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4. évfolyam · 4. szám · 1994. április

IMPAKT

TÉNYEK A TUDOMÁNYOS ALAPKUTATÁSRÓL

Szilárd: Csak a tényeket írom le – nem azért, hogy bárki is elolvassa,csakis a Jóisten számára.
Bethe: Nem gondolod, hogy a Jóisten ismeri a tényeket?
Szilárd: Lehet, hogy ismeri, de a tényeknek nem *ezt* a változatát.

[*Leo Szilard, His version of the Facts. S.R. Weart & Gertrud Weiss Szilard* (Eds), MIT Press, Cambridge, MA, 1978, p.149.]

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Áprilisi komoly élcek

Bár folyóiratunk áprilisi száma nem elsején jelenik meg, az elsejei bocsánatos élcelődési lehetőséget nem szeretnénk elmulasztani.

Talán olvasóink elnézik nekünk, amiért élve a lehetőséggel, most két karikatúrát közlünk a címoldalon. Ezek — úgy érezzük — jellemzőek a tudományos alapkutatás hazai helyzetére, és összecsengenek lapunk egyik fő irányvonalával, azaz a kvantitatív kutatáselemzési szempontok bevezetésével ill. elfogadásával kapcsolatos szemlélettel.

"My research covers two fields: The behavior of matter under high pressure, and the behavior of scientists under high pressure."

A nemzetközileg legelismertebb természettudományos folyóiratcikkek az NSzK-ban (1980-1989)

Az MTA Könyvtár Tudományelemzési Osztálya újabban a sokat idézett tudományos munkák azonosítására egy matematikai statisztikán alapuló módszert dolgozott ki. A módszer a tudományelemzési mutatószámok kidolgozásának és alkalmazásának többéves tapasztalatán alapszik. Olyan eredményt ad, melyet sem a vizsgált szakterület, sem az idézési időtartam lényegesen nem befolyásol.

Röviden megfogalmazva, a publikációkat akkor nevezzük sokat idézetteknek, ha idézettségi gyakoriságuk egy előre meghatározott időtartam alatt egy tudományos folyóiratban közölt közlemények átlagos idézettségét x-szeresen ($x > 1$) meghaladja; a referenciaszint ebben az esetben tehát az a folyóirat, melyben a közleményt publikálták. Amennyiben az alapul szolgáló folyóirat átlagos idézettsége 1-nél kisebb lenne, akkor a műtermék elkerülése végett a küszöbértéket x-nek választjuk. Ezzel elkerüljük, hogy elsősorban igen rövid idézési időtartamok esetében egyes folyóiratokban megjelent közlemények pl. már két vagy három idézet esetén is kielégítsék a "sokat idézett" kritériumot. Tekintsünk egy példát: $x = 10$ választással egy tekintélyes német folyóiratban, az Angewandte Chemie-ben 1983-ban közölt cikknek 1983 és 1989 között legalább 160 idézetet kellett kapnia, egy ugyanabban az évben a Cancer Research-ben közölt dolgozatnak ezzel szemben 254-et ahhoz, hogy a sokat idézett kategóriába kerülhessen.

Több, mint négymillió dokumentum adatai

Egy, az 1980-1989-es évtizedre vonatkozó széleskörű vizsgálat keretében, az SCI adatbankjában tárolt mintegy 4,3 millió dokumentumból, a fenti kritériumok szerint kikerestük a sokat idézetteket. A világon megjelent és tekintetbe vett összes publikációnak mintegy 0,22 %-a volt sokat idézett. A sokat idézett közlemények közül összesen 743-nak a megírásában vettek részt a Német Szövetségi Köztársaság kutatói. A sokat idézett német munkákat öt nagy tudományterületbe (élettudományok és orvostudomány, fizikai tudományok, kémia, mérnöki tudományok és matematika) soroltuk. Minden nagy tudományterületen a sokat idézett német közlemények aránya a világátlaghoz képest kiemelkedően magas (l. az 1. ábrát).

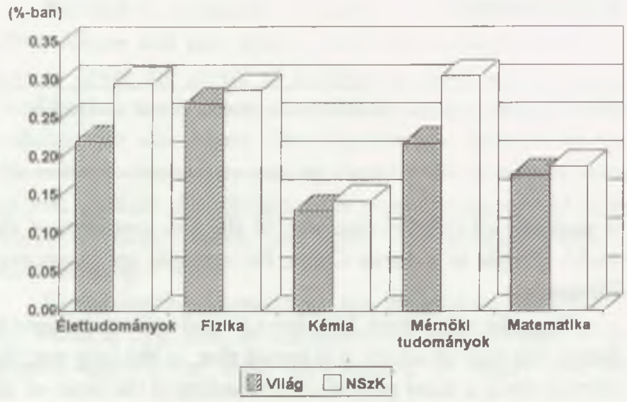
A sokat idézett német publikációk témái között megtalálhatók a nagyhőmérsékleti szupravezetés (különösen az 1987 és 1989 között megjelent közlemények), az Alzheimer betegség, a fotoszintézis, és a nagyenergiájú fizika különböző problémái. A nemzetközi szemszögből nagyon jelentős munkák szerzői között néhány német Nobel-díjast is találunk.

Különösen érdekes a sokat idézett német publikációknak az intézmények és tartományok szerinti eloszlása. Mivel ezek jelentős hányada intézmények közötti együttműködés eredménye, ezért minden intézményhez hozzárendeltünk minden olyan közleményt, amelyben közreműködött, úgy, hogy az eredmények nem additivak. A 2. ábra a sokat idézett publikációknak a szövetségi tartományok közötti eloszlását mutatja. Az élen Baden-Württenberg található, ezt szorosan követi Bajorország és a legsűrűbben lakott szövetségi tartomány, Észak-Rajna-Vesztfália.

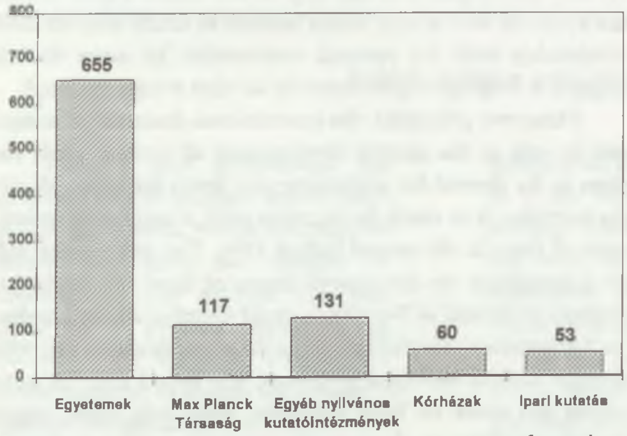
Kiemelkedő helyet foglalnak el (655 tételt) az egyetemi kutatóhelyek, összehasonlítva a Max-Planck intézetekkel és más állami intézményekkel, így pl. a nemzeti kutatóközpontokkal (Großforschungseinrichtungen) (l. a 3. ábrát). Az egyetemek rangsorolását, mely — és ezt itt ismételten hangsúlyozzuk — nem tükrözi közvetlenül az ott végzett kutatás minőségét, az 1. táblázat mutatja be. Valószínűleg nem véletlen, hogy több, nemzetközi szemszögből nagyon jelentős munkát a nagy hagyományokkal rendelkező egyetemek kutatói írtak. Egy olyan egyetem esetében, mint pl. a bielefeldi, mely főleg a társadalomtudományokat műveli, a kutatási súlypontok természetesen máshol vannak, és ezért itt az öt sokat idézett munka, a Berlini Műszaki Egyetemmel (TU) összehasonlítva, jelentősnek mondható.

1. táblázat
A nemzetközi viszonylatban leggyakrabban idézett természettudományi és műszaki közleményeket publikáló német egyetemek (1980-1989)

Egyetem	Sokat idézett közlemények száma
Müncheni egyetem	66
Heidelbergi egyetem	46
FU Berlin	31
Freiburgi egyetem	27
Müncheni műszaki egyetem	25
Erlangen-Nürnbergi egyetem	23
Majna-Frankfurti egyetem	23
Göttingeni egyetem	23
Kieli egyetem	23
Bonni egyetem	22
Kölni egyetem	22
Münsteri egyetem	22
Tübingeni egyetem	22
Hamburgi egyetem	17
Düsseldorfi egyetem	17
Mainzi egyetem	17
Aacheni műszaki főiskola	16
Regensburgi egyetem	16
Marburgi egyetem	14
Ulmi egyetem	14
Würzburgi egyetem	14
Giesseni egyetem	13
Esseni egyetem	12
Bochumi egyetem	11
Karlsruhei egyetem	11
Dortmundi egyetem	9
Homburgi egyetem	9
Hannoveri egyetem	9
Stuttgarti egyetem	8
Darmstadti műszaki főiskola	8
Wuppertali egyetem	7
Bayreuthi egyetem	5
Bielefeldi egyetem	5
Berlini műszaki egyetem	5
Konstanzi egyetem	4
Saarbrückeni egyetem	4
Paderborni egyetem	4
Clausthali műszaki egyetem	3
Siegeni egyetem	3
Egyéb egyetemek és főiskolák	25



1. ábra A sokat idézett publikációk részaránya az NSzK-ban és a világon



3. ábra A sokat idézett német publikációk eloszlása intézménytípusok szerint

2. ábra A sokat idézett német publikációk regionális eloszlása 1980 és 1989 között

Értékes felvilágosítások nagy jelentőségű kutatási témákról

Függetlenül az ilyen értékelő vizsgálatoktól, a sokat idézett dolgozatok elemzése értékes adatokat szolgáltathat a nemzetközi kutatás tematikai súlypontjait illetően, ahogyan ezek a formális tudományos kommunikációban tükröződnek és a német kutatók itt elfoglalt helyéről tájékoztatnak.

W. Glänzel és H.-J. Czerwon, Deutsche Universitätszeitung, 49 (21)(1993) 15-17

Is the Scientific Journal to Disappear?

The active researcher needs to communicate the results of his findings to his colleagues in the same field. To such an end, several avenues are available: oral contact, personal or in congresses, communication by letter or, finally, the publication in specialized journals. The first two were the chief means available until the end of the 17th Century, a time when the *Philosophical Transactions* of the Royal Society in England (1665) and the *Journal des Scavans* of the Académie des Sciences in Paris begin their activities.

Books as means of communication deserve mentioning. Actually, and possibly excepting social sciences, when books are published they are hopelessly obsolete.

According to the late Derek de Solla Price, from the time scientific journals began being published until 1960, more than ten million articles had been published, a figure which increases at the annual rate of 6%; this means that annually 600,000 new articles are published, in the various 30,000 journals then existing. The same source points out that the doubling time of published journals in 15 years; thus we can estimate that there are today some 120,000 journals.

However, the process of publishing an article follows complicated meanders; the article must be carefully written; it is then submitted to the journal which proceeds to evaluate is carefully, preferably by means of "peer" referees who submit a critical judgement and the respective comments. The Journal's Editor has the final say-so as to whether the article is published or not. Finally, the mechanism of printing the article crowns the process.

Such a process takes more or less one year, and can be in certain cases extended to two years, so that, at least in the "hot" areas, the article is long past its prime when it appears, even though less so than when we deal with a published book.

For such reasons, during the past few decades, communication among different investigators' laboratories has returned to its former style: now there is more person to person than article to article communication. This tendency has been increased by the birth of electronic mail, which accelerates communication among researchers.

Does this all mean that the scientific journal must disappear, or take on a much more modest role?

By no means. What is communicated directly, or by electronic mail is usually constituted by trivial details which do not go to the core of the findings. What is concealed in this fashion is more than what is revealed. And it is probable that the scientific journal will continue to be, possibly helped by electronic means, the choice for communication with "peers" and to establish priorities.

M. Roche, Interciencia, 19 (1)(Jan-Feb 1994)

What birthday should be celebrated?

Nature is planning to celebrate its 125th anniversary this year with a series of events that may reinforce its international and public regulation.

Nature will be 125 years old on 4 November this year and plans to mark the occasion in several ways but with appropriate diffidence. As our regular (and self-styled) anniversarists imply in the very last paragraph of their article on page 11, a 125th anniversary (which so obviously almost escapes their attention) is not calculated to excite the imagination generally, as do anniversaries whose ordinal numbers are divisible by one hundred. But that is too narrowly based a proposition. In earlier times, the magic number might just as well have been 7, 13 or many other integer, suggesting that *Nature*'s way out of this dilemma would be to invite its friends to a decent dinner on 4 November each year and leave it at that.

There are nevertheless good historical reasons for paying attention to this year's birthday, all of them to do with the present condition of science and with general expectations of it. A century ago, *Nature* had won a reputation as the champion of Darwinism and of a rationality in the explanation of natural phenomena, but had not become a scientific journal in the modern sense. (Even the discovery of the electron still lay ahead.) Instead, the first editor's helpers, people like T.H. Huxley and John Tyndall, wrote their hearts out in telling what excited them in science. Presciently, as it turns out, *Nature* then as regularly berated the British government for its neglect of science as it does now.

But *Nature* is not longer a British journal. How could it be when almost 90 per cent of its readers are elsewhere? Indeed, from the start, *Nature* took the view that science is international, regaling its readers with the agendas of the academy meetings at St Petersburg and Philadelphia as regularly as with those of the Royal Society (of London) and of the Academie des Sciences (of Paris). *Nature* is now printed once a week at four centres (in Britain, the United States, Tokyo and Beijing) and once a month (as *Monthly Nature*) in Moscow.

The events of the very recent past can only reinforce that cosmopolitan tendency. All of us know of bright people who have been driven from Russia by repression (or recently, uncertainty) and from India's Indian Institutes of Technology by deprivation, and who are now card-carrying honoured members of the international community. Could even a low-grade 125th anniversary celebration help to deepen these international connections? (*Nature*'s fondest wish is that there were another name for "English" — "Esperanto" is bespoken — but that it were permissible to write it with a British accent.)

There is another pressure towards the celebration of this anniversary — the gulf that has recently emerged between science and those whom scientists believe should be science's beneficiaries, people at large. The past few decades have, it is true, seen a powerful growth of the health and wealth that people enjoy, much of which is a consequence of science and its many applications. But recent decades have also seen a growth of suspicion of science, especially in the rich countries of the world. (People in India or China, for example, see things very differently.)

Over the years there have been several valiant attempts to change this state of affairs. It is agreed that, in the long run, the solution lies in a more general understanding of the roots of science and also of its role in the remarkable history of this century, now almost past. But that may be too distant a goal. Should not a journal such as this, which benefits so much from its close relationship with the research community, be more directly engaged in helping to give currently to what is now exciting?

These two principles, the international character of science and its role in the general development of society, guide the plans so far devised for celebrating this year's birthday. Mostly the intention is to mark the occasion with a number of events, most of them in the second half of 1994. The centre-piece will be a conference on the general theme of how our world has evolved, to be held in New York on 31 October and in London on 3 November. We shall of course be as much concerned with physical as with biological evolution; it is hoped that the same people will speak on both occasions. With luck, these events should contribute to the general appreciation of our place in nature.

It is also planned to hold two one-day symposia on the mainland Europe. The first, in Berlin, will deal with contemporary problems in genetics. The venue has been chosen because of the difficulties encountered in Germany in recent decades with matters such as legislation on genetic manipulation. Can informed discussion help to resolve these, or are they a foretaste of things to come elsewhere?

We also plan a symposium in Paris, later in the year, on the theme of the distinctiveness of science in Europe (where, after all, modern science began). the venue marks the recent resurgents of science in Europe and the part played in that encouraging development by the consistency of French public policies on science over recent decades. Can the rest of Europe follow suit?

We have not forgotten Central Europe and Japan, where there are also tentative schemes that will introduce to audiences there people from elsewhere who may have interesting things to say about contemporary problems. These plans depend on the outcome of discussions with institutions in the regions concerned; details will be published later.

The ambition to make a more direct contribution to the general understanding is best realized by the means that *Nature* best understands — publication. It is hoped that 1994 will see the realization of a long-cherished project to produce a collection of the miscellaneous contents of the issues of *Nature* over the past 125 years which are a remarkably rich record of the development of modern science.

But that is necessarily a domestic preoccupation. During 1994, *Nature* will also attempt more deliberately than in the past to foster the spread of intelligence about science and its implications, not so much as a publisher in its own right but in collaboration with others. One objective is to demonstrate that the research community can, by its own efforts, help to make its own intellectual birthright more generally appreciated in the world at large. There are, after all, several routes to understanding.

Explaining the intricacies of, say, the structure of DNA to the world at large may be less effective a reinforcement of the general appreciation than, say, helping to ensure that the significance of DNA is understood within the general culture. But those who battle in the front line for understanding, in the world's classrooms, need more assistance than they are given now. That is another field in which these research community, and *Nature* in particular, could do more to help.

Further details of these events will be published in the next few weeks, as and when they are decided. Meanwhile, *Nature* would welcome suggestions from readers who may have other schemes for celebrating an off-season birthday. The guiding principle should be modesty. After all, *Nature* does not wish to take the wind out of the full-throated celebration there will be 25 years from now.

J. Maddox,
Nature, 367:15 (6 January 1994)

Polish science gets priority — but little extra funding

Poland's new government has proposed a small increase in this year's budget for science, which was suffered severe cuts since the collapse of communism. The increase falls well short of a promise made by in November by Waldemar Pawlak, the prime minister, to double the country's research budget in 1994; but it does not reflect the government's decision to make science one of its priorities.

The Polish Scientific Research Committee (KBN), which functions as a research ministry, is hoping to persuade the government to give it more money later in the year. It is also encouraging research institutes to raise the notoriously low salaries of young researchers, many of whom have been leaving basic research for better paid careers.

To his inaugural address to the national Parliament, the Sejm, last November, Pawlak promised to increase support for science, and in particular to follow guidelines adopted by the previous government in July, proposing to increase the research budget from 9,000 billion zloty (US$ 450 million) to around 20,000 billion zloty in 1994, subject to general budgetary constraints.

Such an increase would have raised the proportion of Poland's spending on research from 0.6 per cent to 1 per cent of its gross domestic product (GDP). But many doubted that such an increase was likely to materialize, given the depth of the country's economic problems.

In fact, next year's science budget will be virtually constant in real terms. The final budget proposals, which will be debated in Parliament within the next few weeks, offer science 11,460 billion zloty, only slightly higher than the predicted rate of inflation. Nevertheless science was one of five sectors given a rise in real terms.

Witold Karczewski, the president of KBN and a neurophysiologist who has already experienced four different governments since the committee was set up in 1991, had been hoping for 16 billion zloty, raising the level of funding to 0.8 per cent of GDP.

But he still hopes that science will benefit from a budget adjustment promised for summer, and is asking for a "top-up" of 4,000 billion zloty. The extra money will be used to support priority research areas identified by the committee, including new technologies such as biotechnology and materials science, heart disease and cancer.

Meanwhile Karczewski is planing to distribute much more of science budget directly to research institutes, and to reduce the amount given to another ministries to provide additional support for research in their own institutes. He hopes this will put pressure on these ministries to support his request for more funds.

The redistribution will have a further benefit. Poland's research institutes now enjoy considerable autonomy, and are able to set their own salaries for researchers. Although salaries differ between institutes, they are invariably low. Young scientists are paid particularly badly, receiving between third and half of what they might expect in industry.

Karczewski, worried by an internal brain-drain of young scientists who are now moving away from basic research, introduced a grant system three months ago under which young scientists can apply for an additional grant up to 70 per cent of their institutional salary while working on their PhDs.

But he would prefer to see a general rise in salaries rather than relying on such *ad hoc* arrangements. His decision to give research institutes more control of their budgets is intended to encourage them to respond to pressure from employees to raise salaries.

A. Abbott,
Nature 367 (13 January 1994) 105

Delayed recognition in scientific discovery: Citation frequency analysis aids the search for case history

An update on ISI's research into delayed recognition is detailed. Five examples of the phenomenon have been identified through citation 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 request 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, Bernard 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 attempts 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 evaluation 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 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].

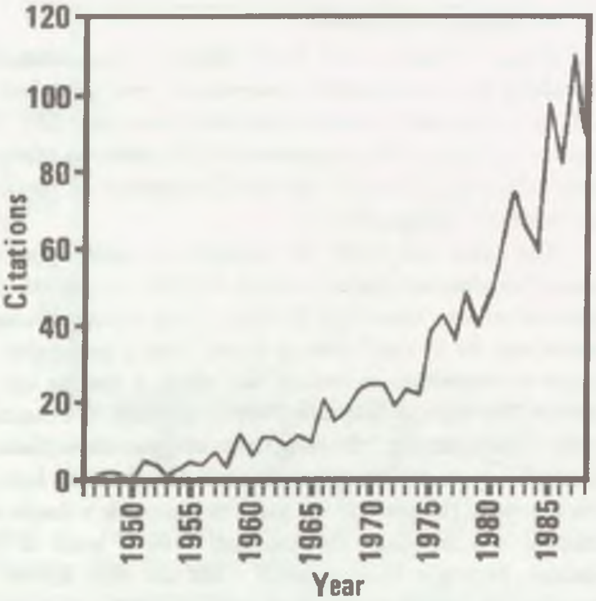


Figure 1. Year-by-year citations to Abercrombie, M., *Anat. Rec.*, 94:239-47, 1946.

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 short comings of scientists... To this, the answer would seem to be no, for 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 essays 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

(Continued on next page)

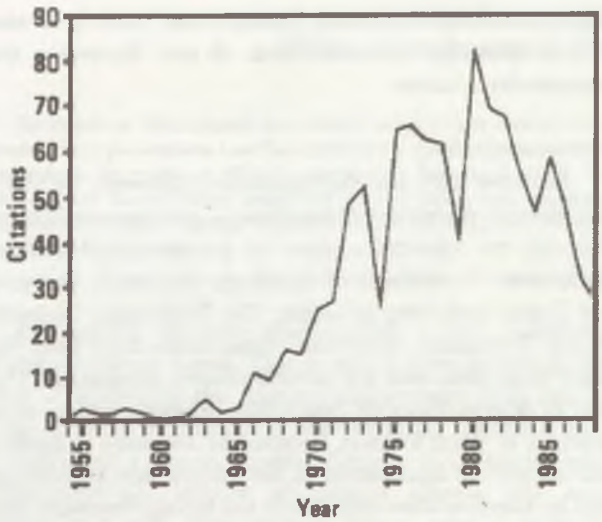


Figure 2. Year-by-year citations to Irving, H.M., Rossotti, H.S. *J. Chem. Soc.* Part III:2904-10, 1954.

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 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 method 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 "Non-parametric estimation from incomplete observations" was published in 1958 in the *Journal of 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 reconsider 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 specific values. Such items should not be simply ignored; they tend to have longer than average lives.

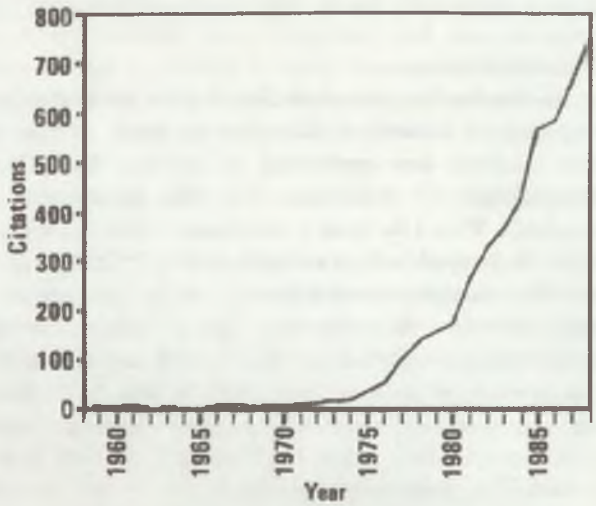


Figure 3. Year-by-year citations to Kaplan, E.L., Meier, P., *J. Amer. Statist. Assn.*, 53:458-81, 1958.

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 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].

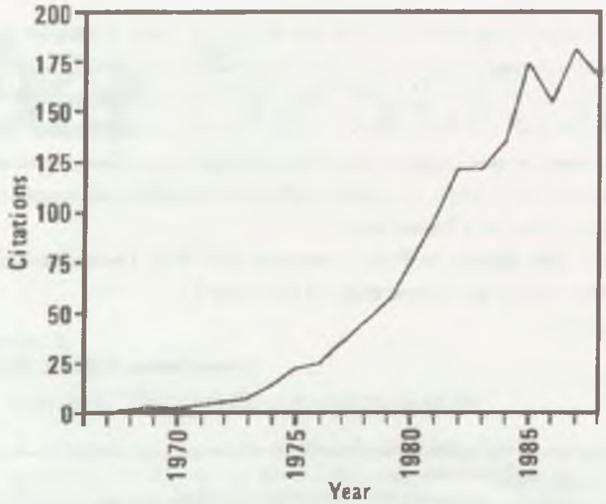


Figure 4. Year-by-year citations to Mantel, N. *Cancer Chemother. Rep.*, 50:163-70, 1966

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 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 characterize 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].

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].

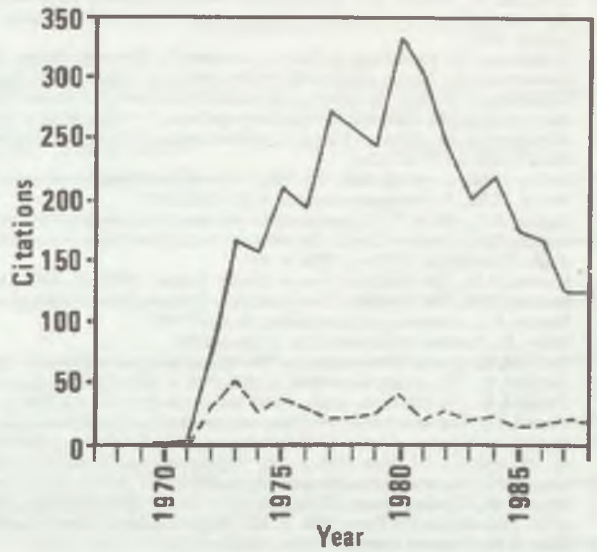


Figure 5. Year-by-year citations to S. Weinberg's Nobel Prize-winning work, and G. t'Hooft's paper, which "triggered" Weinberg's citation increase. Solid line = Weinberg, S., *Phys. Rev. Lett.*, 19:1264-6, 1967. Broken line = t'Hooft, G., *Nucl. Phys. B.*, 35:167-88, 1971.

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 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 succeeded. 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.

(My thanks to Peter Pesavento and Eric Thurschwell for their help in the preparation of this essay.)

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When you have eliminated the impossible, what ever remains, however improbable, must be the truth.

*

It is a capital mistake to theorise before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts.

*

As... the worthy professor's stock of knowledge increased — for knowledge begets knowledge as money bears interest — much of which had seemed strange and unaccountable began to take another shape in his eyes. New trains of reasoning became familiar to him, and he perceived connecting links where all had been incomprehensible and startling.

([Sir] Arthur Conan Doyle)

Der Forschungs Index

A kutatási index
A német kutatás vezető intézetei

HIV és AIDS kutatás

Rangsorok: "Nem kegyetlenebb, mint maga az élet"

Az a módszer, amely a kutatók és intézetek teljesítményét a publikációs és idézettségi rangsorokban elfoglalt helyük alapján értékeli, érvényesülni kezd — akár egy állás betöltéséről, akár egy kutatási támogatás odaítéléséről van szó. Ezt egy amerikai tudományos folyóirat, a *Science* állapította meg. Bár néhány kutató az idézettségi rangsorokat "borsószámlálásnak" tartja, és az angol *Nature* szerkesztője, John Maddox, úgy ítélte, hogy "egy kutató minőségére vonatkoztatva az idézettség nagy száma ugyanazt jelenti, mint publikációinak súlya grammban kifejezve". Ezzel szemben a *Science* többek között Fernand Labrie biológus kutatóról, a Laval Egyetem (Quebeck) igazgatójáról azt írja, hogy munkatársait az alábbi szempontok alapján értékeli:

- beszerzett kutatási eszközeinek mennyisége szerint (40 százalékban)
- személyes benyomása szerint (20 százalékban)
- az idézettségi rangsorokban elfoglalt helyük szerint (40 százalékban)

Ez a módszer, Labrie nézete szerint, arra ösztönzi kutatóit, hogy a legjelentősebb szakfolyóiratokban közöljenek, ahol azokra a legjobban felfigyelnek. Arra a kérdésre, hogy módszere nem túlzottan személytelen-e, Labrie ezt válaszolta: "Ez a módszer nem kegyetlenebb, mint maga az élet".

A befolyásosak

	Intézmény	Idézetek száma 1990 és 1993 június között	Publikációk száma 1990 és 1993 március között
1	Müncheni egyetem	312	102
2	Szövetségi közegészségügyi hivatal, Berlin	197	51
3	Freie Univrsität Berlin	179	81
4	Mainzi egyetem	167	42
5	Bernhard-Nocht trópusi orvostani intézet, Hamburg	150	21
6	Paul-Ehrlich intézet, Langen	147	30
7	Német rákkutató központ, Frankfurt	141	31
8	Kemoterápiai kutatóintézet, Frankfurt	119	34
9	Frankfurti egyetem	107	66
10	Német főemlős-kutatóközpont, Göttingen	102	34

A müncheni AIDS kutatók aktívabbak, a hamburgiak hatékonyabbak

Az orvosbiológiai kutatás vezető nemzetközi folyóirataiban 1990 január és 1993 március között mintegy 20000 szakcikk szerepel, melyek a a HIV-virus és az AIDS-betegség kutatásával foglalkoznak. Német kutatók ehhez évente 250 közleménnyel járultak hozzá.

A legtöbbet publikáló intézmények a Müncheni egyetem, a Freie Universität Berlin, és a Frankfurti egyetem. A müncheni és berlini kutatókat egyben legtöbbször idézik is, úgyhogy ők foglalják el a legbefolyásosabb kutatóintézetek rangsorában az első helyeket. A leghatékonyabbak azonban a hamburgi Bernhard-Nocht trópusi betegségek intézete és a Frankfurt-Langen-i Paul-Ehrlich intézet. Ezek az AIDS-kutatásban viszonylag kevés publikációval szép sikereket értek el.

(Folytatás a következő oldalon)

A rekordot egy Ralf Schreck és Patrick Bäuerle professzorok körül csoportosuló müncheni kutatócsoport tartja a HI-vírus szaporodásánál jelenlévó aktiváló transzmittensekről szóló munkával. Már másfél évvel a megjelenése után 129-szer idézték.

Az aktívak

	Intézmény	Publikációk száma
1	Müncheni egyetem	102
2	Freie Universität Berlin	81
3	Frankfurti egyetem	66
4	Szövetségi közegészségügyi hivatal, Berlin	51
5	Mainzi egyetem	42
6	Német főemlős-kutatóközpont, Göttingen	34
7	Kemoterápiai kutatóintézet, Frankfurt	34
8	Heidelbergi egyetem	32
9	Német rákkutatóközpont	31
10	Paul-Ehrlich intézet, Frankfurt-Langen	30

A hatékonyak

	Intézmény	Egy publikációra eső idézetek száma	Publikációk száma
1	Bernhard-Noch trópusi orvostani intézet, Hamburg	7,1	21
2	Max-Planck orvostani kutatóintézet, Heidelberg	6,0	12
3	Paul-Ehrlich intézet, Frankfurt	4,9	30
4	Német rákkutató központ, Heidelberg	4,5	31
5	Max-Planck genetikai intézet	4,2	12
6	Mainzi egyetem	4,0	42
7	Szövetségi közegészségügy hivatal, Berlin	3,9	51
8	Max-Planck biokémiai intézet	3,5	11
9	Kemoterápiai kutatóintézet, Frankfurt	3,5	34
10	Behringművek, Marburg	3,3	12

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(Forrás: Institut für Wissenschaft- und Technikforschung, Bielefeld, a Science Citation Index alapján)

Készült az Argumentum Könyv- és Folyóiratkiadó Kft. nyomdájában
Felelős kiadó: az MTAK főigazgatója