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## **WHAT MAKES THE US PROFIT RATE FALL?**

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## ABSTRACT

Since world war II there have been two quite distinct phases of world growth. In about 1965, a long slowdown set in which has still not ended. Robert Brenner (2002, 2003) has re-ignited the debate about its causes, claiming that nothing in either present or past economic theory explains it. He argues for a 'third explanation', alternative both to the profit-share hypothesis which dominates today, and the rising output-capital ratio account associated with Marx and Kalecki.

Empirically, the evidence overwhelmingly shows the output-capital ratio is a dominant cause of postwar movements in the US profit rate; thus what Brenner maintains is theoretically impossible, is empirically true. The paper dissects this contradiction which, if economics proceeded scientifically, would lead to a radical critique of its own paradigm, but has instead led it to suppress and ignore the only coherent alternative.

The paper shows Brenner's rejection of the Marx-Kalecki framework arises because his theoretical paradigm, adapted uncritically from his critics, cannot allow for the effect of falling prices on capital stocks. His own 'third explanation' is incompatible with this same framework and can be sustained only by understanding it as the mechanism behind, or ultimate cause of, the movement of the output-capital ratio in price terms.

# WHAT MAKES THE US PROFIT RATE FALL?

## Introduction

Historians and economists agree that growth is a decisive indicator of the state of any economy, and most recognise that since world war II there have been two quite distinct phases of world growth. In about 1965, after twenty years of rapid expansion (the 'long boom' or 'golden age'), a long period of slow growth set in which has still not ended. This tectonic economic shift has historic significance. It marks the boundary between the 60s and the 80s. It produced 1968; it provoked the revolutions and coups of the 70s, and then it brought Thatcher and Reagan. Arguably, it led to the rise of the US neoconservatives and the Bush administration. Explaining why it happened is thus one of the principal challenges facing political economy.

The 70s and 80s saw a major debate about its causes, started by critics of the market economy such as Mandel (1974), Armstrong et al (1984), and others. Writers such as Maddison (1982) continue to document the phenomenon, but more or less empirically. Although the period of slow growth is still with us – so much that many have forgotten the world was ever different – discussion around its causes has fizzled out.

Robert Brenner (2002, 2003) has re-ignited this debate, showing that almost nothing in contemporary economic theory explains this long slowdown. If he had gone no further, there would be little to differ about; however he does go farther, claiming that nothing in past economic theory explains it either.

My main aim in this paper is to show that this last claim is misplaced. Earlier theoretical frameworks do exist, most notably the account of the determinants of the profit rate to be found in the work of Karl Marx, that furnish perfectly valid explanations not merely for the growth slowdown of the 1970s but for the three previous such slowdowns – that of 1815-1853, of 1873-1893, and of 1917-1939.

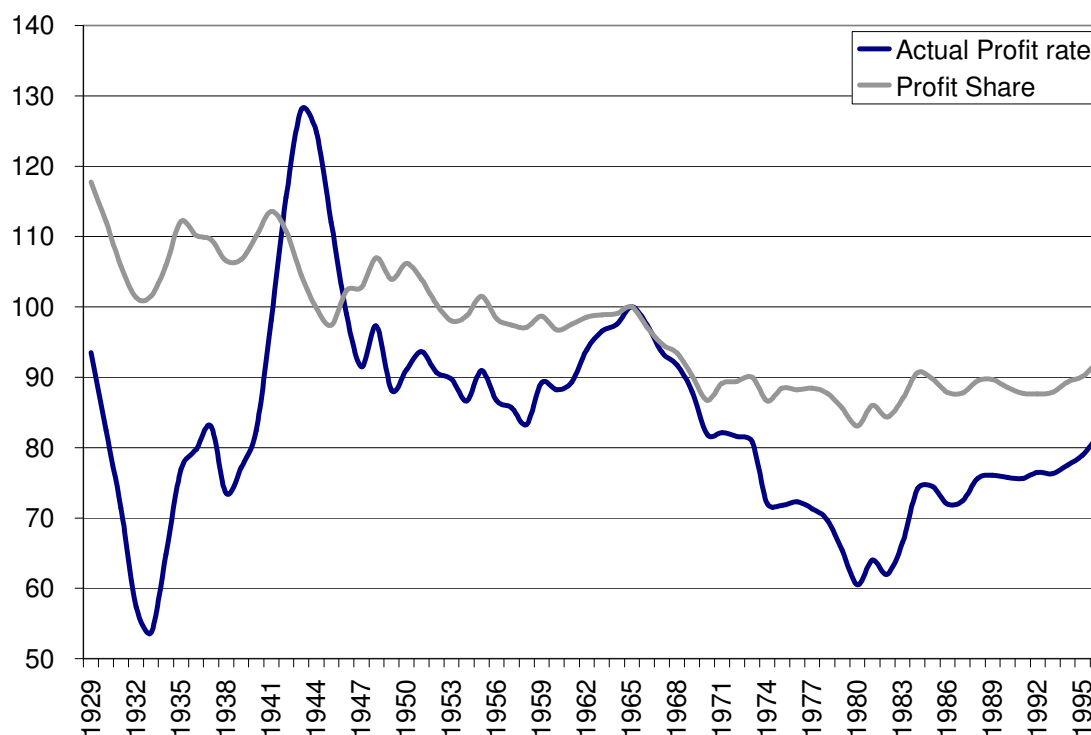
This leads to my second aim, which is to lay bare an intellectual paradox at the heart of Brenner's work. Along with the contemporary economists of whom he is so critical, he rejects an entire body of theory which *empirically* provides a perfectly successful explanation for the facts, on purely *theoretical* grounds – supplied by very contemporary economists whose theories he seeks to replace. Having refuted the economists with the evidence of history, he proceeds to refute history with the evidence of the economists.

This shows he has seriously underestimated their errors, and my third aim is to prove this places his own contribution in mortal danger. Modern economics is more than just false: it is a conspiracy against truth. It does not just fail to explain reality: it is organised to reject those who can. Brenner's explanation is not logically compatible with it. In joining its chorus of rejection, he invites it to suppress his own ideas.

Economics has to be rescued from the economists. To this end any serious critic must recover the economic heritage which the economists have buried. The same discipline that has happily shredded the insights of Keynes, of Marx, and countless lesser-known figures, will ingest the Brenners of this world, chew up their ideas and spit them out, and in twenty years time no-one will have heard of them except as a footnote to 'past errors'. The purpose of this article is to ensure that, in spite of Brenner, that does not happen.

## What explains the profit rate?

Chart 1: US profit rate and US profit share  
Index: 1965=100



For sources see note at end of this article.

All series are indexed to the year 1965=100 so that their variations can be compared.

I begin with the body of theory which Brenner – rightly, in my view – sets out to refute. What he objects to is the idea that both long-run and short-run growth slowdowns originate in falling profits, caused by a rising share of wages in output, and hence a diminishing share of profits in output. He summarises this as follows:

‘Marxists and radicals have joined liberals and conservatives in explaining the long downturn as a ‘supply-side’ crisis, resulting from a squeeze on profits, reflecting pressure on capital from labour that is ‘too strong’.  
(Brenner 2002:12)

This leads him to focus on the profit rate – the ratio between profits and invested capital stock, sometimes called the return on capital (ROC). Because the purpose of this article is to re-assess his theory of this variable, I leave out any wider economic discussion about whether it really is the main determinant of growth. I assume as common ground his view that it is ‘not only the basic indicator but also the central determinant of the system’s health’<sup>1</sup> and, via its impact on investment, the main factor accounting for growth.

I agree with him that the issue is not the level of wages but their share in output, and this is obviously central. A rise in wages will not reduce the profit share unless output rises more slowly.<sup>2</sup> This is the underlying idea behind what he calls ‘Malthusian’ theories

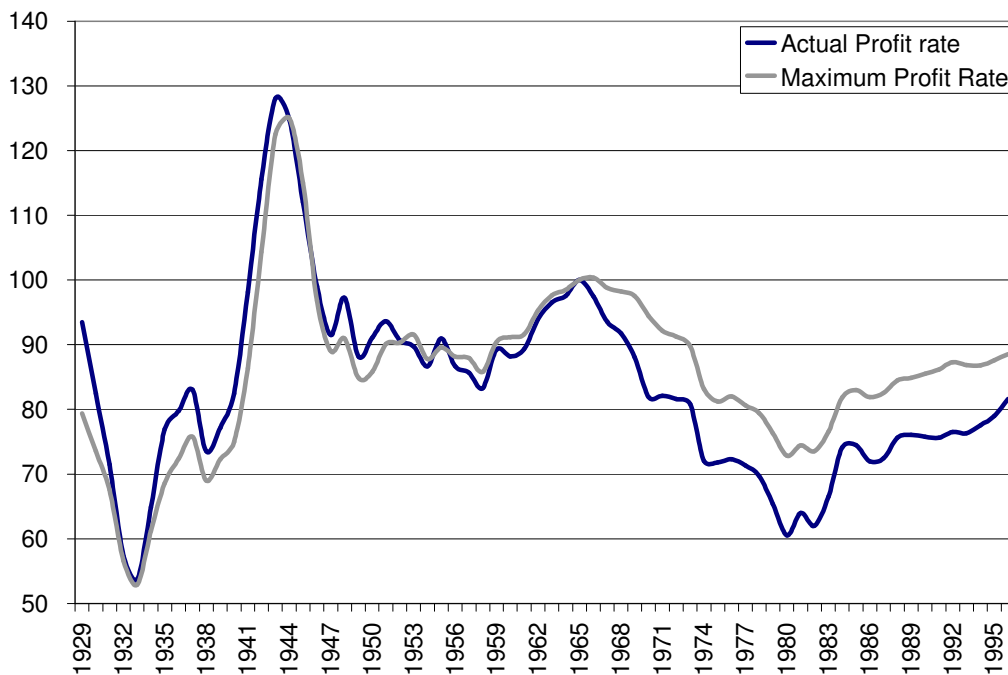
<sup>1</sup> Brenner (2002:6)

<sup>2</sup> as Brenner makes clear (xxxx, xxxx) in replies to critics.

which stress the failure of productivity to rise in line with wages, rather than excessive wage rises *per se*.

Brenner reserves the phrase ‘supply side’ for such theories. The rest of economics uses this phrase with a different meaning, so I will use the more neutral term ‘profit-share’ theory. This covers all theses which hold that growth gets choked off when wages grow faster than output, including those which Brenner calls ‘Malthusian’ and also the original profit-squeeze hypothesis which enjoyed prominence in the 1970s, for example as an explanation for the crisis of that time in the UK.<sup>3</sup>

**Chart 2: US actual and maximum profit rate (‘output-capital’ ratio) index: 1965=100**



**to be re-entitled ‘maximum profit rate (‘output-capital’ ratio) in graph itself**

In common with other writers,<sup>4</sup> I will deal with the profit rate in the whole (US) economy and not just manufacturing, to which Brenner attaches more emphasis. Since 1965 US capital has migrated into services, and manufacturing worldwide has migrated to the global South. Capital may thus be making less profits in US manufacturing because it can make more profits somewhere else. The falling profit rate in manufacturing in the advanced countries does reflect the global trend in profits as a whole, but may in part express a redistribution of profit between branches of production; I therefore focus on the most general indicator of profitability – the profit rate in the whole of the USA, the most powerful economy in the world.

It may be thought that this is the source of my differences with Brenner. This is not so. I do not take issue with Brenner for his focus on manufacturing, and my case is just as valid for the profit rate there as for the whole economy. However, my argument will be clearest, and most general, if I can set aside all extraneous factors which might arise not from the movement of the economy as a whole, but one part of it.

<sup>3</sup> Cf Glynn and Sutcliffe (1972)

<sup>4</sup> cf Dumenil and Levy(1993)

I now return to the profit-share theory. This is trivially easy to demolish by misstating it, for example to interpret it as meaning that nothing else affects the profit rate. When, for example, we say that gravity causes objects to fall, we do not mean they never do anything else or that no other circumstance can make them move downwards. We need to refine, and test, the idea that the movements in the profit share are the ‘main cause’ of movements in the profit rate.

Any alternative approach should of course be tested in the same way, and not by reducing it to an unacceptably simplified form. For example the idea that the ratio of output to capital dominates profit rate movements, abusively labelled ‘fundamentalist’ by Fine – a designation perpetuated by Brenner – cannot be reduced to the idea that the profit rate falls for ever,<sup>5</sup> or that nothing else has any effect on the profit rate.

I will therefore try to formulate the idea that the profit share is a cause that *dominates* over all other causes. As Brenner notes, all sources of variation in the profit rate can be partitioned into two. The profit rate is defined as

$$\frac{\Pi}{K} \tag{1}$$

where  $Y$  means annual output,  $K$  means accumulated capital stock, and  $\Pi$  means annual profits. Dividing top and bottom by  $Y$  gives

$$\frac{\Pi}{Y} \times \frac{Y}{K} \tag{2}$$

The first term is the profit share. The second is, basically, the largest possible profit rate. If wages were zero, profits would be equal to  $Y/K$ . This gives us

$$\text{Rate of profit} = \text{Profit share} \times \text{Maximum profit rate}$$

Brenner adapts a different terminology from the economists, conceding to them a confusion. In this terminology,

$$\text{Rate of Profit} = \text{Profit share} \times \text{Output-capital Ratio}$$

This calculation effectively divides all possible causes of the rate of profit into two great camps. As it stands it is, of course, only an algebraic formula. It is only a way of thinking about the profit rate and has no necessary causal implications. Nevertheless, discussions in the last century have invested it with an almost scriptural significance, as rival camps apply mathematical arts to demonstrate the primacy of one or other term in it.

One of the main things I want to establish is that this debate long ago parted company from the most basic principle of science, which is that facts come before theory. As it stands, the formula is just that – a formula. Writing it in symbols does not give it magical qualities. Before making any theoretical deductions from it, we first have to study the actual behaviour of the terms in it, to assess the evidence that the possible causal relations it implies do, or do not hold, in reality.

Before doing that, one last clarification. Brenner’s preference for the term ‘output-capital ratio’ lays him open to a confusion, which is not the central problem but nevertheless bars the way to solving it. He consents to a well-known but misleading error in his discussion with his critics in allowing them to identify  $Y/K$  with the productivity of capital.<sup>6</sup>

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<sup>5</sup> This caricature is advanced for example by Laibman (xxxx)

<sup>6</sup> Thus Brenner seems to assume that any explanation rooted in a falling output-capital ratio must suppose that the productivity of capital is falling. This hobbles his response to trivially false criticism. Against Zacharias (Brenner 2002:p36) he writes “My argument emphatically does not, *contra* Zacharias, entail a rising real Output–Capital ratio (rising capital productivity).” But he has nothing to fear. A rising output-

Actually, productivity, as it figures in the work of the economists whose work Brenner discusses, has no necessary relation to the output-capital ratio. By the term ‘capital productivity’ these economists all mean the ratio between output and *consumed* capital (raw materials plus depreciation). Not only is the term hardly ever applied to invested capital, but it is highly contestable that it can be so applied, as is testified by a long, arduous, and famous debate in economics.<sup>7</sup>

It is perfectly possible for the productivity of consumed capital to decline whilst the ratio of output to invested capital systematically rises. This happens when, for example, entrepreneurs invest in machinery that saves on raw materials, or if the machinery itself wears out more and more slowly. In the ultimate case of a machine that never wears out and consumes no raw materials (eg a perfect computer) the physical productivity of consumed capital would be infinite because nothing is consumed, but the output-capital ratio could fall without limit.

This point is emphatically registered by Marx as has been noted by, for example Fine and by Saad-Filho. It does not, it should be noted, vindicate him, which is why I don’t think these authors have actually settled any important dispute. It does not dispose of the main argument against Marx’s analysis – that although the physical amount of invested capital systematically rises, its price falls due to technical progress.

Nevertheless, the confusion casts such a pall over the main discussion that it has to be cleared up before proceeding to the main issue.

## **The facts of the US profit rate**

To proceed to the main issue: given equation (2), clearly, three outcomes are empirically possible. First, we may find that the profit share accounts for nearly all the variation in the profit rate, and that any residual movement is insignificant, relatively small, or has little or no additional explanatory value after the effect of the profit share has been removed. In that case we can say that the profit share is a dominant cause of movements in the profit rate.

We may find alternatively that the output-capital ratio accounts for nearly all the variation, and that any residual is insignificant, relatively small, or has little or no additional explanatory value after this effect has been removed. In that case we can say that the output-capital ratio is a dominant cause of movements in the profit rate.

There is a third possibility: we could find that neither variable on its own can be discounted, that each plays some necessary role, and neither is a dominant cause. In that case, as discussed later on, the analytical separation of the profit rate into two mutually exclusive causes is not particularly useful, and we should look for some third explanation.

Charts 1 and 2 allow us to verify which of these three alternatives actually holds for the case of the US profit rate. Chart 1 shows what would have happened if the only thing affecting the profit rate was the profit share – that is, if the output-capital ratio (maximum

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capital ratio is simply not the same as rising capital productivity. Zacharias’ case can be dismissed by recognising only this, but Brenner does not do, apparently for fear that the mere mention of a rising output-capital ratio will lead to the Sin of Malthus.

<sup>7</sup> namely, the Capital Controversy (see Harcourt xxx). The difficulty with fixed capital is the principal reason that Piero Sraffa (xxxx) introduced the concept of joint production, which he and the physicalist school needed in order to *reduce fixed capital to circulating capital*, by supposing that a machine each year jointly produces its product, and a vintage of itself. See Pasinetti (xxxx), Dumenil and Levy (tba), Freeman (2005).



profit rate) were held constant. Chart 2 shows what would have happened if the profit share had no effect at all – that is, if the profit share is held constant.

Note that because of formula (2) each of these charts is also the residual of the other. Thus, chart 1 shows the variation which remains, after that due to the output-capital ratio (maximum profit rate) has been removed. Chart 2, less obviously, shows the variation which remains after that due to the profit share has been removed. This is because the grey line in that chart, which shows the maximum profit rate, would be the same for any constant profit share.

If the profit share was a dominant or even significant cause of the boom and bust, the grey line in chart 1 should exhibit the same general features as the actual profit rate: a sharp rise, followed by a long decline. It exhibits neither. The recovery in the profit rate from 1933 to 1943 has clearly nothing to do with the profit share, which over this period even fell slightly. As for the decline, between the high point of 1943 and the low point of 1980 the profit rate fell by 60 percentage points while the profit share fell by only 20 per cent, accounting for at most one third of this movement. If we take the start point most favourable to the profit share hypothesis, between 1965 and 1980 – the period which occupies Brenner's attention the most – the profit rate fell by 40 percentage points and the profit share by half this much.

Almost all of the recovery, and between a half and two-thirds of the main thing Brenner wants us to explain – the postwar decline in the profit rate – is unaccounted for by the profit share. For those who consider regression analysis a useful tool, a regression of the profit rate against the profit share between 1929 and 1965 yields an  $R^2$  of 0.008, that is, 99.2 per cent of the variation in the profit rate is unexplained by the profit share. Over the whole of 1929 to 1996  $R^2$  is 0.193, that is, 80.7 per cent of the variation in the profit rate is unexplained by the profit share.

Turning now to the residual variation: if the profit-share were a dominant cause of the observed long-term changes in the profit rate, then when we eliminate this cause, what remains should be negligible or random – that is, the grey line in chart 2 would either be flat, or its movement would be unrelated to the movement of the profit rate.

It is not flat, and it clearly is related to the profit rate. On both grounds, the profit share cannot be considered dominant. It does not explain the observed variation, and when it is removed, the residual is not negligible.

This is in complete contrast with the output-capital ratio, which *on its own* accounts for 91.9 per cent of the variation in the profit rate between 1929 and 1965, and 75.7 per cent of the variation between 1929 and 1996. With the sole exception of the five years of decline from 1965 to 1970, it accounts for almost the whole variation in the profit rate since 1929. The key, broad historical movements in the profit rate are present whether or not the profit share varies: in particular an extremely strong upward swing in the profit rate from 1933 to 1943, followed by a prolonged decline until 1981 interrupted between 1958 and 1965,<sup>8</sup> followed by a shallow recovery bringing profit rates only back up to their 1958 level.

What of the residual after the effect of the output-capital ratio is discounted? Symmetrically, this is the grey line in chart 1. Essentially this shows random fluctuations about a trend declining at a rate of 30% over sixty years, that is 0.5 per cent per year. This

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<sup>8</sup> Brenner concentrates on the decline from 1965. But as can be seen, aside from minor fluctuations the profit rate declined continuously from 1943 with the exception of the 7-year rise of 1958-1965. Any verdict on the decline must be set in this context: the problem is not merely to explain what happened after this limited seven-year recovery, but to ask what happened over the entire Kondratieff up- and downswing from 1943.

trend has no explanatory value whatsoever in relation to the key historical phenomenon, which is a rise *followed by* a long fall.

Thus the facts offer very strong confirmation for an alternative hypothesis, namely Marx's much-maligned argument that the long-term rise in the organic composition of capital – to which the output-capital bears a simple and direct relation – is the most significant cause of the long-term fall in the profit rate. *The empirically dominant cause of all long term movements in the US profit rate between 1929 and 2000, that is, the whole period for which records have been kept, is the ratio between output and capital stock.*

## **The elephant in the dining room: why does the output-capital ratio fall?**

We now turn to the second main point of our article. According to the economists upon whose theoretical framework Brenner rests his work, what we have just empirically shown is theoretically impossible. The reason is a justly-famous theorem due to Nobuo Okishio (1961) which shows that if the capitalists innovate in such a way as to reduce their input costs (henceforth 'cost-saving technical innovation'), the profit rate must rise provided that the real wage is constant. Brenner endorses this conclusion extremely early in his article, without first examining the empirical evidence, in a long footnote dedicated to rejecting the account provided by Marx, the principal architect of the view that the profit rate is dominated by movements in capital stock (Brenner 2002:12ff; emphasis in original except where indicated)

[I]f, as Marx himself seemed to take for granted, the capitalists are assumed, in response to competition, to adapt technical changes that raise their own rate of profit by reducing their *total cost* (labour plus capital, or direct and indirect labour) per commodity, it seems intuitively obvious that the ultimate result of their innovation, when it is generally adopted in their line, can only be to reduce the exchange value of the goods produced in their line and thus, directly or indirectly, to reduce the exchange value of the wage, and thus to *raise* the average rate of profit, given again the (Marxian) assumption that the real wage remains constant. *It certainly cannot be to reduce the rate of profit.*<sup>9</sup> Formal proofs of this result can be found in Okishio [1961] as well as in Roemer [1978a, 1978b]

There is only one problem. What Okishio says cannot happen, does happen. The statement that 'the ultimate result of their innovation...certainly cannot be to reduce the rate of profit' is, unfortunately, demonstrably false. As with many of the most central scientific advances of the age, the 'intuitively obvious' happens to factually untrue.

Indeed since the theorem must apply for the special case of a zero real wage, it applies to the maximum profit rate – that is, the output-capital ratio. Hence, if Okishio's theorem is right, the output-capital ratio cannot be a cause at all – let alone a dominant cause. Indeed the output-capital ratio *cannot possibly fall*.<sup>10</sup>

*Eppur si muove.* Actually, the output-capital ratio does indeed fall, for considerable periods of history, as chart 2 shows. Moreover when it falls, the actual profit rate moves with it, as chart 2 also shows. And even if it is not accepted that the output-capital ratio is

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<sup>9</sup> The emphasis here is mine – AF

<sup>10</sup> unless, of course, the capitalists behave irrationally, by innovating in such a way as to raise costs. However, there is absolutely no empirical evidence for this, nor does Brenner suggest it.

the dominant determinant of the profit rate, it remains the case that the Okishio theorem predicts a historical tendency – an inevitable rise in the output-capital ratio – which manifestly does not occur.

A truly radical critique of contemporary theory would have cut a path straight through this central contradiction, concluding the necessity of discarding contemporary theory along with its erroneous reconstruction of Marx's law, stock and theorem. Confronted with the fact that theory rules out Marx's account as something which 'certainly cannot reduce the profit rate' in a situation where, empirically, it does indeed reduce the profit rate, ought to lead directly to the conclusion drawn by researchers in the Temporal Single System School (TSSI),<sup>11</sup> that this theory must have misunderstood Marx's account and that we should carefully re-read and reconstruct what Marx actually said, since the most obvious explanation is that we put to the test some different theory, other than the theory which made the correct prediction.

This is the only possible basis for a scientific alternative that can lead out of the blind alley in which contemporary economics finds itself. The central intellectual contradiction of Brenner's work is that he does not follow the scientific path (and in so doing, significantly discredits the historian's craft). Instead he sets out down the third road of trying to establish a 'new' dominant cause alternative to both the profit-share and the output-capital ratio.

My alternative approach to the long downturn takes as its point of departure the results of the foregoing critique...my point of departure is thus simultaneously that capitalism tends to develop the productive forces to an unprecedented degree, and that it tends to do so in a destructive, because *unplanned and competitive*, manner. (Brenner 2002:23, emphasis in original)

In taking as point of departure a *theoretical critique*, rather than empirical reality, Brenner unfortunately follows contemporary economics down the path it has followed since 1905 when Bortkiewicz first reinterpreted Marx as a general equilibrium theorist. The mechanism behind the debate itself is the following: economists discount *a priori* what we actually see in reality. They deny that it is empirically possible for the output-capital ratio to operate as a cause, on purely theoretical grounds. Brenner not only accepts this: sadly, he gives in to it. He does not even study the actual empirical evolution of the output-capital ratio with the minor exception of a couple of paragraphs halfway through his article, which I assess later.

Instead, he sets out in search of alternative explanations which circumvent the central issue, namely the observed behaviour of the output-capital ratio. By this route he does arrive at a genuinely innovative account of which the rational core is the idea that the profit rate falls as a result of the declining prices of capital goods. But this merely returns him to the central paradox; actually, his alternative explanation can only be sustained by rejecting the entire theoretical framework within which Okishio's theorem is situated, which framework is the 'point of departure' of his analysis.

The reason is that the output-capital ratio does indeed fall – *in price terms*. But, as we shall also see, the fundamental theoretical method employed by Brenner's critics – and accepted by Brenner – denies that the output-capital ratio in price terms can differ from the output-capital ratio in physical terms. The entire *folie à deux* of the debate which has followed Brenner's contribution arises from this basic confusion.

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<sup>11</sup> Cf Freeman and Carchedi (1995) and Freeman, Kliman and Wells (2004); also [www.iwgv.org](http://www.iwgv.org)

## Time and money: why the price rate of profit is lower than the physical rate, when prices are falling

The preceding argument brings us to the main point, which is elementary but vital: the rate of profit is an ratio of price, not physical, magnitudes. Therefore, if in the price ratio  $Y/K$ ,  $K$  rises in price terms more rapidly than  $Y$ , even though in physical terms it rises more slowly, then the maximum profit rate in price terms can fall, and under the appropriate circumstances will fall, even when in physical terms does not.

Why should  $K$  rise more rapidly than  $Y$ ? And why should falling prices affect the outcome? At first sight this is difficult to understand how this can make a difference, even to a person prepared to abandon the unfounded theoretical prejudice that the price rate must be the same as the physical rate: to be sure, the price of invested capital goods changes at a rate which is different from the price of currently-produced output and so the profit rate in price terms is different from the physical rate. But surely, if anything the price of capital goods ( $K$ ) is falling faster than that of currently produced goods ( $Y$ )?

This omits a decisive point, which Brenner himself understands, and which is the core of his argument but which, blinded either by the Okishio theorem, or by the pious esteem in which it is held by the economists, he fails to incorporate into his general theoretical framework. *If the price of invested goods falls, the capitalists lose money. This reduces output.*

Therefore any fall  $\Delta K$  in price of  $K$  constitutes not merely a reduction in the denominator of  $Y/K$  but is also a deduction from the numerator. The money profit rate is not simply

$$p_Y Y / p_K K$$

where  $p_Y$  is the price of output, and  $p_K$  the price of capital goods. It is

$$\frac{p_Y Y - K \Delta p_K}{p_K K}$$

where  $\Delta K$  is the loss of money experienced by the capitalists as a result of the decline in the value of their invested or, as Brenner terms it ‘sunk’ capital.<sup>12</sup>

The the discussion among the economists into which Brenner has entered assumes that the profit rate in price terms cannot possibly be different from the profit rate in terms of physical magnitudes. This is not a theoretical accident. It is an outcome of the very same approach to the profit rate which informs the Okishio theorem and which informs the Marxist tradition since Paul Sweezy, which in turn accepts the very same fundamental premise of pre-monetarist neoclassical theory that Keynes so systematically demolished in the *General Theory*. This approach, general equilibrium, theoretically entails that price cannot matter because it assumes away *a priori* the impact of any movement in prices. The equilibrium approach dictates that we should suppose all prices, profits and indeed all economic magnitudes, are given by the levels which they *would attain, if all economic movement ceased* – in this case, if prices stopped falling.

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<sup>12</sup> It may be supposed that the above predicts that  $K$  will fall, rather than rise. However there are two sources of change in  $K$ . One is the price mechanism just described and the other is accumulation.  $K$  falls in price terms by  $K \Delta p_K$  but rises through investment by an amount  $K p_K$  determined by the capitalists propensity to invest. The final outcome is the resultant of these two effects:

$$\frac{p_Y Y - K \Delta p_K}{p_K K + p_K \Delta K - K \Delta p_K}$$

If investment outstrips the fall in prices, as is the case except during an exceptionally deep recession (as actually took place from 1929-1937) then  $K$  in price terms rises and the output-capital ratio falls.

If prices are fluctuating randomly, this does not matter. But if – as is the case – they are falling in a secular way as a result of technical change, equilibrium theory predicts a systematically higher profit rate than is actually observed. Indeed, it predicts – as Okishio indeed proves – that the maximum profit rate will rise without limit, under circumstances when the price profit rate is perfectly capable of falling, and does indeed fall.

To be precise, it predicts that regardless of money and prices, the maximum profit rate will simply be equal to the ratio of the physical magnitudes of  $Y$  and  $K$  and the actual profit rate will be the ratio of the physical magnitudes of  $\Pi$  and  $K$ .

The core insight in Brenner's analysis of the impact of competition is that the falling prices of 'sunk' capital – which he attributes to increased competition, but for which technical innovation is surely the most general underlying cause – impacts on the profit rate. However, this insight can be preserved *only* by the most thorough-going theoretical break with the equilibrium method, here concentrated in demanding of economics the recognition that the temporal factor  $p_k \Delta K$  is decisive in determining the actual, observed, money rate of profit.

## **Is moral depreciation an illusion?**

It may be argued that the the accountants' practice is an arbitrary whim and that different depreciation rule would eliminate the output reduction brought on by technical change. In fact this is not so as the following considerations show. What must a capitalist do, in order to run a business? She advances a sum of money and buys things – machinery, buildings, equipment, raw materials, and so on. These are purchased at the price of the time, not the price of the future. What money sums must the capitalist recover from sales before a profit can be made? The money advanced in order to purchase the inputs. The denominator in the profit rate is the capital originally advanced, which is a millstone that does not get ground down until and unless someone, somewhere, actually pays for it.

Thus whereas the physical quantities involved are untouched by such changes in price, money profits are not. It is therefore entirely wrong to identify physical surplus with money profit. Once prices start changing, the two are simply not the same.

Once production has taken place and time has elapsed, productivity advances will make it possible to purchase new inputs in the future at lower prices. But the capitalist didn't buy the inputs in the future. She bought them in the past. And she must recover what she paid, not some fictitious sum that she would have paid, had she been able to take a train to the future, buy the cheaper inputs, and then return to the present together with the inputs, and feed them into the assembly line which she presumably also bought in the future.

It is even clearer if one supposes the money was borrowed although this does not change the substance of the matter. Suppose a banker lends the capitalist £100,000 to buy computers which, by the time the loan is due to be repaid, could have been purchased for £50,000. What banker would concede that, since the goods purchased with the money are now worth only £50,000, half the loan can be forgotten? The bank lent the capitalist *money*, not computers: and that's what the banker wants back.

The capitalists can, it is true, wrongly estimate the impact of depreciation, and this frequently happens, especially when the company has an interest in doing so – as did Enron, for example. Indeed, one function of a crash is to reveal the 'true' values of businesses buried under mountains of suspect paper. But the very fact that such crashes occur, and above all the fact that when they do occur, creditors suddenly become uninterested in fraudulent depreciation and begin a very energetical search to ascertain the

exact true value of the company's invested capital, is the clearest proof that this is a real phenomenon of capitalism and not at all an accountant's fiction.

Nevertheless, it is true that the full effect of moral depreciation is rarely known immediately and therefore, for some period of time – at least the duration of the business cycle and perhaps even longer – official figures for output and capital stock<sup>13</sup> are out of step with reality. This is one of the main reasons that it is difficult to study the true causes of movements in the profit rate over short periods and that the picture is much clearer over reasonable historical periods of time.

Companies can and do write down their capital in different ways and they do get it wrong. The key point is they do not escape the impact of price falls on the mass of their profit in this way; they merely cause this impact to be expressed in a different way. A classic very long run example is the British ship-building industry,<sup>14</sup> which refused for a century to write down the value of its landed property – a critical element of infrastructural investment in shipbuilding which requires deep harbours – on the grounds that land does not depreciate. Their Korean competitors invested in artificial harbours – lowering the world value of what was hitherto a monopoly input to ship production.

During its decline, British shipbuilding declared record profits and consistently figured among the profitable British industries. The true state of affairs revealed itself in its systematic loss of market share and finally became inescapable – as is often the case – at the point of bankruptcy. Only finally in this historical reckoning was it finally discovered that land has no intrinsic value at all, that the assets of the shipbuilders had become worthless, and that the companies had been decapitalised without anyone admitting it.

Thus, the need to deduct the depreciation of capital from profits is not some statistical or accounting artefact but a defining and irreducible feature of capitalist production. Brenner's perspicacity as a historian shows up in the fact that he recognises this, noting that the impact of competition on existing investment is to impose forced losses on the capitalists that have 'sunk' capital into their investments in outmoded technology. What he must do, if this insight is to stand the test of time, is rise to the task of integrating it into a more general theoretical understanding. This is the problem that has to be overcome.

## **Summary: what happens when investments lose their value?**

What, actually, is profit? The accountants are very clear about this and history, we have seen, is on the side of the accountants. Profit is the difference between the money value of a business at the beginning of a given period and its money value at the end, including of course any money that the owner has withdrawn from the business.

If the investments of the business rise or fall in value for any reason at all, the profit of the business is correspondingly increased or diminished. If for example I am in the business

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<sup>13</sup> Which is, incidentally, depreciated on a different basis from that undertaken by the capitalists and different yet again from that imposed by the tax authorities. Capitalist accountancy factors in moral depreciation, and writes down goods over periods generally much less than their physical lifetime. The national accountants make a correction for this with the perpetual inventory method for capital stock, which attempts more accurately to include equipment that is still functioning but has been written down completely in company accounts. Paradoxically they do not apply the same correction the capitalists' estimate of output. This means that the output measure in the national accounts is out of phase with the capital stock measure, in that it includes the impact of depreciation earlier. The result is probably that the profit rate is to some extent understated during periods of rapid technical change.

<sup>14</sup> Johnman, L. and Hugh Murphy (2002)

of making computer equipment, and if – like CISCO during the crash of 2000-01 – I have laid out \$3bn of semiconductors to make them with, and if the price of semiconductors falls by 50 per cent, then I must write down these losses in my accounts before I declare my profits. This is a general law: a fall in the value of invested capital is deducted before profits are declared. Any other claim is literally fraudulent – people go to jail for doing it.

But what *causes* the price of semiconductors to fall? Of course there may be any number of immediate causes – the construction of a new plant, overcapacity, a failure of the tacit oligopoly which is well-known to regulate semiconductor prices, a sudden collapse in demand due to reduced consumer purchases of computers. It is a mistake, however, to identify these immediate causes with what underlies them, namely, inexorable technical progress in refining and endlessly improving the process of semiconductor production.

In short, *the cause of the decline in the profit rate is technical progress*. This is the awkward, ‘intuitively difficult’, ideologically unacceptable conclusion against which economic theory has in its total effect been organised more or less since the time of Marx. It is the idea which Marx himself notes is the most difficult idea for scientific economic theory to accept. It is the idea from which all but the most morally incorruptible (or foolhardy) economic theorists withdraw, once the scale, extent, and socially profundity of the opposition to it is made clear to them by one or other of the many devices at the disposal of polite intellectual society to secure recantation or compliance – the closing of all doors to careers, systematic refusal to publish on a succession of more or less spurious grounds, the rejection and ridicule of peers, the endless repetition of insulting and abusive designations (‘fundamentalist’), the strange forgetfulness to which all sophisticated economists fall prey when it comes to the citation or even the study of any ideas other than their own – all these are of course present no threat to life, liberty or even happiness, and so they will not be found in any catalogue of political or human abuse. But their purpose is much more subtle and, ultimately, devastating to human survival: they function as a gigantic ideological enterprise whose sole final result is to ensure that people do not understand that the greatest threat to the capitalism is – capitalism itself.

Three observations are pertinent:

- (1) Technical progress itself reduces the monetary magnitude of profits through its effects on previous investments, as Brenner recognises.
- (2) This ‘moral depreciation’, as Marx termed it, is not caused by physical deterioration or ‘material depreciation’ and should be distinguished from it. It is entirely a result of lower prices which are in turn a result of technical progress.
- (3) The greater the technical progress, the greater the reduction in profits.

A series of further conclusions follow.

First, whilst the impact of competition as such – particularly international competition – does not ultimately produce the fall in the output-capital ratio and with it the profit rate, at certain historical points it clearly played a decisive immediate role. Notwithstanding the points at the start of this section, I think Brenner is right to give this attention as a particular historical cause in the 1960s.

Difficulties arise, however, if we make competition the primary cause. As historians we run up against a problem: the *same* phenomenon took place in at least two previous periods of history, namely 1873-1893 and 1922-1937. But arguably during 1922 and certainly during 1873-1893, the principal feature of the market régime was monopoly.

How, then, can competition explain these previous cycles of decline? Of course, the ‘general’ fact that technical progress provokes the decline in the profit rate is insufficient to explain why at particular points in history, the profit rate should rise more or less

rapidly and at others, it should decline for a long time. A more general historical theory would apply Marx's understanding of the impact of technical progress on growth, I think, more or less in the same way that physics deals with gravity. It would have to study the particular circumstances which result in this factor expressing itself openly at particular times. The problem with Brenner's elevation of competition into a general cause is that, *ceteris paribus*, one would like a theory that explains each and every Kondratieff downswing, not just one of them.

Second, a simple empirical calculation explains why chart 2 has the form which it does, and shows why, in general, the temporal effect of falling investment prices is likely to dominate over profit share effects. Empirically, capital stock is rarely less than eight times bigger than output. But it follows that even a one per cent reduction in the value of investments, will result in an eight per cent loss of output. Indeed, the greater the accumulated stock, the greater the temporal effect.

This is precisely why, when the accumulation of stock has risen as high as it had by the sixties, it became more or less inevitable that technical innovation, above all when concentrated in the goods that have accumulated as productive resources, will eat into the profits arising from enhanced productivity to such a degree as to offset them more or less completely. The reduction in profit rate due to this phenomenon is absolutely sufficient to cancel out or indeed reverse the positive impact on the profit rate of the original fall in the price of invested capital stock, and this is clearly not a negligible or discountable effect as equilibrium theory would have us believe, but one of the most fundamental mechanisms of the economy.

Third, does the above imply that the rate of profit must fall for ever? This extreme form of Marx's conception of the tendency of the rate of profit to fall has played a disproportionate role in Marxist discussions and has to some degree contributed to the scepticism with which it has been received particularly in US circles.<sup>15</sup> This tendency within Marxism dates back to a very specific discussion, which took place among Russian Marxists in the 1920s.<sup>16</sup> A catastrophist view emerged according to which the collapse of capitalist was imminent and driven by automatic economic mechanisms, a view which Marx himself certainly never held and which cannot be found in his writings. This catastrophist view vanished from the mainstream but survived in an admittedly millenarian form into the second half of the century, where it found the strongest theoretical currency in those places where the practical prospects for political change were weakest, namely, the Anglo-Saxon countries.

Actually, the profit rate can be and was restored by a simple but destructive mechanism, namely *disaccumulation in value terms*. That this is what really happened is clear from the timing of the 'bounceback' after the great depression, which took place over the period 1937-1943. This occurs when the economy either contracts, or runs at a sufficiently low rate that the capital stock depreciates faster than it is replenished. This may happen through a more or less catastrophic collapse of personal consumption, and accounts for the 1933-37 partial reconstitution of the US profit rate. A second form of the same mechanism is the creation of extra non-investment demand which raises economic activity back to levels approaching full capacity, but maintains private (profit-appropriating) investment at low levels so that disaccumulation proceeds without the socially-disruptive consequences of very low levels of economic activity.

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<sup>15</sup> cf Shaikh **tba**, probably the most apocalyptic of a number of attempts to demonstrate that the profit rate never stops falling.

<sup>16</sup> See Day (1981)



This is the rational kernel in the somewhat unjustly maligned account of Luxemburg, which notes the key role played by war – obviously one of the most unproductive possible use of social resources. To the extent that war expenditure simply uses up output or, being state expenditure, does not place the relevant investment within the sphere of private capital, it plays the same role as pure slump.

In summary the profit rate can be restored – but only through the kind of profound crisis seen in thirties, when recession is so deep and so long that accumulation itself halts and the value of accumulated capital stock is permitted to fall, contrary to all the natural tendencies of capitalism, can lay the basis for a recovery. Even this is not enough, since the same crisis also drastically reduces output and further drastic measures are required – eg war, fascism, etc – to bring about the sharp upswing seen between 1933 and 1944.

### **Is a ‘third explanation’ possible?**

We hold the thread of an unravelling cloth – the fabric of the post-Marxist, equilibrium-centred reconstruction of classical economics. This single fact, the impact of the depreciation of capital on profits, is the economic equivalent of the Lorentz contraction in relativistic physics. The distinction between a scientific and a theological approach lies in whether it is treated as an irritating anomaly to be worried about at another time, or recognised for what it is: the fundamental flaw in the modern economic conception of price and profit.

The task facing anyone that really wants to understand why Okishio’s theorem is empirically false is to follow this thread quite relentlessly, setting aside all previous ‘received knowledge’, abandoning in the process the entire physicalist, equilibrium framework for understanding what profits really consist of, and all preconceived notions that follow from it.

A correct scientific approach would have been to look at whether these contesting causes were plausible *before*, not *after*, introducing any theoretical preconceptions which rule out these causes – especially, and above all, since theory has systematically failed and must therefore be subjected to the most radical possible critique. If Brenner had done this, he would have been led to a different and very straightforward enquiry: how is it that the rate of profit in price terms can differ from the rate of profit in physical terms?

But he does not. Having correctly, in his introduction, noted that the profit rate can be decomposed into profit share and capital-output ratio, he undertakes no study of the actual movement of these two magnitudes.

Indeed his empirical discussion of the actual movement of the output-capital ratio is remarkably weak. It does not seem to appear until p101 of his article and deals only with the period 1965-1973, which as we have seen is the only period between the whole of 1929-1996 during which there is a significant residual after the impact of the output-capital ratio has been taken into account. Even then, as we have seen, the output-capital ratio accounts for more than half the variation, a fact that Brenner discounts by introducing a new definition of the profit rate used nowhere else, in terms of constant prices: ‘If the output-capital ratio is to express capital productivity it must be expressed in terms of *real* output compared to real capital input, in terms, that is, of constant output and capital stock prices.’ (Brenner 2002:101).

But first, if this is the correct way to measure profits, why not do it throughout? Why use it only when dismissing an uncomfortable fact? Second, as we have already explained, the output-capital ratio is a *money* magnitude and it is in this form that it enters the formula for the rate of profit. Indeed, the very fact that he can calculate a physical

measure of it, and find that this physical measure differs from the price form, only emphasises that the price form itself cannot be explained by a purely physical analysis.

Brenner does indeed wish to integrate the impact of price movements into his analysis, and this is clear from his discussion of the impact of prices changes on capitalists who have already invested massively in fixed capital.

If this is to survive as an explanation, as noted it must be shown that price movements can impact on the profit rate. But as we have seen, in endorsing the approach of Okishio he has endorsed a theoretical framework *within which no change in prices can have any impact on the profit rate whatsoever*. Therefore, within this framework, his own explanation cannot hold. This is the fundamental contradiction in his article.

Brenner believes that he can escape these paradoxes by constructing a ‘new’ explanation for the decline in the profit rate. Let us therefore ask what it might mean, and under what circumstances a third cause could be considered dominant, given that we have partitioned the movement of the profit rate entirely into two rival contenders for the place of dominant cause.

As we have seen, the argument as to whether the profit share and output-capital ratio is a dominant cause arises from the formula for the profit rate, which is the product of these two magnitudes. This is a however a purely algebraic separation; there is no guarantee that it will produce meaningful results. Under what circumstances would it be meaningful or necessary, therefore, to produce a third, alternative explanation? Only, I argue, if *neither* the profit share nor the output-capital ratio emerged empirically as dominant cause. We would then have to conclude that though theoretically interesting, the division of causes into two great and apparently conflicting camps which is expressed in equation (2), is analytically insufficient to furnish useful explanations and constitutes a nothing more than a mathematical curiosity. In this third case, and only in this third case, the way is open to consider different analyses which operate in a mixed way, modifying both the profit share and the output-capital ratio in some interlinked manner.

It would then make sense to investigate a great variety of alternative causes – competitive behaviour, market régime, entrepreneurial psychology, microeconomic behaviour, and so on, and determine whether, in their own right, they can explain the variation in the profit rate.

If however one or other of the terms in equation (2) explains the great bulk of the variation in the profit rate, then any such third cause has to be linked to the relevant term, because if it is a valid cause, then it and the relevant term would have to be collinear, that is they would have to vary together. In this case, although these many interesting mechanisms may still be invoked to explain the variation in the profit rate, such ‘ultimate causes’ must operate through their effects on one or other of these two variables. The problem is not, then, to invoke these complex mechanisms as an alternative to movements in either profit share or the output-capital ratio but to show how they themselves cause the movements in the profit share or the output-capital ratio.

Thus the ‘profit-share’ hypothesis in its most general form amounts to saying that all other causes must operate by modifying the profit share. When we put things in this way it is easy to see the alternative: if the output-capital ratio is a dominant cause, then all other causes must operate by modifying the output-capital ratio or, alternatively, must be the outcome of some deeper process which also causes the output-capital ratio to fall.

As a demonstration of how this point applies in practice, consider the argument that the remedy for falling profits is to cut wages. As noted, the output-capital ratio is in fact the same as the maximum profit rate. Of course there is no *lower* bound to the profit rate. By

increasing wages far enough, the profit rate under any conditions can be reduced to zero. There is, however, an *upper* bound. The profit rate cannot be raised above the output-capital ratio by decreasing wages.  $Y/K$  is thus just a technical ratio: it is the maximum profit rate.<sup>17</sup>

Now, it can be seen from chart 2 that the decline from 1965 to 1980, taken as a whole, would have happened *whatever* had happened to the profit share: since the maximum rate was falling, *no* reduction in wages, no matter how far, could possibly have restored the profit rate. The best that could have been achieved would have been to stave off the decline till 1970.

Therefore Brenner's 'third explanation' cannot be an alternative to a fall in the output-capital ratio, and this is the difficulty. Since the output-capital ratio clearly is falling during the period of the decline, and moreover this fall explains virtually all the decline in the profit rate, there isn't 'room' for an additional explanation. The explanation must in fact serve as the mechanism of the movement in the output-capital ratio, or as the outcome of some other mechanism which also causes the output-capital ratio to fall. If it really were a completely different cause, then when we eliminate the effect of the movement in the output-capital ratio – as we did in chart 1 – the variation due to the 'third cause' would be present in some way.

## The Scientific and the Religious method in Economics

We now turn, finally, to the current state of economic theory. *Empirically*, the profit share theory is unacceptable, at least as an explanation for the movement of the profit rate. The evidence that the output-capital ratio is a dominant cause of the variation in the profit rate is overwhelming.

If Okishio's theorem prohibits this conclusion so much the worse for Okishio's theorem, not to mention innumerable contributors to the *Review of Radical Political Economy* who uncritically and monotonously cite it as 'evidence' that Marx – the father of all output-capital theories – must be wrong.

When humans fail to accept what they see, no amount of illumination will improve matters. The searchlight must swerve: through a hundred and eighty degrees, from the external facts on which its operatives have trained it, to the internal world of these same operatives. The enquiry is no longer limited to what is observed. It must deal with the mind of the observer.

The first theoretician to pose the question of the profit rate in terms of the ratio of total output to total capital invested was Ricardo. Ricardo's explanation, which Brenner rightly in this case dubs 'Malthusian', was that output itself must be declining because of the declining marginal output of land.

The whole of post-Ricardian theory revolves around a simple issue: is this the only way that output can decline? In this discussion, the most central issue of all is the interpretation of Ricardo's most famous successor, Karl Marx. And the most central issue in this assessment is, in turn, was Marx merely a Ricardian? Does Marx's reformulation of Ricardo's theory of the profit rate merely restate it in rigorous terms, or does it involve

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<sup>17</sup>  $Y/K$  may also be thought of as a multiple of the *constant profit share* profit rate. Chart 1 is what we get if we set any fixed value of the profit share and index the result to 1965=100. This could be any value. I happened to construct it by looking at  $r_{max} = Y/K$  where  $Y$  is output and  $K$  is capital stock; but if the profit share were equal to its average over the period, namely 33.6 per cent, this curve would be  $r_{constant} = 0.336Y/K$ , and when indexed to 1965=100 would look exactly the same..

a theoretical innovation which would lead us to a *different* explanation for the decline in the profit rate from Ricardo's?

As we have seen, there is a different explanation. It is that the fall in value of invested capital, arising in turn from the declining prices of invested capital goods, constitutes a deduction from output so that the value or price rate of profit diverges systematically from the physical rate.

The question is, did Marx understand this? Or to be more precise, is Marx's theory open to an interpretation which integrates this insight? The answer has been provided by the work of the TSSI school, which has shown that indeed, the most transparent, obvious, simple and evidentially satisfactory interpretation of Marx is that his thinking, on economic theory, in fact diverges radically from that of most modern 'Marxists'; that his account of the falling rate of profit is rooted in the recognition that temporal factors such as price (and value) movements over time play a decisive role in determining both the rate of profit, and the movement of the economy.

At the very least, even for those who reject this interpretation, it must be conceded that this is a perfectly reasonable and valid way to understand Marx's value theory. Thus, it must be conceded that explanations and theories are indeed accessible in the writings of the past, which provide a completely reasonable framework to understand the actual empirical movement of the rate of profit. Why, therefore, has economics not built on these completely reasonable, and empirically valid, theories? It is at this point we have to begin to grasp that the reaction of economics is not accidental, but constitutes an ideological response.

Marx's theory is held to be logically false by all modern economists, not least the Marxists, who in fact lead the charge. As noted, there is a flat contradiction between Okishio's theorem and reality. It predicts a phenomenon – a rising output-capital ratio – which simply does not occur. Why then, does economics not simply scrap the theoretical framework of the Okishio theorem and start over? Because it has immunised itself against the obvious conclusion that an empirically false theorem is wrong. This is most clear in a remarkable passage from Roemer (1979:380), one of the two principal economists, as we have seen, on whom Brenner relies for his discounting of Marx's empirically correct account:

Responses to this claim, of Okishio and others, have been of three types. These are, first, what Fine and Harris (1976) call fundamentalist positions on FRP. Second, there are empirical discussions of whether or not the organic composition of capital is indeed rising. While this sort of investigation may be useful, it does not bear upon the *theoretical* issue of whether or not the rate of profit falls due to technical change. That is, either such investigation will be consistent with the Okishio conclusion, or it will not be; in the latter case, it would show the need for a different microeconomic argument of capitalist technical innovation; it would not, however, show Okishio's *argument* to be wrong. The empirical investigations, then, are certainly necessary, but they cannot provide refutation of a theory.

The idea that there exists a theory which cannot be refuted by empirical investigation is categorically false. No logical sleight of hand can rescue anyone who believes otherwise. Two centuries of historical experience have confirmed that Okishio's theorem is factually false. My view is dogmatically straightforward: if theory does not match reality, the theory must be wrong, whether or not this theory contains within itself any clues as to how or why it is wrong. There are no exceptions. A discrepancy between observation and theory *in every case* informs that we should adjust our theory. Any other view is not

science but theology. There is no position more ‘fundamentalist’ than to assert that evidence cannot refute a theory.

Roemer’s error goes to the heart of the question ‘why has economics failed?’ Actually, Okishio’s theorem is logically and mathematically impeccable. As a straightforward exercise in thought it is one of the great achievements of the second half of the century. But it is for this very reason that the underlying mistake in it must be much too profound to be solved by a ‘different microeconomic argument’ – by a theory that only rejigs models or mechanisms but does not seek out the underlying conceptual flaw which renders all such models incapable of predicting what actually in fact occurs.

Consider, for example, how science finally reacted to the late 19<sup>th</sup> Century discovery that the speed of light in space is independent of the observer, which led to the theory of relativity. At the time, theory could not even explain why it had no explanation. It could do so only when Einstein showed that the error did not consist in a wrong ‘argument’, in a wrong set of equations linking ‘speed’, ‘space’ and ‘observer’, but in the actual meaning which thinkers had assigned to the words themselves.

Pre-Einsteinian theorists, quite obviously, could not guess what Einstein was going to say. They were left simply with a contradiction between their theory and reality. However if they had declared that *nevertheless the theory must be true*, they would have ceased to operate as scientists.<sup>18</sup> The fact that we do not know what is wrong with our theory does not excuse us from accepting that it is wrong. We cannot anticipate what *future* theoretical innovations will explain why we were wrong; we can, however, recognise that they will have to. This is not altered by our present inability to explain our failure. What outrageous vanity dares legislate against the future of thought?

In the case of the profit rate, matters are far worse. Marx’s theory is not a future innovation. It is an existing account. If the economists cannot even adjust their thinking to understand why an *existing* theory explains reality better than their own – which is in essence what Roemer is saying – then they will make no further useful contribution to the advance of human knowledge no matter how profound their theoretical insights.

This in turn explains why the recovery of Marx’s ‘own’ theory is indispensable – along with, of course, the ‘true’ theory in every account under consideration including, not least, Keynes’s original theory which has much in it, if prised loose from the hands of the Keynesians, that leads to an adequate understanding of the crisis of profitability.

The motivation is not some obscure attachment to past doctrine but a realisation that present theory is so deeply incapable of understanding reality that we have to conclude it has organised itself to exclude precisely those insights that it needs to break out of its self-constructed prison. It is precisely because Okishio’s theorem refutes Marx, but yet Marx is empirically correct, and because despite this fact economics has constructed a wall of censorship which excludes all empirically correct answers, that we cannot proceed until and unless we can find the insights that permitted the originators of those answers to reach their empirically valid analyses.

The scientific procedure is this: if we cannot reconcile our thinking to reality, we must amend our thinking. The greater the contradiction, the more profound is the effort required. In fact we must do for economics what Einstein did for physics: Just as he reconstructed space and time, we have to reconstruct profit, price, and capital. We have to

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<sup>18</sup> As some German theorists of the time actually did until the thirst for nuclear power asserted its domination over mere metaphysics: see for example the unlamented *Hundert Autoren Gegen Einstein* (Israel 1931),.

recognise that what is wrong is our very understanding of what these words mean. It requires in short the critical method.

If our existing theory does not permit this reconstruction, so much the worse for theory. It shows only that our limited comprehension is so poor that we must root around in all our preconceptions until we have finally unlocked the secret prejudice which prevented us from understanding not only the world we live in, but why we could not understand it.

The true fundamentalism is thus not that of the maligned capital logic school which, albeit somewhat bullheadedly, refused to abandon Marx's explanation when economics proved it theoretically 'wrong'. The ultimate dogmatism is that of those who, like Roemer – who is only one among many – refuse to ask how Marx got it empirically right. The only way to advance is to reject every ontological preconception which prevents us from seeing how the rise in capital stock really does outstrip the rise in output despite the unshakeable prejudice that this cannot occur. Everyone has three choices: abandon prejudice, abandon science, or abandon hope.

## Data sources

Data for capital stock are taken from the Bureau of Economic Affairs publication *Fixed tangible Wealth 1929-1996*. Data for GDP, profits and wages are taken from the Bureau of Economic Affairs website which, at the time of writing, are given by the publication *GDP 2004 revision with components*.

Capital stock is defined as the total of private fixed tangible assets (omitting government and private assets, which do not enter the equalisation of the profit rate), valued at current prices. GDP is the annual seasonally adjusted output also at current prices and wages are the compensation of employees, including employers' contributions, at current prices. Profits, which are pre-tax, are defined as GDP less wages.

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