4. évfolyam · 8. szám · 1994. augusztus

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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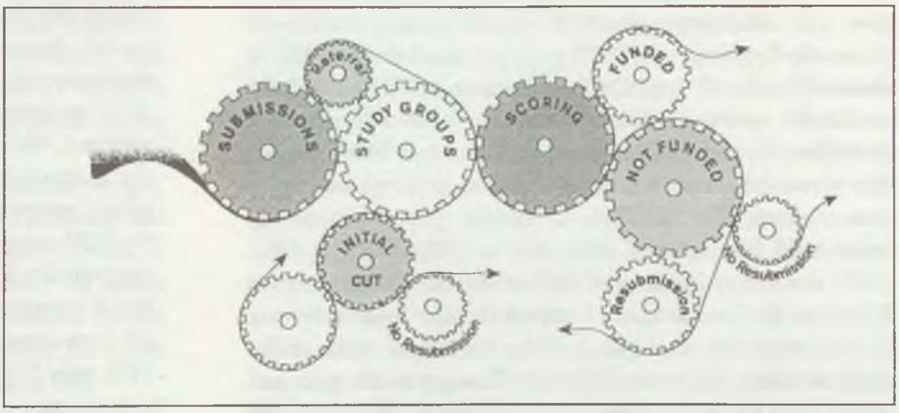
NIH Tunes Up Peer Review

To relieve stress on an overburdened grant system, NIH has begun asking members of its study sections to reject up to half the applications without any panel discussion

If you're waiting for your grant proposal to be reviewed by the National Institutes of Health (NIH) in a handful of areas, you could soon be in for a nasty shock. Normally, you would expect your grant to get an exhaustive peer review, and even if it was rejected, you would expect a long critique from the reviewers — a critique you could use to modify the proposal and resubmit it. Now, however, you may be unlucky enough to receive a quick rejection and a short written explanation. At least you won't be kept waiting for more than a month to get the bad news, and you can take some comfort from the fact that you won't be alone.



A grinding sound. The current peer-review machine spits out very few proposals before study sections meet and assign scores and percentile ratings to most submissions

Up to half the grant proposals routed to selected peer review committees, or study sections, may be quickly rejected as part of an experiment NIH launched last week as it tries to relieve the pressure on its overloaded peer-review system. If the experiment, which NIH calls triaging, is successful, it could eventually be extended to study sections. The goal is to allow peer reviewers to spend more time on top proposals and less efforts reviewing — and re-reviewing — grants that are unlikely ever to get funded. And this could be just the first step in overhauling NIH's peer-review system, for NIH officials are also talking about refocusing study sections and relying on electronic data submission.

These changes are signs that NIH has recognized that, after nearly half a century of reliable service, the engine of peer review has begun to show signs of wear and tear and needs tune-up. Harold Varmus, director of NIH, says reviewers complain about the "arbitrary" decisions they must make, the welter of "low-priority applications" they must review, and the number of repeat submissions they're seeing. Microbiologist Charles Moran of Emory University, who recently ended a stint on an NIH review panel, calls the experience "incredibly frustrating" and says he doesn't want to repeat it.

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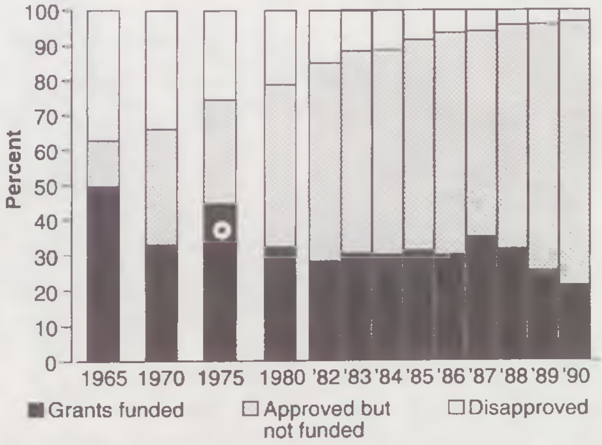


Can't say no. Peer reviewers over the years have found it harder and harder to reject a proposal at the outset.

At its core, the problem is a fiscal one: Too many good ideas are chasing too few dollars. The heavy demand — only 21% of proposals are funded, most at less than the original request — forces study section members to pick and choose among equally good proposals. What's worse, they must make such life-or-death decisions without time to consider fully the merits of each proposal.

Moran says that NIH's system is mired in paperwork, much of it pointless. In 1993, for example, NIH received 19,072 new and competing requests for small, investigator-initiated proposals that NIH calls RO1 grants; it funded only 4121 of them. Virtually all the 14,951 applications that didn't make it were fully reviewed, critiqued, and sent back with extensive comments — even if they stood no chance of being funded. Moreover, the system is now swamped with second- and third-time visitors: The number of revised grants among the submissions has increased from 25% in 1980 to 34% in 1992. AIDS researchers Jay Levy of the University of California, San Francisco, for one, not only bemoans the time spent reviewing resubmissions but also questions the wisdom of some of the final decisions. He says people are "coming back again and again" until the review panel finally surrenders and says, "My god, give it to him!"

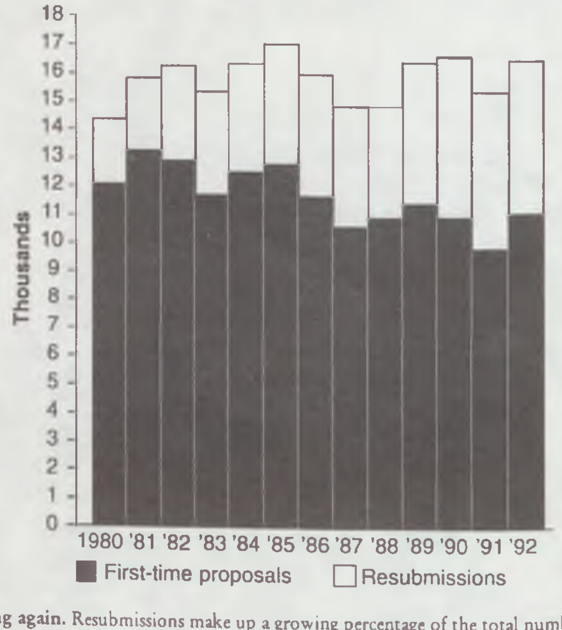
Moran and Levy — like many others — find the process particularly discouraging because of the difficulty of making distinctions among grants in the middle range — those beyond the top 10% but within the top quartile. Yet this is exactly where the ax falls when NIH draws a "pay line" dividing the fundable from nonfundable in what many reviewers consider to be an arbitrary, and, Moran adds, "depressing" process.

But relief — or at least an expression of sympathy — is on the way. Even before he came to NIH, Varmus had been talking about the need for improvements (*Science*, 26 November 1993, p. 1364), and last week he put the experiment in triaging into effect. The goal is to eliminate the least viable proposals — the bottom half of those submitted — before they are discussed at the meetings of the study sections. Triaging has already begun in four study sections that met during the last week of February and early March (Cellular Biology and Physiology-2, Human Development and Aging-1, Metallobiochemistry, and Experimental Virology). Within the next 2 weeks, the first letters bearing a new acronym — "NC" for not competitive" — will be mailed to applicants who failed to make the cut. That's when Varmus and his staff will begin to hear how the community likes it.

A strong backbone

Triaging isn't entirely new to NIH. Already, according to Wendy Baldwin, who was confirmed last week as NIH's director of extramural research, the institutes routinely set aside weak proposals that arrive in response to targeted research efforts, called "requests for applications". But for the investigator-initiated grants, which NIH calls the "traditional" form of biomedical research, the blunt 50% cutoff would be new. Thirty years ago, reviewers dropped more than one-third of the applications as not viable, but in recent years the number of "disapproved" submissions has dropped below 10%. NIH staffers have tried to put more steel into reviewers' backbones, so far with little success. However, some reviewers who spoke with *Science* — including Lawrence Rothfield of the University of Conneticut and Barry Honig of Columbia — said their study sections do practice a form of triage, by agreeing not to waste time discussing the least competitive grants. This sensible approach, says Rothfield, requires a strong chair; by endorsing its use, NIH is giving others a gentle push in the same direction.

To understand how triaging is meant to work, a quick tour though the Byzantine structure of NIH's granting system is needed. Researchers seeking new or renewed grants submit their proposals to NIH, which assigns them to one of about 100 study sections. Each proposal gets a primary and secondary reviewer, who prepare a written critique. A third reader may also examine the grant and help lead the discussion during the formal review session. The study section meets for 3 days near the NIH campus for a grueling session in which 80 proposals are typically dealt with. Every accepted proposals gets a score that is converted to percentile, after which it is forwarded to the advisory councils at each institute for funding or rejection. In 1993, only 21% of the reviewed RO1 proposals were funded.



Trying again. Resubmissions make up a growing percentage of the total number of ROI grant proposals sent to NIH.

Within a couple of weeks after the study section's meeting, says Anthony Demsey, an official in NIH's division of research grants, applicants receive word of their score. It takes another 6 to 8 weeks for the NIH staff to compile a "pink sheet" that contains a summary of the reviewers' written critiques and comments made during the meeting. These summary sheets, according to Baldwin, were once brief, but they now run to five pages and have acquired a "tutorial" quality. Preparing them is one of the most ardous and time-consuming aspects of the review process. Triage — if it succeeds — will do away with some of the tutorial essays.

To streamline the process, Demsey has been instructing panel members in the four experimental study sections to regard any proposal that they would rank below the 50th percentile as being "not competitive". Before coming to the meeting, the reviewers draw up lists of NC proposals. It takes two to nominate a grant for this status, and just one objection would "bring it back to full review." Proposals remaining on the list at the first day of the meeting receive no further review. Other proposals are handled in the usual way.

The process is flexible, Demsey explains. We are not saying, 'Hey look, you have 100 applications; get 50 of them out of here.' We understand that you might have a good pool of applications. Maybe out of 100 only 35 would be rejected at the start. But he is pushing reviewers to trim the ranks early. Once the initial cut has been made, the NIH staffers who assist the panels will quickly send out notices of rejection. And instead of receiving a long summary sheet, reviewers will get a short statement that does little more than convey notes written by the primary and secondary reviewers.

This truncated process, Varmus says, is intended to give reviewers more time to spend on the top-ranked proposals and to make reviewing a more satisfying experience. Varmus is worried about a "self-perpetuating disenchantment" in which study group members get turned off by the tedium of the process, avoid service, and allow it to decline further. He would like to see "the highest quality people" serving on panels.

Varmus also argues that quick reviews will benefit those who are rejected by giving them a "very clear signal that this is not an application that can be moved into the fundable category simply by responding to a series of complaints" listed in a pink sheet. Institute staffers, he claims, will gladly help with advice on "retooling", taking a new tack, or considering another topic.

It's too early to tell how the experiment is working, but Demsey reports that the first panel to meet was able to isolate only 20% of the low-ranking grants. He notes, however, that this panel had already done some of its reviewers and wasn't able to make the adjustment to triage quickly enough. The second panel did achieve its goal of 50%. Others are still in progress. Next, NIH will survey reviewers and reviewees for their reactions, and then, says Demsey, NIH will either drop the triage idea, apply it to all study sections, or mount another trial run.

More changes ahead

If triage works, it may be just the first step in a tune-up of the peer-review system. NIH has already regrouped its 100 study sections into 19 broad "review groups" — a move that should make it easier to realign study groups as areas of science wax and wane. (NIH made the change in part to get around a White House mandate last spring ordering all agencies to lower costs by cutting the number of advisory panels by at least 33%.) Although the change won't reduce paperwork or save money, Baldwin says it will "give us more latitude" to shift members from one panel to another as needed to cope with technical questions. It may also make it easier to change assignments and possibly to alter what molecular biologist Keith Yamamoto of the University of California, San Francisco, refers to as "anachronisms" that have been "locked into the system."

Other changes will be considered at a 2-day "brainstorming" meeting to be held this spring, organized by Yamamoto with encouragement from Varmus. Yamamoto wants NIH staffers and reviewers in the areas of cell and molecular biology to consider whether study sections reflect the best science being done in laboratories and whether "we are overcovering or undercovering any areas," says Yamamoto. The goal, according to Yamamoto, is to help the NIH review system focus on the most exciting research.

Because the peer-review system is at the heart of NIH's operations, researchers are likely to be skittish about even the most modest changes. So NIH officials are already bracing for complaints from applicants who feel they haven't been given a fair shake under the new system. But Yamamoto thinks that triage is already an unqualified success in one small way: It shows researchers that someone is paying attention to their concerns, and that's bound to improve morale.

Eliot Marshall,
Science, 263:1212-3 (4 March 1994)

In science the excellent is not just better than the ordinary; it is almost all that matters. It is therefore fundamental that this country should energetically sustain and strongly reinforce first-rate work where it now exists.

(*Scientific Progress, the Universities and the Federal Government,* The White House, Washington, DC, 15 November 1960)

Research trends
... a performance report

Each month this feature exploits ISI's unique publication and citation data to illuminate current trends in research and to reveal research performance. A quantitative approach defines each story and an accompanying commentary highlights the most prominent results of the analysis.

Top 25 U.S. Institutions in Chemistry Ranked by Citation Impact, 1988-1992

Rank	Name	Papers	Citations	Cites/ Paper
1	Harvard University	937	8,465	9.03
2	Caltech	821	6,817	8.30
3	Yale University	749	5,953	7.95
4	University of Chicago	713	5,606	7.86
5	Rice University	404	3,014	7.46
6	AT&T Bell Labs	1,091	8,088	7.41
7	Northeastern University	256	1,840	7.19
8	Univ. Calif., Santa Barbara	808	5,776	7.15
9	Univ. Calif., Los Angeles	894	6,165	6.90
10	Stanford University	1,105	7,578	6.86
11	Univ. Colorado, Boulder	737	5,008	6.80
12	MIT	1,486	10,076	6.78
13	Lawrence Berkeley Laboratory	890	6,021	6.77
14	Univ. Calif., Berkeley	1,680	11,310	6.73
15	Argonne National Laboratory	885	5,818	6.57
16	Indiana University	848	5,545	6.54
17	Northwestern University	928	6,063	6.53
18	Univ. North Carolina, Chapel Hill	690	4,487	6.50
19	Sandia National Laboratory	540	3,509	6.50
20	Univ. Calif., San Diego	625	4,053	6.48
21	Univ. Calif., Irvine	489	3,105	6.35
22	IBM	1,603	9,922	6.19
23	University of Utah	1,069	6,593	6.17
24	University of Pittsburgh	974	5,934	6.09
25	DuPont Corporation	1,177	7,123	6.05

Source: ISI's Science Indicators Database, 1988-92

New Method, New Numbers Yield
Latest List of Chemistry's Comers

Which institutions worldwide rank at the top for recent research in chemistry?

That's a question *Science Watch* asked two years ago, when it drew up a list of 50 universities from around the globe that scored best in terms of citations per paper for chemistry articles published 1984-1990. The results of this survey excited much interest and not a little controversy in the United States and abroad, since in some instances peer judgment and citation statistics contrasted sharply. In particular, certain historically strong universities found themselves bested by younger up-and-comers. Naturally, the Young Turks hailed these findings, while the Old Guard grumbled about the method used. Different methods produce different results, as all scientists know. So, with this issue, *Science Watch* returns to ask the same question but in different way.

This time, chemistry articles indexed by ISI between 1988 and 1992 were surveyed. Included in the current analysis were all types of journal articles, whereas previously only discovery accounts, reviews, and notes were counted. But the most significant difference in methodology this time out was the identification and inclusion of chemistry papers published in the high-impact journals *Science*, *Nature*, and *Proceedings of the National Academy of Sciences of the USA*. In the last study, papers published in multidisciplinary journals were not considered because there was no way to select chemistry articles from among those on other subjects that also appeared within these titles. Now ISI's Research Department employs an algorithm that scans the reference lists of papers published in multidisciplinary journals to find those that frequently cite a specific field. Such papers can then be categorized or tagged, according to the subject area most cited. About 60-70% of these papers can be categorized in this way.

Although the omission of papers published in high-impact multidisciplinary journals was uniform for all institutions, it nonetheless provoked criticism in that some of the best papers from the various institutions had not been taken into account (see, for example, *Science*, 260[5110]:885, 14 May 1993). *Science Watch*, thus, sought to address this criticism and to look for any large differences in the results.

For the new ranking, only those institutions that published 250 or more papers were ranked. U.S. and non-U.S. institutions are presented separately here, since, owing to the large number of U.S. publications in the ISI database and the tendency for U.S. researchers to cite the work of colleagues in the United States, the citation scores for U.S. institutions are typically higher than those for non-U.S. institutions.

Top 25 Non-U.S. Institutions in Chemistry Ranked by Citation Impact, 1988-1992				
Rank	Name	Papers	Cit-ations	Cites/ Paper
1	Tel Aviv University	394	2,522	6.40
2	Fritz Haber Institute	457	2,532	5.54
3	Weizmann Inst. Science	515	2,555	4.96
4	Max Planck Inst. Coal Research	379	1,832	4.83
5	University of Cambridge	1,809	8,531	4.72
6	National Research Council Canada	1,339	6,296	4.70
7	University of Strasbourg 1	810	3,807	4.70
8	Max Planck Inst. Biophys. Chemistry	257	1,204	4.68
9	Swiss Federal Inst. Technology (ETH)	1,372	6,396	4.66
10	University of Basel	453	2,112	4.66
11	University of Southampton	743	3,344	4.50
12	Centre d'Etudes Nucleaires	385	1,702	4.42
13	University of Bristol	849	3,738	4.40
14	University of Lausanne	426	1,867	4.38
15	Inst. Molecular Science, Okazaki	746	3,257	4.37
16	University of Mainz	897	3,862	4.31
17	Philips Research Labs	356	1,535	4.31
18	University of Oxford	1,574	6,722	4.27
19	KFA Julich GmbH	449	1,910	4.25
20	University of Florence	576	2,445	4.24
21	State Univ. of Groningen	672	2,832	4.21
22	University of Frankfurt	488	2,038	4.18
23	University of Zurich	410	1,711	4.17
24	Australian National University	711	2,933	4.13
25	University of Sussex	714	2,928	4.10

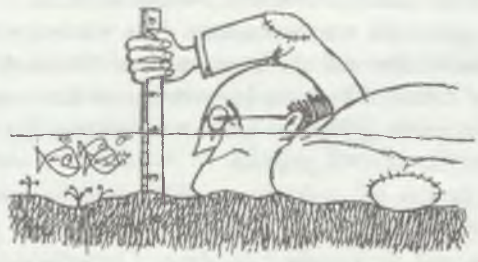
Source: ISI's Science Indicators Database, 1988-92

Harvard tops the table among U.S. institutions, having placed second in the last survey. Caltech, which previously capped the chemistry chart, ranks second this time. Chicago, UC Santa Barbara, Yale, and Stanford, which turn up in the new top ten, also ranked in the top ten in the previous analysis. MIT dropped considerably from last time to this time, while Rice rose sharply in rank. All in all, however, the same cast of characters tends to appear in both lists. Notable in the new list are three corporations (AT&T, IBM, and DuPont) and three government-sponsored laboratories (Lawrence Berkeley, Argonne, and Sandia). These rank side by side with the top ranked U.S. universities.

Among non-U.S. institutions, Tel Aviv University, the Max Planck's Fritz Haber Institute in Berlin, and the Weizmann Institute rank first, second and third. Israel's strong showing is apparent in the national rankings for chemistry. 1988-92, where it ranks second only to the United States in terms of citation impact (see *Science*, 260[5115]:1738, 18 June 1993). The United Kingdom's top ranked university in chemistry turned out to be Cambridge, followed by Southampton, Bristol, Oxford, and Sussex. With the exception of Bristol, all scored the top rating of "5" in Britain's most recent assessment exercise; Bristol received a "4." Three of Germany's six institutions listed in the chart are affiliated with the Max Planck organization. Switzerland is represented by the ETH and the Universities of Basel, Lausanne, and Zurich. Philips Labs and the State University of Groningen show the colors for The Netherlands, while the CEN and the University of Strasbourg 1 stand tall for France. Australia, Canada, Italy and Japan each fielded one.

The new study surveyed a total of 393,898 chemistry papers published during 1988-92, which were cited a total of 1,007,624 times by the end of 1992, for a world citations-per-paper average of 2,56.

Science Watch 4(6):1-2, June 1993



The statistician drowning into water of average depth six inches

**Nobody before the Pythagoreans had thought that mathematical relations held the secret of the universe.
Twenty-five centuries later, Europe is still blessed and cursed with their heritage.
To non-European civilizations, the idea that numbers are the key to both wisdom and power, seems never to have occurred.**
(Arthur Koestler)

Chemistry panel weighs misconduct cases

An Ohio State University chemistry department committee, asked to assess the gravity of two findings of plagiarism by chemistry professor Leo A. Paquette, has conducted that the case involving a National Institutes of Health grant application was serious, but that one involving a National Science Foundation proposal was not. Asked to consider whether Paquette should be allowed to continue supervising graduate students and postdoctoral associates, the committee has recommended that Paquette reduce the size of his research group.

Paquette and his lawyers contend the report by the chemistry department's graduate studies committee exonerates him of the charge in the NSF case and have asked university officials to clear him of an earlier, investigatory, committee's finding of scientific misconduct. Paquette and his attorneys reject the recommendation that this research group be limited, saying it based only on the NIH incident, for which Paquette has accepted responsibility and has already been sanctioned. Further punishment would be "triple jeopardy," Paquette argues.

The Ohio State administration disagrees with Paquette's lawyers' characterization of the chemistry committee's conclusions. "We don't feel it's appropriate to bargain with Dr. Paquette over whether he committed plagiarism," says Steven J. McDonald, associate legal counsel in Ohio State's Office of Legal Affairs. The university has not acted on the chemistry committee's recommendations, preferring to wait until NSF's Office of Inspector General finishes its ongoing review of the case before proceeding further.

In 1992, an Ohio State investigation concluded that Paquette had plagiarized material from an unfunded research proposal, which Paquette had reviewed for NIH, by chemistry professor Stephen F. Martin of the University of Texas, Austin. Paquette did not contest the finding of scientific misconduct, but said his inclusion of Martin's words and ideas in his own grant application was inadvertent. The university formally reprimanded him and the Department of Health & Human Services' Office of Research Integrity barred him from serving on NIH review groups or advisory committees for 10 years (C&EN, July 12, 1993, page 22).

In the case still under dispute, NSF asked the university to look into an allegation of plagiarism in regard to a paper by Paquette and six coauthors [*J. Amer. Chem. Soc.*, 114, 2644 (1992)]. The controversy concerns the introductory background material in the paper — but not the experiments, discussions, or conclusions (C&EN, Aug. 16, 1993, page 5). None of Paquette's coauthors are in any way implicated.

NSF specifically pointed to a sentence in the paper that says two cyclobutadiene rings held close to each other in a cyclophane structure could be stabilized by more than 50 kcal per mole. The references cited for the 50-kcal figure do not contain that number, which is found, however, in an unsuccessful proposal to NSF that Paquette had reviewed earlier. The proposal cites several related studies using the very references that turned up later in Paquette`s *JACS* paper, even down to the same misspellings.

Paquette explained to the committee convened by Ohio State to investigate the NSF allegation that he had not learned of the number from the proposal, but from conversations with a colleague. Also, the 50-kcal figure had been made public by the proposal writer in a poster presented at a 1989 American Chemical Society meeting that Paquette had attended.

The investigatory committee found that "Paquette plagiarized portions of an NSF proposal... and did not treat the submitted material with the standards of confidentiality accepted by the scientific community." The committee did not confine its concerns to the origin of the 50-kcal number but concluded that Paquette condensed and paraphrased sections of the proposal into his paper's two introductory paragraphs. "Paquette's possible prior knowledge of the number does not alter the committee's findings that the larger body of work was taken by Professor Paquette without attribution."

Paquette has argued vigorously that the introduction of his paper resembles the NSF proposal only because they both summarize the same, limited background material. He admits that he copied citations from the proposal, but contends such references are in the public domain and are not confidential. He and his lawyers assert that if the investigatory committee had had any chemists among its members, it would have recognized that his actions were within the norms of the chemistry community.

The prominent chemists — Philip E. Eaton, professor of chemistry at the University of Chicago; George A Olah, professor of chemistry of the University Southern California; and Nobel Laureate Donald J. Cram, professor of chemistry at the University of California, Los Angeles — have submitted letters or affidavits stating they do not believe Paquette to be guilty of plagiarism in the NSF case.

One of the recommendations of the investigatory committee was that the chemistry department's graduate studies committee consider Paquette's authority to supervise research students. The chemistry committee was scheduled to meet with the university provost to discuss its report last week.

The 25-page report concludes "Professor Paquette's action in the NSF case could be considered sloppy, but do not constitute plagiarism by most definitions." The committee finds "Paquette's alleged infraction in the NSF case to be minor, involving information in the public domain. It was not evident to the [committee] that Dr. Paquette intended to deceive the reader. The [committee] cannot exclude the possibility that Dr. Paquette had actually taken known facts from the NSF proposal. However, we expect that when two scholars review the history of a shared technical specialty, there will be similarities in their presentation."

The chemistry committee writes that it believes Paquette's actions stem from attempts to "accomplish too much," noting that his research group has consisted of 30 to 40 people and that he has published 30 to 50 papers a year. It recommends he cut back his total number of graduate students and postdocs to 20.

"Our feeling was there was a serious misconduct in the NIH case", says chemistry professor Daniel L. Leussing, who chaired the committee. "We don't wish to imply we think there is plagiarism in the NSF case. Our position is that we can't determine that. Our recommendations, which we hope would be corrective, are absolutely based on the NIH case."

Paquette is pleased that the chemistry committee appears to support him in the NSF case, but is unhappy with its recommendations. He denies that his research group is too large or that he tries to do too much. He is still hoping the university will drop the NSF case, without his having to go to court. "I wish I could get someone to listen to the facts," he says. "It sure is a lonely feeling."

Pamela Zurer,
C&EN (March 21, 1994) p. 18

Nonagenarians Stay Active

Generally, when a reader sees a reference to a work published in 1926 or 1927, years in which Tadeus Reichstein published some of his earliest papers, he or she assumes that the author has long since departed this earth.

But this is a false assumption in the case of Reichstein, who at age 95 is still publishing. Reichstein, who shared the 1950 Nobel Prize in physiology or medicine for discoveries related to the hormones of the adrenal cortex, is still hard at work at the Institute of Organic Chemistry at the University of Basel, Switzerland, actively participating in international collaborations.

His 1992 paper "The phloroglucinols of *Dryopteris stenolepsis*" (C.J. Widen, P. Ayras, T. Reichstein, *Annales Botanici Fennici*, 29[1]:41-54), for example, was coauthored with Finnish researchers from the University of Helsinki and the University of Turku.

In the United States, a change in a federal law may make nonagenarian researchers like Reichstein more common on university campuses. January 1 of this year marked the end of the exemption for university faculty to the Age Discrimination in Employment Act, which in essence will prohibit mandatory retirement for professors.

Now that U.S. faculty members don't have to retire at age 70 — the mandatory age at many schools before the law went into effect — they can continue to work indefinitely. And, indeed, scientists who are remaining vital and active into their 90s can already be found on American soil. It's hardly surprising: scientists, after all, possess a boundless curiosity. Their quest for new knowledge is timeless.

New Providence, N.J.-based R.R. Bowker Co.'s directory *American Men and Women of Science* lists more than 350 scientists who are 90 years old or older and living in North America. Many are no longer actively involved in their profession. Many are in poor health. But a select few continue to defy time.

While most interviewed for this article acknowledge that they don't quite get around the way they used to — they rarely attend conferences or gives lectures, for example — they all agree that they continue to work because they love what they do.

"I'm a scientist, and I keep working because I enjoy it," says ornithologist and evolutionary biologist Ernst Mayr, the Alexander Agazziz Professor of Zoology, Emeritus, at Harvard University's Museum of Comparative Zoology, who will turn 90 in July. "I'm interested in science, in finding new things out, and in communicating with the public. Why shouldn't I continue to work?"

Making Room for Others

Their continued high productivity notwithstanding, many of these mandatory retirement age. They contend that scientists can continue their work in other venues, and that retiring is often necessary to allow younger researchers and scholars the opportunity to make their own marks.

Take Mayr, for example. His official university retirement in 1975 "has been a godsend" to him, he says. He's busier than ever lecturing, consulting, and writing. In fact, says Mayr — who received the National Medal on Science in 1969 and is a member of the National Academy of Science (NAS) — since age 64 he's published nine books, and he has two more that will go to the publisher this year. He still writes some five to 10 scientific papers a year.

This past January, Mayr spent the month as a visiting scientist at the Archbold Biological Station in Lake Placid, Fla.

"I have a very active mind," says Mayr. "Any person worth his salt should know what to do when retires; if he doesn't, he probably wasn't any good in the job to begin with. People should retire so young people can have opportunities."

Mayr has written some 17 books. His latest, *One Long Argument* (Harvard University Press, 1991), focuses on the philosophical foundations of Charles Darwin's theories of evolution.

A Household Name

Few scientists' careers rival that of 93-year old Linus Pauling, winner of two Nobel Prizes (for chemistry in 1954 and for peace in 1962). His 1970 book, *Vitamin C and the Common Cold* (New York, W.H. Freeman & Co.), helped make him a household name. When he was 73, he founded the Linus Pauling Institute of Science and Medicine in Palo Alto, Calif., where he has continued studying vitamins and disease. He maintains a rigorous schedule, despite having been diagnosed with prostate cancer two years ago.

(Continued on next page)

"I don't lecture much anymore, but I continue to write and collaborate with colleagues at the institute on their research on vitamins," says Pauling, speaking from his ranch at Big Sur, some 200 miles from the Palo Alto institute, where he spends most of his time. He likes to devote two or three weeks of "high energy" at a time to a project.

Last year, he published the second edition of his book *Cancer and Vitamin C* (Philadelphia, Camino Books), and he's currently collaborating on a second edition of his 1986 book, *How to Live Longer and Feel Better* (New York, W.H. Freeman & Co.).

"Age 70 may be too early for people to retire, with people living longer and healthier," Pauling says. "The previous policy seemed a good one. But a professor who is retired at one university is occasionally offered a job by another university... the rule never prevented another university from hiring you."

"Professors who were handicapped by age could retire; others, if they were healthy and wanted to continue to work, could [do so]," Pauling says. He retired from Stanford University when he was 73 rather than further abuse the university's retirement policy, he says. "There was no pressure on the administrators that way."

Why does Pauling continue to work? He has few financial worries; he's paid a salary from the institute under a lifetime appointment. He also receives a pension from his days on university faculties at the California Institute of Technology, Stanford, and the University of California, San Diego.

Pauling, an NAS member, contends there's little else left for him to do, save visiting with family. "I've always been interested in learning new things, and have always liked research," he says. "I've always taken pleasure in discovering something new and thinking of something that no one else had."

Today, he keeps up with major scientific journals; in February, he published a "Technical Comment" in *Science* ("Triethylsilyl cations," *Science*, 263:983). He continues to be interested in the fields of atomic structure and nuclear physics.

Life After Research

John T. Edsall, 91, a professor, Emeritus, of biochemistry at Harvard University, still walks to work daily in Cambridge, Mass., weather permitting. But when the biochemist-physician officially retired in 1973 from teaching at Harvard, he decided not to continue his bench research. "I had seen other people who continued their research into their old age, but it inevitably was less significant than their youth," says Edsall.

Edsall's retirement from the classroom hardly seems to have slowed him down. He has turned to the history of science, an old interest of his. Edsall also had written extensively on the history of the study of blood and hemoglobin. His latest publication came out this past December: a review of a bibliography of Nobel laureate Hans Krebs for *Nature* (366:417-8).

Like Pauling, Edsall has been deeply involved in social issues. In 1973, the American Association for the Advancement of Science asked him to help develop its proposed Committee on Scientific Freedom and Responsibility, which he later chaired. Edsall, an NAS member, also serves on the academy's committee that addresses human rights violations.

Edsall's expertise is in the chemistry and structure of blood and muscle proteins. During the 1940s, he and his colleagues helped develop new uses of blood plasma proteins and blood fractionation processes in medicine and surgery.

Edsall says he's "somewhat concerned" about the abolition of mandatory faculty retirements. "Older faculty may want to hang on," he worries. "There may be too many holding and not enough opportunities for young people."

Historical Insight

Mathematician Dirk J. Struik, who will turn 100 in September, has little doubt he could still teach a college class in calculus. However, he no longer does mathematical research. Though early in his career he focused on tensor analysis and differential geometry, in the 1960s he turned to the history and sociology of mathematics and science. "Creative mathematics is for the young," he says, "but historical insight can last till the end."

Struik, an emeritus professor of mathematics at the Massachusetts Institute of Technology, taught at MIT from 1926 to his retirement in 1960, save the five-year period during the McCarthy era when he was accused of subversive activities and suspended from the faculty.

Struik continues to work every day, "perhaps three hours, answering calls from colleagues and students, writing letters or articles and book reviews, as well as some biographical notes" for a planned autobiography. He remains an associate to the history of science department at Harvard.

"Tons of people can do good work after age 70 or 75," says Struik. "I didn't necessarily want to retire when I did, but I had to. Of course, it goes other way, too. There are those who should retire at age 50."

A Life's Work

"We spend a lifetime in a career doing something [we] like, and you don't just give that up," says Ernest P. Hilgard, a professor, emeritus, of psychology and education at Stanford who will turn 90 in July. Hilgard, who retired from teaching in 1969, continued an active research program from another decade. Today he attends a conference every so often and lectures on occasion. His most recent books include a history of psychology in the United States, which was published in 1987 (*Psychology in America: A Historical Survey*, San Diego, Harcourt Brace Jovanovich Inc.).

Hilgard, a member of both NAS and the American Philosophical Society, sees nothing wrong with mandatory retirement. "If you want to continue to work, the university will almost always let you," he says. "I liked the option of a formal retirement. That way the administration doesn't have to make the decision."

Pioneering plant physiologist Paul J. Kramer, James B. Duke Professor of Botany, Emeritus at Duke University, who will celebrate his 90th birthday in May, believes that the ending of mandatory retirement will "create an embarrassing situation" for university administrations.

"It was easy for andministrations: once you turned 70, they could say it's time to retire," he says. "Now they will have to give a great deal of thought to this."

Kramer, an NAS member and one of the founders and past presidents of the American Institute of Biological Sciences, currently is collaborating on a fourth edition of his 1949 textbook *Water Relations of Plants* (New York, McGraw-Hill Inc.).

An Individual Decision

Meanwhile, in Canada, Gerhard Herzberg believes that retirement should be left to the individual, although, he says, "it's perfectly acceptable to have an age of retirement — say 65, or 70." Herzberg is Distinguished Research Scientist at the Herzberg Institute of Astrophysics in Ottawa, which is a member institute of the National Research Council of Canada. He won the Nobel Prize in chemistry in 1971 for his pioneering work in spectral analysis and molecular structure.

Herzberg plans to retire when he turns 90, on Christmas Day. Although there is no longer a mandatory retirement age for Canadian government workers, there was a mandatory age of 65 at the time of Herzberg's 65th birthday. (In Ontario, university faculty currently must retire at age 65, though other provinces may differ.) The mandatory age notwithstanding, Herzberg has kept on working past his 65th year. He was still discovering new molecules when he was 75.

"There should be allowances in special circumstances for some people to continue working," says Herzberg. In his own case, the Nobel Prize was just the ticket.

Benowitz, S.,
The Scientist, (April 18, 1994) pp. 11, 21, 22.

Citation Analysis Reveals Organic Chemistry's Most Active Research

In its July-August 1993 issue, the newsletter *Science Watch*, published by the Institute of Scientific Information (ISI) in Philadelphia, reported on its most recent examination of publishing productivity in the field of organic chemistry. Using information from ISI's Science Indicators database, *Science Watch* listed the most-referenced papers in organic chemistry — a subdiscipline of chemistry that employs a substantial number of research chemists — for the years 1988 to 1991.

Following is *Science Watch's* report, written for the newsletter by John Ensley, who is a science writer in residence at the department of chemistry, Imperial College, London. The article is reprinted here with permission of *Science Watch* and ISI.

In alternating issues, *Science Watch* reviews the top 10 most-cited papers in chemistry. For more than two years the lists have been dominated by fullerene papers, sometimes exclusively so, with only one occasional paper on organic chemistry making the grade. This is rather odd, because most chemists who are engaged in chemical research are using organic chemistry. Rather than wait for the flood tide of fullerene citations to ebb, *Science Watch* decided to look beneath the waves and see what pearls of organic chemistry are lying unnoticed.

We have combed the ISI lists of highly cited papers for the years 1988 through 1991, specifically seeking those that cover organic topics. The three top-cited papers for each year are listed in the accompanying table.

In drawing up the list, I considered only primary research papers and not reviews, which by their very nature collect many citations. Having carried out the citations exercise in organic chemistry, we then needed to check if the topics being cited most really were the hot areas of organic chemistry.

I consulted one of Britain's leading organic chemists, Steve Ley of the University of Cambridge, and asked what he regarded as the active areas of organic chemistry at present. His reply was immediate and reassuring: asymmetric synthesis methods, catalytic antibodies, enediynes, taxol, and immunosuppressants, such as rapamycin. The subjects in the table cover three of these current hot topics.

Jumping JACS

The papers featured in the list are from well-known organic chemists: K.B. Sharpless, C.-H. Wong, D.A. Evans, E.J. Corey, D.P. Curran, E. Negishi, P.B. Dervan, R. Noyori, and S.L. Schreiber. Their publications are, for the most part, short communications in the *Journal of the American Chemical Society*, which attests to the continuing dominance of this primary journal.

Asymmetric synthesis accounts for more than a half of the subjects in the table, and drug-related research most of the remainder. Two names appear twice: K. Barry Sharpless, formerly of the Massachusetts Institute of Technology and now of the Scripps Research Institute in La Jolla, Calif., and David A. Evans of Harvard University, the former in the top three for 1988 and 1989, the latter in 1988 and 1991.

Not all the articles are short communications, witness the 1988 paper on asymmetric synthesis by Evans and colleagues. This runs to 19 pages, of which 10 are devoted to experimental details. His 1991 paper, on the other hand, is less than two pages long.

The first of the Evans papers deals with a variant of the Diels-Alder reaction, which was discovered more than 60 years ago as a useful method of synthesis starting with a diene and ending with a cyclic compound. Evans reports that unsaturated *N*-oxazolidinones make very versatile Diels-Alder reagents. (The oxazolidine ring is five-membered, with a nitrogen and oxygen atom separated by carbon.) These chiral compounds, with various substituents attached to the nitrogen, have several advantages: They are easy to make and are often crystalline; they are highly reactive; and, most important of all, they are diastereoselective, in many cases giving yields of more than 99 percent of the endo isomer. Clearly the significance of Evans's discovery was not lost on others in the field, hence the frequency of citations.

Most cited papers in organic chemistry, 1988-91		
Rank	**1988**	**Total Citations**
1	**E.N. Jacobsen, I. Markó, W.S. Mungall, G. Schröder, K.B. Sharpless**, "Asymmetric dihydroxidation via ligand-accelerated catalysis," *Journal of the American Chemical Society*, 110:1968-70, 1988.	154
2	**Y.-F. Wang, J.J. Lalonde, M. Momongan, D.E. Bergbreiter, C.-H. Wong**, "Lipase-catalyzed irreversible transesterifications using enol esters as acylating reagents: Preparative enantio- and regioselective syntheses of alcohols, glycerol derivatives, sugars, and organometallics," *J. Amer. Chem Soc.*, 110:7200-5, 1988.	131
3	**D.A. Evans, K.T. Chapman, J. Bisaha**, "Asymmetric Diels-Alder cycloaddition reactions with chiral α, β- unsaturated N-acyloxazolidinones," *J. Amer. Chem. Soc.*, 110:1238-56, 1988.	113
	1989	
1	**E.J. Corey, R. Imwinkelried, S. Pikul, Y.B. Xiang**, "Practical enantioselective Diels-Alder and aldol reactions using a new chiral controller system," *J. Amer. Chem. Soc.*, 111:5493-5, 1989.	118
2	**J.S.M. Wai, I. Markó, J.S. Svendsen, M.G. Finn, E.N. Jacobsen, K.B. Sharpless**, "A mechanistic insight leads to a greatly improved osmium-catalyzed asymmetric dihydroxidation process", *J. Amer. Chem. Soc.*, *J. Amer. Chem. Soc.*, 111:1123-5, 1989.	89
3	**D.P. Curran, C.-T. Chang**, "Atom transfer cyclization reactions of α-iodo esters, ketones, and malonates: Examples of selective 5-Exo, 6-Endo, 6-Exo and 7- Endo ring closures," *Journal of Organic Chemistry*, 54:3140-57, 1989.	88
	1990	
1	**E. Negishi, S.J. Holmes, J.M. Tour, J.A. Miller, F.E. Cederbaum, D.R. Swanson, T. Takahashi**, "Metal promoted cyclization, 19. Novel bicyclization of enynes and diynes promoted by zirconocene derivatives and conversion of zirconabicycles into bicyclic enones via carbonylation," *J. Amer. Chem. Soc.*, 111:3336-46, 1989.*	72
2	**M. Konishi, H. Ohkuma, T. Tsuno, T. Oki, G.D. VanDuyne, J. Clardy**, "Crystal and molecular structure of dyemicine-A: a novel 1,5-Dyin-3-ene antitumor antibiotic," *J. Amer. Chem. Soc.*, 112:3715-6, 1990.	53
3	**D.A. Horne, P.B. Dervan**, "Recognition of mixed-sequence duplex DNA by alternative-strand triple-helix formation," *J. Amer. Chem. Soc.*, 112:2435-7, 1990.	53
	1991	
1	**D.A. Evans, K.A. Woerpel, M.M. Hinman, M.M. Faul**, "Bis(oxazolines) as chiral ligands in metal-catalyzed asymmetric reactions: Catalytic asymmetric cyclopropanation of olefins", *J. Amer. Chem. Soc.*, 113:726-8, 1991	51
2	**R. Noyori, M. Kitamura**, "Enantioselective addition of organometallic reagents to carbonyl compounds: Chirality transfer, multiplication, and amplification," *Angewandte Chemie-International Edition in English*, 30:46-9, 1991	50
3	**H. Fretz, M.W. Albers, A. Galat, R.F. Standaert, W.S. Lane, S.J. Burakoff, B.E. Bierer, S.L. Schreiber**, "Rapamycin and FK506 binding proteins (immunophillins)", *J. Amer. Chem. Soc.*, 113:1409-11, 1991.	43

* Article apperaed late in 1989 and was not cited until 1990.

Source: ISI's Science Citation Indicators Database, 1988-92.

His 1991 paper is also devoted to asymmetric synthesis using bis(oxazolines) in which two of these five-membered rings are directly linked, or joined through an intervening carbon. These compounds are used in the form of Cu(I) complexes to catalyse the conversion of styrene to a mixture of *cis* and *trans* cyclopropane molecules, again with a marked preference for one of the forms.

And what of the future? What topics might we see heading a list of the most-cited papers in organic chemistry in four years' time?

For a glimpse into the crystal ball I turned to 41-year-old Tony Barrett, holder of the newly created Glaxo Chair of Chemistry at Imperial College, London. The three topics he thought might be found on such a list were catalytic asymmetric synthesis; non-linear optical materials; and "smart" polymers. Would-be chemistry stars, please note.

The Scientist, (March 7,1994) 15

Der Forschungs Index

A kutatási index
A német kutatás vezető intézetei

A fullerének

A befolyásosak

	Intézmény	Idézetek száma (1990-1993 október)	Publikációk száma (1990-1993 június)
1	Heidelbergi Max Planck Magfizikai Kutatóintézet	2148	30
2	Freiburgi Egyetem	172	18
3	Stuttgarti Max Planck Szilárdtestfizikai Kutatóintézet	123	27
4	Berlini Műszaki Egyetem	112	13
5	Karlsruhei Magkutató Központ, KfK	73	15
6	Jülichi Kutatóközpont, KfA	39	4
7	Töbingeni Egyetem	31	5
8	Berlini Hahn Meitner Intézet	24	7
9	Freie Universität, Berlin	22	10
10	Mainzi Max Planck Műanyagkutató Intézet	20	4

Az aktívak

	Intézmény	Publikációk száma (1990-1993 június)
1	Heidelbergi Max Planck Magfizikai Kut. Int.	30
2	Stuttgarti Max Planck Szilárdtestfizikai Kut. Int.	27
3	Freiburgi Egyetem	18
4	Karlsruhei Magkutató Központ, KfK	15
5	Berlini Műszaki Egyetem	13
6	Freie Universität, Berlin	10
7	Berlini Hahn Meitner Intézet	7
8	Wuppertali Egyetem	6
9	Tübingeni Egyetem	5
	Frankfurti Egyetem	5
	Karlsruhei Egyetem	5

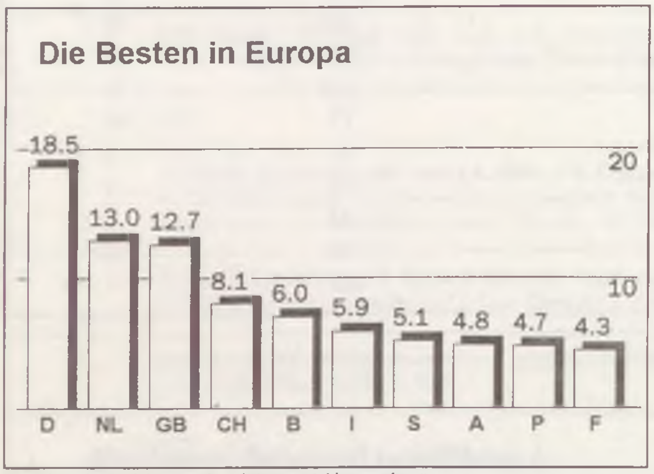
A heidelbergi buckyball zsonglőrök állnak az élen

A fullerének, a molekuláris szén "futball-labdák" (buckyballs) három éve a kémia legintenzívebben kutatott objektumai. A heidelbergi Max Planck Intézet fizikusainak, Wolfgang Krätschmer és Konstantinos Fostiropoulos munkái, melyekben 1990-ben ezeknek a molekuláknak az előállítását közzétették, 1993 októberéig már 1250 idézetet kaptak. Innen ered egyedülálló helyük a rangsorban. A fullerén kutatásban különösen tevékenyek még a stuttgarti Max Planck Szilárdtestfizikai Kutatóintézet fizikusai is.

A német buckyball zsonglőrökön kívül Európában különösen a britek közölnek a szén "futballokról" újabb ismereteket (lásd a grafikont). Ha a közlemények nemzetközi jelentőségét (az egy közleményre eső idézetek számával mérve) vizsgáljuk, akkor a németek után elsősorban a hollandok és a britek tűnnek fel, őket a svájciak és a belgák követik. Ez azt mutatja, hogy a kisebb pénzügy forrásokkal rendelkező kisebb nemzetek is hatékonyan tudnak a legaktuálisabb kutatásokban résztvenni.

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

A hatékonyak			
	Intézmény	Egy publikációra eső idézetek száma	Publikációk száma
1	Heidelbergi Max Planck Magfizikai Kutatóintézet	71,6	30
2	Jülichi Kutatóközpont, KfA	9,8	4
3	Freiburgi Egyetem	9,6	18
4	Berlini Műszaki Egyetem	8,6	13
5	Tübingeni Egyetem	6,2	5
6	Mainzi Max Planck Műanyagkutató Intézet	5,0	4
7	Karlsruhei Magkutató Központ, KfK	4.9	15
8	Stuttgarti Max Planck Szilárdtestfizikai Kutatóintézet	4,6	27
9	Konstanzi Egyetem	3,8	4
10	Berlini Hahn Meitner Intézet	3,4	7



Európa legjobbjainak "átlagos idézettsége"

Nemzetközileg a németországi fullerén-kutatások a legelismertebbek, átlagosan minden közleményt 18,5-szer idéznek a többi kutatók. Meglepetést jelent a franciák tizedik helye.

"A kutatási teljesítményt nem lehet rőffel mérni"

bdw: Dr. Weidemann, a médiumok közölték, hogy az új szövetségi tartományokban a kutatóintézetek teljesítőképességének átvizsgálása után most nyugaton a "kék jegyzék" egyetemen kívüli intézményei kerülnek sorra.

Weidemann: Az átvizsgálások nem tekinthetők a keleti értékelésre adott reakciónak. Az ottani intézményeket elsősorban olyan szabályok szerint vizsgáltuk, melyek itt már a nyolcvanas évek eleje óta érvényben vannak — és a Tudományos Tanács döntése alapján továbbra is érvényesek maradnak.

bdw: Milyen módszerekkel kísérlik meg egy intézet teljesítőképességének felmérését?

Weidemann: Erre mindig egy bel- és külföldi szakértőkből álló testület jogosult, ők a tudományos eredményeket vizsgálják. A jelentés a Szövetség-tartomány Bizottság elé kerül, mely a Kék jegyzék intézeteiért felelős.

bdw: Voltaképen miért éppen "Kék jegyzékről" van szó?

Weidemann: A név annak a papirosnak a színétől ered, melyre 1977-ben alapításukat rögzítették. Ezek, ellentétben a Max Planck intézetekkel, önálló irányítású kutatóintézetek. A főiskolákon kívül ezek foglalkoznak általános jelentőségű kutatásokkal.

bdw: Milyen következményekkel járhat a vizsgáló jelentése egy ilyen intézmény számára?

Weidemann: Voltak olyan esetek, amikor a Szövetség vagy a tartományok a pénzügyi támogatást megszüntették vagy csökkentették. Legtöbbször azonban megkísérlik a hiányzó teljesítményt az intézet átszervezésével és új kutatási súlypontok megállapításával elősegíteni.

bdw: Milyen szerepet játszik itt a közlemények és a más kutatóktól kapott idézetek összeszámlálása?

Weidemann: Ezt természetesen figyelembe veszik, de itt nemcsak a mennyiségről van szó. A tudományos teljesítményt nem lehet rőffel mérni. Így például egy valamilyen házi kiadványban közzétett nagyszámú közlemény nem egyenértékű az olyan munkákkal, melyeket nemzetközi szakfolyóiratokban közöltek. Egy intézet munkáját az is minősíti, ha külső munkatársak jelentkeznek egy közlemény társszerzőiként, mivel úgy vélik, hogy így kivánnak maguknak elismerést szerezni.

(Dr. Konrad Weidemann a Mainzi Központi Múzeum vezérigazgatója és a "Kék jegyzék munkabizottság" vezetőségi szóvivője.)

Bild der Wissenschaft, 6 (1993) 6-7

Készült az Argumentum Könyv- és Folyóiratkiadó Kft. nyomdájában

Felelős kiadó: az MTAK főigazgatója