2. évfolyam • 5. szám • 1992. 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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Kutatás és fejlesztés (K&F)

Fogalmak és definíciók

Nem könnyű feladat a címben említett fogalmak és a mögöttük álló eszmei tartalom pontosítása. Nem is vagyunk annyira optimisták, hogy azt reméljük, ezt a feladatot néhány oldalon egyértelműen meg tudjuk oldani. Közismert tény, hogy a szakirodalomban fellelhető K&F fogalomelemzések és definíciók kismillió változatával találkozhatunk. Ezek közül számos olyan van, amelyik önmagában nagyon helytállónak és mélyrehatónak tekinthető. Azonban éppen ezen változatok sokasága bizonyítja, hogy nem alakult ki egy teljesen egyértelmű, általánosan elfogadott konszenzus a K&F fogalmak értelmezésére.

A végleges és teljesen egyértelmű megoldás helyett megkíséreltük a ma elfogadható optimális kompromisszumot megtalálni.

Valószínűnek tartjuk, hogy ma a legcélravezetőbb megoldás az OECD által választott definíciógyűjtemény elfogadása. Nem azért, mert ez a legjobb, hanem azért, mert a legegységesebb összeillesztését adja egy olyan rendszer összetevőinek, amelyek – az ismeretek mai állása szerint – valójában *teljesen logikusan* összeilleszthetetlenek.

Az OECD fogalom- és definíciógyűjteményt a következő publikációkban adták közre:

– The Measurement of Scientific and Technical Activities. Frascati Manual, 1980, OECD, Párizs, 1981.

– The Measurement of Scientific and Technical Activities. Frascati Manual, Supplement, OECD, Párizs, 1989.

Az alábbiakban kigyűjtöttük ezekből a legalapvetőbbeket.

Mindezek kiegészítését szolgálja az ezt követő táblázat, amiben elsősorban egyesült államokbeli források alapján kísérlik meg az alapkutatás és alkalmazott kutatás további pontosítását a "curiosity-oriented research", a "strategic research" és a "tactical research" fogalmak bevezetésével. Meggyőződésünk, hogy a "stratégiai alapkutatás" fogalmának átgondolt alkalmazása a hazai kutatáspolitikai elgondolásokban hasznosnak bizonyulhatna.

Basic Definitions and Conventions

Research and experimental development (R&D) comprise creative work undertaken on a systematic basis in order to increase the stock of knowledge, including knowledge of man, culture and society and the use of this stock of knowledge to devise new applications.

The Basic Criterion

The basic criterion for distinguishing R&D from related activities is the presence in R&D of an appreciable element of novelty.

(Folytatás a következő oldalon)

Functional Distribution

Basic research is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundations of phenomena and observable facts, without any particular application or use in view.

Applied research is also original investigation undertaken in order to acquire new knowledge. It is however, directed primarily towards a specific practical aim or objective.

Experimental development is systematic work, drawing on existing knowledge gained form research and practical experience, that is directed to producing new materials, products and devices, to installing new processes, systems and services, and to improving substantially those already produced and installed.

Classificatory framework for R&D activities

Nature of R & D	Main performer(s)
Basic Research: Original investigation with the primary aim of developing more complete knowledge or understanding of the subject(s) under study.	
Pure or curiosity-orientated research:[a] Basic research carried out without working for long-term economic or social benefits other than the advancement of kowledge, and no positive efforts being made to apply the results to practical problems, or to transfer the results to sectors responsible for its application.	Normally (together with teaching) the main function of the academic university-based research system.
Strategic research: Basic research carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of recognised current or future practical problems.	Carried out in universities and government laboratories, as well as in most larger science-based companies (in which it typically accounts for no more than 5-10 per cent of the R&D budget).
Applied or tactical research:[b] Original investigation undertaken in order to acquire new knowledge, and directed primarily towards specific practical aims or objectives such as determining possible uses for findings of basic research or solving already recognised problems.	Mainly carried out by industry and laboratories of mission-oriented government agencies, although also undertaken (under contract or as part of targeted government research programmes) within the academic research system.
Experimental development: Systematic work drawing on existing knowledge gained from research and/or practical experience that is directed towards producing new or improved materials, products, devices, services, systems or methods, including design and development of prototypes and processes.	Overwhelmingly carried out in industry (where it typically accounts for 80-90 per cent of company R&D budgets) and in mission-oriented government agencies (often where the state is also the customer for the final envisaged products, such as advanced military hardware).

Source: Derived from National Science Board, Science Indicators – 1982, (1983, p. 237), The Measurement of Scientific and Technical Activities, Paris, OECD, (1981, pp. 25-36) and Ronayne, Science and Government, E. Arnold, London, 1983, p. 35.

[a] This is sometimes referred to as 'fundamental' research, although the term can also refer to certain longer-term elements of strategic research.

[b] This is also sometimes referred to as 'mission-oriented' research, particularly in US government agencies, although such work often incorporates shorter-term elements of strategic research.

Mérjünk, de lehetőleg mindenkit ugyanazzal a mércével

Az MTA KKKI-ban gyakori vitára ad okot, hogy *különböző korösszetételű*, az Intézetben más-más hosszúságú időt eltöltött kutatócsoportok publikációs mérőszámainak összehasonlítását hogyan végezzük el. Ezzel kapcsolatosan előrebocsátandó, hogy akár egyének, akár csoportok összehasonlító értékeléséről legyen is szó, az értékelendő eredményt egy jól megválasztott nemzetközi standardhoz kell viszonyítanunk, vagy pedig az értékelendők adatait egymással kell összemérnünk. Ez utóbb említett módszerhez azonban *specifikus mutatókat* kell választanunk. Ezeknek a mutatóknak a segítségével ugyanis a különböző méretű (kutatók száma), különböző anyagi ellátottságú (ráfordítások), illetve különböző hosszúságú időtartam folyamán működő egységeket *"közös nevező"*-re lehet hozni.

A folyóiratok hatástényezőinek (impact factor) kiszámításához Garfield[1] az egy-egy folyóirat által két egymásra következő évben (pl. 1988-1989) publikált cikkekre a rákövetkező egy évben (1990) érkezett idézeteket veszi figyelembe. Braun és munkatársai[2] viszont szinkronizált időablakokkal dolgoznak (pl. 1981-85 között publikált cikkekre az ugyanebben az időszakban kapott idézeteket számolják).

Az MTA KKKI-ben, különböző okok miatt, egy 10 éves időszak alatt megjelent cikkekre a rákövetkező egy évben érkezett idézeteket vesszük számba. A kutatócsoportok azonban különböző hosszúságú szakmai (intézeti) karriert befutott tagokból állnak. Összehasonlítani pedig csak a többé-kevésbé hasonló elvek és feltételek szerint kiszámolt mutatószámokat szabad.

1. táblázat
A csoportok főbb publikálási adatai

Csoport	A	B	C
Értékelt publikációs időtartam (1978-1987)	10	10	10
A kutatók átlagos publikálási ideje 1978-1987 között, években (t)	10	5	1
Kutatói létszám (k)	5	5	5
Publikációs produktivitás (n/(k × év))	1	1	1
Idézetek száma 1988-ban cikkenként (I/n)	2	2	2
A csoport cikkeinek száma 1978-1987 között (n)	50	25	5
A csoport idézeteinek száma 1988-ban (I)	100	50	10

Vegyük illusztrációképpen az 1. táblázat adatait. Tegyük fel, hogy mindhárom kutatócsoport azonos létszámmal (öt fő) dolgozik az értékelés idején. A korábbi tízéves periódusban azonban volt olyan csoport, amelyikben mind az öten végigdolgozták az egész időszakot (pl. A) az átlagos publikálási idő itt t = 10 év, míg a B-csoport 1981-ben jött létre, s így átlagos publikálási ideje csupán 5 év lehetett. C csupa olyan új emberből áll, akik 1986-ban léptek be az Intézetbe s így egy év állt csak rendelkezésükre ahhoz, hogy publikációt jelentessenek meg, illetve idézeteket gyűjtsenek. (Természetesen egy csoport átlaga az egyénekre vonatkozó adatokból számolható.)

Nyilvánvaló, hogy *ugyanolyan publikációs produktivitás* (mennyiségi mutató) *és ugyanolyan cikkenkénti idézetszám* (átlagos hatást "minőséget" jelző szám) mellett a tíz év alatt különböző átlagos időtartamokat ledolgozott csoportok különböző végeredményt – azaz összes cikkszámot, összes idézetszámot – mutatnak fel. (2. táblázat) Ebből az is következik, ha 10 évre vetítjük a teljesítményt, akkor a hosszabb publikálási időtartamú csoportok jutnak indokolatlanul előnyhöz (lásd 2. táblázat). De vajon miért lenne célszerű – az igazságosságról már nem is szólva – ha a mindössze egyetlen évet ledolgozott s így kevesebb publikációt készített, kevesebb idézetet kapott kutatókat ugyanolyan kritériumok alapján ítélnénk meg, mint azokat, akiknek mind a tíz év rendelkezésükre állt? Éppen ezért, ha a kutatócsoportok tényleges erősségét (és érdemeit) akarjuk felmérni, akkor a tízéves időszak alatti átlagos publikálási időt kell figyelembe vennünk és egy évre kell normálnunk az adatokat, a 3. táblázat szerint. A 3. táblázat adatai igazolják azt a helyes feltételezést, hogy azonos publikációs produktivitással dolgozó és cikkenként azonos számú idézetet begyűjtő csoportok teljesítménye is azonos.

2. táblázat
A csoportok látszólagos átlagos publikációs és idézettségi teljesítménye

Csoport	A	B	C
Cikkek száma évente kutatónként (n/(k × 10))	1	0.5	0.1
Idézetek száma évente kutatónként (I/(k × 10))	2	1	0.2

3. táblázat
A csoportok tényleges átlagos publikációs és idézettségi teljesítménye

Csoport	A	B	C
Cikkek száma évente kutatónként (n/(k × t))	1	1	1
Idézetek száma évente kutatónként (I/(k × t))	2	2	2

Kézenfekvő, hogy bármilyen, de főként anyagi kihatásokkal járó értékeléshez (így pl. a jutalom vagy kutatási támogatás felosztásához) kizárólag az átlagos publikációs idővel (t), mint vetítési alappal számolt mutatókat szabad felhasználnunk.

Vinkler Péter MTA KKKI

[1] Garfield E.: Science Citation Index, Journal Citation Reports, – A Bibliometric Analysis of Science Journals in the ISI Data Base, Institute for Scientific Information, Philadelphia 1982.
[2] Braun T., Glänzel W., Schubert A.: Országok, szakterületek, folyóiratok tudománymetriai mutatószámai (1981-1985) MTA Könyvtára, 1992.

The tyranny of peer review

"Only a healthy grant-giving system which allows an honest game will produce optimal research quality. And one of the conditions is a reasonable funding probability for good projects."

Peer review is *the* mechanism of the scientific community. It is the basis of most decisions, such as the recruitment of staff, the publication of scientific papers and, most important, the allocation of funds. In recent years, much has been published about the process, which is perceived as inadequate and in need of replacement by something more objective. There have even been experiments which demonstrate that the decision-making appears to be almost random. However, most of the perceived problems are due, I believe, to inappropriate boundary conditions.

The basic rule of the peer review game is the *consensus of experts*. This does not mean that the experts have to meet to discuss the matter through, but it means that a sensible use of their expertise will have to be based on what consensus emerges from the evidence. It does not mean, either, that the "experts" have to be the closest competing collegues of the object of judgement. On the contrary, there is an optimal distance: as far away as possible to still be able to assess the details of the subject. As a programme director of DFG, the German research council, I have seen this consensus successfully at work for many years. Decision were not all random when funding could be extended to most projects which the reviewers (with information about the money available) found worth funding.

Nowadays, the system is perverted in most countries, including Germany. The new rule is the *tyranny of the single fault-finder*. Cut-off occurs somewhere in the range of what is rated "excellent", and reviewers who know the game recognise that, when they would want to say "solid piece of work, fund it", they have to claim "world class, no flaw, no improvement possible, excellent with stars and diamonds". These statements do not even count, because sometimes there may be an expert who is innocent, realistic, refuses to adapt to the required grade inflation, or he or she may just say wrongly that the project could be improved by also trying to gold-plate four electrodes on the twenty-atom cluster to measure conductivity by standard techniques. The grant committee is grateful because now it has variations – there is at least one criticism to justify the application getting the thumbs down. Of course, nobody wants to know about the gold-plated cluster; all that is needed is a little hesitation by one expert, and out goes another project.

You see how this means playing the original game upside down? Of course under such conditions the outcome is random. A new set of reviewers, asked to vet proposals which the first assessment had considered fundable, will have some other proposals haunted by an odd fault-finder. It might make sense to acknowledge this randomness by putting all the excellent proposals into a lottery, and letting the peer review process only go as far as consensus will carry it.

All this sounds as if a principal objective of peer review was to make the reviewers happy with the decision. This is, of course, not important. Nor is it really important that proposals are well written: the primary objective is to fund excellent research. Shortage of funds always favours the well written, results-guaranteed proposal, which is not always the most interesting.

When individual applicants do not have a reasonable chance of success, there are other undesirable effects. Morale is destroyed if one cannot be confident that quality alone is the deciding factor. Writing and vetting an unsuccessful proposal is waste of time, and usually it hits worst those people who could otherwise be enormously productive. As Konrad Weil from Darmstadt, a journal editor and committee chairman puts it: "the system is driving towards self-consistency; in the morning we write our own proposals and papers, and in the afternoon we are occupied with those of our colleagues."

Countries should decide how much research they need: that could be determined by the number of graduates with the research experience which their economy demands. Research money commensurate with such figures should be made available. But if the money is not sufficient to support the existing groups who produce research, the numbers of grant-seeking scientists will have to be reduced. Only a healthy grant-giving system which allows a honest game will produce optimal research quality. And one of the conditions is a reasonable funding probability for good projects.

M. Mahnig, Physics World (November, 1991) 14.

End of the peer production?

I was pleased to see your Forum article by Manfred Mahnig on "The tyranny of peer review" (November, p. 14). It is encouraging to read an article from a senior person in a grant funding agency letting us know, straight, what the current situation really is. I also consider the present system, in the UK and the USA at least, to be strongly "perverted", as Mahnig puts it. His proposal for a lottery as an alternative, which has also been suggested by several of my own collegues, at least acknowledges openly that we are dealing with an absurd, and very damaging, situation.

Let me be clear that I am not complaining about my own treatment at the hand of the grant funding agencies. However, the refereeing and committee decision-making process is currently an absurdity. How can it be right to ask senior professionals to give their valuable research time to comment in detail about a particular proposal, when the chances of success are so low? A typical SERC subcommittee will consists of 10-15 people of professorial or equivalent status, who will spend a whole day ordering some 50+ applications, all of which have been doubly refereed and which have been previously read by each member of the committee. Several meetings later the members will learn that somewhere between one and five (maximum) of these proposals have been funded. A typical NSF proposal goes to six referees, all of whom have to rate it "excellent" for it to stand a chance of success. Would anyone care to measure the "productivity" of such activities?

To state the problem is of course easier than to find a solution. In the UK, where the research base in universities is relatively small and where the people involved are well known to the funding agencies, there is clearly a need for a larger, and stable, funding base. I would also argue for a more personal approach, in which the committees of the funding agencies ask the (known) research groups what their needs are, on a sliding scale from what they must have to continue, to what they would like in an ideal world. New entrants to the system are almost all participating in such groups, but individual approaches to committee chairmen would be encouraged.

In the USA, where such an approach is out of the question, there is a major conflict of expectations: each new assistant professor must produce five *Phys. Rev. Letters* to get tenure, and each state university must achieve "research 1" status, preferably within the next five years, on a static science budget! An acquaintance explained to me that the previous year he had written 40 proposals, a feat which would have been physically impossible before word processors; none had been founded. I hope that either he gets somewhere, or that someone suggests an alternative strategy, before he is carried away to an asylum.

In the UK, we await a future in which the Universities Funding Council research funds are distributed (but how?) via the research councils, and in which European funding is bound to play a major role. Can we please have an open debate amongst scientists, and a realistic dialogue with politicians and the general public, on how to enable research scientists to spend their time more productively?

J. Venables, Physics World, (December, 1991) 16.

The rush to publish

As the pace of discovery quickens, molecular bilogists scramble for rapid publication – a trend exacerbated by the heightened competition among the top three journals, *Cell*, *Nature*, and *Science*.

The rush to publish goes back to the 17th century when, in an effort to force scientists to divulge their data, an obscure secretary of the Royal Society of London came up with the rule that priority goes to whoever publishes first – not to who discovers first. Researchers have been vying to publish firts ever since.

Watson and Crick's seminal 1953 paper describing the structure of DNA, for example, was published in just 3 weeks. And when high energy particle physics exploded in the 1960s, that field was gripped with a mad scramble for scientific precedence – and for instant publication – as it has been several times since (see box). But while the fast track was once largely reserved for extraordinary discoveries like the double helix, it is now becoming almost commonplace, especially for advances in molecular biology of human disease.

This trend is fueled partly by the quickening pace of discovery in that field – the tools to fish out disease genes have been available for less than a decade – and by the technological advances that make rapid publication feasible. It is aggrevated by scientists' increasing perception that mass media publications publicity can bring in badly sought grant money – as well as by pressure from the charitable foundations that want publicity for "their disease". And it is also driven, in no small part, by the relatively new and often bitter competition among journals, which exacerbates existing tensions among investigators and enables them to play one journal against the other.

While most investigators applaud the faster turnaround journals are offering, some are wondering if perhaps a good thing has gone too far. Two or three months from receipt to publication is one matter, they say; 2 or 3 weeks is another.

The critics have several gripes about this ultrafast publication. They talk of cronyism at the journals and speculate that the fast track is available just to insiders, which makes it difficult if not impossible for any but the chosen few to compete. Still other critics, are galled by the power of the editors to decide, according to their whims, which papers are "hot" enough to warrant the fast track in the first place. "What is important or not is taste, and taste today may not be taste tomorrow."

But the biggest worry is that rapid publication may, in some cases at least, be premature publication; that in their rush to publish, scientists may cut corners and the review process may be compromised, leading to incorrect or incomplete work. Stanley Pons and Martin Fleischmann's cold fusion paper, published by the *Journal of Electroanalytical Chemistry* in just 4 weeks, is a precedent no one wants to repeat.

Is there an alternative to the race? Yes, agree researchers, administrators, and at least two editors: for competing teams to arrange simultaneous publication, preferably in the same journal, as Hendrickson and Harrison did last fall for their papers on the AIDS binding site. They decided to publish jointly, Hendrickson says, because they realized that "if we went to two different journals, it would inevitably lead to a race between us and the two journals, and then other elements would enter in than who did it first, like how good was your choice of journal and your relationship to it".

While investigators and editors would prefer publication in the same journal, if two authors have unknowingly submitted their work to different journals, the editors at *Science* and *Nature* will sometimes try to coordinate publication. Simultaneous publication "probably serves everyone's needs the best", says Collins, "but it requires people to give a little". It won't work when the two teams are competing and not communicating. Nor will it work when the journal editors won't talk to each other. And that means that unless editors come up with some alternative procedures to handle these priority scrambles, as the physics journals have attempted to do, the trend is likely to be with us for some time.

Excerpt from L. Roberts, Science, 251(1991, January) 260-263

Selected fast track papers

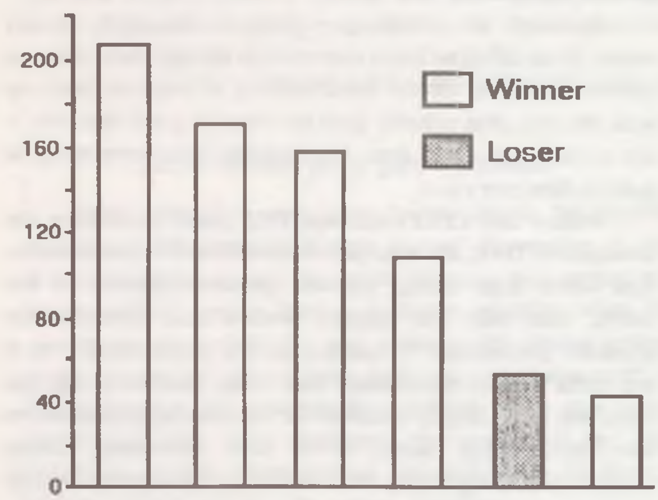
Group	Topic	Journal	Date	Turnaround
Watson, Crick	Double helix	Nature	4/25/53	3 wks
Cherwick	Noble gases	Science	10/12/62	10 days
Aaronson	V-sis oncogene	Science	7/15/83	6 wks
Waterfield	V-sis oncogene	Nature	7/7/83	3 wks
Gallo	AIDS virus	Science	5/4/83	5 wks
Fry	Chernobyl	Nature	5/15/86	4 days
Chu	Superconductivity	Phys.Rev.Lett.	3/2/87	3 wks
Sleight	Superconductivity	Science	2/26/88	17 days
Fermi Group	Z particle	Phys.Rev.Lett.	8/14/89	4 wks
SLAC Group	Z particle	Phys.Rev.Lett.	8/14/89	3 wks
Pons, Fleischman	Cold fusion	J.Elect.Chem.	4/10/89	4 wks
Tsui	CF gene	Science	9/8/89	4 wks
Collins	NF gene	Science	7/13/90	4 wks
White	NF gene	Cell	7/13/90	17 days
White	NF-gap	Cell	8/10/90	18 days
Welsh	CF-gene transfer	Nature	9/27/90	3 mo
Wilson	CF-gene transfer	Cell	9/21/90	15 days
Hendrickson	AIDS receptor	Nature	11/29/90	6 wks
Harrison	AIDS receptor	Nature	11/29/90	6 wks
Tamanoi	NF-gap	Cell	11/16/90	7 wks
McCormick	NF-gap	Cell	11/16/90	6 wks
Collins	NF-gap	Cell	11/16/90	3 wks

Academic promotion in Italy

As the actual winners pilloried in the letter on "Academic promotion in Italy" (*Nature* 353(1991) 10; *Impakt* 1(1991) (4) 8), we should like to reply to the comment and assessments made therein by the author, who was one of the losers in the competition in question. His graph showed that his four selected losers had bibliometric evaluations more weighty than ours. Because of the well-recognized 'impact factor' of *Nature*, this article has been reported by several leading Italian newspapers with vignettes of a typical professor endowed with donkey's ears.

Bibliometrics may be widely used in the evaluation of research performance but, like statistics, it is nonetheless best left to experts. Thus a bibliometric analysis carried out over a long period of time will chiefly reflect past performance, instead of present scientific activity. Moreover, for academic promotion in a specific area, the appropriate number of candidate's citations must be that in this particular area, and not the whole field of medical science. These simple criteria were not respected in the analysis presented by the loser: a period of 23 years was evaluated, with no attempt to confine the citations to a specific field. Such an approach could, for exemple, enable an ophthalmologist on the verge of retirement to win a chair of urology.

We, nonexperts, like the loser, have undertaken our own examination of the bibliographic material. We carried out a very simple count of the number of citations provided by the *Science Citation Index* during the past seven years (1984-90) for the five winners and the loser in question.



Interestingly, four of the winners had at least twice as many citations as the loser, whereas the fifth had a comparable number (see figure). It is curious that these results are diametrically opposite to those found by the loser. This is just further proof that bibliometrics is a complex matter. This is not to deny the need for improvements in the complex machinery for academic promotion in Italy. But the thinly veiled complaints of a loser cannot but cast aspersions on the reputation of all holders of academic posts.

S. Amadori, C. Bernasconi, M. Boccadoro, R. Giustolisi, M. Gobbi Nature, 355(13 February 1992) 581.

The Group of Seven's Fortunes in the Physical Sciences: 1981-90

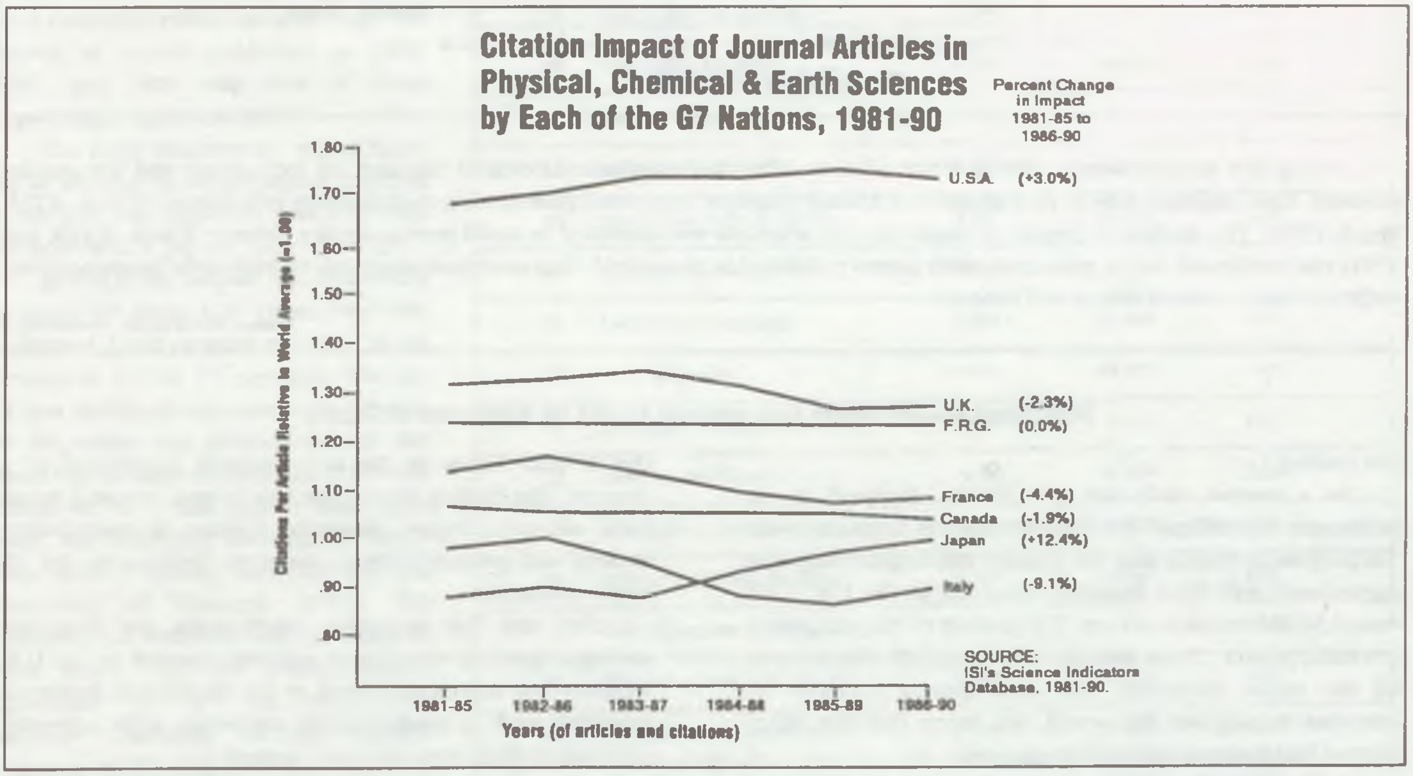
Throughout this year, *Science Watch* has examined the performance of the Group of Seven (G7) in the sciences, as measured by the output and impact of the research papers each nation published between 1981 and 1990.

The first installment in the series reported the performances of each of the G7 members in science overall (see *Science Watch*, 2[1]:1-2, January/February 1991), while the following installments detailed each country's performance in five separate fields of research, two in the physical and three in the biological sciences (see *Science Watch*, 2[2]-2[9]:8, March-September, 1991).

The former Soviet Union, not a member of the G7 but of interest to science analysts, was the focus of the last report (see *Science Whatch*, 2[9]:8, October 1991).

Now, to round off the series, *Science Watch* is once again presenting its citation-impact statistics on the G7 group in each of the five fields-but this time in a different way. This issue features the two areas of the physical sciences; the next issue features the three biological-sciences sectors. By showing how the G7 members have performed in a particular sector – rather than how a particular country has performed in all five sectors as was previously done – comparisons between nations can be more easily made.

In the fields of physics, chemistry, and geosciences, there was not a lot of change in position among the G7, according to the measure of citations-per-paper relative to the world average (see time-series chart in upper right). The exceptions were Italy and Japan: Italy fell from sixth to seventh place in the second half of the period, while Japan moved up sharpiy, taking sixth postion. In fact, if Japan's rate of increase in impact continues (+12.4% from 1981-85 to 1986-90), it will soon displace both France and Canada, which held fourth and fifth places throughout the decade.

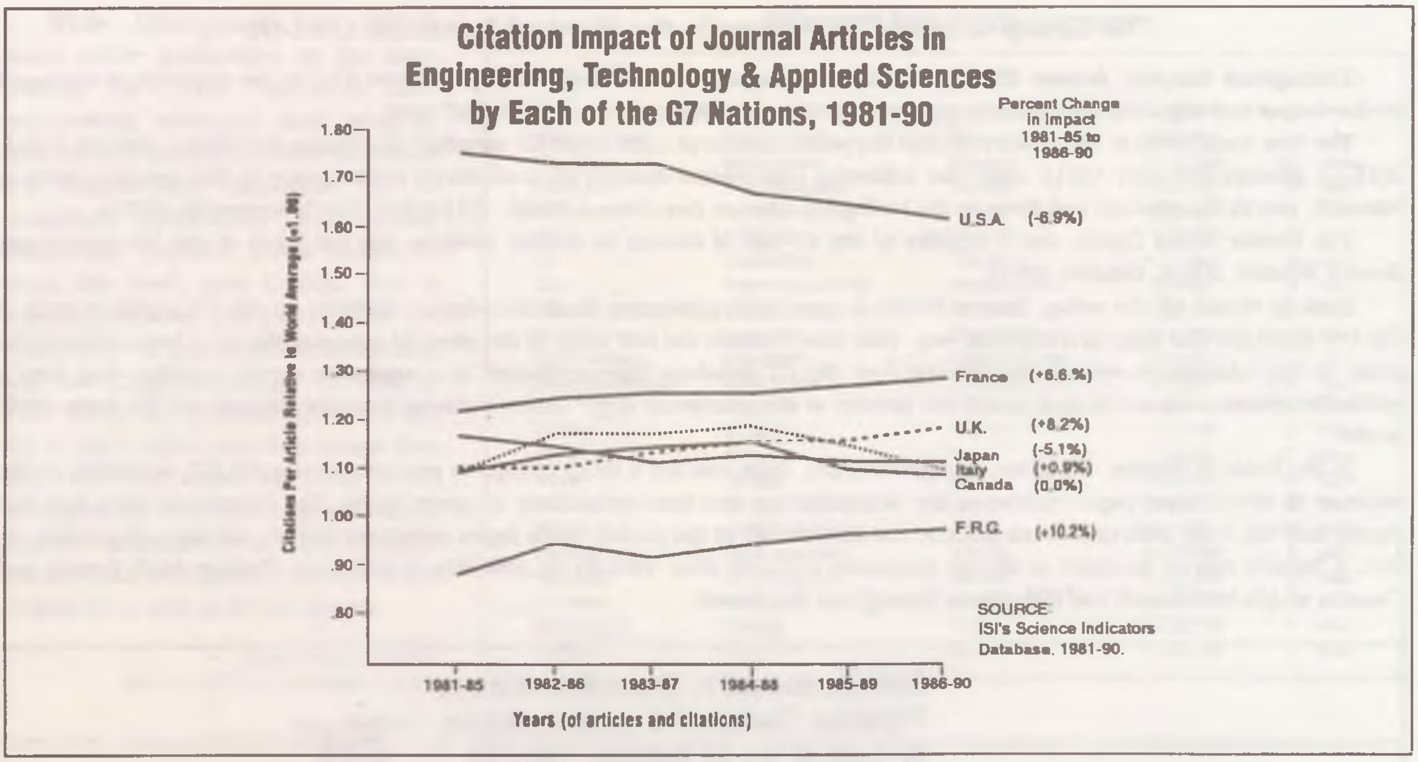


It is also noteworthy that the United States and Japan were the only G7 members to advance in terms of relative citation impact. The other five lost ground. With the United States and Japan setting the pace, it would seem that other nations must work very hard just to stay even.

In the realm of engineering, technology, and applied sciences, there was a bit more movement (see time-series chart on next page), and in this sector the United States and Japan lost ground in relative terms while four other nations advanced and one (Canada) stayed even.

Germany, which increased the most in relative citation impact during the decade (+10.2%), was bottom among the G7. On the other hand, the United States, which declined the most in impact (-6.9%), was nonetheless still first in rank. This seems to signal that the competition is tightening up in this sector.

France held second place throughout the period and exhibited a healthy increase in impact (+6.6%). The United Kingdom, which began the decade at fourth and finished at third, improved its performance even more than did France (+8.2%). Canada, Italy, and Japan traded ground in the middle range; only Japan – somewhat contrary to expectations – lost significantly in relative impact.



During her tenure, former British Prime Minister Thatcher emphasized research support for technology and the applied sciences. That emphasis may in part explain the United Kingdom's improved performance in this sector (see *Science Watch*, 2[2]:8, March 1991). The decline in impact of papers by U.S. scientists was examined in detail previously (see *Science Watch*, 2[4]:8, May 1991) and was found to rest principally with papers published in those fields that underpin advanced, or high-tech, manufacturing, such as computer-aided design and robotics.

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National technology has strong roots in national science

The finding

In a recent study for the Centre National de la Recherche Scientifique and Observatoire des Sciences et des Techniques in France, and the Science and Engineering, and Agricultural and Food Research Councils in the U.K., CHI traced 30,000 references from U.S. patents to the underlying scientific papers. These patents were issued to inventors in all the major countries, and cited papers authored by scientists throughout the world. We found that the citing pattern had a strong national component.

Each country's patents cited their own papers two or three times as often as expected, when adjusted for the size of national science. For example, German scientists author approximately 6 percent of scientific papers, while 12 percent of the scientific references from the front pages of German-invented U.S. patents are to German papers, with a resulting ratio of 2. This shows directly, for the first time, that national technology has strong roots in domestic science.

The implications

The policy implications of this finding are important. One argument for support of a domestic science program is that it contributes to the technological capabilities of a country. The finding shows that this is true: across a broad band of technologies, domestic science is contributing directly and preferentially to domestic technology, for all major countries.

This also has important implications for corporate strategy, especially since some ongoing research in the U.K. suggests that this carries down to the local level. Industrial scientists tend to preferentially cooperate with university scientists in their own cities and regions.

It has been obvious for a very long time that scientific and technological capability within a country are linked. The U.K. has a strong Pharmaceutical industry and a strong Molecular Biology research position. Germany has a major Chemical industry and a strong position in chemical publications. The Japanese have their strongest scientific performance in Engineering and Technology research. The implication of our finding is that this is not coincidential, and there is, in fact, a very strong feedback path between domestic science and domestic technology, and that companies should consider this carefully in locating their R&D facilities.

CHI's Research, New Jersey, USA, January 1992.

Clinical Medicine: The Top 50 U.S. Universities Ranked By Citation Impact, 1986-1990
(Among Those Publishing > 1,000 Papers)

The ranking above rests on a recent analysis of some 534,000 articles, reviews, and notes published from 1986 to 1990 in the approximately 900 journals of clinical medicine indexed annually by ISI for *Current Contents/Clinical Medicine*. The citations (some 2.4 million) recorded to these papers appeared between 1986 and the end of March 1991. Papers published in the first few years of this five-year period had, of course, more time to collect citations than those published at the end of the period.

Thus, citations-per-paper figures for each university reflect more strongly the impact of papers published in 1986, 1987, and 1988 than that of those appearing in 1989 and 1990.

The mean citations per paper figure for all clinical medicine papers surveyed (both U.S. and non-U.S.) was 2.48. The average for U.S. papers alone was 4.19.

In this study, *Science Watch* decided to focus on those U.S. universities that published 1,000 or more papers – or an average of at least 200 per year. The use of this threshold represents an attempt to emphasize the performance of the moderate to large universities. If a lower threshold of >500 papers had been used, the following universities would have placed among the Top 50: the University of Vermont (4.91); the University of Medicine and Dentistry of New Jersey, Nerwark (4.84); Brown University (4.71); the Uniformed Services University of the Health Sciences (4.38) and New York Medical College (3.91).

The vast majority of papers analyzed here were produced by medical school faculty within these universities. The reader should realize, however, that the foregoing does not represent a ranking of U.S. medical schools because publications in the basic biological sciences, which constitute a significant portion of the output of medical faculty (and, some say, an increasing portion), were not included here.

Rank	Institution	Papers 1986-90	Citations 1986-91	Citations Per Paper
1	Harvard University	7,084	53,996	7.62
2	Cornell Univ., New York	1,902	14,146	7.44
3	Stanford University	2,526	17,386	6.88
4	Univ. Calif., San Francisco	4,546	30,440	6.70
5	University of Washington	3,567	23,419	6.57
6	Yale University	2,834	17,990	6.35
7	Tufts University	1,884	11,822	6.27
8	Boston University	1,498	9,381	6.26
9	Univ. Calif., Los Angeles	5,206	31,032	5.96
10	Univ. Calif., San Diego	2,237	13,274	5.93
11	Baylor College of Medicine	1,678	9,916	5.91
12	Johns Hopkins University	4,486	26,103	5.82
13	Washington Univ., St. Louis	2,111	11,938	5.66
14	Vanderbilt University	1,598	9,032	5.65
15	University ot Pittsburgh	2,851	15,587	5.47
16	Duke University	2,664	14,474	5.43
17	University of Texas, Dallas	1,620	8,642	5.33
18	University of Miami	1,580	8,196	5.19
19	University of Pennsylvania	3,578	18,359	5.13
20	University of Texas, Houston	2,919	14,940	5.12
21	Univ. Minnesota, Minneapolis	3,285	16,738	5.10
22	Mayo Clinic and Med. School	4,258	21,492	5.05
23	Albert Einstein, Yeshiva Univ.	1,487	7,499	5.04
24	University of Chicago	1,545	7,779	5.03
25	Univ. of Colorado, Denver	1,631	8,143	4.99
26	Emory University	1,526	7,571	4.96
27	Univ. Mich.. Ann Arbor	3,653	17,857	4.88
28	Columbia University	2,738	13,189	4.82
29	University of Massachusetts	1,008	4,826	4.79
30	Univ. of Southern Calif.	2,658	12,665	4.76

(contd. on next page)

It is also important to explain that hospitals affilated with universities (especially with medical schools) were not automatically assigned to the affilated university unless the name of the university also appeared in an article's author-address listing. A hospital that is a subsidary of a university was, however, included with the parent university.

The hospitals that published at least 1,000 papers during 1986-90 and that ranked highest according to citation impact were: Brigham and Women's Hospital (8.17); Massachusetts General Hospital (7.24); and Hammersmith Hospital in London (5.96).

The three highest ranking non-U.S. universities among those publishing at least 1,000 papers in clinical medicine during the period 1986-90 were the Universiy of Amsterdam (6.12); McMaster University, in Hamilton, Ontario (5.27); and Erasmus University in Rotterdam (4.79)

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Rank	Institution	Papers 1986-90	Citations 1986-91	Citations Per PaaP
31	University ot Rochester	1,473	6,989	4.74
32	Indiana University	1,550	7,282	4.70
33	CUNY. Mt. Sinai	2,835	13,133	4.63
34	Univ. Utah. Salt Lake City	1,545	7,129	4.61
35	George Washington Univ.	1,050	4,768	4.54
36	University of Virginia	1,530	6,922	4.52
37	Med. College of Wisconsin	1,380	5,971	4.33
38	Univ. of Arizona, Tucson	1,134	4,862	4.29
39	Northwestern University	1,923	8,223	4.28
40	New York University	1,867	7,852	4.21
41	Univ. of Iowa, Iowa City	2,662	11,183	4.20
42	Univ. Texas, San Antonio	1,517	6,374	4.20
43	Case Western Reserve Univ.	1,589	6,641	4.18
44	Univ. N. Carolina, Chapel Hill	1,970	8,107	4.12
45	Oregon Health Sciences Univ.	1,103	4,457	4.04
46	Univ. Wisconsin, Madison	1,645	6,591	4.01
47	SUNY Stony Brook	1,143	4,510	3.95
48	Georgetown University	1,248	4,901	3.93
49	Univ. Ala., Birmingham	1,897	7,396	3.90
50	Virginia Commonwealth Univ.	1,297	4,934	3.80

"You must realize, Erskine, that preventive medicine does NOT consist of giving patients medication for diseases they don't have."

"Of course confidentiality is very important, but we have to be told what this case is about."

"It's another letter from NIH. None of my business, boss, but you really ought to think about publishing."

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