3. évfolyam • 7. szám • 1993. július

I M P A K T

TÉNYEK A TUDOMÁNYOS ALAPKUTATÁSRÓL

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

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

A tartalomból:

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Ki bírálja a bírálókat?

A kutatásértékelés minőségi és mennyiségi lehetőségeiről

A címben említett kérdés ugyan általános érvényű, de azzal egy konkrét eset kapcsán foglalkozunk. Kutatócsoportunk egyik tagja egy német kollégával együtt kutatási pályázatot nyújtott be "A természet- és társadalomtudományi szakirodalom avulási és befogadási folyamatainak tudománymetriai vizsgálata" témában az MTA és a DFG által közösen finanszírozott német-magyar kutatási együttműködés keretében.

Az MTA Nemzetközi Együttműködési Iroda 1993. február 3-i levele a következőket tudatta a magyar pályázóval: "Sajnálattal értesítjük, hogy az MTA és a Deutsche Forschungsgemeinschaft közötti egyezmény keretében benyújtott fenti projektjavaslat nem nyert jóváhagyást." Jóváhagyott, ill. visszautasított kutatási pályázatok mindig voltak, vannak és lesznek, és természetesnek vehető, hogy az előbbiek szerzői elégedettek, ill. az utóbbiaké elégedetlenek lesznek a döntésekkel. Mindez közhelyként kezelhető és természetesen nem indokolná e kis esszét.

Ami ennek ellenére írásra ösztökélt, az a fent említett levélhez csatolt "opponensi vélemény", ill. az abban felsorolt visszautasítási érvek első pontja:

"A tudománymetriai módszerek – mint kvantitatív eljárások – bár igen fontos szerepet játszanak a kutatásértékelésben, fontosságukat nem szabad túlbecsülni. Magyarországon sajnos az utóbbi években a kvalitatív kutatásértékelési módszerek – bár jelen voltak – kissé háttérbe szorultak. Kutatáspolitikai szempontból az értékelés minőségi oldalára kellene jelenleg inkább koncentrálni, és az ezzel kapcsolatos kutatási témákat támogatni".

Mondandóm itt következő részét egy olyan kutató jogán ismertetem, aki bár nem részese a fentebb vázolt pályázati kérelemnek, ismerője és aktív művelője annak a tématerületnek, amelyik a pályázat tárgyát képezi.

Ennek kapcsán hadd keressem a válaszokat a következő kérdésekre:

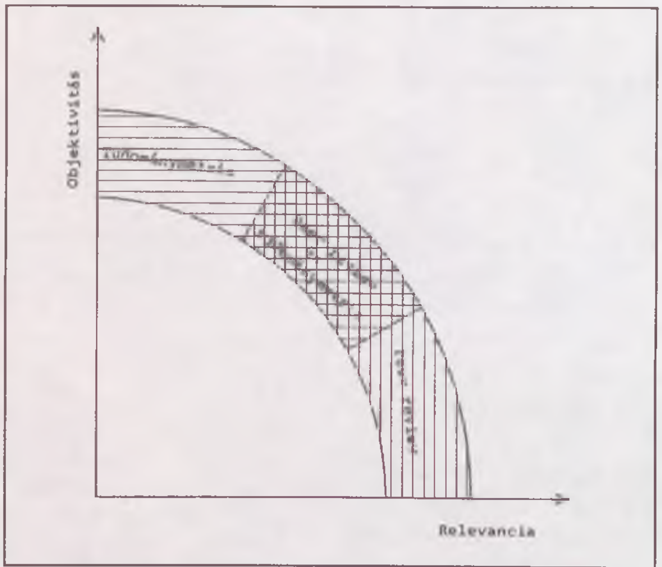
1. Mennyiben és hogyan becsülte túl a pályázó a tudománymetriai kutatásértékelési módszereket? Hacsak nem annyiban, hogy ezen módszerek alkalmazása képezte a pályázat tárgyát.

2. Mennyivel több egy bizonyítatlan állításnál az a kitétel, hogy a kvalitatív kutatásértékelési módszerek Magyarországon az utóbbi években háttérbe szorultak? Attól eltekintve, hogy mit ért a bíráló kvalitatív kutatásértékelésen (feltehetőleg az angol peer review által körülírt fogalmakat), mivel lehet hitelt érdemlően a háttérbeszorulást alátámasztani?

Enyhén szólva, furcsának tűnik a szakértői bírálatot (peer review) és a szakirodalmi tevékenység és hatás mérhető komponenseinek (pld. a publikációs tevékenységnek, ill. annak idézettségi hatásának) statisztikai elemzését egymással szembehelyezni, ill. azok vagylagos alkalmazását javasolni. A szakirodalom [1-5] ugyanis inkább igazolja azt a nézetet, amely szerint az értékeléseknél a két módszer együttes alkalmazása ugyanazon cél két oldalról való megközelítése útján látszik a legcélravezetőbbnek és legmegbízhatóbbnak.

Amennyiben pld. a kutatási pályázatok értékelésénél a relevanciát és az objektivitást tartjuk elsősorban szem előtt, és azok egymáshoz való viszonyát grafikusan ábrázoljuk, akkor a minőségi (peer review) és mennyiségi (tudománymetriai) módszerek egy körív mentén helyezhetők el a két koordinátán ábrázolt szempont között

(1. ábra). Mint látható, a peer review módszerei relevánsak, de az ismert okoknál fogva kevésbé objektívek, míg a tudománymetriai eljárások objektívebbek, de kevésbé relevansak. Ezért javallja a szakirodalom a körív közepe táján elhelyezkedő peer review és tudománymetriai eljárások együttes alkalmazását. Az ábrán latható három metodikai zóna határai – természetesen – nem merevek és a valós összetételi arányokat a vizsgált szakterület jellegzetességei, ill. a konkrét értékelés céljai, feladatai határozzák meg. Az eddigi alkalmazások főleg a természettudományi szakterületekre vonatkoztak, de a szakirodalomban szép számmal akadnak példák a társadalomtudományok terén végzett együttes kvantitatív és kvalitatív elemzésekre is.



1. ábra. Kutatásértékelési módszerek objektivitása és relevanciája

A kétoldalú MTA-DFG kutatási projekt-pályázatok elbírálásához rendszeresített értékelő lap a pályázatok négy szempont szerinti elemzését kéri a bírálóktól. Ezek a következők: tudományos érték; alkalmasság (kivitelezhetőség); az együttműködés nemzetközi jelentősége; a kutatás aktualitása. A fenti szempontok szerinti értékelés alapján nem a bíráló, hanem az MTA tudományági főosztálya kell, hogy megválaszolja az ötödik szempont szerinti kérdést, amely a pályázati téma jelentőségét, prioritását van hivatva eldönteni. Ezen irányelvektől eltérően a fent említett pályázat opponensi véleménye 1. pontjának utolsó mondata viszont így hangzik: "Kutatáspolitikai szempontból az értékelés minőségi oldalára kellene jelenleg inkább koncentrálni, és az ezzel kapcsolatos kutatási témákat támogatni". Mint olvasható, a bíráló etikailag súlyosan kifogásolhatóan a főosztályi döntést úgy próbálja befolyásolni, hogy megválaszol egy nem neki feltett kérdést. Ez a szakmai szakértői szerep jelentős félreértelmezését jelenti azáltal, hogy a pályázat konkrét szakmai, tudományos értékének elemzése helyett nem a bíráló feladatait képező általános tudománypolitikai érvekkel igyekszik a végső döntést befolyásolni. Ugyanis az a javaslat, hogy a neki szakmai bírálatra kiadott konkrét pályázat témája *helyett* milyen témákat kell(ene) a támogatónak támogatni, nem képezheti a szakmai bírálat tárgyát.

A pályázati kutatástámogatási rendszer Magyarországon még csak rövid ideje került bevezetésre, és így nem meglepő, hogy a rendszer szabatos, korrekt működtetési körülményei még nem alakulhattak ki. Még fejlettebb országokban is, ahol e rendszernek több évtizedes hagyományai vannak, gyakran merülnek fel etikai és más problémák [2].

Az egyik ilyen gyakran felvetett kérdés a bírálók és a bírálatok szakmai korrektsége és megalapozottsága. Ezt a problémát ott is, és itthon is csak egy olyan mechanizmus irányíthatja a korrekt megoldás felé, amely kialakítja és folyamatosan műveli a kutatási pályázatok értékelésével párhuzamosan a bírálók és bírálatok elemzését és bírálatát. Enélkül ugyanis fennáll a veszély, hogy a bírálatok néha személyi, csoport, politikai és egyéb torz érdekek és indulatok alap- és háttérnélküli kinyilatkozásává degradálódhatnak.

Braun Tibor, MTAK, Magyar Tudomány, 1993 július

[1] Y. Elkana et al., J. Ledenberg, R.K. Merton, A. Thakray, H. Zuckerman (Eds): Toward a Metric of Science: The Advent of Science Indicators. Wiley, NY, 1974.
[2] D.V. Cicchetti: The Reliability of Peer Review for Manuscript and Grant Submissions: A Cross-Disciplinary Investigation. *Behav. & Brain Sci.*, 14 (1991) 119-186.
[3] H.F. Moed et al.: A Comperative Study of Bibliometric Past Performance Analysis and Peer Judgement. *Scientometrics*, 8 (1985) 149-159.
[4] G.M. Carter: Peer Review, Citation, and Biomedical Research Policy: NIH Grants to Medical School Faculty, Report R-1583-HEW, Dec. 1974.
[5] M.C. La Follette: On Fairness of Peer Review. *Science, Technology and Human Values*, Fall, 1983, p. 3-5.

Not in our Nature

Several *Nature* issues have contained advertisements mentioning some of the important scientific discoveries originally published in *Nature*, among them Darwin's theory of natural selection, the first rocket, Watson and Crick's DNA double helix, the production of monoclonal antibodies and male development of chromosomally female mice.

I understand that science is a commercial activity and that scientific journals, keystones in the edifice of science, must compete to attract readers. But scientific journals, like scientific researchers, can make mistakes, and it is the sum of all these successes and failures that determines the general course of the advance of science. As well as publicizing the most important scientific discoveries published in *Nature*, you should also admit to the errors you have made in rejecting important papers that went on to influence and shape their disciplines. In some cases their authors received the Nobel prize.

(1) In 1981, *Nature* rejected a paper by the British biochemist Robert H. Michell on signalling reaction by hormones. This paper has since been cited more than 1,800 times.

(2) In June 1937, *Nature* rejected Hans Krebs's letter describing the critic acid cycle. Krebs won the 1953 Nobel prize in physiology or medicine for this discovery.

(3) *Nature* initially rejected a paper on work for which Harmut Michel won the 1988 Nobel prize for chemistry; it has been identified by the Institute for Scientific Information as a core document and widely cited.

(4) A paper by Michael J. Berridge, rejected in 1983 by *Nature*, ranks at number 275 in a list of the most-cited papers of all time. It has been cited more than 1,900 times.

J.M. Campanario, Nature 362 (February 1993) 488

On the shoulders of giants

John Gribbin investigates an epic scientific row

Isaac Newton's note-books show that even an undergraduate in the 1660s he kept abreast of new ideas, including those of Galileo Galilei and the French philosopher René Descartes . These marked the beginning of the new view of the Universe as an indicate machine, an idea that had yet to officially penetrate the great universities of Europe.

Newton, who obtained his bachelor's degree from the University of Cambridge in 1665, had been a satisfactory but not brilliant student. That year, the plague broke out in London and the university was closed. Newton went home to Lincolnshire, where he stayed for the best part of two years, until the normal academic life of Cambridge resumed.

It was during those two years that Newton derived the inverse square law of gravity. Tradition has it that his conception of it was sparked by the fall of an apple. In doing this, he conceived a new mathematical technique, differential calculus, which made the calculation more straightforward. And, as if this were not enough, he also began his investigation of the nature of light, discovering and naming the spectrum, the rainbow pattern of colours produced when white light passes through a prism. None of this made any impact on the scientific world at the time, because it seems that Newton did not tell anybody what he was up to. When the university reopened in 1667, he was elected to a fellowship at Trinity College, and by 1669 he had developed some of his mathematical ideas to the point where they were circulated to the cognoscenti.

At the age of 26 Newton became Lucasian Professor of Mathematics at Cambridge – a secure position for life if he so wished it, with no tutoring responsibilities other than to give one course of lectures each year. (The present Lucasian professor is Stephan Hawking.) He used these lectures to develop his ideas on light into the form which later became the first part of his epic treatise, *Opticks*. But this major scientific work was not published until 1704, as a result of one of the most protracted personality clashes of Newton's tempestuous career: he was unable, and never learned, to cope with criticism of any kind, and he did not suffer fools gladly.

The problems began when Newton started to communicate his new ideas through the Royal Society, an organisation which had been founded only in 1660, but which was already established as the leading channel of scientific communications in Britain. The row, with polymath Robert Hooke, also inspired Newton's famous remark – which recent research now suggests has been misinterpreted for 300 years.

The Royal Society first learned of Newton as a result of his interest in light – not his new theory of how colours are formed, but his practical skill in inventing the first telescope to use a mirror for focusing light. When the learned "fellows" of the society saw the telescope, in 1671, they liked it to so much that they elected him to their fellowship in 1672. So delighted was Newton with this recognition that he presented a paper on light and colours to the society.

Robert Hooke was the first "curator of experiments" at the Royal Society. Today he is best remembered as having formulated Hooke's law of elasticity. He seems to have regarded himself as the society's (if not the world's) expert on optics, and he responded to Newton's paper with a critique couched in such condescending terms that it would surely have annoyed any young researcher. Newton was driven to rage by the comments. Within a year of gaining his fellowship of the society he had retreated back into safety of his Cambridge base where he kept his thoughts to himself and avoided the usual scientific to-ing and fro-ing of the time.

But early in 1675, during a visit to London, Newton claimed he heard Hooke saying that he now accepted Newton's theory of colours. Newton was sufficiently encouraged by this to offer the society another paper on light, which included a description of the way coloured rings of light (now known as Newton's rings) are produced when a lens is separated from a flat sheet of glass by a thin film of air. When Newton submitted a second paper on the nature of light in the same year, Hooke immediately complained, both privately and publicly, that most of the ideas were not original at all, but had simply been stolen from his own work. In the ensuing correspondence with the secretary of the society, Newton denied this, and made the counterclaim that in any case, Hooke's work was essentially derived from that of Descartes.

Seemingly under pressure from the society, Hooke wrote a letter to Newton couched in terms which could be charitably interpreted as conciliatory but in which he still managed to repeat all his allegations and to imply that, at best, Newton had merely tidied some loose ends. It was this letter that provoked Newton's famous remark to the effect that if he had seen further than other men, it was because he stood on the shoulders of giants.

Traditionally, this remark has been interpreted as indicating Newton's modesty, and his recognition that earlier scientists such as Johannes Kepler, Galileo and Descartes had laid the foundations for his laws of motion and his great work on gravity – which is odd, because in 1675 Newton had not made public his ideas about gravity and motion. The charge of modesty does not, in any case, seem one which would stick to such a prickly character as Newton.

John Faulkner, a British researcher now based at the Lick Observatory in California, has come up with a persuasive new interpretation of Newton's remark, based on a study of the documents related to the feud. Far for being modest, Newton was being arrogant, says Faulkner; and Newton was not referring to Kepler and Galileo. Rather he was referring to his own work on light.

In Newton's days references to the giants of the past were generally used to express indebtedness to the ancients, especially the Greeks. Seventeenth century scientists in general seem to have thought that they were doing no more than rediscovering laws known in much more detail to the ancients. Bearing in mind their previous disagreements and that Hooke had a distinctly unprepossessing appearance, Faulkner claims that Newton was particularly careful in his choice of words to Hooke in the letter dated 5 February 1675.

Quoting from 17th-century contemporaries of Newton and Hooke, including Hooke's friends, Faulkner creates a picture of Hooke resembling nothing so much as William Shakespeare's caricature of Richard III – distinctly twisted and dwarfish. Even taking some aspects of this portrait with a pinch of salt, there is no doubt that Hooke was a little man.

In this context, says Faulkner, the sentence in Newton's letter leading up to the remark about giants set the remark itself in a quite different context. Remember that this was, after all, not a hurried not despatched to a friend, but a letter written at the behest of the Royal Society in order to publicly resolve an embarrassing public quarrel between two of its fellows. Newton certainly chose his words carefully to achieve that objective, but in the light of his previous and subsequent behaviour it seems more than likely that, as Faulkner suggests, he took equal care with the hidden subtext. Faulkner has selected a number of relevant sentences, and has attempted to interpret Newton's intended meaning.

"What Des-Cartes did was a good step." (Interpretation: he did it before you did.) "You have added much in several ways, & especially in taking ye colours of thin plates into philosophical consideration." (Interpretation: all you did was follow where Descartes led.) "If I have seen further it is by standing on ye shoulders of Giants." (Interpretation, taking particular notice of Newton careful use of the capital "G"; my research owes nothing to anybody *except* the ancients, least of all to a little runt like you.)

Taking the exchange of letter at face value, they achieved the society's objective of pouring public oil on troubled waters and restoring respectability to the dealings between two of its fellows. But the upshot was that Newton retreated even further into his shell following this encounter. He waited until Hooke died before publishing his *Opticks* in 1704, when he could safely have the last word.

J. Gribbin
New Scientist, 13 (February 1993) 48-49

Absolute Peer Review

Peer review of proposals requesting federal funding in support of research has recently come under fire by Congress (*Science*, vol. 256, p. 959, May 15, 1992). There is the suspicion that peers often do not adequately understand all the ramifications of the proposed research that they are asked to judge. Webster's Dictionary defines "peer" as "a person of the same rank, value, quality, ability, etc." It is obvious that no two people on earth are identical in rank, value, quality, ability, etc. Peership is therefore approximate, which causes the problems that have caught the congressional eye.

In the absolute peer review system that I propose, only the author – the absolute peer – is asked to review his or her own proposal. Revolutionary as this concept may seem to be, it has in fact already been tested in the field – and with remarkable success. In January 1984 I submitted a proposal to the National Science Foundation requesting funds to analyze isotopically a set of deep-sea cores that I collected in the Cariaco Trench, a small basin off the coast of Venezuela. On June 2, 1984, the National Science Foundation sent me a copy of my own proposal for review. Here is a summary of my completely objective and remarkably knowledgeable review:

> I have nothing but unabashed praise for this proposal. Moreover, the budget is extraordinarily modest. This is a huge bargain for the National Science Foundation. Consequently, I recommend that the amount awarded be double the amount requested. I rate this proposal EXCELLENT and request that you send me more of Emiliani's proposals for review.
>
> Signed: Cesare Emiliani
>
> OTHER SUGGESTED REVIEWERS: Cesare Emiliani

The absolute peer review system that I recommend has many advantages. The vexing problem of trying to reconcile the usual vastly discordant reviews is eliminated – there is only *one* review. The saving in time, money, and aggravation at the program manager level is simply inestimable. As long as our scientists are bombarded with requests to review proposals that they do not understand (but that they review anyway just to show that they actually understand them), their precious time is taken up, their train of thought is interrupted, their research work falls into disarray, and the United States will lag farther and farther behind. I thoroughly agree with Senator Robert C. Byrd (Democrat, West Virginia): The peer review system as now practiced must go. Absolute peer review is the way of the future

Cesare Emiliani,
Journal of Irreproducible Results, 37 (6)(1993) 12

Science and wealth creation

How can governments ensure that wealth is created from the scientific enterprise? In an abbreviated version of his talk at *Nature*'s recent meeting [1], Sir Mark Richmond assesses the probable British strategy.

Without an excellent basic research base, I do not believe Britain can ever produce a highly developed workforce for the needs of its industry in the next century. Research provides the essential background against which the specific programmes of industry and commerce can be pursued. But even though a strong basic research base is essential, it is folly, in my view, to expect miraculous benefits to flow simply from having one, however distinguished. One must have some overall strategy and the necessary mechanisms to facilitate its exploitation.

The development of a "mission" for the science base in general and for basic research in particular [1,2] immediately raises issues of who is to shape and coordinate it. I would see the research councils (or analogous bodies), with their ability to fund research both proactively, in the context of strategy, and responsively, responding to bids arising ultimately from the imagination of gifted scientists, as a crucial importance in this respect. Such a dual mode of proceeding allows a degree both of direction and of free thought to flow in science.

Of course, not all public money for research in Britain flows through the research councils. Departments of government have science budgets to pursue their objectives as set by parliament when their receive their parliamentary votes; any national strategy for wealth creation through research will need to integrate spending by these departments with that of the research councils.

Recent events have tended to work in the opposite direction. Up to the last general election, research in higher education institutions was supported very substantially by money flowing through a simple department of government, the Department of Education and Science. Until then the two legs of the dual-support system for university research were the responsibility of one secretary of state. Now, with the setting up of the Office of Science and Technology (OST) and the demise of the University of Polytechnic funding councils to form regional funding councils, the dual-support system is in the hands of five government departments and five cabinet ministers. Some feel that this will give the consideration of science by the cabinet greater weight, others are not so sure. At all events, all the signs are that the integration of an overall wealth-creation policy will not be helped by these changes. The birth of OST has had the effect of constructing additional interfaces without reducing the number elsewhere.

Then there is the matter of the money the research councils/OST spend compared with government spending departments. This is an issue of absolutely central importance to any strategy for the science base and for wealth creation. In fact so important is it that one can only imagine that the critical decisions in this area must have already been taken by the government before the announcement of the forthcoming White Paper (policy document) on scientific research. In one important regard matters in this area is obscure, at least to outsiders. It relates to how government departments bid for science money and how overall priorities are set. Each government department certainly bids for money under the annual public spending procedure, but it is quite unclear to those outside "the ring fence" whether these bids contain specific lines for research. The whole area of public expenditure bidding is covered by low cloud. Consequently it is unclear whether the government has anything approaching an integrated research budget – even in concept – or whether there are merely a series of individual departmental bids against headings of *research* over which the chief scientist at OST casts an eye.

Perhaps one example will illustrate the importance of this issue. The pursuit of wealth creation will require an effective interface between the government's science spending and that of industry and commerce. As far as physics-based industries are concerned, the Department of Trade and Industry has a central role. Other government departments are in the lead for other areas: for example the Ministry of Agriculture, Fisheries and Food for the food industry and the Department of Health for pharmaceuticals. So who is to coordinate these activities, let alone coordinate them with the relevant research councils, who themselves are supporting (and in some cases doing) research in cognate areas?

The setting up of OST and the announcement of the White Paper with OST central to its production suggests that it is that department which is to play the coordinating role. But can one expect OST, itself a vote holder from parliament for a part of the science base, to combine an executive role with respect to that money with an advisory/supervisory one for the spending of other government departments, particularly when they are likely to be bidding against one another? I do not believe OST, at least as operating at the moment, can be both executive and advisory/supervisory on the same topic.

So should all the spending departments' science money be transferred to OST? Probably. But unless things are already decided, there is not much time to arrange that, even in principle, before publication of the White Paper. Nor can one believe this is actually to happen – on a large scale at least. The alternative possibility – to transfer all OST's money to spending departments – would also have serious disadvantages. Apart from the politically difficult step of reversing the recent dual-support transfer, depriving OST of virtually all its funds would weaken it disastrously.

Because I believe the generation of advice should be clearly separated from executive control. OST may well have to be divided functionally to reflect two distinct roles. I don't feel scientists, or even those in industries concerned with wealth creation, should be too sanguine about this. The motivation for any decision in this area is likely to be almost exclusively one of raw politics, and the decision will have a profound effect on how science is managed in the future.

As well as influencing the structures and missions of the research councils, the White Paper may have a profound impact on the way they operate. Speaking personally, I am attracted to a "mission-oriented" way of operating; but it would have important implications. In particular a research council would seek to fulfil its mission wherever the work could be done most effectively. For the Science and Engineering Research Council this would not necessarily be in higher education institutions, and so the White Paper could indirectly signal a sharp change in funding patterns.

M. Richmond, Nature, 362:584 (15 April 1993)

1. Dickson, D., *Nature*, 362:285-286 (1993)
2. Dickson, D., *Nature*, 360:705-706 (1992)

Top 10 US Universities

Top 10 Universities: Biological sciences (Citation analysis, Sept. 1987-Aug. 1990)

Rank	Institution	Number of papers	Total citations	Citations per paper
1	Rockefeller U.	1,646	13,094	7.96
2	Caltech	837	6,450	7.71
3	MIT	2,025	14,246	7.04
4	Stanford U.	3,962	24,539	6.19
5	Princeton U.	612	3,713	6.07
6	U. California, Berkeley	2,353	14,025	5.96
7	Harvard U.	10,610	59,557	5.61
8	U. California, San Francisco	5,908	29,838	5.05
9	U. California, San Diego	3,824	17,566	4.59
10	U. Oregon	378	1,731	4.58

SOURCE: *Science Watch*, Institute Scientific Information, Philadelphia

Top 10 Universities: Chemistry (Citation analysis, 1984-91)

Rank	Institution	Number of papers	Total citations	Citations per paper
1	Caltech	873	16,101	18.44
2	Harvard U.	856	15,035	17.56
3	U. Chicago	729	11,709	16.06
4	U. California, Santa Barbara	691	10,519	15.22
5	MIT	1,415	21,405	15.13
6	U. Colorado, Boulder	698	10,373	14.86
7	Yale U.	732	10,809	14.77
8	Stanford U.	952	14,049	14.76
9	U.N. Carolina, Chapel Hill	722	10,648	14.75
10	Northwestern U.	871	12,328	14.15

SOURCE: *Science Watch*, Institute Scientific Information, Philadelphia

Top 10 Universities: Physical Sciences (Citation analysis, Sept. 1987-Aug. 1990)

Rank	Institution	Number of papers	Total citations	Citations per paper
1	U. California, Santa Cruz	547	2,495	4.56
2	Harvard U.	2,253	9,479	4.21
3	Princeton U.	1,933	7,273	3.76
4	U. Chicago	1,327	4,959	3.74
5	U. California, Santa Barbara	1,522	5,438	3.57
6	Yale U.	1,177	3,940	3.35
7	Boston U.	561	1,848	3.29
8	Caltech	3,121	10,224	3.28
9	Stanford U.	2,887	9,445	3.27
10	U. Houston	892	2,757	3.09

SOURCE: *Science Watch*, Inst. Scient. Inform., Philadelphia

Top 10 Universities: U.S. Nobel Prize Winners (In residence at time of award)

Rank	Inst.	Number of laurates
1	Harvard U.	24
2	Rockefeller U.	13
3	Caltech	11
4	U. California, Berkeley	10
5	Stanford U.	9
6	Columbia U.	7
–	MIT	7
7	Cornell U.	6
–	U. Chicago	6
8	Princeton U.	4

SOURCE: *Nobel Foundation Directory*

Top 10 Universities: Total R&D Expenditures (Dollars in Thousands, 1991)

Rank	Institution	Amount
1	Johns Hopkins U.	$710,095 *
2	U. Michigan	363,582
3	U. Minnesota	331,471
4	U. Wisconsin, Medison	326,489
5	MIT	318,901 **
6	Stanford U.	310,429
7	Cornell U.	309,535 **
8	Texas A&M U.	288,005
9	U. Washington	274,423
10	U. California, San Francisco	268,700

SOURCE: National Science Foundation (NSF 92-329)

The Scientist, (March 8, 1993) 8-9

* *Includes Applied Physics Lab., with $439 million in total R&D expenditures.* ** *Does not include R&D expenditures at univ.-associated federally funded R&D centers.*

Why cheat?

The present debate over fraud and unethical behaviour in science (*Nature* 356, 730; 1992) fails to ask why people cheat. Until this question is asked and answered, and the incentives for cheating are removed, some people will continue to do so, regardless of the rules and guidelines.

People cheat only if their expectation of gain exceeds their fear of exposure. Cheating does not advance science, so its presence indicates that something is wrong with the reward system. People are rewarded (by jobs or grants) for filling their *curricula vitae* with unexamined papers. This reward system is also responsible for such phenomena as the 'minimum publishable unit' and competitive 'grantmanship', by which the efforts of honest scientists are diverted from productive research.

The present funding system in science (at least in the United States) is destructive because it has replaced healthy competition (to make discoveries or to develop useful products) with wasteful competition (to have the longest *c.v.* or highest ranked funding proposal). Cheating is rarely caught, and is rewarded in this wasteful competition. The solution must restructure the evaluation process so that it is based on a proposer's entire record of accomplishment rather than on the promises made in a proposal. Reform should also guarantee a minimal level of support to every productive and original scientist, drawing the required resources from larger groups organized around a successful grant-getter. Such a concentration of people and resources into large groups chokes original and venturesome research because only the group leader is independent, and even he is constrained by the need to market his funding proposals to his peers and the pressure to find support for his group.

Competitive review of proposed research ensure that only consensus science will be supported. New and original ideas are unlikely to attract consensus approval, and are discouraged by the present system. Most scientists therefore disguise their intent in the proposals, which encourages more serious cheating.

J. Katz, Nature, 358 (2 July 1992) 10

Tények a magyar meteorológiai kutatásról

1. Bevezetés

A Magyar Tudományos Akadémia (MTA) Földtudományi Osztályának keretében működő Meteorológiai Tudományos Bizottság (MTB) az 1990-93-as ciklusban számos alkalommal foglalkozott a hazai meteorológiai kutatás kérdéseivel. Ennek aktualitást adott (és ad még ma is) a tudományos közélet átalakulása, valamint az Országos Meteorológiai Szolgálat (OMSz) nagymértékű létszámvesztésével együttjáró átszervezés. A helyzet áttekintése céljából állíttatta össze az MTB az alábbi anyagot. Ebben nem tekintjük át a magyar meteorológiai kutatás történetét, sőt a jelenét sem kívánjuk teljes részletességgel bemutatni (elkerülendő a terjengősséget és a kutatás határairól szóló vitát). Bemutatunk azonban néhány alapvető tényt, amelyek meghatározóak a meteorológiai kutatás tekintetében; felhívjuk a figyelmet bizonyos tendenciákra; elvégzünk néhány összehasonlítást, valamint megfogalmazzuk javaslatainkat is.

I. táblázat
Szervezetek, létszámok, minősítettek

Szervezet	Meteorológusi tevékenység kezdete	Szakdiplomások létszáma 1950	1960	1970	1980	1990	1992	Minősítettek száma 1993. március
Országos Meteorológiai Szolgálat	1870	(44)	116	123	217	204	102	13
KLTE Meteorológiai Tanszék	1934	7	3	3	3	3	4	2
ELTE Meteorológiai Tanszék	1945	5	6	6	9	8	8	4
Magyar Honvédség	1951		2	6	6	12	12	
ORFI	1951		2	2	2	2	2	1
JATE Égh. Tanszék	1952		3	4	5	4	5	1
GATE Vízgazdálkodási Tanszék	1954		3	3	2	3	3	1
VITUKI	1956		1	3	3	1	1	
Kertészeti Egyetem, Agrometeorológia & Vízgazdálkodás	1963			2	2	2	2	
ELTE Regionális Földr. Tanszék	1964			1	1	1	1	1
DATE Agromet. & Agrofiz. Tanszék	1967			2	4	4	4	1
Szombathely Tanárképző Főiskola	1982					1	1	1
Pécsvárad Mezőgazdasági Szakm.	1986					1	1	1
Keszthely Agrártudományi Egyetem	1988					1	1	
Pécs NEFELA Társaság	1991						3	1
Környezetvédelmi és Területfejlesztési Minisztérium	1991						2	1
Miniszterelnöki Hivatal	1992						1	1
Veszprém Egyetem	1992						2	1
Mosonmagyaróvár Agrártudományi Egyetem	1992						1	1
Repülőtéri Meteorológiai Szolgálat	1992						2	
Összesen:		56	136	154	254	248	157	31

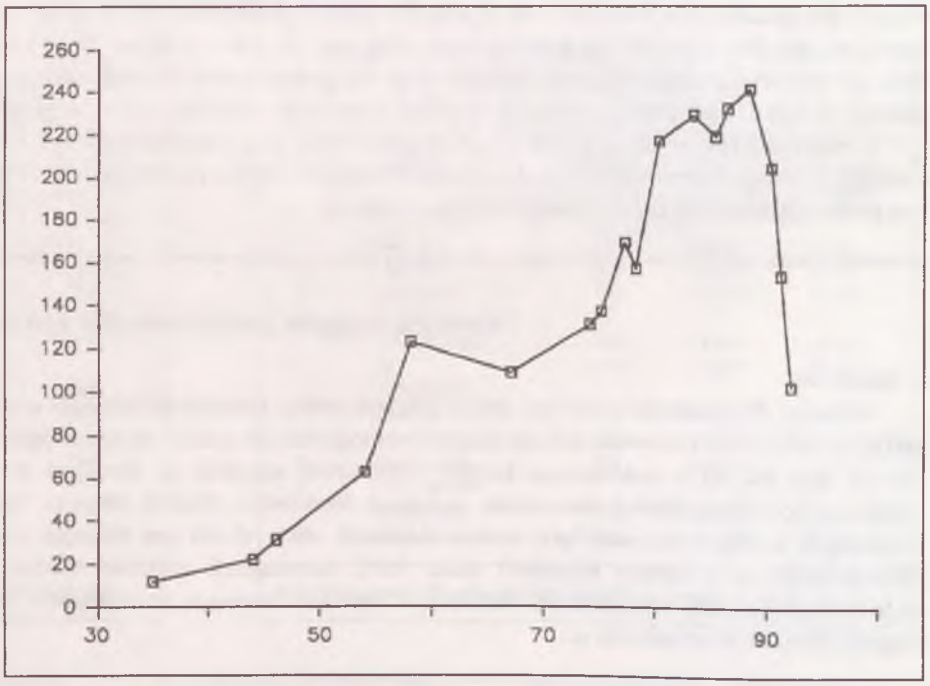
II. táblázat A minősítettek eloszlása fokozat és aktivitás szerint				
Tud. fokozat	Aktív	Nyugd.	Külf.	Összes
Akadémikus	2	-	-	2
Doktor	6	2	-	8
Kandidátus	23	11	2	36
Összes:	31	13	2	46

2. *Szervezetek, létszámok, minősítettek*

Annak ellenére, hogy az ország területén a meteorológiai tevékenység mintegy két évzázados múlttal bír, kimondottan kutatásra orientált meteorológiai szervezet eddig még nem létezett. (Igaz ez akkor is, ha az 1950-es évektől 1984-ig az OMSz kutatóintézeti besorolásban volt, hiszen el kellett látnia a hagyományos szolgálati teendőket.) Ez nem volt akadálya annak, hogy a mindenkori meteorológusok – bárhol dolgoztak is – kisebb-nagyobb intenzitással kutatói tevékenységet is végezzenek .

Minden korban nehéz pontosan megvonni a határt a kutatói és az egyéb szakmai tevékenység között, ráadásul ez a határ az időben változik. Ezért nem vállalkozunk arra, hogy bizonytalan kimutatások és becslések alapján megadjuk, hogy hány meteorológus foglalkozott és fogalkozik kutatással, hanem a szakdiplomások és tudományos minősítésűek létszámát mutatjuk be az I. táblázatban. Szakdiplomásnak tekintjük azokat, akik meteorológiai tárgyú tanulmányt készítettek, illetve készíthetnek munkájuk és felkészültségük alapján, függetlenül az oklevelükben szereplő szakmától.

A meteorológusok körében szokásosnál aktívabban vettek/vesznek részt a kutatásban, legalábbis pályájuk egy szakaszán azok, akik tudományos minősítést szereztek. Az ő számukat mutatja az I. táblázat utolsó oszlopa a munkahelyek szerint, a II. táblázat pedig a tudományos fokozat szerint. Ez utóbbi táblázat számot ad a külföldön lévő és a nyugdíjas minősítettekről is.



1. ábra
Az Országos Meteorológiai Szolgátalnál dolgozó diplomások létszámának változása 1935 és 1992 között

3. *Tudományos kiadványok*

A hazai meteorológiai tudományos kiadványokban megjelent tanulmányok száma az elmúlt 50 évben általában növekedett. Egyedüli kivételt az OMSz "Beszámoló Kötetek" című sorozata jelent. Különösen örvendetes a hivatásos szakmai kiadóknál megjelentetett meteorológiai tárgyú könyvek számának ugrásszerű növekedése az utolsó évtizedben. Az *Időjárás* teljesen angol nyelvűvé és nemzetközivé válásával (1992-ben a szerzők fele külföldi volt) hiányozni látszik a *Beszámoló Kötetetek* folytatása.

4. *Idézettség*

Philadelphiában (USA) működik az Institute of Scientific Information, amely összeállítja az *SCI* (*Science Citation Index*) adatbázist. Ennek alapján, de az adatok jelentős szűrésével, állítja elő az MTA Könyvtárának Informatikai Igazgatósága "A Magyar Természettudományos Alapkutatás Publikációs Adatbankjá"-t, amelynek az 1980-1989 évekre vonatkozó része már a hazai kutatás rendelkezésére áll: MTA-PAB, Számítógépes Felhasználói Változat I.0 néven [1]. Az SCI-ben a világnak közel 4000 természettudományos és műszaki folyóirata szerepel, nemcsak a megjelent cikkekkel, hanem a rájuk történő hivatkozásokkal is. Az MTA-PAB tartalmazza az 1980-89 közötti 10 évre a kiválasztott folyóiratokban megjelent azon cikkeket, amelyeknél legalább egy szerző magyarországi munkahelyet adott meg; valamint az adott cikknek az adott 10 évben az SCI-ben megtalálható összes idézettségét.

A III. táblázatban bemutatjuk az MTA-PAB-ban szereplő meteorológiai cikkek és idézettségeik számát, ez utóbbit felbontva saját és idegen idézettségekre. (Önidézettségnek tekintjük, ha az idéző cikk szerzői közül legalább egy azonos az idézett cikk valamelyik szerzőjével.) Itt igen sajnálatos tendenciát látunk: az adaibázisban szereplő 45 cikkből az első 5 évben jelent meg 29, a második 5 évben mindössze 16. Az 1983. évi csúcsról meglehetősen egyenletes a publikációk számának csökkenése. A IV. táblázatban felsoroljuk, hogy a 45 cikket mely folyóiratok jelentették meg. Érdemes felfigyelni rá, hogy a 22 folyóirat között 3 magyar is van az SCI-MTA-PAB adatbázisában.

III. táblázat
Magyar meteoroógiai cikkek és idézettségeik száma 1980-89 között
az MTA-PAB adatbázisból

Év	Cikkek száma	Idézetek száma	Ön-idézet	Idegen idézet
1980	3	4	1	3
1981	5	11	1	10
1982	3	12	0	12
1983	11	21	3	18
1984	7	13	6	7
1985	6	6	0	6
1986	1	0		
1987	4	4	0	4
1988	3	4	1	3
1989	2	0		
Összes	45	75	12	63

IV. Táblázat
A magyar meteorológiai cikkeket közlő folyóiratok
az 1980-89-es MTA-PAB adatbázisból

Folyóirat neve	cikkek száma 1980-89 között
Acta Agronomica	2
Acta Chimica Hungarica	1
Aerosol Science	1
Agricultural and Forest Meteorology	2
Archiv für Met. A	1
Archiv für Met. B	3
Atmospheric Environment	10
Climatic Change	1
Journal of Climate and Applied Met.	2
Journal of Climatology	1
Journal of Environmental Management	1
Journal of Geophys. Res.	1
Meteorologische Rundschau	1
Mikroskopie	1
Növénytermelés	3
Physica Scripta	1
Pure and Applied Geophysics	1
Science of the Total Environment	3
Solar Energy	2
TELLUS	5
Theoretical and Applied Climatology	1
Water Air and Soil Pollution	1
Összesen:	45

5. Az idézettség elemzése

Az 1981-85-ös évekre az MTA Könyvtára által az SCI adatbázis alapján épített nemzetközi tudománymetriai adatbázison (SCINDO9) elvégzett elemzés [2] szerint a természettudományok területén Magyarország a publikációknak 0,47%-át produkálta (ez a 27. helyet jelenti az országok sorrendjében), ezekre átlagosan 1,19 idézet jutott. A fizika és a földtudományok területén az ország produkciós részesedése 0,34 % (ez is 27. hely), az idézettségek átlaga 2,25. A szakterületet tovább szűkítve a meteorológia és a légkörtan területére, Magyarország nem szerepel a jegyzett országok között (részesedése kisebb 0,33%-nál, ami 51 cikket jelent 5 év alatt). A szakterületen az átlagos idézettségi szám 3,61. A III. táblázat alapján a hazai meteorológusok által publikált cikkekre az átlagos idézettségi szám 1,40.

V. táblázat
Meteorológiai cikkeket közlő folyóiratok
átlagos idézettsége 1981-85. (*: közölt magyar cikket 1980-89-ben)

Folyóirat neve	átlagos idézettsége
Agricultural and Forest Meteorology*	1,35
Archiv für Met. A*	0,51
Archiv für Met. B*	0,82
Atmospheric Environment*	3,27
Bulletin Am. Met. Soc.	1,96
Boundary Layer Met.	1,92
Climatic Change*	2,18
Dynamics of Atm. and Oceans	2,33
Izv. A.N. SSSR Fiz. Atm. Okeana	0,64
Int. Journal of Biomet.	0.78
Journal of Air Poll. Contr. Ass.	1,26
Journal of Atm. Terr. Phys.	2,63
Journal of Atm. Sci.	5,11
Journal of Climate and Appl. Met.*	2,15
Journal of Geophys. Res.*	5,73
Monthly Weather Rev.	4,35
Meteorological Magazine	0,59
Meteorologische Rundschau*	0,59
Pure and Applied Geophys.*	0,57
Quarterly J. Roy. Met. Soc.	3,59
Remote Sensing of Environment	1,91
Solar Energy*	1,26
Tellus*	2,54
Zeitschrift für Met.	0,30
Összes átlag:	2,01
* átlag:	1,91

Tudjuk, hogy vannak rangosabb és kevésbé rangos folyóiratok. Nagyobb hatása van azoknak a folyóiratoknak, amelyek cikkeit többen idézik, ezért a folyóirat rangjának tekintik a cikkeire vonatkozó átlagos idézettségi számot (impakt). Mennyire rangos az a 22 folyóirat, amely a magyar meteorológiai cikkeket megjelentette? Szintén az 1981-85-ös adatbázison elvégzett munkára [2] támaszkodva közöljük az V. táblázatot, amelyben 24 olyan folyóirat szerepel, amely közöl meteorológiai cikkeket, az 5 év alatt végig benne volt az SCI adatbázisban, így meghatározható volt az ötéves (Schubert-Glänzel-Braun féle) impakt faktora. Ezen számok egymásközti összehasonlítása megmutatja, hogy bármely két folyóirat közül várhatóan melyik hatékonyabb. Az 1980-89 között megjelent 45 magyar cikket közlő 22 folyóirat közül 11 volt benne 1981-85 között végig az SCI adatbázisban, a másik 11 vagy rövidebb ideig, vagy/és máskor volt benne. A 24 folyóirat átlagos idézettségének átlaga alig nagyobb, mint a magyar meteorológiai cikkeket közlők átlaga, így ezen cikkek szerzői megtalálták a megfelelő helyet a nemzetközi szakmai kommunikációra.

6. Következtetések

6.1 A meteorológiai területeken dolgozó diplomások számának csökkenése igen jelentős. Valószínűleg a tudományos produktumok számában a változás nem lesz ilyen irányú, hiszen a létszám növekedésekor sem volt az. A csökkenő létszámú szakembergárda egyre több munkahelyen dolgozik, ezért keresni kell az informálás és együttműködés újabb eszközeit és formáit. A tudomány terén erre az MTB hivatott.

6.2 A tudományos cikkek számából arra következtethetünk, hogy szerzői oldalról létjogosult volna egy magyar nyelvű szakmai lap, amely nem az eredetiségre számottartó kutatási eredményekről, hanem azok alkalmazásairól tájékoztatná a hazai érintetteket, ápolná a magyar szakmai nyelvet (noha erről nincsenek illúzióink), rendezvényekről, eseményekről, stb. informálná a szakembereket. Kísérletet kell tenni a pénzügyi és technikai feltételek megteremtésére.

6.3 A tudományos minősítés új rendszerében gondoskodni kell a követelmények magas szintjéről és arról, hogy ez az időben együtt fejlődjön a tudomány nemzetközi elvárásaival. Ugyancsak ügyelni kell arra, hogy a különböző egyetemeken szerzett fokozat azonos telesítményt fejezzen ki.

6.4 Az SCI-MTA-PAB adatbázissal kapcsolatban el kell érni, hogy

– a meteorológusok megismerkedjenek ennek rendszerével és publikációs stratégiájuk kialakításakor használják fel ezeket az ismereteket, hiszen az eddigi eredmények megmutatták, hogy kollégáink egy kis része jó publikációs stratégiát tudott érvényesíteni,

– az *IDŐJÁRÁS* kerüljön be az adatbázisba, mert ez erkölcsi elismerés a magyar meteorológiának és egyúttal jobb lehetőséget biztosít arra, hogy a külföld megismerje a hazai meteorológiai kutatás eredményeit.

Major György, Tänczer Tibor, Iványi Zsuzsanna, Pálvölgyi Tamás,
Országos Meteorológiai Intézet

[1] A Magyar Természettudományi Alapkutatás Publikációs Adatbankja (Publikációs és idézettségi adatok, 1980-1989). *Impakt* különszám, 1992.
[2] Schubert, A., Glänzel, W., and Braun, T.. *Scientometric* Datafiles: A comprehensive set of indicators on 2649 journals and 96 countries in all major science fields and subfields, 1981-1985. *Scientometrics*, 16, (1-6)(1989).

Academica Cosa Nostra?

● A szerkesztőség kötelességének érzi a cikk szövege előtt megjegyezni, hogy minden hasonlóság a cikkben leírtak és a hazai helyzet között csupán a véletlen műve.

The august pages of the scientific journal *Nature* were filled last autumn with an unseemly debate about the nature of promotion at Italian universities. In a debate illustrated with graphs showing international scientific recognition against level of appointment, scientists at one of the larger Italian universities complained that individuals were being promoted internally on the basis of political connections rather than on scientific merit. North European and American academics were more than happy to accept this argument. "Of course Italian universities don't promote on merit. The mafia runs everything." So we tut-tutted and cluck-clucked.

But what was striking about this unwholesome debate was the lack of comparison with the promotion system at British universities. There are good arguments for suggesting that our system is just as arbitrary. For example, a friend of mine (who for obvious reasons had better remain anonymous) working at university in the south of England tells me that there is no relationship between the level of appointment and scientific merit for most members of academic staff at her institution. She works in a department with forty full-time members of staff of whom five are professors, five readers, ten senior lecturers and twenty lecturers. About thirty full-time research staff employed on grants bring the numbers up to some seventy.

As a matter of curiosity, she recently went to the *Science Citation Index* to examine the relationship between the level of appointment of each academic individual in her department and their scientific standing and output. Using the fancy new computerised CD database, she examined various key indices. She asked how many papers her colleagues had published over the past five years. And she asked how often her colleagues had been cited in other papers in the same time frame. She then compared their rankings with these indices.

All the correlations she found were weak to the point of nonexistence. There were individuals who published masses (very weak positive correlation with rank) but were rarely cited and there were those who published modestly (zero correlation) but were cited masses. The latter group were clearly the international authorities in their fields judging by the frequency of their invitations to international meetings. But being an international authority seemed to have no bearing on rank. She thought there might be a correlation with age (which seemed a bit stronger – obviously a case of "Buggin's turn"), and even speculated about a relationship with individual weight. Certainly, there were more fat cats who were corpulently professiorial than slimline types who were rakishly lecturous but, again, it was no absolute rule. Thinnies were professors and fatties were also lecturers.

She tried numbers and size of external grants (weak relation), numbers and size of internal grants (stronger positive association with rank), amount of teaching (very powerful negative correlation – no surprise there) and, finally, she hit on the answer: nepotism. The single most important factor in dictating rank of an individual was the timescale between arrival of the latest head of department (her place has a notoriously high turnover) and arrival of the ranked individual in question. Heads of departments come, these days, with a couple of new appointments thrown in. And all of us like to go to a party with a couple of familiar faces when we are not sure how many people we are going to know.

And so it is with the British university system. If you want to get ahead then chat up anybody who you know who is about to get a departmental headship. You can be the lowliest of the low in your current job. But going along to press the head's hand when he is lonely, new and frightened, guarantees a senior lectureship at least. Of course, my friend might have been kidding. Italians do have a strange sense of humour.

Simon Wolff, New Scientist, 17 April 1993, p. 46.

Pain relief

Forschen auf Deutsch: der Machiavelli für Forscher – und solche die es noch werden wollen. (Researching in German: The Machiavelli for scientists – and those who still want to become one.) By Siegfried Bär, with cartoons by Irena Volpi. Harri Deutsch, Frankfurt, 1992. Pp. 125. DM 20 (pbk)

You crawl home after one of those days in the lab. Your graduate student has wasted the whole digitonin batch. The manuscript that you have slaved over for past year has been rejected – but then again, so has your grant application. You are dangerously close to the end of your contract, a fact that has just today been brought to your attention (again!) by your supervisor. To round things up, you had to spend the past hour convincing the usual salesman that you really didn't need a third centrifuge, much less could afford one. You are in terrible need of something, someone to soothe your soul.

Here it is! Written by a postdoc, the same downtrodden academic creature as yourself, this guide in German offers a wildly biased, cynical view of academic life. You feel immediately drawn to the slashing description of all that oppresses you, your neck aches from nodding in bitter appreciation, sometimes you find yourself rolling in the floor with laughter. You have found a voice, someone exposing all the injustices that have befallen you. You cheer, you agree wholeheartedly, and you quickly skip those pages the author offers his own ambitious advice for solving your problems (after all, you don't want to spoil that sweet feeling of complete harmony with the author).

Then, after a while, with composure regained, you are able to take a look at yourself, your own failings and shortcomings, your own efforts that, this very evening, look so hopeless to you. So many refreshing passages convince you that you are not alone. For example:

> "Usually, the researcher reads only articles of his very own field in their entirety. Nevertheless he spends a considerable part of his time copying papers which, mostly unread, are stacked or filed. For our time-pressed researcher, copying represents spiritual ownership, an act taking on the importance of a ritual that replaces the tedious process of actually reading the paper. Copying soothes the researcher and blesses him with inner peace and the feeling not to have missed an important bit of information."

There are lots of cartoons, ideally suited for the department coffee room, depicting, for example, the Measurements Flunky (his head "contains raisins in a cake mix of little material expectations, ambition and *naiveté*) or a chart on Academic Metabolism (with the inevitable "USA shunt"). There are interesting bits of information snatched from *Nature* and *Science* on science politics: 20 per cent of all biochemical or molecular biology articles – or 40 per cent of all medical papers, for that matter – are never cited. And how about this: in 1982, the average German research paper (which cost the taxpayer $188,570) was cited only 0.85 times, whereas the average British one could boast an equally meagre 0.96 citations (while, admittedly, costing the UK citizen only $81,260). Sobering facts for all, whether graduates, postdocs, research group leaders, chairmen, deans or government science administrators.

This a book that you will want to give your child to prevent it from foolishly entering academic science as you yourself did. And it is just the book you need to hang on to until you are rewarded, again, with that exhilarating feeling that, after all, science is the "most exciting thing you can do with your pants on", as you will hear – and wholeheartedly agree with – during the late hours of the banquet of your next congress, when all will have ended well.

Gerald Zernig,
Nature, 362:708, 22 April 1993.

Genuine genius

Jonathan Katz [1] complains about systems of competitive review which encourage "consensus science", and wants to reform the system so as to encourage "original and venturesome research". But I doubt that any system, conservative or reformed, can either silence or create genuine genius.

It is part of the nature of new and original ideas that present systems tend to discourage them. This was true at the time of Jeroboam in Israel, where King Solomon first tried to silence Jeroboam's ideas for social reform by offering him a lucrative job within the system [2]. And it was true at the time of Descartes in Europe, where even the University of Leyden forbade mention of his name and where the atmosphere that led to Galileo's persecution led Descartes to decline to publish *Le Monde* during his lifetime [3].

Katz suggest reforming the present funding system in science to encourage original and venturesome research, but the sort of people who are good at manipulating systems will probably quickly learn to succeed in whatever new system we adopt, while genuinely original and venturesome thinkers will probably always have a hard time. That is not necessarily a bad thing. The fact that an idea is original and venturesome does not make it true. The conservativeness of social and academic systems tends to weed out ideas that are original but not true, and helps to ensure that the original thinkers who survive the tendency of the system to discourage them will be those whose ideas are truest and most deserving of a wide and lasting hearing. The conservatives who tried to silence Descartes succeeded in silencing hundreds or thousand of lesser thinkers, but Descartes' ideas, 300 years later, are still influencing our physics, physiology, geometry and philosophy.

F.F. Leawitt, Nature, 360 (December 1992) 505

[1] Katz, J., *Nature*, 358, 10 (1992)
[2] 1 Kings 11:40
[3] Russel, B.: A History of Western Philosophy 559, Simon and Schuster, New York, (1945)
[4] Hart, J.T., *The Lancet* I, 405-412 (1971)

NRC Panel: Abolish Mandatory Retirement

University professors coming up to their 70th birthday should have more than a life of enforced retirement to look forward to. So says a panel of experts on college financing at the National Research Council (NRC). The report concludes that if current rules requiring tenured faculty to step down at age 70 are abolished, colleges would not find themselves so clogged with dead wood that they would be unable to hire young faculty. That was the reason most often cited for not ending the age limit. But the panel's recommendation that mandatory retirement be dropped seems likely to meet with little resistance – even from the research universities – which may be hit hardest by the aging faculty syndrome. As one lobbyist for universities said, "The last thing we need right now is to be seen defending a special privilege" – in this case, the privilege to put 70-year-olds out to pasture.

Congress asked the council for this study after doing away with mandatory retirement for most professions in 1986. But astute lobbying by college administrators persuaded several senators to add professors to a motley group – including police officers and firefighters – exempted from the law until 1994. Congress held out the possibility that it might continue to impose the age 70 retirement rule on these people, but that now seems unlikely.

The National Research Council, meanwhile, was to examine problems that might occur if age limits were abolished for tenured faculty and then report back to Congress. Would this create a "bulge" in demographics, packing the universities with octogenariams? Would is slow the rate of turnover in the lower ranks? Would it smother intellectual ferment or delay the hiring of women and minorities?

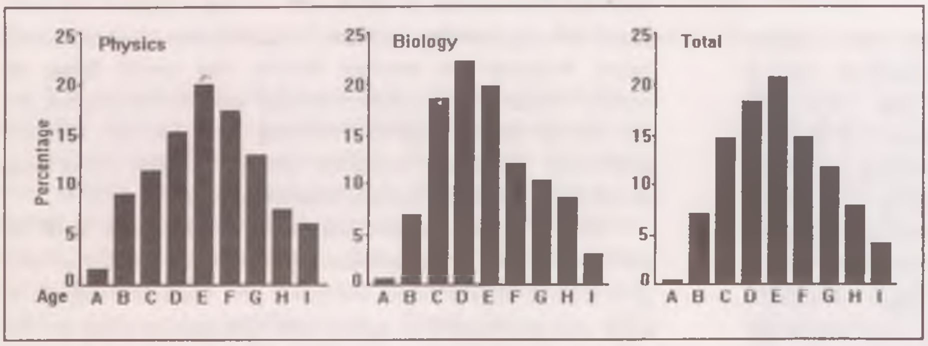


Figure 1. Middle-aged bulge.
The most populous cobort of tenured faculty in all disciplines (rigbt is tbe 45 to 49 age group, beading for retirement in 2112.

A: under 30
B: 30-40
C: 35-39
D: 40-44
E: 45-49
F: 50-54
G: 55-59
H: 60-64
I: 65+

According to the chairman of the panel, Ralph Gomory of the Alfred P. Sloan Foundation, the short answer is: Don't worry. Gomory and his colleagues surveyed 250 universities and found that most public universities and colleagues wouldn't have a problem. Some of the best research universities would, however, see an increase in the average age of faculty. Already, professors at these elite schools seem to enjoy their situation so much that many remain on the payroll until they are compelled to retire. These people would probably stretch their careers even further given the chance, Gomory says. This would present a special problem for places like the University of Chicago, Harvard Medical School, and Yale University, where a large proportion of the faculty – 64%, 85%, and 76%, respectively – already wait until age 70 to retire.

The Gomory committee recommends that these institutions develop special incentives to encourage early retirement. Stanford, for example, is now considering a plan that would offer part-time pay and extra health benefits during a quasi-retirement period to those who agree in advance to retire at 70. It's not clear just how much incentives such as these would cost, however.

The only note of dissent at the meeting where the NRC report was released came from Sheldon Steinbach, representing the American Council on Education. He said that, unlike the big universities, small private colleges would be devastated by the change in rules. Their budgets are so tight already that they won't be able to find the extra cash needed to create special incentives. In a weak economy, Steinbach thinks, aging faculty members will cling to their jobs. The notion that virtually all colleges will be able to coax older faculty into retirement with payoffs is simply a "pipe dream" he says.

The Gomory committee didn't see this as a significant problem, largely because past retirement patterns indicate that there will be no big change in 1994. One batch of data comes from states such as Florida and Wisconsin that have already "uncapped" the age limit, and another from a period in the late 1970s when Congress raised the mandatory retirement age from 65 to 70. "Few faculty chose to continue working past age 70" in the uncapped states, the report says. For example, at the University of Florida, since mandatory retirement ended in 1976, only 1.6% of the faculty have remained beyond 70.

To estimate what might happen to hiring patterns, the committee took past trends and projected them into a variety of scenarios, using the faculty age profiles of real universities as models. At worst, said Donald Hood of Columbia University, a few universities might expect to see the age of the faculty rise over a long period, perhaps leading to a 15% decline in available new posts. The committee concluded that even this would be manageable.

Eliot Marsball, Science, 252, p. 1246

Készült az MTAK bázi sokszorosító részlegében

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