Three Letters from *Current Contents* Readers on Delayed Recognition

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June 12, 1989

Dear Dr. Garfield,

I read with interest your article...concerning "Delayed Recognition in Scientific Discovery." You ask for more examples of this phenomenon. I think our discovery of the phosphatidylinositol [PI] effect in 1953 might be a good example. As shown by the graph presented in the preface of the book, *Phosphoinositides and Receptor Mechanisms*,[1] our *Journal of Biological Chemistry* [*JBC*] paper in 1953[2] received very little attention until about five years after Bob Michell presented in a 1975 review[3] his theory relating phosphatidylinositol turnover to calcium mobilization. From there on, both our 1953 *JBC* paper and Michell's review showed a dramatic and parallel rise in citations. Now, phosphoinositides lead the list in number of publications in biochemistry. It was not possible to clarify the biological significance of the PI effect until certain theoretical and technological advances had been made by the late 1970s and early 1980s.

Sincerely yours,

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29 June 1989

Dear Dr. Garfield:

I read with great interest your..."Delayed Recognition in Scientific Discovery..." Following the remarks you make in your conclusion requesting information about other "late developers," I bring to your attention the work of Professor J.G. Oldroyd (JGO) in the fields of non-Newtonian fluid mechanics and polymer rheology.

As a graduate student at the Massachusetts Institute of Technology I am involved in experimental and numerical work with Professor R.C. Armstrong on the development and verification of quantitative constitutive relations for polymeric fluids. These equations attempt to describe the rheological behavior of viscoelastic materials and are of great interest to researchers involved with fiber-spinning, injection molding, extrusion and almost all areas of polymer processing. A huge number of largely empirical constitutive models have been proposed by scientists in the last 40 years, many of which predict the desired result in one specific application only to give completely *aphysical* behavior in other areas. However, as far back as the 1950s Professor J.G. Oldroyd specified in two papers[1,2] a set of invariance rules which constitutive models should obey if they are to behave sensibly, i.e., physically. These rules are described simply and clearly in a book coauthored by my advisor,[3] which itself is a *Citation Classic*® [*Current Contents/Physical, Chemical & Earth Sciences* (34):18, 22 August 1988].

I do not claim to be the first to recognize this early work as fundamental to the field and I enclose part of an article by Professor R.B. Bird which formed a keynote address at the last International Congress on Rheology.[4] To quote from this article: "Thus in 1950, when most rheologists were still struggling with solving rather elementary problems using the 'power-law model,' JGO was laying down the ground rules that would be used for the next four decades of rheological problem solving." Professor Oldroyd was also the subject of a special memorial issue of the *Journal of Non-Newtonian Fluid Mechanics* (Vol. 14, 1984). The field of theoretical polymer rheology is still relatively small and most work is published in a core of three journals: the *Journal of Non-Newtonian Fluid Mechanics, Rheologica Acta,* and the *Journal of Rheology*; together with additional peripheral sources such as the *Journal of Chemical Physics*, the *Journal of Fluid Mechanics*, and the *Journal of Polymer Science*. I have not tracked the number of citations to these two seminal papers;[1,2] however, using your ISI® database I expect it would not be hard to do so. I do not suppose that even at their peak these papers achieved the rather impressive citation rates of the examples in your article, but the gestation period for them may prove to be even longer than the examples you have discovered! This could be due to the following reasons:

i) The articles were published in England in an era when awareness and transfer of international information was much slower than it is today (provincialism in science has also been the subject of a previous *Current Comments*®).[5]

ii) The highly mathematical foundation of the papers had very little to offer the early, largely empirical experimental work on polymer processing; much of which was carried out in industry.

iii) The ability to solve flow problems using the nonlinear constitutive equations proposed by Oldroyd[2] in anything but trivial cases has been intimately linked with the recent development of large supercomputers.

Even if you do not use this example in any subsequent article, I would be extremely interested to hear the results you may generate from a citation rate analysis. It is also worth noting that at least within this small branch of current research our common "intellectual debt" has been recognized and acknowledged.

Yours sincerely,

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Dear Dr. Garfield: June 24, 1989

I cannot resist your call...to communicate other cases of delayed recognition. I myself published a paper on the problem of delayed recognition.[1] My final conclusion was: in fields of high complexity "the originality of scientific achievements and the latencies of their general acceptance bear inverse correlations." However, the paper was published in German, and, I am sure, you are not aware of it.

In some fields the number of workers was and is small, and in these fields your method of searching for citation peaks or trends may not always work. The most exciting case of delayed recognition which I know is the case of George H.M. Waaler. "There are two main periods in the story, the first from 1917 to 1927 and the second from 1965 to 1973. During the intervening years the author had the subject in his subconscious...but plans for reexamination...were not realized until after retirement."[3] (p. 7) As a young man Waaler tested 18,000 subjects and developed a theory on the genetics of color blindness. His ideas were sometimes cited, but never recognized.[2] Waaler could never work as a paid research worker and had to earn his living in the institute of forensic medicine in Oslo. After retirement he reanalyzed his empirical data and published an updated version of his theory.[3] Thirteen years or 59 years later, respectively, his results were confirmed.[4] This is the most extreme history of stubbornness in science and final success I ever heard of. I do not know whether Waaler is still among us or dead.

Another case is Helmar G. Frank, now a professor in Paderborn, West Germany. As a young man, in 1959 in his dissertation he developed a theory of the entropy of short-term memory capacity. From the beginning Frank's theory was far deeper than similar ideas in the English-speaking world (Broadbent, Atkinson, Shiffrin, Baddeley...). Frank's ideas proved to be of empirical and practical relevance, and he became the founder of a flourishing school of German "information psychology." However, Frank studied in Paris (before 1959) and never did publish in English. Despite [that] his ideas and results are part of many textbooks in West and East Germany, his contribution is virtually unknown in the English-speaking world. In vain or with little success colleagues tried to convince Frank and his followers to publish something substantial in English. One result of such efforts was [published by Siegfried Lehrl and Bernd Fischer].[5]

The failure of Frank and his school (at least until now on the international scene) is, in my opinion, a typical example of a misguided publication policy. Scientists should do themselves some kind of "market research," and examples of delayed recognition are always helpful to see the world as it is.

Sincerely yours,

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