4. évfolyam · 9. szám · 1994. szeptember

# 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.]

## A tartalomból:



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## Cautious welcome to NIH peer review reforms

Encouraged by preliminary results, the US National Institutes of Health (NIH) are to expand an experiment in modifying the peer-review system for research grants. The goal is to reduce the time spent dealing with applications that are clearly not likely to win a grant and to increase that spent evaluating borderline cases.

Nearly 80 per cent of the reviewers who took part in an initial small trial said that they had the same level of confidence in their recommendations as under the current system. Almost 60 per cent said they would approve the new approach if some additional modifications were made.

The approach is known as "triage", after the military procedure for sorting battlefield casualties into priority of treatment. Jerome Green, director of NIH's Division for Research Grants (DRG), says that a decision on whether to extend it to the whole of NIH is likely to be made at the end of the year.

There are several reasons behind NIH's desire to modify its peer review procedures. One is to speed up the process; at present, grant applicants have to wait nine months to hear whether they have been successful, and most are disappointed. The NIH can fund fewer than a quarter of the applications it receives, and reviewers therefore spend much unpaid time evaluating applications that will be unsuccessful anyway. This has created a sense of frustration in the scientific community, making it difficult for the NIH to recruit reviewers.

Unsolicited proposals are the bedrock of biomedical research in the United States, and account for nearly half the NIH's extramural research funding ($8.5 billion in the financial year 1993). The triage approach focuses on the first of two stages in the review process, that concerned with a purely scientific review of applications. The second involves evaluating applications that pass the first stage against the research and other priorities of the individual institute.

At present, the DRG assigns all unsolicited proposals either to a study section within the division, or to a panel within an individual institute, each made up of 15 to 20 senior scientists or specialists in the field.

Before the panel or section meets, the NIH scientist responsible for its work sends every application to two reviewers. These assign a score to the application ranging from 500 (lowest) to 100 (highest), based on factors such as its significance, the appropriateness of methodology, the qualifications of investigators and the resources available in the investigators' home institution.

The scores are passed to the DRG, where they are adjusted to compensate for differences between study section members. The panel then discusses all applications eventually deciding not to recommend some for further consideration. An NIH staff member then sends unsuccessful applicants and remarks emerging from the panel discussion.

The applications that survive their scientific peers are sent to the second level of review within the individual institutes, and eventually about 20 per cent of the applications are funded. In general, successful applications have both the lowest scores from the scientific reviewers and are ranked above the fourteenth or fifteenth percentile or so of the overall range of scores.

*(continued on next page)*

Triage aims to identify as "noncompetitive" applications below the fiftieth percentile. If both initial reviewers agree with this assessment, and no other scientific reviewer disagrees, the application is not discussed further and the NIH staff member does not write a summary of the reasons for rejection.

The applicant does, however, receive the criticism of the two reviewers, thus maintaining most of the tutorial element of peer review. Of the four (out of over a hundred) study sections involved in the trial so far, only the Human Development and Aging Study Section winnowed out all those below the fiftieth percentile, which in that case constituted 51.1 per cent of applications.

The new approach has met with a generally favourable response. Keith Yamamoto, chair of the department of pharmacology at the University of California, San Francisco, who is organizing a seminar on peer review at the NIH in the autumn, says that "on balance" he is in favour of triage: "It gives more time to discuss the top proposals."

At the same time, says Yamamoto, "there is a real danger that innovative research will be triaged out". The concern stems from a suspicion that reviewers, under the pressure of tight budgets, are becoming increasingly conservative, and now favour proposals backed by significant preliminary data.

"I can't tell you how often I have read a review that says this proposal has the potential to advance the field tremendously, but it is high risk," says Yamamoto. "In fact, I've written that a few times myself."

Yamamoto believes that, in order to preserve high-risk, high pay-off science, the instruction to reviewers should explicitly require them to consider innovativeness along with other criteria. "What has made American science so enterprising is the ability of some people to move nonlinearly," he says. "We must not cut these people out of the loop."

Others are concerned that eliminating the detailed assessments of applications that unsuccessful applicants receive will remove a valuable service to the scientific community, as they are essentially free consultation with leading scientists in a field.

Green says he is aware of this, but it needs to be put in perspective. "We need to balance the need to shorten the time that scientists and NIH staff spend on each applications against the importance of maintaining the tutorial aspect of peer review," he says.

Although the triage trial is to be expanded, a significant reduction in the time it takes for applicants to hear their fate is likely to come only with a move to electronic filing and processing of grant applications. "At present, we are in the ridiculous position where the researchers will have written up their application on computer, yet we receive them and key them in, then print out numerous hard copies," says Green. Tight funding means that full implementation of electronic processing will probably take at least five years.

*Helen Gavaghan, Nature, 369:269 (26 May 1994)*

## Quantity No Longer Counts in Britain

Chalk it up a modest victory in the battle against publication inflation. Earlier this month, the councils that allocate core funding to British universities announced that they will no longer use total publication counts as a measure of the relative strengths of research departments. Instead, they will take into account only the four best papers individual researchers in each department have published in the previous 3 years. "The funding bodies wish to signal clearly that... the number of publications... is not considered necessarily to be an indicator of research quality," the councils said in a statement.

The shift has important implications. The councils — the Higher Education Funding Councils for England, Scotland and Wales and the Department of Education for Northern Ireland — will soon begin a quadrennial assessment of the quality of each university's research departments. The councils will use the results to divvy up about $200 million in block grants a year.

The first assessment took place in 1992. The councils based their rankings on information supplied by the universities about the number of research staff members and students, total publications, external funding, and plans for future research. The councils then graded each department by peer review on a scale of 1 to 5, with grade 5 getting 4 times as much funding as a rating of 2, while a rating of 1 attracts no funds. The universities of Cambridge, Oxford, and London generally received the lion's share of top grades across many subject areas.

The next assessment, to be completed in 1996, will include similar measures, with the exception of publication counts. The decision to consider only a few top papers reflects a growing concern over some researchers' frantic efforts to accumulate publications by splitting results up into series of short papers and appending their names as co-authors on as many publications as possible. "We strongly welcome the decision to drop publication counts... [This reflects] the widespread view in the academic community that publication counts are a crude and unreliable measure of research performance," says David Triesman, general secretary of the Association of University Teachers.

A few other research granting bodies and tenure committees are also trying to deemphasize publication volume as a measure of a researcher's productivity. Three years ago, for example, the U.S. National Institutes of Health (NIH) revised grant application forms to stop researchers from submitting page after page of references. Now applicants must fit biographical and publication data on just two pages. Anthony Demsey, acting deputy director of HIH's division of research grants, who was instrumental in making the change, says "We've done a certain degree of curtailing but not the same extent [as the British funding councils]."

*Claire O'Brien,*
*Science, 264:1840 (24 June 1994)*

## The Impact Factor

Librarians and information scientists have been evaluating journals for at least 75 years. Gross and Gross conducted a classic study of citation patterns in the '20s [1]. Others, including Estelle Brodman with her studies in the '40s of physiology journals and subsequent reviews of the process, followed this lead [2]. However, the advent of the ISI citation indexes made it possible to do computer-compiled statistical reports not only on the output of journals but also in terms of citation frequency. And in the '60s we invented the journal "impact factor." After using journal statistical data in-house to compile the *Science Citation Index* (*SCI*) for many years, ISI began to publish *Journal Citation Reports* (*JCR*) [3] in 1975 as part of the *SCI* and the *Social Sciences Citation Index* (*SSCI*).

Informed and careful use of these impact data is essential. Users may be tempted to jump to ill-formed conclusions based on impact factor statistics unless several caveats are considered.

*Definition*

The *JCR* provides quantitative tools for ranking, evaluating, categorizing, and comparing journals. The impact factor is one of these; it is a measure of the frequency with which the "average article" in a journal has been cited in a particular year or period. The annual *JCR* impact factor is a ratio between citations and recent citable items published. Thus, the impact factor of a journal is calculated by dividing the number of current year citations to the source items published in that journal during the previous two years (see Figure 1).

**Figure 1**: Calculation for journal impact factor.

A = total cites in 1992

B = 1992 cites to articles published in 1990-91 (this is a subset of A)

C = number of articles published in 1990-91

D = B/C = 1992 impact factor

The impact factor is useful in clarifying the significance of absolute (or total) citation frequencies. It eliminates some of the bias of such counts which favor large journals over small ones, or frequently issued journals over less frequently issued ones, and of older journals over newer ones. Particularly in the latter case such journals have a larger citable body of literature than smaller or younger journals. All things being equal, the larger the number of previously published articles, the more often a journal will be cited [4,5].

*Applications*

There have been many innovative applications of journal impact factors. The most common involve market research for publishers and others. But, primarily, *JCR* provides librarians and researchers with a tool for the management of library journal collections. In market research, the impact factor provides quantitative evidence for editors and publishers for positioning their journals in relation to the competition — especially others in the same subject category, in a vertical rather than a horizontal or intradisciplinary comparison. *JCR* data may also serve advertisers interested in evaluating the potential of a specific journal.

Perhaps the most important and recent use of impact is in the process of academic evaluation. The impact factor can be used to provide a gross approximation of the prestige of journals in which individuals have been published. This is best done in conjunction with other considerations such as peer review, productivity, and subject specialty citation rates. As a tool for management of library journal collections, the impact factor supplies the library administrator with information about journals already in the collection and journals under consideration for acquisition. These data must also be combined with cost and circulation data to make rational decisions about purchases of journals.

The impact factor can be useful in all of these applications, provided the data are used sensibly. It is important to note that subjective methods can be used in evaluating journals as, for example, by interviews or questionnaires. In general, there is good agreement on the relative value of journals in the appropriate categories. However, the *JCR* makes possible the realization that many journals do not fit easily into established categories. Often, the only differentiation possible between two or three small journals of average impact is price or subjective judgments such as peer review.

*Using the Impact Factor Wisely*

The Institute for Scientific Information (ISI) does not depend on the impact factor alone in assessing the usefulness of a journal, and neither should anyone else. The impact factor should not be used without careful attention to the many phenomena that influence citation rates, as for example the average number of references cited in the average article. The impact factor should be used with informed peer review. In the case of academic evaluation for tenure it is sometimes inappropriate to use the impact of the source journal to estimate the expected frequency of a recently published article. Again, the impact factor should be used with informed peer review. Citation frequencies for individual articles are quite varied.

There are many artifacts that can influence a journal's impact and its ranking in journal lists, not the least of which is the inclusion of review articles or letters. This is illustrated in a study of the leading medical journals published in the *Annals of Internal Medicine* [6].

*Review Articles.* Review articles generally are cited more frequently than typical research articles because they often serve as surrogates for earlier literature, especially in journals that discourage extensive bibliographies. In the *JCR* system any article containing more than 100 references is coded as a review.

*(continued on next page)*

Self-Citation study of journals in the Reproductive Systems category of the 1992 *SCI Journal Citation Reports* (*JCR*).

Table 1: Calculation of impact factors without self-citations.

| Reproductive Systems Journals | (A/D) JCR Impact Factor | A Cites in 1992 to 1990-91 Articles | B Self-cites in 1992 to 1990-91 Articles | C (A-B) Minus Self Cites | D Articles Published 1990-91 | E (C/D) Revised Impact Factor |
|---|---|---|---|---|---|---|
| AM J REPROD IMMUNOL | 1.931 | 224 | 54 | 170 | 116 | 1.466 |
| ANIM REPROD SCI | 0.701 | 110 | 23 | 87 | 157 | 0.554 |
| BIOL REPROD | 3.257 | 1726 | 265 | 1461 | 530 | 2.757 |
| EUR J OBSTET GYN R B | 0.449 | 169 | 19 | 150 | 376 | 0.399 |
| HUM REPROD | 1.328 | 627 | * | 627 | 472 | 1.328 |
| INVERTEBR REPROD DEV | 0.899 | 98 | 8 | 90 | 109 | 0.826 |
| J REPROD FERTIL | 2.211 | 1287 | 209 | 1078 | 582 | 1.852 |
| J REPROD IMMUNOL | 1.442 | 137 | 20 | 117 | 95 | 1.232 |
| MOL REPROD DEV | 2.003 | 597 | 107 | 490 | 298 | 1.644 |
| OXFORD REV REPROD B | 1.765 | 30 | * | 30 | 17 | 1.765 |
| REPROD DOMEST ANIM | 0.565 | 39 | 2 | 37 | 69 | 0.536 |
| REPROD FERT DEVELOP | 1.493 | 221 | 40 | 181 | 148 | 1.223 |
| REPROD NUTR DEV | 0.579 | 84 | 10 | 74 | 145 | 0.510 |
| REPROD TOXICOL | 0.859 | 79 | 26 | 53 | 92 | 0.576 |
| SEMIN REPROD ENDOCR | 0.347 | 25 | * | 25 | 72 | 0.347 |
| SEX PLANT REPROD | 1.659 | 136 | 38 | 98 | 82 | 1.195 |

* In 1992, *Human Reproduction* was not covered in a citation index, but has been added to the *Science Citation Index* (*SCI*) for 1993. The 1992 issue of *Oxford Reviews of Reproductive Biology* was not received in time to process its citations for ISI's 1992 database. *Seminars in Reproductive Endocrinology* is not covered in a citation index.

Table 2: Comparison of *JCR* impact factors to revised impact factors.

| | Journals ranked by *JCR* impact factor: | | Journals ranked by an impact factor calculated without self-citations: | |
|---|---|---|---|---|
| 1 | BIOL REPROD | 3.257 | BIOL REPROD | 2.757 |
| 2 | J REPROD FERTIL | 2.211 | J REPROD FERTIL | 1.852 |
| 3 | MOL REPROD DEV | 2.003 | OXFORD REV REPROD B | 1.765 |
| 4 | AM J REPROD IMMUNOL | 1.931 | MOL REPROD DEV | 1.644 |
| 5 | OXFORD REV REPROD B | 1.765 | AM J REPROD IMMUNOL | 1.466 |
| 6 | SEX PLANT REPROD | 1.659 | HUM REPROD | 1.328 |
| 7 | REPROD FERT DEVELOP | 1.493 | J REPROD IMMUNOL | 1.232 |
| 8 | J REPROD IMMUNOL | 1.442 | REPROD FERT DEVELOP | 1.223 |
| 9 | HUM REPROD | 1.328 | SEX PLANT REPROD | 1.195 |
| 10 | INVERTEBR REPROD DEV | 0.899 | INVERTEBR REPROD DEV | 0.826 |
| 11 | REPROD TOXICOL | 0.859 | REPROD TOXICOL | 0.576 |
| 12 | ANIM REPROD SCI | 0.701 | ANIM REPROD SCI | 0.554 |
| 13 | REPROD NUTR DEV | 0.579 | REPROD DOMEST ANIM | 0.536 |
| 14 | REPROD DOMEST ANIM | 0.565 | REPROD NUTR DEV | 0.510 |
| 15 | EUR J OBSTET GYN R B | 0.449 | EUR J OBSTET GYN R B | 0.399 |
| 16 | SEMIN REPROD ENDOCR | 0.347 | SEMIN REPROD ENDOCR | 0.347 |

Articles in "review" sections of research or clinical journals are also coded as reviews, as are articles whose titles contain the word "review" or "overview."

The Source Data Listing in the *JCR* not only provides data on the number of reviews in each journal but also provides the average number of references cited in that journal's articles. Naturally, review journals have some of the highest impact factors. Often, the first ranked journal in the subject category listings will be a review journal. For example, under Biochemistry, the journal topping the list is *Annual Review of Biochemistry* with an impact factor of 35.5 in 1992.

*Methods Articles.* It is widely believed that methods articles attract more citations than other types of articles. However this is not in fact true. Many journals devoted entirely to methods do not achieve unusual impact. But it is true that among the most cited articles in the literature there are some super classics that give this overall impression. It should be noted that the chronological limitation on the impact calculation eliminates the bias super classics might introduce. Absolute citation frequencies are biased in this way, but, on occasion, a hot paper might affect the current impact of a journal.

*Variation Between Disciplines.* Different specialties exhibit different ranges of peak impact. That is why the *JCR* provides subject category listings. In this way, journals may be viewed in the context of their specific field. Still, a five-year impact may be more useful to some users and can be calculated by combining the statistical data available from consecutive years of the *JCR* (see Figure 2). It is rare to find that the *ranking* of a journal will change significantly within its designated category unless the journal's influence has indeed changed.

**Figure 2**: Calculation for five-year impact factor:
One year of citations to five years of articles.
A = citations in 1992 to articles published in 1987-91
B = articles published in 1987-91
C = A/B = five-year impact factor

An alternative five-year impact can be calculated based on adding citations in 1988-92 articles published in the same five-year period. And yet another is possible by selecting one or two earlier years as factor "B" above.

*Item-by-Item Impact.* While ISI does manually code each published source item, it is not feasible to code individually the 12 million references we process each year. Therefore, journal citation counts in *JCR* do not distinguish between letters, reviews, or original research. So, if a journal publishes a large number of letters, there will usually be a temporary increase in references to those letters. Letters to the *Lancet* may indeed be cited more often that letters to *JAMA* or vice versa, but the overall citation count recorded would not take this artifact into account. Detailed computerized article-by-article analyses or audits can be conducted to identify such artifacts.

*Cited-Only Journals in the JCR.* Some of the journals listed in the *JCR* are not citing journals, but are cited-only journals. This is significant when comparing journals by impact factor because the self-citations from a cited-only journal are not included in its impact factor calculation. Self-citations often represent about 13% of the citations that a journal receives. The cited-only journals with impact factors in the *JCR* Journal Rankings and Subject Category Listing may be ceased or suspended journals, superseded titles, or journals that are covered in the science editions of *Current Contents*, but not a citation index.

**Figure 3**: Calculation for impact factor revised to exclude self-citations.
A = citations in 1992 to articles published in 1990-91
B = 1992 self-citations to articles published in 1990-91
C = A - B = total citations minus self-citations to recent articles
D = number of articles published 1990-91
E = revised impact factor (C/D)
(see *Table 1* for numerical example)

Users can identify cited-only journals by checking the *JCR* Citing Journal Listing. Furthermore, users can establish analogous impact factors, (excluding self citations), for the journals they are evaluating using the data given in the Citing Journal Listing (see Figure 3).

*Title Change.* A user's knowledge of the content and history of the journal studied is very important for appropriate interpretation of impact factors. Situations such as those mentioned above and others such as title change are very important, and often misunderstood, considerations.

**Figure 4**: Unified 1992 impact factor calculation for title change.
A = 1992 citations to articles published in 1990-91 (A1 + A2)
A1 = those for new title
A2 = those for superseded title
B = number of articles published in 1990-91 (B1 + B2)
B1 = those for new title
B2 = those for superseded title
C = unified impact factor (A/B)
C1 = A1/B1 = JCR factor for the new title
C2 = A2/B2 = JCR factor for the superseded title

A title change affects the impact factor for two years after the change is made. The old and new titles are not unified unless the titles are in the same position alphabetically. In the first year after the title change, the impact is not available for the new title unless the data for old and new can be unified. In the second year, the impact factor is split. The new title may rank lower than expected and the old title may rank higher than expected because only one year of source data is included in its calculation (see Figure 4). Title changes for the current year and the previous year are listed in the *JCR* guide.

*Conclusions*

The impact factor is a very useful tool for evaluation of journals, but it must be used discreetly. Considerations include the amount of review or other types of material published in a journal, variations between disciplines, and item-by-item impact. The journal's status in regard to coverage in the ISI databases as well as the occurrence of a title change are also very important. In the next essay we will look at some examples of how to put tools for journal evaluation into use.

## Using the Impact Factor

The impact factor, as explained is one of the evaluation tools provided by the Institute for Scientific Information's (ISI's) *Journal Citation Reports* (*JCR*). Many features of the *JCR* can be applied to the real-world task of journal evaluation, and the specific needs of the user ultimately determine which of those component is the most appropriate for the task.

*Bradford's Law*

Doomsday predictions about the exponential growth of scientific literature have not come to pass . While the growth has been slower than forecasted, it nevertheless warrants concern. Even though the reality of the current situation is not nearly as frightening as had been anticipated, the need to be selective in journal management is all the more imperative.

As Bradford's Law predicts, a small percentage of journals account for a large percentage of what is published. An even smaller percentage accounts for what is cited. In other words, there are diminishing returns in trying to cover the literature exhaustively. Careful selection is, therefore, an effective way to avoid "documentary chaos." This term, coined by Samuel C.

Bradford, the former librarian of the Science Museum in London, refers to the anxiety that one feels in contemplating the information explosion. Recognizing the need of readers to scan the most significant journals published was the *raison d'etre* for *Current Contents*.

It is understandable that publishers are concerned that their journals are selected by ISI for inclusion in its database. Indeed, it is sometimes argued that the survival of a particular journal depends on ISI's decision to cover it in *Current Contents*. A journal's ultimate success depends upon its quality, distribution, and many other competitive factors including cost and timeliness. Any one of these factors, including coverage by ISI, can make the difference between success and failure.

*Market Research*

Many publishers regularly use the *JCR* to conduct market research. A concrete example of the *JCR*'s role in journal market research was presented in an essay about pathology journals [7]. As a result of evaluating *JCR* data, it was possible to show that a journal of applied virology was needed. Not long after that essay appeared, such a journal was established.

The JCR can benefit the user in a number of ways. Not only are rankings important, but even more interesting are trends that can be gleaned from the various listings, including the source data, the half-life, and the cited and citing journal listings.

*Prestige and impact*

A journal's reputation may not tell the complete story about its impact on the scholarly community. In fact, a study by Christenson and Sigelman on social science journals suggests quite the opposite [8,9]. Their research showed that there is a nonlinear relationship between a journal's reputation and its impact, especially at the extremes of the prestige scale. They conclude that citation data "permit scholars to evaluate the importance of journals based not on opinion but on the frequency of citations" and that "frequency of citation implies scholarly acceptance, or at least acknowledgment of importance through utilization of others' work." The researchers go on to mention that "journals have prestige, but their prestige is only derived from the usefulness of the articles they publish."

The *JCR* satisfies the need for quantitative measures. It provides a detailed picture of the scientific literature. It shows the journal-to-journal relationships and permits the discerning user to track important trends or changes over the years, such as a shift from pure to applied research. The changes are not always reflected in the names of the journals. For instance, while the title of the *Journal of Experimental Medicine* conveys one image, its primary focus today is in fact immunology. [10]

*Cost-Effectiveness and the Impact Factor*

Realizing the need for selectivity and recognizing the JCR as a valuable tool for finding information about journals are both key to effective management of library collections. Strategies to implement effective selection plans include use of the impact factor to determine cost-effectiveness and to identify appropriate journals for a collection.

To deal with essentially static budgets in the face of rising journal costs, Prof. Henry H. Barschall of the University of Wisconsin suggests that the ratio of printed character cost to journal impact is a good indicator of a journal's cost-effectiveness. [11, 12]

*Selection by Impact Leaderhip*

Tony Stankus, science librarian at the College of the Holy Cross in Worcester, Massachusetts, has written several articles and books on the use of citation data to characterize publishing trends. In an article coauthored with Carolyn Mills, Stankus suggests that a good rule of thumb is to include in a science library collection the journals that have held impact factor leadership within their specialty over the course of a 10-year period. Those journals, in turn, will lead to others cited by them [13].

*Conclusions*

Evaluation of journals is a formidable but necessary task considering the wide range of choices available. Limited funding and space, as well as other factors, dictate the need for a carefully planned strategy of journal selection. The *JCR* offers many valuable indicators — including the impact factor — to help deal with the series of decisions involved in the establishment and maintenance of an effective library collection.

*E. Garfield,*
*Current Contents (June 20, 1994) 3;*
*Current Contents (July 18, 1994) 3*


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# The Art of the Scientific Insult

At seminars and conferences, physicist Wolfgang Pauli was famous for dismissing work he particularly disliked as "*ganz falsch*", completely false, or, yet more damning: "*not even false.*" Pauli was not the only practitioner of the deft put-downs the British call pinking. Scientists show a surprising historical talent for the insult. Beneath the impersonal surface of their formal scientific discourse lies a vast subtext of bloodletting.

Isaac Newton, for instance, was a scathing polemicist who honed his attacks on Robert Hooke and Gottfried Wilhelm Leibniz to razor-sharpness in successive drafts, each of them, according to a biographer, more offensive than the last. "Mr Hook thinks himself concerned to reprehend me for laying aside the thoughts of improving Optiques by *Refractions*," Newton wrote in one salvo. "But he knows well y$^t$ it is not for one man to prescribe Rules to y$^e$ studies of another, especially not without understanding the grounds on w$^{ch}$ he proceeds." When Leibniz published his calculus without acknowledging what he knew of Newton's progress, Newton attacked, sometimes through John Keill (Johann Bernoulli referred to Keill as "Newton's toady"), sometimes through a committee of the Royal Society. He even tucked one beautifully compressed accusation of what was then known as plagiary into the mathematical footnotes of *Commercium epistolicum*: "Thus the method which earlier he [Leibniz] wanted, asked for, received, and understood with difficulty, he discovered forsooth..."

Priority fuels the bitterest assaults, but ignorance, particularly when scientists move outside their area of expertise, also provides a rich source. For instance, the pugnacious Hermann Kolbe, a major 19th-century research chemist with an unfortunate lack of interest in optical activity, used the *Journal für Praktische Chemie* to ridicule Jacobus Henricus van't Hoff, who had clarified the relation between optical activity and molecular structure in a brilliant pamphlet. "A Dr J. H. van't Hoff of the veterinary school at Utrecht, finds, as it seems, no taste for exact chemical investigation," Kolbe wrote. "He has thought it more convenient to mount Pegasus (obviously loaned at the veterinary school) and to proclaim in his *La chimie dans l'espace* how during his bold flight to the top of the chemical Parnassus, the atoms appeared to him to have grouped themselves throughout universal space." Van't Hoff had his revenge; he reprinted Kolbe's jibes in the second edition of *La chimie*, guaranteeing Kolbe's historical reputation as a fool.

Chauvinism, too, gives impetus to the scientific put-down. The French scientist Marie Jean Pierre Flourens said in his review of *The Origin of Species*, "... [W]hat unclear ideas, what false ideas!... O lucidity! O French stability of mind, where art thou?" And the Swedish chemist Jöns Jakob Berzelius, who took to studying textbooks other than his own when confined to bed by gout, wrote, "The chemists of England live in their own world.... There is a great deal of litigation here about priority in the most petty matters.... one can regard them as one would puppies who stand and snarl over their bones, from which the meat has been gnawed on the continent."

Singularly unprophetic remarks abound in the history of science, from the comments of Charles Darwin's doctor father to his son ("You care for nothing but shooting, dogs, & rat-catching, & you will be a disgrace to yourself & all your family") to those of Albert Einstein's headmaster when asked what profession Einstein should follow ("It doesn't matter; he'll never make a success of anything"). Elihu Thomson, electrical engineer of note, visited Thomas A. Edison's workshop and told the newspapers that he "did not think very highly of the Edison lamp and expected no great future for it." J. Louis Agassiz, Harvard University professor and naturalist, maintained that he would "outlive this mania" of evolution, which he characterized in the *American Journal of Science and Arts* as "a scientific mistake."

Press conferences eventually sped up the tempo of scientific sniping, infighting and backstabbing once conducted in the main through debates, letters, symposia and book reviews. Edison used the newspapers in his well-publicized fight to fend off the new AC (alternating current) distributing systems that were far more powerful than his limited DC (direct current) stations. In what became known as the "War of the Currents," Edison invited reporters and guests in daily throughout 1887 to watch the stray cats and dogs of West Orange, N.J., totter onto tin sheets and be electrocuted by high-tension AC currents. Red-lettered pamphlets warned that "patent pirates" like George Westinghouse were bent on introducing these hazardous currents "into the American home." Despite Westinghouse's constant explanations that AC was received only at low voltages because of Stanley transformers, Edison and his colleagues were so successful in arousing the public that the eponym "to Westinghouse" was suggested as an alternative to " to electrocute."

In the present age of litigation, scientists are usually more circumspect than Edison. They consult attorneys before they talk, as did Steven E. Koonin, then chairman of the nuclear physics division of the American Physical Society, before speaking out on cold fusion at a meeting of that society in 1989. Advised to avoid the "F-word" (fraud), Koonin summed up Stanley Pons and Martin Fleischmann's cold fusion work with the deadly phrase "incompetence and perhaps delusion." Nathan S. Lewis, who also spoke, provided a list of pointed questions the press might want to ask Pons and Fleischmann, adding that if the two scientists were going to have publication by press conference, he would institute peer review by press conference.

Not all put-downs are scathing — some small part of them manages to be affectionate, as was the comment of an Edinburgh professor on an evening spent with Charles Babbage ("It was with the greatest difficulty that I escaped from him at two in the morning after a most delightful evening"). Some of them are even meant to be compliments, although, of course, they also pink. For instance, Pauli, already a master of dis-ing when only a graduate student, attended a seminar given by Einstein and generously acknowledged, "You know, what Professor Einstein says is not so stupid."

*Anne Eisenberg, Scientific American (June 1994) 116*

## A write way with papers

At a dental conference a few years ago a prize was on offer for the best paper written by a single author. Sadly, it couldn't be awarded because every paper had multiple authors. Anyone familiar with the pressing need to publish will know why: multi-author papers are a neat way of increasing one's output for no extra work, thereby defeating the best efforts of bureaucrats to assess academic worth. All it takes is a little cooperation among colleagues, and everyone in a department can be tagged on to everyone else's paper for the merest contribution to a coffee-break discussion.

After a while, the production of papers takes on the character of an assembly line — the only delay being the time it takes to dwell on referees' comments and to put into practice the unwritten rules underlying the whole system. For example, the title must be a little obscure and not convey the purpose of the work except to anyone already familiar with the subject. The opening paragraph should be stylised, with a pedantic reference to earlier papers by others, ending with a sentence stating the reason for the current work. It is best if couched in language that is stilted and archaic to the point of being off-putting; the purpose here is to limit the readership and thereby the chances of being proved wrong. Practised authors can construct such a paragraph semiautomatically without a great deal of thought.

The rest of the paper then follows according to equally predictable rules. The customary linguistic forms and constructions are gleaned by experience rather than taught formally, but any paper that does not use them stands out as odd. So standardised has the process become that the experienced researcher will instantly know when there is something wrong with a novice's paper — though it may be difficult to explain why. For example, sentences can sound more learned, and so be more acceptable to a referee, if the words are shuffled into a different order, even if the meaning remains unchanged. Getting this across to the junior researcher or the enthusiastic amateur can be difficult.

A research paper condensed to its essential facts — what was actually done, why it was done, what was found and what it might mean — might be rather short. But it would not be the same as the abstract of the paper, because abstracts omit experimental data, and in any case they don't always convey the essence of what the researchers have done, but rather the author's interpretation of the essence.

Since papers are written in stylised form, condensing them to their essentials is simply a matter of removing the superfluous language to find what is left. The converse must also be true: the creation of papers is merely a matter of assembling the essential facts and filling them out with standard phrases. I now pose a simple question. If the construction of a paper is so ritualised, why can't a computer do it for me? All I need is a program which prompts for the essential facts and then wraps the necessary convoluted language around them. By analogy with a word processor, such a program could be called a "paper processor", although the name sounds a little clumsy and I'm open to alternative suggestions. "Manuscript assembler" has the right sort of ring to it. Special features would be needed, including the ability to vary manuscript length, with phrase insertion automatically adjusted to satisfy even the most demanding bureaucrat as to the quantity of research output. Then again, there would have to be an inbuilt randomness to phrase selection so that infill phrases do not all look the same. Perhaps the program could learn an author's personal style by analysis of previously published work, and so reproduce manuscripts indistinguishable from the originals. Finally, there would have to be a manual word processor override so that the finished paper could be altered here and there by the "author" to give it an individual touch.

So there it is: a modern-day philosopher's stone that turns even the basest data into a respectable paper. Since the apparent purpose of most papers is not to be read but to be counted in a curriculum vitae, no one will ever be the wiser. So easy would the process become that the entire system could get bogged down in an avalanche of publications.

What would then be needed would be the corresponding abstracting program, which would take published papers and automatically condense them down to their essentials by stripping out the superfluous words. Perhaps this too could be a feature of the paper processor: the reduction back to the bare facts if the author doesn't like the finished manuscript. Come to think of it, a paper "compressor" or "disassembler" is something I need right now to cope with even the current welter of publications.

There is but one snag with this grand scheme. If a "compressor" becomes available to help scientists read papers, it could also be used by bureaucrats to discover the essence of what is being done...

*R.D. Bagnall, New Scientist (25 June 1994) 49*

## Federal Science Grants: The Top 20 Universities

| | Institution | Total $ (millions) |
|---|---|---|
| 1. | Johns Hopkins U. | 660.7 |
| 2. | U. of Washington | 280.1 |
| 3. | Mass. Inst. ot Tech. | 275.3 |
| 4. | Stanford University | 270.1 |
| 5. | U. of Michigan | 233.8 |
| 6. | Wisconsin-Madison | 222.2 |
| 7. | Cornell University | 218.5 |
| 8. | U.C. Los Angeles | 215.8 |
| 9. | U.C. San Diego | 215.0 |
| 10. | U. of Minnesota | 212.1 |
| 11. | U.C. San Francisco | 209.6 |
| 12. | Columbia University | 202.1 |
| 13. | Harvard University | 198.3 |
| 14. | U. of Pennsylvania | 193.4 |
| 15. | Yale University | 190.4 |
| 16. | U.C. Berkeley | 185.2 |
| 17. | U. of Pittsburgh | 178.1 |
| 18. | Pennsylvania State U. | 176.0 |
| 19. | U. of Colorado | 167.4 |
| 2O. | UNC-Chapel Hill | 151.1 |

**Source**: NSF/SRS, Survey of Federal Support to Universities, College, and Nonprofit Institutions in FY 1992

# Citations Reveal Leaders in Neuroscience

| Neuroscience Research, 1988-92: Institutions Ranked by Citation Impact (among those publishing at least 200 papers, 1988-92) | | | | |
|---|---|---|---|---|
| Rank | Institution | Papers | Citations | Impact |
| 1 | Salk Institute, La Jolla, Calif. | 304 | 5,019 | 16.51 |
| 2 | California Institute of Technology, Pasadena | 210 | 2,740 | 13.05 |
| 3 | Max Planck Institute for Psychiatry, Germany | 54 | 5,633 | 10.30 |
| 4 | Brigham & Women's Hospital, Boston | 236 | 2,367 | 10.03 |
| 5 | Stanford University, Calif. | 1,001 | 9,810 | 9.80 |
| 6 | University of California, San Francisco | 1,268 | 11,626 | 9.17 |
| 7 | Yale University, New Haven, Conn. | 1,454 | 13,100 | 9.01 |
| 8 | Washington University, St. Louis | 1,034 | 9,251 | 8.95 |
| 9 | Harvard University, Cambridge, Mass. | 2,194 | 19,373 | 8.83 |
| 10 | Rockefeller University, New York | 604 | 5,308 | 8.79 |
| 11 | Scripps Research Institute, La Jolla, Calif. | 288 | 2,523 | 8.76 |
| 12 | University of California, Irvine | 862 | 7,520 | 8.72 |
| 13 | University of Heidelberg, Germany | 587 | 5,086 | 8.66 |
| 14 | Massachusetts Institute of Technology, Cambridge, Mass. | 419 | 3,583 | 8.55 |
| 15 | National Institute of Neurological Disorder and Stroke, Bethesda, Md. | 1,062 | 8,768 | 8.26 |
| 16 | National Institute of Mental Health, Bethesda, Md. | 1,490 | 12,249 | 8.22 |
| 17 | University of Chicago | 620 | 4,919 | 7.93 |
| 18 | University of London, University College | 513 | 3,902 | 7.61 |
| 19 | Columbia University, New York | 1,539 | 11,650 | 7.57 |
| 20 | Massachusetts General Hospital, Boston | 807 | 6,048 | 7.49 |
| 21 | University of California, San Diego | 1,478 | 11,061 | 7.48 |
| 22 | University of Miami, Coral Gables, Fla. | 518 | 3,808 | 7.35 |
| 23 | Georgetown University, Washington, D.C. | 371 | 2,697 | 7.27 |
| 24 | McLean Hospital, Belmont, Mass. | 267 | 1,882 | 7.05 |
| 25 | Johns Hopkins University, Baltimore | 1,698 | 11,909 | 7.01 |
| Source: ISI's Science Indicator Database | | | | |

*(continued on next page)*

| The Most-Cited Papers in Neuroscience, 1988-92 | | |
|---|---|---|
| Rank | 1988 | Total Citations |
| 1 | **N. Kitaguchi, Y. Takahashi, Y. Tokushima, S. Shiojiri, H. Ito**, "Novel Precursor of Alzheimer's disease amyloid protein shows protease inhibitory activity," *Nature*, 331:530-2, 1988 | 432 |
| 2 | **E.S. Levitan, R.R. Schofield, D.R. Burt, L.M. Rhee, W. Wisden, M. Köhler, N. Fujita, H.F. Rodriguez, A. Stephenson, M.G. Darilson, E.A. Bernard, P.H. Seeburg**, "Structural and functional basis for $GABA_A$ receptor heterogeneity," *Nature*, 335:76-9, 1988 | 348 |
| 3 | **L.D. Hirning, A.P. Fox, E.W. McCiesky, B.M. Olivera, S.A. Thayer, R.J. Miller**, "Dominant role of N-type $Ca^{2+}$ channels in evoked release norepinephrine from sympathetic neurons," `Science`, 239:57-61, 1988 | 315 |
| 4 | **N.W. Kleckner, R. Dingledine**, "Requirement for glycine in activation of NMDA receptors expressed in *Xenopus* oocytes," *Science*, 241:835-7, 1988 | 298 |
| | 1989 | |
| 1 | **M. Hollman, A. O'Shea-Greenfield, S.W. Rogers, S. Heinemann**, "Cloning by functional expression of a member of the glutamate receptor family," *Nature*, 342:643-8, 1989 | 241 |
| 2 | **J. Leibrock, F. Lottspeich, A. Hohn, M. Hofer, B. Hengerer, P. Maslakowski, H. Thoenen, Y.-A. Barde**, "Molecular cloning and expression of brain-derived neutrophic factor," *Nature*, 341:149-52, 1989 | 221 |
| 3 | **M.C. Raff**, "Gilal cell diversification in the rat optic nerve," *Science*, 243:1450-5, 1989 | 206 |
| 4 | **M.R. Plummer, D.E. Logothetis, P. Hess**, "Elementary properties and pharmacological sensitivities of calcium channels in mammalian peripherical neurons," *Neuron*, 2:1453-63, 1989 | 202 |
| | 1990 | |
| 1 | **P.C. Maisonpierre, L. Belluscio, S. Squinto, N.Y. Ip, M.E. Furth, R.M. Lindsay, G.D. Yancopoulos**, "Neurotrophin-3: a neurotrophic factor related to NGF and BDNF," *Science*, 247:1446-51, 1990 | 225 |
| 2 | **K. Keinänen, W. Wisden, B. Sommer, P. Werner, A. Herb, T.A. Verdoorn, B. Sakmann, P.H. Seeburg**, "A family of AMPA-selective glutamate receptors," *Science*, 249:556-60, 1990 | 222 |
| 3 | **A. Hohn, J. Leibrock, K. Balley, Y.-A. Barde**, "Identification and characterization of a novel member of the nerve growth factor/brain-derived neutrophic factor family," *Nature*, 344:339-41, 1990 | 212 |
| 4 | **J. Boulter, M. Hollmann, A.O.'Shea-Greenfield, M. Hartley, E. Deneris, C. Maron, S. Heinemann**, "Molecular cloning and functional expression of glutamate receptor sububit genes," *Science*, 249:1033-7, 1990 | 158 |
| | 1991 | |
| 1 | **R.K. Sunahara, H.-C. Guan, B.F. O'Dowd, P. Seeman, L.G. Laurier, G. Ng, S.R. George, J. Torchia, H.H.M. Van Tol, H.B. Niznik**, "Cloning of the gene for a human dopamine $D_5$ receptor with higher affinity for dopamine than $D_1$" *Nature*, 350:614-9, 1991 | 152 |
| 2 | **M. Masu, Y. Tanabe, K. Tsuchida, R. Shigemoto, S. Nakanishi**, "Sequence and expression of a metabotropic glutamate receptor," *Nature*, 349:760-5, 1991 | 144 |
| 3 | **V.M.-Y. Lee, B.J. Balin, L. Otvos, J.Q. Trojanowski**, "A68: a major subunit of oaired helical filaments and derivatized forms of normal tau," *Science*, 251:675-8, 1991 | 110 |
| 4 | **B.T. Hope, G.J. Michael, K.M. Knigge, S.R. Vincent**, "Neuronal NADPH diaphorase is a nitric oxide synthase," *Proceedings of the National Academy of Sciences USA*, 88:2811-4, 1991 | 97 |
| | 1992 | |
| 1 | **T.E. Golde, S. Estus, L.H. Younkin, D.J. Selkoe, S.G. Younkin**, "Processing of the amyloid protein precursor to potentially amyloidogenic derivatives," *Science*, 255:728-30, 1992 | 52 |

Source: ISI's Science Indicators Database

Perhaps forever destroying the stereotype that Californians are wanting when it comes to gray matter, two research institutions in the Golden State — the Salk Institute for Biological Studies in La Jolla and California Institute of Technology in Pasadena — have captured the top two spots in *Science Watch*'s latest ranking for neuroscience research. The survey examined about 147,000 papers published and cited during the period 1988-92.

Half of the top 12 institutions in this field call California home. The other four from the sunny state ranking near the top are Stanford University at fifth; the University of California, San Francisco, at sixth; and Scripps Research Institute, La Jolla, Calif., and the University of California, Irvine, at 11th and 12th, respectively.

In the table on page 9, the top 25 institutions (among those that published at least 200 papers during the five-years span) are ranked according to their citations-per-paper scores, a weighted measure of research impact. The current ranking actually updates a survey of neuroscience research that *Science Watch* featured three years ago, based on papers published between 1986 and 1990 (*Science Watch*, 2[6]:1-2, July 1991). In the previous study, neuroscience papers from the multidisciplinary journals *Science, Nature*, and *Proceedings of the National Academy of Sciences* were not included in the analysis, since such papers could not, at that time, be selected out from the countless other types of reports appearing in those journals. This time around, however, neuroscience papers appearing in the Big Three multidisciplinary journals *were* taken into account. And, not surprisingly, the heavyweight trio provided nearly all the action in terms of highly cited papers.

The table (page 10) lists the most cited neuroscience papers of each year from 1988 through 1992. Of the 17 papers, *Science* and *Nature* published 15 between them, and *PNAS* and *Neuron* published one apiece. Among the top three institutions from the table, two managed to get more than one paper on the list of most-cited reports: the Max Planck Institute for Psychiatry, Martinsried, Germany, fielded three of the papers (J. Leibrock et al., in 1989; K. Keinänen et al., in 1990; and A. Hohn et al., in 1990), while the Salk Institute fielded two papers (M. Hollman et al., in 1989; and J. Bouler et al., in 1990).

Although *Science Watch* examined only those institutions that produced more than 200 papers between 1988 and 1992, a few smaller producers, whose output of papers was just below the cutoff for inclusion in the study, deserve mention.

They include the international pharmaceutical firm Merck, Sharp & Dohme (United States offices, under the name Merck & Co., located in Rahway, N.J.; 176 papers; impact of 14.41); the National Institute of Child Health and Human Development, based in Bethesda, Md. (173 papers; impact of 8.18); the University of Geneva, Switzerland (186 papers; impact of 7.82); and Memorial Sloan-Kettering Cancer Center in New York City (189 papers; impact of 7.58).

As the table of papers illustrates, the hot areas of investigations in neuroscience during 1988-92 include amyloide proteins in Alzheimer's disease, glutamate receptors, and the role of calcium channels in neuronal function.

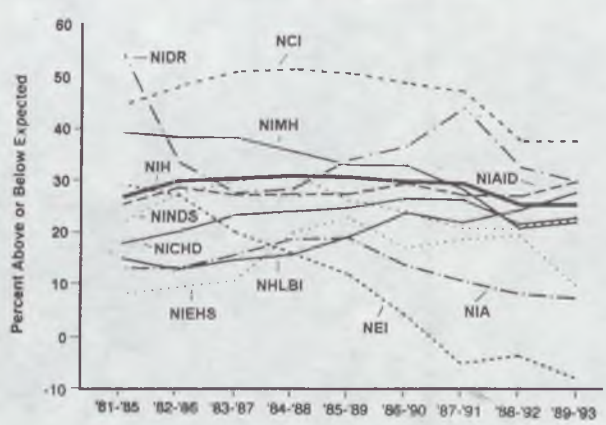
*The Scientist (June 13, 1994) 15*

## Charting the Course

A recent study by the Institute for Scientific Information compared citations to 92,961 NIH papers published between 1981 and 1993. The number of citations a paper collected was compared with the paper's expected number of citations, which was calculated by finding all papers of the same type (such as article, review, or note) from the same year and journal and determining their average number of citations. The papers were then grouped into nine sets of papers published during overlapping five-year periods.

*The Scientist, May 30, 1994.*

**Ratio of Actual to Expected Citations for NIH Papers, 1981-93**




**Years**

**(papers published during five-year period and cited through 1993)**

## "Irreproducible" team clones a rival

The American staff of *The Journal of Irreproducible Results* (JIR), the oldest and best-known satirical science journal, have left to set up a competing magazine after several years of disagreements with its British publisher. The 40-member board of scientists, which includes seven Nobel prizewinners, went with them.

The first electronic issue of the new *Annals of Improbable Research* (AIR), including news of what it described as the "revolt of the mad scientists", appeared on the Internet last week. The first paper edition is expected in the autumn.

But John Conibear, deputy managing director of Blackwell Scientific Publications in Oxford, England, which publishes *JIR*, denies claims that the future of the original journal is uncertain beyond the next edition. "The journal will continue to be published as before," he says, claiming that the launch of *AIR* is not seen as a threat to *JIR*.

According to editor Mark Abrahams, the split is the result of long-lived antagonism with Blackwell. "It made them very uncomfortable that, of all the journals they published, this one was intentionally funny," says Abrahams, who claims that a largely volunteer staff was carrying out all marketing and publicity for the journal with no help from Blackwell.

"It finally seemed clear to us that there was no reasonable way we could either improve that situation or buy the rights to the name," says Abrahams. "We all therefore decided to do the only reasonable thing: start a new magazine — one that has no legal connection to the magazine we left behind, on which we all worked so hard and loved so much."

But Conibear says that Abrahams was asked to leave as a result of irreconcilable editorial differences with *JIR*'s Chicago-based owner George Sherr. Ownership of *JIR* is due to revert back to Sherr after the June issue. Sherr is expected to announce a replacement editor later this month.

*JIR* was founded in 1955 by Alex Kohn, a virologist at Tel Aviv University, best known for his scientific paper proving that the North American continent is likely to sink under the accumulated weight of stockpiled National Geographic Society magazines. He eventually sold the rights to *JIR*, but remained as editor until 1989, when Abrahams took over. He and Abrahams are the co-founders of *AIR*.

At the time of the revolt, *JIR* had a circulation of 17,000. including the electronic issue. Abrahams claims that it was once as high as 40,000, although Conibear disputes this figure.

The new publication takes with it the infamous Ig Nobel Awards, given out each year to people whose achievements in science "cannot or should not be reproduced". The awards ceremony will henceforth be sponsored jointly by *AIR* and the Massachusetts Institute of Technology Museum, where the award ceremony takes place.

*Joel Shurkin,*
*Nature, 369:433 (9 June 1994)*


*Készült az Argumentum Könyv- és Folyóiratkiadó Kft. nyomdájában*

*Felelős kiadó: az MTAK főigazgatója*