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THE BIBLIOMETRIC ASSESSMENT OF UK SCIENTIFIC PERFORMANCE – SOME COMMENTS ON MARTIN'S "REPLY"

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No new arguments or evidence that undermine our conviction that available scientometric measures do not indicate a statistically significant "decline" of British science in the first half of the eighties have been found in *Martin*'s reply.

Martin's meticulous scrutiny[1] actually confirmed the main message of our "Flash"[2], i.e., that extremely inconsistent conclusions can be drawn from the very same set of data using formally correct arithmetical manipulations. We restrain, therefore, enumerating any more arguments in the continuing debate on the decline of British science until a considerable amount of new data becomes available.[3] In what follows we are commenting on some of the methodological questions alluded to in *Martin*'s "Reply".

1. Our critic pinpoints as one of our supposed "flaws" that we did not take into account that "an increase in the absolute number of papers produced by a single country [...] may or may not correspond to a genuine increase in scientific output." Although it is not quite clear what is to be understood under the term "genuine increase", this assertion seems to be practically another version of what was formulated in our "Flash" as "any attempt to measure national performances by total [publication or citation] counts proved to be uncontrollably corrupted".

2. In our "Flash" we naively reported the range and the average of the "percentage share" indicators and of their "annual mean relative change". Never in our wildest nightmares would we have thought that one of these innocuous figures (+0.93%) would be practically considered the "final score" of our whole exercise and would be attacked accordingly. We rather intended to put the emphasis on the *ranges* of the

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indicators and on the fact that while *any* of the "percentage share" indicators firmly supports the UK's second place in the world rank (and that's what we consider a shining instance of *converging partial indicators*), the "annual change" indicators appear to be bewilderingly diverging.

3. As to the question of using "adjusted" or "unadjusted" world totals in "all author" publication counts, we definitely vote for the "unadjusted" ones. With the rare exceptions of some comparative methodological studies, we have always used this version because of its clear "physical" meaning. A UK share of 10% means in this sense that 10 out of every 100 papers in the world have at least one contributor from the UK. It is hard to explain, however, the "meaning" of a 10% share in the "adjusted" scale. All author counts using "unadjusted" world totals appear to be a rather solid ground for publication productivity assessments, the "subadditivity" of percentage shares, however, meets certain incomprehension in the lay public.

In our opinion, as contrasted with *Martin*'s, "all author" indicators based on "unadjusted" world totals are *not* directly influenced by the trends in international co-authorship or by the cooperativity of other countries. The fact that 10 out of every 100 papers is coauthored by a UK author does not change whatever the co-authorship pattern of the other 90 papers may be. An increasing co-authorship in the "outside world" would dramatically alter, however, the "adjusted" version of the percentage share, which would then indicate a spurious "decline". What is worse, a tendency of increasing international co-authorship of the country in question (the UK, in our case) would result in a decrease in the indicator usually considered the most "realistic" one: the percentage share in "fractional authorships"! In an extreme example, if a country instead of publishing in total national isolation, would publish "in pairs" with foreign coauthors, its percentage share in "fractional co-authorships" would drop by 50%. In addition, one must not forget that international co-authorship surely has some effect on publication productivity. This is a "dynamic" interaction, which cannot be taken into account by any simple arithmetical manipulation.

In most of our assessments we are using indicators based on first author counts. By no means are they free from any of the abovementioned problems. Our choice is motivated by an argument which is bashfully substituted by suspension points in *Martin*'s extract from one of our earlier papers: it "offers the unquestionable advantage of technical simplicity"[5] (and inexpensiveness -- may we add even more unabashed here).

4. In the "publication year" *vs* "tape year" dichotomy, *Martin* seems to throw out the baby with the bath water. Although for the last year of the period under study

(1985, in our case) the "publication year" data were incomplete and therefore biassed, there is no justification for discarding (or even just "adjusting") the data of the other four years. In fact, differential time delay in entering the database may corrupt "tape year" data for all but the last year. Among two countries having the same *decreasing* percentage shares, the one with less of a time lag will clearly be underestimated by the "tape year" method. In our opinion, the most reliable information available about the "age" of a publication is its publication year and it seems that it is worth even losing the data of the last available year in order to win a more balanced view on the others.

5. The question of whether one should or should not include "letters" when counting "relevant publications" is a particularly subtle one in assessing British science. As it was pointed out earlier,[6] the share of "letters" in UK publications is close to 10%, a rather high percentage as compared to the 2-4% of most other Western countries. About 80% of the papers of *The Lancet*, a traditional British journal of highest prestige in its field publishing nearly 12000 papers in the period 1981-1985 (half of them by British first authors), are labelled as "letters" in the *SCI* database. Citation analysis, while disclosing several specific features of the various publication types, gives clear evidence of the significance of the contribution of "letters" to the body of scientific knowledge. Our decision of including "letters" in our datafiles was based on careful consideration of well-documented facts.[6,7] Nothing similar can be found in the literature to support the view of *CHI Research* that "such 'letters' are generally less significant than the types of publication included in S[cience] L[iterature] I[ndicators] D[atabase], and there would be no advantage to any extension in coverage".[8] We readily agree that a major source of difference between *CHI*'s figures and ours is in the difference between handling "letters" but we completely deny to consider this difference a "flaw" in our methodology. We are also inclined to accept our critic's proposition to resolve the apparent contradiction by considering both approaches in parallel, so much so that our "Flash" was the only publication up to now where this proposition was actually realized.

6. Indicator #17 in Table 4 (estimate of tape year/fix jrnl/fract author/ANR) is the preferred option of *CHI Research* and, if we understood correctly, also of *Martin*. The "mean annual relative change" of this indicator for the period 1981-1985 is -1.10%, its standard error is 1.14% . Would anybody call this a statistically significant "decline"? We certainly won't, but rather feel our opinion confirmed: "neither the view of a continuing decline nor that of a remarkable increase of British science in the first half of the eighties can be supported by valid bibliometric/scientometric

arguments. The annual changes of any of the indicators considered had no statistical significance, and no trend distinguishable from the effect of random fluctuations could be observed."[2]

"British science, like science in other countries, is in decline in some fields and on an upwards move in others. Moreover, in any population of papers, the distribution according to quality is highly skewed. [...] Probably the most important factor in improving sceintific performance of a country is finding a way to raise the quality and not the quantity of the publications."[9]

Postscript to Martin's Postscript

Referring to our paper on the facts and artifacts concerning the decline of British analytical chemistry,[10] *Martin* attempts to highlight a certain inconsistency between various bibliometric approaches we adopted there. We are afraid that *Martin* overlooked the simple fact that one class of indicators used (activity and attractivity indices) reflect the internal publication and citation balance of science subfields *within a country*, while relative citation rate measures citation impact as compared to international reference standards. Thus, it is not surprising at all if a field (or subfield) in a given country proves to be a flourishing one (as reflected in relative citation rates) in spite of its low activity and/or attractivity indicators (see Ref.5).

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