

"CURRENT COMMENTS"

Citation Frequency as a Measure of Research Activity and Performance

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A few years ago, Professor Derek Price suggested that we confer on Professor Oliver H. Lowry a "citation laureateship," since his 1951 paper on protein analysis was, as it continues to be, the most frequently cited item in all the scientific literature.¹ Asked to comment, Professor Lowry replied:²

"... It is flattering to be 'most cited author,' but I am afraid it does not signify great scientific accomplishment. The truth is that I have written a fair number of methods papers, or at least papers with new methods included. Although method development is usually a pretty pedestrian affair, others doing more creative work have to use methods and feel constrained to give credit for same

"The protein determination paper . . . is a good example of what I mean. We had need of a sensitive protein method and adopted one used by immunologists. It wasn't as reproducible as we liked, and so we fiddled with it until it was satisfactory. We probably would never have published the method except perhaps as a footnote, but Dr. Earl Sutherland, who had used it and liked it, urged me to do so. This meant considerably more work to test the method and get it in shape for publication. But there is really almost nothing original in the paper. It just happened to be a trifle better or easier, or more sensitive than other methods and of course nearly everyone measures protein

these days. This is made worse by a perhaps unfortunate inertia in regard to methods. Once a substantial number of people use a certain method, then others feel obliged to at least check their results with the same procedure."

While it is true that being the "most-cited author" doesn't itself signify great scientific accomplishment, it is true also that Professor Lowry's modest disclaimer can't gainsay the fact that he is author also of numerous non-methods papers which, though less cited than the protein-determination paper, are cited nevertheless with a frequency and endurance that clearly place his work on a citation level associated with Nobel Prize winners.³

It is important also to observe that the phenomenon of the Lowry method paper is peculiarly unique. Why is it that a "classical" methods paper like this is *explicitly* cited so frequently, even in biochemical journals, when it is to be expected that such a well-known method would be *implicitly* cited, that is, mentioned but not formally cited?

Is it possible that if more scientists built and published on their better

"mousetraps" that science would progress even more rapidly? Is the simplicity of an idea that catches on to be as easily dismissed as Professor Lowry would have us do? Is he aware of the thousands of other methods papers that have been published which have never been cited?

Several colleagues have written to me regarding what they believe to be possible abuses of citation analysis in evaluating the work of certain scientists. I have repeatedly stressed that the use of citation data in evaluating individual performance is valid only as a starting point in a qualitative appraisal. Is any reasonable person going to claim that the intellectual achievement represented by Einstein's Unified Field Theory is less significant than a convenient method of protein determination simply because Einstein is cited less frequently than Lowry? However, citation analysis reflects accurately that far more scientists are concerned with protein determination than with the Field Theory.

Citation frequency is a measure of research activity, or of communication about research activity. The measure is a sociometric device. In itself, the number of citations of a man's work is no measure of *significance*. Like one scale on a nomogram, it must be used along with other scales to obtain anything useful or meaningful, particularly if the object of the evaluation is in any way qualitative.

Recently a reduced university department budget required that one faculty member had to go. When the

painful choice was announced, the appraisal was challenged, and the man's citation record was 'cited' to prove that he was just as "eminent" as anyone else in the department. The mere counting of citations proved nothing until the faculty got down, as they should have done before, to a close examination of each man's publications and of the comments of those who had cited them. In this case, the man had been clearly underestimated. I'm glad to say that the ultimate decision to retain him was made on the basis of teaching ability and commitment, rather than either publication or citation counts.⁴ But clearly, use of the citation record as a starting point helped to prevent an unfair decision.

These caveats do not imply that citation analysis cannot be quite validly used in making large-scale appraisals. This has been brought out by Hagstrom and many others^{5,6} in situations where it would be almost impossible for even an unbiased committee to fairly appraise the work of hundreds or thousands of candidates, or of groups of research workers. Though there are good reasons to believe that citation counting may be more valid than publication counting,⁴ that fact doesn't relieve us of the responsibility for dealing with the individual case in a responsible way.

There has been a good deal of similar discussion about the use of IQ's in judging and selecting people for jobs. Anyone who chooses between two candidates merely on the basis of a few points difference in IQ scores deserves

what he will undoubtedly get using such an approach. It would be only slightly less foolish to select a person with an IQ of 70 for a faculty chairmanship because of "overriding" political considerations.

Citation frequency reveals the impact of a particular publication or scientist. Admittedly, that doesn't tell the whole story. As we found in using citation data to reconstruct the history of the genetic code,⁷ there are papers which are not frequently cited but which nevertheless are significant because they help to bridge gaps of knowledge in rapidly developing fields. It is difficult to understand why such papers aren't more generally esteemed. Yet leaders in a field readily concede their import if not their impact. For this reason, I have wished that we could do more research at ISI on the reasons so many papers are never or rarely cited. It is something I recommend that graduate students consider in their fishing expeditions when looking for dissertation topics.⁸ I also recommended recently that granting agencies might similarly investigate why papers emanating from their programs are disregarded in the literature. Journal editors might try the same exercise.

1. Lowry, O.H. et al. Protein measurement with the Folin phenol reagent. *J. Biol. Chem.* 193:265, 1951.
2. Personal communication to D.J.D. Price, November 11, 1969.
3. Garfield, E. Citation indexing for studying science. *Nature* 227:669-71, 1970.
4. Garfield, E. Publication counting vs. citation counting in evaluating research. *Current Contents* No. 22, 2 June 1971, p. 5-7.
5. Hagstrom, W.O. Inputs, outputs, and the prestige of university science departments. *Sociol. Educ.* 44:375, 1971.
6. Margolis, J. Citation indexing and evaluation of scientific papers. *Science* 155:1213, 1967.
7. Garfield, E. & Sher, I.H. *Genetics Citation Index: Experimental Citation Indexes to Genetics with Special Emphasis on Human Genetics*. (Philadelphia: Institute for Scientific Information, 1963), 854 pp.; cf. introductory material, pp. i-xviii.
8. For an interesting discussion of this problem of research project selection, of "making known the unknowns," see: Serratosa, F. "Ignoratia." In: *The Scientist Speculates: An Anthology of Partly-Baked Ideas*, ed. by I.J. Good et al. (London: Heinemann, 1962), pp. 4-9.