

Victorian Growth: A Rejoinder

By DONALD N. McCLOSKEY

I AM glad that Dr Aldcroft and I agree on so many points of method and substance. We agree, for example, that one must inquire into the elasticity of aggregate factor supply before explaining Victorian growth in terms of aggregate demand; that total, not partial, productivity is the relevant measure of how well the Victorians used their factor supplies; and that the record of total productivity, pieced together in a rough way in my 1970 article and now calculable in greater detail from Charles Feinstein's pathbreaking work, belies any assertion of a mid-Victorian failure. Since these three points are the essence of my article and since, judging from his previous work on the subject, one could have expected Aldcroft to disagree with them, it would appear that I have little cause for complaint.

Still, there are some significant points of disagreement. The first is the timing of the break in the trend of productivity, an issue in descriptive statistics. If one accepts Aldcroft's reasonable procedure of dividing the period 1855-1913 into business cycles, there are four peaks in business cycles available to stand as "the" date of the climacteric, 1873, 1883, 1890, and 1900. The averages of the annual rates of productivity change calculated from Feinstein's data are:¹

1856-66	1.090	1884-90	1.090
1867-73	1.390	1891-1900	0.717
1874-83	0.693	1901-13	0.233

It should be noted that the pattern, allowing for the differences in dates chosen for comparison, is similar to that in my original calculation and in Aldcroft's calculations. We are not disagreeing about the evidence but about its interpretation. The issue can be put as follows: if for purposes of description one wishes to distinguish two periods within each of which a single rate of growth in total productivity is supposed to obtain, what year should be chosen to separate the two periods? That is, between which periods is the difference between the average rates of growth most significant, in the statistical sense of "significant"? Aldcroft believes that it is "almost certain" that the periods should break in 1890, whereas I believe, in the words of my article, that "given the uncertainties of the data . . . the most precise defensible statement is that there was little cause for alarm in the

¹ The source for these figures, as for Aldcroft's Table 2, is app. Table 20 in C. H. Feinstein, *National Income, Expenditure and Output in the United Kingdom, 1855-1965* (Cambridge, 1972). I have subtracted the annual rate of change of fixed reproducible capital per man, multiplied by capital's share in domestic product (assumed to be 0.4 throughout on the basis of a rough adjustment for the capital component of Feinstein's estimates of income from self-employment and income from land rents), from the annual rate of change of gross domestic product per man. The reported statistics are averages of these annual figures. The choice of peaks in the business cycle is W. W. Rostow's (his major peaks, in which "conditions of virtually full employment were reached") in his *British Economy of the Nineteenth Century* (Oxford, 1948), p. 33.

behaviour of British productivity" down to 1900. A way to resolve this dispute is to examine the results of tests for the significance of difference between two means for each of the four alternative periodizations. The materials for these tests are given in Table 1.

Table 1. *Tests of the Differences Between Mean Annual Rates of Productivity Growth for Four Alternative Periodizations, 1856-1913*

<i>Last year in the first period (date of climacteric)</i>	<i>Difference between means of first and second periods</i>	<i>Variance of first period</i>	<i>Variance of second period</i>	<i>Students t—statistic</i>	<i>Probability level at which difference is significant (one-tailed)</i>
1873	0.5865	2.410	2.329	1.347	0.10
1883	0.4277	2.117	2.770	1.039	0.15
1890	0.5931	1.868	3.076	1.444	0.08
1900	0.7322	2.465	1.848	1.522	0.06

The test discerns whether one can reject the null hypothesis of no significant difference between average rates of growth in the first compared with the second period, as compared with the alternative that the rate of growth in the first is larger than in the second period. The last column gives the level at which the difference is significant, that is, it gives the probability of making the error of accepting that there was in fact a deceleration in growth between the two periods when in fact it arose by chance. It is clear why no one has chosen 1883 as the climacteric year and equally clear why each of the remaining three dates has its enthusiasts. Of the three, as far as descriptive statistics on this aggregate level can distinguish them, 1900 is the appropriate choice for the climacteric.

The distinctions in significance among the three dates are not great, and this brings me to the second point of disagreement between us. In the article I emphasized that "even with very good data the range of doubt in the results is large." By contrast, Dr Aldcroft believes that the aggregate measures of productivity warrant assertions about when, "certainly", productivity turned down. The tests reported in Table 1 bear out my original judgement, for they imply that, even if one believes that the measure of productivity change is exact, there is so much annual variation that the three potential climacterics are statistically speaking nearly indistinguishable. To take moving averages of the annual statistics ("some smoothing technique", Aldcroft suggests) would merely conceal the variability of the measure, not remove it. Although I did not, as Aldcroft believes, make "the assumption that labour and capital inputs grew steadily through each decade", he himself apparently does: if this were the case there would be considerably less variability in the rate of productivity growth to disturb the certitude of comparisons between periods. But the uncertainty cannot be ignored. Indeed, as I emphasized in the article and as Feinstein emphasizes in his book, the uncertainty is deeper, in the statistics themselves. Measures of productivity are residuals and are therefore especially sensitive to errors in the series on which they are built. The appropriate inference, as I concluded in the article, is that "the case for failure or success in the growth of productivity must rest ultimately on international comparisons of productivity in specific industries, not

on the aggregate measures about which the controversy on British economic performance has hitherto revolved.”

The final, and most important, point of disagreement is a related one. Before turning to an explanation of a depressed productivity performance it is desirable to establish that it was in fact depressed by some relevant standard. Aldcroft, in common with many other writers on the Victorian economy, is vague on what standard he has in mind. My article, on the other hand, is explicitly comparative, adopting as the standard of comparison the performance of the most successful economy of the time, the economy of the United States. This is a stringent test: the United States was catching up to British standards of technology in many industries during the late nineteenth century, and its rate of productivity growth could be expected on this account to have been higher than the United Kingdom's. Yet their rates of productivity growth were roughly comparable down to 1900, and the Anglo-German comparison yields similar results.¹ The divergence after 1900 could be explained in terms of British exhaustion of a technology that Germany and the United States were still acquiring. This is true, for example, of the industry that has served most often as the worst case of slow British productivity growth, iron and steel.²

In any case, in the absence of comparative perspective and industrial detail it is exceedingly difficult to discover how the British economy performed and why. Aldcroft's hypothesis that “productivity improvements . . . were more difficult to exploit in conditions which facilitated growth through factor accumulation” provides a case in point. In British shipbuilding, open-hearth steel-making, and retailing, for example, productivity growth was rapid after 1890 despite rapid factor accumulation. And factor accumulation was more rapid in the American and German economies than in the British, even though it is the allegedly superior performance of these economies that motivates the entire discussion. If coal and cotton in Britain awaited the inter-war period to “disgorge their over-inflated labour supplies”, agriculture in Germany and America did the same. And so forth. The discipline of a detailed comparative perspective is lacking in Aldcroft's argument.

The central message of my article, then, stands unrevised. To go beyond its purpose of casting reasonable doubt on the traditional tale of British economic failure after 1870 would have required a book, not an article. The book is in the process of being written, by many hands.

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¹ From 1880 to 1910 the rate of growth of total factor productivity in Germany was 1.13 per cent per annum (calculated from W. G. Hoffman, *Das Wachstum der deutschen Wirtschaft seit der Mitte des 19. Jahrhunderts* (Berlin, 1965), pp. 87, 204-6, 253-4, 507-9, 827 ff; I am indebted to Peter Lindert of the University of Wisconsin for pointing out to me the similarity of German and British rates of Productivity growth). This is comparable to the rate of 0.87 per cent per annum for the United Kingdom, 1884-1900.

² I refer the reader to my recent book, *Economic Maturity and Entrepreneurial Decline: British Iron and Steel, 1871-1913* (Cambridge, Mass. 1974), and, for a review of the relevant literature to, D. N. McCloskey and L. G. Sandberg, ‘From Damnation to Redemption: Judgments on the Late Victorian Entrepreneur’, *Explorations in Economic History*, ix (1971), 89-108.