

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.]

A tartalomból:

The limitations and the virtues of citation analysis.....	1
What lingua franca?.....	2
Consolation for the scientist.....	3
Max Planck expands toward the east.....	8
Drawbacks of peer review.....	9
Az MTA Könyvtár szakirodalmi szolgáltatásairól.....	10



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Szerkesztők:

Braun Tibor (főszerkesztő)
 Schubert András (szerkesztő)
 Toma Olga (munkatárs)
 Zsindely Sándor (főmunkatárs)

Postacím:

MTA Könyvtára
 1361 Budapest Pf. 7
 Telefon: 111-5433
 Telefax: 131-6954
 Telex: 224132
 E-mail: h1533bra@ella.hu

Megjelenik havonta
 Évi előfizetési díj: 2400 Ft + 10% ÁFA

Scientists Should Understand The Limitations As Well As The Virtues Of Citation Analysis

Two letters challenging the function and value of citation analysis arrived in my office recently, and I believe they merit a response for all of *The Scientist's* readers to ponder. Monitoring the scientific literature and developing statistics on the extent to which articles are referenced by subsequent authors have been consuming interest of mine for more than 30 years. I have consistently maintained that it is both intellectually worthwhile and professionally beneficial for scientists to understand the limitations as well as the virtues of the field of scientometrics.

One of the letters echoes a surprisingly widespread notion that citation analysis encourages mediocrity, or "consensus science." The writer asks: "If success is measured by citation analysis, won't scientists shy away from original and venturesome fields that don't produce strong citation records?" That is, will scientists abandon an exciting, potentially ground-breaking project or area of study that, when reported on, is unlikely to gain immediate attention from other researchers?

For responsible, intellectually honest scientists, the notion of statistics functioning as a determinant, rather than a reflection, of behavior is absurd. Of course, a few dubiously motivated researchers, hoping to grab in any way they can the attention of department heads or potential employers, will seek out any device that may enhance their name recognition and beef up their c.v.'s. But this is clearly an abuse of citation analysis. Blaming a measurement method for its abuses is like blaming a badly used slide rule for the collapse of the Tacoma Bridge.

The truth is, citation analysis, like peer review, can deliver a variety of messages — good, bad, or indifferent. It's a valuable indicator; it's one way of revealing where the action is in science; it's not a directive to the research community to drop what it's doing and suddenly switch focus.

It should be noted that, over the years, *Current Contents* has identified through citation frequency analysis thousands of published articles that attracted a relatively high number of references — works we call "Citation Classics." However, along with the many strikingly "original and venturesome" articles that drew immediate attention were hundreds that the science establishment at first refused to acknowledge. They weren't on the "hit" charts at all. Indeed, leading journals such as *Nature*, *Science*, and the *New England Journal of Medicine* rejected many manuscripts that ultimately were published elsewhere. Over time, they went on to be highly cited and very influential. Citation analysis is a messenger. It doesn't "prescribe" anything; it merely "describes." If thousands of scientists around the world suddenly start to "shy away" from inspired, authentically driven research — don't blame this messenger!

The second communication, from an academic scientist in Arizona, urged me to "stop writing such nonsense." He was referring to an article on page 14 of the April 19, 1993, issue of *The Scientists*, which listed the most cited — "hot," in our terminology — papers of 1992. He was particularly aggrieved that most of the papers had been published early in 1992, thus giving them more time than others to be references in subsequent works. Well, that's true — but we make note of that in our text. Anyone who has studied the Institute for Scientific Information's data over the years knows enough to make allowances for this obvious qualification.

(Continued on next page)

On the other hand, we've learned from experience that a high percentage of papers in the annual citation "hit parade" go on to be cited in an extraordinarily large number of publications over the long run.

Again, our effort is to present a useful and interesting indicator of trends and achievement — and what we do present is incontrovertibly true. We present the facts, the numbers — the "stats", if you will. What others do with those facts is a function of their intellectual curiosity, their wit, their "human interest" in the activity of the international scientific community, and their fascination with what is transpiring in their own disciplines and the ones that may be far afield.

To my two correspondents: Citation analysis provides a perspective for examining scientific activity; only if it is misunderstood and misused could it possibly discourage scientific inventiveness or justify its characterization as "nonsense."

E. Garfield
The Scientist, (28 July, 1993)

What lingua franca?

In your coverage of "Science in Japan" [1] you refer to the language barrier of "Japan's full participation in international science". I believe that this represents only a Western view. In terms of total number of native speakers of a language, English comes a distant second to Chinese. In Asia alone, native speakers of Chinese live in China, Taiwan, Thailand, Hong-Kong, Malaysia and Singapore. Most foreign students studying in Japan are also natives of these countries, as is also the case in Korea. Furthermore, the Japanese language boom is also seen predominantly in these countries. Because of the cultural relationships of the Chinese and Japanese languages, ethnic Chinese students find it easier to learn Japanese than English.

To identify a simplified universal language of science and communication, Margaret Mead noted that "there might be a written language to permit the visual representation of ideas, independent of existing languages, rather like the relationship of Chinese script with the various spoken languages of China" [2]. How the Japanese language fits this description can be deduced as follows: (1) the Chinese use approximately 5000 *kanji* ideograms; (2) the Japanese have reduced the *kanji* ideograms from 5000 to nearly 2000 for their daily use; (3) according to Daub *et al.* [3], a foreigner can comprehend 90 percent of the technical literature in Japanese if he or she can recognize 500-600 *kanji* ideograms.

For students whose native tongues are Chinese and Korean, this simplicity makes the Japanese language a useful tool to learn science.

Sachi Sri Kantha

Department of Enzymes & Metabolism, Osaka BioScience Institute, Osaka, Japan

[1] *Nature* 359, 573-582 (1992)

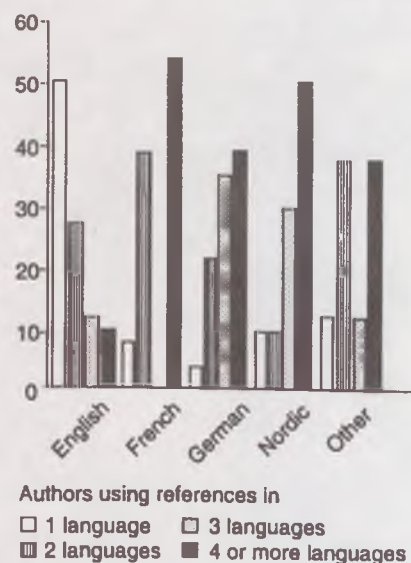
[2] *New Scientist* 23 December 1971, 225

[3] Daub, E.E., Bird, R.B., & Inoue, N. *Comprehending Technical Japanese* (University of Tokyo Press, 1975)



There is a general trend to adopt English as a lingua franca in scientific communication. The director of the Library of Geological Sciences of the University of Barcelona, Mr. Jordi Casadellà, and I are trying to analyse the actual situation of

English as lingua franca in geological sciences. To this end we have taken a sample of the recent geological journals and books to see how fast English is becoming the exclusive, or at least the main, language used in geological publications. In Western European countries in which the language of the people is not English, the geological papers in reputable scientific journals are not predominantly English. At the same time, most English



speaking scientists tend almost exclusively to cite English-written scientific literature. This is a serious problem because it seems evident that a full appreciation of the breadth of geological knowledge makes necessary a knowledge of the scientific production of Western European countries, written mainly in French and German but also in Spanish and Italian and other languages. In geological sciences, the evidence that an author does not read languages other than English is a sign that his information is significantly incomplete.

Salvador Reguant
University of Barcelona, Faculty of Geology,
Barcelona, Spain

Nature, 361:107 (14 January 1993)

Consolation for the scientist: Sometimes it is hard to publish papers that are later highly-cited

A fenti, sokunk számára vígasztaló címet Juan Miguel Campanario adta cikkének [*Social Studies of Science*, 23 (1993) 342-62]. A címben foglalt következtetést az alábbiakban sorra vett 35 példa támasztja alá. Az (i)-(xvi) példákat a korábbi irodalomból gyűjtötte a szerző, míg az (1)-(19) eseteket a *Current Contents* 1989-1990 évi *Citation Classics* kommentárjai alapján válogatta össze. (A *Citation Classics* fogalmát és definícióját illetően lásd pl. az *Impakt* 1991. májusi próbaszámát.) Ez utóbbiakat a felmerült problémák jellege szerint csoportokba is sorolta. A következtetéseket és tanulságokat illetően az eredeti cikke utalunk.

A korábbi irodalomból vett példák

(i) Working with yeast, Professor Roger J. Williams discovered one of the B vitamins in 1919. His commentary recalls a letter from an editor of the *Journal of Biological Chemistry*, advising him to attack the subject of vitamins in a more realistic way, using laboratory animals. As Williams wrote "if I had followed his advice, many of the vitamins we have known for 30 years might still be undiscovered" [2]. An account of scientific resistance to discovery of vitamins can be found in a paper by Aronson [3].

(ii) A highly-cited paper by Raymond P. Ahlquist on adrenotropic receptors, published in the *American Journal of Physiology* [4], was originally rejected by the *Journal of Pharmacology and Experimental Therapeutics*. As Ahlquist has recognized [5], his paper was finally published in part due to his personal friendship with the physiologist W.F. Hamilton. Scientists ignored this paper for five years. As of 1958 the citations began to grow, and the paper has continued to receive 50 to 120 citations per year [6]. It has been cited over 2400 times since 1948 [7].

(iii) Another surprising instance is a letter to the editor by Sen-itiroh Hakomori which has been cited more than 3200 times — one of the 100 most-cited papers of all time [8]. The paper was first submitted to the *Journal of Biochemistry (Tokyo)*, but received strong criticism from a pre-print reviewer. Hakomori withdrew it, but a colleague advised him to resubmit the manuscript; it was finally published as a letter.

(iv) A report of Rosalind Yallow submitted to the *Journal of Clinical Investigation* was initially rejected, although it was based on the research that subsequently earned her the Nobel Prize. She is one of the few scientists who has since had the opportunity of writing to other editors who rejected her papers saying: "You may not become as famous as [editor] in being identified in a Nobel lecture, but you are on the right track" [9].

(v) In June 1937, *Nature* rejected Hans Krebs's letter describing the citric acid cycle, and a full article appeared two months later in *Enzymologia* [10]. Krebs won the 1953 Nobel Prize in physiology or medicine for this work, and has published an account of the episode [11]. As has since been pointed out, "the rejection of Hans Krebs's discovery of the tricarboxylic acid (or Krebs's) cycle, a pivot of biochemical metabolism, remains *Nature's* most egregious error" [12].

(vi) *Nature* initially rejected a paper by the 1988 Nobel Prize winner Harmut Michel, based on that research, that has since been cited 140 times [13]. A cluster analysis has subsequently identified the paper as a core document in several research fronts [14].

(vii) A Royal Society referee rejected a paper by Waterston on a new molecular theory of gases, thinking that "the paper is nothing but nonsense". As a result, Waterston's work lay in utter oblivion until rescued by Lord Rayleigh forty-five years later [15].

(viii) The most cited paper ever published in *Cytogenetics and Cell Genetics* was rejected when the author Arthur D. Riggs, submitted it to one of the leading journals on molecular biology [16]. This paper proposed that DNA modification by enzymatic methylation was important for the X-inactivation process, and also suggested a new somatically heritable, information-coding system based on methylation patterns. A review on this topic by Riggs and Aharon Razin has been cited over 685 times [17].

(ix) While his doctoral dissertation was being typed, Harland G. Wood discovered that CO₂ is used as a substrate by heterotrophic bacteria. He considers this discovery to be the most significant contribution to his scientific career, since it destroyed an erroneous dogma then held by biochemists. Wood told his thesis director, Professor C.H. Werkman, that he wanted to rewrite the thesis, only to be told: "the thesis is all typed except the bibliography; we don't want to type it again" (!). The discovery was finally presented in microbiology meeting in 1935 [18].

(x) In 1943, David Nachmansohn and A.L. Machado were the first researchers to observe the enzymic acetylation obtained in a soluble system in which the free energy of ATP hydrolysis was used. However *Science*, *Journal of Biological Chemistry* and *Proceedings of the Society of Experimental and Biological Medicine* all refused to publish a paper reporting this crucial finding [19].

(xi) Peters and Ceci had problems in publishing their paper [1]. After a long delay, the paper was rejected by *Science*, and then by *American Psychologist*. Submission was encouraged by the editor of *Behavioral and Brain Sciences* and, after major revisions, was accepted for publication [20]. According to the *Social Sciences Citation Index* and the *Science Citation Index*, this paper has been cited over 100 times.

(xii) British biochemist Robert H. Michell developed the idea that receptor-triggered inositol lipid hydrolysis is a transmembrane-signalling reaction that causes the mobilization of Ca²⁺ within cells by hormones. This finding was sent to *Nature* and rejected. Michell

then presented his new idea as the central conclusion of a review paper for *Biochemica and Biophysica Acta* [21], since cited over 1835 times [22]. Another Michell article *Nature* did not want to publish appeared in an invited paper, cited over 325 times since 1981 [23].

(xiii) W. Neal Burnette discovered a method whereby specific antigens can be distinguished in a complex protein mixture. He submitted a manuscript to *Analytical Biochemistry*, but reviewers rejected the paper. As Burnette tells us, "the few pre-prints sent to colleagues seemed to have undergone logarithmic Xerox multiplication" [24]. He began receiving phone calls from researchers unable to read the umpteenth photocopied generation of the pre-print. Finally, *Analytical Biochemistry* agreed to publish the paper; it appeared in 1981 [25], and has been cited in more than 3810 publications [24].

(xiv) The law of the conservation of energy (the first law of thermodynamics) was reported by J.R. Mayer in 1842. Mayer's paper was rejected by the leading physics journal *Annalen der Physik*, and was eventually published in a relatively obscure chemical journal [26]. According to Ziman [27] the paper almost entirely ignored by physicists and, possibly as a result of this, Mayer suffered a mental breakdown which he never recovered.

(xv) In 1983, a paper by Michel J. Berridge on inositol triphosphate and diacylglycerol as second messengers was rejected by *Nature*; it was accepted by the *Biochemical Journal*, published in 1984 [28], and has been identified in a study of "hot" articles that were highly cited within two years of their publication [29]. With a citation tally of 1938, the paper ranks number 275 of the most cited papers of all time [22]. Berridge later received the Feldberg Award in 1984, the King Faisal International Prize in Science in 1986, the Louis Jeantet Prize in Medicine in 1986, and has shared the 1989 Lasker Award for basic research with Alfred G. Gilman and Yasutomi Nishizuka [30].

(xvi) The most cited paper in the story of science is that by Oliver H. Lowry, N.J. Rosebrough, A.L. Farr and R.J. Randall on protein measurement with the Folin phenol reagent [31]. This paper has been cited more than 187,000 times (!); the next most cited paper only scores 59,000 [8]. Lowry's paper was not rejected but, in an autobiographical account, Lowry recalls that, after its first submission to the *Journal of Biological Chemistry*, it was returned by the editors for drastic shortening. According to Lowry, "this shortening may have improved the paper, but forced us to omit some details that perhaps would have lessened the plethora of papers by others describing improvements and precautions" [32].

Citation Classics példák

(a) Pre-publication difficulties.

(1) V.J. Balcar and G.A.R. Johnston, The Natural Specificity of the High Affinity Uptake of L-glutamate and L-aspartate by Rat Brain Slices, *Journal of Neurochemistry*, 19 (1972) 2657

In 1972, Vladimir J. Balcar and his PhD supervisor, Graham A.R. Johnston, published a study on about two-hundred compounds, selected on the basis of their pharmacological characteristics and chemical structure. These compounds were tested against high affinity uptake of L-glutamate by brain slices. This work demonstrated that glutamate uptake and glutamate receptors were two distinct entities (not obvious in 1972). In this commentary Balcar recalls that "some difficulties were encountered with getting the manuscript past the head of the department", although it was quickly accepted by the *Journal of Neurochemistry*.

(2) I. Gupta and J.C.M. Li, Stress Relaxation, Internal Stress, and Work Hardening in Some Bcc Metals and Alloys, *Metal Transactions*, 1 (1970) 2323

Another paper which had difficulty before publication was the Gupta and Li paper on stress relaxation, internal stress and work-hardening in metals and alloys. In 1940, E. Orowan had proposed an equation relating the strain rate to dislocation velocity, but in 1970 this equation still had not been verified experimentally. As part of Gupta's thesis, the authors attempted to verify it. In their paper, the stress-time relation during stress relaxation is interpreted in terms of a power relation between dislocation velocity and effective stress. The work was supported by US Steel and, before publication, all papers had to be approved by their scientific advisors. The US Steel management advised Gupta and Li to shorten the manuscript, and make changes in the section on materials and specimen preparation. Since they did not follow all these suggestions, the manuscript was not approved. US Steel then suggested that the authors submit the manuscript without mentioning the company, and without its approval. The paper was finally submitted with the approval of Columbia University, and it was published without revisions.

(b) Difficulties during the revision phase

(3) M.S. Paterson, X-ray Diffraction by Face-Centered Cubic Crystals with Deformation Faults. *Journal of Applied Physics*, 23 (1952) 805

In 1951, while he was spending a year at the Institute for the Study of Metals of the University of Chicago, Mervyn S. Paterson proposed to sort out the theory of stacking faults in deformed metals. In his 1952 paper, the X-ray diffraction effects were calculated for face-centered cubic crystals with stacking faults. The positions, widths and intensities of the X-ray diffraction lines were predicted as a function of fault density. The referee from the *Journal of Applied Physics* told Paterson many years later that he had thought of recommending rejection, believing that the predicted effects of the theory would not be observable. Curiously, he and one of his students were the first to publish experimental observations that fitted the theory.

(4) L.B. Robinson and E.J. Wampler, The Lick Observatory Image-Dissector Scanner, *Publications of the Astronomical Society of the Pacific*, 84 (1972) 161

A reviewer of the *Publications of the Astronomical Society of the Pacific* almost rejected the most cited paper from the journal. It was co-authored by L.B. Robinson and E.J. Wampler (Lick Observatory, University of California), who were trying to increase the effectiveness of a telescope. Two expensive ways of doing this are to increase the size of the telescope, and to improve the associated optics. Instead, Robinson and Wampler developed a less expensive computer-controlled device that can simultaneously examine several hundred wavelength intervals, improving the performance of the detector that records the photons collected by the telescope. They experimented with image-intensifier tubes, but the reviewer thought that image tubes could not provide quantitative data, and initially rejected the paper. Some colleagues also suggested that Robinson and Wampler should "stop developing new gadgets and do some real astronomy". Eventually, their device became the most-used instrument at Lick, until the new sensors became competitive. The authors believe that the high number of citations represents those astronomers who have used these scanners at various observatories.

(5) H.J. Hsü, Principles of *Mélanges* and their Bearing on the Franciscan-Knoxville Paradox, *Geological Society of America: Bulletin*, 79 (1986) 1063

Although it is hard to believe, in 1984, Kenneth J. Hsü received the highest award of the American Geological Society, the Wollanston Medal, for a 1979 paper initially rejected by a reviewer of *The Geological Society of America Bulletin*. It was accepted only after the author agreed to publish it in a section for trivial communications. In this paper Hsü formulates new principles and proposes a non-Smithian stratigraphy to guide students of the *mélanges* that are found in mountain ranges all over the world. His new ideas were not easily accepted by the establishment. Hsü points out "for more than a decade, I was pictured as a Don Quixote waving my lance against a windmill that was the US Geological Survey". But young geologists adopted Hsü's principles and, ironically, the non-acceptance of his ideas increased the number of citations to the paper, because the word *mélange* appeared in few textbooks.

(6) M. Grunberg-Manago and S. Ochoa, Enzymatic Synthesis and Breakdown of Polynucleotides: Polynucleotide Phosphorylase, *Journal of the American Chemical Society*, 77 (1955) 3165

The Spanish Nobel Prize winner Severo Ochoa had difficulties in publishing a paper with M. Grunberg-Manago on polynucleotide phosphorylase (PNPase). This is revealed by Grunberg-Manago in his commentary on a 1956 paper published in *Biochimica and Biophysica Acta*. The problematic paper was published in the *Journal of the American Chemical Society* while Grunberg-Manago was working with Ochoa in the Department for Biochemistry of the New York University of Medicine. This paper has also been highly cited. However, the reviewer was strongly critical and Ochoa had to argue for the paper to be published. PNPase was the key to the development of modern molecular biology, and it is still used in medical studies on interferon, cancer, AIDS and to synthesize long polymers in protein synthesis. The Nobel Prize in 1959 awarded to Ochoa, and the Charles-Leopold Mayer prize awarded to Grunberg-Manago, finally recognized its importance. An account of this episode can be found in a paper by Ochoa [33].

(7) W.D.M. Paton and E.S. Vizi, The Inhibitory Action of Noradrenaline and Adrenaline on Acetylcholine Output by Guinea-Pig Ileum Longitudinal Muscle Strip, *British Journal of Pharmacology* 35 (1969) 10

W. Feldberg, chief editor of the *British Journal of Pharmacology*, allowed publication of the first paper reporting neurochemical evidence for a fundamental type of "cross talk" between neurons. A referee was sceptical of the findings of W.D.M. Paton and his Hungarian postdoctoral assistant, E. Sylvester Vizi, which provided neurochemical evidence for the existence of presynaptic alpha-receptors and functional interaction between neurons. But this paper laid the ground for later studies of physiological control of disinhibition and by negative feedback, and several drugs have been developed according its findings.

(8) K.M. Anderson and S. Liao, Selective Retention of Dihydrotestosterone by Prostatic Nuclei, *Nature* 219 (20 July 1968) 277; S. Liao and S. Fang, Receptor Proteins for Androgens and Mode of Action of Androgens on Gene Transcription in Ventral Prostate, *Vitamins and Hormone-Advances Research Applications*, 27 (1979) 17

Shutsong Liao and his graduate student, Kenning M. Anderson, at the University of Chicago, also encountered difficulty in convincing the reviewers of *Nature*. Their work addressed the retention of dihydrotestosterone (DHT) in rat prostate cell nuclei. DHT comes from testosterone that is converted by a reductase. Selective binding of DHT to an androgen receptor explains its retention. As Liao and Anderson say in their commentary, the finding was unusual and very important, but reviewers did not agree on its significance. Nor did they approve the use of the term "steroid receptor". In fact, a section of another paper of S. Liao and S. Fang on DHT and androgen receptor was removed (one could say "sensored") at the editor's suggestion. The importance of the relation testosterone-DHT-receptor is now well-known. New drugs based on these findings are being developed for treatment of acne, prostate diseases, female hirsutism and male pattern baldness.

(9) J.P. Sutherland, Multiple Stable Points in Natural Communities, *American Naturalist*, 108 (1974) 859

In contrast to the above, John P. Sutherland's difficulties were not particularly important. Sutherland investigated the way in which some events, which determine the presence and absence of important consumers, influence structure in natural communities. He

submitted a paper to *American Naturalist*, and it was provisionally accepted. However, the editor suggested that Sutherland replace the data tables with a diagram. In his commentary, Sutherland says that he resisted because he couldn't think of a way to plot percentage data that had been transformed into arcsines. The solution was to transform the y axis, and the manuscript was finally accepted.

(c) *Authors encountered strong resistance*

- (10) W.R. Gove, The Relationship between Sex Roles, Marital Status, and Mental Illness, *Social Forces*, 51 (1972) 34

As Walter S. Gove recognizes, his 1972 paper on differences in rates of mental illness between men and women ran counter to accepted notions in the social-science community. At that time, it was assumed that there were no such differences. Gove personally interviewed 458 mental patients. He concluded that women did have higher rates of mental illness than men, and married women than married men. He also found that among the non-married, divorced and widowed, rates of mental illness were as high or higher in men than in women: women's higher rates can thus be due to the married women's role. However, it was generally believed that higher rates of reported symptoms among women only reflected their willingness to report them. As Gove remarks in his commentary, these preconceptions may have accounted for his difficulties. He feels that the negative critiques of some reviewers did not reflect the paper's quality. Its hypothesis is now largely accepted.

- (11) J.G. Vos, J.H. Koeman, H.L. van der Maas, M.C. ten Noever de Brauw and R.H. de Vos, Identification and Toxicological Evaluation of Chlorinated Dibenzofuran and Chlorinated Naphthalene in Two Commercial Polychlorinated Biphenyls, *Food and Cosmetics Toxicology*, 8 (1970) 625

Paper published by Jan Koeman's research team at the University of Utrecht reported the high toxicity of polychlorinated dibenzofurans (PCDFs) in two commercial polychlorinated biphenyls (PCB) mixtures. This paper was submitted to *Nature* as an article, but journal staff informed the authors that they should write a short letter to the editor, examining just one aspect, because they could not offer space for the full report. The authors preferred to send their paper to *Food and Cosmetics Toxicology*. Curiously, *Nature* later published a paper by other authors that confirmed the findings of Koeman's team [34]. The latter has been cited 105 times.

- (12) R.J. Gelles, Child Abuse as Psychopathology: A Sociological Critique and Reformulation, *American Journal of Orthopsychiatry*, 43 (1973) 611

When he was a fourth-year graduate student in sociology at the University of New Hampshire, Richard J. Gelles took a course on family violence with the sociologist Murray A. Strauss. Strauss assigned Gelles to review some articles on child abuse which used a psychopathological explanation. Gelles was not convinced, and Strauss encouraged him to write a critical article. Using the same data on child abuse, Gelles suggested a multi-dimensional social-psychological explanation. It was not easy to communicate his new interpretation. First, Gelles submitted his paper to an American Sociological Association meeting. It was accepted, but scheduled for a final catch-all session on the last day, and only Gelles's wife was present. The paper was then rejected by the first journal to which Gelles submitted it. The *American Journal of Orthopsychiatry* eventually accepted the paper, and it has been much cited. As Gelles points out, these difficulties are not surprising, giving the extent of his re-interpretation of data on a sensitive topic.

- (13) M. Cohn and T.R. Hughes, Nuclear Magnetic Resonance Spectra of Adenosine Di- and Triphosphate, II: Effect of Complexing with Divalent Metal Ions, *Journal of Biological Chemistry*, 237 (1962) 176; M. Cohn and T.R. Hughes, Phosphorus Magnetic Resonance Spectra of Adenosine Di- and Triphosphate, I: Effect of pH, *Journal of Biological Chemistry*, 235 (1960) 3250

In this case, a paper's rejection led the authors to publish two reports. Mildred Cohn and the physics graduate student Tom Hughes obtained high-resolution ^1H and ^{31}P -NMR spectra of some diamagnetic ions complexes of both ADP and ATP, showing a change in chemical shift for the β -P of ATP. They also investigated the effect of pH in NMR spectra of ATP. They submitted a paper to the prestigious *Journal of the American Chemical Society*, but the manuscript was rejected. They then published two papers in the *Journal of Biological Chemistry*. The chemical shift change in β -P of ATP has been widely used to determine the free Mg^{2+} concentration *in vivo* in cells and in animal organs.

- (14) A.D. Baddeley, Short-term Memory for Word Sequences as a Function of Acoustic Semantic and Formal Similarity, *Quarterly Journal of Experimental Psychology*, 18 (1966) 362

The most-cited paper from the *Quarterly Journal of Experimental Psychology* was first rejected by the *Journal of Experimental Psychology*. It was written by A.D. Baddeley while he was working at a Medical Research Council centre, on a grant from the British Telephone authorities. He was trying to contrast the effect of acoustic similarity with that of similarity of meaning in the short-term recall of words. He found that memory performance was lower when words were similar in sound, but that meaning similarity had no effect; he suggested that short-term memory, in contrast to long-term memory, relies on acoustic coding. His findings were rejected by the *Journal of Experimental Psychology*. The referee suggested that the "author should do more parametric studies". Baddeley's later work continued to focus on short-term memory. He proposed the very important concept of working memory, and it is now widely used [35].

(15) J.D. Rowley, Identification of a Translocation with Quinacrine Fluorescence in a Patient with Acute Leukemia, *Annales Génétiques de Paris*, 16 (1973) 109

Again, the most-cited paper of one journal was first rejected by another journal. In this case, Janet D. Rowley's paper on chromosome translocation was submitted as a short letter to the prestigious *New England Journal of Medicine*, which rejected it. As she just received a form letter, Rowley decided to phone and ask why the paper had been rejected. She was told that it was unimportant. Luckily, Jean de Grouchy, editor of *Annales de Génétique*, did not think so, and agreed to publish the paper. It was one of the very first reports of the discovery of a translocation in malignant cells.

(16) G. Pontecorvo, Production of Mammalian Somatic Cell Hybrids by Means of Polyethylene Glycol Treatment, *Somatic Cellular Genetics*, 1 (1975) 397

Resistance to new ideas is also illustrated by the response of a referee to a paper describing some experiments carried out by Guido Pontecorvo at the Imperial Cancer Research Fund Laboratories. In short, Pontecorvo's idea was to use polyethylene glycol (PEG) for fusion of mammalian somatic cells in culture. It was well-known that this botanical technique could also work with animal cells. He was right, and PEG is now used generally for (*inter alia*) monoclonal antibody development. However, his short note was rejected by the *Proceedings of the Royal Society*, because the referee did not accept the significance of Pontecorvo's new idea: "the paper submitted does not permit one to decide whether PEG will prove to be no better than others that have been tried and rejected".

(17) I. Ofek, D. Mirelman and N. Sharon, Adherence of *Escherichia coli* to Human Mucosal Cells by Mannose Receptors, *Nature*, 265 (17 February 1977) 623

In this case, *Nature* rescued a paper that was first rejected by *Science*. It was co-authored by I. Ofek, D. Mirelman and Nathan Sharon, and it demonstrates that *Escherichia coli* binds to epithelial cells by a mannose-specific lectin present on the bacterial surface. It also points out the importance of lectin-carbohydrate interactions in the initiation of infection. However, the *Science* referee thought that there was nothing new in it, and recommended rejection. It was then submitted to *Nature*, quickly published, and its findings confirmed in other laboratories. The authors believe that the paper has been highly cited because it stimulated much research on bacterial adherence. Unfortunately, no human application has yet been found.

(18) P.A. Werner, Predictions of Fate from Rosette Size in Teasel (*Dipsacus fullonum* L.), *Oecologia*, 20 (1975) 197

Pursuing her doctoral research at the Kellogg Biological Station of Michigan State University, Patricia A. Werner published her 1975 paper on field population of a biennial plant species. Its main conclusion was that the probability that an individual either died, remained vegetative or flowered in any growing season is highly correlated with the size of its vegetative rosette in the preceding year. These ideas, new in 1975, are well known and fully accepted today. The referee for the first journal to which Werner sent her paper thought that her findings were trivial, and advised rejection. *Oecologia* finally accepted the paper, although their referee asked Werner to cite a prestigious plant ecologist who was actually using her own unpublished work!

(19) H.C. Ellinghausen and W.G. McCullough, Nutrition of *Leptospira pomona* and Growth of 13 other Serotypes: Fractionation of Oleic Albumin Complex and a Medium of Bovine Albumin and Polysorbate 80, *American Journal of Veterinary Research*, 26 (1965) 45

Practically all leptospiral bacteria producers use bovine albumin polysorbate 80 as a growth medium. It was first proposed by Herman C. Ellinghausen and W.C. McCollough, in a paper published in 1965 in the *American Journal of Veterinary Research*. It was earlier rejected by a major bacteriological journal, on the grounds that it was insignificant and lacking originality. The referee argued that bovine albumin had been used in studies fifteen years before. This growth medium, in the US in 1964, made the first isolation of *Leptospira grippotyphosa* possible, and led to great progress in the isolation of *Leptospira Hardjo*, which it is almost impossible to cultivate in other media.

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Indian libraries pool science journal subscriptions

Universities and research laboratories in India, forced by lack of funds to cut out subscriptions to foreign journals, are now networking their science libraries.

Under this scheme, no two libraries will import the same periodical, and all journals will be pooled. A scientist who cannot find the journal he wants on his own campus can get a photocopy of any article he needs from another library in the network.

Researchers complain that shrinking libraries are slowing down their work. The number of foreign journal subscriptions at Indian science libraries has fallen from nearly 22,000 in 1983 to 9,500 as a result of budget cuts, increased prices and erosion of the value of rupee.

For example, the Indian Institute of Science in Bangalore and the Tata Institute of Fundamental Research in Bombay have reduced their subscriptions by a third and the Indian Institute of Technology (IIT) in New Delhi has dropped half the foreign journals on its subscriptions list. The situation is even worse in universities and in the 40 laboratories of the Council of Scientific and Industrial Research, which says it has funds only to pay salaries.

"Resource sharing through computer interconnection is the only way to cope with the information drought in academic and research institutions", says Dr. T. Viswanathan, director of the Indian National Scientific Documentation Centre (INSDOC), which is leading the network movement. The first network, which started last month in Madras, already connects ten libraries. They exchange the contents pages of periodicals, and scientists order the articles they want electronically. A courier service run by the network centre delivers a photocopy within 48 hours.

"By paying only a \$7,000 annual membership fee, a library has access to all the foreign journals collectively subscribed to by all the network members", says Viswanathan. The libraries

save about \$200,000 a year by not duplicating subscriptions. According to Viswanathan, all the 60 science libraries in and around Madras will have joined the network in the next few months. In Delhi, 28 libraries have formed a network and have saved \$170,000 by pooling journals. Similar networks are being set up in Bangalore and Calcutta.

Researchers whose libraries do not belong to a network are being encouraged to subscribe to INSDOC's Contents Abstracts and Photocopying Service (CAPS). For a nominal fee, subscribers receive every month the content pages of up to 40 journals of their choice by fax or e-mail, and can then order abstracts or complete articles. INSDOC's own library can service 20 per cent of requests. "If we don't have the journal, we get it from a library that has", says Viswanathan.

INSDOC has regional offices in Calcutta, Bangalore and Madras, and a van makes a daily round of local libraries to pick up journals and photocopy the articles on the spot. "If the subscriber is in Delhi and the journal is also in Delhi, the photocopy will be hand-delivered within 48 hours", says Viswanathan. "Otherwise it will take up to a month." INSDOC has links with 800 science libraries and has access to every foreign science journal imported into India.

According to Viswanathan, most CAPS users are university and industrial scientists, and more than half the requests are for articles in medicine, the life sciences and engineering, most of them from *Nature*.

But most scientists in India believe that the problems caused by lack of access to journals can be only partly solved by networking and CAPS. "Networking of national science libraries will take time", says Mohinder Singh, chief librarian of IIT. "This cannot immediately make up for the steep fall in acquisition of journals and books."

K.S. Jayaraman,
Nature, 364:4, (1 July 1993)

Max Planck expands toward the east

The Max Planck Society (MPG) has been tipped off by the federal government that funding for five new Max Planck institutes in the former East Germany may indeed be forthcoming, the general squeeze on resources precipitated by reunification notwithstanding. Last week's meeting of the MPG senate at Trier was therefore able to endorse its plans, agreed in principle last year, to set up in the new *Länder* three new institutes in the natural sciences and two in the social sciences.

Uncertainties over finance have delayed the setting up of the institutes. The federal funds provided in the 1990 reunification treaty for building up research in the new *Länder* did not stretch as far as funding all seven institutes originally earmarked. But setting up institutes virtually from scratch has also required more time than been predicted. Only two — the Institute for Microstructure Physics at Halle and the Institute for Colloid and Surface Science in Berlin — are so far established.

There are now signs that the federal government is willing to provide its share (half) of the costs of the five new institutes. Research minister Paul Krüger, himself an east German, is firmly committed to giving priority to initiatives in the new *Länder*, and in this week negotiating with finance minister Theo Waigel in preparation for the federal budget on 13 July.

The other half of the budgets of Max Planck institutes normally comes from all the German *Länder*, but the reunification treaty requires only the new *Länder* to contribute to costs for their own institutes, at least until 1995. Thus new institutes will cost western German *Länder* nothing for the time being, giving the lie to complaints that the east is "draining resources" from the west. Most of the *Länder* are said to be ready to sign contracts.

The new institutes include:

* An institute for infection biology in east Berlin, to be

established by the end of 1994. It will eventually be housed in a new building near the Charité university clinic to be built by 1998. It will have 100 positions, 35 for scientists, and an annual operating budget of around DM11 million (US\$7 million).

* An institute for molecular plant physiology near Potsdam in Brandenburg. Work will begin in 1994, but the 90 staff (26 scientists) will move into a new building in 1998. The institute will have a budget of about DM9 million a year.

* An institute for the physics of complex systems in Dresden opens next month, and moves to a new building on the campus of the Technical University in 1998. Although small (37 positions, of which 17 will be scientists), it will be one

of the few in the world dedicated to this multidisciplinary field; an extensive guest programme is planned.

* Institutes of economics (Jena) and history of science (Berlin) will increase the number of Max Planck institutes in the social sciences, which have been much neglected. The new institutes are much welcomed in the east, where unemployment among scientists has been high — and confidence low — since reunification. However, all three directors so far designated are from western Germany, matching current trends (see *Nature*, 362:685, 1993).

But the good news has not ended. MPG says that it is considering further initiatives in the new *Länder* for the second half of the 1990s.

Alison Abbott, *Nature*, 364:8, (1 July 1993)

Drawbacks of peer review

All reputable learned journals employ the peer-review system to help to decide on publication or rejection of submitted articles. Thus the system can be viewed as a method for evaluating the quality of potential publications. Every scientific method should be validated before is generally used. One essential step in this validation is the assessment of the method's reproducibility. We have attempted to define the reproducibility of the peer-review method.

Copies of one paper submitted to a medical journal were sent simultaneously to 45 experts, all of them members of editorial boards of journals relating to the subject of the submitted paper. They were asked to express their opinion of the paper on the journal's standard questionnaire judging 8 quality criteria on a numerical scale from 5 (excellent) to 1 (unacceptable). In addition, an overall judgement of the paper had to be indicated on the same scale. No reviewer was told that he or she was being tested. Thirty-one adequately filled forms were received back; 14 potential reviewers declined because they claimed to be not fully competent or had too little time to do the work within the deadline. On the basis of the individual ratings, the reproducibility was estimated by calculating descriptive statistics for each item. The results are depicted in Table 1.

They demonstrate disappointingly poor reproducibility with extreme judgements ranging from "unacceptable" to "excellent" for most criteria. Even the criterion "linguistic merit", which could have been expected to yield a fairly uniform judgement because the paper was written by two native English-speakers from the United Kingdom, was no exception. Similarly, the overall judgement rating was spread over the whole range. The paper sent out for review was by no means on a controversial subject. The results therefore suggest that the method of evaluating scientific papers by peer review is unreliable and open to bias and should itself be submitted to evaluation.

Since acceptance or rejection of a paper nowadays strongly influences the career of the authors, the poor reproducibility is a cause of concern. One would therefore hope for an improvement of the method. A system incorporating regular, blind tests concerning the quality of the reviewers might exclude some sources of publication bias [1-3]. The absence of reliability suggested by the present findings seems unacceptable for anyone aspiring to publish in peer-review journals.

E. Ernst, T. Saradeth, K.L. Resch, 363:296 (27 May 1993)

Table 1. Descriptive statistics for each quality criterion



	Frequency of ratings					q1	q3
	U (1)	A (2)	F (3)	G (4)	E (5)		
Scientific merit	2	3	7	14	5	3.00	4.00
Clarity of description	1	0	6	15	8	3.75	5.00
Statistical methods	4	2	7	11	7	3.00	4.00
Methodology	2	0	5	17	6	3.75	4.00
References	0	2	2	15	11	4.00	5.00
Quality of tables /figures	0	1	10	16	3	3.00	4.00
Discussion	1	3	8	13	5	3.00	4.00
Linguistic merit	0	1	6	16	7	3.75	4.25
Overall judgement	2	2	4	16	5	3.00	4.00

U, unacceptable (1); A, acceptable (2); F, fair (3); G, good (4); E, excellent (5).
q1 = 25 % percentile (first quartile), q3 = 75% percentile (third quartile) median and mode for all criteria; 4 (frequencies of median and mode are printed in bold)

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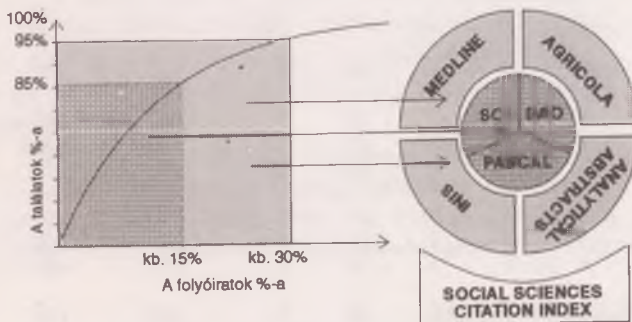
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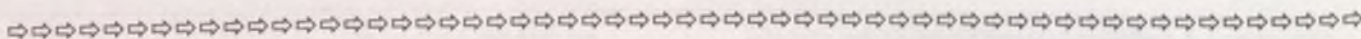
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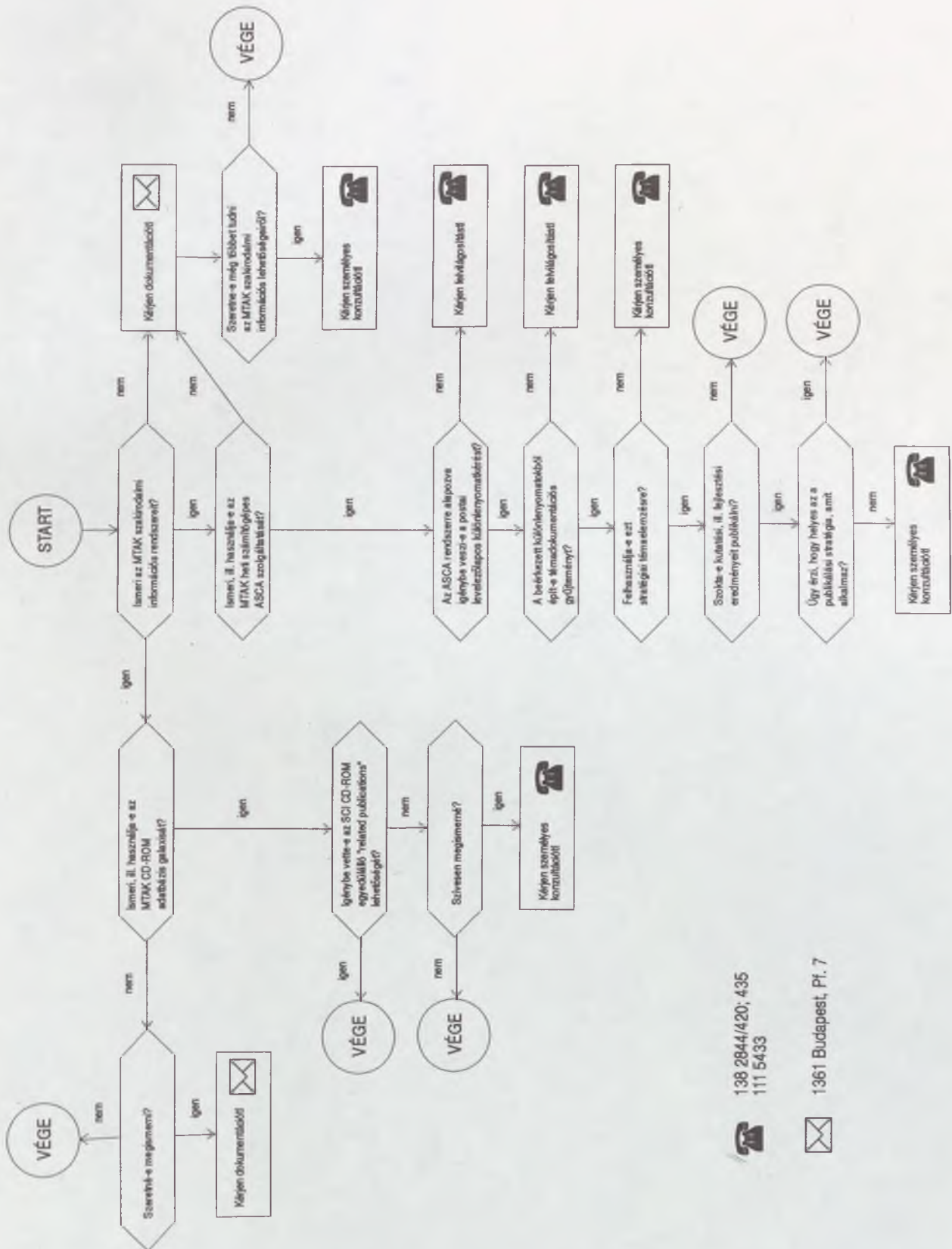
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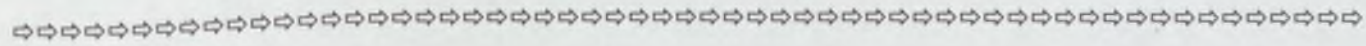




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