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More Delayed Recognition. Part 1. Examples from the Genetics of Color Blindness, the Entropy of Short-Term Memory, Phosphoinositides, and Polymer Rheology

Number 38 Go to Reprint -- three letters September 18, 1989

Three responses to a call for examples of delayed recognition, publications initially unappreciated but eventually recognized as significant, present four candidates from the fields of cell biology, genetics, memory, and chemical engineering.

This past June we published a second essay[1] on delayed recognition that followed an earlier review nine years before.[2] I discussed how citation analysis could help to identify examples of papers that were not appreciated until long after publication and presented five examples. The essay closed with an appeal to readers to let us know about other cases. And readers did indeed respond. The following four examples, from the three letters that follow these comments, were sent to us from the US and the German Democratic Republic (GDR).

They represent a potpourri of topics: the phosphatidylinositol effect, polymer rheology, the genetic basis of color blindness, and the entropy of short-term memory. I hope that the publication of these letters will encourage other readers to write.

We also received a letter about Afzelius disease, otherwise known as Lyme disease. This will be the subject of a separate essay, as will commentaries concerning inhibin, the male antifertility factor, and wartime studies of vitamin B excretion.

The Phosphatidylinositol Effect

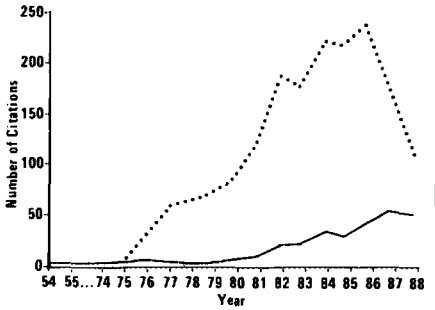
Lowell E. Hokin, Department of Pharmacology, University of Wisconsin Medical School, Madison, wrote us about his own work with phosphoinositides. This is a beautiful case of delayed recognition. The 1953 Hokin paper[3] was published in one of the most prestigious and widely distributed journals in the field of biochemistry. It was widely available and undoubtedly seen by many readers who didn't appreciate or recognize its significance. Only after the connection between phosphoinositol receptors and calcium mobilization in the cell in 1975[4] did the Hokin paper become part of the required reading for a rapidly expanding field of research (see Figure 1), a point already made by J.W. Putney, Jr.,[5] Department of Pharmacology, Virginia Commonwealth University, Richmond.

Oldroyd on Polymer Rheology

Gareth H. McKinley, a graduate student with R.C. Armstrong, Department of Chemical Engineering, Massachusetts Institute of Technology, Cambridge, wrote us a very well-documented letter on J.G. Oldroyd's papers published in the 1950s defining the basic requirements of rheological models.

The citation records for Oldroyd's two major papers[6,7] are shown in Figure 2. From R.B. Bird's biographical paper on Oldroyd,[8] it is clear that Oldroyd was well recognized and respected in his field from the 1950s through the 1980s (see Table 1). The citation records of two highly cited papers[9,10] from the 1950s from another journal in the field, the *Journal of Fluid Mechanics*, show the same jagged pattern with relative peaks in citation frequencies after 10 to 15 years, followed by a gradual increase of the baseline to the present (see Figure 3). Oldroyd's papers evidently did suffer some delayed recognition. It may be, as McKinley suggests, that this was due in part to his having published in English journals at a time of less international exchange of scientific

Figure 1: Citation curves for two important papers on the phosphoinositide effect. Solid line = Hokin M R. *J. Biol. Chem.* 203:967-77, 1953. Dotted line = Michell R H. *Biochim. Biophys. Acta* 415:81-147, 1975.



information, although he plainly obtained recognition in the form of his post at University College, Swansea, UK, and his presidency of the British Rheological Society. More to the point, perhaps, is McKinley's further suggestion that the highly mathematical basis of Oldroyd's classic papers may have been difficult for a largely empirical field to assimilate.

In any case, McKinley's idea about "provincialism in science" certainly relates to one of the examples in the following correspondence from Volkmar Weiss of Leipzig, GDR.

Color Blindness and the Entropy of Short-Term Memory

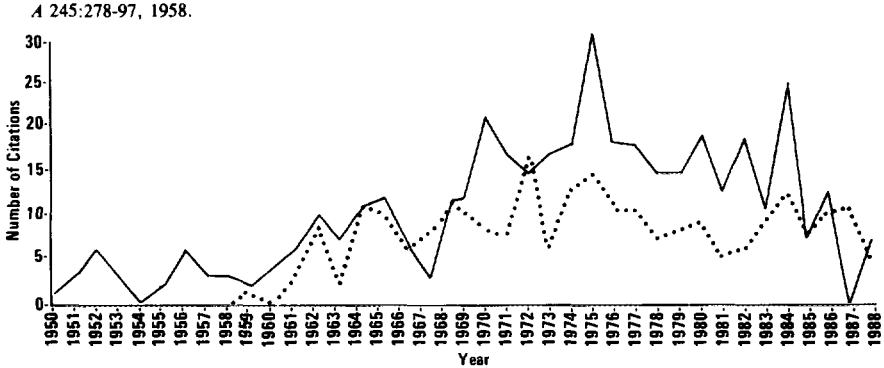
Weiss sent us two examples with his own evaluations of the delays. The first, G.H.M. Waaler's study of the inheritance of color blindness, is a poignant story of a man who worked outside his field of research interest but, nonetheless, managed to publish two key papers—remarkably, 46 years apart.

Waaler's 1927 paper[11] was cited 31 times from 1945 to 1988, a period of 43 years. During that time he published the second paper in two parts in 1973 and 1974.[12] The 1927 paper has received only seven citations since the 1973-1974 papers drew attention to the research again. We do not have the early citation data for 1927-1945, but clearly Waaler's papers on color blindness, together, have been cited in fewer than 35 papers in the *Science Citation Index®* (*SCI®*)/*Social Sciences Citation Index®* (*SSCI®*) database. In contrast, the J. Neitz and G.H. Jacobs paper,[13] mentioned by Weiss, has already been cited in 10 papers since 1986.

Not only did Waaler reanalyze his data after his retirement, but he published the later paper in an ophthalmological journal in 1973-1974. The earlier paper was published in *Zeitschrift für Induktive Abstammungs-und Vererbungslehre*, a journal concerned with genetic inheritance and descent. In this case he had a second chance to demonstrate his theory on the genetic basis of color blindness. Nevertheless, his papers are cited infrequently. Perhaps Waaler's 1927 paper could be classified an "uncited classic."

Weiss's second example, the paper by Helmar G. Frank, represents the disturbing theme of a scientist resistant to putting ideas

Figure 2: Citation record for J.G. Oldroyd's two classic papers on models in polymer rheology. Solid line = Oldroyd J G. *Proc. Roy. Soc. Ser. A* 200:523-41, 1950. Dotted line = Oldroyd J G. *Proc. Roy. Soc. Ser. A* 245:278-97, 1958.



before the widest possible scientific audience. Our *SCI/SSCI* database provides no evidence of Frank being credited with any discoveries or conceptions. Whatever means of communication Frank and his colleagues have used to build a school of "information psychology" in the GDR and the Federal Republic of Germany, the larger scientific community deserves a chance to see his theories in widely circulating professional journals.

Conclusion

These examples communicated to us by readers provide one clear case of delayed recognition: the Hokin example demonstrates how recognition for earlier research expands when theoretical analysis gives the subject a new immediacy—here, the association of phosphoinositides with calcium controls. The Oldroyd papers are not clearly cases of delayed recognition, since Oldroyd was honored throughout his professional career. These papers apparently reflect the slow incorporation of a highly mathematical foundation to a subject that was largely empirical in the 1950s. The Oldroyd citation pattern seems to reflect the gradual growth of the field of mathematical methods in modeling non-Newtonian fluid mechanics.

The Waaler and Frank examples represent research that has not attained much recognition altogether in the scientific literature. The confirmation of the Waaler research in the 1986 publication of Neitz and Jacobs may be some catalyst for increased recognition for Waaler's earlier work. Weiss's appraisal of the Frank problem is probably correct: the Frank work is not likely to gain recognition in the international scientific literature until it is presented in scientific publications that are more widely disseminated and, perhaps, more highly regarded. We invite your reactions and comments.

Table 1: Selected noteworthy events in the professional life of J.G. Oldroyd (1921-1982).[8]

1939	Scholarship, Trinity College, Oxford, UK
1947	Fellow, Trinity College, Oxford, UK
1948	Paper presented, 1st International Congress of Rheology, Scheveningen, The Netherlands
1953	Professor, University College, Swansea, UK
1955	President, British Society of Rheology
1957	Dean, Faculty of Science, Swansea
1963	Adams Prize, University of Cambridge, UK
1965	Professor of Applied Mathematics and Theoretical Physics, University of Liverpool, UK
1973	Head, Department of Mathematics and Theoretical Physics, University of Liverpool, UK
1980	Gold Medal, British Society of Rheology
1984	Memorial issue: *Journal of Non-Newtonian Fluid Mechanics*

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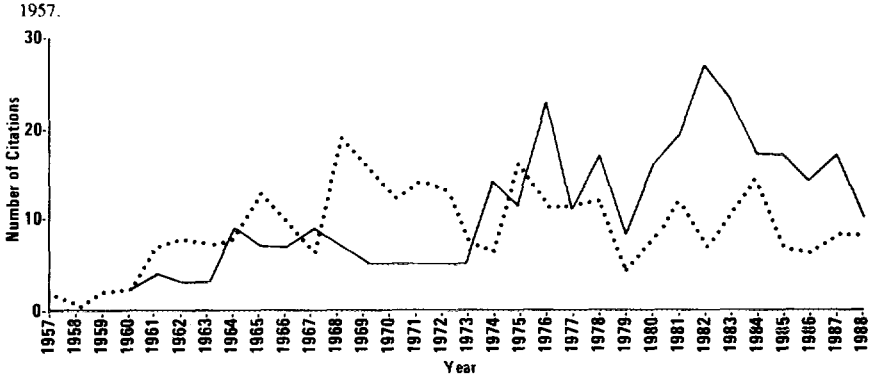
My thanks to James Mears for his help in the preparation of this essay.

Figure 3: Citation record of two highly cited papers from the 1950s from the *Journal of Fluid Mechanics*. Solid line = Kraichnan R H. *J. Fluid Mech.* 5:497-534, 1959. Dotted line = Proudman I. *J. Fluid Mech.* 2:237-62, 1957.



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Three Letters from *Current Contents* Readers on Delayed Recognition

Back to Introduction

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,

Lowell E. Hokin
Department of Pharmacology
University of Wisconsin—Madison
Medical School

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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,

Gareth H. McKinley
Department of Chemical Engineering
Massachusetts Institute of
Technology, Cambridge

(continued on next page)

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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,

Volkmar Weiss
Leipzig, GDR

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