

CITATION THEORY AND THE ORTEGA HYPOTHESIS

A comment on "Testing the Ortega Hypothesis: Facts and Artifacts"

by M. H. MACROBERTS and B. R. MACROBERTS, *Scientometrics*, 12 (1987) 293.

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(Received February 6, 1987)

In this paper, we primarily comment on *McRoberts* and *McRoberts'* (*M&M*) article published in this issue of *Scientometrics* but in general also on their earlier paper¹ because all this work focuses on one main—and undoubtedly very important—topic: the validity of quantitative measures of knowledge transfer in science and their usefulness in identifying role, position, and influence of scientists in the advance of science.

We are particularly interested in this topic as both basic research on testing the Ortega Hypothesis² as well as policy-oriented studies on university research performance,^{3,4} based on quantitative methods, are important parts of the research programme of our group.

First some relevant findings of our performances studies will be shortly reviewed. In the second part of this paper, we will discuss our work on the Ortega hypothesis. From our performance studies, we know the constraints and pitfalls, but also the potentialities of citation analyses and, in general, quantitative (bibliometric) methods in science studies. To our opinion, the Leiden Indicators Project (Refs³⁻⁴) was the first very detailed bibliometric analysis on a large scale (two big faculties of a university with a strong international research position) but focused on the micro-level of aggregation (a university research group). Our experience is based on bibliometric trend and level analyses over a period (after recent updating) of 14 years for about 200 research groups in the natural and life sciences, on numerous, extended and often very detailed discussions with scientists in these groups, with chairmen of research committees, etc. The main conclusion from this work was that quantitative analysis as performed in the Leiden project, provides a very useful *monitor* of research performance, identifying in a first and good approximation excellent, mediocre and weak groups. However, background information is *always* necessary to understand the quantitative monitor fully.

*Elsevier, Amsterdam—Oxford—New York
Akadémiai Kiadó, Budapest*

From our findings, we learned that by no means the assumption is necessary, that scientists cite in their papers *all work used* in their research, but still citations can be used to monitor scientific influence.

If one looks at the references contained in one individual paper, many irregularities may be found, such as missing references to important papers, or to the work of authors which have made important contributions to the body of knowledge in a field. Thus, a seriously mistaken picture of the main influences in a particular field would be obtained when only one particular paper is used for this purpose. If one samples additional papers, they may all be subject to similar important irregularities in their reference lists. Papers on a closely related topic may not even share one reference in common. Would this imply that, even if one took a larger sample of papers in a specific field of science, one would never be able to get any sensible idea at all of what papers are more important in one sense or another than other papers for that specific field in a certain period of time? This would be the case if researchers refer (give citations) in a completely arbitrary way.

However, even if all papers would to a large extent (but not completely) cite in an arbitrary way, it would still be possible to detect valid patterns in the citations, if a sufficiently large number of papers would be sampled.

A more serious matter—and directly related with the discussion on the *M&M* paper—would be if authors would cite in very biased ways, for instance by systematically referring to particular papers which did not contribute at all to their papers, or by systematically excluding papers which were important for their paper. But even these types of biases need not be problematic, provided that large numbers of scientists do not share the same biases. By statistical means, one would still be able to estimate within certain bounds whether two (or more) papers are cited significantly different or not. So far, research has failed to show that biases in citation studies are extensive, and do not cancel each other.

M&M take another stance in their paper in this volume. First, they attribute to a number of researchers using citation measures the assumption that scientists cite in their papers *all work used* in their research. As we already pointed out, this assumption is not commonly shared among citation researchers, and even if they do, it would be an unnecessary assumption, as we also have shown above. More commonly, it is assumed that scientists mainly refer to papers which they found useful for their work at the *research frontier* (cf. Ref.⁵). Therefore, many influences which belong to the background knowledge of scientists are not referenced, although scientists would usually admit, when asked, that these influences are important for the work. These influences may vary between the educational influence of a teacher, the ideas of scholars in quite other fields of science, the 'climat' in a research institute, and even many scientific publications. But all these uncited influences can be regarded as part

of or related to the 'tacit body of knowledge'. Indeed, referencing by scientists is, in this sense, incomplete. Complete referencing would require enormous lists of references, and there would be no difference between references and acknowledgements. Therefore, scientists always *have to* make a choice. We think that this choice is induced by the concept of (momentary) 'usefulness'. This is nicely illustrated by the finding that short and rapid publications in a specific field (letters) contain, on the average, considerably less references than 'normal' papers, and these normal papers contain, in their turn, considerably less references than typical review articles. (An average article in a prestigious letter journal like *The Physical Review Letters* contains 13 references; an average article in a prestigious 'normal paper' journal like *The Physical Review B* contains 21 references; and an average article in a prestigious review journal like *Review of Modern Physics* contains 261 references!)

The fact that a paper can be used to underpin arguments in order to reach scientific conclusions, is another important aspect of usefulness. It is difficult to build strong arguments on irrelevant, bad, or even mediocre prior scientific work. However, from our perspective, *convincing* rather than *persuasion* would be the aim of scientific papers. It could be argued, perhaps, that in addition to the above 'regular' argumentation, papers contain also persuasive arguments, resulting in ceremonial citations.

Recent research of ours² has addressed the Ortega hypothesis from a quite different perspective than usual. Instead of using 'highly visible' scientists and 'less visible' scientists as contrasts, the work of PhD students was selected. As it seems highly unlikely that the work of non-graduates will be used in ceremonial citations, or for other purposes of persuasion, the persuasion hypothesis would predict very few differences in citations to the work of graduate students. However, it was found that students which would later receive their PhD with the award 'cum laude' were cited much more frequently by scientists outside their university—especially the papers published two to three years before graduation—than students receiving their PhD without 'cum laude'. Therefore, small elite groups received most of the share of citations, whereas much larger non-elite groups received considerably less than would be expected on the base of their number. It is hard to interpret these findings as confirming the Ortega hypothesis. For full details we refer to *Nederhof and Van Raan*.²

What then is the role of mediocre science? This in fact is the crucial question in the *M&M* discussion. According to *M&M* (who, however, fail to supply exact references here!), some have answered this question in the 'brute way': the majority of scientists is mediocre, and could be dispensed with, without impeding scientific advance. We think that such an answer needs differentiation. Many 'mediocre' scientists work in applied fields, instrumentation, etc. *They* are the 'dozens' of non-cited

scientists meant by S. A. *Goudsmit*, quoted in *M&M*'s earlier paper,¹ p. 155. Their work is of vital importance to science, they are the 'water-carriers' of *Ortega y Gasset*.

Yet there is another group of mediocre scientists: those working at universities and trying to play a role at the research front of their fields, but never reaching a high performance level. Could these researchers be dispensed with? We think that their role in the advancement of science is indeed much less important than mediocre scientists using their capacities not for desperately trying to play a role at the research front but for new instrumentation and technology.

Further research on the above problem is undoubtedly needed to test our ideas. To our opinion, *M&M* have attempted to stretch the concept of 'influence' beyond all reasonable recognition, with subsequently using this distorted concept to show that citation analysis does not entirely meet their premises. To give only one example, their concept of influence is so bizarre, that they seriously (?) consider the problem of how the influence on scientists should be measured of work of which it is certain that these scientists never even knew of its existence.

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