries and Caveats, *Social Studies of Science*, 7 (1977) 257–267.
7. For example, H. G. SMALL, A Co-citation Model of a Scientific Specialty: A Longitudinal Study of Collagen Research, *Social Studies of Science*, 7 (1977) 139–166; H. G. SMALL, Cited Documents as Concept Symbols, *Social Studies of Science*, 8 (1978) 327–340; D. SULLIVAN, D. H. WHITE, E. J. BARBONI, Co-citation Analyses of Science: An Evaluation, *Social Studies of Science*, 7 (1977) 223–240.

Sincerely yours,

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### *Response By E. Garfield*

Sir:

Chubin is correct, of course, in asserting that my May 1979 paper<sup>1</sup> did not cite a number of pieces critical of the concept of citation analysis. Nor did it, I might add, cite all those papers that support the concept.

The reason for these omissions is simply that the paper was not intended to be a comprehensive review of the literature. It was written to put into a useful perspective the most common objections raised in the scientific community against the use of citation statistics to evaluate research at the individual and group level.

The papers I cited were the ones I needed to create that perspective. Those left uncited were not needed. That is not to say, as Chubin infers, that they are trivial.

There is a large body of work having to do with the sociology of citation practices and the use of citation analysis in studying the sociology of science that I do not think fit into the type of paper I have written. The omissions cited by Chubin<sup>2-11</sup> are a part of it. My opinion of this work, in general, is that it is interesting, important, and has the potential of significantly increasing our understanding of both the sociological and cognitive structures of science. For these reasons, I applaud those doing the work and urge them on. If Chubin, or others, feel that a comprehensive review of the literature would further this work, I encourage him, or them to prepare one.

The additional question that Chubin raised about the identity of the peer groups used to validate the results of citation measurements is answered in the material I cited in support of the statement.<sup>12-18</sup>

Chubin's assertion, however, that "responsible use of the *Science Citation Index* and *Social Sciences Citation Index* is not the norm" is completely unsupported by any reference. That is not, of course, surprising. No one really knows how much *SCI* and *SSCI* are used for evaluations. And there certainly is no data on which part of that use is "responsible".

It is interesting to me that *Chubin* apparently perceives the paper as a broad defense of citation analysis. Robert *Merton* perceived it quite differently. Writing in the Foreword of my recently-published book on citation indexing<sup>19</sup> he said about the chapter on which the paper was based: "That chapter can be read less as a newly-developed defense of the use of citation analysis for assessing individual scientific performance than as a methodological manual for those who venture into those dangerous waters. A recurring theme in the chapter is the strong reminder that citation counts cannot be responsibly taken as the controlling basis for appraisals of individual performance."

Apparently I communicated to *Merton* considerably more effectively than I did to *Chubin*.

Incidentally, *Merton* cited, in his Foreword to the book, two of the papers *Chubin* identified.<sup>9,10</sup>

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2. D. E. CHUBIN, The Conceptualization of Scientific Specialties, *Sociological Quarterly*, 17 (1976) 448-476.
3. D. E. CHUBIN, S. MOITRA, Content Analysis of References: Adjunct or Alternative to Citation Counting? *Social Studies of Science*, 5 (1975) 423-441.
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17. E. GARFIELD, Is the Ratio between Number of Citations and Publications Cited a True Constant? *Essays of an Information Scientist*, Vol. 2, Philadelphia, ISI Press, 1977, p. 419-421.

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18. E. GARFIELD, Will ISI's *Arts & Humanities Citation Index* Revolutionize Scholarship? *Current Contents*, No. 32, August 8, 1977, p. 5-9.
19. E. GARFIELD, *Citation Indexing: Its Theory and Applications in Science, Technology, and Humanities*, New York, John Wiley & Sons, 1979, p. 274.

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