growth). This is comparable to the rate of 0·87 percent per annum for the United Kingdom, 1884–1900.

3 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), pp. 89–108.

A Counterfactual Dialogue with William Kennedy* on Late Victorian Failure or the Lack of It

Kennedy: I read your article and liked the way you set up the questions but not the way you answered them. You're right, I think, to emphasize that the elasticity of the supply of factors is important in assessing what it was possible for late Victorians to accomplish, that the arithmetic of savings rates and capital accumulation is a proper part of this assessment, and that the desirability of investment abroad is a central issue. But I would reverse each of your conclusions. And I do think that in one important respect you have set the question up incorrectly, by ignoring structural change. Firms adjust their decisions in response to changes in available technology. That adjustment, however, is influenced by a wide variety of factors. Hence it may not be sufficient simply to assume that the British economy, on the strength of private markets, was able to transform its structure before 1914 so rapidly as to take full advantage of evolving production possibilities.

McCloskey: Wait a minute: I didn't 'assume' that the British economy worked well; I tried to show that it in fact did by comparison with America, at least in terms of aggregate productivity. I do not suppose any economy has taken '*full* advantage of evolving production possibilities'. That is why it is important to have an explicit and reasonable standard of comparison in mind when assessing British performance. I agree with your implication that it would be more persuasive to look at productivity in individual industries – I suppose that's what you mean by 'structure' – and I say this towards the end. The paper was self-consciously crude in speaking in aggregates like national income and the rest. Other essays (including some of my own), such as Sandberg's on ring spinning, Saul's on engineering and Lindert and Trace's on the Solvay process, look behind the aggregates. What one is looking for behind the aggregates is evidence that there was some advantage to be had in specific industries from adopting new

*The opinions attributed to Dr Kennedy are those in a draft presented to the third Anglo-American conference on British economic history in September 1973 and printed in his 'Foreign investment, trade, and growth in the United Kingdom, 1870–1913', *Explorations in Economic History*, vol. XI, 1974, pp. 415–44.

production possibilities. If unusual profits could have been earned from, say, adopting basic steelmaking in the 1890s, this would be an indication that private markets were for some reason weak in allocating resources to the right processes, whether from the causes you list or from the cause I was chiefly concerned with in the paper, sloppy entrepreneurship. This is a good way to test for the adequacy with which Englishmen met new opportunities: could they make money by taking them? In the case of basic steelmaking (this is an advertisement for a chapter of my forthcoming book on the iron and steel industry) they could not. And so it has gone for the most part in the few other specific cases of alleged missed opportunities that have been examined recently.

K: Now *you* wait a minute. The missed opportunities were *structural* as well as technological. Isn't it obvious that Britain was overcommitted in the staple industries of the industrial revolution? After all, the new industries grew fastest after the war, while coal, cotton, shipbuilding and steel collapsed. Furthermore, in chemicals, electrical engineering and parts of mechanical engineering Britain before the war had not committed as much as had Germany or America. These were the industries of the future and of the nations with the most on the ball economically. You talk of comparative perspective! Use it!

M: I don't think that kind of international comparison is to the point. The US and Germany had enormous agricultural sectors: would you say, then, that Britain's should have been larger? Britain had little trouble moving capital out from agriculture faster than the US and Germany when agriculture was no longer profitable, which is at least some evidence that structural change (a quite large one at that) was possible in Britain when it was desirable. That America had, say, more telephones than Britain is not evidence that Britain should have had more, unless one wants to assume at the outset what is supposed to be the conclusion, namely, that America did everything better. The reasoning is circular.

K: But isn't it obvious from Britain's experience after the war that the failure to get out of textiles, coal and shipbuilding before 1914 made the British economy, as Austin Robinson put it, 'fragile'? These were the sectors that were large in Britain relative to the US, and each suffered heavy unemployment and a contracted labor force after the war. Even if you do not accept the structure of foreign countries as indicating a bad allocation of resources in Britain before the war, surely you'll concede that the structure that actually developed in Britain by 1939 is relevant.

M: No, I won't. The point is that you can't tell what was an appropriate structure for the British economy before the war – interpreting 'structure' to mean 'the pattern of allocation of resources among different activities, whether within individual industries or among them' – without looking into the profitability of the existing structure

relative to alternatives *at the time.* I think that remarks about the 'fragility' of British industry on the eve of the war or its 'overcommitment' to old industries, when they are applied to the prewar period, express simply a feeling of irony, using the historian's most valuable but often most overused resource, hindsight. The writer is saying, 'Look at the irony: the very industries that proved so profitable for Britain in the nineteenth century turned out to be bad investments after the war. It is sad indeed that Britain was led down this garden path.' But this says nothing about the adequacy of the late Victorian (or Edwardian) response to opportunities as they could have been perceived by sensible men at the time. The reasoning involved is, once again, circular. One assumes that what eventually happened should have happened earlier and – presto! – one is able to indict the British capital market or British entrepreneurs for not leaping to the allocation of the future. The trouble, of course, is that one could on the same basis indict anyone. A small case in point (more advertising for my book, this) is by-product coking, which was used in Germany on a large scale long before it was used in the UK. By the retrospective identification of desirable changes that you are using the UK should have adopted it earlier. What is amusing about this case is that those paragons of technological progressiveness, the Americans, were even slower than the British to adopt it. The people who use the slow adoption as a club with which to beat late Victorian entrepreneurs in Britain seldom recognize this fact. It suggests that the adoption of the process was a response to relative factor prices (namely, the price of labor relative to the price of coal) rather than a quirk of national character.

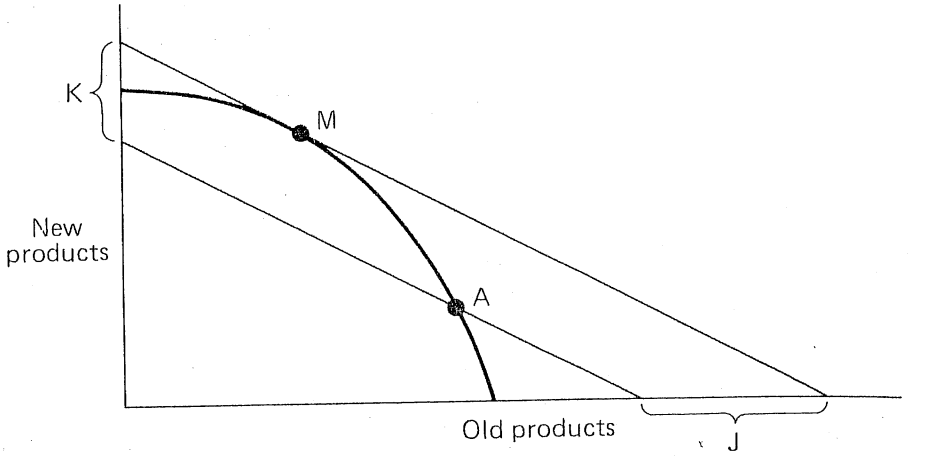
K: Well, I don't know about coke ovens, but I do know that it was the industries in which British resources were concentrated that were vulnerable after the war.

M: True enough, but conditions changed after the war in a way that British entrepreneurs and investors could not have been expected to predict. Indeed, many informed prewar observers, among them Alfred Marshall, riveted their attention on the competition from America and Germany in the old industries. My working hypothesis is that the British were doing pretty well before the war adjusting to conditions that they then faced. That conditions turned against them later does not illuminate the working of the prewar economy. I don't like to keep throwing agriculture back in your face, since I think I can sustain this argument in industry alone, but look at the postwar depression in American agriculture. Are we to conclude that there was some failure on the part of American farmers and their bankers before the war?

K: But surely the British *could* have moved into the new industries. I've done some calculations, in a less offhand fashion than you did in one sentence and a footnote in your paper, that identify vast low-wage (and presumably low-productivity) areas of the British economy, a reserve of

low-paid labor to meet the demands of expansion, if such a demand had ever been made.

M: Sure, they *could* have moved into the new industries faster, just as Americans *could* have moved out of agriculture before the war faster than they did. But the issue is, *should* they have done so? One way to answer the question is to calculate the gain to national income from the reallocation or, equivalently, to calculate the profitability of it. Look at this diagram:



You're saying that the UK was at a point such as A and in view of the prices of the products should have been at a point like M. I'm saying that the way to find out is to measure the potential gain K (or, expressed in units of the old industry's output, J), not to look at allocations between the two sorts of industries at other times and in other places (although this might be a way of generating alternative allocations to examine for profitability). The measurement of the potential gain is what is involved in the papers I mentioned earlier looking at specific innovations; it is also involved in my crude calculations of the rate of productivity change compared with other countries.

K: You still haven't commented on my evidence of the reserve of low-paid labor, which you argued in your paper was small. But on productivity change: the American rate you report from 1869 to 1909, taken from Kendrick, was 1·5 percent, higher than the British, and this rate is a not unreasonable standard, for nineteenth-century technology was widely available. You use a lower rate.

M: I note in the paper, as does Kendrick, that there are serious deficiences in the American census for the earlier years. He himself gives annual estimates only after 1889. Since I wrote the paper I've calculated productivity change in Germany, using Hoffman's evidence, as 1·13 percent per year from 1880 to 1910. Anyway, the American or German rates, whatever they might be, are overly stringent standards against which to judge British performance, because Britain *started*

with an advanced industrial technology and the others were catching up (I think most people would agree; I've shown it to be the case in the iron and steel industry). The underlying question is one about levels of productivity, not rates of change, although the latter might throw light on the former.

K: Well, what about the evidence of a reserve of low-paid labor?

M: I think you've got another piece of meaningless evidence. With it one could show that any economy with any dispersion in wages could grow indefinitely as demand increased, or nearly indefinitely. I admit that I started this hare in the paper, although I didn't use low relative wages as the criterion of 'underemployment'. It's no excuse, just an explanation, to say that I was momentarily adopting a popular view of economies with which I do not in fact agree for the purpose of arguing on the same grounds as my potential critics. They would say (with, I might say, very little supporting evidence, whether historical or current) that economies undergoing industrialization are 'dual economies', with sectors whose wage is higher than the true social opportunity cost of labor. Were this the case it would mean that markets would not yield maximum income and that income would grow as labor was transferred from agriculture (the usual sector chosen, except in Denmark and New Zealand) to industry. The limitation would then be the demand for industrial products, not their supply, and larger British exports of industrial products, as some have argued, could have increased national income. I take it you accept this view?

K: Yes, approximately, although I do concede that my calculation only counts heads and takes no account of skill requirements.

M: Precisely. If workers were homogeneous then any divergence in wages could indeed be attributable to disequilibrium, that is, to too many workers in, say, domestic service and too few in auto making. But workers are not homogeneous. When you switch a man from being a footman in m'lud's household to being a mechanic in an auto factory national income does not rise magically by the relabeling of the man. Skilled (or for that matter unskilled but disciplined) industrial workers are not free for the asking. That is why some of them earned high incomes. Your argument supposes that they were free. And the lowness of an industry's wages is no indication of its surplus or unprogressive character. Laborers in chemical factories and in electrical substations were no doubt poorly paid by comparison with weavers or coal miners and better paid by comparison with farm laborers and domestic servants. So what?

K: My calculations were only meant to be suggestive. Anyway, I've got some other calculations that nail you on a number of technical issues. Look at these. *(He shows McCloskey pages 419 and 420 of an essay.)* I think I've shown that the calculation of the limits to growth in your one-equation model is sensitive to the data and assumptions employed.

M: (after a long pause) No, I think you've shown that one can get any result if the evidence and the theory are used in an inconsistent way. For one thing you've missed the point of note 14 on page 108. The point was that if the capital stock were to grow much faster than it in fact did (holding other things constant, which was my experiment) it would be growing faster than income, and the capital–output ratio would be continuously rising. Since, in your notation, $\mathring{K}/K = s/k$ the higher k would require a higher and higher s to maintain the frenetic pace of growth. You've assumed that the capital–output ratio would be constant at the observed value 4·0, which is impossible under the hypothetical circumstances you set up. Further, you express surprise in your footnote 9 that I omit foreign capital. But the domestic capital stock and domestic output, which are measured in Feinstein's capital–output ratios (the ones you use and that I would have used, rather than being forced to estimate k from the identity just given), are just what I wanted to talk about. The are surely the correct concepts for assessing the efficiency of the British productive machine. Again, you use my estimate of 10·4 percent as though it were the rate of savings for *domestic* capital formation alone, which it is not (again, note 14 tells all), for it includes investment abroad. The roughly correct figure for the correct concept, savings for domestic investment, is 7 percent. You guessed it: it is given in note 14. You should have seen that something was wrong, for by the identity that you give $(10{\cdot}4)/(4) = 2{\cdot}6$ percent should have been the rate of growth of Feinstein's gross domestic reproducible capital stock at constant replacement cost from 1870 to 1910, which is far too high a figure (it is actually 1·6 percent; notice that the *net* capital stock grows slower, at 1·34 percent; I admit to some uncertainty as to which is the relevant concept).

Suppose that I accept 4 as the relevant capital–output ratio in 1870 and, with greater reluctance, accept your arguments that the rate of growth of the labor force could have been 1·6 percent and the rate of productivity growth 1·5 percent (I've already mentioned why I think the latter figure too high). And, to mend the gap in the argument left by the misinterpretation of the savings figures, suppose that the rate of savings out of income for domestic investment rose in proportion to the rise in all savings that you posit (namely, from 7 percent to

$$\frac{13{\cdot}7}{10{\cdot}4}(7) = 9{\cdot}22 \text{ percent}).$$

Then your calculation of the rate of growth would be in *1870*:

$$0{\cdot}44\left(\frac{9{\cdot}22}{4}\right) + 0{\cdot}52\,(1{\cdot}6) + 1{\cdot}5 = 3{\cdot}34 \text{ percent}.$$

The rate of growth of the capital stock that sustains this rate of growth is $(9 \cdot 22)/(4) = 2 \cdot 3$ percent. This means that output is growing faster than the capital stock, and the capital–output ratio is *falling*, making it easier in terms of the savings effort to sustain this path of growth once it is started.

The reason we get different answers is *not* that my calculations are sensitive to the particular numbers I've chosen but that you are not making the same calculations. The article looks at each growth-inducing factor by itself, whereas you've put them all together. My procedure is relevant for examining arguments that posit simultaneous failure along a wide front. The latter class of arguments is harder to put in jeopardy by the sorts of calculations we are making here. In the limit it amounts to saying at the outset that anything that affects growth can be changed in any convenient way. Any growth rate will be achievable if every piece of behavior affecting the growth rate is freely altered. If, say, hours of work were included in the list of factors, income could be increased 50 percent by increasing hours 50 percent. To test a composite hypothesis (e.g. that there was a massive technological failure *and* low savings *and* income-responsive emigration and population growth) one must shift to examining the reasonableness of the growth-inducing changes contemplated. It is this, not a more disaggregated approach (as you put it), that would be necessary. Good luck.

K: But you yourself disaggregated to the extent of talking about the distribution of savings between home and foreign investment. And I've spotted holes. For one thing, you give a peculiar definition to the elasticity of demand for capital, name r^*/K^* (where asterisks signify proportional rates of change) rather than K^*/r^*, the usual definition, according to Samuelson.

M: It's the usual definition only if you want that definition, and I wanted the other.

K: Well, here's another point. Your analysis is insufficiently general. For example, as foreign investment was brought home, eliminating the differential return between home and foreign investment, the demand curve for goods would surely have shifted outwards. Therefore, whether the 'price' (and hence the marginal revenue) of the composite good would have risen or fallen depends on whether or not the shifts of the entire demand curve would have outweighed movements along the curve.

M: That strikes me as a meaningless assertion. Demand curves have *relative* prices in them, not absolute prices or aggregate price indexes. But I agree with your feeling that it would be good to look into second-order effects. Still, when you've shown that the first-order effects are small there is some warrant for ignoring the second-order effects on the first pass.

K: Be that as it may, I think the central issue here is the gap between

foreign and domestic returns. Only a small portion, no more than a quarter, of total British foreign investments earned a yield as high as that which was earned on the average unit of domestic real capital (around 10 or 11 percent compared with 4 or 5 percent). The majority of British foreign investments were characterized by a pervasive conservatism which forsook considerably higher yields in the pursuit of safer yields. This is evidence of the conservatism of British investors and of the huge loss from not investing at home.

M: If that's your argument I think you've missed the boat. In the first place, that foreign investments were mostly in bonds rather than in equities says nothing about the conservatism of British investors as a group. It says that the conservative lenders invested abroad. Further, as I said in the paper, it is wrong to compare yields on foreign *bonds* with yields on domestic *real capital.* The total return to real capital is composed of a return to bondholders and a return to equityholders. Bonds are less risky, so the return to bonds at home is necessarily less than the 10 or 11 percent you use. In fact, as Cairncross showed (I cite him on this, on p. 109), interest rates for securities abroad were normally higher than securities at home with the same risk. Railroad bonds in India and in Lancashire earned about the same interest.

K: Hmm.

Victorian Britain Did Fail

by N. F. R. CRAFTS

In a recent extremely influential article Prof. McCloskey challenged central tenets of the conventional wisdom concerning the British economy of the late nineteenth century. He argued that significantly higher income growth could not have been obtained by a higher investment rate[1] and that growth was constrained by inelasticities on the supply side.[2] Instead of seeing the period as one of entrepreneurial failure he painted 'a picture of an economy not stagnating but growing as rapidly as permitted by the growth of its resources and the effective exploitation of the available technology'.[3]

These views were later criticized by Dr Kennedy who concluded 'not that British resources were incapable of sustaining more rapid growth, but rather that resources were not deployed to exploit opportunities which did exist'.[4] Kennedy reached his conclusion by maintaining that McCloskey's model was misspecified and thus the actual historical record had been compared with an inappropriate counterfactual situation.

This comment maintains that McCloskey's reinterpretation of the late nineteenth century should be rejected even on the basis of his own model. It is shown using this model that a higher domestic investment