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Current Comments®

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Delayed Recognition in Scientific Discovery: Citation Frequency Analysis Aids the Search for Case Histories

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An update on ISI®'s research into delayed recognition is detailed. Five examples of the phenomenon have been identified through citation frequency analysis. These include the works of Michael Abercrombie (histology), Henry M. Irving and H.S. Rossotti (metal complexes), Edward Kaplan and Paul Meier (nonparametric studies), Nathan Mantel (life-table statistics), and Nobelist Steven Weinberg (leptons).

Recognition, far more than money, is what makes the scientific world go round. That is what I have learned in the course of developing the *Science Citation Index*® (*SCI*®) and its associated index products. It is not surprising, therefore, that, for more than a decade, *Current Contents*® (*CC*®) has included commentaries by authors of *Citation Classics*®. My own preoccupation with these authors derives from a desire to recognize the hundreds of scholars and scientists who, in many cases, have received little recognition (beyond the scientific audiences for whom they write) for their often critical role in the progress of science.

I am sure that many *CC* readers can think of colleagues who have been instrumental in their field but whose citation record does not adequately reflect their impact. This lack of explicit recognition may be due to the vagaries of citation behavior.[1] But many of these cases are—in fact—examples of the widespread phenomenon of delayed recognition, about which I wrote nearly a decade ago.[2] It is tempting simply to repeat that article here for the benefit of the readers not familiar with the ground it covers. However, I'll be glad to send a reprint to any reader who requests a copy.

Definition

To begin with, delayed recognition contains several different kinds of related phenomena. Sociologist Stephen Cole, now at the Department of Sociology, State University of New York, Stony Brook, was the one who first suggested the term *delayed recognition* and whose paper looked at the timing of response to a scientific discovery.[3] Bernard Barber, Department of Sociology, Barnard College, Columbia University, New York, called those cases "resisted discoveries"[4] and Gunther S. Stent, Department of Molecular Biology, University of California, Berkeley, called them "premature discovery."[5] Both Barber and Stent emphasized in their papers that discoveries that were not consistent with the accepted knowledge at the time or not verifiable technologically would experience the delayed phenomenon. Delayed recognition papers are those that are initially unappreciated or unused but are later recognized as significant. When we look at the citation record for such papers, we often find a sudden or gradual accumulation of citations at a point in time well beyond what is typical for that field (usually, a normal paper has its citation curve peak within five years following publication). For each scientific field, the citation curve would be different; delayed recognition may occur over centuries, decades, or a few years. The most famous case of delayed recognition is that of Gregor Mendel, with a time delay of 35 years. The *reasons* for the delay are by no means obvious. The attempt to understand those reasons is of interest to historians, sociologists, and contemporary critics of science.

Citation Analysis in the Study of History

I have always been interested in how earlier scientific work contributes to later efforts years in the future. In a paper originally published in 1963, I borrowed the term *critical path* from the field of operations research.[6] A critical path is the sequence of crucial tasks necessary to complete large and complex projects, such as the design and construction of rockets, missiles, and jet aircraft, that require the coordination of several thousand subcontractors and government agencies.[7]

It seemed to me, intuitively, that the critical path concept could be extended, by analogy, to the sociology and history of science. I thought it would be an excellent way of getting at the antecedents of later achievements. In my 1963 paper, I stated that it was possible, using computers and comprehensive citation indexes, to produce "network diagrams which show the chronological and derivational relationships between scientific papers and...discoveries."[6] These network diagrams or "maps" could identify key antecedents and descendants of scientific discovery. Some of these would be "critical" points in the path of discovery.

Since 1963 ISI® has developed a method to generate maps that illustrate the development of science; this method can be focused on specific research problems or on entire disciplines and fields. The method is based on co-citation analysis, which identifies clusters of earlier papers that are being cited together in later papers. By tracking these clusters over time, we can show the historical evolution of ideas and disciplines. Interested readers should refer to the earlier essays on co-citation clustering techniques and cluster tracking.[8,9]

Recognition is one of the most valued rewards of science. It often is conferred exclusively on the individual or team responsible for a particular breakthrough. These fortunate few certainly deserve the media attention and awards that come with the success of discovery. But the investigators responsible for prior advances that led to the breakthrough also deserve recognition—if not by the awards committees, then certainly by their peers and historians of science. A critical path concept—whether of an aspect of science or of a mapping effort that highlights research clusters through time—has the great merit of allowing the scientific community to recognize the many individuals whose work contributed to the path of a discovery.

It is almost impossible to identify useful, important, yet unrecognized papers by any but highly subjective evaluation, but we can recognize a special class of undervalued papers—those that were recognized long after they were published. Such papers represent delayed recognition and sometimes are associated with premature discovery.[2]

Premature Discovery

As stated earlier, premature discovery is a subset of delayed recognition. A definition, according to Stent, is that the discovery "was not appreciated in its day. By lack of appreciation I do not mean that [the discovery] went unnoticed.... What I do mean is that [scientists] did not seem to be able to do much with it or build on it."[5] This can occur when the contemporaneous knowledge, technology, and social issues prevent the discovery from being extended experimentally or applied to other related scientific efforts. Some possible factors have been noted by William Goffman, then professor of library science, Case Western Reserve University, Cleveland, Ohio, and Kenneth S. Warren, then at the Rockefeller Foundation, New York:

> The question arises whether the lack of appreciation of premature discoveries is attributable merely to the intellectual shortcomings of scientists.... To this, the answer would seem to be no, for at all times there seems to exist a predominantly accepted scientific view of the nature of things, in the light of which research is conducted.... A strong presumption prevails that any evidence that contradicts the

> accepted view is invalid and must be disregarded, even if it cannot be explained, in the hope that it will eventually prove to be false.[10]

Back in 1961 the topic of resistance by scientists to new discoveries (especially those that challenge commonly held precepts) was well covered by Barber.[4] One may speculate whether resistance to new discoveries will change as the number of working scientists continues to increase. While growth in science increases the likelihood that new techniques and new ideas will be more quickly verified, it may also increase the number of new ideas that need to be verified. As time goes on, the burgeoning literature may come to be more of a problem for the assimilation of new discoveries than resistance by scientists.

Postmature Discovery

Since we have discussed premature discovery, we should also mention prematurity's antithesis—postmature discovery. Recently, we reprinted a paper on postmaturity by Harriet Zuckerman, Department of Sociology, Columbia University, and Joshua Lederberg, president, The Rockefeller University.[11,12] Postmature discoveries, those made later than they might have been, need not involve delayed recognition. They refer to delayed discovery, rather than delayed recognition, and attention is called to them because the necessary technology and the relevant information on the subject were available and used by scientists some time before the specific discovery event—and yet the discovery was not made. Postmature discovery can be thought of as deterred; premature as resisted.[13,14]

Methodology

The phenomenon of delayed recognition lends itself to citation study because citations are a measure (or an indicator, if you will) of recognition. It is a practical impossibility to review the cited record for the millions of scientific papers in order to spot rare instances of delayed recognition. However, we can use the citation record to look backwards at highly cited papers and to determine whether any of them were at first cited infrequently.

Earlier this year, therefore, we ventured to see if our *SCI* database would enable us to find unequivocal examples of the phenomenon. We chose these criteria:

1) Highly cited papers that had low citation frequencies for the first 5 or more years, with more than 10 years being preferred.
2) Low initial citation frequency was defined as being near the average of one cite per year for a typical paper.

Without going into great detail about the procedures, this was accomplished. I report that we managed to find five interesting papers (not discussed in our 1980 essay on delayed recognition) that exemplify delayed recognition in the post-World War II period. (Access to the new 1945-1954 *SCI* cumulation will make it possible to go back further.) All these papers are *Citation Classics*.

Our Examples of Delayed Recognition

Michael Abercrombie

The late Michael Abercrombie's "Estimation of nuclear population from microtome sections" was published in 1946 in the *Anatomical Record*,[15] while he was a biologist at the Department of Zoology, University of Birmingham, UK.

His paper involves the accurate estimation of the numbers of cell nuclei in microtome (superthin) sections. Abercrombie explained its significance: "Estimations of the numbers of nuclei in microtome sections are frequently made in some branches of histology.... The curious neglect of this and indeed all quantitative methods in most other fields of histology will no doubt soon be a thing of the past. It is therefore important to consider how best to get reliable conclusions from such nuclear counts."[15]

His method made cell counting easier and more accurate despite the usual problems of microtome sectioning (the production of cell fragments). Abercrombie hinted at its wider application "to any discrete component of

Figure 1: Year-by-year citations to Abercrombie M. *Anat. Rec.* 94:239-47, 1946.

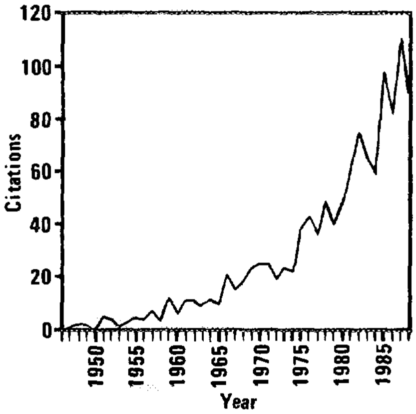
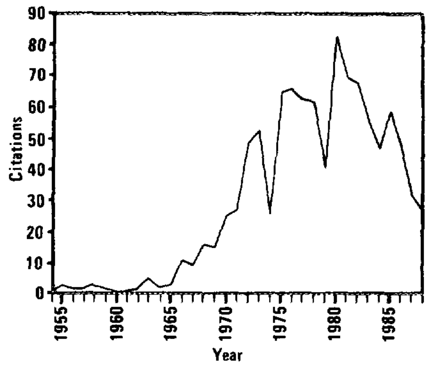


Figure 2: Year-by-year citations to Irving H M & Rossotti H S. *J. Chem. Soc.* Part III:2904-10, 1954.



tissues" in any branch of histology, rather than the limited use the method had at the time.[15] Figure 1 shows that a surge in citations to his work began only in the early 1960s. Why? Was this related to improved or new technologies for superthin sectioning? Or to the fact that cell nuclei counting became important in cancer research? We invite readers to comment.

As it turns out, in 1980 we published Abercrombie's commentary about another *Citation Classic*—his review on the surface properties of cancer cells.[16] Unfortunately, and perhaps significantly, Abercrombie did not mention the 1946 methods paper.

Henry M. Irving and H.S. Rossotti

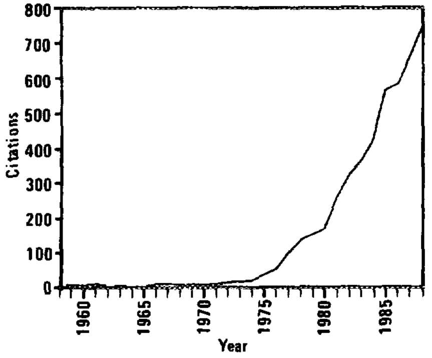
In 1954 Henry M. Irving and H.S. Rossotti's "The calculation of formation curves of metal complexes from pH titration curves in mixed solvents" was published in the *Journal of the Chemical Society*.[17] Both authors worked in the Inorganic Chemistry Laboratory, Oxford University, UK. This paper shows how the formation curve of metal-ligand complexes can be calculated directly from pH-meter readings during titration without regard to H-ion concentration or activity. Figure 2 depicts a delay in citation until 1966, when citations increased markedly, peaking in 1980 at over 80 cites per year. Irving, now at the Department of Chemistry, University of Capetown, Republic of South Africa, has commented that he does not know why the paper has exhibited the delayed recognition phenomenon and doubts that an adequate explanation can be found.[18]

Edward Kaplan and Paul Meier

Edward Kaplan and Paul Meier's "Nonparametric estimation from incomplete observations" was published in 1958 in the *Journal of the American Statistical Association*.[19] The authors are now at the Department of Mathematics, Oregon State University, Corvallis, and the Department of Statistics, University of Chicago, Illinois.

The paper reconsiders the analysis of survival data, in which the observed times to event may be incomplete (the technical term is "censored"). In other words a random sample, which may be of small size, is drawn from a population of people or organisms or devices, for which a lifetime can be defined. The method describes a way to estimate, as a function of the variable time "t," the proportion of items whose lifetimes exceed "t." No unnecessary restriction is placed on the form of this function. The point of the paper is to provide a simple and effective way to make this estimate, even if some of the lifetimes have not been observed—but are only known to exceed some specified values. Such items should not be

Figure 3: Year-by-year citations to Kaplan E L & Meier P. *J. Amer. Statist. Assn.* 53:457-81, 1958.



simply ignored; they tend to have longer than average lives.

The paper is a *Citation Classic*,[20] cited over 3,800 times to date. Figure 3 shows its utility increasing rapidly to a high of over 700 explicit citations in 1988, with no indication that it has yet hit a citation peak. Kaplan expressed surprise at the citation history but conjectured that the delayed recognition of the paper is related to the low visibility both of its authors (the "Matthew effect"[21,22]) and (in the paper's earlier years) of its advantages.[23] According to the comments of coauthor Meier,

> The needs of applied researchers were generally quite well met by the existing methodology at the time (employing arbitrary grouping intervals), so there was no pressing need for the more refined procedure we came up with—which, although conceptually similar, requires more tedious calculation. With the advent of computers, and increasing mathematical sophistication of clinical researchers, the appeal of the newer method grew and came to be adopted as standard.[24]

This is a fine example that shows how a new technology makes a previous contribution more useful and more appealing to scientists.

Nathan Mantel

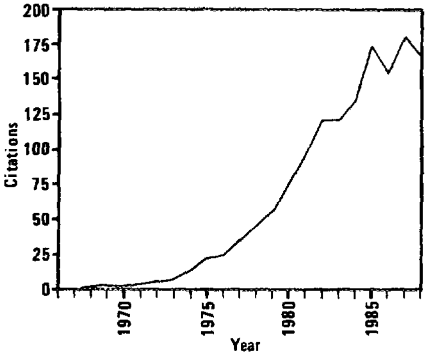
Nathan Mantel, then at the Biometry Branch, National Cancer Institute, Bethesda, Maryland, but now a statistical consultant as well as a research professor, Department of Mathematics, Statistics, and Computer Science, American University, Washington, DC, appears in several of our most-cited studies.[25-27] He published "Evaluation of survival data and two new rank order statistics arising in its consideration"[28] in 1966. The paper is a follow-on to another statistical work on the analysis of epidemiological data, for which Mantel also wrote a *Citation Classic* that appeared in *CC* in 1981.[29] Mantel comments on the phenomenon of delayed recognition in general:

> For example, if I have a paper which involves the life-table method, it can be much easier to give a reference about that method than to give a clear explanation of that method. Even some weak papers can serve usefully to avoid the need for precise explanation. But whether something provides a particularly useful technique, or just a useful reference, there is a seeming pattern of delayed recognition.... Was there delayed recognition of Columbus' discovery of America (as evidenced by the number of Europeans and descendants), or was that just the normal course of events? At one time I saw figures on the number of Christians in the world. That number was pitifully low for hundreds of years [following the initiation of the religion].... Actually, slow initial rise characterizes nearly everything.[30]

Although an initial eight years of low citation preceded the 1966 paper's rise to fame (depicted in Figure 4), Mantel did not mention that in his *Citation Classic* commentary that appeared in *CC* in 1983.[31] However, he said:

> Well, [the paper] originally appeared in a cancer journal, and those in statistics and epidemiology were not initially aware of its existence. It took time for those camps to catch on to the paper's wider significance. It is now considered a standard statistical method. However, it is coming to the point that it is now so standard that I have seen examples where the paper is only alluded to without an explicit reference.[32]

Figure 4: Year-by-year citations to Mantel N. *Cancer Chemother. Rep.* 50:163-70, 1966.



This is, of course, the ultimate compliment for a paper—first delayed recognition, then obliteration by incorporation![33]

Steven Weinberg

I have previously discussed, in another connection,[34] the 1967 paper "A model of leptons"[35] authored by Steven Weinberg, now at the Department of Physics, University of Texas, Austin, but it is included here as an example of delayed recognition of an unusual type. *SCI* data indicate that Weinberg's breakthrough paper was largely ignored for over four years before it was cited at any detectable level (although Weinberg notes that it was quoted before 1971 in two publications, a book and a technical symposium not covered in the *SCI*[36]).

When presented with the evidence of the phenomenon affecting his paper, Weinberg suggested that it was probably due to the initial lack of proof that his theory was "renormalizable," or mathematically consistent. (Abdus Salam, Nobelist cowinner in 1979 with Weinberg for the physics prize, was also working on the leptons proof.[37]) Mathematical proof of the theory was not contained in the leptons paper, and Weinberg observes that, as a result, many physicists reserved judgment. Weinberg reports that he did work on the proof from 1967 through 1971, but that he "was following the wrong path."[36] It wasn't until 1971 that Gerard 't Hooft, a young graduate student at the University of Utrecht, Belgium, published a paper that showed Weinberg's theory was indeed mathematically satisfactory.[38] Interest in Weinberg's leptons paper then increased, as evidenced by the meteoric rise in citations to it.

Figure 5: Year-by-year citations to S. Weinberg's Nobel Prize-winning work, and G. 't Hooft's paper, which "triggered" Weinberg's citation increase. **Solid line**=Weinberg S. *Phys. Rev. Lett.* 19:1264-6, 1967. **Broken line**='t Hooft G. *Nucl. Phys. B* 35:167-88, 1971.

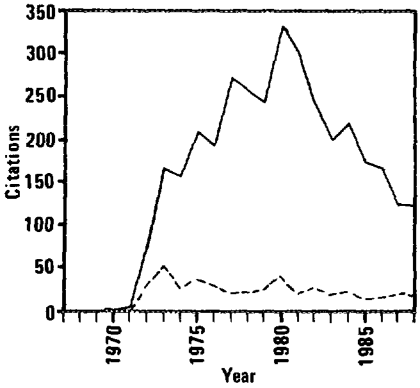


Figure 5 shows the year-by-year cites to Weinberg's epic work and 't Hooft's paper to depict the effect of an accepted proof on a previously untested theory.

Conclusion

The phenomenon of delayed recognition in the classic sense appears to be relatively unusual. But clearly such papers do exist. Undoubtedly there are dozens of other examples that may or may not be identified by citation analysis. However, where the expert systems may fail, the human brain may succeed. So if you know of a scientific contribution that belongs in the category of delayed recognition, please send me the details. I hope to review such new examples and comment upon them in a future essay.

* * * * *

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