

About “Measuring” Croatian Science

I do not know how much, if at all, the content of the article by B. Klaiæ published in this Journal (1) is of interest to scientists abroad. However, its methodology is related to the psychology of our – peripheral – scientific community. As such, it might be of somewhat broader interest for science studies. (Hence this commentary in English.)

Klaiæ’s paper appears to be important for our scientific community: it was, on one side, published in Croatian (2) as a leading article, whereas in English it was an invited article (1). Moreover, a serious criticism of it has already been published (3), which is mentioned by Klaiæ only *in passim* at the end of the English version (1). Namely, Klaiæ takes up Andreis’ article as if it were only a criticism of errors in citation indexes and not of his methodological approach. Let’s hope Klaiæ will eventually reply to Andreis in more detail.

The data obtained by Klaiæ’s study are, doubtless, very valuable and should have indeed been published, but preferably without the Tables 3 and 4. The reason for that is of ethical origin supporting the following viewpoint: this particular piece of research was done within the context of our small scientific “community” and was intended for it [at least its Croatian version (2)]; I cannot discern a positive role of publishing the ranking-lists of our scientists (it could do more harm than good).

In the Introduction to the Croatian version Klaiæ states: “...it is permissible to analyze the scientific contribution and the quality of individual *research*, projects, *groups* of scientists or scientific *institutions*, there is quite a number of scientometric parameters yielding objective pictures about the scientific contribution of a *given author* or scientific work.” (The emphasized words are important for the ensuing discussion.) The border-line between the applicability of an evaluation method to *individual* authors and to their aggregates is not clear in the above quoted passage (what does it mean, for instance, “individual research”?). However, in the English version the author clearly states, in a somewhat milder manner, that the individuals are, too, subject to such analysis.

Those of Croatian authors who have established themselves within the “mainstream” world science (even without the assistance of citation indexes) are in no need of such ranking lists. Those who have not succeeded in this respect (and thus are not ranked in Klaiæ’s tables) remain unknown. There is even no knowledge of the number of scientists without *any* publication whatsoever and the number of those with publications outside the ISI’s journal coverage. The attempt to estimate the extent to which publicizing the best ones would lead by itself to a positive selection through cuts in financing the “scientific” research of those “hidden” authors, would imply “trespassing” into the sociology (and policy) of science.

Anyway, within this “ranking” framework, my primary objection is that Klaiæ “wrapped” the clear and objective indicators of scientists’ *productivity* (i.e., simply counting their publications) in a theoretically obscure “cover” of *quality*. The latter is supposed to be reflected by the “impact factor” of the journals in which the papers have been published. Without it, the ranking lists would at least signify something directly and would be the most objective “measure” although less relevant for the evaluation of scientific achievements. [The notion of inverse relationship between the relevancy of a given method in evaluating scientific achievements and its objectivity was introduced already in 1978 by Francis Narin (4).]

Another drawback of Klaiæ’s work is that he reached for the social and humanities citation indexes. Namely, it is quite a well known fact that the world picture about those scientific fields as obtained through the citation indexes of the Institute for Scientific Information from Philadelphia (ISI) is one-sided (to put it mildly). Even within the Anglo-Saxon production the citation indexes data mostly refer to the American publications (for instance in sociology and the like). When it comes to other languages, even the French works are neglected, so what can we, from Croatia, expect? This is also revealed by Klaiæ’s results: (a) there are only three subjects from those (“soft”) sciences in (1) [the fourth, from (2) – literature science – is missing in (1)]; (b) Klaiæ (rightly) does not discuss them.

However, in view of the distorted picture which the citation indexes yield, it is useless, and also misleading to publish such research “torsos”.

Nevertheless, numerous Klaiæ’s conclusions about the “production” structure of our science and science policy based upon it are acceptable without the ranking lists of individual scientists. It is especially important [as pointed out by Klaiæ in (1) and (2)]: “Finally, it should be said that *more than four thousand* out of some nine thousand scientists registered in Croatia *did not publish a single world-accessible paper within a sixteen-year-period.*” (Emphasis by B.K.) The term “world-accessible” means, according to Klaiæ, of course, that they did not publish in journals processed by ISI, which I will further discuss. Bearing in mind this finding of B.K., our science does not seem to have changed for the better in the last twenty-odd years. About half of the 4,600 scientists, who cared to fill-in a questionnaire about their scientific production in 1973 (5,6), did not have a single paper published in Croatia, and even three quarters of them had no publications abroad between 1968 and 1973. This implied that only 1/4 (1,200 scientists) of that sample “bore” most of the scientific production in Croatia. I believe that we should not be taking care “about the most beautiful roses in our scientific garden while the weed around them flourishes with no trouble at all”.

My third objection to Klaiæ’s paper is that the literature used is very limited. At the time when many bibliographic data bases are easily accessible, as well as various discussion lists about science-in-society on the Internet (7), one is not allowed to neglect in a paper with scientific aura all those numerous contributions well established in the scientific literature of science studies (“science of science”, and within it also “scientometrics” – as a quantitative, predominately bibliometric analysis of science). For instance, Klaiæ pays much attention to the appropriation of the “publication cake(s)” to multiple authorship, whereas he does not refer to a single specific reference. There are already several books about “measuring” science published during the past decade, but Klaiæ does not find anything important or useful to quote even from a Croatian book (8). If his research was put within the methodological and subject context of the world science studies the author could raise the value of his findings. His “bibliographic amnesia” is counterproductive.

Finally, there is another flaw in Klaiæ’s paper which is not actually important for deductions *within his data about the science in Croatia*. It might be of importance when comparing his data with that from other scientific communities, because his definition differs from the original one. According to Klaiæ (1,2), the impact factor is a quotient between the sum of citations from the preceding two years and the number of papers which received those citations within that same period. Klaiæ shows here (2) a degree of superficiality in writing about the impact factor as “defined for a *given year*”, while he counts the citations from the *two preceding years*. Journal impact factors as used in the Institute for Scientific Information’s (ISI’s) *Journal Citation Reports* “are calculated by dividing the number of current-year citations (e.g., 1997) of the papers published in a journal in the previous two years (i.e., 1996 and 1995) by a combined sum of these papers” (9). A number of studies cast doubt on and even disclaim the value of the journals’ “impact factors” in evaluating scientific research (10,11).

On the basis of a few citation analyses of a positivistic slant (number of citations as a key parameter) one may get the impression that there is no dispute whatsoever about the validity of this method in our institutionalized (scientific and administrative) public domain. However, it is not so in the least, and the doubts do not stem from the trivial errors in the citation counts. The MacRoberts bring to the fore a confrontation between two understandings of the science process (11,12). The traditional one is favored by our “amateurish professionals” in “measuring” science, from which follows the acceptance of citations for evaluative purposes without any consideration, as contrasted with the constructivists’ understanding that citation cannot be made use of unreflectively. The following is a paragraph from their “Conclusion”: “The basic assumptions of citation analysis are clearly false. The assumption that research cited by scientists in their own papers represents a roughly valid indicator about the influence (of those cited works – S.M.) upon their work has been falsified repeatedly, as well as the assumption on which the latter is based: that scientists are motivated to cite their influences in order to give credit where it is due” (12). In this paper the authors cited all the previous ones in which they showed that only a small portion of what had to be cited – was cited indeed, a finding they empirically confirmed recently (13).

But the most acid criticism was formulated by Woolgar (14): (to quote from the “Conclusion”)...“We should eschew attempts to arbitrate on the accuracy of citation analysis in favor of a sociological

analysis of its institutionalized practice. This means, in particular, that we try to anticipate the circumstances under which the application of this measurement technology to the concept of quality will result in modifications (or redefinitions) of what counts as quality”.

Bearing in mind Woolgar’s categorical negation of the value of citation analyses published so far, my colleagues and I have undertaken a longitudinal *context* citation analysis of 219 papers published by scientists from “Rugjer Boškoviæ” Institute within the first ten years of the institute’s existence, which were cited in the course of 21 years after their publication. Apart from the citation counts and their yearly incidence (15,16,17), we finally paid a special attention to the citation context (18). We recorded for each citation (i) the section of the paper in which it was found, and (ii) the very nature of the citing sentence(s). Three different ranking scales were devised and all of them were congruent, with the simplest, that of taking into account only the way citations were given, being sufficient. We ranked neither the individual authors, nor their individual papers, but the research topics (28 of them) each comprised of half a dozen or so papers. The ranking appeared to be a function of the importance of the cited papers for those citing them. Eventually, when comparing the ranking of the topics by this citation *context* analysis with the ranking obtained by citation *counts* (frequencies), no correlation was found.

In (1) Klaiæ says that there is no national (Croatian) bibliography suitable for scientometric analysis. That is correct as far as the on-line (or CD-ROM, for that matter) accessibility is concerned. Still, something could be done. In the National and University Library there are continuous yearly computer bibliographic recordings of the papers which are being published in some 300 Croatian journals, in addition to the bibliography of monographs. What is indeed necessary is a data base of all the publications from a broad spectrum of scientific research, from “hard” to “soft” disciplines, like the one in Australia (19,20) and Latvia (21). The former served to determine the contribution of their publications outside the ISI’s coverage. They came to the conclusion that one must not rely exclusively on the ISI’s data. The selection of the bibliographic data for such an analysis will depend on the science structure in a given country, but it is time to finally get rid of the (Klaiæ’s, too) notion that the “world accessible” publications are only those covered by the ISI. The latter is indeed an efficient tertiary data basis for *retrieval* of scientific literature, but there are numerous secondary bibliographic data bases securing a broader accessibility of scientific literature.

Some more desirable discussions than the present one could be possible if the Croatian Ministry of Science allowed an approach similar to that in Australia (or in Latvia) incorporating (eventually) the research of a *professional team* for the analysis of scientific production. For the beginning at least, it would be desirable to secure a continuous financing of one of the existing journals under the provision to publish papers about the studies of science. We have no (domestic) journal dealing exclusively with that subject. One of the candidates could be *Rugjer*, as it has already attracted the authors writing *about* science itself, although it is preponderantly orientated towards presentation of scientific matters for the lay-public. Should the Ministry of Science decide on a new financial pull towards science studies it would be beneficial to discuss *in public* the choice of a proper journal for that purpose.

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