3. évfolyam · 11. szám · 1993. november

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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The Changing Face of Evaluation

If you ever get a chance to hear Michael Quinn Patton speak about evaluation — seize it! His keynote speech at the annual conference of the Canadian Evaluation Society in Banff demonstrated both brilliant showmanship and superb communication of ideas. Patton donned a wild tribal mask to help evaluators see themselves as others see them: the witch doctors. He also shared his version of Genesis with the audience:

After creating the world, God rested and looking about Him said, "It is good!"

Then a tiresome archangel, the very first evaluator, appeared at His side and whined,

"But what are Your criteria? What data are You basing Your assessment upon?

Aren't You a little too close to the situation to make an objective assessment?"

In response God sent the archangel, Lucifer, straight to Hell.

Evaluation is moving away from a dispassionate illumination of truth to a determined search for useful results. Patton emphatically pronounced dead the tedious debate on qualitative versus quantitative research. Concern should not lie with the propriety of evaluation techniques but with the utility of the information they elicit.

Patton favours just-in-time evaluation. We need quick sorties into the field and speedy reporting of results. Evaluators must be part of the fray, involved in the provision of essential information about management processes. In Patton's view, evaluation should not aim to be scientific, academic or value-free. Evaluation is political, unless one of the following conditions is met:

- ⇨ no one cares about the program;
- ⇨ no one knows about the evaluation;
- ⇨ no dollars are at stake;
- ⇨ no power is at stake; or
- ⇨ no one related to the program is sexually active

There have been enough program evaluations over the past decades that it is time to focus more on synthesizing results and looking for effective general models for programming. We also need to stop perceiving programs in a piecemeal fashion and begin thinking about evaluating entire systems. Interestingly, in a well-functioning system, no single component operates at maximum potential. Perhaps the ideal of a maximally performing program is not appropriate. Patton asked the audience to imagine a car made with the world's best carburettor from one manufacturer, the most efficient transmission from another, the best engine from another, and so forth. The result would be a system composed of excellent parts, but it would not perform. Evaluation should be examining the interaction of system components.

Patton also foresees increased recognition of the cultural context for evaluation. For example, after advising Japanese organizations on evaluation, he realized for the first time that the North American approach might be considered "macho." We tend to assume that evaluations should expose information without regard for the consequences. This attitude does not make sense to the Japanese, who place a high value on harmony. In their culture, an evaluation should not cause others to lose face.

Keynote Address at the Banff Conference
Evaluation of R&D 1 (3) (1993) 1

Cosmology Returns to the Physics Top Ten

Cosmology and fundamental theory papers leap to positions #3, #4, and #5 in the physics Top Ten, without having been listed before. Two fullerene reports hold the lid over these newcomers, maintaining the first and second rank positions, but citations for both are now falling back from their peaks of a few months ago.

Paper #3, describes observations with the Infrared Astronomy Satellite (IRAS) on the local structure of the universe. For cosmologists, "local" means reaching a depth of 300 million light years. On this length scale, galaxies — the building blocks of the universe — are of the same importance as atoms in a solid. Astronomers are interested in the clustering of galaxies, the voids between them, and the relative velocities of the galaxies. These data give a dynamic view of the local cosmos, from which the mass distribution is modelled.

Interest in paper #3 is intense because of the battle currently raging among cosmologists on Cold Dark Matter (CDM). Standard models for the origin of the universe based on the hot big bang predict a matter density up to 50 times higher than that observed directly as luminous matter by telescopes. The missing mass could take a variety of forms, including weakly interacting massive particles.

Will Saunders of the University of Oxford is lead author of the paper presenting results from a team of U.K. astronomers who surveyed 2,163 galaxies detected by IRAS. The team, having obtained distances for all galaxies, finds the local universe is like a sponge with big holes — there are many voids, as well as new clusters of galaxies. These data, together with the latest results from NASA's COBE satellite, place very strong constraints on the nature of CDM and models for galaxy formation. Furthermore, the British group reports the mean density of matter is close to the critical value needed to close the universe.

Project leader Michael Rowan-Robinson of Queen Mary and Westfield College, London tells *Science Watch*: "Our results agree extremely well with the density variations found by COBE on much larger scales. Suddenly we are beginning to understand in detail how structure formed in the universe."

What's hot in physics...

Rank	Paper	Citations This Period (Nov-Dec 92)	Rank Last Period (Sep-Oct 92)
1	**A.F. Hebard, M.J. Rosseinsky, R.C. Haddon, D.W. Murphy, S.H. Glarum, T.T.M. Palstra, A.P. Ramirez, A.R. Kortan**, "Superconductivity at 18 K in potassium-doped C_{60}," *Nature*, 360(6319):600-1, 18 April 1991. [AT&T Bell Labs, Murray Hill, N.J.]	34	1
2	**P.A. Heiney, J.E. Fischer, A.R. McGhie, W.J. Romanow, A.M. Denenstein, J.P. McCauley, A.B. Smith, D.E. Cox**, "Orientational ordering transition in solid C_{60}," *Phys. Rev. Lett.*, 66(22):2911-4, 3 June 1991. [U. Penn., Philadelphia, Brookhaven Natl. Lab., Upton, N.Y.]	25	2
3	**W. Saunders, C. Frenk, M. Rowan-Robinson, G. Efstathiou, A. Lawrence, N. Kaiser, R. Ellis, J. Crawford, X.-Y. Xia, I. Parry**, The density field of the local universe, *Nature*, 349(6304):32-8, 3 January 1991. [U. Oxford, U.K.; Quenn Mary & Westfield Coll., London; U. Durham, Durham, U.K.; U. Toronto, Canada]	21	*
4	**U. Amaldi, W. de Boer, H. Fürstenau**, Comparison of grand unified theories with electroweak and strong coupling constants measured at LEP, *Phys. Lett. B*, 260(3-4):447-55, 16 May 1991. [CERN, Geneva, Switzerland; U. Karlruhe, Germany]	20	*
5	**E. Witten**, String theory and black holes, *Phys. Rev. D*, 44(2):314-24, 15 July 1991. [Inst. Advanced Study, Princeton, N.J.]	19	*
6	**S. Saito, A. Oshiyama**, "Cohesive mechanism and energy bands of solid C_{60}," *Phys. Rev. Lett.*, 66(20):2637-40, 20 May 1991. [NEC Corp., Tsukuba, Japan]	18	9
7	**D.E. Spence, P.N. Kean, W. Sibbett**, 60-fsec pulse generation from a self-mode-locked Ti:sapphire laser, *Optics Lett.* 16(1)42-4, 1 January 1991. [U. St. Andrews, Scotland]	18	6
8	**J.R. Clem**, Two-dimensional vortices in a stack of thin superconducting films: a model for high-temperature superconducting multilayers, *Phys. Rev. B—Condensed Matter*, 43(10):7837-46, 1 April 1991. [Iowa St. U, Ames Lab, Ames, Iowa]	17	*
9	**P. Langacker, M. Luo**, Implications of precision electroweak experiments for m_t, ρ_0, $\sin^2\theta_W$, and grand unification, *Phys. Rev. D*, 44(3):817-22, 1 August 1991. [U. Penna., Philadelphia]	17	*
10	**R.C. Haddon, A.F. Hebard, M.J. Rosseinsky, D.W. Murphy, S.J. Duclos, K.B. Lyons, B. Miller, J.M. Rosamilia, R.M. Fleming, A.R. Kortan, S.H. Glarum, A.V. Makhija, A.J. Muller, R.H. Eick, S.M. Zahurak, R. Tycko, G. Dabbagh, F.A. Thiel**, "Conducting films of C_{60} and C_{70} by alkali-metal doping," *Nature*, 350(6316):320-2, 28 March 1991. [AT&T Bell Labs, Murray Hill, N.J.]	16	*

SOURCE: ISI's Hot Papers Database. Only papers published since November 1990 are tracked.

Grand Unification

Papers #4, and #5 both have cosmological resonances, dealing respectively with unification of the fundamental forces and the quantum mechanics of matter interacting with black holes.

Unifying all the forces in nature into a single theory has been the dream of many physicists. "Since the operation of the powerful electron-positron collider (LEP) at CERN, Geneva, physicists have gleaned some hints how all forces might be unified during the first moment of the Big Bang, when particles interacted at energies above 10^{16} GeV," Wim de Boer tells *Science Watch*. "It turns out that our new precise data exclude unification within the standard model of the weak electromagnetic and strong forces. Extrapolation of the strengths of these forces from LEP energies to Big Bang energies shows they never merged at a single energy."

In place of the symmetric standard model, paper #4 calls for supersymmetric theory to be added to the standard model. Then the data are consistent with a single electrostrong force at 10^{16} GeV. "Supersymmetry predicts new supersymmetric partners for each known particle. These should be relatively light, and final proof for supersymmetry should be possible with the next generation of colliders," adds de Boer. For cosmology, the results touch on many puzzles. Grand unification through supersymmetry can explain the absence of antimatter and the excess of photons over baryons in the universe. Furthermore, the lightest of the predicted particles is an excellent candidate for weakly interacting dark matter.

Grand unification is also the theme of newcomer #9, which finds striking agreement between theory and observation for the masses of the heaviest particles when supersymmetry is used.

String theory return to the Top Ten with the debut of paper #5 by Edward Witten of the Institute for Advanced Study, Princeton, who considers an exact field theory for describing a black hole in two-dimensional space-time. The motivation for studying topological field theories is to make a link between quantum gravity and string theory.

Simon Mitton

Science Watch, (February 1993) 6

European Elites Envy American Cohesion

Ask a dozen of Europe's top chemists how European chemistry is faring and where the hot research groups are, and you'll get a dozen different answers. But try asking them the same questions toward the end of March and you are likely to get no answers at all, because many of Europe's chemistry elite will be on the other side of the Atlantic, at the American Chemical Society's (ACS) spring meeting. The diversity of responses — and the nonresponses during March — say a lot about the state of chemistry in Europe.

European chemistry has a distinguished history, helped by the fact that eight of the 10 largest chemical companies in the world are based on the continent and have pumped hundreds of millions of dollars into their own and university labs over the years. And many of Europe's leading chemists

(continued on next page)

Table 1. Country Scorecard

Rank	Nation	Papers	Nation	Cites/paper
1	USA	94,237	USA	4.47
2	USSR	47,870	Israel	4.01
3	Japan	42,229	Switzerland	3.92
4	Germany*	36,859	Netherlands	3.48
5	United Kingdom	26,685	Canada	3.37
6	France	21,342	Sweden	3.36
7	India	15,719	Denmark	3.11
8	Canada	13,430	United Kingdom	3.00
9	Italy	12,508	Australia	2.98
10	Spain	10,566	New Zealand	2.96
11	Poland	8408	Ireland	2.94
12	Netherlands	6872	France	2.88
13	People's Republic of China	6178	Germany*	2.87
14	Australia	5716	Hong Kong	2.80
15	Czechoslovakia	5681	Italy	2.75
16	Switzerland	5197	Japan	2.64
17	Sweden	4440	Austria	2.51
18	Hungary	3570	Belgium	2.32
19	Belgium	3316	Norway	2.22
20	Egypt	3067	Greece	2.13

* The listings for Germany include the papers and citations of the German Democratic Republic and the Federal Republic of Germany together. The paper and citations per paper for each, 1981-91, are as follows: F.R.G. 23,547 papers, 4.01 citations per paper; G.D.R. 5,389 papers and 1.60 citations per paper.

express optimism when asked about the future of the discipline. Take University of Birmingham organic chemist Fraser Stoddart, who is working in one of the hottest fields, self-assembling molecules: "Chemistry is doing exceptionally well in Europe," he says. Or listen to Dieter Seebach of the Swiss Federal Institute of Technology: "Europe," he says, "is doing excellently compared with both the United States and Japan."

There's some evidence to back up those impressions. Europeans have carried off 23 of the 55 the Nobel Prizes for chemistry awarded since 1960. And European groups including those led by Harry Kroto at Sussex University and Wolfgang Kratschmer at the Max Planck Institute of Nuclear Physics in Heidelberg, helped establish the fast-moving field of buckyball chemistry. But the jewels of European chemistry are scattered widely across the continent. "We see excellence in particular fields in laboratories all over Europe," says Nobel Prize-winner Jean-Marie Lehn, whose own lab at the University Louis Pasteur in Strasbourg is at the forefront of self-assembling molecules. "It is difficult to say [what Europeans do best] because research is so varied," he adds.

One reason European chemistry is fragmented is that there's no central funding body like the U.S. National Science Foundation to focus money on the top labs. There are few major European centers of excellence in basic research — of the likes of Caltech, Berkeley, and the Massachusetts Institute of Technology — that are strong across most subdisciplines of chemistry. Instead, each country has its own national research bodies that spread resources around dozens of labs. And there's no European equivalent of the ACS to provide a continent-wide sense of community, nor a European chemistry journal.

Table 2. Europe's Top 25

Rank	Name	Papers	Citations	Cites/Paper
1	Fritz Haber Institute	457	2532	5.54
2	Max Planck Institute Coal Research	379	1832	4.83
3	University of Cambridge	1809	8531	4.72
4	University of Strasbourg 1	810	3807	4.70
5	Max Plack Institute Biophysical Chem.	257	1204	4.68
6	Swiss Federal Institute Tech (ETH)	1372	6396	4.66
7	University of Basel	453	2112	4.66
8	University of Southampton	743	3344	4.50
9	Centre d'Études Nucléaires (all)	385	1702	4.42
10	University of Bristol	849	3738	4.40
11	University of Lausanne	426	1867	4.38
12	University of Mainz	897	3862	4.31
13	Philips Res. Labs (worldwide)	356	1535	4.31
14	University of Oxford	1574	6722	4.27
15	KFA Jülich GmbH	449	1910	4.25
16	University of Florence	488	2445	4.24
17	State University of Gronigen	672	2832	4.21
18	University of Frankfurt	488	2038	4.18
19	University of Zurich	410	1711	4.17
20	University of Sussex	714	2928	4.10
21	University of Exeter	396	1608	4.06
22	University of Bielefeld	410	1651	4.03
23	Catholic University of Nijmegen	600	2406	4.01
24	University of Liverpool	484	1918	3.96
25	University of Constance	335	1323	3.95

Table 3. America's Top 5

Rank	Name	Papers	Citations	Cites/Paper
1	Harvard University	937	8465	9.03
2	Natl. Inst. of Standards & Tech.	393	3513	8.94
3	Caltech	821	6817	8.30
4	Yale University	749	5953	7.95
5	University of Chicago	713	5606	7.86

Citation rankings.
Some small European countries do well in terms of citations per paper (Table 1). European institutions with the highest average citations per paper (Table 2) rank below the top U.S. institutions (Table 3). Citation counts were conducted for *Science* by ISI's research department, which surveyed papers published between 1988 and 1992 in journals of chemistry and multidisciplinary journals such as *Science* and *Nature*. Rankings include only institutions that published more than 250 papers.

A closer look at publication statistics reveals some of the problems. Data produced for *Science* by the Institute for Scientific Information in Philadelphia indicate that in terms of output and impact — the average number of times chemistry papers are cited — European nations as a whole fall behind the United States (see "Country Scorecard"). Citation data for individual institutions tell a similar story: highly cited papers are produced all over Europe, but papers from these elite

European centers are cited on average less frequently than those from top U.S. institutions.

Europeans might argue that these data simply reflect the fact that American researchers tend to read and cite American journals and American papers more frequently than those from abroad. But, says Kroto, Europe does have some disadvantages. He sees the sheer number of researchers in the United States as the major stumbling block for Europe to compete. "The are more people, better off [in the United States]," he says. "Compared with the United States, our universities are not doing too well."

All this leads researchers like Stoddart and Kroto to argue that it's time to establish a European chemical society that would do for European chemists what the ACS does for their U.S. colleagues. The nearest things Europe has at present are the European Communities Chemistry Committee (ECCC) and the Federation of European Chemical Societies (FECS). The two organizations have different members and goals, however. The ECCC consist of the national societies of the community's member nations and, according to the organization's secretary, Evelyn McEwan, its main aim is to "look after the interest of chemists at the European level." FECS, on the other hand, includes non-EC countries, such as Israel and Eastern European nations, and is mainly concerned with the promotion of the science of chemistry.

For those who advocate either beefing up these bodies or creating a whole new pan-European society, the main role for such an organization would be to publish a European journal of chemistry that would rival the *Journal of the American Chemical Society*. Researchers such as Stoddart have argued for such a journal for years, but no one has yet taken the plunge, and for good reason. A European chemistry journal would have to compete not only with a plethora of small-circulation commercial and "national" chemistry journals, but also with two existing top-rated journals: *Angewandte Chemie*, published by VCH Publishers Inc. under the auspices of the German Chemical Society, and the UK Royal Society of Chemistry's *Chemical Communications*. Lehn suggest that a European journal could exist alongside *Angewandte* and *Chemical Communications*, but at least some rationalization of the smaller journals might be needed. Says Manfred Reetz, director at the Max Plack Institute for Nuclear Research in Mulheim: "Each relevant country would have to 'sacrifice' one of its own present journals."

So far, the idea of a European society and journal has not gone much past the discussion stage. And even if Europe's chemists were to organize themselves on a continental scale, true Europeanization of the discipline would require much greater central funding of research — a prospect that most top chemists view with mixed feelings. The reason: The EC can play a valuable role in supporting intra-European fellowships and helping less scientifically developed countries raise their standards, but researchers who have dealt with the Brussels bureaucracy almost invariably come away frustrated. "The inefficiency with which Brussels handles applications for grants, etc. strongly suggests some innovative thinking [is needed]," says Per Ahlberg of Gothenberg University in Sweden. To solve this problem, Lehn for example, argues for decentralized management of EC research programs.

The recent change at the top of the EC's science programs — in particular research commissioner Antonio Ruberti's efforts to reach out to scientific groups for help in running the programs — may make European chemists more favorably disposed toward Brussels. Add to that the growth of programs such as COST (European Cooperation in the Field of Scientific and Technical Research), a 22-nation collaborative research effort that now includes seven chemistry projects, and the prospects for greater European collaboration in chemistry in the next few years begin to look distinctly brighter.

David Bradley, Science 260 (18 June 1993) 1739

Busting the Fraudbusters

Bureaucracy: The curious case of two NIH sleuths

"You only have to be wrong once in this business," Walter Stewart said several years ago about his work investigating misconduct in science. Bureaucrats at the National Institutes of Health, where Stewart and his partner Dr. Ned Feder created the business of fraudbusting, claim the two sleuths did make a misstep. The pair did not unjustly accuse anyone of misconduct or exonerate the guilty. Exactly what caused NIH to order them to stop investigating plagiarism, data falsification and other scientific misconduct speaks volumes about science's continuing antagonism to anyone who questions its integrity.

Stewart, 48, and Feder, 65, have been the Holmes and Watson of science scandals. They were instrumental in uncovering the fraud in the John Darsee case and the scandal in the David Baltimore case. The two pursued their work despite fierce opposition from the scientific community at large and NIH in particular.

The official reason Stewart and Feder are out of business is that after they wrote a computer program to detect plagiarism they "moved outside [NIH's] mission" of biomedical research. Specifically, they turned their plagiarism detector on "With Malice Toward None," a biography of Abraham Lincoln by Stephen Oates. The program matched more than 100 phrases in Oates's text to those in a book by another author, say Stewart and Feder. Oates, who had been investigated by a panel of the American Historical Association on the same accusations, denies plagiarism. The AHA panel concluded that Oates's work was "derivative to a degree requiring greater acknowledgment"

(continued on next page)

of the earlier work. AHA did not accuse Oates of plagiarism. But Stewart and Feder did.

Prompted by a "Dear Paul" letter from Oates, Illinois Sen. Paul Simon wrote a letter to NIH Director Bernadine Healy disapproving of the pair's actions. Thereafter, Feder was ordered to review grant proposals and Stewart to work in another scientist's lab. But the move had been in the works much earlier. Stewart and Feder's NIH boss, L. Earl Laurence, wrote in a memo last September that he had "been assured" that Healy was aware of the plan to reassign them. Three weeks before their transfer, Stewart and Feder say the NIH approved $9,500 for new computers to use in ferreting out plagiarism. And the next week Stewart and Feder received "excellent" performance appraisals.

The peremptory transfer stands in marked contrast to the treatment of others at NIH. According to a December report commissioned by NIH, a group of male supervisors allegedly gave promotions to female employees in exchange for sex. So far, none has been transferred or put on leave based on the report. Explained one NIH official, "due process has to be done." As famed air force whistle-blower Ernest Fitzgerald wrote Simon, "Why is it that alleged molesters and sexual harassers are entitled to due process and Ned and Walter are not?"

NIH's objection to nonbiomedical research seems odd. Just last month NIH hosted a conference on plagiarism "in literature and science, including the historical" context. One moderator was a historian; one speaker, an AHA official. Another speaker explained how she's used Stewart and Feder's software to resolve two cases of alleged plagiarism.

The NIH's decision could be reversed by the new secretary of health and human services, Donna Shalala. But she hasn't. As chancellor of the University of Wisconsin when it absolved two suspects of alleged science misconduct, Shalala stonewalled official NIH investigators who questioned the conclusions of the university's internal investigation of the NIH-supported work She refused to provide a key original notebook. Federal investigators eventually closed their inquiry in frustration. The university had collected millions of dollars from a disputed patent at the heart of the case, and Stewart and Feder were unpaid expert witnesses for the plaintiffs in the patent suit. The two had reviewed all the primary data, including the key notebook. When NIH transferred Feder and Stewart, it sealed their records, including boxes of documents from the Wisconsin case.

To protest his transfer, Stewart began a hunger strike. After 33 days Arkansas Sen. David Pryor intervened and Stewart broke his fast. But NIH still hasn't budged. Stewart and Feder put misconduct on the agenda of the scientific community, prompting professional organizations to establish codes of conduct. If the NIH prevails, it will have sent a message that the diehards who said that misconduct does not merit attention will have won after all.

Robert Bell,
Newsweek, (July 26, 1993) 52

Decline of the British university system

History shows that decline can be a natural process, but that degrees, self-interest, mismanagement, indecision and incompetence can accelerate it. In Britain, the decline of the university system has probably been enhanced over the past 20 years by all these influences. In the mid-1970s, numerous cracks appeared in the system. To mention a few: it was clear that some universities were in the wrong places or located too closely together, too many had high-cost buildings, many academics were past their sell-by date, administrations were frequently antique, some colleges had too few students and their funding requirements were escalating. So no one was really surprised when the government, soon after Margaret Thatcher became Prime Minister in 1979, announced a programme of academic reform. And the process of reform has continued under the present Prime Minister, John Major.

Fourteen years on, there is now mounting concern, both inside and outside the academic community. Is the British system, famed throughout the world and envied by international competitors, still accelerating towards terminal decline? Space prohibits a review of all the problems of science, for example. But to my mind the worst mistake was not having a minister at Cabinet level responsible for science and technology for most of those 14 years. So I was especially disappointed to find the White Paper on science from William Waldegrave's Office of Science and Technology to be typical of the sort of woolly thinking that prevailed in those 14 years.

It is alarming that any policy maker should think that captains of industry and commerce know best

Most Britons consider academia a mish-mash, if they think of it all. It's a place of learning, a place of research, a place of innovation and radical thinking, isn't it? And many people probably realise that academia, just like the welfare state, is an expensive constituent of an advanced democracy and therefore requires tuning or structural change from time to keep it in step with the means and expectations of the nation.

Sadly, the reforms aimed at retuning and restructuring academia have failed. The university system is now larger, total funding has been reduced and an increased student population have had their grants reduced to Third World standards. Some of the failure must be assigned to factions in the academic camp, some to outside pressure groups, who acquiesced with the government during the period, as Roy Rothwell of the Science Policy Research Unit at the University of

Sussex and others make clear in *Technology Transfer Mechanisms* (NRDC, London, 1990). There have also been key policy mistakes, including the failure to close universities, such at those at Aston and Salford, where the physical and social fabric had in general deteriorated, and research performance had fallen well below the standards expected in Britain.

Then there was the failure to promote polytechnics to the status of universities by location and academic performance rather than at the whim of entrepreneurs, such as happened with the John Moores University in Liverpool. It lowered the status of universities *per se*, as also did the failure to target increased funding for education and research to long-established, top-tier universities and to highly specialised facilities such as the Open University at Milton Keynes. These were topics high on the conversational agenda among academics at a recent workshop of the Science and Engineering Research Council in Bristol.

The most fundamental mistake, though, has been political and relates to the relationship between industry and the universities. This can best be summarised as the concept that "British industry knows best". The very idea is flawed in every respect. To anyone who, like me, has a long experience of British industry, and in particularly of hands-on research and development, it is alarming that any policy maker should ever conceive the idea that captains of industry and commerce know best. There is absolutely no evidence to support such a concept either statistically or by case history. In fact, statistics and case histories show a completely different picture — one of decline and mismanagement in British commerce, manufacturing and service industries (see for example "Chronic joblessness", *The Economist*, 24 July, and Denis Tither's "A study of technology transfer and funding mechanisms in an industrially supported university research initiative", *Technovation*, volume 10, No 1, 1990).

It is a mystery, therefore, how such a myth, started by politicians in government and aided by others outside government, has gained momentum. The record is that Britain's universities are world class in the education stakes, and even with one hand tied behind their back they are world class in research. Though I would agree with the critics when they claim the universities have not always managed their affairs in a way which would satisfy an accountant. This is their imperfection.

By contrast, one has to be highly selective when identifying British industry with anything that is world class: their imperfections are numerous and legendary. For example, their staff training programmes are invariably inadequate, as also are their investment policies, management and short-term strategic planning, all of which *New Scientist* a adroitly summarised recently (Comment and This Week, 12 June).

The White Paper on science and technology has generated a considerable amount of hype. The cornerstone of the document is that Britain should gear its university system to the requirements of British industry. The implication is that key committees, laden with industrialists or tame industrial scientists, are able to run the research councils and consequently to control funding for academia. The overriding theme is that the responsibility for improving Britain's industrial and commercial performance will fall onto the academic community. This scenario can only be a recipe for disaster for the university system.

Over the years, universities in Britain have made great strides in bridging the gap between academia and industry and commerce. Some are now expert in the process of education and technology transfer and many have built expertise and reputations in this area which are comparable with similar institutions is any country in the world. The most successful universities have managed to maintain a delicate balance between their role in society, education, learning, research and academic activities and the requirement of industry and commerce to exploit these skills and services.

The past 14 years of attempted reform of the university system by governments, with an increasing input from industry and commerce, has been an utter failure. Confusion and indecision have led not only to a reduction in the number of staff but also to their de-skilling. Many university buildings are in a dilapidated condition, research equipment is often out of date, students have been forced into a spiral of debt and the morale of many key workers has hit an all-time low.

There can be little doubt that the university system required reform 14 years ago and still requires it today. But to anyone working in industry, there can be little doubt that these reforms should not be engineered or carried out by the captains of industry and commerce unless the requirement is for the complete decline of academia in Britain.

Waldegrave's White Paper on science and technology signals that it is now the government's intention to make academia subservient to industry and commerce. It is imperative that all who disagree and who want to maintain Britain's intellectual base make their opinions known. And they must assist those in academia who intend to fight to arrest the accelerating decline of the British university system.

Denis Tither,
New Scientist (11 September 1993) 50-51

Szakértői bírálat (peer review) a tudományos kutatásban (Válogatott tanulmányok)

címmel a közeljövőben új kötet jelenik meg az MTA Könyvtára kiadásában *Braun Tibor* és *Schubert András* szerkesztésében. A kötethez *Láng István* írt előszót; ebből idézzük a következő gondolatokat.

"Szinte minden olyan jelentésben, amelyet amerikai vagy nyugat-európai tudósok írtak a magyar tudomány jelenlegi helyzetéről, megtalálható az az ajánlás, hogy fordítsunk nagyobb figyelmet a jövőben a peer review módszer szakszerű alkalmazására, és ahol lehet és indokolt, ott a tudománymetriai módszerekkel összekapcsolva hajtsuk végre az értékelési és elbírálási munkákat.

Ezt az igényt kívánja részben kielégíteni a jelen kiadvány, amelyben a peer review módszer alkalmazásáról, sajátos problémáiról találhatunk eredeti tanulmányokat.

Kívánom, hogy hasznosítsák mindazokat a gondolatokat, amelyek ezekben a cikkekben találhatók, és amelyek valóban újak és tényleg hasznosíthatók számunkra. Nem a nulláról indulunk a szakértői bírálati módszer alkalmazásánál, de van még mit tanulnunk és elsajátítanunk olyanoktól, akik valóban magas szintű módszertani vizsgálatokat végeztek."

A kötet tartalomjegyzéke a következő:

Bár a kötetben a tanulmányok eredeti angol nyelvű facsimiléje található, az alábbiakban magyar fordításban ismertetjük néhány cikk rövid kivonatát.

Garfield, E.: *Bírálat és peer review*

A "peer review" annyira része a tudományos vizsgálódásnak, hogy gyakran már természetesnek tartják. Az évek során a szerző számos olyan tanulmányt írt, amely vagy közvetlenül, vagy közvetve a "peer review"-val volt kapcsolatban. Ezek közül némelyik a szerzőséggel és szerkesztéssel, fakultások elemzésével, a Nobel díjra érdemes tudományos tevékenység idézettségelemzés alapján történő azonosításával foglalkozott, de volt néhány olyan is, amely a bírálat különböző szempontjait választotta tárgyául. Azonban eddig sohasem esett szó részleteiben a bírálati rendszer bonyolultságáról. Mivel ez a téma központi helyet foglal el a tudományos életben, a szerző egy négyrészes tanulmányt szentelt ennek tárgyalására.

Az első két rész a publikációk bírálatával foglalkozik. Az első részben a szerző megvizsgálja, hogyan működik a bírálati rendszer, és felsorol néhány általánosan elfogadott véleményt ennek előnyéről illetve hátrányáról. A 2. rész a bírálatokkal foglalkozó tudományos tanulmányokat, és a jelen rendszerrel szemben javasolt néhány lehetőséget ismerteti. A 3. rész a pénzügyi támogatás megítélése érdekében végzett "peer review"-val foglalkozik, míg a 4. rész a pénzügyi támogatásra tett javaslat érdekében végzett bírálatra vonatkozó kutatásokat, és a javításukra tett javaslatokat foglalja össze.

Cicchetti, D. V.: *A peer review megbízhatósága kézirat és támogatás megítélésénél: Egy interdiszciplináris vizsgálat*

A szerző kritikailag újraértékelte a tudományos dokumentumok "peer review"-jának megbízhatóságát, és azokat az értékelési kritériumokat, amelyeket a kutatók peerjeik munkájának megítélésére alkalmaznak, különös tekintettel arra a következetesen alacsony megbízhatósági szintre, melyet általában közölni szoktak. A pénzügyi támogatást megítélő bírálók jobban megegyeznek abban, hogy mi "érdemtelen" a támogatásra, mint abban, hogy minek van tudományos értéke. A kéziratok beküldése esetében ez úgy tűnik, függ attól, hogy egy adott tudományág (vagy szakterület) általános és diffúz (pl. interdiszciplináris fizika, az orvostudomány általános területei, kulturális antropológia, szociálpszichológia), vagy speciális és fókuszált (pl. magfizika, speciális orvosi szakterületek, fizikai antropológia, és viselkedési neurológia). Az előzőekben nagyobb az egyetértés az elutasításban, mint az elfogadásban, de az utóbbinál a nagy eltérés a kézirat elutasítások mértékét illetően, és a nagy korreláció a bíráló ajánlásai és a szerkesztő döntései között azt sugallja, hogy a bírálók és a szerkesztők jobban egyetértenek az elfogadásban, mint az elvetésben. Számos javaslat született a "peer review" megbízhatóságának és minőségének javítására. További kutatásokra van szükség, különösen a fizikai tudományok területén.

Mitroff, I. I., Chubin, D. E.: *Peer review az NSF-nél: Dialektikus politikai elemzés*

A szerzők a "peer review"-val kapcsolatos ellentmondást dialektikusan értelmezik. Áttekintik a rendszer védelmezői és kritikusai által elfogadott érveket, melyekben a kutatásra tett javaslatokra pénzügyi támogatást nyújtó ügynökségek tanácsadóinak értékelését revideálják, különösen a *National Science Foundation* "peer review"-val kapcsolatos legújabb két tanulmányának következtetéseit. Ezek a megállapítások úgy tűnik, megérdemlik, hogy elsődleges tényezőként vegyék figyelembe a bírálók a javaslatok támogatására irányuló ajánlásaiknál. A megállapítások szintén számos kérdést vetnek fel, így pl. az "elfogadható" definiciót az érdemlegességre és a megújulásra, kapcsolatot a hiedelem, érzékelés és értékelés között, és a partikuláris tényezők szankcionált működését a bírálati rendszerben. Azt javasolják, hogy a jövőbeni tanulmányok tartalmazzanak pszihológiai változókat — különösen a kérelmezők és a bírálók "kognitív stílusának" mérését —, amennyiben az adatok túl kevés ismeretet és információt szolgáltatnak magához a vitához. Végül három olyan modellt tárgyalnak, amelyek alátámasztják a "peer review" nézeteit, és ezeket a vita alapvető társadalmi tételeihez viszonyítják.

DeNisi, A.S., Randolph, W.A., Blencoe, A.G.: *A peer értékelés lehetséges problémái*

A szerzők tanulmányozták a munkatársak reakcióját "peerjeik" értékelésével szemben, és azt találták, hogy a negatív peer bírálat visszacsatolásként szignifikánsabban kisebb teljesítményt, kohéziót, megelégedettséget, és rosszabb peer értékelést eredményez egy azt követő feladatnál. A pozitív peer bírálat ezekre a változókra jobb, de nem szignifikáns értékeket szolgáltatott egy azt követő feladat esetén.

Ruderfer, M.: *A peer review tévedése: ítélkezés tudomány nélkül, és egy esettörténet*

A "peer review"-t, azaz azt a folyamatot, amellyel megítélik a tudomány archívumait felépítő hozzájárulásokat, ez idő szerint nem tartják igazi tudományos tevékenységnek. Mégis ez a folyamat a tudomány archívumai útján kulcstényezőt képez abban, hogy az ember képes legyen megküzdeni azzal a globális problémával, amelyet a múlt tudományos fejlődésének "népességrobbanása" idézett elő. Sürgős szükség van a peer review javítására annak érdekében, hogy biztosítani tudjuk azt a technológiai növekedési sebességet, mely a hosszútávú túlélés érdekében életfontosságú. Azonban a "peer review" tudományát eddig eleve megakadályozta a primér nyers adatok, a bírálat titkossága. Ennek helyesbítésének megkezdése érdekében egy hibás elutasítás történetét részletesen ismertetjük. Az elutasított cikket, mely azt állította, hogy helyesbíti az atomos időméréssel kapcsolatosan közzétett vitát, 1979-ben közölték az *SST*-ben, egy azt követő cikk kíséretében, mely azt megerősítette és kibővítette. Az esettanulmány ahhoz a feltételezéshez vezet el, hogy a visszutasítás valószínűsége egy közölhető cikk újítási fokával növekszik. Ezt az azt követő cikk igazolta, mely kimutatta, hogy az elutasított cikknek paradigmaváltásra van szüksége ahhoz, hogy a most helytelenül a speciális relativitás elméletéhez kapcsolt "forgó óra viselkedés" széles körben elterjedt helytelen értelmezését helyesbítse. Ez az atomos időmérés forgó óra viselkedésének, a Sagnac effektusnak és a Hafele-Keating kísérletnek egyszerű egyesítését eredményezte. Ez az esettörténet, mely képes arra, hogy világosan ábrázolja az emberi tévedés eredetét egy bírálati folyamatban, egyre jobban megmutatja a publikálás szükségességét, és azt, hogy legalább annyira kihangsúlyozzuk a "peer review" hibás elutasításait, mint a hibás elfogadás hagyományos hangoztatását. Ez az egy eset is alátámasztja azt, hogy sürgősen szükség van a "peer review" pontosságának és sebességének javítására és számos speciális eszközt ajánl a cikk ennek megvalósítására.

A kötet megrendelhető az Impakt szerkesztőségi címén, ára 900.- Ft (+ÁFA).

United States National Labs: How Does Their Research Measure Up?

For some time now, the national laboratories of the United States Department of Energy (DOE) have been the subject of increasing scrutiny. Policymakers are openly questioning the necessity of funding of weapons labs — Sandia, Lawrence Livermore, and Los Alamos — at the same levels as during the 1980s, when the threat from the Soviet Union was considerable.

Those wrestling with the ever-expanding federal budget deficit are wondering how much of the billions currently spent each year on the national labs might be saved. And some politicians, worried about America's economic competitiveness are asking whether the DOE labs can shift their missions toward civilian research and work more closely with industry — in fact, in some cases to become contract research shops for industry.

There is little question that changes are coming for the national labs, but when, how much, and what type of changes are yet to be determined. In light of all this, it seems appropriate for *Science Watch* to examine how scientists themselves regard the research conducted by the DOE labs. The method used here is citation analysis, which reflects the influence that research at a given facility has had on others in the scientific community.

(continued on next page)

Science Watch surveyed the scientific papers from eight large DOE labs that were published in journals indexed by ISI from 1981 to 1992. The papers of each were divided into subfields based on the journals in which they appeared and a journal-subfield classification scheme employed by *Current Contents* an ISI publication. The labs were then ranked according to their mean citations-per-paper record in 1981-92 (papers published during 1981-92 and cited over the same period) and in the most recent five year period, 1988-92 (papers published during 1988-92 and cited during the same period). To be ranked in a subfield, a lab had to have produced at least 100 papers in a given period; an exception was made for Lawrence Berkeley Laboratory in analytical, inorganic, and nuclear chemistry in 1988-92, when it produced 90 papers.

For each subfield, and for each period surveyed, the average citation impact scores for all U.S. papers are indicated at the top of each ranking.

The results show that the research impact of these large DOE labs, as measured by citation per paper, generally exceeds the U.S. average. In fact, there are signs of improvement: More of the labs surpassed the U.S average in 1988-92 than they did in 1981-92.

Different labs clearly have different areas of strength and weakness. As for strengths, Brookhaven ranked first in general physics; Argonne topped the list in applied physics; Ames placed first in analytical, inorganic, and nuclear chemistry; Berkeley bested all others in materials science; and Sandia took top honors in nuclear engineering, for both periods.

As for weaknesses, Oak Ridge was last in physical chemistry and in biochemistry/biophysics for both periods, and it fell from sixth to last in applied physics, comparing 1981-92 with 1988-92; Brookhaven ranked at the bottom or near to it in physical chemistry and materials science during both periods; Livermore placed last or next to last in applied physics; and Los Alamos was last in analytical, inorganic, and nuclear chemistry during both periods.

General Physics

Laboratory	Cites/Paper	No. Papers
1988-92 (U.S. average = 5.80)		
Brookhaven	9.44	759
Argonne	7.51	767
Berkeley	7.43	1,015
Ames	7.36	229
Livermore	5.95	977
Oak Ridge	5.81	868
Los Alamos	5.60	1,680
Sandia	5.28	221
1981-92 (U.S. average = 11.94)		
Brookhaven	17.37	1,951
Berkeley	16.42	2,410
Argonne	13.39	1,728
Sandia	12.73	529
Los Alamos	12.47	3,947
Oak Ridge	11.78	1,976
Livermore	11.69	1,936
Ames	9.78	550

Applied Physics/Condensed Matter Physics

1988-92 (U.S. average = 4.31)		
Argonne	6.26	1,474
Brookhaven	6.23	1,006
Ames	5.85	597
Sandia	4.99	1,434
Berkeley	4.91	1,372
Los Alamos	4.79	1,813
Livermore	3.71	1,108
Oak Ridge	3.70	1,448
1981-92 (U.S. average = 7.83)		
Argonne	9.09	2,988
Berkeley	8.94	2,871
Brookhaven	8.80	2,220
Sandia	8.55	3,017
Ames	8.54	1,205
Oak Ridge	6.84	3,156
Los Alamos	6.60	3,579
Livermore	5.66	2,145

Physical Chemistry/Chemical Physics

1988-92 (U.S. average = 4.60)		
Sandia	6.63	380
Berkeley	6.57	637
Argonne	6.36	549
Ames	5.26	242
Los Alamos	4.76	338
Livermore	4.61	152
Brookhaven	4.60	323
Oak Ridge	3.51	301
1981-92 (U.S. average = 9.44)		
Berkeley	15.58	1,655
Los Alamos	12.75	965
Sandia	12.09	941
Livermore	10.82	329
Ames	10.46	512
Argonne	10.42	1,331
Brookhaven	9.58	796
Oak Ridge	8.93	782

Analytical, Inorganic, and Nuclear Chemistry

1988-92 (U.S. average = 3.90)		
Ames	5.62	246
Oak Ridge	5.41	328
Argonne	4.76	164
Brookhaven	4.10	105
Berkeley	3.58	90
Los Alamos	2.26	155
1981-92 (U.S. average = 8.60)		
Ames	11.42	563
Argonne	9.50	346
Berkeley	9.06	256
Brookhaven	8.26	307
Oak Ridge	7.09	659
Los Alamos	5.81	353

Source: Science Watch/Institute for Scientific Information

Materials Science

Laboratory	Cites/Paper	No. Papers
1988-92 (U.S. average = 2.20)		
Berkeley	3.69	352
Oak Ridge	3.47	399
Sandia	3.16	350
Los Alamos	2.99	218
Ames	2.99	116
Argonne	2.72	247
Livermore	2.56	186
Brookhaven	1.94	158
1981-92 (U.S. average = 3.85)		
Berkeley	5.80	842
Oak Ridge	5.08	789
Sandia	4.81	1013
Ames	4.27	211
Los Alamos	3.85	470
Livermore	3.56	404
Brookhaven	3.40	401
Argonne	3.36	647

Nuclear Engineering

1988-92 (U.S. average = 1.85)		
Sandia	3.64	332
Berkeley	2.63	351
Brookhaven	2.61	324
Argonne	1.96	451
Los Alamos	1.93	546
Livermore	1.83	321
Oak Ridge	1.62	582
1981-92 (U.S. average = 2.81)		
Sandia	5.27	889
Berkeley	4.15	865
Brookhaven	3.33	1,021
Oak Ridge	3.17	1,830
Argonne	3.09	1,645
Los Alamos	2.73	1,581
Livermore	2.50	797

Earth Sciences

1988-92 (U.S. average = 3.62)		
Sandia	4.79	101
Livermore	4.30	214
Berkeley	4.30	128
Los Alamos	3.91	307
1981-92 (U.S. average = 7.93)		
Berkeley	9.36	291
Los Alamos	9.28	676
Livermore	6.64	396
Sandia	6.17	224

Experimental Biology/Medicine

1988-92 (U.S. average = 3.38)		
Berkeley	4.78	200
Livermore	4.36	100
Los Alamos	3.70	136
Argonne	3.68	130
Oak Ridge	2.96	176
Brookhaven	2.92	111
1981-92 (U.S. average = 7.22)		
Oak Ridge	7.42	706
Brookhaven	7.39	404
Berkeley	7.29	506
Livermore	7.18	254
Argonne	6.75	386
Los Alamos	5.45	355

Biochemistry/Biophysics

1988-92 (U.S. average = 6.83)		
Los Alamos	6.91	129
Berkeley	5.60	220
Brookhaven	5.32	140
Oak Ridge	4.44	157
1981-92 (U.S. average = 13.94)		
Berkeley	14.75	483
Brookhaven	13.17	423
Los Alamos	13.09	303
Oak Ridge	11.21	359

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Science Publishing is Urgently in Need of Major Reform

The function of science publishing today is to get information about new findings in science to at least three different communities:

◆ *Group A*, the specialists working in the same field as that in which the findings were made (numbering anywhere from 10 to 10,000 scientists);

◆ *Group B*, the general community of scientists and engineers who, although not in that field, are nevertheless interested in major advances in scientific areas other than their own — advances that may, indirectly or in the long term, be significant to them (probably between 1,000 and 10,000);

◆ *Group C*, the general, attentive public and the policymakers who want or need to know of scientific developments that could have economic or social consequences (10,000 to 100,000).

The classic media for science publishing — journals put out by societies and other discipline-oriented organizations — were designed only for Group A, while the media serving Group B are the short news articles in society house organs, such as *Chemical Engineering News*, *Physics Today*, and *MRS Bulletin*, and in such multidisciplinary publications as *Science* and *Nature*.

Group C is served (very poorly, in my opinion) by the general media — including newspapers and magazines such as the *New York Times*, *Omni* and *Discover* — which tend to oversimplify and inappropriately dramatize the "breakthroughs" they consider newsworthy. Since maximum publicity — not accuracy — today serves both the scientist and the journalist, these and other such publications, in my opinion, tend increasingly to fall for exaggeration and hype. Meanwhile, such publications as *New Scientist* and *The Scientist* seek to bridge with both accuracy and social relevance the B and C groups.

Confronted by such a menu of publishing alternatives, how does the responsible scientist announce effectively a discovery that she or he thinks would be of interest to one or all of the A, B, and C groups?

The establishment position

The route accepted by the establishment is that one should submit one's research report to a standard journal (the A group), have it go through the arcane ritual called "peer review", and — only after it has been accepted and published — send publicity releases or other notification of its findings to the wider press.

The process is deficient, in my opinion, first of all because peer review — the first hurdle — is biased against innovation. In the path toward publication in the A group journals, anything that extends beyond the current ruling paradigms of scientific thought will most likely experience inordinate delays in being considered; matters of routine science will get into print much faster. (There is an accompanying danger: that during the peer review process, especially when an article is dealing with a dramatic discovery in a fast-breaking field and has potentially profitable application, two or three scientists — the so-called peers — and, hence, possibly their laboratories, companies, and colleagues may inappropriately be privy to advance information on a significant innovation that they can use to their own professional gain.)

I strongly believe that when research results are really important, they deserve a wider academic audience than the specialized readers of the A-type journals and that, in line with this, the significant scientific innovations of today need new methods of publication.

Beating the system

A good example of how a creative scientist can circumvent the current publishing system in order to get his or her significant work out to the public is the case of Henry Heimlich, president of the Heimlich Institute in Cincinnati. Although Heimlich does not fit the traditional profile of the basic lab researcher working toward an esoteric advance, his approach is worthy of emulation: When he discovered his now-famous life-saving manoeuvre for discharging obstructive material from the windpipe, he wanted to get news of it without delay to the public who could use it. After agreeing with the editor of *Emergency Medicine* (a non-peer reviewed journal) to simultaneously send out a story to the general press, he published it in the June 1974 edition of that publication (6:154-5, 1974). Newspapers all over the United States picked up and ran the news release sent to them. Although the *Journal of the American Medical Association* made note of Heimlich's technique in a news blurb a few months later, the technical article in *JAMA* did not appear until October of the following year (234:398-401, 1975). By this time, of course, the Heimlich manoeuvre was already saving lives all over the world.

When research results are really important, they deserve a wide audience; innovation of today need new methods of publication.

For the bench scientist, the effort to penetrate or circumvent the peer review barrier in a way that Heimlich did can be perilous indeed to the prospect of ever seeing a research report in print. I know this from several personal, time-wasting experiences with leading science journals. For example, in October 1992, having made a scientific advance in diamond synthesis, I sent off a paper to *Science*, then held a press conference in Washington discussing my findings. At the press conference, I handed out copies of the paper, and I also distributed some copies by fax. After a month, *Science* returned the paper — unreviewed, yet rejected — on the procedural ground that preprints had been distributed, which, in *Science*'s terms, constituted publication.

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An obsolete process

For the A group, the classic model of writing a paper, submitting it to an editor, and having it go through the arcane peer review system described above may once have been suitable for science done by an elitist minority (known to each other) with relatively high ethical standards. But the A community itself is no longer served by this process because of several factors — and the process is clearly inadequate to address the needs of groups B and C. Among the factors are:

◆ The volume of the current literature is so large that peers and editors have greater difficulty in determining whether work reported is really significant or has been done before. (Indeed, I belive that significant numbers of working academic scientists essentially ignore the literature as they go about their investigations.)

◆ The peer review process now practiced in most journals is biased against real innovation, in part because of problems associated with selection of knowledgeable and objective reviewers. Even the key word in the term "peer review" is difficult to define: Who, for example, is a "peer" of Linus Pauling? Any assistant professor of chemistry at a university? His arch-enemies in the anti-vitamin C camp? Yet although no clear definition of "peer" has as yet been created, the term continues to be used by the science publishing industry as its needs require. Most devotees of the process forget that the peer review system cannot be any guarantor of correct or good science; all its practitioners really can do is check the plausibility of the case and determine whether the author got the sums right.

◆ The current process in a peer reviewed journal also gives an unfair advantage of from three to six months in a given field to two or three groups or individuals who are chosen to review an article. This may be no problem for pedestrian science, but it is unacceptable in many cases, especially those of interest to groups B and C.

Devotees of peer review commonly assert that there are no alternatives to the system. For research proposals, however, there are dozens of alternatives in use all over the world. At the U.S Department of Defense, for example, a knowledgeable R&D manager seeks out the very best researchers and takes the responsibility for judging the best proposals with no authority at all being given to peers. Every mid-level or higher manager in industry does the same every day with no help from peers. For publishing papers, the science community, in theory devoted to "experimentation" often appears unwilling to try new approaches for the new situations, although there have been exceptions. In 1965, I participated in the founding of the *Journal of Materials Research Bulletin*. This journal, which has a good impact factor in citations, runs open review either by associated editors or by anonymous reviewers. Until recently, the *Proceedings of the National Academy of Sciences* did not mandate peer review. Quality presumably was maintained by examining an author's previous track record. However, this is no longer true for *PNAS*.

Recommendations

I believe that new journals should be started (or new sections started in established journals) for non-reviewed papers by authors who establish their credentials by their track record. In this process, a scientist who submits a list of published works with a minimum of, say, 50 or 100 papers in the regular peer reviewed journals could publish a paper with only a brief delay in the process for copy editing. Or any scientist with the same or an ever better track record could officially communicate a colleague's paper.

This scheme would achieve several goals at once. First, it would open up the system to genuinely new ideas. Second, it would speed up the process by several months. Third, it would bring some lively debate into science by having authors use the media Group B to discuss or critique the work. Fourth, it would protect the new work from some of the dangers inherent in the current system — such as the prospect of the paper's never being published.

Reporters should be sceptical of claims that a paper makes concerning applications of economic impact.

In the case of a paper reporting on what the author considers an especially important research advance that would be of interest to audiences B and C, as well as A, standard practice should entail the author's first sending the manuscript to a journal, then distributing preprints by mail and fax, or calling a press conference or doing whatever else is necessary to get the word out.

Meanwhile, reporters covering such an announcement should examine carefully the claims and explicitly report the author's previous track record in the field. This is by far the best predictor of reliability. And the reporters should seek comment on the record — in radical contrast to the anonymity of peer review — from other knowledgeable colleagues on the available preprint version. The reporters should be sceptical of any claims the paper makes concerning applications of technological or economic impact, preferably ignoring them.

Rustum Roy, The Scientist (September 6, 1993) p11, p22

I don't mind your thinking slowly:
I mind your publishing faster than you think.

Wolfgang Pauli (1900-1958)

First get your facts;
and then you can distort them at your leisure.

Mark Twain (1835-1910)

Készült az MTAK házi sokszorosító részlegében

Felelős kiadó: az MTAK főigazgatója