

Publication Counting vs Citation Counting
in Evaluating Research.

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In a previous editorial,¹ I discussed the use of the *Science Citation Index*® in evaluating research. In this editorial, I am reprinting part of a letter which expressed my reaction to the use of publication counting—which underlies the whole publish-or-perish syndrome—as a method of evaluating research.

Some background may be necessary. In 1963, Hodge suggested that “publication of scientific papers is a key you can use to rate your company’s research productivity.”² His article in the *Harvard Business Review* provoked a spate of letters,³ some of them reasonable, but most of them ignoring Hodge’s warning that “this method of analysis should be helpful”, but “used with restraint.”

Also in 1963, Cuadra applied citation counts as a method of evaluating contributions in the field of science information. His report⁴ provoked as intense a response in the pages of *Special Libraries* as had Hodge’s elsewhere. Sherrod⁵ objected that Cuadra’s study failed to identify the contribution undoubtedly made by administrators responsible for some of the largest and most important governmental and industrial information centers. And he found the ranking of the identified leaders questionable. The ranking put Kent first, followed by Taube, Perry, Luhn and Shera. Among the replies to Cuadra’s study, in addition to Sherrod’s article, was a letter from the late Mort

Taube,⁶ whose energy and charm his letter will immediately recall to anyone who had the privilege of association with him. Another reply was a letter of my own,⁷ parts of which are reprinted below. It attempts to put into perspective the difference between “publish or perish” and “be cited or damned”. The signal difference is in *impact*, which I have discussed briefly elsewhere.⁸

“Citation indexing can be used to facilitate evaluation of individual scientists or laboratories, but especially individual discoveries or inventions. ‘Impact’ factors are in many ways superior to publication counting, but each has its own special values. For example, publication counting can tell you little about the effect of a man’s work on others. Citation indexing can. We recently determined that two chemists, one American, the other Soviet, had each published 117 papers during a four-year period. However, the work of the American chemist was cited hundreds of times, while, during the same period, the Soviet chemist’s work was almost completely ignored in the broad literature covered by the 1961 annual *Science Citation Index*. Several interesting interpretations can be given to such data.

“Hodge claims that the use of citations limits its applications due to the inherent time lag involved. This is not generally true. For instance, if a research laboratory is well established, it will have a long record of publication,

and its publications will also be cited to the extent that the work has impact. However, if a young firm is involved, then its publication count will be small in most cases. And yet, a single great breakthrough will be cited frequently, even within a short time. For socio-metric purposes, this time lag will be inconsequential.

"While Hodge is correct in stating that it was not previously a practical matter for the individual administrator to make citation counts, this is no longer true due to the availability of the *Science Citation Index*. Data obtained from this index would, among other things, not suffer from the biases inherent in Hodge's study. While Randall⁹ points out the consequences of Hodge's failure to cover the biological sciences, there are non-random factors involved which make any sampling procedure suspect. This may appear to be a strong statement, but the size of the populations involved is such that the addition or omission of any one journal might significantly affect most of Hodge's tables. Many journals of this type were omitted from his study. On the other hand, his list was well chosen in that it included many of the top-ranking journals—those which contain large numbers of articles. Indeed, if anything, his study shows that people rate journals as important in proportion to the number of articles they contain—a valid measure—but not as revealing a measure as impact factor,¹⁰ which ranks a journal on the basis of the average number of citations to the average article. These qualitative differences in journals are similar to those observed for individual papers.

"On the specific problem of the prime ranking of Kent in the Cuadra study, it is very easy to trace one source of the 'error'. We have found that individuals who edit multi-authored works, as is the case for Kent, will be 'credited' with citations that are made to specific portions of the multi-

authored work. Indeed, in the *Science Citation Index* we used to create two distinct entries—one for the editor and another for the specific author, if both are given in the reference citation. This obviously creates a bias for editors, but on the other hand, editors tend to become better known in their fields. (This practice has been discontinued in the *SCI*[®].) Administrators are important in any field, but Cuadra is trying to measure research contributions. In contrast to the research or idea man, therefore, it is not surprising that administrators are not as frequently cited. Incidentally, some administrators publish a great deal, but their work would not ordinarily be considered original. That is why evaluation by publication counting has its dangers, as does citation counting. Be that as it may, citation counting does appear to identify, in a more objective fashion than any other method so far proposed, the key research contributors to a field. We have gathered considerable data along these lines at ISI and plan to publish when it is possible to do so without creating unnecessary resentment.

"Cuadra's particular methodology is itself open to question, but this does not mean citation counting methods are valueless. A more interesting application of citation data is to identify the particular paper or book that is cited an unusually high number of times. Such an analysis of the documentation literature would produce far different results from those reported by Cuadra. In a special experimental citation index we prepared a few years ago covering literature of documentation and information science, some of the most frequently cited papers were by people who did not appear on any of Cuadra's lists. Neither Taube nor Kent are on that list either. Contrary to Cuadra's study, Taube did rank higher than Kent in our study in terms of total citations. This would

parallel his rank as one who has many publications to his credit. However, if we brought our study up-to-date, I am confident it would show a considerable difference in cumulative data since

much more work today is concerned with automatic indexing, citation indexing, etc., which were less fashionable five or ten years ago.”

1. Garfield, E. Citation indexes in sociological and historical research. *Current Contents/Life Sciences* 12(34):4, August 26, 1969.
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3. Reiss, H. *Harvard Business Review* 42:42, 1964; Suits, C.G. *HBR* 42:44, 1964; Schairer, G.S. *HBR* 42:46, 1964; Hodge, M.H. Jr. *HBR* 42:44-48, 184, 1964.
4. Cuadra, C.A. *Identifying Key Contributions to Information Systems*. System Development Corporation Report SP-1467, Dec. 10, 1963, 23 pp. See also *American Documentation* 15:289, 1964.
5. Sherrod, J. Selective publication of information. *Special Libraries* 55(6):386-387, 1964.
6. Taube, M. More on evaluating published contributions. [A letter to the editor of] *Special Libraries* 55(8):584-585, 1964.
7. Garfield, E. Evaluating published contributions. [A letter to the editor of] *Special Libraries* 56(2):134-135, 1965.
8. ----- . Citation measures used as an objective estimate of creativity. *Current Contents/Life Sciences* 13(34):4-5, August 26, 1970.
9. Randall, R.L. *Harvard Business Review* 42:184, 1964.
10. Garfield, E. & Sher, I.H. New factors in the evaluation of scientific literature through citation indexing. *American Documentation* 14:195-201, 1963.