3. évfolyam • 1. szám • 1993. január

IMPAKT

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:

ISSN 1215-3702

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A peer-review problémáiról

Olvasóink talán még emlékeznek folyóiratunk 1991 májusában publikált próbaszámának "ajánlásában" közöltekre, amelyek szerint a tényekről főként tudománymetriai szemlélet alapján szeretnénk tájékoztatni, de foglalkozni szeretnénk a peer review kérdéseivel is. Bár az alábbi cikk a peer review problémáit egy kémiai folyóirat ill. téma kapcsán taglalja, úgy véljük, hogy a tárgyalt kérdések és vonatkozások egyaránt érvényesek a természettudományok bármelyik területére.

The following is an account what happened when two researchers impudently tried to correct errors in articles published in the leading US chemical journal.

It has recently been said of the US scientific establishment: "As the number of scientists reaches 1 million and their share of the nation's federal budget reaches $25 billion, the demands for greater accountability and openness are understandably more insistent" [1]. This increasing need for openness prompts us to report our attempts to disclose errors in the leading journal of the American Chemical Society. We show that the peer-review system broke down when papers with egregious errors were accepted for publication. Moreover, we demonstrate that the self-rectifying mechanism for maintaining the integrity of science initially failed when the editors refused to publish corrections in the journal in which the errors originated. Our experiences are revealed here because they might represent problems that have become ingrained within the chemistry establishment. Solution to the problem are possible only if the scientific community is made aware of what can happen.

Events began with the publication of two articles [2,3] in the *Journal of the American Chemical Society* (henceforth *JACS*) by Professor Ronald Breslow of Columbia University. E. Anslyn was the coauthor of one paper [2] and D. Huang was the coauthor of the other [3]. Both papers dealt with the kinetics of cleavage and isomerization of dinucleotides (reactions important as ribonuclease models). To document the course of events after the appearance of these papers, we will use quotation from letters, reports and articles taken from our 'Breslow file', now several inches thick.

It was immediately apparent to us (working independently and unaware of each other's efforts) that both papers were beset with problems. Rate-constant units were garbled; experimental error was very large; pH and ionic strength had not been properly controlled; straight lines were drawn through graphs having only two experimental points although equations predicted non-linear plots; and rate constants, invoked to fit the data, never had their physical significance explained. Most puzzling of all, however, was the reporting of negative rate constants. Even the title of Breslow and Huang [3] stressed this aspect of the work: "A Negative Catalytic Term Requires a Common Intermediate in the Imidazole Buffer Catalyzed Cleavage and Rearrangement of Ribonucleotides". Usually, of course, rate constants are positive numbers.

(continued on next page)

On 8 March 1991, one of us (F.M.M.) decided to alert the readers of *JACS* to these difficulties, and submitted a short note to the senior editor, Professor Allen J. Bard. Bard asked one of his associate editors, Professor Richard L. Schowen, to handle the manuscript. Two weeks later, the manuscript was returned without the benefit of reviews by independent referees. Schowen wrote: "I don't think this paper is any good. Breslow and Huang never said they measured a negative rate". It was difficult to understand the reply as Breslow and Huang had published an entire list of negative rate constants.

Independently, A.H. submitted a critique of Breslow's papers to Bard on 19 March 1991, pointing out that Breslow's proposed mechanism was incompatible with his own kinetic measurements. Moreover, if one inserted the Anslyn and Breslow [2] rate constants into their published equations, many of the published plots could not be duplicated. Bard also assigned this manuscript to Schowen, who returned it without seeking referees' advise. Schowen wrote: It may well be that there are flaws in the Breslow papers. In order to publish criticisms of the papers in *JACS*, however, it would be necessary to present material which is itself a contribution to the understanding of these systems as opposed to merely correcting, or as it seems to me that you are doing, taking issue with Breslow's interpretations."

Following the rejection of the paper, F.M.M. felt obliged to contact Bard to voice his unhappiness over Schowen's decision. In the meantime, A.H. wrote to Schowen (and sent a copy to Bard) on 24 April 1991: "The peer review system has broken down and you are not correcting the problem. How is science to correct itself if *JACS* does not allow the publication of articles that point out errors?" Bard decided that both manuscripts merited formal reviews by independent referees and asked Schowen to proceed accordingly. Thus, on 28 April 1991, F.M.M. submitted a manuscript entitled: "The Negative Rate Constants of Breslow and Huang". A.H.'s paper, submitted on 7 May, 1991, was entitled "Mechanisms, Kinetic Models, Functional Dependencies, and Negative Catalytic Terms in Imidazole Catalyzed Cleavage and Isomerization Reactions of Dinucleotides."

We must now backtrack slightly. Before submitting his paper, A.H. had written to Breslow about the problems he discovered in Anslyn and Breslow [2]. Breslow passed this letter to Anslyn, who by then was at the University of Texas. On 8 April 1991, Anslyn wrote Breslow a letter dealing with these questions (a letter that was passed on *in toto* to A.H.). From the standpoint of F.M.M.'s paper, the most interesting sentence of Anslyn's letter was the following: "The fact that 7B has a negative rate constant intercept is an artifact of our data correction process." Thus Breslow's own collaborator had alerted him, in writing, to the fact that the negative rate constants were suspect.

Both our manuscripts were rejected by Schowen in July 1991. The referees' report turned out to be a strange mixture. On the one hand, they were critical of the Breslow's papers as the following quotes show:

> There are undoubtedly problems with Breslow and Huang. The reactions are very slow and cannot be followed directly. Most of the so-called buffer runs are not buffered, ionic strength was not kept constant; the extrapolations described are optimistic; and the presentation is vigorous but simplistic. Many people – including one of my research students – have pointed out these shortcomings.
>
> The rate constants given by Anslyn and Breslow do not reproduce the lines given in their paper for isomerization.

On the other hand, both referees ultimately concluded that our papers should be rejected.

> One must recommend against publication although it is to be hoped that Huang and Breslow will publish a full paper which gives a correct and intelligible account of what was done.

F.M.M. appealed to Bard to overrule Schowen's decision. On 2 August 1991, Bard wrote to F.M.M.: "Based on my reading of this material I feel that Professor Schowen's decision not to publish your paper was the correct one." He continued: "I am less familiar with the representation of rate constants as negative numbers, but I am not sure this is improper." Thus F.M.M. had apparently been unable to persuade the editors that the negative rate constants, on which Breslow and Huang [3] was based, were not simply the result of a scholarly formalism, a semantic flourish or graphs with negative slopes. Instead, negative rate constants arose by improperly adjusting raw experimental data for a large background rate.

F.M.M. wrote to Bard by return, stating "I also recognize that errors are part of science and an inevitable component of scientific articles. One cannot turn journals into a debating ground for minor mistakes and oversights. If the Breslow papers had contained only minor peripheral errors, I would not have written a critique. But the papers were so ladened with incorrect equations, statements, and conclusions that I (along with Dr. Haim) felt a duty to respond. In my opinion, the Journal was not well served by allowing this bad science to remain uncorrected within its covers."

Following rejection of his paper, A.H. sent a fax message to Schowen (sending a copy to Bard) pointing out the American Chemical Society ethical guideline: "If an editor is presented with convincing evidence that the main substance or conclusions of a report published in an editor's journal is erroneous, the editor should facilitate publication of an appropriate report pointing out the error and, if possible, correcting it. The report may be written by the person who discovered the error, or by an original author." A.H. then wrote to Schowen (copying the letter to Bard), requesting publication of the manuscript (or a suitable revised version) on the basis of the ACS ethical guidelines. Bard rejected the request on 2 August 1991, to which A.H. responded on 11 August 1991:

> It is disturbing to reflect upon the fact that the editors had an opportunity to rectify this unsatisfactory situation by informing the scientific community of the grave errors in Breslow's papers but refused to do so. Indeed, this whole affair represents a dark chapter in the annals of Science.

On 18 September 1991, Breslow unexpectedly wrote to A.H. stating: "I don't understand what you are so excited about". "Are you being led astray by a notoriously unstable individual?" There was no doubt to whom Breslow was referring.

At this point, we had both failed in our effort to correct errors in the literature. Unwilling to let the matter drop, A.H. submitted his paper to the *Journal of Physical Chemistry*. A lone referee wrote the following sentences (abstracted from a long report):

> To this reviewer the greatest mistake that Breslow and co-workers did was to subtract out the non-buffer catalyzed reaction before attempting to fit their data to a particular mechanistic scheme. This led to them reporting ridiculous negative rate constants in the Breslow and Huang paper.
>
> Haim is right that there are serious problems with the Breslow papers, but I am not sure his contribution is going to help much. This reviewer is not convinced that the general outlines of the proposed mechanism are wrong; it is just that Breslow and co-workers have not proven (or disproven) it.

A.H.'s paper was rejected.

In the meantime, F.M.M. had sent his manuscript, still entitled "The Negative Rate Constants of Breslow and Huang", to Professor Clayton Heathcock, senior editor of the *Journal of Organic Chemistry* (*JOC*). Heathcock asked one of his associate editors, Professor Andrew Streitwieser, to handle the manuscript. On 16 August 1991, Streitwieser accepted it. The report from the sole referee is quoted in its entity:

> I recommended the publication by Fred Menger, and I am very surprised that *JACS* would not accept the contents of this Communication in some form. On first reading the Breslow, Huang manuscript I was dumbfounded by the three figures, each with a linear plot of two points, and the negative rate constants. Clearly, there is no way that the rate expression provided by Breslow could provide negative rate constants. I sat down with [name withheld] and he agreed with me that a horrible mistake had been made. Subsequently, others in other portions of the country, who need not be named, agreed that there was something terribly wrong with this study. I myself could not understand how the Communication could have gotten through the referee process and have passed an editor. I believe Fred Menger in putting together this Communication has done what had to be done.

F.M.M. could not resist sending both Schowen and Bard a copy of this report. Their reactions were quite different but equally perplexing. Bard wrote: "I really am pleased that your Communication on the Breslow/Huang paper was accepted by *JOC*." Schowen wrote: "Nothing you sent *JACS* enumerated aerated any real errors, horrible or otherwise."

F.M.M. received many letters after publication of the *JOC* paper [4], including an unsolicited letter written to Bard by a Nobel laureate:

> It has come to my attention that the enclosed comment, published by the *Journal of Organic Chemistry*, was turned down by the *Journal of the American Chemical Society*. This I find extremely disturbing. It seems, to me there is a long-established scientific etiquette which says that papers pointing out errors should be published in the same journal in which the original paper appeared.

It is now necessary to address the issue of "footnote 6" in the *JOC* article. The footnote read:

> Dr Albert Haim of Stony Brook simultaneously and independently uncovered a variety of other problems with the Breslow manuscripts (e.g. the reported rate constants and equations do not fit the theoretical plots). His analysis will be published elsewhere. Neither of us was permitted to publish our work in the journal where the errors originated.

On 1 November 1991, F.M.M. received a letter from Heathcock stating that an erratum would be published in the earliest possible issue of *JOC* to state that the above passage was "inserted during the galley proofs and did not appear in the version of the manuscript that was approved by the editor; the editor would not have permitted publication of the manuscript with this passage" [5].

F.M.M. had indeed altered footnote 6 at the proof stage. The original, approved manuscript had read:

> Dr. Albert Haim of Stony Brook simultaneously and independently uncovered a variety of problems with the Breslow manuscripts. His analysis will be published elsewhere. Attempts to document these errors in the journal from which they originated were unsuccessful.

Admittedly, the recasting of the sentences in proof rendered them more acerbic. Although footnote 6 is a correct statement, and can be proved to be so, and although the footnote was a modification and not an "insertion", the change in proof was an error of judgement. Any modification should have received prior approval from the editors. F.M.M. sent an apology to Heathcock and, via this account, extends it publicly.

On 2 December 1991, F.M.M. received another letter from Heathcock with a shocking paragraph: "We have received requests that your paper be retracted. One of these comes, of course, from Breslow, but we have obtained a totally independent request as well". There was, of course, no indication as to the identity of the second critic, a point to which we shall return later. Heathcock ended: "These are significant charges that require responses of your part. Are these statements correct? Can you indicate to me why your paper should not be retracted?"

F.M.M. responded immediately to the critic's statements, and on 27 December 1991, Streitwieser wrote to F.M.M. "I consider you responses to be satisfactory and no retraction will be made". He continued, "you may be interested to know that I went through a careful kinetic analysis myself of Breslow's mechanism. The problem with having k_w (i.e. background) terms in both the numerator and the denominator is that a dissection into a water part and a buffer part is no longer possible". "This result has been communicated to Professor Breslow".

It is now necessary to take a short detour. Another researcher, unknown personally to either of us, exchanged correspondence with Breslow on another matter. Breslow wrote a letter dated 8 January 1992 in which he states: "A *JACS* editor, who is an expert kineticist has written demanding that the Menger paper be retracted because it is fraudulent". This remark provides telling information as to the identity of the person behind the "significant charges" sent to Heathcock at *JOC*. It is unclear how Breslow learned of the editor's action or whether the editor was aware that Breslow was using knowledge of the retraction demand for his own purposes.

Again we must backtrack. On 10 September 1991, F.M.M. wrote to Professor Ernest Eliel, president of the American Chemical Society, explaining the problems with *JACS*. Three days later, Eliel referred the complaint to Professor Jeanine Shreeve, chairman of the ACS committee on publications. F.M.M. then sent Shreeve all the relevant papers as they appeared, so that she had in her possession the main part of our Breslow file.

By 8 March 1992, F.M.M. had not heard from Eliel or Shreeve, so he wrote to Eliel inquiring as to the status of the ACS review. Eliel replied, in a letter dated 20 March 1992, that Shreeve was no longer chairman of the publications committee. The file had never been transferred to her successor. All attempts to contact Shreeve, including a certified letter, failed to elicit a response. In his letter, Eliel said "I continue to think that the cause of science has been adequately served." "I feel ACS's hands and my own are clean".

In a letter to A.H. dated 23 October 1991, Schowen had expressed the opinion that Breslow's papers "represent a very good contribution to defining the baseline behaviour of dinucleotides". In another letter dated 13 November 1991, Schowen wrote to A.H.: "My opinion of your criticism is that it is inappropriate". Thus, when A.H. submitted a new version of his paper to Bard on 18 November 1991, he requested that an editor other than Schowen deal with the manuscript. Bard refused, but he did ask A.H. for a list of ten suitable chemists from which Schowen could select referees. On 15 January 1992, Schowen wrote to A.H., saying that he had obtained four reviews. One of these, from Breslow himself, was negative:

> If Haim tries to publish it elsewhere, in spite of its flaws, he is either unable to grasp simple arguments or dishonest enough to publish what he knows is wrong. He should not imitate another fraud perpetrator in this.

Another referee wrote the following:

> It is deplorable that Breslow's papers on these hydrolyses were ever published. It is immediately obvious that Anslyn and Breslow is sloppy: the ambiguity over the state of protonation, the use of "buffers" of zero or infinite buffer ratio, the failure to control changes in pH and ionic strength (which probably would not even compensate a specific ion effect at 2M buffer), lines in Figs 1B and 2B (which duplicate 1A and 2A and should not have been included) that erroneously miss the origin, confusion of rates and rate constants. How could such manuscript pass the referees?

Despite the above, the referee did not consider Haim's paper a sufficient advance in understanding to justify publication. "The proper remedy is to do the experiments correctly".

A third referee voiced the opinion that the matter was best dropped. He or she wrote:

> It could lead to a reputation, rightly or wrongly, of the author being a nitpicker and Breslow would certainly fight back loudly. Who needs such things?

The final referee, an authority in modern bio-organic chemistry, sent a signed review stating that "I do not believe that it is proper to delay any longer and recommend that Haim's paper be published in *JACS*". It had been 11 months since A.H. first submitted a manuscript for publication.

Schowen did not take the advice of the fourth referee, but suggested A.H. revise the manuscript without any commitment to publication. On 15 March 1992, A.H. submitted a revised manuscript, and in his cover letter he asked: "Will you, as one of the gatekeepers of Science, knowingly perpetuate the fatal flaws in Breslow's papers within the pages of *JACS*, or instead exert your responsibility and duty as associate editor and help my extraordinary efforts to expose major flaws in Breslow's work?"

On 12 May 1992, Schowen rejected A.H.'s manuscript. Schowen's letter of rejection and the accompanying referee's report are too long to quote in their entirely. It is fair to state, however, that no referee ever defended Breslow's work as being correct. There were, however, differences of opinions as to how deal with the problem. For example, one referee recommended publication:

> I can sympathize with Haim's attitude that *JACS* is the appropriate medium to call attention to Breslow's errors which are truly gross.

Another referee did not:

> The present paper would simply call attention to Breslow and his erroneous details rather than deal with the old, and clear, literature which is to be read and enjoyed.

In his letter of rejection to A.H. Schowen wrote: "I don't think anyone has ever suggested that a paper pointing out serious errors in another publication should not be published. As far as I know, everyone thinks such a paper should be published". Yet Schowen's own referee had called Breslow's errors "truly gross". A.H. again appealed to Bard, and on 12 June Bard accepted the manuscript for publication as a full paper in *JACS* (ref. 6).

As the dust settles, it is comforting to reflect that the system ultimately worked. After all, both of us succeeded in getting our papers published. Yet this was accomplished only at the cost of considerable anguish to both of us. Few people, we presume, would be willing to go through this experience. Certainly, had either of us known at the beginning that the 'Breslow file' was to become inches thick, filled with frustration and insult, we would have been reluctant to pursue publication in the first place.

Two problems are involved. The first is the mishandling of the original publications, which many people have come to regard as substandard. The second is the position taken by the associate editor after flaws were pointed out. The position can be described only as defensive and evasive. Without attributing motivation for his actions, we simply state that we believe them to have been inimical to the best interests of science.

Editors, perhaps more than anyone, suffer from an almost unmanageable flood of literature. The burden is passed on to referees who, at times, are too busy to read manuscripts carefully. In this manner, errors creep into journals. Certainly one of the best protections is for editors to permit scholarly rebuttals when a published article is read carefully and when serious errors are thereby uncovered. The very knowledge that such a policy is in force will lead to the best policing of all – from the authors themselves.

F.M. Menger and A. Haim
Nature, 359(1992) 666-668

[1] Hilts, P.J.: *The New Republic*, 24 (18 may 1991)
[2] Anslyn, E., Breslow, R.: *J. Am. Chem. Soc.*, 111, 4473-4482 (1989)
[3] Breslow, R., Huang, D.: *J. Am. Chem. Soc.*, 112, 9621-98623 (1990)
[4] Menger, F.M.: *J. Org. Chem.*, 56, 6251-6252 (1991)
[5] *J. Org. Chem.*, 56, 6960 (1992)
[6] Haim, A.: *J. Am. Chem. Soc.* (in the press).

Going Deutsch

It beats me, a friend said recently, how on earth the Germans can be so good at science and yet speak so impossible a language. I countered that it was precisely that: the German language is so highly structured that, on one hand, it is a finely tuned instrument where everything fits into place, and on the other, one of infinite flexibility. No other living language is so mathematically precise. Of course German is the ideal language for science.

What astonished my friend was that the Germans should speak such an impossibly contorted language that they should never be able to make their science known to other groups in the scientific community. They would remain isolated in their logic.

To an extent I agree, for, while most scientists can manage with a reading knowledge of French, and even Russian extracts can be handled with a limited vocabulary, once the initial scarecrow barrier of the alphabet has been crossed, German appears so convoluted to the normal brain that many scientists refuse to make the effort to deciphering it and rely on translated extracts to provide them with the essentials.

German remain the second foreign language in most British schools, and its position is not much better, thanks to the supremacy of English, in most other countries in Europe. The notable exceptions are those regions where German is one of the national languages, such as the South Tyrol on northeastern Italy, and Switzerland, where German is a compulsory part of the national curriculum, and English often an optional extra.

However, with the new united Germany swelling the ranks of the native German speakers in the European Community, there is much to be said for a change in attitude towards the language that once was spoken in many of the courts of Europe.

Such a change would be most welcome, in particular among those working for the Community. Currently the most favoured languages in the corridors of Brussels and Strasbourg are English and French, which says to a lot about attitude among Eurocrats who are purportedly open to a multilingual, multicultural society.

None of the larger international organisations has German as an official language. But, while this is permissible on a worldwide scale, there being other language groups with much greater clout, this argument simply cannot hold water in Europe. In the whole of "Europe", as opposed to the European Community, more than 90 million people claim German as their native tongue. This figure does not include the many other minority groups of Eastern Europe, not the bilingual populations in the border areas of the eight peripheral countries.

As Germany regains its former importance in the Western world, German will be of growing importance to the science of tomorrow's world. But how neglected it is today, despite the twinning of towns throughout Europe, an inevitable *rapprochement* within the Community, and the worldwide promotion of German by the Goethe Institute.

Britain in particular neglects language study at school. How many countries allow their school children aiming at higher education to abandon all study of foreign language at the age of 16, sometimes even earlier?

It is true that in Britain an increasing number of universities and polytechnics are now offering combined courses in business of science with a language. And language courses are mercifully introducing other modern aspects to traditional language teaching, acknowledging that it is all very well to be able to quote from *Le Roman de Renard* (France's Chaucer), but it will not get you more *baguette* in the real world. But these causes are still few and far between.

What is finally comes down to is that language is communication with the outside world, no matter whether you are a journalist, teacher or scientist. Living should be a sharing experience and science a coordination of minds, a competing of ambitions.

Had history turned out a little differently, had Britain been less of a colonial success, and had we vote to decide the official national language of the newly formed United States not swung in favour of English (just pipping German at the post, winning by one vote in the Continental Congress convened in Philadelphia during the American Revolution), the current complacency among English-speakers may be somewhat less in evidence.

If Britain is to play a decisive part in the scientific Europe of tomorrow perhaps it is time to come out from behind its own Iron Curtain and assume responsibility towards its scientists. And it is not cuts in funding that enable us to do that.

Not only have we been depriving our science students of the joys of international communication but, through this, also limiting our future role in scientific Europe.

M. Ecott,
New Scientist, May 23(1992) 45-46

Trends in research productivity among senior faculty

Data from the vitae of 411 senior faculty at Syracuse University were analyzed to uncover trends in productivity over time. Results show that productivity is related to status and academic discipline. Overall, productivity earlier in a career is a good indicator of later productivity. Not surprisingly, full professors increase productivity to a greater extent than do assistant and associate professors. Increase in productivity among females is greater than among males, but males are more productive overall. Humanities and science/mathematics faculty increase productivity to greater extent than social sciences and professional school faculty.

Average number of various publication types per year among three professional ranks

Average Publications	A	B	C	D
Journal articles	1.10	.66	.39	.96
Conference proceedings	.18	.17	.10	.18
Book chapters	.27	.21	.07	.25
Books	.09	.05	.01	.08
Other	.20	.15	.03	.18
Total	1.84	1.25	.59	1.65

A: Full Professor (n = 289) B: Associate Professor (n = 11)
C: Assistant Professor (n = 411) D: All faculty (n = 411)

Average number of various publication types among four broad disciplines

Publications	A	B	C	D	E
Journal articles	1.48	.66	.87	.70	.96
Conference proceedings	.40	.08	.06	.12	.18
Book chapters	.14	.20	.40	.25	.25
Books	.03	.07	.11	.09	.08
Other	.17	.14	.20	.19	.18
Total	2.22	1.17	1.64	1.35	1.65

A: Sciences & mathematics (n = 118) B: Humanities (n = 76)
C: Social sciences (n = 112) D: Professional schools (n = 105)
E: All faculty (n = 411)

Average productivity earlier and later in career

	Early in career (years 1-6)	Later in career (years 7+)	Average increase
Science and mathematics	1.38(117)	2.43(117)	76.1%
Humanities	0.66 (67)	1.22 (67)	84.8%
Social sciences	1.17(111)	1.79(111)	53.0%
Professional schools	1.04 (94)	1.52 (94)	46.2%
Males	1.14(345)	1.84(345)	58.0%
Females	0.91 (44)	1.64 (44)	80.2%
Professors	1.24(275)	2.05(275)	65.3%
Associate Professors	0.85(104)	1.34(104)	57.6%

Average number of various publication types among males and females

Average publications	A	B	C
Journal articles	1.00	.72	.96
Conference proceedings	.18	.13	.18
Book chapters	.24	.30	.25
Books	.08	.06	.08
Other	.18	.16	.18
Total	1.69	1.38	1.65

A: Males (n = 355)
B: Females (n = 56)
C: All faculty (n = 411)

Degree of increase or decrease in productivity over time

	Increase (n)	Decrease (n)	P. i. p.
Science and mathematics	1.77 (80)	0.54 (35)	70.0%
Humanities	0.87 (50)	0.41 (15)	76.9%
Social sciences	1.27 (70)	0.50 (40)	63.6%
Professional schools	0.94 (67)	0.69 (26)	72.9%
Males	1.27(235)	0.54(104)	69.3%
Females	1.22 (32)	0.58 (12)	72.7%
Professors	1.36(196)	0.58 (78)	71.5%
Associate Professors	1.07 (64)	0.50 (35)	64.4%

P.i.p. : Percent increasing productivity

S. Bonzi
Information Processing & Management, 28(1992) 111-120

Concerning the Future of the NSF

During the past decade, the National Science Foundation (NSF) has taken strides into the areas of applied science and technology and science education. The engineering directorate has enjoyed a constantly increasing budget under the mandate of Congress. The creation of new, multi-investigator centers, with tasks such as cement research and new methods for building construction, testify to the movement of the NSF's center of gravity away from basic research and toward highly applied projects. The question facing the Special Commission on the Future of the NSF can be phrased as follows. Should that center of gravity: (i) move further and faster into the realm of engineering, technology, and applied sciences; (ii) stay about where it is; or (iii) recover some of the distance by which it has moved away from pure science and basic research? Code phrases and buzz words like "international competitiveness" and "technological infrastructure" should not obscure the real issue. The NSF is the only U.S. governmental agency ever created specifically to maintain the strength of basic research. Should it now accord a higher priority to applied research?

There has been a deeply troubling crescendo in the view that the public should pay only for such applied science and engineering as is clearly aimed at solving recognized economic, environmental, and even social problems. There is the implication that basic research may be a luxury we can no longer afford. Although the United States must convert its scientific leadership more effectively into technological leadership, it must also continue to lead in basic science. Apart from massive programs such as space exploration and the supercollider (about which many have well-founded misgivings), the NSF supports the only well-rounded and consistent basic research program in the country. The idea is that professors and their students, following their own curiosity about how nature works, can produce new knowledge that will support the technology of the future.

In 1945 Dr. Vannevar Bush, the respected maker of science policy, wrote a report to President Truman in which the general purpose, the design, and the philosophical basis of NSF were promulgated. In his foreword to the 1980 reprint of Bush's report, *Science – The Endless Frontier*, Norman Hackerman said, "Dr. Bush words sound just as topical in 1980 as they did in 1945". I hope that the men and women of the commission will find them "just as topical" in 1992. Seven statements made by Bush deserve special consideration:

"Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown."

"Basic research ... creates the fund from which the practical applications of knowledge must be drawn."

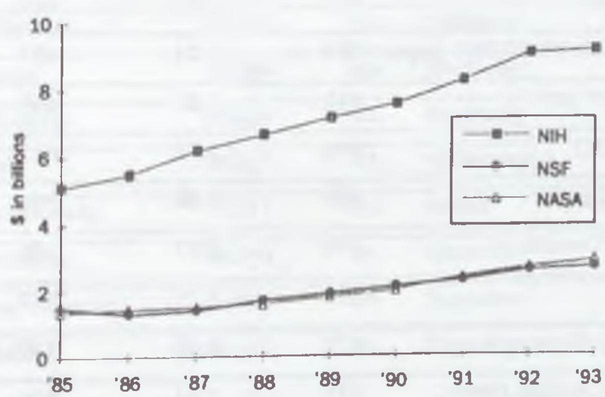
"A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade..."

"The simplest and most effective way in which the government can strengthen industrial research is to support basic research..."

"Basic research is performed without thought of practical ends. Basic research is a long-term process – it ceases to be basic if immediate results are expected on short-term support."

"...[T]here is a perverse law governing research: Under the pressure for immediate results, and unless deliberate policies are set up to guard against this, applied research invariably drives out pure ... The moral is clear: It is pure research which deserves and requires special protection and specially assured support."

Growth slows for US research budgets



NIH figures do not include ADAMHA agencies. NASA figures represent space science and applications budget. Source: Office of Management and Budget, Federation of American Scientists.

These statements are in now way taken out of context. Bear in mind, also, that Bush himself was an engineer, and thus not at all unfriendly to technology nor unappreciative of the need for society to derive practical benefits from science and technology. The commission would do well to ponder deeply these statements. Let us instead rededicate the NSF to its true purpose, to foster in American universities free, basic, curiosity-driven research.

F. A. Cotton,
Science,
258 (16 October 1992)

The companies with high-impact patents...	
Company (Headquarters country)	1991 Current impact index
CORDIS (U.S.)	2.25
INTEL (U.S.)	2.16
PROCTER & GAMBLE (U.S.)	2.08
ALZA (U.S.)	1.96
NISSAN MOTOR (Japan)	1.91
TEXAS INSTRUMENTS (U.S.)	1.87
AT&T (U.S.)	1.84
NIPPON TELEPHONE (Japan)	1.84
AMP (U.S.)	1.84
FUJI HEAVY INDUSTRIES (Japan)	1.83
MAZDA MOTOR (Japan)	1.81
MITSUBISHI MOTORS (Japan)	1.78
XEROX (U.S.)	1.75
COLGATE-PALMOLIVE (U.S.)	1.72
IMB (U.S.)	1.71

... and those closest to the cutting edge	
Company (Headquarters country)	1991 Technology cycle index
FUJI HEAVY INDUSTRIES (Japan)	4.1
MAZDA MOTOR (Japan)	4.5
PIONEER ELECTRONIC (Japan)	4.5
MITSUBISHI MOTORS (Japan)	4.6
RICOH (Japan)	4.6
NISSAN MOTOR (Japan)	4.7
OLYMPOS OPTICAL (Japan)	4.8
NIKON (Japan)	4.8
INTEL (U.S.)	4.9
AISIN SEIKI (Japan)	5.0
NEC (Japan)	5.0
SONY (Japan)	5.0
MITSUBISHI ELECTRIC (Japan)	5.1
MINOLTA CAMERA (Japan)	5.1
TOYOTA MOTOR (Japan)	5.2

Data: CHI Research Inc.

A test of high-tech strength

The **number of patents** granted by the U.S. government is a traditional test of hightech strength. But CHI Research Inc. has come up with more sophasticated measures, based on earlier patents cited as building blocks in new patents. The **current impact index** measures how often a company's patents are cited relative to those of all other companies. The 1.45 for Toshiba, for example, means its patents are cited 45% more than average. CHI multiples the index by number of patents to get **technological strength**, the basis for this ranking. **Technology cycle time** is the median age in years of patents cited in a company's new patents. The shorter the cycle time, the faster the company is developing new technology.

Business Week, August, 1991 nyomán

Company (Headquarters country)	Number of U.S. patents 1991	Current impact index 1991	Techno-logical strength	Technology cycle time 1991
TOSHIBA (Japan)	1156	1.45	1677	5.4
HITACHI (Japan)	1139	1.43	1633	5.7
CANON (Japan)	828	1.45	1201	6.0
MITSUBISHI ELECTRIC (Japan)	959	1.24	1190	5.1
EASTMAN KODAK (U.S.)	887	1.34	1186	7.8
IBM (U.S.)	680	1.71	1161	5.9
GENERAL MOTORS (U.S.)*	863	1.32	1139	7.9
GENERAL ELECTRIC (U.S.)	923	1.16	1069	8.8
FUJI PHOTO FILM (Japan)	742	1.42	1056	5.8
MOTOROLA (U.S.)	631	1.54	969	5.5
AT&T (U.S.)	487	1.84	895	5.3
PHILIPS (Netherlands)	768	1.02	781	6.0
NISSAN MOTOR (Japan)	385	1.91	736	4.7
TEXAS (U.S.)	380	1.87	709	6.2
NEC (Japan)	482	1.46	706	5.0
MATSUSHITA ELECTRIC (Japan)	561	1.24	694	5.6
DU PONT (U.S.)	631	1.06	669	9.7
XEROX (U.S.)	353	1.75	619	6.8
FUJITSU (Japan)	382	1.56	596	5.3
SIEMENS (Germany)	610	0.97	589	6.6
3M (U.S.)	374	1.39	519	10.9
HOECHTS (Germany)	575	0.88	507	8.6
MINOLTA CAMERA (Japan)	315	1.61	506	5.1
AMP (U.S.)	275	1.84	505	7.1
SHARP (Japan)	388	1.27	493	5.4

* Including EDS and GM Hughes Electronics — Data: CHI Research Inc.

A világ 100 legnagyobb gazdasági egysége

A	B	C	A	B	C	A	B	C
1.	USA	5237707	34.	**IBM**	63438	67.	Irország	30054
2.	Japan	2920310	35.	**Toyota**	60443	68.	**Toshiba**	29469
3.	Németország	1272959	36.	Hongkong	59202	69.	**Chevron**	29443
4.	Franciaország	1000866	37.	Jugoszlávia	59080	70.	**Nestlé**	28364
5.	Olaszország	871955	38.	**General Electric**	55264	71.	Egy. Arab Emirátusok	28449
6.	Nagy-Britannia	834166	39.	Görögország	53626	72.	Nigéria	28314
7.	Kanada	500337	40.	Algéria	53116	73.	Szingapúr	28058
8.	Kína	393006	41.	**Mobil**	50976	74.	**Renault**	27456
9.	Brazilia	375146	42.	Hitachi	50894	75.	ENI	27119
10.	Spanyolország	358352	43.	**British Petroleum**	49484	76.	Magyarország	27078
11.	India	287383	44.	**IRI**	49077	77.	**Phillips**	26992
12.	Ausztrália	242131	45.	Venezuela	47164	78.	**Honda**	26484
13.	Hollandia	237415	46.	Izrael	44131	79.	BASE	25317
14.	Svájc	197984	47.	Portugália	44058	80.	**Nippon Elect. Corp.**	24594
15.	Dél-Korea	186467	48.	**Matsushita**	43086	81.	**Hoechst**	24403
16.	Svédország	184230	49.	Fülöp-szigetek	42754	82.	**Amoco**	24214
17.	Mexikó	170053	50.	**Daimler-Benz**	40616	83.	**Peugeot**	24090
18.	Belgium	162026	51.	Pakisztán	40134	84.	BAT	23528
19.	Ausztria	131899	52.	Új-Zéland	39437	85.	**Elf Aquitanie**	23501
20.	**General Motors**	126974	53.	**Philip Morris**	39069	86.	**Bayer AG**	23021
21.	Finnország	109705	54.	Kolumbia	38607	87.	Peru	23009
22.	Dánia	105263	55.	Malaysia	37005	88.	Chile	22910
23.	**Ford**	96932	56.	**Fiat**	36740	89.	CGE	22575
24.	Norvégia	92097	57.	**Chrysler**	36156	90.	Marokkó	22069
25.	Szaúd-Arabia	89986	58.	**Nissan**	36078	91.	**ICI**	21889
26.	Indonézia	87936	59.	**Unilever**	35284	92.	**Procter & Gamble**	21689
27.	**Exxon**	86656	60.	**Du Pont**	35209	93.	**Mitsubishi**	21213
28.	Dél-Afrika	86029	61.	**Samsung**	35189	94.	**ABB**	21209
29.	**Shell**	85527	62.	**Volkswagen**	34746	95.	Bulgária	20860
30.	Törökország	74731	63.	Kuvait	33082	96.	**Nippon Steel**	20767
31.	Argentína	68780	64.	**Siemens**	32659	97.	**Boeing**	20276
32.	Lengyelország	66974	65.	Egyiptom	32501	98.	Puerto Rico	20118
33.	Thaiföld	64437	66.	**Texaco**	32416	99.	**Occidental**	20068
						100.	**Daewoo**	19981

HVG, 1992. január 25.

A: Helyezés
B: Ország / Vállalat
C: Nemzeti össztermék (GNP) / forgalom (millió dollár)

Neurosciences & Behavior: Top U.S. Institutions

Among Those Publishing > 500 Papers, 1986-90

Rank	Name	No. Papers 1986-90	No. Citations 1986-90	Citations Per Paper
1	Stanford University	660	6,229	9.44
2	Washington University, St. Louis	695	6,125	8.81
3	NIMH	1,185	9,944	8.39
4	Yale University	919	7,318	7.96
5	Cornell University	767	6,087	7.94
6	University of California, Irvine	655	5,048	7.71
7	Harvard University	1,149	10,905	7.68
8	Johns Hopkins University	955	6,675	6.99
9	NINCDS	674	4,671	6.93
10	New York University	604	4,083	6.76

Among Those Publishing 100-500 Papers, 1986-90

Rank	Name	No. Papers 1986-90	No. Citations 1986-90	Citations Per Paper
1	Memorial Sloan Kettering Cancer Ctr.	102	1,365	13.38
2	Merck, Sharp & Dohme	117	1,545	13.21
3	Salk Institute for Biological Studies	221	2,914	13.19
4	NICHHD	143	1,624	11.36
5	Caltech	115	1,247	10.84
6	University of Miami	265	2,304	8.69
7	Baylor College of Medicine	226	1,917	8.48
8	Scripps Clinic & Research Foundation	188	1,523	8.10
9	McLean Hospital, Belmont, Mass.	171	1,382	8.08
10	NIAAA	218	1,605	7.36

Source: ISI's Science Indicators Database, 1986-90

Neurosciences & Behavior: Top Non-U.S. Institutions

Among Those Publishing > 500 Papers, 1986-90

Rank	Name	No Papers 1986-90	No. Citations 1986-90	Citations Per Paper
1	University of Lund	534	4,045	7.57
2	University of Oxford	611	4,217	6.90
3	Karolinska Institute	1,080	7,193	6.66
4	University of Cambridge	530	3,074	5.80
5	Institute of Psychiatry, London	568	3,135	5.52
6	INSERM	653	3,521	5.39
7	McGill University	811	4,295	5.30
8	University of British Columbia	605	3,156	5.22
9	University of Toronto	834	3,907	4.68
10	CNRS, Paris	817	3,737	4.57

Among Those Publishing 100-500 papers, 1986-90

Rank	Name	No Papers 1986-90	No. Citations 1986-90	Citations Per Paper
1	Sandoz	172	1,785	10.38
2	Hammersmith Hospital, London	157	1,471	9.37
3	University of Basel	138	1,262	9.14
4	University of Bristol	176	1,596	9.07
5	Max Planck Inst. Psychiatry, Martinsried	377	3,266	8.66
6	St. George's Hospital, London	130	1,034	7.95
7	College of France	202	1,514	7.50
8	Flinders University	212	1,545	7.29
9	MRC, Cambridge, U.K.	286	2,082	7.28
10	University of London, University College	359	2,439	6.75

SOURCE: ISI's Science Indicators Database, 1986-90

Science Watch 2, 6(1991) 1-2

Hypertension Study Stands Tall as Model of Meta-Analysis

What's hot in medicine...

Rank	Paper	Citations This Period (Mar-Apr 92)	Rank Last Period (Jan-Feb 92)
1	L.C. Cantley, K.R. Auger, C. Carpenter, B. Duckworth, A. Graziani, R. Kapeller, S. Soltoff, "Oncogenes and signal transduction," Cell, 64(2):281-302, 25 January 1991. [Tufts U., Boston, Mass.]	54	2
2	E.R. Fearon, B Vogelstein, "A genetic model for colorectal tumorigenesis," Cell, 61(5):759-67, 1 June 1990. (Johns Hopkins U. Sch. Med., Baltimore, Md.]	45	1
3	R. Collins, R. Peto, S. MacMahon, P. Hebert, N.H. Fiebach, K.A., Eberlein, J. Godwin, N. Qizilbash, J.O. Taylor, C.H. Hennekens, "Blood pressure, stroke, and coronary heart disease. Part 2. Short-term reductions in blood pressure: Overview of randomized drug trials in their epidemiological context," The Lancet, 335(8693):827-38, 7 April 1990. [ICRF, Oxford, U.K.; John Radcliffe Hosp., Oxford, U.K., U. Auckland, New Zealand; Brigham & Womens Hosp., Boston, Mass.; Yale U., New Haven, Conn.; U. Leeds, U.K.; Harvard U. Sch. Med., Cambridge, Mass.]	33	*
4	D. Malkin, F.P. Li, L.C. Strong, J.F. Fraumeni, C.E. Nelson, D.H. Kim, J. Kassel, M.A. Gryka, F.Z. Bisehoff, M.A. Tainsky, S.H. Friend, "Germ line p53 mutations in a familial syndrome of breast cancer, sarcomas, and other neoplasms," Science, 250(4985):1233-8, 30 November 1990. (Mass. Gen. Hosp., Boston; U. Texas, M.D. Anderson Hosp. & Tumor Inst., Houston; Harvard U., Children's Hosp., Boston; NIH, NCI, Bethesda, Md.]	32	3
5	M. Hollstein, D. Sidransky, B. Vogelstein, C.C. Harris "p53 mutations in human cancers," Science, 253(5015):49-53, 5 July 1991. (NCI, Bethesda, Md.; Johns Hopkins U. Sch. Med., Baltimore, Md.]	28	*
6	B.A. Werness, A.J. Levine, P.M. Howley, "Association of human papillomavirus type-16 and type-18 E6 proteins with p53," Science, 248(4951):76-9, 6 April 1990. [NIH, NCI, Bethesda, Md.; Princeton U., N.J.)	26	9
7	G. Brown, J.J. Alhers, L.D. Fisher, S.M. Schaefer, J.L Lin, C. Kaplan, X.Q. Zhao, B.D. Bisson, V.F. Fitzpatrick, H.T. Dodge, "Regression of coronary-artery disease as a result of intensive lipid-lowering therapy in men with high-levels of apolipoprotein-B, " New Engl. J. Med, 323(19):1289-98, 8 November 1990. [U. Washington, Seattle]	26	6
8	A.J. Levine, J. Momand, C.A. Finlay "The p53 tumor suppressor gene," Nature, 351(6326):453-6, 6 June 1991. [Princeton U., N.J.]	26	*
9	C.J. Marshall "Tumor suppressor genes," Cell, 62(2):313-26, 25 January 1991. [Inst. Cancer Res., London, U.K.]	24	*
10	S.J. Baker, S. Markowitz, E.R. Fearon, J.K.V. Willson, B. Vogelstein, "Suppression of human colorectal carcinoma cell growth by wild-type p53," Science, 249(4971):912-5, 24 August 1990. [Johns Hopkins U. Sch. Med., Baltimore, Md.; U. Hosp. Cleveland, Ohio; Case Western Reserve U., Cleveland, Ohio]	23	4

SOURCE: ISI's Hot Papers Database

NB. Only papers published since March 1990 are tracked. As asterisk indicates that the paper was not ranked in the Top Ten during the last period. In the event that two or more papers collected the same number of citations in the most recent bimonthly period, total citations to date determine the rankings.

Work on oncogenes and tumor suppressor genes – and in particular, the p53 gene – continues to dominate the list of medicine's hottest papers. However, a new entry on a different topic has stolen part of the spotlight this period. Paper #3 examines 14 large-scale trials of antihypertensive drugs involving a total of 37,000 patients. This form of review, which combines statistical data from several studies and attempts to establish conclusions that may not have been evident in the individual reports, is known as "meta-analysis."

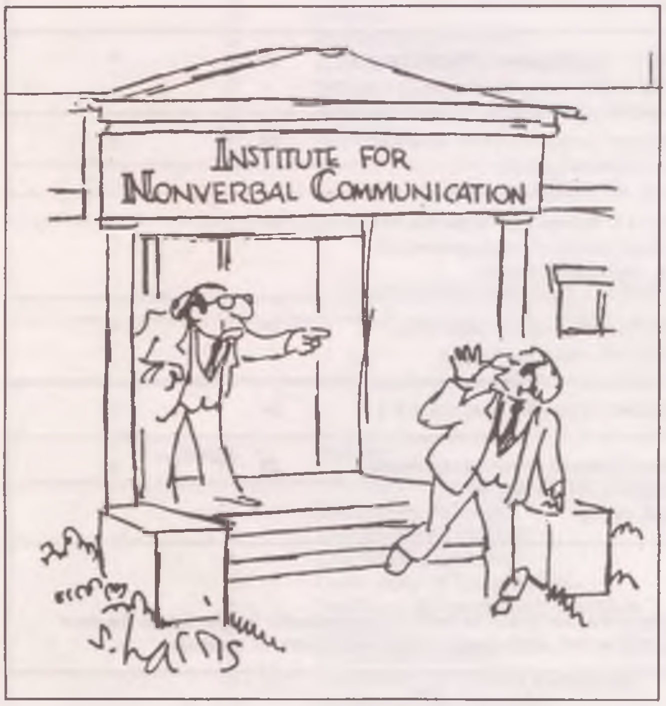
In this meta-analysis, the authors conclude that drug treatment of hypertension substantially reduces the risk of stroke. The effects of antihypertensive treatment on the risk of coronary heart disease are less clear, however.

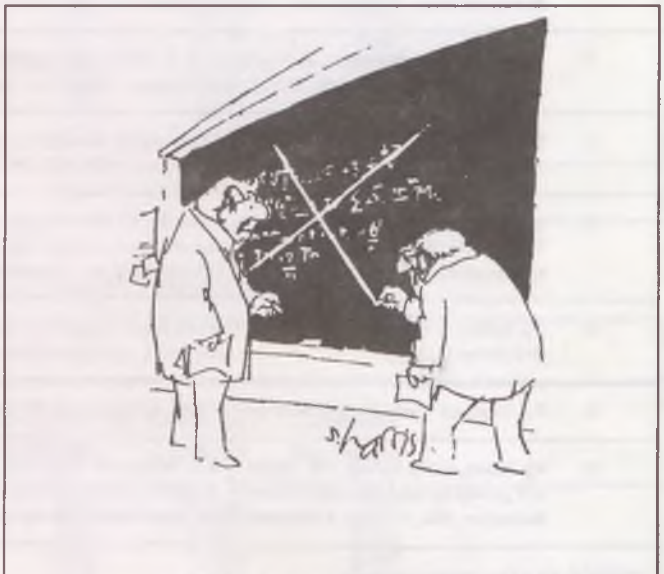
"This study has quite a high degree of value," John D. Swales of the University of Leicester Medical School, U.K., tells *Science Watch*." It shows that within the period of the trials – up to about five years or so – the risk of stroke attributable to hypertension was completely reversed. And that was a most dramatic and impressive finding. This approach had considerably more power than looking at the individual component trials. It reinforces the importance of treating hypertension from the point of view of preventing strokes. It also suggests that we still need to clarify the effect that treating hypertension has on heart attacks, because there's still some slight ambiguity in the data. This study obviously gives one a good current overview of what the clinical trials have told us."

Not all studies employing meta-analysis are held in such high regard. To some observers, the growing practice of collectively evaluating previous clinical trials has serious, even ominous, short-comings. "What is most alarming," writes cardiologist William E. Boden, Veterans Affairs Medical Center and Tufts University School of Medicine, Boston, Massachusetts, "is the *manner* in which meta-analysis has emerged as a virtual surrogate for sound, clinical decision making that derives from original prospective research, despite the fact that meta-analysis is, above all, observational in nature and represents a *retrospective look at data already collected*.... Perhaps even more alarming is the developing trend that many physicians and investigators view the results of meta-analysis as therapeutically definitive" (see W.E. Boden in *Amer. J. Cardiol.*, 69[6]:681-6, 1 March 1992).

Swales, too, has been critical of meta-analysis (see J.D. Swales in *J. Hypertension.*, 9[suppl. 6]:42-6, December 1991) "When you're using a meta-analysis," he tells *Science Watch*, "you've got to make sure you've got access to all the studies, and that there's none of what's called positive publication bias, when researchers tend to publish only the positive findings. When they do that, of course, they're going to come up with some wonderful results. It's important to look at the quality of the individual studies themselves and make sure they don't contain any bias or consistent design flaws. I think it's important to proceed critically and cautiously before you leap in, because you can get into very deep water with this sort of analysis. The paper on your list, however, is a good one. It's probably one of the best."

Aside from a report on the benefits of lipid-lowering therapy in reducing coronary artery disease, which fell this period from sixth to seventh position, the rest of the Top Ten is given over to studies of oncogenes and tumor suppressor genes. New arrivals include a review of p53 mutations in human cancers and another on p53's role as a tumor suppressor (paper #5 and #8). A third, discussing tumor suppressors in general, currently ranks ninth.





"That's it? That's peer review?"

Tisztelt Előfizetőink!

Köszönjük érdeklődésüket és bizalmukat, amellyel 1992-ben fogadták az *Impakt*-ot. Reméljük, hogy előfizetésükkel 1993-ban is támogatni fogják lapunk működését. Csakis olvasói táborunk segítégével van lehetőségünk változatlan áron, növekvő színvonalon előállítani folyóiratunkat.

Kérjük, hogy szándékukat a mellékelt szelvényen jelezzék!

Készült az MTAK házi sokszorosító részlegében

Felelős kiadó: az MTAK főigazgatója