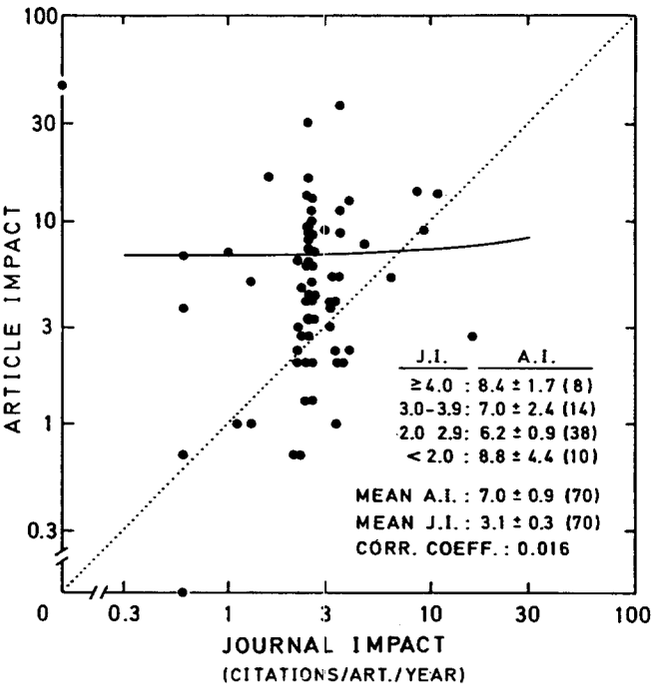

Fig. 1. The relationship between Journal Impact and real article impact (citation rate) for all articles published by one author over a 17-year period, showing that there is no correlation.

Impact factors[8]. Although the bias is strongly in the favour of biochemists, we will hopefully have enough professional pride to refrain from using this as an argument for the adoption of a notoriously misleading evaluation method.

Widespread implementation of the Journal Impact method for scientific evaluation would, in the long run, encourage authors to publish in those journals which presently have a high impact, to the detriment of low-impact journals (why don't publishers protest?) and probably to the detriment of some scientific disciplines better served by specialty journals. However, a word of warning should be issued: to correct for the above-mentioned field differences, some evaluation studies have chosen to measure scientific quality by 'Relative Impact', i.e. the ratio between the real article impact and the corresponding journal impact, the argument being that an impact *above* the journal average indicates supremacy within the corresponding scientific field[4,5,9]. If this method of evaluation becomes fashionable, it will be profitable to publish in low-impact journals rather than in high-impact journals!

The citation-based evaluation methods are clearly founded on erroneous assumptions, and should be discontinued immediately. As responsible scientists we should insist on the same quality standards for scientific evaluation as we require of the scientific work itself. The only acceptable method at present seems to be the peer review, in which published papers are read and evaluated by experts within the same field. Attempts to improve evaluation procedures should be directed towards the standardization of evaluation criteria and conclusion formats, and towards international cooperation to compensate for the lack of objective expertise in small countries. Such efforts would certainly be more worthwhile than the introduction of quick and easy methods which reduce the evaluation workload at the expense of quality and fairness.

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The bias of citations

The use of Citation Analysis as a tool to evaluate science is unfair, erroneous and dangerous. It leads to wrong ideas about what science is. It will have perverse effects, as any system designed to evaluate human endeavour modifies the behaviour of the tested population to suit the criterion measured: students prepare themselves for the type of questions that can be expected in multiple choice exams. Unfortunately, Science Citation analysis is easy, provides numbers, and is being lobbied for by administrators and by armchair scientists who derive salary and power from it.

A citation index is not a measure of quality or importance but a measure of recognition. Recognition in science, as in art or other activities, reflects quality but also publicity, power and fashion. The best player at Wimbledon wins by a definite score, the fastest runner wins the race. Unfortunately there is no such objective, absolute criterion for the best scientist.

Computer people are well aware that whatever the apparent precision or fancy representation of computer output, the output is only worth what the input was worth: 'garbage in garbage out'.

Let us start from the input, i.e. the data used in Citation Analysis. The basic assumption of such an analysis is that scientists writing papers by and large apportion their citations proportionally to the importance of the works cited in the methodological or conceptual support of their papers. As pointed out by Cole and MacRoberts, scientists writing a paper do not generally set themselves up as judges of the literature but in a very mundane way try to support as best and as easily as they can their own results and the importance of their own present and past contributions. Moreover, citation may be, consciously or unconsciously, oriented in various ways. I will describe several types of biases:

Self-citation bias: biases towards one's own work, without any malicious intent.

In-house bias: bias for work that one is well acquainted with, because of proximity, friendship, etc.

Journal biases: if you send an article to a journal be sure not to offend the referee by ignoring his work! Why am I cited more frequently by the *same* authors in journals in which I am an

Editor than in others?

Former citation bias: any check of the literature cited in an article reveals numerous paging or other errors than can be traced in previous articles. The lifting of references is very common. Once a paper is cited (even erroneously), its chance of being cited again increases.

The 'review bias': editing a paper is tedious; why hunt for an appropriate reference when one can rely on a recent and easily accessible review?

The 'oral tradition bias': Anglo-Saxon tradition gives more importance to what is heard rather than what is read. Latin and French traditions are the opposite. Thus when writing an article an English speaking author will often remember what he heard and then seek the reference in the literature. In the Anglo-Saxon world it is important to participate in the conference circuit to be cited.

National and cultural biases: quite normally one tends to cite what one knows best. If in a small country like Belgium, scientists have a national bias, on the whole, it will not change the index much. However, in a big country like the US the effect is enormous.

It would be interesting for a scientometrist to compare for the same subject the pattern of European, vs. US, vs. Japanese citations. Moreover, cultural differences may affect the relative 'dishonesty' of omitting an important reference.

The English language bias: English is the presently recognized international language of science, and this places some at a disadvantage to 'native' speakers. Moreover, scientific journals in other languages are often overlooked: compare the citation indexes of Russian vs. US Nobel prizewinners.

All these are normal biases with no malicious intent, but there are other less innocent biases.

The 'powerful person' bias: people may get very sensitive about being cited . . . so the researcher learns not to offend those in power: editors of journals; referees acting as gatekeepers of some famous journals; members of prestigious committees who organize international congresses, who give awards; or more importantly members of peer review committees and of grant awarding bodies. Being much cited may help become powerful!

Bias by omission: the 'competitors biases'. Some literature may be annoying if it contains the same information that is to be presented as novel, or, equally bad, it disagrees with one's 'own' results. In these cases safety rests in ignorance of these potentially damaging references.

The 'second but most prolific' (Bandwagon) bias: if a new line is opened by X, Y might take it over, refer in its first article to X and quite honestly (?) thereafter, follow through in waves of succeeding articles referring to the first Y article.

The 'Science Citation Index bias': as people are compared by their relative indexes it becomes self serving to favour self-citation rather than refer to the work of competitors. Thus, the instrument of measurement perverts the measurement.

It is striking that many of these biases represent positive feedback. Any system with positive feedback tends to behave in a binary fashion: the function is either at a maximum or a minimum. This is what is observed with published indexes. It gives a wrong impression of science: a few creators and a multitude of followers. The scientist as star may please the media, but it distorts science ethics. This 'star' concept gives dangerous weapons to those who seek to cut science budgets; it gives a wrong sense of values to younger people; to have an 'impact' (scientometrists even use the terms of publicity, public relations and marketing) is more important than really to contribute; it turns what should be a common humanistic endeavour into a frenzied competition. It is no surprise that in such an atmosphere authorship without responsibility proliferates, and fraud is mentioned more and more . . . 'The merchants are in the temple'.

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Citapeer or peercit?

How does a paper become quotable? By peer review; are some papers more amenable to review than others? Are only these manuscripts acceptable? In reality, acceptance of papers for peer-reviewed journals differs amongst reviewers and from journal to journal; there is no good comparability amongst journals. Hence the basis for a citation-based assessment is undermined.

An author's scientific achievement is usually not reflected in the actual mass of his papers; 300 g per year or 29 g per paper or 3123 g papers per grant are bad parameters. Is the difference between weight of papers and citation frequency incidental or really useful?

It is the policy of some journals (*TIBS* included) to limit the size of reference lists, encouraging some references to be cited, for example, as footnotes. There were good reasons when a well-known scientist was greeted by 'Hi, footnote-Smith (or -Miller)'. Footnotes escape the citation index.

The prevalence of citation of the author's previous work, particularly in the Introduction and Discussion sections of a paper, also creates a bias in citation frequency. Should the elimination of 'self-citations' be tried in the attempt to rescue the citation index?

Methodological papers also pose problems for citation analysis. The famous Lowry *et al.* procedure for determining protein (*J. Biol. Chem.* 193, 1951) is unsurpassable in usefulness, e.g. for speed, reliability, reproducibility and cost. It still attracts hundreds of citations per year. O. H. Lowry deserves every possible credit for his work, and he surely would become eligible for the Nobel award if the Stockholm committee were to use citation analysis as a basis for their decisions.

Finally, quantity versus quality remains the unsolved problem of any citation analysis.

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