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3. évfolyam • 5. szám • 1993. május

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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Basic research

The good guys have gotten into an argument that should be settled quickly before damage is done. On the one side are the good guys in Congress who are looking out for the good of the country but are involved in the mechanics of how the country is governed. On the other side are the good guys in science who are looking out for the good of the country but are involved in the mechanics of how science is done. There is a problem in mutual communication that is not helped by terms such as "basic science," "applied science", "industrial policy," and "technology transfer," each of which can be interpreted in very different ways.

Basic research is not pure or unuseful or ivory tower research. It is undoubtedly the most useful and the biggest payoff – but it is also the biggest gamble and the least likely to produce an immediate predictable outcome. Basic research is like gambling on individual numbers in roulette where the chances of success are low but the payoff is big when it comes. Applied research is like gambling on the red or the black number where the successful outcome is more likely but the payoff is lower. The difference between research and gambling is that research provides a gain in knowledge even if an immediate practical application is not forthcoming. The difference between basic and applied research is the scope of the applicability, the time scale for the expected profit, and the predictability of the outcome. The discovery of Hertzian waves could not have predicted a world of radio, but it was the essential first step. The elucidation of enzyme specificity and enzyme pathways did not come from any desire for drug design, but it was the knowledge that allowed the development of the wonder drugs of today. Applied research has a narrower, more immediate goal than basic research. And if the applied research fails, it usually leads to new questions that give clues to new basic research horizons.

Basic research has brought us x-rays, penicillin, polio vaccines, light-weight polymers; computers, the green revolution, and recombinant DNA, to name a few of the discoveries that have changed the world in revolutionary ways. The investigators were never paid in advance to develop the industries that resulted from their discoveries but were rather looking into a phenomenon of nature and uncovered surprising new theories and other phenomena. Applied research has taken basic findings and provided the additional information needed to bring us vaccines, radio sets, television sets, the light-weight polymer wrappings that preserve our foods, and the heavier polymers that form our toys. Applied research has thus taken the basic research discoveries and converted them into practical products. Basic research is not necessarily more intellectually demanding than applied research, and indeed many of the discoveries of basic research have been revealed in the process of developing and exploiting new technologies. In applied research the successful application is expected; in basic research a successful application is astonishing.

Because of the unpredictability that any one basic project will succeed in giving a new product, it is unrealistic for Congress to expect, or for scientists to promise, immediate payoffs. On the other hand it would be a monstrous policy error to cut back on basic research if a significant increase in jobs or standard of living is wanted. The developed countries – the United States, France, Germany, England, Japan, and so on – are extraordinarily wealthy, and their citizens live extremely well compared to most of the people of the world. The investment in basic research is a small part of their total income; it is in their own interest to make that investment because they can retain their competitive advantage only if they are in the forefront of the new revolutionary industries. It is also their obligation because basic research in one country helps it in the short run and the entire world in the long run. Thus the developed nations have the expertise and money and are the immediate beneficiaries, but they also have a noblesse oblige to those less fortunate to carry on basic research – the long-range gamble that will ultimately dramatically change the living standard of the world.

So perhaps the debate should be reformulated in the terms of "revolutionary (for basic) research" and "evolutionary (for applied) research." The next question that arises is how to divide the organizational responsibilities, the funding levels, and the priorities of these two types of research.

If one concludes that research, both basic and applied, are essential to the improvement of the quality of life in a developed country, the questions of priorities, funding level, and organization will inevitably arise. At each of these levels a symbiotic arrangement must be developed between the political structure and the scientific structure in order to maximize benefits and to eliminate friction.

With regard to the strategic goals there is no question that the ultimate arbiter will be the government acting as a spokesman for the citizens of the country, but it would be a poorly advised government that would proceed to establish priorities with no understanding of what is scientifically possible or likely. For example, it is apparent that an automobile that could travel 100 kilometers on a liter of gas and would not release any carbon dioxide would be highly desirable in the current world. Scientists would be needed to convince legislators that such an achievement is not scientifically possible although some increased car efficiency is possible. On the other hand, when AIDS or some other epidemic spreads in the world, the scientific expertise can inform legislators that money for research can be well spent and will hasten cure and prevention of the disease.

An understanding of the successful symbiosis of government and science has no more shining example than the National Institutes of Health (NIH). When the New York Hygienic Laboratory ultimately became the NIH, it was asked by Congress to attack the cancer and infectious disease problems. The NIH officials, as well as officials at the National Foundation for Infantile Paralysis, correctly deduced that massive efforts in hit-and-miss chemotherapy would be ill-advised and decided that because cancer is growth and viruses are a source of infections a basic understanding of both processes was needed. They initiated a program of basic research (investigator-initiated) to understand growth and infectious diseases at a fundamental level. Because some scientists then believed (correctly, as it turned out) that viruses could cause cancer as well as diseases, a program to be able to grow viruses in the laboratory was initiated. That basic research endeavor led eventually to the development of the polio vaccine, recombinant DNA, oncogenes, and retroviruses. That knowledge not only forms a basis for much of our improved treatment of cancer, but the research effort had spin-offs in the treatment of polio and virus diseases in general, an understanding of genetic causes of disease such as cystic fibrosis, and the emergence of a biotechnology industry. Thus, basic research can flourish as part of a strategic target as long as the legislators are patient, that is, receptive to the serendipitous nature of research. When the research advanced to the point that a polio vaccine was possible, that was the time for the applied research aspect. Impatience could have created a world filled with iron lungs instead of healthy people with circulating antibodies.

This research on cell growth and infectious diseases is only one example of unexpected benefits that derive from organized serendipity, and the example can be repeated in many other areas such as transistors, lasers, polymers, and weather prediction. Nothing as tidy as having all basic research in one agency such as the National Science Foundation (NSF), nor anything as short-sighted as having no agency, that encourages basic research in nontargeted areas, such as NSF, is sensible in the modern era. The line between the roles of government and industry in basic research is a blurry one that only individuals who understand complexity can handle, and those who think it can be made simple do not understand the problem.

The key to good science policy is informed assent, in which legislators accept the need for scientific advice on the mechanics of achieving their goal, and scientists recognize that legislators have the right to set the strategic goals based on societal needs. The current debate on converting the NSF to a targeted research agency is an example of inappropriate ideas in which some in the Congress are implying that basic research is not useful for the nation and some scientists are implying that the government has no right to interfere in the research process. The record of the NIH is a glorious example of the expected and unexpected benefits of a system in which the proper mixture of basic and applied research was implemented. The advanced developed nations that are not bent on territorial conquest have no alternative except research to improve the quality of life of their citizens and the citizens of the world. It is vital that research be administered with mutual respect for the responsibilities and expertise of science and government.

Daniel E. Kosbland, Jr.
Science, 259(1993) (15 January) 291
(29 January) 579

Nem kicsi az SzBK, de erős

A *Science Watch* egyik legutóbbi intézet-rangsora a molekuláris biológia nagyjait adta közre. [1] A *Current Contents/Life Sciences*ben referált mintegy hetven molekuláris biológiai folyóiratban 1981-1991 között legalább 200 cikket közlő 50 kutatóhelyet sorolja fel az írás. A *CC* mintafolyóiratok említett 11 éves cikktermésébõl 23451-et (24%) írtak az ötven legtermékenyebb intézet kutatói. (Amennyiben egy cikket csak egy készítő helyhez soroltak be az értékelés készítői. Ez valószínű, bár nem tesznek említést a hogyanról.)

A rangsor alapja nem a cikkszám volt, hanem a cikkekre ugyanezen időszak alatt történt hivatkozások alapján számított átlagos idézettség. Érdekes, hogy míg az ötvenek leggyengébbje is 14,24 idézetet kapott cikkenként, átlaguk pedig 21,00, addig a cikkek háromnegyed részét adó kisebb produktivitású kutatóhelyek cikkeinek idézeti átlaga csupán 7,30.

Természetesen nem igaz, hogy a nagyok mögött csupa gyenge intézetecske sorakozik. A kivételek közé tartozik az MTA Szegedi Biológiai Központ is, amely minden valószínűség szerint a legnagyobb molekuláris biológiai kutatóhely Közép-Kelet Európában, bár az intézet témáit korántsem meríti ki a szűkebb értelemben vett molekuláris biológia. 1981-1991 közöt 185 cikk jelent meg a molekuláris biológia fentemlített folyóiratai közül 21-ben, az SZBK kutatóinak részvételével. A 185 cikkben 105 SzBK-s kutató vett részt, az átlaglétszám mintegy 55%-a. A cikkek összesen 3987 idézetet kaptak, egyre tehát 21,55 jut, ami a 21. helyre lenne elég az ötvenek közt. Ez az eredmény egy szegény magyar kutatóhely számára túlságosan is hízelgő ahhoz, hogy ne vetődjenek föl ünneprontó kérdések:

- Hátha csak egy-két kivételes cikk viszi fel az átlagot?
- Hátha csak a külföldön készült cikkek miatt ilyen jó az idézettség?

Az első kérdéssel kapcsolatban a *Science Watch* cikke is szolgáltat adatot, kirívó példaként megemlítve, hogy az egyik nagy intézet idézeteinek 65%-át mindössze két kutatója szerezte. Nos, az SzBK cikkek idézeteloszlása (ehhez képest) nagyon egyenletes. A legidézettebb cikk is "csak" 227 idézetet kapott összesen.

A második kérdésre nem tudunk egyértelmű választ adni, mert a *Science Watch* nem ad kritériumot az intézeti besorolásra. Tagadhatatlan, hogy az SzBK-ban igen élénk a nemzetközi kooperáció [2,3]. Tájékozódás céljából a következő differenciálást végeztük: elkülönítettük azokat a cikkeket, amelyek szerzői több, mint 50%-ban az SzBK-ból kerültek ki. 78 ilyet találtunk, ezek átlagos idézettsége 22,23, azaz nemhogy nem rosszabb hanem még jobb is valamivel a nem SzBK szerzőtöbbségű SzBK cikkek átlagánál.

Következésképpen az SzBK molekuláris biológiai kutatásai világszínvonalon állónak tekinthetők az idézettség alapján, akár a kooperációs, akár az itthoni teljesítményt nézzük. A miértre és a hogyanra legyen elég itt csak egy okot megemlíteni: a hetvenes évek végétől tudatos az SzBK kutatók törekvése és ösztönzése a legjobb folyóiratokban való publikálásra.

Marton János, SzBK

[1] Top 50 Research Institutions in Molecular Biology Ranked by Citation Impact, 1981-91 (Among those publishing > 200 papers): *Science Watch* (May 1992) 7. Közli: *Impakt*, 3 (1993)(2) 5-6.

[2] **Koleszár Viktória, Lovas Laura, Marton János:** Színvonal és kooperáció. Magyar-japán összehasonlító elemzés. *Magyar Tudomány*, 37:983-986 (1992)

[3] **Anveiler Judit, Tóth Erika, Marton János:** Tudóselvándorlás. Nem az megy és nem az marad, akinek kéne. *Magyar Tudomány* (közlés alatt)

Japanese Scientists Predict the Future

Date	Breakthrough
1998:	Substitutes for ozone-damaging CFCs
1999:	Large-volume, coherent optical communication systems
2000:	Silicon memory with 1-nanosecond access time
2001:	Economical way to remove usable products from urban waste
2002:	1-gigabit memory chips
2003:	Technology to prevent NO_x emissions
	Widespread use of biodegradable packaging materials
2004:	Ultrahigh-speed computers
2006:	Cure to AIDS (a vaccine is predicted by 2003)
	Prediction of volcanic eruptions 2-3 days in advance
2007:	Method to prevent cancer metastasis
2008:	Methods to limit $C0_2$ emissions
2009:	Elucidation of cancer-related genes and carcinogenesis
2010:	Understanding mechanisms for almost all types of cancer
	Ability to predict earthquakes of 7 or higher several days in advance
	Nursing robots
2013:	Drugs to prevent cancer
2015:	Cure for Alzheimer's disease
2017:	Fast-breeder reactors
After 2020:	Fusion reactors

Science, 259(1993)(January 22) 461

A Magyar Tudományos Akadémia és a magyar felsőoktatási intézmények természettudományi publikációs és idézettségi profilja, 1980 – 1989

A következő táblázatok a Magyar Tudományos Akadémia és a magyar felsőoktatási intézmények publikációs és idézettségi adatait mutatják be 1980-1989 között a *Magyar Természettudományi Alapkutatás Publikációs Adatbank*jának adatai alapján. Ez az adatbank a *Science Citation Index* (*SCI*) adatbázisban foglalt minden olyan tételt tartalmaz, amelyhez tartozó munkahelyi címek között az *SCI* legalább egy magyarországi címet tart nyilván. (Az adatbank számítógépes változata három mágneslemezen, használati útmutatással együtt díjmentesen beszerezhető az MTA Könyvtára Informatikai Igazgatóságán.) Az *MTA* kategóriába soroltunk minden olyan cikket, amelynek legalább egy szerzője akadémiai munkahelyet tüntetett fel, hasonlóképpen a *Felsőoktatás* kategóriába beleszámoltunk minden olyan cikket, amelyben legalább egy felsőoktatási intézmény volt megadva. A számottevő mértékű átfedés miatt előfordul, hogy a két kategória összege meghaladja a 100%-ot.

A teljes magyar tudományra vonatkozó adatok mellett (1. táblázat), a 2.-3. táblázatban megadjuk az élő, ill. az élettelen természettudományokra vonatkozó adatokat is. A területek megkülönböztetése a közlő folyóiratok szakterületi besorolása alapján történt (lásd pl. Braun, Glänzel, Schubert: *Országok, szakterületek, folyóiratok tudománymetriai mutatószámai, 1981-1985*. MTA Könyvtára, Budapest, 1992). Természetesen az idézettségi átlagokat mindig a megfelelő részterületek átlagaihoz viszonyítottuk.

1. táblázat
Természettudományok, orvosi, műszaki és mezőgazdasági tudományok együtt

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Százalékos részesedés a magyar publikációkból:										
MTA	27.6	29.0	29.9	30.7	28.0	28.3	31.9	33.3	35.1	38.8
Felsőoktatás	57.9	55.7	55.9	57.0	59.0	62.1	61.1	58.9	56.8	56.4
Százalékos részesedés a magyar publikációkra kapott idézetekből:										
MTA	39.1	42.6	38.9	39.5	40.1	37.8	46.1	45.1	46.0	52.3
Felsőoktatás	58.0	55.5	56.1	59.8	57.7	61.8	54.5	54.1	54.7	48.8
Százalékos részesedés a magyar publikációkra várható idézetekből:										
MTA	32.8	34.4	34.6	35.8	33.7	32.7	39.7	40.5	42.0	44.6
Felsőoktatás	58.5	56.3	55.6	58.6	59.1	61.3	56.2	56.1	54.6	53.6
Egy cikkre kapott idézettség a magyar átlag százalékában:										
MTA	141.5	146.9	130.2	128.5	143.2	133.5	144.8	135.4	131.0	134.7
Felsőoktatás	100.1	99.7	100.4	104.9	97.9	99.6	89.2	91.9	96.4	86.4
Egy cikkre várható* idézettség a magyar átlag százalékában:										
MTA	118.7	118.5	115.7	116.6	120.4	115.6	124.5	121.5	119.6	115.0
Felsőoktatás	101.0	101.1	99.6	102.9	100.2	98.7	92.0	95.3	96.2	95.0

2. táblázat
Élő természettudományok, orvosi és mezőgazdasági tudományok

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Százalékos részesedés a magyar publikációkból:										
MTA	16.7	18.5	17.4	19.6	16.9	16.9	19.8	20.8	20.9	25.5
Felsőoktatás	63.5	59.7	60.4	61.3	64.8	68.5	68.2	64.9	62.6	60.8
Százalékos részesedés a magyar publikációkra kapott idézetekből:										
MTA	27.0	32.0	24.9	31.0	29.4	28.3	34.7	37.2	35.2	48.7
Felsőoktatás	62.9	63.6	65.0	67.0	66.2	68.1	61.0	56.5	57.2	42.9
Százalékos részesedés a magyar publikációkra várható idézetekből:										
MTA	23.2	23.6	23.9	27.2	23.3	24.6	26.8	32.2	29.6	34.1
Felsőoktatás	63.0	63.7	62.8	65.1	66.2	66.4	63.1	59.3	59.2	55.5
Egy cikkre kapott idézettség a magyar átlag százalékában:										
MTA	161.4	173.4	142.8	158.4	173.7	167.3	175.0	179.2	168.6	191.4
Felsőoktatás	99.0	106.6	107.5	109.3	102.2	99.4	89.5	87.0	91.5	70.5
Egy cikkre várható* idézettség a magyar átlag százalékában:										
MTA	138.9	128.1	137.2	138.8	138.0	145.3	135.1	155.4	141.8	133.8
Felsőoktatás	99.1	106.7	104.0	106.2	102.3	96.9	92.6	91.3	94.7	91.3

3. táblázat
Élettelen természettudományok és műszaki tudományok

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Százalékos részesedés a magyar publikációkból:										
MTA	45.6	41.8	46.3	44.4	46.0	45.3	48.5	47.7	51.9	51.0
Felsőoktatás	48.6	50.8	49.9	51.7	49.6	52.6	51.3	52.1	49.9	52.4
Százalékos részesedés a magyar publikációkra kapott idézetekből:										
MTA	54.6	55.8	57.2	50.8	54.0	53.6	57.8	55.7	56.0	54.9
Felsőoktatás	51.8	45.4	44.6	50.0	46.8	51.5	47.9	51.0	52.4	53.0
Százalékos részesedés a magyar publikációkra várható idézetekből:										
MTA	50.4	51.4	52.5	49.2	50.9	49.2	58.7	53.5	56.8	53.8
Felsőoktatás	50.1	44.6	43.6	48.7	47.3	51.1	45.8	51.2	49.1	51.9
Egy cikkre kapott idézettség a magyar átlag százalékában:										
MTA	119.9	133.5	123.6	114.6	117.3	118.3	119.3	116.9	108.0	107.5
Felsőoktatás	106.5	89.4	89.3	96.8	94.4	97.8	93.3	98.0	105.1	101.2
Egy cikkre várható* idézettség a magyar átlag százalékában:										
MTA	110.7	122.9	113.5	111.0	110.6	108.7	121.0	112.2	109.4	105.5
Felsőoktatás	103.1	87.8	87.4	94.1	95.4	97.2	89.4	98.3	98.3	99.0

* Várható idézettségen a cikkeket publikáló folyóiratok átlagos idézettségét értjük

Report details Eastern Europe's scitech problems

The current science and technology environment in Eastern Europe is described in a report by the Federal Coordinating Council of Science, Engineering & Technology, led by the President's science adviser, D. Allan Bromley.

- An artificial separation of basic research and education exists despite new efforts to develop affiliations between research institutes and universities. Graduate research authority is gradually being transferred from academies of science to universities.
- Ideological control of research is slowly being replaced by planning based on objective evaluations of research productivity. Peer review is increasingly being used as the criterion for determining research support mechanisms.
- Erosion of strong R&D personnel base continues because of reductions in staff at research institutes and the "brain drain" of scientists to the West.
- Sharp cuts continue – up to 50% in research budgets from 1989 level – and many institutes are desperately seeking support from foreign governments and private foundations.
- Research centers are largely unable to purchase research equipment and supplies for modernizing their laboratories, and thus have difficulty benefiting from recent progress in such areas as computer-assisted data collection and analysis methods.
- Access to international science and technology literature and communications networks remains inadequate. Most researchers are isolated from Western contacts and the cost of travel to international meetings is beyond their reach.
- Transfer of research into application is weak and the infrastructure that supports feedback from design to marketing to R&D is almost nonexistent.
- Linkages between industries and research are weaker now than in the recent past because of a decline in support for R&D institutes.
- Traditional strengths remain in neurosciences, mathematics and theoretical physics, materials science, analytic, inorganic, nuclear, and electrochemical research, but the future of even the best R&D centers is uncertain.
- New research priorities are just developing in applied engineering, technology, environment, energy, health sciences, agricultural research, biotechnology, materials research, and electronics.
- There is an emergence of new leaders at research institutes, but they have little experience in research management.
- Private-sector development of R&D is poor.
- Civilian sector use of military research scientists is inadequate.
- There is little use of East European R&D personnel by Western countries or Japan even though costs for R&D in East Europe are only about 10% those in the West.
- Competitive grant mechanisms have been established in Poland, Czechoslovakia, and Hungary, and have been favorably received by scientists.

C&EN, 1993 (January 4) 27

Scientific Research in China: A Sleeping Giant Starts to Stir

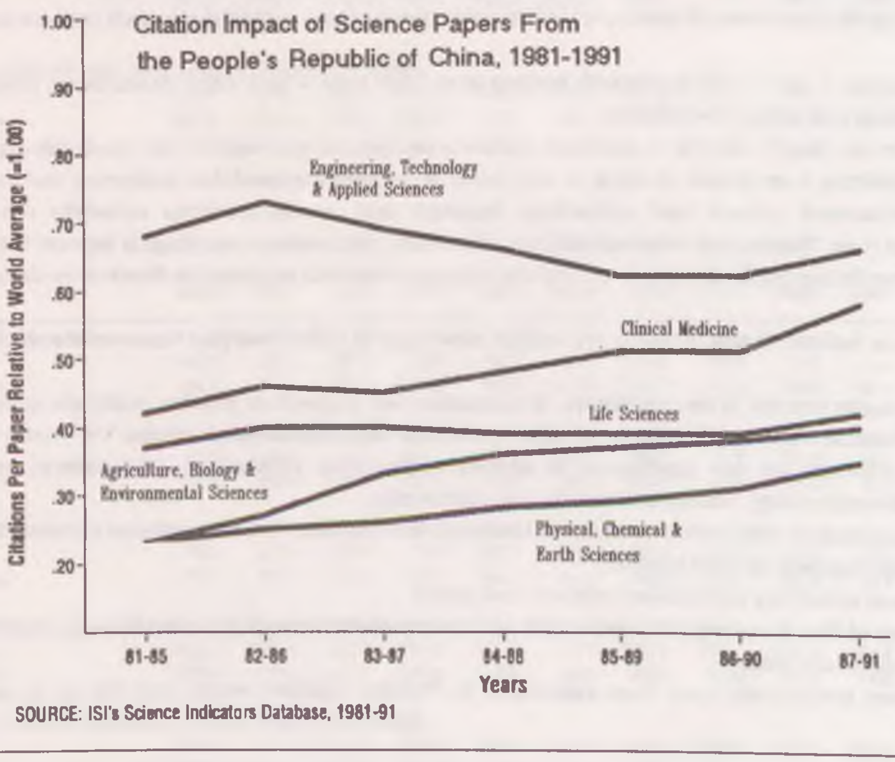
Despite its size, China at present makes only a modest contribution to the international scientific literature that the Institute for Scientific Information indexes for its *Science Citation Index* and *Current Contents*. However small the contribution currently, however, there are clear signs that China will substantially increase its presence in the scientific sphere, as it is beginning to do economically.

Both publication and citation data reflect China's increasing presence on the world's scientific scene. ISI's Science Indicators Database grew as a whole by 18.2% from 1981 to 1991 (from 566,868 items to 670,188), while Chinese papers increased 448.2% (from 1,410 to 7,730). China's share of the literature indexed by ISI was just .3% in 1981 but rose to 1.2% by 1991. In terms of average citations per science paper (all fields taken together), Chinese papers earned 30% of the world average for the period 1981-85, but increased its impact to 39% of the world figure in the most recent five-year period, 1987-91.

In *Science Watch*'s view, China is a sleeping scientific giant that has now begun to stir. In fact, as the graph below illustrates, the citation impact of Chinese papers took a sharp turn upward, comparing papers published and cited during 1986-90 to those of 1987-91. Clinical medicine showed the biggest gain, but the increases in impact were significant for engineering, technology, and applied sciences, and for physical chemical, and earth sciences, as well. Gains in the sciences and in agriculture, biology, and environmental sciences were smaller, but they were gains against the world average nonetheless.

Table 1 provides a more detailed view of Chinese science in terms of citation impact. *Science Watch* separated 1987-91 papers by Chinese scientists into subfields, based upon the journals in which papers appeared and the journal classification scheme of disciplines employed by ISI for *Current Contents*. Only subfields in which China produced at least 100 papers over this five-year period are listed. The average citations-per-paper figure for each subfield for China was compared to the respective average for the world, and a ratio of China to the world was obtained. The subfields were then ranked according to China's relative citation impact. In essence, the table identifies the strengths and weaknesses of Chinese science as compared with the world standard for each subfield.

Table 2 compares the relative citation impact of Chinese papers by subfield in 1981-85 to those of 1987-91 and identifies the areas in which China improved its standing as well as those in which it lost ground. For example, in the neurosciences (#3 in the list of "gainers"), China's relative citation impact score for 1981-85 was −64 in comparison to the world, but by 1987-91 it was −17 in comparison to the world: a change in relative citation impact of +47. So, while China's performance in this area is still below world level, it has improved dramatically during the past decade.



SOURCE: ISI's Science Indicators Database, 1981-91

Table 1
China's Research Strengths and Weaknesses:
Subfields Ranked by Citation Impact Relative to World Average, 1987-91

Rank	Field	Papers 1987-91	Impact of China: World
1	Agriculture & Agronomy	118	+19
2	Metallurgy	529	+17
3	Computer Sciences	379	+9
4	Medical & Lab Technology	232	+8
5	Instrumentation & Control	512	+7
6	Surgery	106	–
7	Nuclear Engineering	368	-3
8	Neurosciences	318	-17
9	Aquatic Sciences	145	-25
10	Agricultural Chemistry	210	-27
11	Experimental Biology & Medicine	988	-28
12	Chemical Engineering	346	-29
13	Electrical Engineering	694	-30
	Environmental & Social Medicine	102	-30
14	General Animal & Plant Sciences	203	-31
15	Analytical, Inorganic Chemistry	1455	-34
16	Organic Chemistry	1029	-35
	Botany	311	-35
17	Applied Physics/Condensed Matter	5732	-36
18	Environmental/Civil Engineering	428	-38
19	Life-Sciences Chemistry	552	-39
	Environment & Ecology	398	-39
20	Mechanical Engineering	1213	-46
	Optics & Acoustics	752	-46
	Zoology	275	-46
	Immunology	137	-46
21	Earth Sciences	1105	-47
22	Materials Science	1274	-48
23	Astronomy & Astrophysics	309	-52
24	Physical Chemistry/Chemical Physics	1229	-57
25	Biochemistry & Biophysics	465	-59
26	Physics	3691	-61
	Pharmacology	1220	-61
	Mathematics	1178	-61
	Microbiology & Cell Biology	333	-61
	Agricultural Biology	103	-61
21	General Clinical Medicine	1191	-68
28	Molecular Biology & Genetics	264	-70
29	Entomology & Pest Control	277	-71
30	Gastroenterology	109	-75
31	General Chemistry	1559	-77
32	Multidisciplinary Journals	4289	-94

SOURCE: ISI's Science Indicators Database, 1981-91.

Table 2
China's Top Gainers, Biggest Losers:
Subfields Ranked by Change in Relative Citation Impact, 1981-85 to 1987-91

Gainers

Rank	Field	Change in Impact, 1981-85 to 1987-91
1	Agriculture & Agronomy	+85
2	Medical & Lab Technology	+55
3	Neurosciences	+47
4	Surgery	+43
5	Zoology	+41
6	Aquatic Sciences	+40
7	General Animal & Plant Sciences	+33
8	Applied Physics/Condensed Matter	+31
9	Earth Sciences	+29
10	Analytical, Inorganic Chemistry	+25
	Chemical Engineering	+25
	Botany	+25
11	Astronomy & Astrophysics	+24

Losers

Rank	Field	Change in Impact, 1981-85 to 1987-91
1	Gastroenterology	-48
2	Instrumentation & Control	-37
3	Agricultural Chemistry	-27
4	Materials Science	-13
5	Mechanical Engineering	- 9
6	Computer Sciences	- 8
	General Clinical Medicine	- 8
7	Environmental & Social Medicine	- 7
8	Mathematics	- 6
9	Biochemistry & Biophysics	- 4
10	Multidisciplinary Journals	- 3
11	Electrical Engineering	- 2
12	Microbiology & Cell Biology	- 1

SOURCE: ISI's Science Indicators Database, 1981-91.

As both the graph and the tables show, engineering, technology, and applied sciences stand out as the strongest areas of Chinese research at present, especially the subfields of metallurgy, computer sciences, instrumentation and control, nuclear engineering, and chemical and electrical engineering.

Although extrapolating the future from the past is a dangerous game, there seems little risk in predicting that the next two to three decades will witness Chinese science emerging strongly in both output and impact.

Science Watch 3 (1992) 9 1-2

Citation Data Is Subtle Stuff – A Primer on Evaluating a Scientist's Performance

When starting to compile citation data from the scientific literature over 25 years ago, I aimed to create a new tool for information retrieval – the *Science Citation Index* (*SCI*). Out of this came a useful by-product: a huge and ever-increasing database containing indicators of intellectual connections among scientists and their publications.

The *SCI* attracted the attention of historians and sociologists of science and served as a catalyst to the field of scientometrics, which uses quantitative methods to analyze the process and development of science. The ISI database has facilitated large-scale quantitative studies of the scientific performance of countries, institutions, fields, departments and individuals. In recent years, such scientometric studies have even contributed to public policy decisions in science. However, it is in assessing the performance of individual scientists, especially in the context of promotion or grant decisions, that the use of citation data is most controversial.

This controversy has arisen for a number of reasons – none more basic perhaps than a common aversion to the impersonal judgment of numbers. Beyond that, however much of the criticism of citation measures is a response to "quick-and-dirty" citation-counting, a thoughtless practice that reveals little more than an amateur at work and gives citation analysis a bad name. I have repeatedly warned about the mistaken conclusions drawn from the crude manipulation of citation data. Recently (*The Scientist*, February 23, 1987, p. 9*), I emphasized the distinct difference between "the simple-minded counting of articles or citations as indicators of quality and the in-depth analysis that can and should be carried out." This statement warrants some expansion. Here, then, is a primer on how to evaluate scientists using citation data.

First, you'll need a complete and accurate bibliography of the candidate's publications. The *SCI Citation Index* lists all cited papers and books under the first author's name. Using the bibliography or full CV will ensure that all coauthored publications are included in your collection of citation data. (If a bibliography or CV is unavailable, a search through successive years of the *SCI Source Index*, which cross-references secondary to primary authors, can provide a reasonably complete list of papers.)

A complete bibliography resolves a second problem: homographs: For example, in the *SCI* the heading SUZUKI T includes the publications of many authors. Individual works, however, are clearly identified: journal title, volume, year and page information separate citations unambiguously.

Having obtained the required data, you have completed only the first, elementary step in citation analysis. You have determined which publications are cited, by whom, and how often but not why they are cited. Nor does this show what these citations mean. The chief difficulty (and responsibility) resides in the interpretation of these data. To assess the individual fairly, some idea of the comparative performance of his or her peers is needed. Since publication and citation rates vary widely from field to field, you' ll need to know what is typical in the candidate's area, as well as for persons of similar experience.

Other aspects to consider include: the extent and nature of self-citation; the chronological distribution of the citations; whether citations are concentrated around a few papers (especially those on methods) or dispersed among many; the extent to which citations are cross-disciplinary or international; the quality and impact of journals from which the citations derive; and, of course, the *SCI's* coverage of the individual's field.

An "in-depth analysis" also requires asking why citations have been given. With the citing papers in hand, you can engage in content and context analysis. Content analysis reveals what idea or fact in a publication is being referred to, while context analysis reveals the judgment of the citing author – favorable, critical or whatever. Clearly, such interpretative work is best done by someone knowledgeable in the field of the scientist under review.

Citation analysis demands further probing questions. Given the varied nature of citations, many critics have asked, and rightly, what after all is being measured? An in-depth analysis such as I have described will go far toward answering that question, but I would add this caution: although many studies have shown a significant correlation between citation analysis and peer judgment, citations are only indicators of influence and impact; they are a partial reflection of the interests of the academic community and the visibility of a person's work. They say nothing about intrinsic value. That is the role of human judgment.

Furthermore, judgment is necessary to understand why a publication is relatively or completely uncited. Relatively low citation-frequency may signal that a publication was superseded by another or that its impact was so profound that it underwent "obliteration by incorporation," a process in which the substance of a work becomes part of the common understanding and explicit citation is deemed unnecessary. (See R. K. Merton, *Social Theory and Social Structure*, 1968, pp. 27-29, 35-38.) Uncitedness, on the other hand, occurs in instances of premature discovery – of work ahead of its time – and not only for work of low utility.

"Citation data is subtle stuff," I wrote in 1979. "Those using it to evaluate research performance at any level, but particularly at the level of individuals, must understand both its subtleties and its limitations. The position of those who advocate the use of citation data to evaluate people is not that it is simple and foolproof, but that problems associated with it can be solved satisfactorily with a reasonable amount of methodological or interpretative effort." (*Citation Indexing*, 1979, p. 241.)

E. Garfield,
The Scientist, 1 (1987)(10) 229

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Opting Out of the Numbers Game
On the Need to Emphasize Quality in Peer Review

As a long-time student of the scientific journal, I have witnessed incidences of unwarranted co-authorship, repeated publication of the same work, and the practice of "salami science" the slicing of a single research project into its least publishable units. In large part, such behavior by authors can be ascribed to a growing and long excessive pressure to publish in great quantity. This pressure has also been cited as contributing to recent, notorious cases of scientific fraud. Unfortunately, our academic review and reward system, which too often focuses on numbers, may occasionally encourage misconduct both great and small.

A good alternative has been suggested. Marcia Angell, deputy editor of the *New England Journal of Medicine*, and DeWitt Stetten, of the National Institutes of Health, have independently suggested that a ceiling be placed on the number of works a committee considers in making promotion and grant decisions. Angell has proposed that a peer-review board examine "only at most, the three articles the candidate considers to be his or her best in any given year, with a maximum of perhaps ten in any 5-year period. Other publications should not even be listed." (*Annals of Internal Medicine* 104, February 1986, 262.) Stetten has suggested, "let the applicant select, say, one dozen of his bibliographic citations that are most meaningful to him." He added, "in this regard it may be pointed out that ... nomination to membership in the National Academy of Sciences requires a selective bibliography of no more than 12 publications." (*Science* 232, 4 April 1986, 11.)

Whatever the ceiling, the idea is the same: to emphasize quality of publications, rather than quantity. If a committee were faced with tens instead of hundreds of papers, it is more likely that members would actually read the work before them and judge its substance. "Each publication would then receive commensurately more attention, both from the researcher and those evaluating the work," wrote Angell.

Imagine the potential result of implementing a quality-oriented review process: researchers would produce fewer, better, and more thoughtful papers, and by doing so would lessen some of the clutter now clogging the journal literature. Investigators, in choosing a research project without regard to its probability for yielding rapid and numerous publications, might feel freer to tackle more difficult questions, whose answers offer greater rewards. Although obviously not a panacea, a ceiling might lessen the excess pressure to publish by degrees.

Neither Angell nor Stetten knows of a single instance in which their suggestion has been adopted by a promotion or grant committee. This raises the question of what might be preventing its implementation. Tradition is probably one inhibiting factor, but there are others.

Some have argued that certain fast-growing fields require the quick, preliminary article to establish priority for the researcher, and that such an article is written chiefly for this reason, not as a substantive investigation of a problem. However, the famous two-page paper by Watson and Crick in 1953 describing the structure of DNA proves that establishing priority in a brief and substantive fashion is not impossible.

Another argument against the proposal involves research reported in a series of articles that reveal the substance of the work only when considered as a group. But a ceiling of a dozen papers or two is certainly high enough to meet this objection.

Still others think it is unfair to base a judgment on a sample. Although it does not wholly refute this objection, I point out that the researcher, not the committee, would select the work being judged. In any case, a sample that is read seems more satisfactory than a large corpus that is skimmed over or not read at all.

No doubt some flexibility and certain refinements, such as including a mechanism to evaluate what a person has accomplished recently, can be built into the system.

I would be most interested to hear from any committee that has actually adopted a quality-oriented peer-review system of the type described here.

Finally, a personal note. I have acquired over the years a reputation as "the great quantifier," owing to the citation-based analyses we at ISI publish. It might therefore seem ironic to have me endorse subjective over quantitative measures. But there is no irony. I have always emphasized the difference between the simple-minded counting of articles or citations as indicators of quality and the in-depth analyses that can and should be carried out. I have repeatedly warned against the cavalier use of citation data. But, sadly, many have found it simpler to "do their additions," in spite of the pernicious implications of this practice. Bibliometric studies can contribute to an evaluation, but ought not to substitute for other more detailed measures.

I use citation analysis as a step toward identifying publications that have elicited great attention among peers – "Citation Classics" which are highlighted each week in *Current Contents*. Researchers also can employ citation analysis to help them choose their own influential works to submit under an Angell-Stetten model of review. Since identification of quality contributions has occupied so much of my energy, my endorsing an emphasis on quality in peer review should come as no surprise.

E. Garfield,
The Scientist 1 (1987)(7) 9

Why Business Needs Scientists

Twenty years ago I was a physicist working on neutron-scattering experiments at Brookhaven National Laboratory. Now, as the vice-chairman of Sony USA and president of Sony Software, I represent Sony in both the electronics and the entertainment business. I spend my days discussing and overseeing projects that range from new developments in high definition to the cutting edge of popular music.

My experience has convinced me that a background in pure science is an ideal preparation for business. I will take that a step further and say that American business would be a lot better off if it had more scientists and fewer M.B.A.'s running its corporations.

Why do I think the neutron detector prepared me for life at Sony? As a physicist, I was doing work I considered important and working with people I admired. But as I looked around the lab, I asked myself whether this was what I wanted to be doing 20 years into the future. I thought I might like to try business, but I was not absolutely sure. When I shared my uncertainty with my thesis adviser, the distinguished researcher Robert Nathans, he gave me some advice I will never forget. "Don't worry about it, Mickey," he said. "You're a physicist. Physicists don't do anything they really don't want to do. If you get into business and find you don't like it, you'll get out."

Obviously, I liked it. I stayed. But I stayed as a physicist. No matter what it says in my job description, I am still a scientist. And I have approached business problems the same way I approached scientific problems. The lessons I learned as a scientist were excellent instruction for business.

Some of those lessons are as basic as a strong work ethic. The business school yuppies of the 1980s glamorized the idea of working long hours. But that trend was in fashion in labs long before anyone ever heard of Michael Milken. I can well remember sitting up until 3 A.M. babysitting our precious high-flux beam reactor through an experiment. The hours didn't matter. It was the result that counted. When you have a meaningful challenge, personal time means very little. That is a lesson I have carried over into corporate life.

Science also encouraged my intellectual curiosity. Of course, that was something that attracted me to physics in the first place. But working in the lab at Brookhaven taught me how stimulating it was to make intellectual curiosity the center of your professional life. My responsibilities have obviously changed. But intellectual curiosity is very much a part of what keeps me going in the business world. In science, you accept intellectual curiosity as a given. I wish it were more common in business.

I would also like to see business people develop some of the tenacity that is common in science. People in business tend to be impatient. The scientists I worked with were anxious to see results. But they realized that you had to build the foundation before you could put on the roof. By example, they taught me the importance of mastering the fundamentals of a field before you could do meaningful new work. Shortly after Sony acquired Columbia Pictures, I began to read the scripts for films we had under production. That didn't endear me to some of the operating people. One of them challenged me about why I wanted the scripts. He as much as told me that they were not going to let me take over the creative decisions. But I told him he was missing the point. I was not interested in telling the creative experts how to make films, but I was intensely interested in understanding the process.

Learning as much as you can about the details is a lesson that is actually discouraged in many business schools. They promote the misleading idea of the generic manager – the consummate professional whose education has prepared him or her to step into any kind of business and run it.

The myth of the plug-in executive created a generation of migratory managers in American business. Most of them do not have the time or the inclination to learn anything in-depth about the business they are responsible for. Instead they bring their business school theories to each assignment. And quite often they do not stay around long enough even to evaluate whether or not the theories are valid. That is a big difference between business graduates and science graduates. The business graduates accept theory as gospel. The science graduates accept theory as the starting point for experimentation.

An equally dangerous trend in the graduate schools of business is their potential to restrict creativity. And the greater the reputation of the business school, the greater the risk that its graduates will rely on management theory instead of personal creativity. There is a time for doing things the Wharton way or the Harvard way. But there is also a time for doing things your way.

To be truly successful in business, you have to be a creative risk-taker. I have spent about $7 billion of Sony's money to acquire companies such as Columbia Pictures and CBS Records. These were strategic acquisitions that supported our long-term vision for Sony. You have to have your own vision of the future. And you need the confidence to invest in that vision. It is not much different from the approach to scientific research. The people I admired most in science had the creativity to develop long-term visions of the future as well as the courage to stick with that vision unless research proved them wrong.

In the years ahead, business people will be asked to solve complex problems with very high stakes, not just for their corporations but for society as a whole. Some of those problems will involve decisions about technology, about the environment, about the economy and the marketplace, even about government. Scientists understand the process of critical thinking. They know how to analyze problems by concentrating on the important elements and filtering out the irrelevant. They understand that worthwhile results require a longlived effort. They are willing to admit there are things they do not understand and then take the time to find out what it is they don't know.

Business needs that kind of vision and that kind of intellectual courage. Business could get that kind of thinking by taking some of its surplus M.B.A.'s and sending them back to school for Ph.D.'s in science. Fascinating, but unlikely. Instead I think business has the responsibility to recruit more scientists.

Michael Schulhof,
Scientific American 1992(November) 96

An editors's reflections on editing

When a scientific society publishes a specialized journal that has a circulation of a thousand or less, the life of the editor is likely to be grim.

At some time in a symposium on publication, a few moments should be devoted to seeking perspective on the changing roles of scientific journals. We know that in the development of science, journals have played absolutely essential roles. They still continue to have important functions, but their overwhelming importance has been somewhat attenuated. Advances in technology have altered modes of communication, and further changes will occur in the future.

At a time when travel was slow and the telephone was nonexistent or unreliable, scientific journals were by far the most practical form of communication with one's peers. The comparatively small group of people devoted to science were held together, their morale strengthened, and they were kept informed by perusing a small number of journals. Scientists, of course, engaged in personal correspondence which transferred information faster than journals, but the tempo of the two modes of communication was not greatly different.

With the advent of the jet airplane, improved telephonic communication, electronic mail and electronic data bases, there have arisen modes of communication much faster than the journals. Typical scientific publications are characterized by delays of six to eighteen months between receipt of manuscripts and their appearance. Very few have delays less than four months. In fast-moving fields in the United States, scientists still publish material of record in journals, but in addition they employ other, faster modes of communication. The telephone, electronic mail, and jet travel are widely used. In addition, there are invisible colleges and closed symposia. In any given field or subfield there often are fifty to one hundred very active productive participants. These scientists use many means of communication including preprints of manuscripts, to transfer information. In addition, they organize formal and informal symposia to talk about the latest developments. Favorite annual examples are the symposia at Cold Spring Harbor and the Gordon Research Conferences. In both cases, the sessions are held at a rural location, participants are housed close to the conference center, and they dine at a common facility. The schedules and circumstances are such as to promote maximum interchange of information and enthusiasm.

Another factor that has attenuated the overwhelming importance of individual scientific journals is their great number. The typical scientist is faced with a nearly impossible task when trying to keep up with developments by reading journals which are behind the times.

In spite of the foregoing, I believe strongly that scientific journals continue to have important roles. These roles differ, depending on the nature of the journal, that is, whether it is strictly devoted to original research or is more general and includes review articles.

The journal devoted to original research serves an archival function that is absolutely essential to the progress of science. It has often been said that present-day scientists stand on the shoulders of giants who have gone before them. Successful research is not complete if there is no archival record, and its value in general is lost. The work may be described in some seminars and conversations, but the impact of the talk is fleeting and it is usually felt by a limited group.

We have heard repeatedly about the publish-or-perish syndrome. There is little doubt that there are abuses and that sometimes those in authority are more impressed by the number of a scientist's publications than by their quality. However, the reviewing process does provide a screening mechanism. A complete absence of a bibliography or a very short one is almost invariably a true sign of lack of productivity. Some individuals are very impressive in conversation but their papers provide a much more objective basis for evaluation.

The need to compile a respectable bibliography has beneficial disciplinary effects on scientists. For many of them, doing experiments is fun; writing about them is a chore. Moreover, so very often when the researcher begins to write about the research, it becomes apparent that the work is not acceptably complete. Often work must be duplicated, and sometimes the contemplation of what has been learned or what has not leads to stimulus of the imagination. Again, it is to be emphasized that without publication, research is not complete.

A journal containing review articles such as *Interciencia* can serve very useful purposes. The value of these is

(Continued on next page)

dependent on the skill and judgment of the editor. An editor such as Marcel Roche can select and recruit especially significant material. The publication informs an important readership of developments affecting the hemisphere, and it assists in promoting interaction among the scientists while informing political people of matters that should concern them.

Thus far, my remarks have been directed to communication among scientists and to the roles of journals. To some of you, journals are objects which appear at stated intervals. In some ways, they are like the municipal water supply. You turn the faucet and out comes the water.

Someone else has the responsibility of bringing the product to you. I would like to take you behind the scenes for a few moments to tell you of some things I have learned and experienced about scientific publication. Beginning about fifty years ago, I observed others doing chores as editors. Then, twenty-eight years ago, I became co-editor of the *Journal of Geophysical Research*. In 1962 I became editor of *Science*. In both positions I was exposed to all facets of producing a journal, including intellectual, management, and financial, but I also have seen other situations in which the editor faced dreadful problems due to lack of resources and help.

With a large circulation such as that of *Science* (155,000), revenues from members and subscribers and from advertising are such that an editor can have adequate support and assistance. With a medium circulation (of the order of 10,000) such as that enjoyed by the *Journal of Geophysical Research*, revenues are still sufficient. In that instance, members' dues, sales to libraries, and page charges are the principal sources. In general, when circulation is 10,000 or less, advertising is not a practical source of revenue.

Many scientific journals have circulations of the order of a thousand or less. Many of these are published by commercial houses such as Pergamon. Their principal sources of revenue is subscriptions by libraries which they charge heavily.

When a scientific society publishes a specialized journal that has a circulation of a thousand or less, the life of the editor is likely to be grim. Financial limitations are ever present. There are insufficient funds for assistants. I have witnessed editors of such journals spending long hours at the mechanics of editing and proof reading.

My advice to those who would consider becoming editors is to first check on two aspects of the circumstances. First, is there a reasonable guarantee of a suitable budget? Second will scientific colleagues cooperate fully in peer review of manuscripts and in sharing other tasks as necessary? I would also caution them about having illusions about the power of an editor. *A good editor is an unappreciated servant.* My experience was that readers were about twenty times as likely to complain (in letters to the editor) as to praise. True, in conversation they might say something pleasant. In my day I received some rather biting letters, though none to match one quoted in a recent book by Irving M. Klotz. From my experience I would advise no one to become an editor unless they can take abuse without being distraught or subject to ulcers.

The prize example from Dr. Klotz was a review of a paper submitted to a journal. The review follows:

"I have searched the tedious studies of this author without success for some trace of ingenuity, acuteness or learning that might compensate for his evident deficiency in the powers of solid thinking or of calm and patient investigation... This manuscript teaches no new truth, reconciles no contradictions, arranges no anomalous facts, suggests no new experiments and leads to no new inquiries... As this paper contains nothing which deserves the name either of experiment or discovery, and as it is in fact destitute of every species of merit, it should certainly be admitted to your Proceeding to join the company of that multitude of other paltry and unsubstantial papers which are being published in your journal."

While I was Editor, I received some rather bitter letters, but none to match the one just quoted. In many instances, the letter writer had either misinterpreted or had made a peculiar interpretation of a sentence or a paragraph appearing in the journal. On first reading of such a letter, my initial reaction was to become angry and to mentally compose an abusive letter in response. However, I did not send such letters. Instead, I waited a week. Then when I was in a pleasant mood, I would decide about a response. In many instances, I wrote no letter. My estimate of the situation was that to reply would only lead to further, unpleasant correspondence. At other times, I sent a letter which began, "*Thank you for your interesting comment on the item that appeared recently in our journal*". I might add a few sentences, but found it best to say as little as possible. It is hard for someone to use what you don't say against you.

When I received two or more letters on the same topic from quite disparate sources, my behavior was different. I looked into the matter carefully and prepared a more thoughtful reply. Indeed, on occasion the letters served to lead to policy changes.

The late President Truman often has been quoted as saying, "If you can't stand the heat, stay out of the kitchen." The comparable statement is, "If you can't stand criticism, if you can't stand an evident lack of appreciation, don't aspire to be an editor."

However, given the right temperament, an editor can find the tasks rewarding. Or may be able to render important services to scientist peers, and there are incentives and opportunities to engage in an intense learning experience.

But as I stated earlier, don't accept the position of editor unless both financial and intellectual support are visible.

Philip H. Abelson,
Interciencia 12 (1987) (2) 81-82

Készült az MTAK házi sokszorosító részlegében *Felelős kiadó: az MTAK főigazgatója*