

ON THE "SCIENTOMETRIC DECLINE" OF BRITISH SCIENCE. ONE ADDITIONAL GRAPH IN REPLY TO BEN MARTIN

L. LEYDESDORFF

*Department of Science Dynamics Nwe Achtergracht 166
1018 WV Amsterdam (The Netherlands)*

(Received May 21, 1990)

With respect to the issue of whether the scientometric measurement of "the decline of British science" is an artifact of the specific database and underlying assumptions in methods, I argue that there are fewer analytical objections against measurement by using *SciSearch On-line* than against other methods (based on the "fixed journal set" and "fractional counting"). The measurement of "international co-authorship", i.e. a network indicator, should not be confounded with measurement of performance of a single nation. The time series for the different subsets of UK-publications, which have been proposed, are given. None of the indicators can be shown to exhibit a trend (in contrast to a drift). The hypothesis of a decline has therefore to be rejected.

In 1985, in the framework of another research project, and as a courtesy for the Dutch Ministry, I measured Dutch performance using the on-line *SciSearch* database for more recent years than the Ministry had obtained from a commissioned project of the Government Council for Science Policy *RAWB*. For the overlapping years in the two measurements my figures were different, but since the trends were both upward, nobody was concerned about the discrepancy. I explained in my report that the differences were probably due to the assumption of the fixed journal set underlying the CHI-data.¹

When I discussed these figures with Ben *Martin* in August 1985, we had an idea that my figures might shed further light on the urgency of the issue of "the decline of British science," about which he and his colleague John *Irvine* had published a paper in *Nature* at that time.² After a telephone call, John *Maddox*, the Editor of *Nature*, indeed urged me to send a short letter ("five hundred words") on it. That evening, I realised that I should check for the comparison between the two countries against the difference between the two measurement methods, since the noted UK data were based on the CHI-database, while mine were based on *SciSearch*.

The next day at noon, I had figures for the UK, which did not show "the decline," but on the contrary, a minor increase. I called John *Maddox* again, and he said to me:

"Very interesting! Make it a thousand words." After some correspondence between John Maddox, Frances Narin of CHI, and me, my letter was accepted in January 1986, but it never appeared. Although kept away from the public, my manuscript was circulating widely among British science policy makers.

In his recent paper,³ entitled "The Bibliometric Assessment of UK Scientific Performance? A Reply to Braun, Gänzel and Schubert," Martin mentions that I challenged his (and Irvine's) conclusion of "the decline of British science," in 1988. Indeed, in that year some of my British colleagues (among them, John Irvine) intervened in order to get the debate going in *Science and Public Policy*. However, in the light of the above story, it is misleading to date my critique as recently as 1988.

In 1985, my point was mainly to be cautious about assumptions and selections. I showed that with other assumptions very different results could be obtained. Already in the 1985 correspondence, issues were raised that "the decline" of British science could be an artifact of (i) the assumption of the fixed journal set, (ii) the specific assumptions about what to count as a paper, and (iii) fractional counting.⁴ Although being most aware of these points of critique since 1985, my opponents⁵ and others (including John Maddox)⁶ repeated the claim of a "further decline", in several papers in 1986 and 1987, without much hesitation, and sometimes obviously for political reasons.

In 1988, my opponents claimed to have discovered eight separate flaws in my approach which would lead to "a spurious upward trend on the UK's share of the world total of publications."⁷ However, they failed to show that therefore their claim of a significant decline could be maintained.

In this paper, Martin admits – in response to another paper –⁸ that "one of the errors (...) can now be overcome, but several of the other problem still remain." Which are these alleged errors of using simple on-line measurement techniques, and how do they affect measurement of the data?

First, in response to my original critique of using fractional counting for national performance measurement – *ceteris paribus*, fractional counting leads to a decline of a nation's performance because of increasing international co-authorship –⁹ Anderson et al. (among them Martin) replied that by using integer counting, i.e., by giving a full point for each paper to each country, the percentage shares adds up to more than 100%. Therefore, they proposed a "corrected divisor" in order to normalize.¹⁰ Indeed, this should create problems, since it is an incorrect normalization. *Ceteris paribus*, the use of this "corrected divisor" leads to a negative

effect on the performance of one country when authors in *other* countries increase their international co-authorship.¹¹

The basic problem is that one should not try to measure two developments of a very different nature with a single indicator. (At least, one should not be surprised by the confusion that may follow.) International co-authorship is a network indicator, while numbers of publications indicate performance for nodes in the network.¹² The developments in the two dimensions (and their interaction) merit separate study.¹³ Therefore, in performance measurement it is best to use integer counting, and if wanted to specify the percentage of internationally co-authored publications, as long as there is no warrant for a weighted attribution of the latter. The "serious problems", which are signalled by *Martin*, are a consequence of his own confusion.

Martin's second objection to my results is "the inclusion of non-research articles such as book reviews, editorials and obituaries (which) tends to inflate the UK share of world publication total." However, it is obvious that using on-line methods, one can most easily disaggregate for the various document types. Actually, I gave curves for the three document types, which CHI selects – articles, reviews and notes – in my original article.¹⁴ However, I did not repeat that yet with the "Corporate Country" instead of the "Corporate Source" as search term.

Therefore, I want to add one more graph to the discussion. Figure 1 gives three curves, all based on using *SciSearch* on-line at the DIMDI-installation in Cologne, and on the "Corporate Country" field in it.¹⁵ The lower dashed line is the on-line equivalent of the CHI-subset (articles + reviews + notes), while the upper dashed line is equivalent to the subset proposed by *Braun* et al.¹⁶ The trends in the two curves are almost identical.

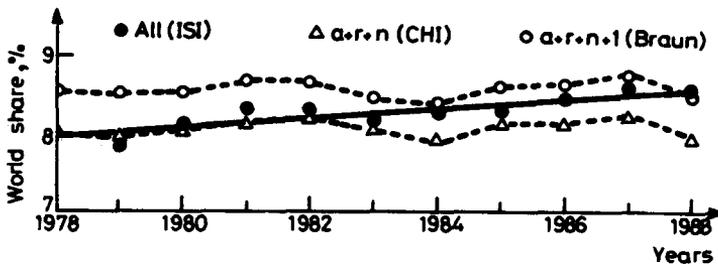


Fig. 1. World share of publications (%) for the UK using SciSearch

The solid line in Fig. 1 represent the linear regression for the total set. In this case, the linear regression is significant at the one percent level with $R^2 = 0.84$; the slope is $+ 0.067\%$ / year.¹⁷ However by using ARIMA time series analysis, none of the series can be shown to exhibit a non-stationary trend (in contrast to a drift). Therefore, whether one finds a decline or an increase upon inspection of the graph is also heavily dependent on which observations one includes in the analysis.¹⁸

As I argued previously,¹⁹ the major argument for the dynamic journal set (in contrast to the "fixed journal set" as used by CHI) is its yearly inclusion of new journals, which may be of particular relevance when measuring performance for advanced industrial countries. By using integer counting in the dynamic journal set, the CHI-indicator gives a stable pattern (with statistical fluctuations) for the UK. Therefore, we may draw the conclusion that "the decline of British science" was indeed an artifact of two erroneous assumptions made in the processing of the data, notably (i) the fixed journal set, and (ii) fractional counting. The latter is confounding two effects, the former inappropriate for studying dynamic effects.

I want to add that despite its methodological fruitfulness, the issue was to a certain extent a non-issue. The trends, particularly, in the UK-data seem sensitive for minor choices, because they exhibit primarily stability. Elsewhere,²⁰ I have shown that in the case of similar performance measurement data for France, one can easily fit the data with curves which show decline, increase or stability. Thus, the question of what the data teach us in terms of trends has to be studied with more sophisticated methods than visual inspection.²¹

Notes and references

1. L. LEYDESDORFF, *Methoden ter Bepaling van de Percentuele Bijdrage van Nederlandse Publicaties aan de "Sources" van de Science Citation Index*, Amsterdam, April, 1985. The figures for Holland were published in: L. LEYDESDORFF, The Science Citation Index and the measurement of national performance in terms of numbers of scientific publications, *Scientometrics*, 7 (1989) 111-20.
2. J. IRVINE, B. MARTIN, T. PEACOCK, R. TURNER, Charting the decline of British science, *Nature*, 316 (August 15, 1985) 587-590.
3. B. MARTIN, The bibliometric assessment of UK scientific performance? A reply to Braun, Glänzel and Schubert, *Scientometrics*, this issue.
4. Additionally, there was an issue about misspellings, but this was later dropped.
5. J. IRVINE, B. MARTIN, Is Britain spending enough on science, *Nature*, 323 (1986, October 16) 591-4; B.R. MARTIN, J. IRVINE, F. NARIN, C. STERITT, The continuing decline of British science, *Nature*, 330 (1987, November 12) 123-6.
6. British science over the hill, Editorial of *Nature*, 323 (1986, October 23) 655-6; D.C. SMITH, P.M.D. COLLINS, D.M. HICKS, S.M. WYATT, National performance in basic research, *Nature*, 323 (1986) 681-4.

L. LEYDESDORFF: SCIENTOMETRIC DECLINE OF UK SCIENCE

7. J. ANDERSONS, P.M.D. COLLINS, J. IRVINE, P.A. ISARD, B.R. MARTIN, F. NARIN, K. STEVENS, On-line approaches to measuring national scientific output - A cautionary output, *Science and Public Policy*, 15 (1988) 153-161.
8. LEYDESDORFF, *op. cit.*, 1989.
9. L. LEYDESDORFF, Problems with the "measurement" of national scientific performance, *Science and Public Policy*, 15 (1988) 153-61.
10. ANDERSONS et. al., *op. cit.*, 1988.
11. L. LEYDESDORFF, Performance figures for British science, *Science and Public Policy*, 15 (1988) 270.
12. G. LEWISON, P. CUNNINGHAM, The use of bibliometrics in the evaluation of Community biotechnology research programmes, in: A.F.J. VAN RAAN, A.J. NEDERHOF, H.F. MOED (Eds), *Science & Technology Indicators. Their Use in Science Policy and Their Role in Science Studies*, DSWO, Leiden, 1989, 99-114.
13. L. LEYDESDORFF, The prediction of science indicators using information theory, *Scientometrics*, 19 (1990) 297-324.
14. LEYDESDORFF *op. cit.*, 1988.
15. H.F. MOED, The use of non-line databases for bibliometric analysis, in: L. EGGHE, R. ROUSSEAU (Eds.), *Informetrics 87/88*, Amsterdam, etc.: Elsevier, 1988 pp. 137f.
16. T. BRAUN, W. GLÄNZEL, A. SCHUBERT, Assessing assessments of British science: some facts and figures to accept or to decline, *Scientometrics*, 15 (1989) 165-70.
17. However, the value of the use of regression models is debatable, since the observations are probably auto-correlated.
18. Additional data for 1989 are:

total set:	8.70% (+0.08%)
CHI-subset:	7.97% (-0.01%)
Braun-subset:	8.53% (0.00%)

These data have not been included in the analysis, since the analysis was done in the framework of a research project which was finished in 1989. See: LEYDESDORFF, *op. cit.*, note 13.
19. LEYDESDORFF, *op. cit.*, 1988.
20. L. LEYDESDORFF, Some methodological guidelines for the interpretation of scientometric mappings, *R & D Evaluation Newsletter* 1989, Nr. 2, 4-7.
21. See also: LEYDESDORFF, *op. cit.*, note 13.