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THE "IMPACT FACTOR" IN THE EVALUATION OF RESEARCH

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ABSTRACT

The "impact factor of journals", in brief IF, has become an important tool for the evaluation of research activities in certain disciplines, while it is gaining additional influence in others. Its widespread use is increasingly determining the fate of individuals, and academic departments. It is also contributing to restructuring of academic libraries and to reshaping the map of academic periodicals. There is no doubt that this index appears useful, and in many respects, attractive. It relieves faculty and administrators of the burden of reading and comparing papers and it seems to provide an "objective" criterion for judging scientific research. In this paper we present a different view and we argue that costs, in terms of potential damage to the scientific system, which are caused by an uncritical application of the IF, exceed by far its supposed merits. One purpose of this paper is to encourage scientists working in disciplines that have not yet been invaded by IF, (such as historians, jurists, social scientists and students of the humanities) not to be intimidated by the fact that other sciences are using IF to as a measure of research quality, and to carefully consider the implications of adopting such an index.

This is not a study based on sophisticated statistical analysis. For such analysis we refer to the paper by Per O Seglen (*a*). Our criticism is based on "political" considerations, for which we deem observing reality with the naked eye to be sufficient.

THE ORIGIN OF IF

"The Institute of Scientific Information" (ISI) collects the data needed for the computation of the IF, computes it, makes it public and advertises its use. Despite its resounding name, the ISI is not a non-profit organization, and its main concern is not the promotion of science. It is a private company, which sells bibliographical repertoires and limited access to a data bank on the market. As a private company, its main interest is to maximize the profits of its owners.

This elementary fact, often ignored, or considered unimportant, by the users of this index, should be always be kept in mind. One should also be aware of the fact that the ISI, as the owner of a gigantic data base on scientific publications and citations, enjoys a dominant position in the market, making it virtually impossible for any public institution or learned society to compete on fair grounds. In other words, there is little or no market control on the decision that this company, moved by pecuniary interests and on the basis of cost-benefit considerations, has been taking. On the other hand these same decisions have an influence on issues which are extremely important for the development of science and for society in general.

The data bank of ISI, which is made available under the trademark of "Science Citation Index", contains, not only the title and authors, but also the full list of references to other papers and authors contained in the paper for each scientific paper published by a selected list of journals.

The inclusion of citations in the data base, an idea which originates with the founder of ISI, Eugene Garfield, makes it possible to perform a kind of bibliographical search, which is not possible with other repertoires of published papers. One can start from an important paper of the past and look for the papers which include it in their list of references. Some of these papers can be used for a new search for citations. In this way one can trace the development of important ideas and methods, through what is rightly called a "forward" bibliographical search, as opposed to the ordinary "backwards" search which traces the origin of an idea or a method by looking at the papers cited in a given paper.

The Science Citation Index, originally made available through subscription to printed volumes, soon became an important tool for bibliographical searches ("backward" and "forward"), especially in the fields of Chemistry and Biology. It was ignored for many years by Physicists and Mathematicians. The former did not really need a bibliographical repertoire, because of efficient and nearly complete distributions of "preprints". The latter were used to their own repertoires, the most important of which was the

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Mathematical Reviews, which publishes critical reviews of virtually every mathematical paper published in the world.

In the mid-sixties SCI aroused the curiosity of some sociologists and historians of science. In particular, the historian Derek De Solla Price was intrigued by the possibility of studying developments in modern science using the statistical tool provided by the SCI. This interest of Price and others was used by the owners of SCI as a "marketing" opportunity aimed at academic institutions as well as individual researchers. Individuals in particular were intrigued by the possibility of counting the number of times their articles and those of their colleagues were cited in the relevant scientific literature. The number of citations could be thought of as a measure of the interest of the results reported in a paper.

But in this context, before concentrating on the explosion in the number of citations, attention was concentrated on the quality of the citations. There are citations of the type "The present article is based on the fundamental and pioneering study of ...". But there are also citations of the type: "The results of ... for this problem are unreliable because based on a wrong methodology". Yet there are citations that mention someone among tens of others that have considered the same problem. One cannot, of course, accept uncritically the first two types of citations. In the third case (which has become prevalent) the information concerning quality of the cited article is essentially non-existent. In many cases one is not even sure that whoever is citing has read the cited article. To speak of the "impact" on scientific research of the cited article is purely arbitrary.

Be this as it may, it is the interest in evaluating the research one's colleagues that gave the impulse for the commercial expansion of ISI. It was at this stage that the "virus" was introduced. From hence forward the way was opened for utilising IF as a parameter in individual and collective evaluation.

THE NATURE OF THE DATA BANK

As mentioned before, the most important asset of ISI is its data bank. Of foremost importance for the collection and elaboration of the data is the choice of the so-called "source journals", that is the journals from which data on papers and citations are extracted. The Science Citation Index aims to be useful across all fields of science, but the cost of including all possible scientific journals, in all languages, would have been prohibitive. It would have meant finding a way to receive each journal regularly, keeping a lengthy correspondence with foreign editors to inquire about volumes not received, and finally, examining each article and its references, and feeding the data to a computer. Faced with such great prospective costs, ISI became keenly aware of the problem of marginal costs and utility.

The marginal cost of adding one obscure journal to the list of "source journals" was very high, especially if compared to the small "marginal utility", measured in terms of better prospects of selling access to the data bank.

It was important however to be able to claim that the ISI data bank was nearly complete and included all relevant journals as sources of published items and citations. For this reasons all (or almost all) journals subscribed to by the major American scientific libraries had to be included in the list of source journals. These libraries are also the most consistent group of potential customers of the products and services sold by ISI. As a result, the original list of source journals, included, first and foremost, the periodicals published in the US and those of the major publishing houses. It was also natural to include periodicals of other English speaking countries and, finally, a few "national" periodicals from other countries (Japan and the main East and West European countries).

The choice as to which journals should be chosen from these countries was not easy. In the end, the most important criterion turned out to be the regularity of publication. This is, of course, a criterion which has nothing to do with quality of the papers published. But it has a lot to do with potential costs of keeping a journal in the list of primary sources. As a result, important journals were excluded from the list, simply because institutional publishers (such as Universities, Academies, Foundations) were not as keen as commercial publishers on understanding what was needed to be included in the data bank.

IF AS A TOOL FOR COMMERCIAL PROMOTION

Beyond the basic decision to exclude periodicals which were not sufficiently regular, it was necessary to devise a tool for measuring the marginal utility of

keeping journal in the list of primary sources. The index known as IF, which was invented to fulfill this need, is defined as follows.

The IF of periodical *X* of year *N*, is the number of citations in year *N*, in any periodical in the data bank, of articles in periodical *X* that appeared in years *N-1* and *N-2*, divided by the total number of articles published in years *N-1* and *N-2* in periodical *X*.

In brief, this is the average number of citations per article in the preceding two years, counting, of course, only citations in journals which are included in the data bank. It is quite clear that for a company whose main product is citations, and in a marketing environment which tends to value timeliness rather than depth of scientific contributions, the average number of citations in the last two years is a good indicator for the utility which can be derived from keeping a journal as a primary source, or adding the journal to the list. A periodical which generates few immediate citations is presumably not widely read in the scientific community, its omission from the list of primary sources, may pass unnoticed to the buyers of the services and products of ISI.

But in fact the real importance of the index, for its inventors, is that it can be presented to the scientific libraries and research centers in America as a tool for deciding whether to subscribe or not to a given periodical. These libraries are ISI's main customers and it is important to stress to them that the ISI data bank is complete and trustworthy, especially in view of the planned the expansion of the company's activities beyond the traditional fields of chemistry and bio-medicine.

If the libraries are induced to believe that only journals with high impact factor are worth subscribing to, they will readily be convinced that the ISI data bank is more than adequately representative of worldwide scientific production. Indeed this data bank contains many journals with a very low impact factor, and indeed all journals for which the impact factor can be calculated, simply because there is no one except ISI which can compute this or a similar index.

The possible circularity of this reasoning should not be a problem, because as soon as this marketing idea starts to be successful, it modifies reality and makes it conform to its claims. Indeed if the most important American scientific libraries decide that it is not worth it to subscribe to a journal, that journal, will be shunned by authors interested in a wide audience. In short, it will be doomed to extinction or to survive as a marginal journal where one publishes only "for the record" with no hope of being read, or perhaps, with the hope of not have been read, by his colleagues.

There is no question that the marketing strategy chosen by ISI, which enjoys a position of virtual monopoly in this particular business, is the right strategy for a business enterprise. Which business would forgo the possibility of actually changing the external world to make it conform to its interests? But it is legitimate to doubt that this strategy corresponds to the interests of science and of scientific communication.

A completely different strategy is followed, for instance, by the American Mathematical Society (AMS), a learned society which has the status of a non profit organization and is committed to the promotion of science. The AMS includes in its repertoire, "Mathematical Reviews", every journal dedicated to research in mathematics. This inclusion is decided by experts, without regard to the cost involved. Many journals are acquired through the procedure of exchange with the journals of the American Mathematical Society, a procedure through which the AMS forfeits some income, but fulfills its mission of promoting research and teaching of mathematics, making his journals available throughout the world.

In pursuit of his marketing strategy, the owner of ISI, E. Garfield, has published tens of articles, comments, letters to the editor, contributions at conferences that have the appearance of scientific studies, or in any case of statements of concern for the improvement of scientific communication. But the underlying arguments are not difficult to identify.

The starting point is a compelling argument for whoever must use limited capital for the acquisitions of a scientific library: it is impossible to buy them all. One must limit oneself to the most important. In Garfield's words *(b)*:

"The fact is, no matter how many journals are on the market, only a small fraction account for most of the articles that are published and cited in a given year"

If there is agreement on the hypothesis that the importance of a periodical is measured by the number of citations received by its articles, then it is sufficient to analyze the data of the citations of the different periodicals. A simple statistical analysis reveals that

even the data of the ISI is redundant:

"... 900 (21%) of the 4,400 periodicals indexed in the 1988 SCI received 83% of the 8,000,000 citations processed for the Journal Citation Report that year. [...] it took 2,000 journals (46%) to publish 86% of the 435,000 original research or review articles and technical notes included in the 1988 JCR"

The argument can be made more sophisticated and "scientific" by citing "statistical distributions" or referring to graphs and diagrams. But it is the IF, in the opinion of its promoters, which should determine which periodicals to acquire. It is interesting to note that in this context the price of a periodical is never mentioned. As we shall see below, the confrontation of cost vs. quality of periodicals would not be flattering to the commercial publishing houses, who are the principal allies of the ISI in the struggle for vital space in scientific libraries.

At this point it is not worth challenging the initial hypothesis, i.e. that the utility of a periodical is measured by the number of citations obtained by its articles in two consecutive years. The periodical will become useless anyway when its potential authors become aware of the fact that it is no longer available at the American libraries *(c)*.

WHAT DETERMINES THE IF OF A PERIODICAL?

First of all for a journal to possess an IF it must belong to the ISI data base. A journal which is not in the data bank is considered to be non-existent, or to have an IF of zero. This fact has given an enormous advantage to the periodicals initially included in the data base, which, as explained above, have been selected keeping in mind the interests of the major clients of ISI, that is the American scientific libraries. These libraries, in turn, are run by professional librarians, who do not always understand the requirements of creative science. Next, periodicals that show some irregularity in their publication are excluded. In fact, if a 1999 volume appears in 2000, this mere fact renders the computation of its IF impossible.

Another important element in the determination of the IF is the promptness of publication. An important article, whose publication is delayed more than a year, is cited many times as a "preprint" and these citations do not add to the IF of the periodical. But waiting time for publication is not an index of quality. Indeed many serious journals require a long time to evaluate an article, especially if they adopt a genuine referee system and do not base the judgement on a cursory glance by a member of the editorial board. It may also be that an article may wait months or a year after being accepted. It happens more rarely for commercially published periodicals. But it can happen for periodicals published by academic institutions, which operate on a fixed budget or rely on voluntary work.

But the most important variable in the determination of the IF of a periodical is the number of citations that the authors that contribute to that periodical tend to insert in their articles. Only in this way can one explain, for example, the enormous difference in IF between periodicals of pure mathematics and of applied mathematics, or computer science. Only in this way can one explain the fact that the highest IF of a mathematics periodical is twenty times lower than the highest IF of a biology periodical.

The mean number of citations per article can in no way be correlated with the quality of the journal. The habit of inserting many or few citations depends on conventions in each scientific field. Theoretically, this should make it possible to compare different periodicals of the same field, where authors share conventions *(d)*. But how is an area to be defined? Does the "Journal of Computational Neuroscience" belong in the area of biology (and its IF of 2.6 is rather modest) or with periodicals of applied mathematics, which is the classification adopted by ISI (with the same IF relatively astronomical)? *(e)*

EFFECTS OF IF ON SCIENTIFIC PUBLISHING

Again mathematics will be used as an example. Until the sixties scientific publishing in mathematics was largely by scientific institutions, such as societies, universities, academies etc.... These institutions had an interest in maintaining the costs of publishing and distribution low, and, especially in Europe, a significant part of the distribution was based on exchanges between libraries.

In East European countries, and in some developing countries, where the production costs were very low, or subsidized by the state, this system allowed the libraries to acquire, without expenditure of foreign currency, western periodicals. Generally, subscription costs were maintained low, despite mounting production costs.

Another characteristic of this type of scientific publishing has been the absence of specialization: articles of all mathematics disciplines were published together.

Starting at the beginning of the sixties commercial publishing has heavily penetrated the market, introducing specialized periodicals. The private publishing companies also succeeded in finding new distribution channels, by mainly approaching the managers of the American university libraries. These libraries, in contrast with their European counterparts, are controlled by specialized personnel, which does not always enjoy easy relations with the professors, considered "users" and not "owners" of the library *(f)*. Typically, also, libraries in American universities are general, rather than departmental, and acquisition criteria must be common to the different disciplines. The simplest way of managing a library in these conditions, for someone without specific competence in particular disciplines who prefers not to be bothered by contrasting pressures of various specialists, is to put himself in the hands of the major distribution channels. Those, in turn, have a natural rapport with the major publishers.

To make a periodical or a book attractive to a distribution firm its price should be sufficiently high. It was a novelty that aggressive salesmanship, based on "scientifically" motivated "objective" considerations, could render the cost of publication almost irrelevant to the buyer. Such considerations, based on numbers, were insistently furnished by ISI.

The final result of this evolution has been to favor the large publishing houses, to the detriment of publications related to academic institutions. It led to an increase in the number of periodicals of "high scientific prestige" *(g)* and in the number of articles published in periodicals of high prestige. Moreover, the cost of publication and distribution has risen, in an era of technological progress. Perhaps the only way out of this oligopoly of the publishing house, mediated by ISI, is the advent of electronic publishing.

THE IF AND THE EVALUATION OF SCIENTIFIC WORK

Garfield states:

"The source of much anxiety about Journal Impact Factors comes from their misuse in evaluating individuals, e.g. during the Habilitation process. In many countries in Europe, I have found that in order to shortcut the work of looking up actual (real) citation counts for investigators the journal impact factor is used as a surrogate to estimate the count. I have always warned against this use. There is wide variation from article to article within a single journal as has been widely documented by Per O. Seglen of Norway and others".

According to Garfield it makes more sense to count the number of citations per article then to refer to the average number of the periodical in which it appears. But Garfield's cautious opinion was not enough to prevent a widespread use of IF for quality evaluation. We must therefore go beyond Garfield's comments and justifications.

The main justification for this use is based on the hypothesis that the value of a scientific study can be estimated by the number of citations it receives. It is further supposed that this number can be estimated from the average number of citations obtained by the articles in the particular periodical, and finally that that number can be estimated from the IF, which is the average over the last two years. Everyone of these hypotheses is in some measure plausible and has some empirical support. But put together they produce a very weak case.

It is plausible that citations measure the influence of a scientific study on the studies of others, and hence may indicate the importance of the study. In this context Garfield cites the sociologist of science Robert K. Merton who has described citations as a currency of science. According to Merton scientists remunerate their inspirers and their masters using citations. But Merton was writing about science at least fifty years ago, prior to the advent of ISI and the IF.

The introduction of citations as an instrument of evaluation has totally modified the social context in which citations are practiced. With the use of IF for the evaluation of research, or merely of periodicals, the system of citations is invaded with direct and indirect interests (career, salary, funding access, periodical distribution) which completely distort its nature.

Once, one cited only articles that all authors have read and used in their research. Today, entire bibliographies are inserted automatically into articles. Since the IF also counts the self-citations of a journal, it is good practice to cite articles published in the journal

one intends to publish one's work in. This makes the articles more acceptable to the periodical because it augments its IF, enhances its prestige and its distribution *(h)*. The choice of citing or not citing is no longer dictated by the need to acknowledge someone's priority, or by the desire for clarity and completeness to the exposition. Most of today's citations can be omitted or substituted by other citations. One cites authors by whom one expects to be cited. In fact, the number of citations per published article has increased enormously, just because, as pointed out by Garfield, there is a high correlation between the IF of a given periodical and the average number of citations in its articles. Citation as a "tribute" to a master or to an inspirer has practically disappeared from scientific literature and is buried under mountains of irrelevant citations. In the ocean of bibliographic references it has become impossible to identify the real inspirer of the author. The success of Science Citation Index has rendered it less useful a tool for bibliographical searches and also as a subsidiary tool for evaluation of research.

Another hypothesis that appears to be plausible at first glance is that the number of citations that an article receives in two consecutive years following its publication can be estimated from the IF. Instead, a simple statistical analysis performed by Per O Seglen reveals that the rate of citation of an individual article is distributed in a very non uniform fashion. It turns out, that the IF is determined by few articles with very many citations and many articles with few citations. In figures: 50% of the citations are determined by 15% of the articles (the most cited). In other words, 50% of the articles contribute a mere 10% of the citations. All this is confirmed by Garfield in the passage referring to Seglen.

One could advance another argument for the faith in the IF of a periodical as a criterion for the evaluation of an article published in it. It is said that periodicals with high IF contain articles of higher quality, or are more selective of the articles to be published. The IF of a periodical would provide an initial qualitative grading of an article under consideration, prior to more detailed consideration. *(i)*

But is it true that the IF measure the quality of articles published in a given periodical? It does not seem possible to establish such a fact "a priori". To recapitulate, the IF of a periodical depends mostly on the habits of the scientific community of reference. It is high where articles are loaded with many citations, and in particular citations of articles of the last two years. It would be low for periodicals in which citations are few or references are made to older work. To actually determine to what extent the IF of a periodical gives a good indication of its quality or its selectivity, one would need to consult experts in the field.

THE EXAMPLE OF MATHEMATICS

Personal familiarity brings to mind mathematics as a case to exemplify the distortions that may be produced by the IF. The three ISI categories: "Mathematics", "Applied Mathematics" and "Mathematics miscellaneous", contain essentially all periodical in which mathematicians publish. First one observes that of 265 periodicals, only 10 have IF above 1.5 (the maximum is 2.6). Among these ten there are three mathematical periodicals of unquestionable quality, two of which are known as the most selective ones. They are third and fifth on the IF scale and with an IF of 1.9 and 1.7. The two periodicals at the top of the list are "Journal of Computational Neuroscience" (IF 2.6) and "Econometrica" (IF 2). Should one exclude them from the list? Should one normalize their IF with that of periodicals in neuroscience or economics? It is not easy to answer. Each of the two choices would have required some arbitrary decision, much like the one that included them in these categories, where they reside uncomfortably.

The problems go further. Among the first ten there are three periodicals of applied mathematics. One cannot imagine that "Applied and Computational Harmonic Analysis" (IF 1.64) can compete in selectivity with "American Journal of Mathematics" (1.05). And the example is not isolated. The explanation for this discrepancy lies again in the arbitrariness of the grouping and the first journal is probably cited often by communication engineers. One may be in favor of inter-disciplinary dialogue, but that can hardly be confused with selectivity grading.

Finally, mathematics brings forth yet another issue. No mathematician will doubt the major importance of Soviet mathematics in the last fifty years. Until recently Russian mathematicians were obliged (or encouraged) to publish in Russian language periodicals. Some of these periodicals were integrally translated into English, several years later of course. Due to this delay, Russian mathematics would have appeared nonexistent, had the IF been taken as an index of quality.

IS THE IF OBJECTIVE?

This argument is often raised by the promoters of the IF. The IF, they argue, despite whatever shortcomings it may have, has the merit of being "objective". But what does objective mean? It is not sufficient for a piece of data to be a number in order to be objective. It must first and foremost not be subject to manipulation by the actors in the evaluation procedure. It turns out though that the IF can be manipulated directly as well as indirectly. Let us see how.

A first (innocent) manipulation is affected by a researcher (or a group of researchers) who, prior to submitting an article, consider the list of periodicals according to their IF. Once the IF is accepted as an index of quality the astute researcher will try to publish his work in periodicals of maximum IF, among the ones that may accept it. Moreover, the researcher who adopts this strategy will become an enthusiastic supporter of the IF as an indicator of quality. This strategy is not available to everyone, but only to those who have access to the to the list of periodicals with their relative IF, at least in their own discipline. This list is not freely distributed. It is sold, by ISI, at high price.

A second possible manipulation is to abound in citations, particularly those of the same periodical and of the last two years. This tactic functions better if one belongs to a group of researchers intended to exchange citations. It functions better still if the group succeeds in dominating one or two periodicals, published by a large commercial house, prepared to invest in "marketing". Also these happy bands become great supporters of the IF. A third possible (yet real) ploy is for the editorial boards of the periodicals to encourage their authors to cite articles in the same periodical in the process of refereeing, which is anonymous. This manipulation is, in the final analysis, not only in the interest of the publisher, but in the interest of all concerned, including the authors and the editorial committee.

Finally there is the freedom to manipulate the IF index at the end user's side. When it comes to using the IF data to evaluate an individual, the numbers associated with the periodicals must be transformed into a number characterizing the individual being consideration. If one makes the logical leap, associating with each article a number equal to the IF of the periodical in which it was published, one is left with the freedom of deciding how to weigh a given individual's different articles. This freedom is very vast. It goes from handling different scales of IF in different disciplines (where journals of mathematics have IF one twentieth of those in biology), to different scales of IF within different specialization groups inside a discipline. Any weighing algorithm implies an arbitrary choice and is open to manipulation.

The above considerations merely succeed in showing that the ISI's claim that the generation of a number for the assignment of value to periodicals is a safeguard for objectivity and impartiality is at least dubious. Instead, the IF tends to produce a screen in the way of transparency of decisions and of assumption of responsibility where responsibility is called for.

THE NEGATIVE EFFECTS OF IF FOR SCIENTIFIC COMMUNICATION

An indiscriminate increase in the number of scientific periodicals and in particular of the more costly ones published by commercial publishers. This is accompanied by a financial crisis of libraries -- a question of life and death in countries of Eastern Europe or in developing countries;

The exclusion of scientific periodicals connected to cultural institutions from international distribution circuits;

An increase in the number of citations that is can neither be justified by acknowledgement of priority nor by rendering the text clearer (this increase renders the use of Science Citation Index for the purpose for which it was originally created more difficult);

The mushrooming of little specialized periodicals managed by small communities dedicated to mutual citations and not interested in confrontation with the rest of the scientific community.

The upsetting of the role of common sense in the choice of publication as a venue of one's results, which should be published where they me be of greatest utility;

Value judgements masqueraded as objectivity;

The acquisition of a very costly bibliographic index (SCI), in all research structures, useful almost exclusively for maximizing IF;

The subservience of cultural and scientific choices of scientific communities to the pecuniary interests of large publishing houses and the ISI.

ON DISCRETIONARY JUDGMENTS AND

EVALUATIONS

In conclusion, it should be mentioned that scientific evaluation cannot eliminate discretional judgement, which can err. Even the judgement involved in accepting an article by a periodical has a discretional character. Not only because the referees exercise their discretion, but also because the selection of the referees and the interpretation of their comments is discretional. And because these choices are discretional, it is common in all countries to turn to experts for evaluation of projects and of results of research. Such evaluation is objective inasmuch as it is not dictated by direct or indirect interests of those who evaluate, that is to the extent that the judgement is "impartial". It is impossible to guarantee impartiality "a priori". But one of the instruments for controlling scientific judgements is censure, in terms of reputation, which can be exercised by an open and vigilant scientific community. An essential precondition for the practicability of such vigilance is that there be a clear attribution of responsibility. A judgement based on discretional choices obscured by supposedly objective parameters escapes criticism and hence is potentially arbitrary. Such considerations apply not only to individuals but also to judging groups or structures in competition.

Good examples of responsible and discretional judgement of merit, by reputed experts in different scientific communities, are the "Research Assessment Exercises" in the UK. These are evaluations that concern the fate of entire university institutions and of the individuals that work in them. They are based firstly on the selection made by the institution's best researchers in every field, and then on an auto-selection by each researcher of a few of their articles which they consider to be the most important. No numerological automatism is introduced.

(a) - "Why the impact factor of journals should not be used for evaluating research", British Medical Journal 1997; 314; 498-502.

(b) - E. Garfield "How ISI Selects Journals for Coverage: Quantitative and Qualitative Considerations", Current Comments, 22, May 28 1990.

(c) - An interesting exception is "Institut des Hautes Etudes Scientific; Publications Mathématiques" which has never entered the ISI data bank because it is published irregularly. No one doubts its importance, despite the fact that it is very difficult to find in American universities except for photocopies of some of its important articles, which circulate as secret documents. This unavailability of the journal does not seem to disturb its authors, who probably feel "above it all".

(d) - Per O Seglen, "Evaluation of scientists by journal impact", in Weingart P., Sehringer R. and Winterhager M. Des, Representation of science and technology, Leiden: DSWO Press, 1992.

(e) - This is in fact an interesting case in point, the word "computational" in the title of this journal is erroneously interpreted as belonging to the realm of applied mathematics, while as a matter of fact it aims to capture "brain phenomena" interpretable as "computation".

(f) - It is of course not the case that European university libraries are perfect, nor that there are no enlightened librarians in American university libraries. See e.g. Joseph J. Branin (responsible for the library system at the State University of NY, Stony Brook) and Mary Case (director of the "Office of Scholarly Communication of the Association of research Libraries") "Notices of the American Mathematical Society" vol. 45 April 1998.

(g) - This apparent paradox is analogous to phenomena in the world of fashion: In Italy of the 60ies no middle-class woman would be seen with a plastic hand-bag, without a lost of reputation. Thirty years later the same ladies, or their daughters, circulate with plastic hand-bags produced at ridiculous costs, but with the right "label" and sold at a price twenty times higher than the identical ones sold on sidewalks by Senegalese vendors.

(h) - It does often happen that a referee suggests to the author to include citations to articles of the same periodical. See e.g. E. Garfield "Editors are justified in asking authors to cite equivalent references from the same journal", British Medical Journal (BMJ 1997; 314:1765, 14 June).

(i) - A striking example of the opposite tendency is an article recently published by Lancet (IF 11.79), which discusses the relation between the weight of back-packs carried by Italian pupils and the weight of the pupils. The study is based on a simple collection of data and the most basic statistics. The knowledge of such correlation may be of value, of course. But here the intention is simply to underline the fact that periodicals of extremely high IF publish routine studies, while "Annals of mathematics", the periodical which published Andrew Wiles proof of the last theorem of Fermat, a problem that haunted the minds of the best mathematicians for centuries, has IF 1.7.