4. évfolyam · 2. szám · 1994. február

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

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

Megjelenik havonta
Évi előfizetési díj: 2400 Ft

Luc Montagnier on Gallo and the AIDS Virus: 'We Both Contributed'

Editor's Note: *"Science is the dominant metaphor of the twentieth century," says author Thomas A. Bass in the introduction to his new book,* Reinventing the Future: Conversations with the World's Leading Scientists (New York, Addison-Wesley Publishing Co., 1994). *"Science is the knowledge in which we place our faith, the solution to our problems, the way out, the way up."*

Bass's admittedly worshipful respect for science, along with his quest to understand it more fully, has prompted him over the past several years to conduct personal interviews with men and women who, given their research achievements, have played major roles in shaping the international science community of today. His book presents 11 of these interviews, touching on subjects as diverse as molecular biology, genetics, chaos theory, and drug research.

Of all Bass's subjects, none, perhaps, has achieved more renown than Luc Montagnier, the French biochemist who laid claim in 1983 to discovering the AIDS virus at his Institut Pasteur laboratory in Paris — a claim that was also subsequently made by United States researcher Robert Gallo, head of the National Cancer Institute's laboratory of tumor cell biology in Bethesda, Md. Acrimonious debate over who discovered the virus and who, as a result, deserves to receive royalties on the AIDS blood test has raged for the better part of the past decade.

According to Bass, the American press has branded Montagnier as "patrician and aloof." What he discovered in his interview with the biochemist, however, was a candid, friendly researcher, willing to give his supposed rival Gallo abundant praise as a scientist, while at the same time determined to retain for himself the distinction of having isolated the AIDS virus.

Following is an excerpt from Bass's interview with Montagnier.

Q The American press describes you as proud and ambitious to the point of arrogance. Are you?

A It depends on the day. When you're climbing a mountain, the last thing you want to do is look behind you and say, "Oh my, it's too high, what am I doing up here?" Even if I keep my eyes fixed on the summit, I realize I'm a long way from the top — in fact, there is no summit! In science there are always new problems. If it weren't AIDS, it would be something else. I'm a gambler out for the big killing. Like a roulette player at the table, I'm addicted to getting results out of my laboratory.

Q You've said many times, "I have lots of enemies."

A I do! In France we're very egalitarian, so if you get out ahead of the pack, they shoot at you. I'm a target. This comes not only from my scientific success, but also from my success in the media, which is something new for a scientist in France. From the start, AIDS has been a show-business disease. The press and media have been fascinated by it. People are making major discoveries in other domains, but they receive none of the attention accorded to AIDS, while I'm being barraged with invitations to appear on TV around the world.

Luc Montagnier: *"I don't want to stir up the past."*

Q To set the record straight, did you discover the AIDS virus?

A There's no debate about this point. The argument with Robert Gallo had to do with proving causality. Did the virus I discovered cause the disease? I don't think Gallo disputes that we were the first to isolate the virus and publish our findings in May 1983. All he has ever claimed is that he isolated the virus at roughly the same time. He wasn't able, however, to characterize it.

Q What was your reaction when Gallo announced that he had discovered the virus?

A I remember quite well the day he came to my office in April 1984. He ... told us he had discovered the virus that causes AIDS, which he was calling HTLV-3. It was obvious his virus was close, if not identical, to ours. My reaction was altogether positive. He was confirming our work.

Q Even though he was claiming all the credit for himself?

A We both contributed to the discovery of the virus. The difference between science and religion is that in science everyone has to agree. For a fact to be a fact, it has to be reproducible. Miracles, by definition, are not reproducible. So if we were capable of isolating the virus that causes AIDS, it's not surprising that others could do it, as well.

Q What was Gallo's contribution?

A He found a way to grow the virus in continuous cell cultures. We developed a similar technique at the same time, but our cell lines were less productive than his. Later we found one equally as good, but in the beginning his line was better. This was important for developing the AIDS blood test. We also owe to him the idea that AIDS was caused by a retrovirus.

Q Some people say that Gallo owes his discovery to samples of virus you sent him in July and September of 1983.

A I don't want to stir up the past. All the details are given in the chronology we published together in *Nature* [R.C. Gallo, L. Montagnier, 326:435-6, 1987]. It says I sent him the virus. These shipments must have been useful to Gallo, and I don't think he denies it.

Q Is it possible that Gallo's cell lines might have became contaminated with your virus, which would explain why he reproduced it so faithfully?

A These accusations were made by the Institut Pasteur. And Gallo himself did not exclude this possibility.

Q Because of his ability to mass-produce the virus, Robert Gallo has been called the Henry Ford of AIDS research.

A Gallo is not someone who has merely perfected other people's discoveries. Many important findings have come from his laboratory, things like interleukin-2, the growth factor that allowed us to isolate the AIDS virus. He generates a lot of creativity. He's not merely a Henry Ford, a biological mechanic. Gallo and I have worked together in the past, and we'll probably do so again. The unhappy period that he and I lived through was distorted way out of proportion by the press and by the politics of the disease.

Q What was your reaction to the political pressures surrounding AIDS research in the United States?

A I was particularly furious that our patent for the blood test was ignored until Gallo's was accepted. That's what pushed me into starting legal proceedings.

Scientists in the United States are forced to produce results, which sometimes warps their sense of ethics.

Q Were you surprised by the nature of American science?

A No, I really don't object to the aggressivity of the Americans. I object to the passivity of the French, who met my work with incomprehension and indifference. Thanks to this research, France could be making breakthroughs in biotechnology, but it's letting the opportunity slip through its fingers.

Q Were you pleased with the legal agreement you and Gallo signed in 1987?

A Yes, I thought from the start there had to be a compromise. No one should be made to look as if he were losing face. The only solution was to split the royalty money 50-50 and establish a foundation for spending it. I was probably happier about the settlement than Gallo, because it was my idea.

The affair caused a lot of ill will, and AIDS is too important for the problem to have remained unsolved. It was giving certain scientists — and science itself — a bad name. Not to have fought would have created a bad precedent. It would have signaled that one can get away with anything in science, which isn't true.

Q *Are you under a gag order that prevents you from talking about the details of the accord?*

A It's not exactly a gag order, although it's stated in the agreement that no one will reopen the scientific argument. There were actually two agreements: a legal accord between the American government and the Institut Pasteur, and a scientific accord between Gallo and me, which was published in Nature.

Now Gallo and I are getting along quite well. We respect each other .... I bear no grudge against him. My rancor is reserved for the people who are still trying to get in the way of my research. I have a reputation for being an imperialist, an expansionist, because I ask for a lot of money. But this is what it takes to do research on AIDS. AIDS is not an affair that's going to last 50 years. It's going to be settled in 10 years, and if you want to put the package together, you can't drag your feet.

Q *Do you deserve a Nobel Prize for discovering the AIDS virus?*

A It's not for me to say. The Nobel committee might want to give the prize to the discoverer of the vaccine, although it was the discovery of the virus itself that allowed for its detection in blood and the development of public health measures that can limit the epidemic, even without a vaccine. The contribution of the American team is also important, so I doubt the prize will go to only one of the virus' co-discoverers. If someone develops a miracle drug against AIDS, that, too, would merit a Nobel Prize....

AIDS is a terrible malady, and I don't want to suggest that scientists are reaping their honors at other people's expense. I haven't changed because of my notoriety, but there's tremendous pressure from the media and the public, who think of us as a cross between magicians and movie stars.

The Scientist, (December 13, 1993) 11

Perchance to scrutinise the field.

Stephen Donovan regrets the demise of book reviews in learned journals

Nowadays, many academics in Britain seem to take an attitude to their work more appropriate to supermarket checkouts than universities: every move they make must ring the till. This insight comes largely from listening to people explaining how they are doing their job. For example, a young friend recently told me that many British academics have stopped writing book reviews, despite rather enjoying the process. The fault lies with their heads of department who view it as a waste of time compared with the main job of producing peer-reviewed papers for flag-waving purposes during the next assessment exercise. The cash register wins again and I can't say I'm pleased.

Academics have never been the kind of hacks who will review one-hundred-plus books a year, like George Orwell's caricature in his essay "Confessions of a Book Reviewer". One or two reviews a year at most, is probably the norm. But even this number is now being regarded as excessive. Reviews of scientific books are worth writing, but the review sections of many scientific journals are being left to die.

Writing research papers shouldn't replace writing book reviews; the two deserve separate places in the workload. The stalwart British biologist Thomas Huxley considered that the secret of being a research scientist was to retain one's ability to work continuously for 16 hours a day. Few of us would go to such lengths, but after a day spent teaching, marking essays and exams, administrating and, in those odd, quiet moments, actually researching, when is it possible to fit in time for reviewing a book? My solution is to read at every chance I get: at home in the evening instead of going to the pub, or while travelling in buses, trains and planes, or when I'm allowed to sit awhile somewhere undisturbed.

Reviewing books is a service to the scientific community. Nobody can read every book published in their own field, however narrow, so book reviews act as a sampler of what is available, providing critical synopses of new publications. They also indicate what is or isn't worth buying. Both functions are important, considering the current cost of most specialist books.

The advantage to the reviewer is that he/she is encouraged to read a book. Most of my reading is limited to short research papers. Books, I generally dip into and I only read the chapters of particular interest to me. Writing a review forces on me a different kind of discipline. It encourages me to read a book from cover to cover. In many ways, it is an important part of my education. Apart from the obvious absorption of scientific information, it is instructive for any author to see how books are structured and produced. I am also alerted to gaps in my knowledge by references to recent (and not so recent) papers that I may have missed.

Reviewing books also provides an opportunity to comment on current trends and ideas. Released from the constraints of the research paper, a book review allows the writer to remark informally on a broad or narrow subject area. Indeed, book reviews are a part of the literature of science. I think Stephen Gould's collection of his own reviews, *An Urchin in the Storm* is one of his best books. While we cannot all write to the same high standard as Gould, not writing reviews will hardly help us to write them better. Practice makes pretty good, even if not perfect.

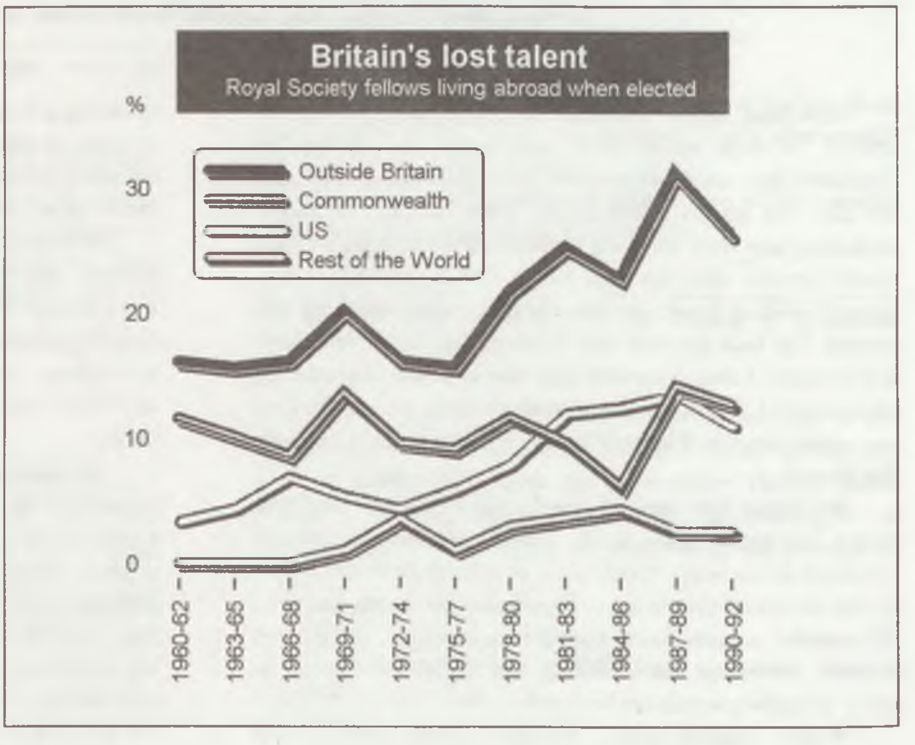
In recent years, some journals have dropped their review sections altogether. (Not, of course, this august organ.) Most researchers can probably point to examples in their speciality or discipline. Of the various fatalities among review sections in my own field of palaeontology, none is more sadly missed than the one in *Paleobiology*. It used to publish essay reviews as long as research papers, which served to introduce the general reader to current areas of scientific debate. Apart from their importance to the researcher, these reviews were invaluable introductions for undergraduates and postgraduate students. While some journals still publish such extended pieces (*Geological Magazine* is a notable example), they are an endangered species.

Why have the book review sections suffered so? The modern reliance on refereed publication as a metric for promotion has displaced more peripheral activities from the agenda of many academic scientists, including such things as book reviewing, editing journals and serving on the committees of scientific societies. This attitude is reflected in the journals' policy of dropping book review sections to make way for more refereed papers. When I edited our local geological journal I reintroduced the book review section, but unfortunately mine was only one vote against many. Although some scientific societies publish book reviews in their newsletters, these publications lack permanence and are unlikely to be preserved on library shelves.

Finally, few academics seem to consider the fun to be had in reviewing books. They miss the chance to play mental tag with the book's author, to enter into written discussion and, perhaps, debate, and to give praise where praise is due. I get the chance to comment on most books only in the classroom, whereas a review will reach a broader and more eclectic audience. As the track shoe advert says, just do it.

Stephen Donovan,
New Scientist, (20 November 1993) 55

High-fliers still leaving Britain



The good news is that the brain drain of researchers emigrating from Britain has slowed, and that the number leaving is roughly matched by the number arriving. The bad news is that many of those who leave are high-fliers, and their departure is depriving Britain of its top talent.

Roughly equal numbers of researchers left the country and arrived between 1984 and 1992, according to a survey carried out by the Science and Engineering Policy Studies Unit, a think-tank run by the Royal Society and the Royal Academy of Engineering. Mike Ringe, the author of the study, sent questionnaires to 325 leaders of research groups and 218 heads of departments at British universities.

The 371 who replied knew of 447 British scientists and engineers who emigrated between 1984 and 1992, compared with 462 who took up posts in Britain. Of these, only 144 were returning Britons.

A survey in 1987 showed a similar rate of emigration. "If you look at the head count, things don't seem to have changed since our last survey," says Ringe. "But there's evidence that there's an imbalance between the quality of people leaving Britain and those arriving."

Answers to the questionnaire consistently expressed concern that it was the best staff who left, "taking their expertise, enthusiasm and central focusing ability with them, to the detriment of the immediate department and Britain as a whole". As further evidence, the report points out that in 1992 a quarter of the British-born Fellows of the Royal Society lived abroad. In 1960 the figure was just 13 per cent.

A growing proportion of British scientists elected to the Royal Society were working abroad at the time of their election. This, says Ringe, is when they are generally reckoned to be at the peak of their careers.

The survey shows that most Britons who left did so improve their career prospects. The next most compelling reason for leaving was to gain access to better-equipped laboratories abroad. Low salaries and the low status of science in Britain were also frequently cited as reasons for moving.

People who came back to Britain usually returned for personal reasons or a desire to "return to UK culture". No one returned because they expected higher salaries, and only one anticipated a higher standard of living.

Jordan Raff, until recently the chairman of British Scientists Abroad, a lobby group of two thousand expatriate scientists campaigning for better working conditions in Britain, says the drain of top staff was particularly damaging to Britain. "When you lose someone who's good, you don't just lose them alone," says Raff, who works at the Department of Biochemistry and Biophysics in the University of California, at San Francisco. "You also lose their dynamism, their enthusiasm and possibly the promising postdocs they work with."

William Waldegrawe, the science minister, takes heart from the study's finding that 54 per cent of the immigrant scientists and engineers were considered by the respondents to be "outstanding", compared with just 50 per cent of the emigrants. "The study shows that the UK is benefiting from a two-way flow of high-quality scientists and engineers," he says.

But according to John Mulvey, the secretary of the Save British Science Society, "the thing that causes greatest concern is that we are losing leaders and potential leaders... it would be extremely unwise for anyone to be complacent about things simply because of the numbers."

A. Coghlan, New Scientist, 7 (20 November 1993)

UK brain-drain "no worse"

Britain's brain-drain is not improving. But neither has it been getting much worse, according to a report published in London last week which will provide ammunition both for the government and its critics in the debate over the number of British scientists leaving to work overseas.

The report has already been welcomed by William Waldegrave, the minister for science, who has homed in on its conclusions that there has been a slight fall in the number of scientists emigrating, and a small increase in the number coming to Britain from overseas. In each case, the number remains small compared to the total number of scientists in the United Kingdom.

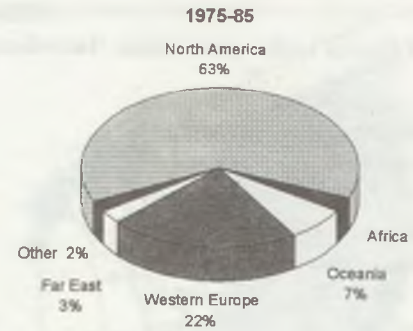
But those responsible for the report have issued a warning about an excessively complacent interpretation of its conclusion. They point out that it still tends to be the highest quality staff who take posts overseas, confirmed by the growing number of fellows of the society with foreign addresses.

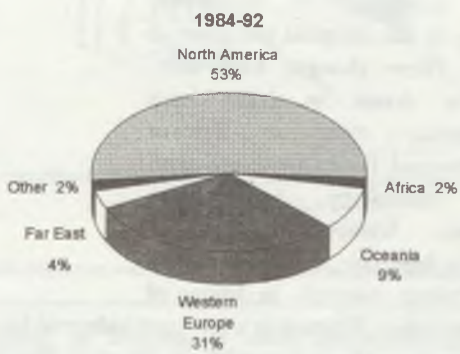
The report has been produced by the Science and Engineering Policy Studies Unit (SEPSU) of the Royal Society. It is based on replies to a questionnaire sent to university science departments, asking for details of individual scientists known to have left the country between 1984 and 1992. Five subjects were covered: biochemistry, chemistry, Earth sciences, electrical engineering and physics.

The report follows up an earlier version, published in 1987 and covering the period 1975-85, which was the first significant attempt to quantify a phenomenon whose evidence tends to be known more through personal anecdote than hard data.

Like the earlier report, the new one points out that, at least in terms of overall figures, the net loss of scientists to Britain — in particular, the tendency of postgraduates to seek research posts overseas — is virtually balanced by the number coming to work in the country from abroad.

Destination of scientists emigrating from the U.K.





The numbers involved have changed little. The new report, prepared with support from the Nuffield Foundation, found, for example, that the number of recently qualified postgraduates leaving to work overseas had risen slightly, from

13.4 to 13.5 per cent. The number of more senior staff leaving in the second period was 2.1 per cent, and heads of department 0.3 to 0.5 per cent.

The biggest change was in chemistry, where the number of departing PhDs rose from 12.0 to 16.2 per cent; in contrast, their peers in Earth sciences fell from 23.6 to 12.7 per cent, largely reflecting declining demand in the oil industry. In biochemistry, the emigration rate for new PhDs remained at about 19.0 per cent.

Nor has there been a significant change in the motivations to leave Britain - or for returning to it. Most scientists left for professional reasons, the most widely quoted ground being enhanced career prospects and "a desire to widen experience"; 73 per cent of those coming back quoted unspecified personal reasons as a motivation, and 64 per cent a desire to return to British culture.

One novel finding of the report is the different statistics between men and women. The study found that, when compared to the total numbers of each category, women were about half as likely to emigrate as men — but equally likely to come to Britain.

The biggest change since the mid-1980s has been the destination of those leaving Britain. The proportion taking up research posts elsewhere in Europe has risen from 22 to 31 per cent. In contrast, there has been an almost identical drop, from 63 to 53 per cent, in the proportion of emigrant scientists taking up positions in the United States.

The government has, perhaps inevitably, taken comfort from the fact that the brain-drain does not seem to be getting any worse. Indeed, the report points out explicitly that those entering Britain's science and engineering base "seem to have been no more likely to emigrate during the past decade than they were during the previous decade."

But the report itself warns of the dangers of complacency. It points out for example that those leaving Britain tend to take up long-term positions abroad, returning only for personal reasons. In contrast, those arriving take short-term posts.

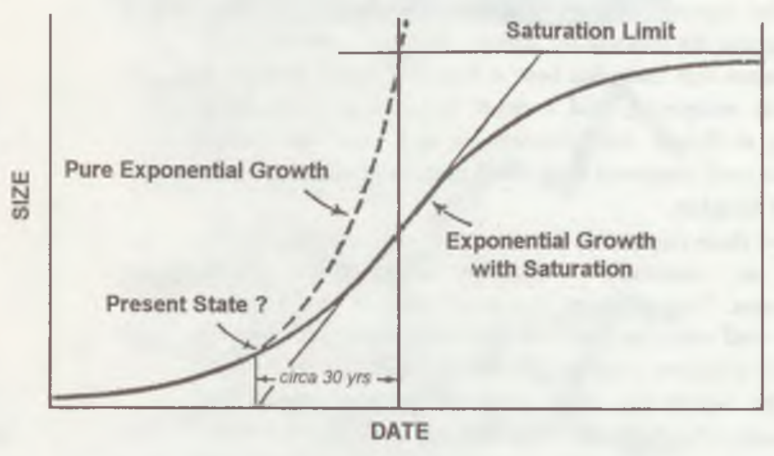
"We should be careful not to draw too much comfort from the simple numerical head count" says Ian Nussey, chairman of SEPSU's management board, pointing to the growing number of Royal Society fellows with addresses outside Britain. Almost two-third of those replying to the survey felt that emigration was having an adverse effect of British science, and half of these felt that the effect was "serious".

D. Dickson, Nature, 366 (18 November 1993) 197

Interdisciplinary Big Science

Editorial to the special issue: "Interdisciplinary High Technology at Karlsruhe Nuclear Research Centre"

The world of science has changed during the past few decades. The end of the Cold War confrontation, the economic crisis in the Western world and increasing concern for the environment have been the factors responsible for these profound changes. They have affected each working scientist, positively or negatively, depending on his discipline and place of research. These changes have been particularly drastic in big science establishments, such as defence laboratories and those concerned with nuclear technology and space exploration. Science faculties in universities have also suffered and have had to reduce research in favour of teaching activities. Whereas in a few great industrial laboratories some basic research was possible in the past, this has now almost ceased with the absolute priority of immediate profitability.



General form of logistic curve from exponential growth to saturation [2]

Big science has always had to be interdisciplinary. Exploration, particularly Arctic and Antarctic, radioastronomy, the creation of the atomic bomb and the Apollo spacecraft, particle accelerators and environmental research have always received, and will continue to demand, interdisciplinary teamwork from engineers and scientists of many different disciplines. In this special issue of ISR we document an outstanding example of how the recent changes in a big science establishment, the Karlsruhe Nuclear Research Centre (KfK), have brought about diversification from its original single objective of nuclear technology to many different aspects of high technology. Because interdisciplinary research had become an essential tool in the Karlsruhe centre, it is now able to use this tool to great benefit in its new spheres of endeavour.

It is exactly 30 years ago that Derek de Solla Price [1] published his profound book "Little science, big science" in which he discussed the fundamental change from small scale scientific research by the single research worker to the large team efforts which had become so common in many disciplines during the 30 year period between the 1930s and 1960s. Now, a further generation on, I am sure Price (a founder member of the Editorial Board of this journal) would have added a sequel to his book, for which he would have found the present issue of ISR excellent source material.

By using the scientific method of measurement, Price drew attention to the ever increasing growth rate of the number of scientists, scientific journals and abstracts and thus demonstrated its adherence to the general form of the logistic curve which leads from exponential growth to saturation (see figure). He illustrated this concept in his book with numerous examples, including a curve of the number of known chemical elements as a function of date. In fact, he defined his concept of "big science" as a discipline which doubled in 15 years. What Price called big science, or *Grossforschung*, would today be referred as "high technology". There are many reasons, political, social, economic and even military developments, which have led to saturation long before Price saw the end of exponential growth of science.

Big science is of course nothing new, and one might well speculate whether perhaps Stonehenge was not the first example of a major technological effort to demonstrate or to obtain scientific knowledge. Other historical examples spring to mind, such as the great astronomical observatories of Tycho Brahe (1546-1601), the first state observatory of modern times in Paris during the seventeenth century, and that of Jay Singh II (1699-1743) in Delhi, the Jantar Mantar. Many great voyages of exploration had a scientific purpose, apart from purely political and economic aims, such as the expedition organised by Henry the Navigator (1415-1460) and particularly the voyages of Captain James Cook (1729-1799). But, apart from the Paris Observatory, all other early examples of big science were the achievements of a single man and were abandoned after his death. Again, the end of the exponential growth of modern science has led to the decline of big science projects in recent decades.

The greatest reduction in big science has occurred in the fields of nuclear and space high technologies. A detailed analysis of their contraction is beyond these few editorial remarks; suffice it to say that the advocates of these two big sciences failed to convince politicians and the public to spend, during an economic recession, the very large sums of money needed for their continued growth. Furthermore, two accidents, one of the space shuttle "Challenger" and the other in Chernobyl, analysed by Dr. Hans Mark and Professor Larry Carver [3], illustrated "the deep irrational response to nuclear power" and how "these fears need to be understood and ultimately... defused". Another somewhat different example of contraction occurred in the big science of supercomputers. Here, progress has been towards smaller and smaller electronic components which, apart from special calculations, allow small desktop computers to perform functions which in the past needed large calculators. One might almost speak of an exponential growth towards minuteness.

As atomic physics and space research have lost their prime position in the scientific aristocracy, new interdisciplinary research fields have come to the forefront. Astronomy has retained its high rank and has extended its vision from optical wavelengths to the whole of the spectrum. Radioastronomy in particular, with its planet spanning baselines, is big science and is now courageous enough even to attempt a Search for Extra-Terrestrial Intelligence (SETI). Great voyages of discovery on Earth are no longer needed, but Antarctic research has taken their place; this is big, interdisciplinary and totally international in all respects. The largest new big sciences are in the medical and biomedical fields, following the precedent set by the physical sciences. The Human Genome Project, the search for more knowledge about cancer, to which this journal devoted a special issue [4], and most recently AIDS research all demonstrate that biological big sciences have staked their claim in the past few decades.

Michaelis, A.R.,
Interdisciplinary Science Reviews, 18 (3) 177-179 (1993)

1. **D.J. de S. Price**, "Little science, big Science"; 1963, New York, Columbia University Press
2. **D.J. de S. Price**, "Science since Babylon", 1961, New Haven, CT, Yale University Press
3. **H. Mark, L. Carver**, "Challenger and Chernobyl, lessons and reflection", *Interdisc. Sci. Rev.*, 12 (3) 241 (1987)
4. **J.H. Nuckolls**, "The state of the laboratory", *Energy Technol. Rev.*, 1 (Jan. Feb. 1993)

"What's most depressing is the realization that everything we believe will be disproved in a few years."

Science, at bottom, is really anti-intellectual. It always distrusts pure reason and demands the production of the objective fact.

(H. L. Mencken)

Science is built up with facts, as a house is with stones. But a collection of facts is no more science than a heap of stones is a house.

(J. H. Poincaré)

Lean, Mean Gene Machines

When the 17th International Congress of Genetics convenes this August in Birmingham, U.K., the participants will have plenty to ponder as they consider the official theme "genetics and understanding of life." To help them organize their thoughts, *Science Watch* decided to rank the highest-impact performers in molecular biology and genetics, based on papers published and cited between 1988 and 1992. The top institutions and individuals are listed in accompanying tables.

In this new survey, *Science Watch* defined the field of molecular biology and genetics as those papers appearing in 190 dedicated journals of molecular biology and genetics, as well as select papers published in the multidisciplinary journals *Science*, *Nature*, and *PNAS*. A previous ranking of institutions in molecular biology and genetics for the years 1981-91 did not include these three high-impact, multidisciplinary journals, nor did it include as many journals (see *Science Watch*, 3(4):7, May 1992). In all, the current study took into account 163,775 papers of all types and the 1,131,016 citations those papers collected through 1992. The mean citations-per-paper score, or world average for U.S. papers was 10.53.

The Salk Institute, Cold Spring Harbor Lab, and the Whitehead Institute, which top the chart, make for something of a Big Three. All are elite independent research institutes. At fourth and fifth are the only industrial firms in the top 25, both biotechnology companies: Genentech and Chiron (the latter including the papers of Cetus).

High Impact Institutions in Molecular Biology and Genetics, 1988-92
(among those publishing > 200 papers)

Rank	Institution	Papers	Citations	Citations Per Paper
1	Salk Institute	403	16,752	41.57
2	Cold Spring Harbor Lab	359	14,641	40.78
3	Whitehead Institute	392	15,543	39.65
4	Genentech	225	7,452	33.12
5	Chiron	200	6,566	32.83
6	Inst. Chimie Biologique, Strasbourg	261	8,315	31.86
7	Fred Hutchinson Cancer Ctr.	413	11,177	27.06
8	MIT	1,060	27,296	25.75
9	Princeton University	369	8,841	23.96
10	MRC Lab Molecular Biology, Cambridge	430	10,193	23.70
11	Childrens Hospital, Boston	433	9,691	22.38
12	Rockefeller University	702	15,285	21.77
13	Harvard University	3,020	62,430	20.67
14	UC San Diego	979	19,923	20.35
15	European Molecular Biology Lab	652	12,998	19.94
16	NICHD	238	4,686	19.69
17	UC San Francisco	1,621	30,570	18.86
18	Natl. Inst. Med. Res., London	344	6,411	18.64
19	NCI	1,787	33,165	18.56
20	Hosp. Sick Children, Toronto	330	6,084	18.44
21	Scripps Clinic & Research Fdn.	526	9,603	18.26
22	Massachusetts General Hospital	649	11,762	18.12
23	Caltech	426	7,708	18.09
24	UC Berkeley	1,369	24,282	17.74
25	Imperial Cancer Research Fund	970	16,892	17.41

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

U.S. institutions take 19 of 25 places in the table. Not a surprise, for two reasons. For one, many of the strongest research centers in molecular biology and genetics worldwide are located in the United States. Second, the population of U.S. researchers active in this area is quite large and is strongly represented in the ISI database; as a consequence, and because U.S. researchers may look at papers published in U.S. journals more than they look at those in non-U.S. journals, U.S. papers get a leg up in terms of citation accumulation. All the more reason, then, to take note of the non-U.S. representatives.

Perched at sixth on the chart is the Institut de Chimie Biologique, in Strasbourg, France. The institution, with Pierre Chambon its most decorated investigator (see table, #13), is affiliated with the University of Strasbourg 1 and receives major research support from both INSERM and CNRS. Chambon and his team fielded the most-cited paper of 1992 (see *Science Watch*, 3 [10]:1-2, 8 December 1992), which dealt with the retinoic X receptor. The other non-U.S. institutions listed are the MRC Laboratory of Molecular Biology, in Cambridge (#10); the European Molecular Biology Lab, in Heidelberg (#15), the National Institute for Medical Research, in London (#18);

Toronto's Hospital for Sick Children (#20), and, as a group, the U.K. laboratories of the Imperial Cancer Research Fund (#25).

Not listed, but worth special mention, is the Howard Hughes Medical Institute and its laboratories worldwide. The Hughes Institute supports its researchers at their respective universities and hospitals. Sometimes the Hughes affiliation is presented in the author's address, but sometimes it is not. A complete picture of this organization was, therefore, impossible to obtain. *Science Watch* did identify, however, 2,514 papers that explicitly listed the HHMI affiliation. These papers were cited 71,251 times, for a citation-per-paper average of 28.34, which would have placed the Hughes Institute at #7 in the ranking.

Other institutes deserving special mention are the Carnegie Institution's Department of Embryology in Baltimore, Md. (citation per paper score of 35.79); the Roche Institute, in Nutley, N.J. (34.53); and, the La Jolla, Calif. (32.86). These three research institutes published fewer than 200 papers in molecular biology and genetics during 1988-92, so they were not ranked.

The table to the right lists the 25 most-cited researchers who published 20 or more papers and ranks them by citations per paper. It is noteworthy that 9 of these 25 are Hughes investigators.

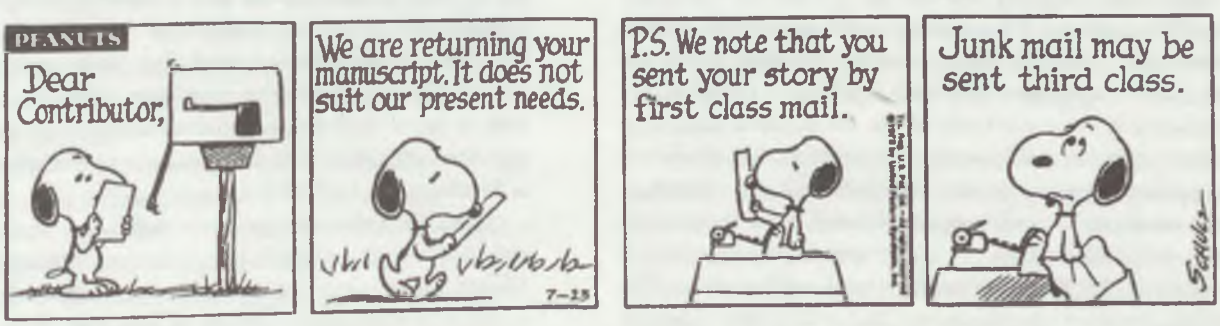
Science Watch, 4(7):1-2 (July/August 1993)

High Impact Researchers in Molecular Biology and Genetics, 1988-92

(25 most-cited scientists, ranked by average cites per paper)

Rank	Name/Institution	Papers	Citations	Citations/Papers
1	S.L. McKnight*[1]/Carnegie Inst. Washington	20	3,006	150.30
2	R.M. Evans*/Salk Institute	32	3,822	119.44
3	B.R. Franza/Cold Spring Harbor Lab	21	2,455	116.90
4	T. Curran/Roche Inst. Mooecular Biology	32	3,626	113.31
5	R. Tijan*/UC Berkeley	52	5,344	102.77
6	E. Harlow/Massachusetts Gen. Hosp.	27	2,394	88.67
7	T. Hunter/Salk Institute	50	4,383	87.66
8	H. Weintraub*/Fred Hutchinson Cancer Ctr.	42	3,487	83.02
9	D. Baltimore/Rockefeller University	87	6,977	80.20
10	M. Karin/UC San Diego	44	3,502	79.59
11	D. Beach*/Cold Spring Harbor Lab	40	3,055	76.38
12	M.G. Rosenfeld*/UC San Diego	38	2,604	68.53
13	P. Chambon/Inst. Chimie Biologique	66	4,402	66.70
14	B. Vogelstein/Johns Hopkins University	43	2,829	65.79
15	P. Nurse/University of Oxford	49	3,178	64.86
16	P.A. Sharp/MIT	64	3,735	58.36
17	L.-C. Tsui*/Hosp. Sick Children, Toronto	54	3,094	57.30
18	I.M. Verma/Salk Institute	46	2,613	56.80
19	M.R. Green/University of Massachusetts	48	2,685	55.94
20	R.D. Klausner/NICHD	43	2,201	51.19
21	R.G. Roeder/Rockefeller University	60	2,951	49.18
22	A. Ullrich/M. Planck Inst. Biochemistry	65	3,161	48.63
23	J. Schlessinger/NYU Medical Ctr.	72	3,354	46.58
24	F.S. Collins*[2]/University of Michigan	70	3,254	46.49
25	R.L. White*/University of Utah	113	3,495	30.93

*HHMI Investigator [1]now at Tularik Inc. [2]now at NIH, NCHGR;
Source: ISI's Science Indicators Database, 1988-92



Is the literature about to be readable?

Dissatisfaction with the standard of today's scientific literature is rife. It does not follow that change will happen, but only that it might.

If the scientific literature is indeed the chief way of setting and maintaining professional standards in science, there is at least a chance that beneficial changes are on the way. The article by J.D. Watson and F.H.C. Crick on the structure of DNA, which appeared in *Nature* in 1953, would probably not now be publishable. That article occupied just over half a printed page. While there are references to other work on the storage of genetic information, the suggestion that it must be DNA emerges like a rabbit from a hat. There is no substantial discussion of the implications of the proposal, merely the now-famous sentence: "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material" (*Nature* 171, 737-738; 1953). Today's referees would have clamoured for a fuller account of the reasons for considering the proposed structure seriously and an account of the implications.

The reasons why the conventions of the scientific literature have so radically changed are well-known, as are the consequences. Most scientific communications have the same ingredients. Authors describe their problem and discuss its antecedents, give the rationale for their own attack on the problem and the techniques which they have used and finally discuss the significance of the results. There are, of course, many minor variations on the theme; experimental details may be printed separately from the rest of the text, or appear as figure legends. But the general architecture of a paper is constant.

Or, at least, it has been. There are now signs of discontent with the literature as it is. The sheer bulk of it is not what matters most; it matters more that the literature may not be suited to its purpose. The prospect for change may now be brighter than for decades.

While electronics will have an important influence on the speed and efficiency of publication, electronics and computer technology are not the central issues. For while there are certain to be some fully electronic journals which can be read only by sitting at a keyboard, most of the literature is likely to remain for a long time as ink on paper. People like to see what they have done; an accession number from a data bank is not a sufficient substitute.

Meanwhile, it is likely that the changes (if any) that come about will consists of a reordering of the priority at present attached to the conflicting functions of the literature, which are several. Thus the published literature is a kind of record of the accumulation of discovery from which, for example, historians may reconstruct the development of innovation. The literature is also a means by which those who would make use of science can gain access to original research, although most technology transfer of this kind entails the use of specialized magazines or trade journals. But there are strictly peripheral functions. The published literature is the means by which the research profession's scepticism is brought to bear on new developments, helping to tell the wheat from the chaff; the claim is undoubtedly correct, but would be more easily accepted if the process were more explicit.

None of this explains why the format of the standard scientific article is what has now become conventional. Historians, for example, would prefer detail of a different kind, those concerned with the application of science would prefer that the literature should be more intelligible, and so on. But the more substantial reason why the literature is in its present form is that the research profession, like other scholarly professions, but more so, has come to rely on the published literature for the continued setting and maintenance of professional standards. Bibliographies are a prominent part of job and promotion applications, while a published "track record" helps in telling which applications for research grants will succeed.

Researcher's assessments of each others' qualities rest, in the last resort, on the published record. Naturally, they are other ways in which the same job might be done; engineers, physicians and the like have professional associations for setting and maintaining standards, for example. In science, where surprising innovation from unexpected sources is welcomed, the use of the published literature for this purpose makes more sense, but also gives it its distinctive characteristics and makes it hard to read.

This is why referees so often give authors a hard time and usually do so gladly, even though theirs is the most thankless of all tasks. In a profession which is necessarily elitist, paying close attention to the quality of what others do is painly a professional task transcending particular questions of which articles should be published in which journals. This is the spirit in which referees demand that references to earlier work should be fair (whatever that may mean) and that authors should be restrained in canvassing the implications of their work: unbridled speculation is a means of laying claim to all kinds of ideas not directly suggested by the data gathered.

Sadly, these are precisely the points on which the contradictions of the literature are most claimant. Speculation of the kind that referees dislike may nevertheless help enormously to define an author's long-term objectives or to explain why he set about his task in a particular way. Detailed accounts of experiments, necessary so that others can set about the repetition of published work (but rarely quite sufficient), are not often the details that most users of the literature would wish to have. And the literature would be written differently, and more palatably, if its function in setting standards was not as dominant as it has become.

The signs that changes may under way are, admittedly, only straws in the wind. Some appointments committees in the United States have begun limiting the allowable size of a candidate's bibliography. There is also a growing uneasiness that the literature may sometimes be manipulated, especially in relation to the assessment of attainment, illustrated by the several cases of fraud brought to light in the United States during the past few years.

There are also more substantial considerations working for reform, one of which is the huge amount of data now unpublished in any formal sense, in fields as different as geophysics and the projects for collecting the nucleotide sequences of genes. Can the long-term interests of the process of discovery be properly served if the bulk of this data remains unanalysed and unpublished because of the convention that the Minimum Publishable Unit should include at least an attempt to make sense of some problem in science. The fact that, in other fields, preprints have replaced the published literature as means of communications in another sign of the pressure on the present system.

Luckily, these are all fields in which electronics may provide both solutions and new opportunities. Whether the literature will ever fully revert to its literary purposes is, of course, another matter.

Maddox, J., Nature, 335:665 (20 October 1988)

Beszámoló egy tudománykörüli találkozóról

1993. október 7-10 között az American Association for the Advancement of Science (AAAS) szervezésében Pultuskban (Lengyelország) a Dom Polonii konferenciaközpontban került sor az "Evaluating Science and Scientists" című rendezvényre. A 20 országból érkezett 52 résztvevő közül 34-en aktív természettudományi és társadalomtudományi kutatók, valamint tudomány-politikusok, kormányszervek és alapítványok tisztviselői, míg 18-an tudománymetriai, szociológiai és informatikai kutatók voltak.

Amint azt a work-shop megnyitójában Mark S. Frankel az AAAS Scientific Freedom, Responsibility and Low programjának igazgatója elmondta, szervezetük fontos célja, hogy elősegítse a kelet- és közép-kelet-európai országokban megindult társadalmi-gazdasági változásokat. E folyamatok kedvező irányba való vitelében, a piacgazdaság kialakításában fontos szerep jut a kutatás-fejlesztésnek és a tudománynak. A jövőben "a K+F és a termelés", "a K+F és az állam", "hogyan menedzseljük a K+F-et" és még néhány hasonló témában is terveznek rendezvényeket esetleg nem csak eszmecsere, de iskola jelleggel is.

Nyilvánvaló, hogy a két és félnapos tanácskozás nem adhatott választ az örökzöld tudománypolitikai alapkérdésekre, de sikeresen hozzájárult ahhoz, hogy a résztvevők megismerjék a K+F szervezeti és anyagi helyzetét, lehetőségeit, az alkalmazott értékelési módszereket egymás országaiban.

A rendezvényen csupán néhány hosszabb, tájékoztató jellegű előadást hallhattunk, a résztvevők döntő többsége 5-7 percig tartó rövid ismertetést tartott (egy-egy alkalommal 4-6 előadó), amelyeket azután mintegy 1,5-2 órás kemény diszkusszió követett. A résztvevők oly aktívak voltak, hogy alig lehetett szóhoz jutni. Az előadások és a diszkussziók szerkesztett anyagai rövidesen meg fognak jelenni.

Heves vitákat váltottak ki pl. a tudománypolitika következő alapkérdései:

- A GDP *hány százalékát* kell (lehet, célszerű) egy országban (a privát szférát is beleértve) K+F-re költeni?
- Mekkora legyen (lehet) az *állam részesedése* a K+F-ből?
- Legyenek-e, s ha igen, akkor hogyan lehet kijelölni a prioritásokat? (Prioritáson értve vagy egyes *tevékenységfajták* (pl. alapkutatás, alkalmazott kutatás, fejlesztés stb.) vagy *szervezetek* (kisvállalatok, nagyvállalatok, független kutatóintézetek, egyetemek stb.) vagy *tematikák*, ill. *projectek* (fizika-kémia-biológia stb., ill. biotechnológia, űrkutatás, szupravezetés, stb.) közötti ráfordítási arányokat.)
- Milyen legyen az egyetemi, a közszolgálati, ill. nemzeti kutatóközponti, továbbá az ipari (mezőgazdasági) K+F egymáshoz viszonyított aránya?
- Hogyan osszuk szét az erőforrásokat az előző kérdésekben felvetett tevékenységek, területek, szervezetek, projectek között?
- Kell-e, lehet-e a tudomány művelőit értékelni? Mit értékeljünk egyáltalán? Milyen értékelő módszereket alkalmazzunk? Mire használjuk az értékelés eredményeit?

Az egyik amerikai előadó — A. Teich — hangsúlyozta, hogy az USA-ban alkalmazott *többcentrumú* és *többcsatornás* finanszírozás — bármennyire is furcsa vagy ésszerűtlennek tűnő, mégis működik. A Science Indicators kötetekből és pl. a Nobel-díjasok számából kiviláglóan pedig eredményes is.

Jellemző, hogy a National Science Foundation és a NASA a Veterans Affairs, a Housing and Urban Development, valamint az Independent Agencies (e.g. American Battle Monument Commission) címekkel együtt szerepel egy rovatban, azaz ezekkel versenyez a támogatási összegekért. A Human Genom Project-et például, amely 2005-re az emberi örökítőanyag 3 milliárd bázispárja sorrendjének megállapítását célozza, és amelyre 1991-1995 között évente 150 Millió $-t költöttek, a NIH (National Institute of Health) és a DOE (Department of Energy) együttesen finanszírozza.

Fontos megjegyezni, hogy az USA-ban egyre nagyobb hangsúly helyeződik a *stratégiai megközelítésre* (Mission Oriented Research, Strategic Research) a pusztán kíváncsiság irányította kutatások helyett. Alapkutatásokat pl. több mint 15 szövetségi hivatal támogat, igaz, a hat legnagyobb adja az összes pénz 96%-át.

Érdekes, hogy a keményebb (számszerűsített) értékeléseket elsősorban a közép- és a kelet-európai országok gyakorlatában alkalmazzák. Mind a cseh, a szlovák, az ukrán, a belorusz, a balti köztársaságok és a lengyel kutatásértékelés gyakorlatában használják az *idézeteket* és a *publikációk számát*, mint fontos tudománymetriai adatokat. Számosan alkalmazzák a folyóiratok *impact faktorait* is. Hallhatunk olyan gyakorlati intézkedésekről, amelyek nem kaptak hazánkban eddig nagy visszhangot. Iyenek pl. azok, amelyek az ország teherbíróképességéhez (szükségleteihez) próbálták igazítani, részben egyetemeket is érintően, a (közszolgálati) kutatóintézeti hálózatok felépítését és méretét Kelet-Németországban és Csehországban. A sokakat egzisztenciájukban érintő drámai, de célszerű redukció a korábbi létszámot, anyagi forrásokat mintegy 40-70%-kal csökkentette.

A nyugat-európai és az amerikai K+F-finanszírozási rendszerek elsősorban *pályázatokon* alapulnak — természetesen részben kivéve a nagy berendezéseket működtető intézményeket. A pályázatok elbírálásában a peer-eknek van döntő szerepe, de alkalmazzák a panel-megoldásokat is.

A tudományos pályázatok értékelésének alapvető szempontjaiként általában a következő kritériumokat alkalmazzák:

- a pályázat originalitása és inventivitása,
- a pályázat témájának, ill. a várható eredményeknek a jelentősége,
- az eredmények széleskörű (tudományos vagy egyéb területen történő) alkalmazhatósága,
- a javasolt project megvalósíthatósága,
- a költségek és a várható (tudományos és ezen kívüli) haszon egybevetése,
- a pályázók korábbi eredményei.

B.R. Martin és T. Luukkonen több nyugat-európai, ill. északi országban folytatott értékelésről számolt be, amelyek egy-egy tudományterület, ill. project eredményességét voltak hivatva megvizsgálni.

Voltak előadók (E. Hacket, G. Siversten), akik rámutattak arra, hogy szükség lenne a peer-ek részére egy *etikai kódex*re, amely segítene kiszűrni az óhatatlanul jelenlévő szubjektív elemeket.

A legnagyobb vitát talán két magyar előadás váltotta ki, amelyek közül az egyik az MTA KKKI hosszú évek óta alkalmazott kutatásértékelési módszereiről, és a költségvetési támogatás ennek alapján osztályokra történő szétosztásáról tájékoztatott, míg a másik beszámoló az Akadémia kutatóintézeteinek és kutatócsoportjainak részben szakértői értékelés, részben tudománymetriai módszerek alkalmazásával történő felülvizsgálatáról szólt.

Számosan erősen vitatták, hogy a tudománymetriai mérések bármilyen, a kutatás eredményeinek *minőségére* jellemző adatot tudnának szolgáltatni. Rámutattak a veszélyre, hogy a hamis mérésekből rossz döntések születhetnek. Többen viszont pártolták az objektív mutatószámok használatát a szubjektív döntések helyett.

E beszámoló szerzőjének hozzászólása szerint a tudománymetriai elemzéseket — ill. az ezek felhasználását, konklúzióinak levonását — ellenző nézetek okai elsősorban a következők lehetnek:

- a kutatók tartanak bármiféle (főként nem tudományos részről érkező) külső beavatkozástól,
- az érdekeltek félnek az értékelés konzekvenciáitól,
- nincsenek standard, elfogadott, és ismert értékelő módszerek,
- rosszak a korábbi értékelések tapasztalatai.

A kvantitatív értékeléseknek számos előnye lehet, így pl: jól átlátható, követhető, ellenőrizhető mind az értékelők, mind az értékeltek számára,

- segíthet szakmai kapcsolatok feltárásában,
- nemzetközi összehasonlításban is alkalmazható tükröt tart az értékeltek elé,
- maguk a résztvevők is többet ismernek meg saját munkájukról.

A konferencia résztvevőinek hozzászólásaiból néhány érdekes ötletet, javaslatot említek még meg:

- Egyetlen vagy néhány bíráló helyett bizottság (panel) értékeljen.
- Az értékelésben *gyakorlatot kell szerezni* mind az értékelőknek, mind az értékelteknek.
- Lehetséges a "pilot-evaluation" alkalmazása is, azaz a késleltetett konzekvenciákkal járó bírálat. Ez azt jelenti, hogy az értékelés tanulságai alapján intézkedéseket még nem léptetnek életbe, így az értékelteknek lehetőségük van a másodszori vagy harmadszori értékelésig a változtatásra, ill. az értékelőknek az értékelő módszerek és a kapott eredmények hitelességének, igaz voltának ellenőrzésére.
- Minél "puhább" az értékelendő terület, (társadalomtudományok legtöbb ága), annál nagyobb szerep jusson a peer-reviewnak, s minél inkább számszerűsíthetőek (ésszerűen) az adatok, információk, annál nagyobb tere legyen a mutatószámoknak. A kétféle értékelést azonban mindenképpen egymás kiegészítőjeként célszerű alkalmazni.
- Külön gondot kell fordítani a különböző területeken működők eredményeinek összevetéséből adódó nehézségek megoldására.
- Az értékelést ne csak az illető szakterületet ismerők végezzék, de azok is, akik az értékelési technikák szakértői.

A konferencián az is kiderült, hogy a piacgazdaság rögös útjára tért, korábbi tervutasításos rendszerű országok egyikében sem jutott még idő a rendszerváltozás után egy nemzeti tudományos és műszaki fejlesztési koncepció kidolgozására, pedig enélkül az elviselhetőnél és célszerűnél erősebb spontán (gyakorta rossz irányú) hatások érik a K+F-et.

A találkozó résztvevői egyetértettek abban, hogy az eszmecserék hasznosak voltak, de még sok-sok konferenciát fognak rendezni addig, míg a tudományos teljesítmény értékelésének standard módszerei kialakulnak, és azokat mind a kutatók, mind a tudománypolitikusok elfogadják és alkalmazzák.

Vinkler Péter, KKKI

Készült az Argumentum Könyv- és Folyóiratkiadó Kft. nyomdájában

Felelős kiadó: az MTAK főigazgatója