

## Assessment by Bulk

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The recent debate on Italian university public examinations for the recruitment of researchers and teaching staff has raised many issues, including the problem related to how to assess candidates' scientific publications in CVs. In a field where even a crumb of scientific seriousness and moral rigour (but what is that) would revolutionise things, the universal remedy of an objective method is advocated. Naturally the idea is not ours, but comes from the USA.

The objective method would award a score to publication depending on the number of times it is cited in a given time interval after publication (citation index or quotation index), or the popularity of the journal it is published in (impact factor). Obviously, I have oversimplified the criteria, in reality the "significance" of a given publication can be balanced in various ways, by combining factors, including correctives, etc. The goal is to provide a number, an objective assessment, able to trounce the most sophisticated arguments and neutralise powerful political ploys. It is a real pity that this system has been challenged for many years (at least 4-5) worldwide, also in the US.

There are two orders of problems. The first general issue worries the editors of scientific journals and is changing the scene of scientific information<sup>1</sup>, while the second questions the validity of the method to assess single researchers' careers<sup>2 3</sup>.

Let's look at the first aspect. The fact that assessment of a single researcher or a group of researchers (for example to obtain funds, etc.) is based exclusively on papers published in specialised journals jeopardises such journals and puts them in a very delicate position where they may become the cause of unscrupulous conflicts and vehicle what is not always fair play. There have been radical changes in the form and production of scientific literature that has impacted, often negatively, on the dissemination and accessibility of scientific knowledge in terms of rigour, correctness and new data.

One of the primary factors of the issue is that the outpouring of publications does not allow researchers in a specific sector to keep updated. Consequently, the same referees have difficulty in reaching a correct and pondered decision on the quality of the paper to be published. The rush to get the paper published before someone else publishes the data has led to poorly written papers containing incomplete, or even fraudulent, data (the best-known cases of fraud can all be ascribed to fierce

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<sup>1</sup> **J.Maddox**, Competition and the death of science. *Nature* vol. 3363 (1993) p.667.

<sup>2</sup> **D.P. Hamilton**, Publishing by-and for?- the numbers. *Science* vol. 250 (1990) p.1331-2.

<sup>3</sup> **L.Roberts**, The rush to publish. *Science* vol. 251 (1991) pp. 260-263.

competition to get research to the press first). Scientific journals are being requested more and more frequently to correct published reports, and even to withdraw entire papers<sup>4 5 6</sup>.

At this juncture the traditional peer reviewing system reveals its intrinsic weakness, also of ethical nature. The editorial boards of scientific journals are swamped with papers that the members should read carefully and comment extensively in any spare time not dedicated to their profession, without receiving any remuneration, yet they are also competitors of the group they are assessing. J. Maddox, editor-in-chief of *Nature*, reported an interesting and pertinent case study demonstrating the improprieties and abuses committed in situations of this type. It also revealed the difficulties a scientific journals can encounter and the consequent need to radically innovate some of its policies.

Another aspect requiring regulation is the issue of “hyper authored” papers. This seems essential in some sectors of molecular biology or medicine (e.g. epidemiology) where the topic of the publication concerns the creation of databases. The hyper authored paper (i.e. with over 200 authors) that highlighted the importance of this issue once and for all was published in the *New England Journal of Medicine*. In June 1994 *Nature* published a multi author paper with 108 authors. The editors hoped that all the signatories had really participated in that study<sup>7</sup>. In other words they hoped that the questionable practice of including the names of those who only provided a material datum (unless the person had requested a specific task involving that research) be discontinued; or that the habit of including the names of the heads of laboratories and departments, who in most cases do not give any contribution, be stopped. Regarding this point it, must be kept in mind that this practice is usually reciprocal, and thus the head ensures that he/she has sufficient publications to guarantee his/her future career, the young researcher is protected and guaranteed by his/her chief when it comes to appraisal of the study by the latter’s “important” peers.

The fierce competition has determined another shift in scientific literature, i.e. more and more assertive titles. As from 1990 J.L. Rosner raised the problem and has harshly criticised this habit in *Nature*<sup>8</sup>. He pointed out that, in particular, molecular biology used this type of title, confirming yet again the pressure it is under to be a goal-oriented science (leaving aside the epistemological meaning of this shift; in effect it greatly consolidates the image of a science of certainties and source of undeniable progress).

Now we address the other aspect of scientific competition involving assessment of single researchers and their scientific output. The real critical point occurred at the end of 1990 when the Institute for Scientific Information (Isi) published statistics revealing that 55% of the scientific papers published between 1981 and 1985 had not been cited in the five years following publication. By adding the papers

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<sup>4</sup> **F.M. Menger** and **A.Haim**, Struggles to correct published errors. *Nature* vol. 359 (1992) pp. 666-668.

<sup>5</sup> **J.Maddox**, Melodrama in research publication. *Nature* vol. 355 (1992) p.767.

<sup>6</sup> **J.Maddox**, Conflict of interest declared. *Nature* vol. 360 (1992) p.205.

<sup>7</sup> **J.Maddox**, Making publication more respectable. *Nature* vol. 369 (1994) p.353.

<sup>8</sup> **J.L. Rosner**, Reflectons of science as a product. *Nature* vol. 345 (1990) p.108.

that had been mentioned, only once to the percentage, the total came to 80%<sup>9</sup>. Obviously, this Isi report created an outcry and animated reactions. It emerged that most eminent representatives of US universities and institutions shared the view that academic culture encouraged spurious publications, and that single researchers were pressurised to produce quantity rather than quality, i.e. bulk, to gain promotion and receive funding.

Isi is a private institute headquartered in Philadelphia and its database contains all the original papers and bibliography reported in about 4,000 scientific journals. Every year this institute publishes 10 issues of the newsletter Science Watch providing all types of statistics regarding contemporary science and its trends.

Nonetheless, reservations and criticisms have been levelled at Isi regarding the journals selected, these being only a very small part of the 108,000 existing ones. Apparently, selection is based on value criteria (the most prestigious), but not only (the most disseminated, the oldest). There is no doubt that many non-English-language journals, as well as highly specialised journals (clearly only read by specialists), have not been included, and that US journals have benefitted.

The survey of the papers includes everything in the journal from the first to the last page (according to the director of Isi). Therefore, not only original papers, but also reviews, letters, comments and so on, are included in the citation index. Hence, the data reported by Isi are strongly influenced by the database set up that seems rather questionable. The person-in-charge state that nonetheless the overall message is still valid as the real influence of some operative decisions would be slight. They object in more detail to more specific objections, e.g. the effect of the widespread practice of citing oneself (legitimate when presenting results connected to a given line of research the previous ones obtained by the author are cited; not so legitimate when one deliberately only cites oneself, and not others who furnished relevant contributions in the sector, in order to raise one's CI) would be only 5-20%. High Citation Indices resulting from negative citations, like the case of the famous paper by Pons and Fleischman on cold fusion, seem to be rare and would not amount to over 7% of cases. Likewise, the incidence of citations of routine methods used that would determine very high Citation Indices is insignificant.

The real problem arose when this newsletter attempted to set itself up as a benchmark for academic and non academic institutions, giving itself a value far beyond its real one and one not supported by the necessary seriousness and precision for it to be adopted to assess the careers of single researchers, or the scientific quality of groups, institutions, etc.

Many sociologists of science are alarmed by the use of Isi data and believe it has introduced aberrations into the scientific world. They also feel that the resulting fierce competition has jeopardised research and

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<sup>9</sup> **G.Tauber**, Measure for measure in science. *Science* vol. 260 (1993) pp.884-886.

the damage is now becoming clear to all. "Time for thought has been eroded" and this undermines the solidity of the same scientific results.

In any case, assessment of individuals is not facilitated (especially if they are one of 200 multiauthors). Indeed, appraisal should assess the quality of their work, their contribution to the field of research, along with more complex skills, such as team-working and talent that make individuals suitable for their role they hold or the funds they manage.

J.Maddox, editor-in-chief of Nature, claimed that objective Citation Index type assessments were equivalent to the bulk (in grams) of publications, and D. Koshland, editor-in-chief of Science went even further when he pointed out that the Citation Index was the product of a database and not a religion. This uproar caused the same IISI to advise that its Citation Index should never be used as to replace careful human appraisal of an individual. Diversely, it should be adopted to assess more complex bodies, such as entire research groups, institutions, and departments.

Nevertheless, some have decided to settle things once and for all and study ways to dampen competition in terms of publications. Indeed, from now on the Harvard Medical School will only take into account the 5-10 most important papers indicated by the same candidate, and other US institutions are following suit. In Europe, some countries, such as Germany and France, pay great attention to these issues and seem heedful not to repeat such unfortunately experimented errors in their institutions.

But what about us? We rush to take out a subscription to ScienceWatch!