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I M P A K T

TÉNYEK A TUDOMÁNYOS ALAPKUTATÁSRÓL

Szilárd: Csak a tényeket írom le – nem azért, hogy bárki is elolvassa, csakis a Jóisten számára.
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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Szerkesztők:
Braun Tibor *(főszerkesztő)*
Schubert András *(szerkesztő)*
Toma Olga *(munkatárs)*
Zsindely Sándor *(főmunkatárs)*

Postacím:
MTA Könyvtára
1361 Budapest Pf. 7
Telefon: 111-5433
Telefax: 131-6954
Telex: 224132
E-mail: h1533bra@ella.hu

Megjelenik havonta
Évi előfizetési díj: 2400 Ft

Bibliometric and other science indicators and their role in research evaluation.

Recent reductions in research budgets have led to the need for greater selectivity in resource allocation. Measures of past performance are still among the most promising means of deciding between competing interests. Bibliometrics, the measurement of scientific publications and of their impact on the scientific community, assessed by the citations they attract, provides a portfolio of indicators that can be combined to give a useful picture of recent research activity. In this state-of-the-art review the various methodologies that have been developed are outlined in terms of their strengths, weaknesses and particular applications. The present limitations of science indicators in research evaluation are considered and some future directions for developments in techniques are suggested.

Main problems with the various partial indicators of scientific progress and details of how their effects may be minimized

Publication counts

Problem	*How effects may be minimized*
(1) Each publication does not make an equal contribution to scientific knowledge	Use citations to indicate average impact of a group's publications, and to identify very highly cited papers
(2) Variation of publication rates with speciality and institutional context	Choose matched groups producing similar types of papers within a single specialty

Citation analysis

Problem	*How effects may be minimized*
(1) Technical limitations with Science Citation Index:	Not a problem for research groups
(a) first-author only listed	
(b) variations in names	Check manually
(c) authors with identical names	
(d) clerical errors	Not a serious problem for 'Big Science' journals
(e) incomplete coverage of journals	
(2) Variation of citation rate during lifetime of a paper – unrecognized advances on the one hand, and integration of basic ideas on the other	Not a problem if citations are regarded as an indicator of impact, rather than quality or importance
(3) Critical citations	
(4) 'Halo effect' citations	
(5) Variation of citation rate with type of paper and speciality	Choose matched groups producing similar types of papers within a single speciality
(6) Self-citation and 'in house' citation (SC and IHC)	Check empirically and adjust results if the incidence of SC or IHC varies between groups

Peer evaluation

Problem	*How effects may be minimized*
(1) Perceived implications of results for own centre and competitors may affect evaluation	Use a complete sample or a large representative sample (25% or more)
(2) Individuals evaluate scientific contributions in relation to their own (very different) cognitive and social locations	Use verbal rather than written surveys so can press evaluator if a divergence between expressed opinions and actual views is suspected
(3) 'Conformist' assessments (e.g.: 'halo effect') accentuated by lack of knowledge on contributions of different centres	Check for systematic variations between different groups of evaluators

(contd. on next page)

Accountability in science

Prior to the 1960's, funding for scientific research in the UK was relatively unrestricted. Resources were allocated largely on the basis of internal scientific criteria (criteria generated from within the scientific field concerned), which were evaluated by the peer review process. Various factors have resulted in a need for greater selectivity in the allocation of funds: an increase in the capital intensity of research (e.g. the 'sophistication' factor; the growth of 'Big Science'); expanding objectives and opportunities, with many new fields emerging; an increase in collaborative, often multidisciplinary, research projects which require coordination; a coalescence of basic and applied research, with much research now of a strategic nature (i.e. long-term basic research undertaken with fairly specific applications in view); finally, economic constraints require choices to be made between different disciplines, fields and research proposals. The peer review process has thus come under increasing pressure.

In 1963, Weinberg, anticipating the present crisis, suggested that both internal and external criteria be used in making such choices [1]. These criteria are related to the questions: "How well is the science done?" (internal criteria) and "Why pursue this particular science?" (external criteria). While placing greater emphasis on external criteria such as social or technological merit, he identified two obvious internal criteria as: (1) "Is the field ready for exploitation?" and (2) "Are the scientists in the field really competent?". It is this second question in particular that lends itself to quantitative analysis using bibliometric and other science indicators.

The peer review system

The peer review system has been critized on several counts:

(1) The partiality of peers is an increasing problem as the concentration of research facilities in fewer, larger centres makes peers with no vested interest in the review increasingly difficult to find.

(2) The 'old boy' network often results in older, entrenched fields receiving greater recognition than new, emerging areas of research, while declining fields may be protected out of a sense of loyalty to colleagues. Thus the peer review process may often be ineffective as a mechanism for restructuring scientific activity.

(3) The 'halo' effect may result in a greater likelihood of funding for more 'visible' scientists and for higher status departments or institutes.

(4) Reviewers often have quite different ideas about what aspects of the research they are assessing, what criteria they should use and how these should be interpreted. The review itself may vary from a brief assessment by post to site visits by panels of reviewers.

(5) The peer review process assumes that a high level of agreement exists among scientists about what constitutes good quality work, who is doing it and where promising lines of enquiry lie, an assumption that may not hold in newer specialities.

(6) Resource inputs to the review process, both in terms of administrative costs and of scientists' time, are considerable but usually ignored.

Various studies have addressed these criticisms. For example, an analysis of National Science Foundation (NSF) review outcomes found little evidence of bias in favour of eminent researchers or institutions [2]. However, a later study, in which independently selected panels re-evaluated funding decisions by NSF reviewers, found a high level of disagreement among peers over the same proposals. It concluded that the chances of receiving a grant were determined about 50% by the characteristics of the proposal and principal investigator and 50% by random elements i.e. the 'luck of the reviewer draw' [3]. The interpretation of this data has been questioned, however, and the fact that most disagreement was found in the mid-region between acceptance and rejection emphasized. In another study, a sample of NSF reviewers and applicants was asked which of two equally good proposals the individuals thought was more likely to be funded (a) from a well-known versus a little known institution and (b) containing mainstream versus radical ideas. The prospects of the proposal from the better known institute and with mainstream views were favoured by the majority of respondents [4]. Several studies of journal referees also found levels of agreement little better than would be expected by chance, while in another report, anonymously-authored papers, identical except for their results, which either agreed or disagreed with the (presumed) standpoint of the reviewer, were less critically examined and received higher ratings when they conformed to the reviewer's point of view [5].

Thus, while the authors of a recent NSF report reaffirm their belief in the effectiveness of its peer review process, (henceforth to be termed 'merit review' to encompass extrinsic as well as intrinsic criteria), a number of problems clearly exists. The peer review process remains an essential element in assessing the quality of science, but improvements to the system are warranted. These might include:

(1) The right of reply by researchers to criticisms of their proposals, a system already operating for grant applicants to the NSF (USA) and the ZWO (Netherlands).

(2) The use of external peers, from neighbouring fields and other countries.

(3) Clear guidelines on the criteria to be employed.

(4) The use of objective scientific indicators to complement the peer review process.

Science indicators

Indicators of scientific activity include measures of research inputs, such as funds, researchers, technical support staff, equipment etc., and of research outputs, for example contributions to scientific knowledge, to education and to technology. For these indicators to be used for assessment purposes, a comparative approach is required since, in basic science, no absolute quantification of research performance is possible.

Comparison may be made between similar research groups or institutes, between countries and/or over time.

Research inputs

The Organisation for Economic Cooperation and Development (OECD) has attempted to up-grade and standardize the research input data which it collects routinely, focusing on R&D expenditures and on people working in R&D [6]. However, both these indicators remain problematical. For example, it is difficult to determine the proportions of university capital expenditures, overheads and central facilities that are devoted to research as opposed to teaching or administration.

International data are confounded by differing funding systems and salary levels, fluctuations in the rate of exchange and variations in the relative costs of equipment.

A further problem arises from inconsistencies in the international classification of research areas where these can produce a distorted picture of research expenditure within a particular field. With labour costs accounting for more than half of total R&D expenditures, and average spending per researcher for the OECD area, (including support staff and equipment) standing at around $100000 in 1981 [7], research personnel constitute a major input to R&D. Yet no uniform definition exists for international comparison, with some countries classifying researchers by occupation and others by level of formal education. For R&D performed by academics, it is difficult to estimate the proportion of total working time devoted to research as compared to teaching and administrative duties. In the UK an earlier estimate of 30%, based on a time-budget survey, has been re-evaluated to around 40%.

Research outputs

These include bibliometric indicators (publication counts, citations), and for applied research, patents. In addition, 'measures of esteem' (invitations to speak at international conferences, attraction of outside funding and visiting researchers, honorific awards etc), may prove useful in assessing research quality. The requirements for a methodology for systematically collecting data on research outputs include that it should be: relatively inexpensive (less than 1%, say, of the research expenditure being assessed); routinely applicable, and capable of focusing on any clearly-defined field. The data thus generated should be: policy-relevant; internationally comparable; capable of identifying time-trends (over at least 6 to 8 years); capable of disaggregation so that they can focus on individual research groups or research techniques (e.g. theoretical *versus* practical work, use of different instrumentation); publicly accessible and easily understood. While no such data collection system is currently in operation, interest has focused of late on the potential role of indicators of research output in research evaluation and science policy formation.

Bibliometric indicators

Rudimentary bibliometric studies have been undertaken almost since the beginning of scientific publishing and publication and citation counts were being used in the early decades of this century to asses scientific activity. In the 1960s De Solla Price's classical essays [8] on the quantification of science and in particular of scientific output and communication, aroused considerable interest in the significance of publication distributions. However, it was with the development in the USA of the Science Citation Index (SCI) in the 1960s and subsequently of the Computer Horizons Inc (CHI) database that bibliometric data covering the world's leading scientific journals became readily available. Interest in bibliometric techniques was further stimulated by the use of literature indicators of national scientific performance in the Science Indiactors reports, sponsored by the NSF [9]. There was, in addition, a growing perception throughout the 1970s of the need for objective measures of scientific productivity and merit.

Publications counts

The simplest bibliometric indicator is the number of published articles that a researcher or group has produced. For basic research the journal article with its accompanying list of citations has always been the accepted medium by which the scientific community reports the results of its investigations. However, while a publication count gives a measure of the total volume of research output, it provides no indication as to the quality of the work performed.

Other objections include:

(1) Informal and formal, non-journal methods of communication in science are ignored.

(2) Publication practices vary across fields and between journals. Also, the social and political pressures on a group to publish vary according to country to the publication practices of the employing and to the emphasis placed on number of publications for obtained tenure, promotion, grants etc.

(3) It is often very difficult to retrieve all the papers for a particular field, and to define the boundaries of that field in order to make a comparative judgement i.e. the choice of a suitable database is problematical.

(4) Over the past few decades the number of papers with multiple authorship has increased considerably. Although this is largely due to a greater prevalence of collaborative research, the gratuitous conferring of co-authorship is not uncommon. Another recent trend had been the shrinking length of papers, resulting in the emergence of the 'Least Publishable Unit'. This may be associated with various factors including fragmentation of data. Thus, an awareness of the use of publication counts for assessment may encourage undesirable publishing practices.

Citation analysis

This involves counting the number of citations, derived from the SCI, to a particular paper for a period of years after its publication. The exact period will vary from field to field, since the time lag between publication and maximum number of citations received in a year differs between specialities. One proponent of the methodology considers citation analysis to be a very useful tool: "Metaphorically speaking, citations are frozen footprints in the landscape of scholarly achievement; footprints which bear witness to the passage of ideas. From footprints it is possible to deduce direction; from the configuration and depth of the imprint it should be possible to construct a picture of those who have passed by, whilst the distribution and variety furnish clues as to whether the advance was orderly and purposive. So it is with citations in respect of the growth of human knowledge; they give substantive expression to the process of innovation, and, if properly marshalled, can provide the researcher with a forensic tool of seductive power and versatility" [10(p.25)]. A recent UK report on the national performance of basic research defines four stages in the citation analysis of a research field:

(1) identification of a suitable field and definition of that field in usable, operational terms;

(2) application of the operational definition to construct a (preferably comprehensive) bibliography confined to appropriate types of publication (e.g. original papers in basic research journals);

(3) examination of the citation record of each item in the bibliography;

(4) analysis of the results.

However, citation analysis presents a number of serious technical and other problems beyond those already discussed for publication counts.

Reasons for citing

(a) Citation analysis makes the assumption that an intellectual link exists between the citing source and reference article, but the act of citing is essentially subjective and is probably governed by a combination of normative (i.e. adhering to a set of universal norms) and particularistic (inconsistent, personal) considerations. Cronin [10] reviews 10 different classifications of reasons for citing, most of which favour the normative view, while one lists a variety of particularistic citing stratagems, including 'hat-tipping', over-elaborate reporting and citing the most popular research trends to curry favour with editors, grant-awarding bodies etc. Murugesan and Moravcsik [11] have produced a four-fold classification, based on citation context analysis which is fairly comprehensive:

(1) Conceptual/Operational (theory)/(method)
(2) Organic/Perfunctory (essential)/(non-essential)
(3) Evolutionary/Juxtapositional (development of idea)/(contrasting idea)
(4) Confirmate/Negative (supports findings)/(opposes findings)

They found that 41% of citations to 30 articles in the *Physics Review* were in the 'perfunctory' category. On the other hand, a study of references in the first 4 volumes of *Science Studies* found that 80% were either substantiating a statement or assumption, or else were pointing out further information [12].

(b) Work that is incorrect may he highly cited. However, it has been argued that even incorrect work, if cited, has made an impact or has stimulated further research efforts, while work that is simply of bad quality is usually ignored by the scientific community [13].

(c) Methodological papers are among the most highly cited, which, it is argued, reflects their considerable 'usefulness' to science. Conversely, many techniques and theories become assimilated into the science knowledge base and their originators cease to be acknowledged (the 'obliteration phenomenon').

(d) Self-citation may artificially inflate citation rates; in one study of psychology papers, 10% of of authors self-cited for more than 30% of their citations [14].

Database limitations

While all publications databases are subject to various weaknesses, such as typographical errors, inconsistent or incomplete coverage of the literature etc., such problems are particularly serious in the case of the SCI since it is the primary source of all citation data. The following factors have been stressed:

(a) A considerable number of citations may be lost in an automated search of the SCI, due to such problems as homographs (authors with the same name), inconsistency in the use of initials and in the spelling of non-English names and citing errors.

(b) There have been substantial changes in the journal set covered by the SCI, with some journals dropped and a larger number of new ones added, so that no consistent set exists. Also, since 1977 non-journal material e.g. conference proceedings, books etc. were included in the database.

(c) The SCI journal set shows a considerable bias in favour of U.S. and other English-language journals and against journals from the U.S.S.R. and other countries with non-Roman alphabets.

Field-dependent factors

(a) Citation (and publication) practices vary between fields and over time. For example, biochemistry papers now generally contain about 30 references whereas mathematics papers usually have less than ten [13]. A study of one Dutch university's publications found that 40% of mathematics articles, while written in English, were not published in journals but in research reports, and therefore were not covered by the SCI. Coverage of physics and astronomy publications, however, was over 80% [15].

(b) The probability of being cited is also field-dependent; a relatively small, isolated field will attract far fewer citations than either a more general field or research within a narrower field that has a wider focus of interest.

(c) The decay rate of citation frequency will vary with each field, and with the relative impact of the paper, i.e. a little cited paper will become obsolete more rapidly than a more highly cited one [16].

The citation rate of a paper may be considered a partial measure of its 'impact' (rather than of its 'quality' or 'importance'), where impact is defined as "its actual influence on surrounding research activity at a given time" [17].

High citation counts have been shown to correlate closely with recognized quality indicators such as honorific awards, including the Nobel Prize. Similarly, significant positive correlations between the aggregate peer ratings of departments or institutes and the citation frequencies of their members have been reported [18]. Also, studies of non-aggregated peer assessments of individuals and of individual papers have been shown to correlate positively with citation counts [19]. Citation analysis has been used to demonstrate that an individual who had been denied tenure had performed better than successfull colleagues and to refute the 'Ortega hypothesis', which states that much of scientific progress is due to the small contributions of a relatively large number of 'average' scientists [20]. In fact, 15% of papers are never cited, and the average annual cilation rate for papers that are cited is a mere 1.7 [13].

In the Netherlands, [15] and in Hungary [21], research groups within departments or faculties have been compared using publication and citation analysis. Where such small groups are concerned, extreme care must be taken to recover all relevant data, since a missing highly cited paper would greatly distort the results. It has been suggested that rates of 99% and 95% coverage for publications and citations respectively are required [22].

Patent analysis

Patent data may be used directly to indicate scientific and technological activity in applied research and development, or the citations in patent applications and examiners' reviews may be used as an additional source of material for the bibliometric analysis of basic research. Limitations to the value of patent data as an index include inter-industry differences in patenting activity and international differences in conditions and charges for patenting. Also, not all inventions are patented or patentable.

However, a study of possible biases in patenting activity found a close inter-country correlation between foreign patenting and R&D expenditure as well as a close inter-industry correlation between foreign and domestic patenting. These results suggest that the concept of foreign patenting is a useful and relatively unbiased addition to the existing array of science and technology output indicators [23]. Patent data were used to develop a 'Revealed Technology Advantage (RTA) Index' in 10 OECD countries, showing each country's relative share of total foreign patents granted in the USA for each of 41 sectors. Thus an RTA Index greater than one showed a sector with a comparative technological advantage whereas an RTA index less than one represented a comparative disadvantage. Innovative success is often associated with good quality research; for example patent applications and numbers oi citations received were strongly correlated for a group of researchers/inventors at a US University of Technology [24].

Patent Citation Analysis

In its study of national performance of basic research, the Royal Society used patents granted in the USA to obtain a different set of citations from that derived from journal references. Papers cited by both applicants and examiners were analysed. While applicants were thought more likely to select their references rigorously, since they would presumably undergo a more thorough scrutiny than the average journal reference, examiners on the other hand were thought to have a more limited literature knowledge base, tending to cite reference books, conference proceedings, abstracting journals and existing patents. Although patent citation analysis was not useful for solid state physics, due to insufficient data, in the field of genetics it yielded interesting information on international citing patterns, and the cited literature was both recent and basic. The data were found to be consisitent with, and to 'usefully complement' the data obtained from journal to journal citation analysis, although the methodology was more labour intensive.

Measures of esteem

A group of indicators collectively termed 'measures of esteem' relates to the recognition by the scientific community of work of outstanding quality.

Invited papers at international conferences

International meetings are well-established and important channels for the communication of scientific information among individuals and between nations, and are often used for the rapid dissemination of results before their eventual publication in journals. The growth of the scientific community and of international travel has resulted in a doubling of the number of scientific meetings held worldwide in the late 1960s to around 10000 by the late 1970's [25]. An invitation to present a paper at an international meeting implies that the invited scientist is held in high regard by his peers in the international community. To identify trends over time, a series of conferences held regularly would need to be analysed. The Royal Society analysed invited papers to the Cold Spring Harbour Symposia in New York by author nationality, and the UK papers delivered at a series of International Physics Conferences. The major constraint to the use of this indicator is the limited number of such conferences.

Migration

The short-term and long-term migration of scientists partly reflects their perceptions of the relative scientific opportunities of the country to which they move. While permanent migration (the 'brain drain') has in the past received most attention, the short-term migration of scientists is both more common and probably more intellectually productive. Thus, an analysis of the numbers, length of stay and standing of their home research institute, for visiting researchers to UK research groups, and a similar analysis for UK researchers visiting abroad, might provide useful data on the esteem in which a UK research group is held internationally.

Attraction of outside funding

The willingness of outside agencies to support research at a department or institute is a measure of its scientific reputation. This indicator, together with a peer assessment of the top 5 departmental publications, was the main method used by the University Grants Committee in its controversial grading of research, and interdepartmental allocations of resources are now being made on this basis at some UK institutions.

Honours and professional status indicators

These include international prizes, membership of professional societies, editorial boards, review panels etc. Limitations to their use, as with other 'measures of esteem' include (1) the involvement of other factors besides scientific merit and (2) insufficient data. However universities and institutes of higher education in various countries now combine bibliometric indicators with such 'measures of esteem' in their evaluation procedures, in recognition of the need for public accountability.

Conclusions

My purpose has bean to outline the different types of science indicators and various methodologies that have been used in the assessment of scientific performance. Most bibliometric studies have operated at the level of the scientific speciality or field, and the reliability of results obtained by applying bibliometric techniques to small numbers of publications has raised serious doubts, due to conceptual and technical problems. However, many managers are today confronted with the need for objective indicators, to complement the peer review process, for small or medium-sized, often multi-disciplinary, research groups. There is therefore a need to develop reliable, preferably field-independent, indicators for use at a disaggregated level. In addition, there is a need for these indicators to be generated on a routine basis; a major constraint to such a system is the labour-intensive nature of the currently accepted manual methodology. The development of online techniques, with minimal and acceptable rate of error, for both publication and citation retrieval, is urgently required; more consistent inter-database formatting and indexing for publications would greatly facilitate this process. It would also be useful to assess whether sampling significantly alters the result obtained from complete sets of publications and citations (e.g. taking peak citation years only), since this would substantially reduce the volume of data to be handled. In terms of research inputs, there is a need for consistent and comparable data at a relatively disaggregated level, to facilitate meaningful input-output assessments. Finally, there is an urgent need for debate among research managers and policy makers about the role that science indicators should play, both in terms of the weight they should carry relative to, for example, peer review, and the level at which they should be incorporated into the decision-making process.

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Hogyan osszuk szét az akadémiai támogatást kutatócsoportok között
(Egy közvéleménykutatás eredményei a Központi Kémiai Kutatóintézetben)

A tudományos cikkek, mint a természettudományi információ "kvantumai", a tudományos kutatások eredményeinek írásos dokumentumai. A tudományos publikációk kvantitatív értékelése az MTA KKKI-ban már hosszú múltra tekinthet vissza. Korábban – míg lehetett – a kutatók éves jutalmának kiszámításánál vettük tekintetbe az egyes csoportok által publikálásra használt folyóiratok hatástényezőit, továbbá az egyes cikkekre érkezett idézeteket. Jelenleg az MTA-támogatás (mintegy 100 MFt) egy részének a tudományos szervezeti egységek közötti szétosztása történik az előbb említett két tényező révén. Számos kutató részéről felmerült az az igény, hogy a jelenleginél (ha lehet) jobb, pontosabb értékelési rendszert dolgozzunk ki és növeljük a tudományos értékelés alapján az egyes részlegeknek juttatott támogatást. Az 1991-ben alkalmazott felosztás ugyanis mintegy 50%-ban a kutatói létszámot, 33%-ban a folyóiratok hatástényezőit és 17%-ban a relatív idézettséget (idézetek tényleges száma a folyóiratok hatástényezőinek összegéhez – ún. elvárt idézettség – viszonyítva vette figyelembe. Ahhoz, hogy az értékelés módszereit kidolgozhassuk, először egy közvéleménykutatást végeztünk. A mintegy 180 kutató közül 59-en válaszoltak a következő kérdésekre. (Akadnak kérdések, amelyekre nem mindenki válaszolt.)

1.) *Egyetért azzal, hogy az MTA költségvetési támogatás (KT) szétosztásánál szerepet játsszon az egységek korábbi tudományos teljesítménye?*

igen:	55	93%
nem:	3	5%
nincs véleménye:	1	2%
összes válaszadó:	59	100

2.) *A tudományos teljesítmény értékelésének alapján a KT hány %-át osszák szét?*

100 %-ot:	14	24%
több, mint 50 %-ot:	21	36%
50 %-ot:	22	38%
kevesebb, mint 50 %-ot:	1	2%
összes válaszadó:	58	100%

3.) *A KT felosztásánál a szakértői vélemény milyen mértékben játsszon szerepet?*

kizárólagos:	5	9%
nagyobb részben:	9	16%
felerészben:	4	7%
kisebb, mint felerészben:	3	6%
egyáltalán nem:	34	62%
összes válaszadó:	55	100%

4.) *A KT felosztása a közleményeket publikáló kiadványok színvonala* szerint történjék?

kizárólag így:	4	8%
nagyobb részben így:	25	47%
felerészt így:	15	28%
kisebb, mint felerészben:	7	13%
máshogyan:	2	4%
összes válasz:	53	100%

5.) *Ismeri-e a "folyóiratokra vonatkozó hatástényező" definícióját?*

igen:	41	70%
részben:	16	27%
nem:	2	3%
összesen:	59	100%

6.) *Alkalmasak-e a folyóiratok hatástényezői a tudományos teljesítmény mérésére?*

igen:	9	15%
részben:	43	73%
nem:	5	9%
nem tudja:	2	3%
összesen:	59	100%

7.) *A KT felosztása az idézetek alapján történjen?*

kizárólag	1	2%
nagyobb részt:	5	9%
fele részben:	17	31%
kisebb, mint felerészben:	28	51%
egyáltalában ne így:	4	7%
összes válasz:	55	100%

8.) *Alkalmas-e az idézetek összes száma arra, hogy a tudományos teljesítmény mértékére és színvonalára következtethessünk?*

igen, de csak a színvonalat tükrözi:	0	0%
igen, de csak a mennyiséget tükrözi:	1	2%
igen, mindkettőt helyesen tükrözi:	8	14%
igen, helyesen tükrözi mindkettőt, de korrekciók kellenek:	41	72%
nem, teljesen hamis:	4	7%
nem tudja:	3	5%
összes válasz:	57	100%

A válaszokból (2. kérdés) egyértelműen az következik, hogy azok vannak többségben, akik a költségvetési támogatás szétosztásánál a *tudományos teljesítmény* alapján több mint 50%-ot osztanának szét. A szakértői vélemény szerinti értékelést kevesen pártolják (3. kérdés). Érdemes néhány megjegyzést, javaslatot felsorolni, amelyeket a megkérdezettek a pénz felosztásának mikéntjére vonatkozóan tettek a 3. kérdéshez kapcsolódva: (zárójelben az adott véleményen lévők száma)

- legyen témapályázat, a témákat egy intézeti bizottság bírálja el (1),
- ne szervezeti, hanem tematikai egységek szerint osszák el a pénzt (1),
- az Intézeti Tanács (IT) ossza el a pénzt (4),
- az IT-ben az érdekeltek képviselete nem arányos (1),
- az Igazgatóság ossza el a pénzt (4),
- külső bírálóknak pályázatok alapján kialakított véleménye szerint osszák fel a pénzt (7),
- erősen függne az intézeti erőviszonyoktól és szubjektív ítéletektől, ha pályázatok alapján osztanák el a pénzt (3),
- a témák heterogének tematikailag, sok bíráló különféle szempontok szerint bírálna, ezért a bírálati rendszer nem alkalmazható (1),

– a bírálók a pályázó által kiválasztott, az utóbbi 5 évben megjelent 2-3 legjobb cikk alapján bírálják (1),
– a külső szakértői véleményekre támaszkodva a főigazgató felelős döntéssel minősítse az egyes kutatásokat (1).

A 4., 5. és 6. kérdésre adott válaszok azt a véleményt erősítik, hogy a KKKI kutatói tisztában vannak a folyóiratok színvonalát jellemző hatástényezők értékével, és nagyobb részük elfogadja, hogy az összes támogatást felerészt/nagyobb mint felerészt ennek megfelelően osszák fel. Ugyanakkor egyértelmű, hogy kevésbé bíznak az idézeteknek értékelésre való felhasználhatóságában (7., 8. kérdés). Ezzel kapcsolatosan néhány megjegyzés:

– az idézetek *négyzetgyöke* tükrözi helyesebben a színvonalat és a mennyiséget,
– az idézet fontos kvalitatív információ, de nem kvantifikálható,
– sok függ attól, mennyire "divatos" egy téma.

Az előzőek ellenére az azért mégiscsak figyelemre méltó, hogy a válaszadók 72%-a nyilatkozott úgy, hogy az idézetek összes számából korrekciók alkalmazásával, de a tudományos teljesítmény mennyiségére és minőségére következtethetünk. Annál inkább figyelemre méltó az előző adat, ha azt is tekintetbe vesszük, hogy 14 % úgy véli: mind a két tényezőt helyesen tükrözi az idézetek összes száma (8. kérdés).

A közvéleménykutatás eredményeit és a megfelelő intézeti bizottságok és fórumok mérlegelését figyelembe véve 1992-re várhatóan mintegy 20-30%-át fogják majd a költségvetési támogatásnak kutatói létszám arányában, mintegy 50%-át azoknak a folyóiratoknak a hatástényezői alapján, ahol az illetők cikkei megjelentek, és mintegy 20%-át pedig az idézetek alapján szétosztani a tudományos szervezeti egységek között. Az egyes módszertani kérdések részletes tiszázása (a pontosan mit és hogyan számoljunk) azonban még hátra van.

Vinkler Péter, KKKI

The ancient art of peer review

On expiration of the statutory 350-year nondisclosure period, the Medical Research Council recently released papers which bear witness to its conservative attitude on progress. Among them is a grant application submitted by the then Regius Professor of Anatomy at the London College of Physicians, one Guilielmus Harveius. The evaluation by one of the MRC's external referees is given below; according to MRC rules, his name is withheld.

Title of proposed research

Anatomical exercises concerning the motion of the heart and blood.

Applicant

Guilielmus Harveius, BA (Cantab 1597), MD (Padua 1602).

Summary of proposed research

The applicant intends to perform a series of experiments on animals to provide evidence for, and to confirm, some of the postulates concerning the flow of blood and the function of the heart which he has publicised in his position as Lumleian Lecturer at the Royal College of Physicians since 1615, namely that, (1), the volume of blood which passes from the veins to the arteries per unit time is too great to be produced from the food consumed, (2), the volume of blood going into the extremities is far in excess of their actual metabolic needs, and (3), arterial blood returns to the heart from the extremities through the veins.

Dr Harvey will measure the volume of blood he claims is ejected by the heart, per beat and per time. He also plans to study the emptying and subsequent refilling of superficial blood vessels in the forearm and he hopes that he will find a pattern such as to support his notion of the circulation as a hydraulic system.

Experience and qualifications of applicant (pertinent to the project submitted)

Dr H. is a distinguished anatomist, has been appointed the King's Physician in Ordinary and practises general medicine at St Bartholomew's Hospital in London. However, he has not received any formal training in physiology and he is not known a' ın astute experimentalist.

The applicant bases his thesis exclusively on modern findings in descriptive anatomy as presented by Andreas Vesalius, Realdo Colombo and Fabricius ab Aquapendente of the University of Padua, his *alma mater*. He has not availed himself as is customary of the privilege of attending lectures at other European universities after graduation, and he might therefore not be wholly familiar with accepted thought on the functions of the blood and the heart.

While qualified to supervise the work of surgeons at his hospiral practice, Dr H. does not himself appear to possess the manual skills which are required to execute the experiments he plans to perform. The scatter of the preliminary data submitted, for example, on the pulse rate and the cardiac output of sheep, shows him as a rather careless experimenter without much concern for statistical analysis.

Critical evaluation of proposed research

The applicant's theory re the existence of a contiguous circulatory system challenges the view, held by all physicians since Galen, that venous blood is alimentary, produced in the liver from food absorbed in the intestines, and endowed with the spirit of vitality through a connection with the arterial system provided by pores in the walls which separate the heart's three ventricles. He claims never to have observed pores or holes in the heart, a statement that is contradicted directly by my own vast professional experience: I find a direct connection between one of the arteries and the principal vein right above the heart in the majority of autopsy cases available for study, mainly pre-term infants.

By contrast, I have never seen any evidence for the existence of blood vessels providing a link between arteries and veins in the periphery of the body, as is stipulated by the scheme of Dr H.; such vessels would have to be of an extremely fine calibre to escape the eye and would pose an insurmountable mechanical resistance to the flow of blood. If, alternatively, arterial blood were to soak the tissue directly, without being contained in blood vessels, one should be able to extract a substantial volume of blood from the isolated organ when squeezing it like a sponge. This is clearly not the case.

Dr H. thus presents us with a circular argument: he opposes accepted doctrine as invalid because he has not been able to see one of its essential features. By contrast, he embraces a theory whose one essential feature cannot be seen, namely the minuscule blood vessels in the periphery of the circulation which supposedly form the connection between arteries and veins.

The applicant provides some experiments as indirect evidence for the existence of a contiguous blood circulation scheme. Some superficial blood vessels in the forearm fill from the side distal to the heart after they were blocked temporarily by point pressure. This result cannot be accepted as the desired proof, and I should ask the reader's indulgence for mentioning the parable of the three blind men describing an elephant. The fact that a circulation loop might exist in the extremities cannot be used as an argument in favour of the existence of such loops everywhere else in the organism.

Final assessment

The opinions expressed in the application are highly speculative. Critical to the applicant's theory is a presumed connection between the arterial and venous sides in the periphery of the putative circulation. It is improbable that the experimental work proposed will prove the existence of such a link.

Given the facts that the applicant has not had any postdoctoral training, has practically no research experience and has no publications by which to judge his research competency, I think that the chances of bringing this project to a successful conclusion are minimal.

I therefore give the application as submitted a rating of 3 on a scale of 1 (low) to 10 (high score) and I recommend that funding be denied.

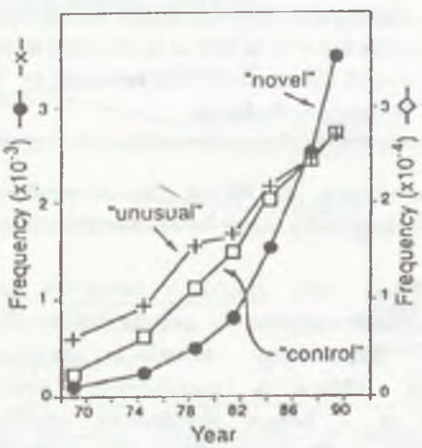
Wolf D. Seufert
New Scientist, (4 January 1992) 39

So what's new?

A feature that distinguishes scientific journals from breakfast-cereal boxes is a preference for the word 'novel' over terms such as 'brand new'. This preference is accelerating, as is evident from the figure, where the frequency of 'novel' in titles and abstracts catalogued in Medline is compared to the frequency of a control word ('control') over the past 25 years. The recent popularity of 'novel' is likely to be an inflationary increase rather an explosion in the number of extraordinary discoveries. For one thing, the increase appears to come at the expense of less conspicuous synonymus, such as 'unusual', which shows a decline in popularity relative to 'control'. For another, 'novel' is especially favoured in journals that are noted more for solid science than for novelty. In the most recent month for which information is available, the *Journal of Biological Chemistry* and *FEBS Letters* each accounted for twice as many 'novel' papers (10) as the combined total for *Nature*, *Science* and *Cell*.

If this expanding use of 'novel' is allowed to continue unchecked, one can foresee that in the near future its presence on a paper will be *de rigueur*, and the word will lose its meaning entirely. To prevent this from happening, we recommend that authors reserve 'novel' for strikingly new discoveries and consider more appropriate terms to describe observations that range from somewhat atypical to unusual. We should also keep in mind that at the supermarket, novelties often end up at the rear of the ice-cream freezer.

Steven Henikoff, and Robert Levis
Nature 350 (7 March 1991)



Doctor who?

Many of us must wonder what proportion of the references that adorn scientific papers have actually been looked up, let alone read and inwardly digested. An example has recently come to my attention of the creation of an entirely new scientific personality as a result of casual citation. A number of articles concerned with fibroblast growth factor have referred in the past few years to the first observation of mitogenic activity in brain extract as having been made by Trowell, Chir and Willmer in 1939 [1]. It is probable that the middle author made only a limited contribution to this work as he is actually the surgical qualification held by the first author. In those days, *Journal of Experimental Biology* listed the degrees of the authors, and so their names on the title page read "O.A. Trowell, M. B., B. Chir. and E. N. Willmer, M.A." The neogenesis of Dr Chir is thus easily explained.

Students of the history of embryology might the interested to know that it was also in 1939 that the first description was published of a mesoderm-inducing activity from adult tissues [2]. Had the two groups got together, they might have anticipated by nearly 50 years the modern flurry of activity on the role of growth factors as agents of embryonic induction.

J.M N.Slack, Nature Vol. 347, 1990.

[1] **Trowell, O.A. & Willmer, E.N.** *J. Exp. Biol.* 16(1939) 60-70

[2] **Chuang, H. H.** *Wilbelm Roux' Arch. Entw. Mech. Orgs* 139(1939) 556-638.

A magyar kutatók eredményeinek visszhangja

Az 1991 májusában megjelent *Impakt* próbaszám 2. oldalán ismertettük a philadelphiai Institute for Scientific Information (ISI) által folyamatosan publikált "idézettségi klasszikusok" összeállítást, és bemutattuk azokat a magyarországi kutatókat és eredményeket, akiket az említett Intézet beválasztott a "klasszikus" kategóriába. Az ISI listáit természetesen a jövőben is figyelni fogjuk és az *Impakt* hasábjain folyamatosan bemutatjuk majd a jövő magyar idézettségi klasszikusait.

Itt most azonban szeretnénk olyan teljes egészében, vagy részben magyar alapkutatások visszhangjáról beszámolni, melyek kimagasló idézettségük alapján nagy eséllyel rendelkeznek a klasszikus kategóriába való bejutáshoz.

Az alábbi szöveg a nagytekintélyű *Scientific American* c. folyóirat 1987 januárjában (256. kötet, 1. szám, 84. oldal) jelent meg.

Annak ellenére, hogy a szöveg egyértelmű, talán nem felesleges megemlíteni, hogy magyar kutató eredményeinek ilyen hangon való ismertetése a lapban rendkívül ritka esemény és komoly tudományos "hatásnak" tekinthető.

Braun Tibor, MTAK

FRAKTÁLNÖVEKEDÉS

Fractal dimension differs from an ordinaly dimension in that it is not expressed as a whole number but as a fraction. To determine the precise fractal dimension of an object one counts the average member of N of fundamental units of repetition found within a sphere of a certain radius r centered somewhere on the object. According to Euclidean geometry, the member of fundamental units then equals a constant C multiplied by the radius raised to the value of the dimension D ($N=C\times r^D$). In the case of a line the dimension is, of course 1: tripling the radius of the sphere triples the member of units inside. For ordinary (nonfractal) bulk matter in two dimensions, tripling the radius of the sphere multiples the member of units by 9. For a fractal of dimension 1.46, in contrast, tripling the radius multiples the member of units by 5. In other words, the member of units grows faster than it does in the case of a line but not as fast as in the case of ordinary bulk mater. In this sense the object is intermediate between a line and a plane. The fractal shown here was invented by Tamás Vicsek of the Research Institute of Technical Physics of the Hungarian Academy of Sciences in Budapest.

Említett szerzőt, *dr. Vicsek Tamást* az Eötvös Loránd Tudományegyetem Atomfizikai Tanszéke valamint az MTA MÜFI munkatársát megkértük, hogy ismertesse fraktálnövekedési kutatásainak eredményeit. Értékelését az alábbiakban adjuk közre.

Az 1980-as évek elején a bonyolult geometriájú struktúrák tulajdonságaival és keletkezési körülményeinek feltárásával kapcsolatos kutatások robbanásszerűen felgyorsultak. Ebben a megélénkült érdeklődésben elsősorban az a Benoit Mandelbrot nevéhez fűződő felismerés játszott döntő szerepet, hogy a környezetünkben lejátszódó fizikai folyamatok gyakran vezetnek igen összetett, önhasonló szerkezetű objektumokhoz. Ezeknek a struktúráknak a dimenziója egy tört (fraction) szám (kisebb, mint azé a téré, amelybe beleágyazhatók, mivel azt nem töltik ki), ezért fraktáloknak szokás nevezni őket. Egy időben a fraktál témájú cikkek száma annyira megszaporodott a legnagyobb presztizsű fizikai szakfolyóiratban, hogy egyes irónikus megjegyzések szerint át kellett volna keresztelni *Fractal Review Letters*-nek.

A komplex alakzatok növekedése többnyire egyensúlytól távoli jelenségek során megy végbe. A sok lehetséges mintázatképződési példa közül az utóbbi időben legtöbbet a túlhűtött olvadékokban vagy gőzökben lejátszódó dendrites kristályosodást (hópelyhek), a dielektromos kisülést (villámlást), az elektrokémiai leválasztást, a diffundáló mikroszkópikus részecskék aggregációját és a viszkózus folyadékok egymásba injektálásakor kialakuló viszkózus ujjasodást vizsgálták.

A fraktálok tulajdonságaira vanatkozó cikkeink nemzetközi visszhangot váltottak ki, amelyet a főbb témák említése után ismertetnék röviden. Itt most eredményeink közül a sok hasonló részecske egymáshoz tapadása (aggregáció) során kialakuló mintázatok viselkedésére vonatkozó négy munkánkat emelném ki.

Sikerült összefüggést találnom a standard – például az ágas-bogas (dendrites) kristályok növekedését leiró – mintázatképződési elmélet és az aggregációs módszer alkalmazása között [1]. A jelenségek megértésének egyik legalapvetőbb formája a közöttük fennálló különböző kapcsolatok feltárása; ebben rejlik ennek publikációnak a jelentősége. Ezen az úton jelentett továbblépést az a cikkünk (Kertész Jánossal), amelyben a fluktuációk szerepét tisztáztuk a mintázatok jellegének kialakulásában [2].

A növekedési jelenségek vizsgálatában fontos szerepet játszik az a dinamikus skálázásnak nevezett elméleti megközelítés, amely a folyamatok időbeli viselkedését egységes formalizmus szerint tárgyalja. Ilyen dinamikus skálaelméletet vezettünk be (F. Family-vel) az aggregálódó klaszterek viselkedésének leírására [3], és a fraktál felületek kialakulását modellező folyamatok időfüggésének tárgyalására [4].

A fenti 4 cikkre eddig több mint 400 alkalommal hivatkoztak az irodalomban. A hivatkozások jellege egyszerű említéstől részletes ismertetésig terjed. Ez utóbbira példa [5] a *Nature* szerkesztőjének egész oldalas cikke [1]-ről, vagy a fraktálnövekedésről a *New Scientist*-ben publikált összefoglalóban [6] megjelent [1]-ből átvett ábra. Cikkeinkből jelent meg ábra (ill. ismertetés) a *Scientific American* c. folyóirat 1987 januári számában és a *New York Times*-ban (1987 január 6. *Science* rovat). Tevékenységünk fogadtatásához tartozik, hogy az utóbbi néhány évben mintegy 30 nemzetközi konferencián tartottunk meghívott előadást.

Az említett cikkekből három íródott külföldön (Amerikában) egy pedig itthon. Másfelől viszont az itthon készült cikkeinkre kapott összes idézeteink száma is több százra tehető, tehát nincs jelentős eltolódás a külföldön publikált munkák javára.

A fenti idézeteket annak lehet tulajdonítani, hogy egy új, rohamosan fejlődő tudományterületbe annak születésekor sikerült bekapcsolódni. Ismét megemlítendő, hogy ez a csatlakozás már ilyen irányú külföldi útjaink előtt megtörtént. Ha egy új témakörben sikerül érdekeset mondani, akkor a területen később születő munkák közül várható, hogy több is épít az ilyen eredményre, idézi azokat.

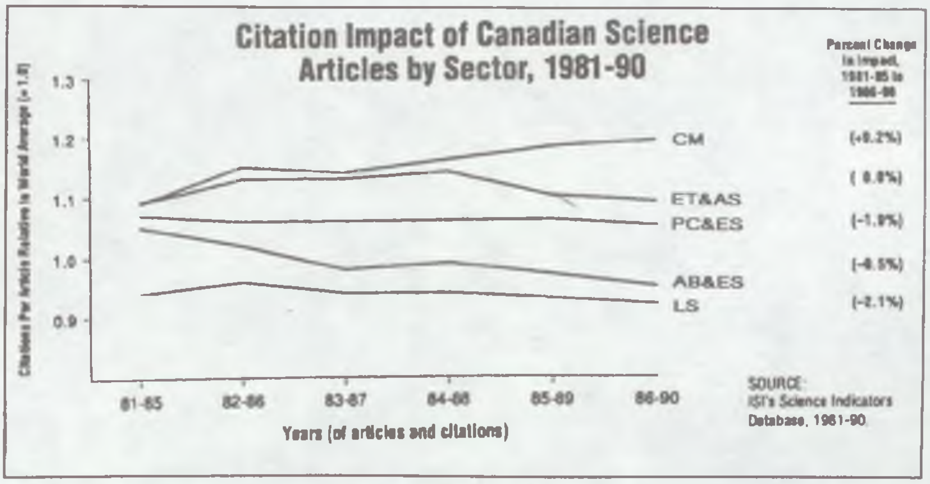
Vicsek Tamás ELTE, MTA MÜFI

[1] **T. Vicsek**: Pattern Formation in Diffusion-Limited Aggregation *Phys. Rev. Lett.* 53 (1984), 2281
[2] **J. Kertész** and **T. Vicsek**: Diffusion-Limited Aggregation and Regular Patterns: Fluctuations versus Anisotropy, *J. Phys.* A19 (1986) L257
[3] **T. Vicsek** and **F. Family**: Dynamic Scaling for Aggregation of Clusters, *Phys. Rev. Lett.* 52 (1984) 1669
[4] **F. Family** and **T. Vicsek**: Scaling of the Active Zone in the Eden Process on Percolation Networks and the Ballistic Deposition Model, *J. Phys.* A18 (1985) L75
[5] **J. Maddox**, *Nature* 313 (1985) 93
[6] **C. Sutton**, *New Scientist* 4 April (1985)

Moving Ahead But Falling Behind: Canadian Science in the 1980s

During the last decade journal articles by Canadian researchers declined in the influence they exerted on the world's scientific community. Overall, Canadian papers slipped 2.8% in citation impact (average citations per paper) over the period 1981-90. That made for the second worst performance among the Group of Seven (G7) nations. Only the United Kingdom turned in a poorer performance: its papers lost 3.4% in impact.

The chart at right shows that, with the notable exception of articles representing clinical medicine, Canadian papers in all sectors of science declined in their relative citation impact during the last half of the decade.



CM: Clinical Medicine; ET&AS: Engineering, Technology & Applied Sciences; LS: Life Sciences
PC&ES: Physical, Chemical & Earth Sciences; AB&ES: Agriculture, Biology & Environmental Sciences

(continued on next page)

After first rising and then falling in impact, articles published in journals of engineering, technology, and other applied sciences managed to end the decade where they began. Papers in the physical sciences (chemistry, physics, and earth sciences), agricultural sciences, and basic biological sciences, on the other hand, lost ground and ended the decade with a lower relative citation impact than they had exhibited at the beginning.

Clinical studies, as mentioned above, bucked the trend and actually increased 9.2% in relative impact over the 1980s. The citations-per-paper performance of this sector has been moving steadily upward since about 1983.

To obtain these results, *Science Watch* sorted Canadian papers published in some 3,000 of the world's leading journals into five categories corresponding to the journal coverage of each of the five science editions of ISI's *Current Contents*. Only articles (and not other types of items such as reviews, editorials, letters, meeting abstracts, etc.) were tracked; by restricting the analysis to articles alone, *Science Watch* attempted to measure, as precisely as possible, the impact of original research from each sector of Canadian science.

The time-series chart above depicts the citation impact figures for Canadian articles published and cited during six successive and overlapping five-year periods. The average (= mean) citations-per-paper scores for each category and for each period were then compared to the respective world averages to arrive at measures of relative performance.

The table below provides supplementary statistics on the "uncitedness" and volume of Canadian articles in five different sectors during the past decade.

The major trends in each area can be summarized as follows:

- Papers published in journals of clinical medicine soared in citation impact. Citations per paper relative to the rest of the world rose 9.2% (in absolute terms the increase was 10.0%). Production of papers in clinical medicine also shot up-an increase of 37.1%, compared with a world average of +23.3%. The uncitedness of Canadian clinical studies declined 2.4%, compared with an increase of 0.3% for the world. That fewer clinical studies were being left uncited as the decade progressed is further evidence of an improvement in quality overall.
- The sector of Canadian science that declined most sharply in impact during the 1980s was agricultural biology and environmental sciences. Papers in this sector showed a loss of 9.5% in relative terms. Absolute citations per paper actually rose, but by only 2.0%, which was not enough to keep pace with the average increase in impact for the world. Both volume and uncitedness were up, too.
- The next biggest decline in relative citation impact for Canadian research came in the life sciences. Papers representing the basic biological sciences lost 2.1% in impact over the decade. Again, absolute citations per paper actually rose (5.2%), but by less than the world average, which made for a relative decline. Uncitedness was unchanged for the decade, but volume increased about half again as much as the world average.
- Articles representing the physical sciences were marginally off in their average impact. Relative citation impact was down 1.9%, while absolute citations per paper rose 2.3%. An increase in volume of 17.2% was slightly above the world average of +15.0%. Uncitedness increased only 1.1%, about half that of the world. All in all, the physical sciences were the closest Canadian science came to a steady performance.
- As mentioned, it was up and then down for papers in engineering, technology, and applied sciences. During the first half of the decade, relative impact increased nicely, but there was a turnabout at mid-decade and a decline set in. Relative impact was unchanged for the decade; absolute citation impact was off 4.8%, closely matching the world average. Volume rose twice the world average, while uncitedness increased 1.4%, compared to just +0.1% for the world in this sector.

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Percent of Uncited Canadian Articles by Sector

Years	CM	ET&AS	PC&ES	AB&ES	LS
1981-85	50.3	59.3	35.7	42.4	31.6
1982-86	49.6	59.6	35.4	42.9	31.6
1983-87	49.3	59 9	35 7	43.0	32.3
1984-88	49 2	59 2	35 5	42.4	32.3
1985-89	49.2	59.7	36.3	43.2	32.0
1986-90	47.9	60.7	36.8	43.6	31.6
Change 81-85 vs. 86-90	-2.4	+1.4	+1.1	+1.2	0.0
Average Change 81 -85 vs. 86-90	+0.3	+0.1	+2.0	+0 2	+0.8

Percent Change in Volume of Canadian Articles by Sector, 1981-85 vs. 1986-90

Years	CM	ET&AS	PC&ES	AB&ES	LS
Canadian articles 81-85 vs 86-90	+37.1	+365	+17.2	+26.4	+29.9
Average for world 81-85 vs. 86-90	+23.3	+18.1	+15.0	+9.5	+19.2

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