Garfield E. "Rejection of 'The Synoptic History of the Science Citation Index' by Science" Journal of Scientific Exploration 22(4):559-560, Winter 2008

Editor: Comment on Dieter Gernert, "How to Reject Any Manuscript," *JSE*, Vol. 22, No. 2, 2008, p. 233–243.

There is very little that is really new in this interesting and provocative paper. It is well known that on rare occasions even Nobel class work has been rejected by one or more sets of referees. A fellow in Spain named Juan Miguel Campanario has written about this subject. He often refers to the *Citation Classic Commentaries* that were published in *Current Contents,* which demonstrated that on occasion these highly cited papers were rejected even by journals as respected as *Nature*. Wolfgang Glanzel and I published a paper in *The Scientist* about the myth of delayed recognition: Glanzel W. and Garfield E., "The Myth of Delayed Recognition–Citation analysis demonstrates that premature discovery, while rare, does occur: Nearly all significant research is normally cited soon after publication", *The Scientist* 18(11): 8–8 June 7, 2004. Original article in *The Scientist* <http://www.the-scientist.com/article/display/14757/>

Quite frankly when you consider the tens of millions of papers and books that have been published, it is surprising to me that it is so rare that such paradigm breaking papers are delayed or rejected. One wonders what Gernert would consider an acceptable level of rejection considering the huge volume of publication. Indeed many people would argue that rejection rates should be even higher. I am glad he agrees that peer review does serve a useful purpose if properly administered. I've had a lot of positive experiences with the system and a few bad ones. The worst two cases involved papers that were actually requested of me by the editors of the *New England Journal of Medicine* (*NEJM*) and *Science*, respectively.

In the case of *NEJM*, the then editor, who is justifiably a highly respected editor and scientist (Arnold Relman), after making me go through several revisions of my manuscript, refused to publish it because it would be "unseemly" for a paper published in *NEJM* to show how much higher *NEJM* ranked as compared with the other journals in the study. After delaying my paper for almost two years, he turned it down but within a few months it was accepted by Edward Huth, the editor of the *Annals of Internal Medicine*.

The second paper was requested by Daniel E. Koshland when he was editor of *Science*. It took me almost two years to write what I thought would be my magnum opus for *Science*, since I had published two core papers there in 1955 and 1964,1 which are both highly cited. By the time I sent in the "Synoptic history of the Science Citation Index" manuscript, Dan had retired from *Science*. His successor Floyd Bloom, a highly respected neuropharmacologist, refused to publish the manuscript after delaying it for six months or more. The extensive revisions he requested would have delayed the paper another year. Shortly thereafter, I was asked to speak in Copenhagen and my "talk" was published in an established European journal of library science. The full text is available under the title "From Citation Indexes to Informetrics: Is the tail wagging the dog?" *Libri*, 48(2), p. 67–80, June 1998. Based on oral presentation–Center for Informetric Studies, Royal School of Librarianship, Copenhagen, December 15, 1997.

<http://www.garfield.library.upenn.edu/papers/libriv48(2)p6780y1998.pdf/> The original title was "A Synoptic History of the Science Citation Index". That it has been cited only 26 times in ten years tells you something about the importance of where you publish. Had it appeared in *Science* or some other leading journal I have no doubt that it would have been more widely read and cited.

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The Myth of Delayed Recognition

Citation analysis demonstrates that premature discovery, while rare, does occur: Nearly all significant research is normally cited soon after publication

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Most scientists can name an example of an important discovery that had little initial impact on contemporary research. Mendel's work is a classic example.[1,2] The phenomenon of delayed recognition is sometimes invoked in disputes about the validity of citation analysis in evaluating scientists. However, as bibliometricians know, actual examples of delayed recognition are rare.

To identify such papers and to shed some light on their role in scientific communication, we analyzed programmatically the citation histories of the 450,000 research and review articles indexed in the 1980 edition of the *Science Citation Index*. Delayed recognition papers were defined as those which, during a period of five years, were initially rarely cited but then became highly cited during the next 15 years.[3] Highly cited was defined as at least 50 citations or 10 times the journal's 20-year cumulative impact factor.

The chance that a paper, uncited for three to five years after publication, will ever be cited is quite low, even in slowly aging fields such as mathematics. The citation impact of papers not cited initially usually remains low even 15 to 20 years later. Clearly, the potential number of delayed recognition papers is extremely small. Among initially poorly cited papers, only 60 were found that could be considered highly cited during the subsequent 15 years. Thus, a statistically marginal share of 1.3 par 10,000 paper published in 1980 were "neglected" at first, and then, belatedly, received relatively high citational recognition.

But what are these papers about? As expected, most (43%) are life sciences papers, 22% are in physics and 12% each are in chemistry, engineering, and mathematics. Four examples from different science fields are listed below.

1. T. Ogino, M. Aoki, "Mechanism of yellow luminescence in GaN," *Jpn J Appl Phys*, 19:2395–405, 1980, presented the first in-depth study that explained the mechanism of yellow luminescence in gallium nitride. This paper was only cited twice until 1992. From 1996 onward, it received 20 to 30 citations per year, and the trend still continues with 261 cites to date.

2. K.M. Fabian, "The intra-prostatic partial catheter (urological spiral)," *Urologe-Ausgabe A*, 19:236–8, 1980, suggested the idea of a temporary urethral stent, and gave a description of the first intraprostatic partial catheter that has become known as the "urological spiral." This paper

received only two citations until 1989. Although it was published in German, it was well cited in the 1990s, and has been cited in 105 papers to date.

3. J. Feder, "Random sequential adsorption," *J Theor Biol*, 87:237–54, 1980. A mathematical paper published in a biology journal, it has been cited in 209 publications to date, especially in physics journals. The author suggested a model to describe protein adsorption on solid surfaces. The random sequential adsorption model has become very popular.

4. G. Buchsbaum, "A spatial processor model for object color-perception," *J Franklin Inst*, 310:1–26, 1980 gave a clear physical interpretation and mathematical foundation for the 'gray-world' model that is among the most widely cited algorithms in color constancy-related literature. The paper was cited once in 1984 and then in 128 papers after 1988.

Like many myths about the flaws of citation analysis, the claims about delayed recognition are extremely difficult to demonstrate. Each of us has specific examples but, as the data demonstrate, they are indeed the exception to the rule. Nearly all significant research is cited within the first three to five of publication.[3]

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Other examples of delayed recognition, including Inhibin, Scanning Electron Microscopy, and the Genetics of Color Blindness, which were identified by citation analysis, can be found at http://www.garfield.library.upenn.edu/delayedrecognition.html

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