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4. évfolyam · 5. szám · 1994. május

IMPAKT

TÉNYEK A TUDOMÁNYOS ALAPKUTATÁSRÓL

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

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

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A fresh start for European science

Instead of becoming embroiled in bureaucracy and the misplaced support of 'network' projects, the European Commission could provide an exciting, long-term science policy.

Let me declare my interest immediately. I strongly support the concept of a single European scientific community — who could not? — and I believe that the European Commission in Brussels can and must play a vital role in promoting this concept. Indeed, I have experienced and benefited from the Commission's efforts in my own research. But after more than 10 years of sponsoring projects, rather than individuals, the Commission's policy needs to be assessed.

This question is particularly crucial at a time when the European Union is floundering in its attempts to maintain the "European ideal". An exciting, long-term European science policy would provide the perfect opportunity for renewing the momentum towards greater integration. Rationalization of higher-education policy and the unrestricted freedom of movement of scientists throughout Europe are two areas that the Commission is in a position to promote with great effect.

In advocating such a policy, I have to say that Brussels, for all the undoubted dedication of the people working there, has been pursuing a science policy that it is ill-suited to carry out. I believe that the Commission should largely jettison its efforts to identify "winning projects", whether fundamental or applied, and should instead promote the concept of a sustained European science policy. The direct funding of projects should be left to national agencies, which are far better equipped to assess fundamental and applied science through hands-on experience and informed peer review.

The scientific arm of the Commission suffers the handicap of having no true roots in academic or industrial communities across Europe. As such it cannot be in tune with the pressing needs of scientific research or its commercial exploitation. Yet the Commission's scientific administrators develop frequent initiatives, no doubt with the highest of motives, in selected areas of basic or applied science, without the knowledge and experience of national agencies, which (quite rightly) are dominated by active scientists with close links with local academic and industrial sectors.

Collaborative bureaucracy

European Union initiatives have the laudable aim of promoting collaborations between member states, but how successful have these been? The Commission has no way of taking the pulse of activity on the ground, or to anticipate the natural collaborations that are being held back for lack of funding or opportunity. Rather, proposals are tossed to the scientific community in the expectation that collaborations will automatically follow, which the Commission has divined as "good". The retort may be that scientists are only too happy to scramble for these delights — well, yes, we are. Scientists, particularly impoverished scientists, have become adept, chameleon-like, in adjusting to the requirements of the most specific projects imaginable. But what a degree of contortion this can necessitate — and what pain, effort and time can be taken to produce in some cases quite phoney collaborations! And this is the easy part. Unlike most national funding agencies with which I am familiar, access to European funding is overwhelmingly bureaucratic: contracts brim with legalistic and semi-commercial detail; cash takes an inordinate amount of time to arrive; and there is no understanding of the realities of recruiting staff and the complexities of managing modern scientific groups.

In my opinion, the unquestioned talents and vision of the Commission could be far more effectively used to promote the greater mobility of European scientists, which would lead to better collaborations than have so far been achieved. This should include greater mobility for graduate students, postdocs and for established scientists as individuals, not linked to cumbersome networks. Movement of graduate students between countries to complete a PhD is essentially zero. Movement of postdocs has improved, but demand greatly exceeds the available funding. Regular programmes to support short-term movement of established scientists are rare, and permanent migration is still greatly inhibited by nontransferable pension schemes, bureaucracy and national idiosyncrasies.

I propose that the Commission devotes the bulk of its funding for the support of PhD students to study in member states other than their own; for postdoctoral fellowships; for short- and long-term visits for established scientists; and for greater support for multinational workshops and symposia. The procedures, while remaining accountable, must be greatly streamlined. Most important, such a programme must be sustained over a decade or more if it is to have real impact (why mast Brussels chop and change its initiatives so frequently?).

The Commission must back this policy with strong leadership to rationalize PhD training and career development through-out member states. The objective would be to produce more flexible and, above all, more rapid career progression to independent status for young scientists. The potential of talented individuals is being held back in many European countries by insufficient support or by suffocation from outdated academic hierarchies, which block mobility even within the national framework. Different member states, no doubt for different reasons, appear unable to deal with the key issues of graduate training or to appreciate the need for attractive, effective career structures. The Commission is in an ideal position to generate ideas and leadership.

Sustained effort

In terms of graduate training, the Commission could draw up guidelines for PhD training — putting its weight behind, for example, the growing consensus for a fully funded master's degree followed by a further 3 years' research training. These guidelines could remove many anomalies, such as costs incurred by non-nationals studying for a PhD (essentially nothing in France compared with several thousand pounds in the United Kingdom), guaranteed length of time for funding (3 years in the United Kingdom but only 2 years in France) and so on. Similarly, the Commission should look at career development: in some countries postdoctoral funding is essentially non-existent, resulting in enormous bottlenecks in people's careers.

The essence of my plea is that we desperately need a much greater concentration of effort on the training of young scientists and the removal of barriers, both physical and psychological, to easy movement for training and to subsequent career development. This must be a *sustained* programme with a minimum of bureaucracy. In my view the Commission, free of national political baggage, is in a far better position to implement such a policy than individual member states. National agencies, of the other hand, are best able to formulate policies for the stimulation of specific areas of science.

I. B. Holland,
Nature, 367 (17 February 1994) 592

French language wars

Francophiles everywhere will be alarmed by the bill on the use of the French language given outline approval last week by the French cabinet. If enacted, it will require, among others things, all scientific conferences in France to be held in French. *Nature*, which is planning two meetings in France this year (in October and December), has a vested interest in the bill's delay, but there is more than that to say.

M. Jacques Toubon, the minister of culture in Paris, is right to proclaim that there is a distinction between "international" and "universal"; French speakers are none the less international because of the language they speak, but would be diminished if compelled to speak some universal (and non-existent) Euroglot. Everybody, of course, agrees. Indeed, the rest of us would also be diminished if French were not the vivid language Toubon pleads for. But the question he raises goes deeper, and will not be put to rest by his hopes for machine translation: can French by compulsion serve the interests of France itself?

Sadly, the answer must be "NO". Two centuries of history have unfolded since the Revolution, and the world partly shaped by those great events is fashioned awkwardly for linguistic purists. The role of the English language in science (where it has only recently taken over from German) is a simple consequence of the growth of science in the United States since the Second World War and of the eagerness of researchers elsewhere to participate in and contribute to that endeavour. It is natural that those concerned should be resentful of this state of affairs; many have to learn a second language, and even then their contributions to the literature win less acclaim than those from familiar laboratories. But logic suggests the remedy is not to compel them to write or speak in their native languages, but to fund their own research enterprises so generously that the balance of linguistic advantage changes again.

Nature, 368 (3 March 1994) 2

New Journal Decision Making

The paper will discuss the steps through which a publisher proceeds in making the decision as to whether to publish a new research journal and some of the consideration involved is establishing prices for a new journal.

In general, the decision to launch a new journal has become more difficult in recent years because of changes in the economic environment in which publishers operate. For example, in 1987 Elsevier Publishing Co. launched five new journals. Significantly, during that year the company *declined* to publish about ten new journals that were considered and researched quite seriously. In addition, the publishing staff corresponded with many individuals with ideas for new journals. Of the five new journals Elsevier did start up, three were sponsored by biomedical societies and therefore would likely have been started even had Elsevier chosen not to published them. Just as perhaps 80 percent of papers that are rejected by one journal are probably published somewhere eventually, many of the journal proposals that one publisher rejects are eventually accepted by another publishing company. Following is a description of the general process by which new journal proposals flow from the idea to the launch stage.

Proposals for new journals

Ideas for new publications are generated from three main sources. First, a researcher may perceive a need for a new journal on a topic of personal interest and approach a publisher with the suggestion to launch this new journal with the researcher at the helm. A second source is scientific societies that may have too small to publish their own journal but that have grown large enough to provide a sufficient author pool. In this case, the publisher might be approached by an ad hoc publication committee of the society. The third source of new ideas is the company's editorial staff. They obtain publication ideas by visiting research institutions and talking with scientists and scholars about information and publication needs, and by attending conferences to keep abreast of developing trends.

Ideas for new research journals may be categorized in two ways. The first type is prompted by growth of a field such that discrete subfields identify themselves as large enough to attract enough papers to feed a journal. An example of this is a journal idea that was eventually rejected. In this case, the colleagues of the editor of a journal on cancer cell growth suggested starting a journal on solid tumors. The proponents of the journal idea noted the importance of the subfield and the fact that they had a list of several thousands researchers actively involved in this area, who read a monthly newsletter they produced on this subject.

When approached with such proposals, the publisher begins its research by talking to authorities in the field to test the idea's strength. The staff editors then examine the prevalence of papers on the subject, what journals the existing papers reference, how many pages those journals have and whether the publication lag times represent a threat to the efficient and timely dissemination of scientific information. After competitive analysis, a market research questionnaire is mailed to determine from investigators whether there is a perceived need among the scientific community for such a journal. One aspect of this is the publisher's very real concern with whether the proposed journal will receive enough papers. Editors investigate where the target researchers are currently publishing their work and whether there is room for a journal on this more highly targeted area. In the case of the solid tumor idea, the publisher decided that the research community did not need a specialized journal for the publication of solid tumor papers.

A second type of idea for a new journal is the perceived need to compile research on various aspects of a single subject. In this case, rather than breaking down a subject into smaller parts, a subject is considered from the point of view of various specialties, the idea being to gather in one place the variety of papers on a single subject that may be dispersed throughout many journals.

An example of a journal that Elsevier launched that fits into this category is *Arthritis Care and Research.* The idea for this journal was presented by a committee of the Arthritis Health Professions Association of The Arthritis Foundation. This group consists of 2,000-3,000 therapists, nurses, and allied health professionals who work specifically with arthritis patients. Their group had grown quickly over the last few years, and they saw a need for a journal devoted to clinical research on the care of arthritis patients. Such a journal, it was reasoned, would focus on research at a different level from that of medical research publications. As well, it would be more targeted to arthritis than any of the existing therapy journals. The association did its homework. It polled its membership to determine how many papers they were publishing annually in this area. It was important to ensure that there would be enough manuscripts from their own membership to seed the growth of a journal until it could attract papers from outside the group. Upon completion of its research, the association presented its proposal to the publisher.

Journal subscribers: Author, researcher, and librarian

This example of the launching of a new journal touches on the very critical subject of the author as customer. Publishers realize that a journal needs to fill a real need in order to succeed. This means that the journal not only must attract subscribers, but also must generate editorial respect, which translates into authors who will submit papers, researchers who will read them, and librarians who will endeavor to add them to their collections. If the journal does not fill a research or scientific need, it cannot be successful. Authors and subscribers may not be the same person, since librarians are usually the actual purchaser of the journal. If authors do not read, cite, or contribute to the journal, no one will subscribe. Librarians, who act as information gatekeepers, will not maintain subscriptions to journals for which authors and researchers do not exhibit a need.

And so the competition analysis must identify a need for the publication by both authors and readers in order for a company to decide to commit its resources to launching a new journal. The existence of a sufficient author pool usually indicates a sufficient subscriber pool. A journal really functions as an archive for the research of a field. For this reason, journal publishers pay attention to measures such as the Institute for Scientific Information's *Science Citation Index* impact factor, as well as accessibility through key indexing and abstracting services.

Criteria for new journals

In sum, what are the reasons for starting a new journal?

From the editorial point of view, there may be no journal that covers the proposed topic in sufficient detail. A subfield may have developed. Alternatively, there may be no journal that brings together papers on all aspects of a given subject. The current journals in a field may be so beleaguered by submissions that the delay between submission and publishing is inordinate, impending the research dissemination process. A journal may be have grown so large that it can no longer be handled by a single editor and needs to be split into discrete subfields to be manageable. Ancillary to these considerations is the availability of high-quality scientists to serve on the editorial board. Behind all of this is the publisher's concern with the quality of the anticipated product. Because the company's imprint appears on its journals, it will try to ensure that new journals will be of the highest quality.

From the company point of view, even if a new idea is determined to be a good one, a journal will only be launched if it complies with the firm's internal criteria. Important among these are whether the proposed journal fits within the subject areas in which the company currently publishes or has identified as areas into which the company plans to expand. Clustering of subject areas is important to maximize the expertise of staff. Financially, a company can support its marketing and customer-service efforts more effectively by publishing within specified subject areas.

Another way publishers weigh proposals is by determining how the level of the material fits with its current output. For example, one publisher may publish very little at the undergraduate level, while another may concentrate its editorial resources and sales effort there. A publisher evaluates the market from the standpoints of who the purchaser is, or where the money comes from to purchase the journal. For example, many journals are designed primarily for the library market, either in the United States or worldwide, and others are designed mainly for the professional or industrial market. There is, of course, always potential for overlap in such market definitions, but the emphasis may vary.

The market for a journal must be evaluated in a fair amount of detail because the library or industrial markets are, in reality, groupings of discrete segments. For example, a biomedical research journal's principal market may be a library. But what does this mean? It may include 2,000 medical libraries worldwide, or as many as 5,000, depending on the applicability of the research. It may be limited to 125 large pharmaceutical companies, or it may extend to 2,000 or more worldwide. Business or technology journals may be of interest to only graduate school libraries or extend to all four-year colleges and universities, or appeal to junior and community college library needs. They may be useful to thousands of companies in the United States or, if the subject matter is international, worldwide. The governing criterion is always the extent of application of the information provided.

Financial projection

Once a proposal has been examined from the editorial side, it is subjected to financial projections. A hypothetical, hopefully realistic, five-year profit-and-loss statement is generated. To build this projection, every line item that needs to be considered in publishing a journal is estimated. This analysis begins by considering the number of pages that are to be published each year. Most often a journal is launched with quarterly or bimonthly issues of, perhaps, eighty or ninety-six pages. The level of manuscript flow is then estimated for the five years of the projection in order to project growth of issues. The next step is to project the potential growth in subscribers over the five years, the anticipated subscription price, and associated marketing cost. The cost of promoting even a moderately sized new journal, for example, can be in excess of $20,000 in each of the first few years of publication. Projection such as these require some understanding of the market and of a likely growth of the specialty field and its research activity. (Journals can only reflect, and not create, the actual research in a field).

From these calculations derive production, editorial, fulfilment, mailing, and postage costs. Even the cost of the editorial office must be included. While the editor-in-chief usually earns a modest stipend and the publisher provides some payment to the editor's institution to help play for office costs, the editorial board more often serves without pay, earning only professional fulfilment and, perhaps, an annual editorial board lunch or dinner in compensation for their service. (While its merits are oft-debated, peer review has as its goal the assurance of publication of valid, significant, and unduplicated research.) Income from the sale of author reprints is estimated along with the attendant costs. The departmental costs for internal management of the journal are estimated, and a share of the corporate overhead costs is assigned. Some examples of these kinds of costs are those of computer input of subscriber information, customer-service personnel, in-house editor/ author negotiations, production department purchasing of raw materials and contracting with vendors, and accounts receivable staff.

The acquisitions department works with editorial office in the early years of a new journal to define the specific aims and scope of the journal, make recommendations for sections to structure the publication, and help initiate and sustain manuscript flow. Acquisitions also works with the scientific editor and the design staff to develop the page and cover design.

Having projected anticipated costs, prices for the journal over the planning period are estimated, and the financial model is built. The price of the journal in the first year is set to be consistent with those for typical journals in similar size. For each year, anticipated cost increases due to inflation and growth in number of issues and pages are computed. Prize increases are dependent to a large degree on the number of pages upon which the costs and prices are based. If for some reason more pages are published than planned in any year, the financial performance of the journal is negatively affected because of the extra costs resulting from those additional pages. In an established, perhaps profitable, journal, this effect may be less damaging because there may be sufficient income to offset some of the costs, although profit level will still be affected. In a young, struggling journal, though, such an effect is likely to eliminate any potential for profit at all within the first few years. Because of growing research needs, editors frequently seek permission to publish more pages. To counter this trend, publishers work with the editors to increase their rejection rate on to limit the scope of the journal to only certain categories of papers. For example, a decision might be made to eliminate case report papers in order to concentrate on original research.

One crucial component of costs is inflation. Costs incurred in the industrial sector, including publishing companies, are different from those included in the Consumer Prize Index. Two significant examples that experienced particularly steep increases in 1988 are paper stock and postage. Prices of publication paper stock increased by over 30 percent during the year. Paper stock can represent 30 to 40 percent of the manufacturing costs of a journal, which in turn compose 20 to 35 percent of a journal's cost. Thus, an increase of 30 percent in paper cost can translate into an overall cost increase of nearly 5 percent for a journal. Postage costs increased in the United States in 1988. Second-class rates, under which most journals are mailed, increased an average of 18 percent. Representing between 5 and 10 percent of total costs, postage expenses have a material effect on a journal's profitability.

Growth projections have become more difficult in the last few years because of the change in the journal marketplace. The tens of thousands of existing journals leave little room for new ones. Comparisons of growth projections for new journals launched in the 1960s and 1970s with those launched in the 1980s indicate that projections are much more modest as journals are launched in smaller, more targeted subfields that represent smaller markets. While direct comparisons are difficult because of intangible variables such as quality and subject area, even a high-quality journal in a good-sized area may today expect to reach only 70 to 80 percent of the number of subscribers as a similar journal started ten years ago. For smaller journals, which less certain editorial quality, the saturation may be lower.

The resources a publisher must commit to building a journal have not decreased, though. Both staff and financial resources are required in sufficient amount to give a journal a fair opportunity for survival. Many publishers commit to three to five years of publication to determine a journal's potential. During, and even after, that period most publishers take every step they can to save an ailing journal.

Such corrective efforts are important for two related reasons. First, publishers realize that establishing a new journal places a strain on the resources of a library unless that journal fulfils a distinct market need. Because every effort is made to ensure that the journal will be needed before it is introduced, publishers believe that they are building a trust by those who enter a subscription, and who then established records, procedures, and space for receipt and storage of the journal. Therefore, the decision to discontinue a new or existing journal is made only after careful deliberation and after attempting to save it by a variety of measures. Second, of course, is the fact that starting a new journal commits scarce staff and fiscal resources. Discontinuing a journal ceases all hope of recovering the significant investment that has been made.

Publishers establish benchmarks by which to measure the success or failure of a new journal for each year. Among those criteria is the stipulation that the total investment in a new journal be recovered by a predetermined year. This investment can exceed $150,000, and it is rare for it to be recouped in fewer than five years of publication. In fact, a new journal does not usually break even on an annual basis on direct out-of-pocket costs until after the second year. It makes sense, therefore, that publishers are vitally concerned that new product ideas be subjected to serious analysis before giving the green light to them.

Once a journal has recouped its investment, it is hoped that it will begin to produce a measure of profit which the company can then earmark to research new, cost saving production technologies and to launch new journals.

Care for the ailing journal

While there is, of course, nothing that can be done if no real market exists for a journal, publishers will attempt to take several types of corrective actions to enhance a journal's potential for survival. Manuscript flow can be encouraged. There are journals for which research has indicated a need an for which customers have entered subscriptions, thus demonstrating a market need, but to which papers, or the right kind of papers, are not attracted. One pediatric journal started seven years ago experienced this problem — it initially had difficulty attracting the type of clinical paper for which it was designed. By careful management of the rejection rate, the journal was able to add quality manuscripts just as effectively as it had added subscriptions. This unique journal is only now reaching its potential, and its investment has not yet been recovered. A similar example is a systems journal that Elsevier started about four years ago. It was chronically late because of difficulty in attracting manuscripts. It had a healthy renewal rate, however, which indicated subscriber interest. This level of interest impelled the company to work to sustain the journal. Research interest paralleled subscriber interest through maintenance of good quality, and the journal is now performing well.

The publishing industry is keenly aware of the disruption to the information market that is caused by fractionalization of journals. Every publisher can cite examples of efforts at consolidation in order to minimize such fractionalization. One example is that of a very promising small medical journal published by Elsevier, growing in size and in subscribers, confronted with a small but expanding society expressing a firm commitment to launching its own journal. Not wishing to further fragment the market, the company reached an agreement to convert the journal it owned into a new journal owned by the society and published by Elsevier. While it was recognized that a compromise such as this would cause some confusion with library record keeping, the advantages to librarians, the scientific community, and the publisher by avoiding publishing an additional journal out-weighed any initial confusion.

The research publishing environment

Most of the factors discussed here are practical considerations that determine everyday decisions. Publishers are fully cognizant of the fact that these daily publishing decisions have an enormous impact on the scientific and information community. They therefore believe themselves to be a partner in that community and think about the worldwide effect of publishing policy on the company level. While conducting the analyses related to introducing a new journal, publishers retain the spectre of tight library budgets, which are odds with the growing needs of researchers for timely access to scientific developments. Publishers participate in dialogue within the professional community about the ethics of dual publication and scientific validity to try to prevent the clogging of information channels with invalid or unnecessary information. Particularly among medical media, publishers compete with non-peer-reviewed journal media that have different cost structures that allow subsidization of prices by the advertising community. Efforts at protecting copyright from international piracy help keep subscription prices down. At the same time publishers establish structures to try to respond quickly to appropriate permissions requests and to ensure access to needed scientific and business information. Publishers consider the potential impact over the long term of resource-sharing and electronic publishing and their effect on pricing. Editors and publishers debate the same question that librarians ponder, that is, whether the printed journal will even exist in twenty years as a subscription product. Even the effect of government policy on transborder information flow is a consideration in determining publishing policy.

Questions such as these may seem so theoretical as to be incidental to the publishing process. However, a journal is launched with the expectation that it will continue for decades, and, of course, many current journals have existed for well over twenty-five years. Consequently, concerns about the direction of scholarly information publishing are not significant.

In conclusion, there is one seemingly small point that infuses the editorial environment in which new publishing products are developed. After all the editorial and competitive analyses, objective financial calculations, and difficult decisions, the excitement generated within the publishing house about a new product is contagious. There is a lot of care and personal enthusiasm that goes into building a new journal. As a result, a new journal idea must have merit to generate the commitment to publish. Journal ideas die for lack of a staff advocate. This is a little-considered but important decision point along the path from initial idea to successful journal.

Janet D. Bailey,
Current Contents, (1990) 5

"Sometimes I just feel like processing some data, but I have no data to process — other times I have the data, but I have nothing to process it with."

NIH begins triage experiment with peer review of proposals

In the hope of easing the workload of reviewers, NIH has begun an experiment involving triage of grant applications. Under normal NIH procedures, all grant proposals undergo detailed, time-consuming reviews from one of the agency's study sections — peer review groups composed of experts who meet for three days three times a year. Only about one in four of the proposals gets funded, however. In the new experiment, four study sections have been told to reject the weakest applications as "noncompetitive" after a preliminary reading. But if even one study section member objects to classifying an application as noncompetitive, the proposal in question will receive a full-scale review, according to Anthony Demsey, associate director for referral and review in NIH's division of research grants. The experiment, which began last month with the February round of study section meetings, likely will continue through the June round before a decision is made to expand or discontinue triage.

C&EN (March 21, 1994) 15

Citation Counts and Social Comparisons: Scientists' Use and Evaluation of Citation Index Data

Data from samples of biochemists and sociologists show that nearly all are familiar with citation indexes and that the two groups are equally likely to have used a citation index for bibliographic purposes. We develop three hypotheses from social comparison theory to account for variation in use and evaluation of citation counts as indicators of scientific achievement: (1) more highly cited scientists will more often use and more highly evaluate citation counts as indicators of scientific achievement than will less cited scientists, (2) these relationships will be stronger for sociologists than for biochemists, and (3) sociologists as a whole will more often use and more highly evaluate citation counts than biochemists. Finally, among sociologists, we hypothesize that those primarily interested in quantitative research areas will use and favor citation counts more than those with primarily qualitative or theoretical interests. Our data support all but one of these hypotheses. We also report unexpected differences in use and evaluation of citation counts by sex and departmental prestige.

Academic scientists are ambivalent about attempts to measure scholarly contributions. They often view such attempts negatively because they fear that using quantifiable characteristics to gauge contributions leads to the distortion of research products. For example, widespread use of publication counts as basis for promotion decisions is sometimes blamed for a deluge of trivial publications. Scientists see their research as craft work (Whitley, 1984:6-7), and many believe that using one or two easily quantifiable aspects to assess a scientist's scholarly product tends to debase that product[1].

Yet assessing scholarly contributions quantitatively has undeniable attractions as well. Decisions about tenure, promotion, and other academic awards are necessary, and quantitative information about performance ordinarily plays a role in them (Braxton and Bayer, 1986). Furthermore, reliance on quantitative measures may protect evaluators from charges that their decisions are pluralistic, or are based on candidates' ascriptive characteristics (Lewis, 1975:40-42). Finally, academic research work is a nonroutine, often ambiguous activity with infrequent formal assessments of one's performance. Individuals in such circumstances are likely to seek evidence about their relative performance (Festinger, 1954), and quantitative forms of evidence may be especially attractive because they appear to be "objective".

Since its initial publication in 1964 by the Institute of Scientific Information, the *Science Citation Index* (*SCI*) has made it relatively easy to count how often an individual has been cited by other scholars during a given year. Although the *SCI* was developed as a bibliographic tool to help scientists trace their areas of interest (Garfield, 1979:49-61), measuring the impact of individuals' work has become the *SCI*'s most visible and controversial use (Wade, 1975; Garfield, 1979:240-252). The controversy about such use bears witness to scientists' ambivalence toward citation counts as measures of scholarly performance.

In this paper, we report results from a survey of academic scientists' use and evaluation of citation count information. In part, we sough to determine if patterns of use and evaluation are consistent with Festinger's (1954) social comparison theory, especially as integrated with attribution theory (Goethals and Darley, 1977). Festinger hypothesized that people desire to evaluate their own abilities, and that when they lack objective measures, they resort to comparing themselves to others. Goethals and Darley added that people want to find that their abilities — necessarily measured in terms of performance — compare favorably with others'. We developed three hypotheses about scientists' use and evaluation of citation counts from social comparison theory. The first and most general hypothesis is

> 1. Scientists who are highly cited will be more likely to use citation counts for gauging scholarly contributions than will infrequently cited scientists. The former will also evaluate citation counts for this purpose more highly than the latter.

We assume that most scientists feel that their own research contributions are important, but that they also seek support for these self-evaluations. Discovering that one's work is highly cited confirms positive self-evaluations and at the same time validates citation counts as a measure of scholarly contribution. This is a kind of construct validation in which *both* constructs — the merit of one's work and the value of citation counts — support each other. In contrast, infrequently cited researchers should be less likely to regard citation counts a valid measure of scholarly contributions because citation counts do not support their tendency to evaluate their own work positively.

We tested this hypothesis by drawing samples of scientists at U.S. universities in two quite different fields: biochemistry and sociology. We selected these fields in part because of the availability of sampling frames that gave university affiliations and other relevant information. We expected the relationship stated in hypotheses 1 to apply in each field, but on the basis of evidence that the natural sciences exhibit a higher level of consensus than the social sciences[2], we also expected certain differences between them. Specifically, studies of the social organization of research work (Lodahl and Gordon, 1972; Hargens, 1975), the evaluation of scholars (Yoels, 1974; Hargens and Hagstrom, 1982), competition for priority in reporting research findings as indexed by both the incidence of being anticipated before publication and publication in the form of articles rather than books (Hagstrom, 1965), and the evaluation of research proposals (Cole and Cole, 1981) and papers submitted to journals (Zuckerman and Merton, 1972; Pfeffer, Leong, and Strell, 1977) all show higher levels of consensus in the natural than the social sciences. Evidence also suggests that differences in overall levels of consensus affect scholars' attributional patterns; Rubin (1975) found that chemists who had been denied tenure at Ph.D.-granting departments were more likely to blame themselves for their failure than sociologists, who more often disputed the validity of the criteria by which they were judged. These considerations led to two more hypotheses:

> 2a. Scientists in fields with relatively low levels of consensus on appropriate research questions and techniques are more likely to use citation counts to measure individuals' scholarly contributions than scientists in fields with relatively high levels of consensus. The former will also evaluate such use of citation counts more favorably than the latter.

This relationship derives from Hypothesis II of Festinger's (1954) statement of social comparison theory: when more objective means of evaluation are unavailable, people evaluate themselves by comparison with others. In this case, lack of consensus about the importance of contributions in a field should lead its members to be less certain about the value of their own and others' research contributions than in fields with high levels of consensus, and this should lead them to seek means of gauging contributions more than members of high-concensus fields[3]. Furthermore,

> 2b. The relationship between one's own citation level and one's use of citation counts to measure scholarly contributions will be stronger in fields with less consensus than in fields with more consensus. Similarly, the relation between one's own citation level and one's evaluation of citation counts as a measure of scholarly contributions will be greater in low- than in high-consensus fields.

The predictions in hypothesis 2b follow from those in hypotheses 1 and 2a. Highly cited biochemists should feel less need to use citation counts for evaluation since at best they would be redundant with widely shared evaluations among others in the field. As a result, the validation citation counts afford to those whose work is highly cited should be less in biochemistry than in sociology. Moreover, infrequently cited sociologists should be more negative toward citation counts than infrequently cited biochemists because the former are more likely to be able to argue that the citation-count "evidence" is inconsistent with other evaluations of their work. Indeed, in sociology, having one's work infrequently cited is sometimes viewed as a sign that one rejects current research fads and instead concentrates on more important, although unfashionable, projects.

We also developed an hypothesis that is unrelated to social comparison theory but which stems from scientists', perhaps especially social scientists, scepticism about trying to measure scholarly contributions. Sociologists often disagree about whether quantitative data can contribute significantly to understanding social behavior. Therefore, we reasoned that those who doubt the value of quantitative data generally should have a low opinion of citation counts quite apart from other factors. Thus, even if sociologists are more positive toward citation counts than biochemists as a result of social comparison processes, the fact that a subset of sociologists denigrate any form of quantitative evidence could obscure the field differences.

Each of the above hypotheses specifies a relationship that should hold independently of other possible causes of scientists' use and evaluation of citation counts. To evaluate the accuracy of the predictions, an analysis must, insofar as possible, include other causes that may be correlated with the independent variables at issue. Thus, we gathered data on other variables that might affect the use and evaluation of citation counts beyond the effects discussed above.

Sampling and data collection

We sampled from the lists of biochemistry graduate faculty in the American Chemical Society's *Directory of Graduate Research* (1984) and sociology graduate faculty in the American Sociological Association's *Guide to Graduate Departments in Sociology* (1985). We decided to draw the samples from high- and low-prestige departments, as measured by departments' reputational rankings reported Jones, Lindsey, and Coggeshall (1982), because the reputational rankings of departments are substantially associated with measures of the eminence of their members (Cole and Cole, 1973; Long, 1978).

We sought responses from at least 50 associate and full professors in each discipline-prestige combination, and expected a response rate about 75% given the brevity of our questionnaire, which we designed to fit a postcard (our questionnaire is reproduced in the Appendix). In addition, we wanted to include no more than one-third of the members of any one department in our sample. Accordingly, we began by determining the number of high-prestige biochemistry departments required to produce a sampling frame of at least 200 persons, the number of low-prestige biochemistry departments with met the same condition, etc[4]. Next, we determined the sampling fraction for each group that would yield a sample of approximately 66 members. We then randomly selected the four samples and mailed explanatory letters plus questionnaires in late April 1985. Three weeks later we mailed follow up questionnaires to nonrespondents. Table 1 gives, for each of the four groups, the range of prestige scores of the departments, the numbers of associate and full professors,

Table I.
Characteristics of Sample Strata and Response Rates, by Discipline and Department Prestige Level

Discipline-prestige combination	Range of prestige scores	No. of Associate and Full Professors	No. Sampled	No. Responded	Response Rate
Biochemistry High prestige	74-65	234	66	46	70%
Biochemistry Low prestige	45-33	200	67	52	78%
Sociology High prestige	71-63	195	64	49	77%
Sociology Low prestige	43-28	209	69	57	83%

Source: Jones *et al.* (1982).

the numbers we selected for our samples, the numbers who returned questionnaires, and the return rates[5].

In addition to questionnaire data, we collected biographical data on the members of our samples. We obtained information on their sex, academic rank, and year of Ph.D. (or M.D. for a few biochemists) from the directories we sampled from. For a few sample members for whom the directories did not include these data, we used the most recent edition of *American Men and Women of Science*. We also collected bibliometric data, including each sample member's number of citations in the 1984 *SCI* or *Social Sciences Citation Index*, and the median number of citation for all of the associate and full professors in each sample member's department. We collected data on the latter variable to assess the possibility that researchers' perceptions of their relative eminence are based on their relative standing among the members of their own departments as well as on their relative standing among all the members of their disciplines. After gathering these and other data, we worked only with identification numbers to protect our respondents' confidentially.

Finally, for our sociology sample we constructed a measure of whether a respondent is likely to view the quantitative analysis of empirical data favourably by using information about the specialties they listed in the 1985 *Guide to Graduate Departments in Sociology*[6]. Our measure classified 34% of the sociologists in our sample as quantitatively oriented, 46% as mixed, and 20% as nonquantitatively oriented[7].

L.L. Hargens, H. Schuman, Current Contents (1991) 7

APPENDIX

The Survey Questionnaire

1. **Are you all familiar with the** *Science Citation Index* (*Social Sciences Citation Index*), **which lists individuals alphabetically and shows the citations to each of their publications during a given year?**

    .....1. Yes .....2. No, never heard of it (please return post card)

2. **Have you ever consulted the** *Science Citation Index* (*Social Sciences Citation Index*)?

    .....1. Yes .....2. No (go to Q.3)

    For what purpose? (Check all that apply)

    .....1. To use citations to an earlier work to locate more recent work on that topic.
    .....2. To determine how frequently particular individuals have been cited during a certain period.
    .....3. Other (please specify)

3. **Has your department ever made use of citation counts in making decisions about hiring, promotion or salaries?**

    .....1. Yes .....2. No .....3. Don't know

4. **Overall, how useful do you think a citation count is in evaluating the contributions of someone in your field? (check one point on the line)**

    Not useful at all....1..2..3..4..5..6..7..8..9..10....extremely useful

Notes

[1]Stigler (1984) makes this point forcefully in his satire "An Academic Episode" in which an academic administrator radically changes faculty members' behavior by setting up and altering a system for measuring scholarly merit.

[2]A number of concepts roughly correspond to our "level of consensus", including "paradigm status" (Lodahl and Gordon, 1972), "degree of codification" (Zuckerman and Merton, 1972), and the "hard-soft" dimension (Biglan, 1973; Smart and Elton, 1982).

[3]Hargens and Hagstrom (1982) studied the link between consensus and the ability to gauge research potential and past contributions and found results consistent with their predictions about how status-attainment patterns should vary across fields with differing levels of consensus.

[4]We needed at least 200 members in each of the four groups because (1/3)(3/4) 200 = 50. We excluded from our sampling frame persons with ranks below associate professor because their typically low citation levels only reflect their professional youth. We also omitted professors emeriti.

[5] Table 1 shows that members of highly ranked departments were less likely to return questionnaires than members of low ranked departments. In addition, within each field citations to sample members' work was negatively correlated with whether they responded: for biochemistry r = -0.17 and for sociology r = -O.18. Thus, eminent scholars are slightly underrepresented in our samples.

[6] We began by listing specialties whose members are, in our experience, typically either favorably or unfavorably disposed toward using quantitative data. Our list of quantitatively oriented specialities included "quantitative methods," "statistics," "research methods," "evaluation research," "demography," and "population." Our list of nonquantitatively oriented specialties included "theory," "interpretative sociology," "comparative and historical sociology," "macro sociology," "religion," "culture," "cultural change," "field methods," "psychoanalytic sociology," "Marxist sociology," and "mathematical theory and modeling" (members of this last specialty often emphasize the importance of normal models for analyzing social phenomena and express scepticism about the value of statistical analyses of empirical data). We classified specialties not included in either of these two lists as "mixed". Next, we examined each sample member's list of specialties. We classified sample members as quantitatively oriented if we they listed only quantitative or both quantitative and mixed specialties. We classified sample members as nonquantitatively oriented if they listed only nonquantitative or nonquantitative and mixed specialties. We classified as mixed sample members with all other combinations. Note that since sociologists typically listed three or four specialties in their entries in the *Guide to Graduate Departments in Sociology*, the validity of our classification of individuals is probably greater than that of our classification of specialties.

[7] We each classified the sociologists in our sample independently and obtained discrepant classifications for only 13 of the 133 sociologists (and resolved the discrepancies on a case-by-case basis). The association between our independent classifications, when we treat the three categories as an ordinal measure of orientation toward quantitative data, yielded a coefficient of 0.997.

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Goethals, G.R. and **Darley, J.M.** (1977). "Social comparison theory: an attributional approach," in Social Comparison Processes (**J.M. Suls** and **R.L. Miller** Eds.) pp. 259-278, Wiley, New York.

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Lewis, L.S. (1975). Scaling the Ivory Tower: Merit and Its Limits in Academic Careers. Johns Hopkins Univ. Press, Baltimore, MD.

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Rubin, L.C. (1975). "The Dynamic of Tenure in Two Academic Disciplines," Unpublished Ph.D. dissertation. SUNY at Stony Brook.

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Wade, N. (1975). "Citation analysis: A new tool for science administrators," *Science* 188, 429-432.

Whitley, R. (1984). The Intellectual and Social Organization of the Sciences. Oxford Univ. Press, Oxford, UK.

Yoels, W.C. (1974). "The structure of scientific fields and the allocation of editorships on scientific journals: Some observations on the politics of knowledge." *Sociological Quarterly* 15, 264-276.

Zuckerman, H., and **Merton R.K.** (1972). "Age, aging and age structure in science," in Aging and Society (**M.W. Riley** *et al.*, Eds.) Russell Sage, New York.

Az alábbi levél sajnos már az áprilisi számunk lapzártája után jutott szerkesztőségünkhöz. Úgy tartjuk azonban, hogy a jó humor sohasem időszerűtlen, és reméljük, hogy e kis remekmű szarkazmusát lapunk olvasói is méltányolni fogják.

Dear Fellow Scientists:

This letter has been around the world at least seven times. It has been to many major conferences. Now it has come to you. It will bring you good fortune. This is true even if you don't believe it. But you must follow these instructions:

- include in your next journal article the citations below.
- remove the first citation from the list and add a citation to your journal article at the bottom.
- make ten copies and send them to colleagues.

Within one year, you will be cited up to 10,000 times! This will amaze your fellow faculty, assure your promotion and improve your sex life. In addition, you will bring joy to many colleagues. Do not break the reference loop, but send this letter on today.

Dr. H. received this letter and within a year after passing it on she was elected to the National Academy of Sciences. Prof. M. threw this letter away and was denied tenure. In Japan, Dr. I. received this letter and put it aside. His article for Trans. on Nephrology was rejected. He found the letter and passed it on, and his article was published that year in the New England Journal of Medicine. In the Midwest, Prof. K. failed to pass on the letter, and in a budget cut back his entire department was eliminated. This could happen to you if you break the chain of citations.

1. Miller, J. (1992): Post-modern neo-cubism and the wave theory of light. Journal of Cognitive Artifacts, 8, 113-117.
2. Johnson, S. (1991): Micturition in the canid family: the irresistible pull of the hydrany. Physics Quarterly, 33, 203-220.
3. Anderson, R. (1990): You place or mine?: an empirical comparison of two models of human mating behavior. Psychology Yesterday 12, 63-77.
4. David, E. (1994): Modern Approaches to Chaotic Heuristic Optimization: Means of Analyzing Non-Linear Intelligent Networks with Emergent Symbolic Structure. (doctoral dissertation, University of California at Santa Royale El Camino del Rey Mar Vista by-the-sea).

Der Forschungs Index

A kutatási index
A német kutatás vezető intézetei

Csillagászat, asztrofizika

A befolyásosak

	Intézmény	Idézetek száma (1993 áprilisig)	Publikációk száma (1990-1992)
1	Max Planck Extraterresztriális Fizikai Intézet, Garching	982	341
2	Max Planck Asztrofizikai Intézet, Garching	888	306
3	Max Planck Radioasztronómiai Intézet, Bonn	812	374
4	Európai Déli Csillagvizsgáló (ESO), Garching	764	264
5	Bonni egyetem	350	179
6	Max Planck Csillagászati Intézet, Heidelberg	287	86
7	Max Planck Aeronómiai Intézet, Katlenburg-Lindau	252	134
8	Központi Asztrofizikai Kutatóintézet, Potsdam-Babelsberg	211	160
9	Kieli egyetem	204	90
10	Göttingeni egyetem	192	81

A csillagászat, az egykori NDK egyik erőssége

A csillagászat és az asztrofizika együttvéve a fizika egyik legnagyobb részterülete. Ezeket azonban nem lehet teljesen elkülöníteni a rokon szakterületektől, mint pl. a nagyenergiájú fizikától. Mivel általában véve a nagyenergiájú fizika szakterületén sokkal többet idéznek, mint a csillagászat szakterületén, átfedések esetén az idézettségi gyakorisági rangsorokban határozott eltolódások következhetnek be.

Németországban a három Max Planck intézet, azaz a Radioasztronómiai, az Extraterresztriális Fizikai és az Asztrofizikai Intézet a legtöbbet publikáló intézmény, és idézettségi gyakorisága alapján egyben a legbefolyásosabb is. Ha hozzávesszük még az Európai Déli Csillagvizsgálót, akkor nyilvánvalóvá lesz, hogy a München melletti Garching a német csillagászat és asztrofizika fellegvára. A hajdani NDK-nak is ez a tudományos szakterület volt az egyik erőssége, amint ezt a Postdam-Babelsbergi Asztrofizikai Központi Kutatóintézet helyzete is mutatja.

Feltűnőek még a hatékonyak rangsorában a Karsruhei Atommagkutató Intézet és a Braunschweigi Műszaki Egyetem vezető helyei is. Itt nem csillagászati intézetekről van szó, azonban ezek munkája a csillagászat szempontjából fontos és ezért ezeket gyakran idézik.

Az aktívak

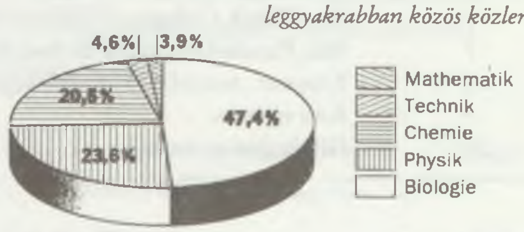
	Intézmény	Publikációk száma
1	Max Planck Radioasztronómiai Intézet, Bonn	374
2	Max Planck Extraterresztriális Fizikai Intézet, Garching	341
3	Max Planck Asztrofizikai Intézet, Garching	306
4	Európai Déli Csillagvizsgáló (ESO), Garching	264
5	Bonni egyetem	179
6	Központi Asztrofizikai Kutatóintézet, Potsdam-Babelsberg	160
7	Max Planck Aeronómiai Intézet, Katlenburg-Lindau	134
8	Bochumi egyetem	96
9	Kieli egyetem	90
10	Max Planck Csillagászati Intézet, Heidelberg	86

A hatékonyak			
	Intézmény	Egy publikációra eső idézetek száma	Publikációk száma
1	Karsruhei Atommagkutató Intézet, KFK	3,9	14
2	Braunschweigi Műszaki Egyetem	3,8	28
3	Max Planck Csillagászati Intézet, Heidelberg	3,3	86
4	Max Planck Asztrofizikai Intézet, Garching	2,9	306
4	Max Planck Extraterresztriális Fizikai Intézet, Garching	2,9	341
4	Európai Déli Csillagvizsgáló (ESO), Garching	2,9	264
5	Müncheni egyetem	2,7	69
6	Berlini Műszaki Egyetem	2,4	31
6	Göttingeni egyetem	2,4	81
7	Kieli egyetem	2,3	90

Németország és Kelet-Európa

A kelet-európai országok (Magyarország, Lengyelország és a volt Csehszlovákia) kutatói közös publikációikat legtöbbször német kollégáikkal írják. A kutatásban tapasztalható nemzetközi együttműködésről a bielefeldi egyetem tudományelemzési intézete a budapesti tudományelemzőkkel együtt írt tanulmányt. A magyarok együttműködő partnerei közül az angolok állnak a második helyen, a lengyeleknél és a csehszlovákoknál a franciák.

Magyar-német közös kutatás: A biológusok jelentetnek meg leggyakrabban közös közleményt.



"Sok kutató sok eredményt hoz létre"

A Bild der Wissenschaft (bdw) tudósítója: Kippenhahn professzor, Németországban a csillagászatot és az asztrofizikát egyértelműen az öt Max Planck intézet (MPI) uralja. Ez nem lesz Önnek, mint az asztrofizikai MPI volt igazgatójának meglepő. Kisebb távolságra, de jóval a többi főiskola előtt, a Bonni egyetem következik. Mi tünteti ki a bonni űrkutatást?

Kippenhahn professzor: Bonnban biztosan kitűnő munkát végeznek, elsősorban a kozmológia és a stellaris asztronómia területén. Ez azonban nem jelenti szükségszerűen azt, hogy a többi főiskolán a kutatás minősége kevésbé jó. Bonn előnye a rangsorokban bizonyára attól is függ, hogy ott a csillagászat különösen jól el van látva kutatókkal. Sok kutató sok eredményt hoz létre.

bdw: Érdekesnek számít a postdami ZIAP asztrofizikai központi kutatóintézetének helyzete. Hová esik az intézet kutatásainak súlypontja?

Kippenhahn prof.: A ZIAP jó példa az olyan kutatásra, mely már a volt NDK-ban is nemzetközi szempontból vezető helyen állt. Itt a napfizika egyike a legjobban feldolgozott témaköröknek, és a stelláris mágnesmezőkre vonatkozó kutatásokat világviszonylatban is először a ZIAP kezdeményezte.

bdw: A hatékonyak rangsorában feltűnő, hogy a csilagászatban és asztrofizikában az egy publikációra eső idézettség, három-négy idézetet jelent és ez nagyon alacsony. A többi tudományterületen ez az arány sokszor kétszámjegyű. Itt úgy tűnik a kutatók sokkal jobban tisztelik egymást, vagy van más magyarázat is erre?

Kippenhahn prof.: Én úgy vélem, ez az elemzés módjától függ. Biztos, hogy minden rangsor elárul valamit az egy szakterületen belül végzett kutatás minőségéről.

Azonban különböző szakterületeket ne hasonlítsunk össze egymással. A nagyhőmérsékletű szupravezetés a csillagászathoz és asztrofizikához képest viszonylag szűken behatárolt kutatási területet képvisel, ahol a kollégák munkáit szükségszerűen veszik figyelembe.

A csillagászati tudományok ezzel szemben nagyon sokrétűek és részben nem sok közük van egymáshoz. Miért idézzék a kozmológusok a napfizikusok munkáit, vagy a kvazárokkal foglalkozó kollégák a bolygókutatók publikációit?

Jürgen Nakott interjúja
Bild der Wissenschaft, 1993(7):6-7

Készült az Argumentum Könyv- és Folyóiratkiadó Kft. nyomdájában

Felelős kiadó: az MTAK főigazgatója