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Production Functions, the Kaldor-Verdoorn Law and Methodology

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1 Introduction

I first met John McCombie in December 1983. I was then visiting my father who was the general consul of Canada in Melbourne. I had written to Robert Dixon about a paper of his in *the Journal of Post Keynesian Economics*, and had told him that I would be in Melbourne around Christmas. Dixon invited me to meet him and to have lunch at the University of Melbourne. John McCombie then had a position there, and so he joined us at lunch, and so did Geoffrey Harcourt who happened to be in Melbourne at the time. The only thing I remember is a brief after-lunch discussion with John McCombie about whether the Kaldor-Verdoorn law could also be subjected to the critique of the neoclassical production function that Anwar Shaikh (1974) had

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made, on the basis of its analogy with the national accounts. McCombie at the time had just published a few papers on the Kaldor-Verdoorn law, and I was using Shaikh's paper in my class on growth theory, so I was curious to know his feeling about this. I do not recollect what his reply was, but McCombie does not seem to recall this conversation either because he has written that he discovered the article of Shaikh by pure luck, 'almost by serendipity' (Felipe and McCombie 2013, p. 11).

I have met John McCombie many times since, in particular when he invited me to participate in a conference on open-economy macroeconomics which was held in 2002 at Emmanuel College, at the University of Cambridge, where I presented a stock-flow consistent model of the Eurozone (Lavoie 2003). One of our recent encounters was at the 2013 Berlin post-Keynesian summer school, where McCombie was asked to do a lecture on open-economy macroeconomics and the balance-of-payment constraint, and to present the main features of his new book on the critique of the neoclassical aggregate production function (Felipe and McCombie 2013). There Eckhard Hein and I had the pleasure of interviewing him for the *European Journal of Economics and Economic Policies* (Hein and Lavoie 2015). And even more recently, John was kind enough to do a long review of my latest book (McCombie 2015).

McCombie is mostly known for his articles on the balance-of-payment constraint, his critiques of the neoclassical aggregate production function and his work on the Kaldor-Verdoorn law. He has also done some incursions into methodology. In what follows, I will skip the first topic and start with a discussion of the neoclassical aggregate production function. The next section will be devoted to the question that I had put to McCombie in December 1983, that is, whether the critique of the neoclassical production function has any relevance for a possible critique of the Kaldor-Verdoorn law. In the final section I will deal with some methodological issues, mostly associated with the difficulties of making changes in macroeconomic theory.

2 Neoclassical Aggregate Production Functions

2.1 Anwar Shaikh's HUMBUG Data

McCombie (1987) published his first paper critiquing the empirics of the neoclassical production function in 1987, going over the critiques made by Shaikh and Herbert Simon. It is also in 1987 that I published my first book, in French, on the capital controversies and growth theory (Lavoie 1987). Early on in my career, I was fascinated by Anwar Shaikh's (1974, 1980) HUMBUG critiques of the neoclassical production function. These appeared in my chapter on the neoclassical response to the Cambridge capital controversies.¹ One of these responses, besides the recourse to general equilibrium theory or to temporary equilibrium, or the refusal, plain and simple, to acknowledge the existence and impact of the capital controversies, was the recourse to empirics. Several neoclassical authors at the time had insisted that the numerous empirical successes of neoclassical production functions such as the Cobb-Douglas production function or the constant-elasticity-of-substitution (CES) production function had demonstrated that the Cambridge capital controversies, with their associated rejection of the principle of relative scarcity due to the possible presence of reswitching and capital reversing, were of no practical significance.

There is the well-known *cri-du-coeur* of Charles Ferguson, who wrote that the validity of neoclassical theory was depending on econometric or empirical arguments rather than theoretical ones and hence that he had faith (Carter 2011). But others made the same pledge. Here I mention just a few additional examples, from authors that were well-known at the time of the Cambridge capital controversies. Sato (1974, p. 383) thought that neoclassical theory could be vindicated by having recourse to empirical analysis, in particular the estimation of CES functions. Jorgenson (1974) also argued that the CES production function seemed to be most appropriate when its constant elasticity was close to unity, that is, when

it was no different or little different from the Cobb-Douglas production function. Similarly Bronfenbrenner (1971, p. 474) argued that the theory of distribution based on marginal productivity was vindicated by the numerous empirical successes in all fields of the Cobb-Douglas production function.

Thus, Shaikh's demonstration, that the empirical successes and good statistical fits of the aggregate Cobb-Douglas production function were due to the fact that the algebraic representation of such a production function was no different from a mathematical transformation of the national accounts identity, should have given a halt to this instrumental defence of neoclassical theory. But of course, retrospectively, we know that it did not. In Lavoie (1987, p. 116), I also recalled that Herbert Simon (1979), in his acceptance speech for the Bank of Sweden prize in economic sciences in memory of Alfred Nobel, had himself underlined this tight link between neoclassical production functions and the accounting identities, demonstrating this in particular for the case of the CES function. I had also noted the curious fact that Simon (1979) had not cited Shaikh (1974) who had written on precisely this topic, despite the fact that Robert Solow had been identified as one of the readers of his draft (Lavoie 1987, p. 196). Obviously, Solow knew about Shaikh's (1974) paper since he had written a rather nasty (and misleading) comment on it, as Shaikh (1980) later demonstrated, and to which he was not allowed to reply.²

As recalled by Felipe and McCombie (2011–12, p. 276), in response to a letter that I had sent to Simon, pointing to the similarities between his and Shaikh's argument, Simon replied that, being less connected with economics, he had to rely on friends and colleagues to keep track of the literature, thus implying that Solow had omitted to inform him about Shaikh's work. It is probable that Solow did not want to give Simon further arguments against the marginal productivity theory of factor pricing. In a conversation with me, Shaikh observed that Solow was still angry about his HUMBUG article since, more than 30 years after its publication, Solow steadfastly refused to shake hands with him during a ceremony in honour of Modigliani which was held at the New School in 2007.

Shaikh (1990) published a third version of the HUMBUG production function which contains an interesting graph. It illustrates the case of an economy based on a Leontief production function with fixed technical coefficients, which is subjected to technical progress of the Harrod-neutral sort, with a constant capital to output ratio. Shaikh (op. cit) shows the straight upward-sloping line that links capital per head on the horizontal axis and output per head on the vertical axis. Neoclassical theory will assume instead the presence of a standard neoclassical production function with diminishing returns, and hence a relationship between capital per head and output per head which has the usual curvature. Neoclassical authors would thus interpret the data of this economy by claiming that there has been a move along the non-linear neoclassical production function accompanied by a shift of the entire production function. Thus, even if the technology is of the Leontief type, neoclassical economists running standard regressions would pretend that they have demonstrated the existence of a well-behaved neoclassical production function. But this was precisely the point that Nicholas Kaldor (1957) had made several years before, when claiming that “any sharp or clear-cut distinction between the movement along a ‘production function’ with a given state of knowledge, and a shift in the ‘production function’ caused by a change in the state of knowledge is arbitrary and artificial” (p. 596).

2.2 Reductio Ad Absurdum Proofs

Shaikh (2005) produced a ‘reductio ad absurdum’ proof of this. He generated data of a fictitious economy subjected to a Goodwin cycle, where technology, as above, is of the Leontief type with Harrod-neutral technical progress and with mark-up pricing. Still, despite all this, once technical progress is assessed in an appropriate way, Shaikh shows that the data can appear to have a high fit with a Cobb-Douglas production function, as he obtains a regression with a very high R^2 and an estimated output elasticity of capital which is very close to the actual profit share, just as neoclassical theory would have it. This is so despite the fact that, by construction, the data has nothing to do with neoclassical theory and violates all of its usual assumptions.

This kind of ‘*reductio ad absurdum*’ proof is the best proof that can be offered to demonstrate that the apparent empirical successes of the neo-classical production functions have nothing to do with reality conforming to neoclassical theory.³ The clearest and most astute such proof was offered by McCombie (2001) himself. He generates microeconomic data, where the output elasticity of labour is 0.25 while the output elasticity of capital is 0.75, so that the sum of the two elasticities is equal to unity, thus assuming constant returns to scale. Inputs and outputs are assumed to be homogeneous, so as to avoid any problem of aggregation. Running a regression over this hypothetical economy, with some random fluctuations, and with physical output being a function of labour and the physical value of machines, the estimates of the output elasticities turn out to be equal to those that were assumed by construction. However, things are quite different when regressions are conducted on the basis of deflated monetary values, that is, constant-price values. McCombie assumes that firms set prices on the basis of a mark-up procedure, with the wage share being 75 per cent while the profit share is only 25 per cent. Running a regression over the same hypothetical economy, but using the deflated values, he finds that the apparent estimate of the output elasticity of labour is now 0.75, instead of the 0.25 true output elasticity of labour that was assumed by construction in the data.

McCombie (2001) thus provides an undeniable proof that regressions over deflated values, the only ones that economists can run in the case of macroeconomic studies or even at the industrial level, will necessarily provide an estimate of the wage share in the economy instead of an estimate of the output elasticity of labour. Thus, as I conclude in Lavoie (2014) “even if the technology is from Mars and Martians manage to produce output independently of inputs, provided Martian firms follow some form of cost-plus pricing, the regressions over deflated values will tell us that the Martians use Cobb-Douglas production technology with diminishing returns, constant returns to scale, and factor pricing following the principles of marginalism” (p. 60).

As recalled by Lavoie (2008, 2014, pp. 60–62) and Felipe and McCombie (2013, pp. 302–306), my former co-author Wynne Godley also engaged into this kind of ‘*reductio ad absurdum*’ proof in a paper that was unjustly neglected (Anyadike-Danes and Godley 1989). They constructed a

hypothetical economy where nominal wages, employment and output are growing independently of each other, with some random fluctuations and with prices once again being set by some cost-plus procedure. Still, a regression equation similar to those ran by the likes of Layard et al. (1991) produced a statistical and significant relationship between real wages and employment, while employment did not seem to depend either on current output or lagged employment, whereas by construction there was no relationship between real wages and employment while current employment was heavily dependent on previous employment.

As noted in an appendix by Felipe and McCombie (2013, pp. 308–310), I also myself provided a kind of ‘*reductio ad absurdum*’ proof when criticizing the wage-setting/price-setting model of Layard, Nickell and Jackman, or what became to be known as the WS-PS model of the NAIRU (Lavoie 2000, 2008). The 2000 paper was written as a reaction to an article by Cotis et al. (1998) which claimed to explain the evolution of the NAIRU from an econometric estimation of this WS-PS model and that claimed that the model was not contradicted by the data. The authors marvelled at the fact that their empirical estimate of the PS curve was fully compatible with the first-order conditions of a well-behaved neo-classical production function, with diminishing marginal product of labour, perfect competition and factor pricing at the value of the marginal product. Their regression had uncovered a positive relationship between the log of real wages and the rate of unemployment. The first author, Jean-Philippe Cotis, had been the chief economist at the OECD and had just then been named head of the French statistical agency—the INSEE—so this was not the work of some innocent bystander. Once again, it was shown that both their so-called medium-run and long-run equilibrium unemployment rates could be derived from the income side of the national income and product accounts and not from some behavioural equation tied to the neoclassical theory of labour demand.⁴

2.3 General Consequences

All these proofs demonstrate that there is no empirical support for neo-classical production and distribution theory. Orthodox authors decorate

their theories, they do not actually attempt to verify them, let alone falsify them. They calibrate their production models; they do not actually demonstrate that their behavioural equations based on neoclassical theory are the proper description of the way the world works. As Kaldor (1972) said a long time ago, “the role of empirical estimation is to ‘illustrate’, or to ‘decorate’ the theory, not to provide support to the basic hypothesis (as for example, in the case of numerous studies purporting to estimate the coefficients of production functions)” (p. 1239).

It is sometimes objected, because critics have paid so much attention to the Cobb-Douglas production function, that only this production function is subjected to the threat of reproducing the identities of the national accounts. But as mentioned earlier, the CES production function was already under attack from Simon. Furthermore, another contribution of McCombie is to have proven that indeed the CES production function and the translog production function were subjected to the very same criticisms (McCombie and Dixon 1991; Felipe and McCombie 2001). This is an important contribution because Kaldor’s stylized fact of a constant wage share has been undermined over the last three decades, thus generating better fits with the CES production functions than with the Cobb-Douglas function, and thus leading several economists to adopt the CES function (while also rejecting Leontief production functions). It is thus important to underline the fact that CES functions seem to perform better now because the wage share has been decreasing over the last 30 years or so, not because they are a better representation of the real production process.

The studies of Shaikh, McCombie, Felipe and others show that the econometric estimates of neoclassical production functions based on deflated monetary values, where direct physical data cannot be used, yield pure *artefacts*, that is, purely imaginary results. This affects all of neoclassical applied aggregate work that relies in some way on well-behaved production functions and profit-maximizing conditions: NAIRU measures, labour demand functions and wage elasticities (Felipe and McCombie 2009); investment theory; measures of multifactor productivity or total factor productivity growth (Felipe and McCombie 2007); estimates of endogenous growth; theories of economic development; theories of

income distribution; estimates of cost functions; measures of potential output; estimates of the impact of changes in the minimum wage, social programmes or in tax rates.

Even when setting aside problems of aggregation, neoclassical economists are claiming to measure something, whereas they are really measuring something entirely different. One may wonder, however, whether the critique could also affect elements of post-Keynesian theory.

3 The Kaldor-Verdoorn Law

3.1 A First Look at the Similarities with the National Accounts Identity

McCombie has been an early advocate of the Kaldor-Verdoorn law, and he has written an extended survey of the studies that have been devoted to it (McCombie 2002). In the introduction of this chapter, I mentioned that when meeting John McCombie in 1983, I asked him if the Kaldor-Verdoorn law could be subjected to the same problems as the neoclassical production function. The discussion did not produce any conclusion, but in my 1992 book, I made the effort to at least reconsider the issue (Lavoie 1992, pp. 322–324).

It has been known for a long time that Kaldor's (1957) technical progress function, which can be considered as a theoretical version of the empirical Kaldor-Verdoorn law, could be rewritten under the form of a dynamic Cobb-Douglas production function, and hence that it could be rewritten under the form of the national accounts identity. The technical progress function, in its linear form, has been formalized as:

$$\lambda = \lambda_0 + \lambda_k \hat{k} \quad (12.1)$$

where λ is the rate of technical progress (the growth rate of output per unit of labour), while \hat{k} is the growth rate of the capital to labour ratio.

A Cobb-Douglas production function of the sort $q = e^{\mu t} K^\alpha L^{1-\alpha}$ can be rewritten as output per unit of labour, hence as $y = e^{\mu t} k^\alpha$, with y the output

per labour, so that in growth terms, we have a relationship which appears to be no different from the preceding one:

$$\hat{y} = \mu + \alpha \hat{k} \quad (12.2)$$

As to the national accounts, it can be shown that their income side can be rewritten in a dynamic form which resembles the above, as we get:

$$g = (1 - \pi)\hat{\omega} + \pi \hat{r} + \pi \hat{K} + (1 - \pi)\hat{L} \quad (12.3)$$

where g is the growth rate of output, \hat{K} is the growth rate of the capital stock, \hat{L} is the growth rate of the labour force, π is the profit share, $\hat{\omega}$ is the growth rate of real wages and \hat{r} is the growth rate of the rate of profit.

By recalling that \hat{k} is the growth rate of the capital to labour ratio, Eq. (12.3) can be rewritten as:

$$g - \hat{L} = \hat{y} = \tau + \pi \hat{k} \quad (12.3A)$$

with $\tau = (1 - \pi)\hat{\omega} + \pi \hat{r}$

From an elementary point of view, these three Eqs. (12.1), (12.2) and (12.3A) look quite alike. Both Kaldor's technical progress function and the Cobb-Douglas production function could be brought back to the national accounts identity. What about the Kaldor-Verdoorn law? Does it suffer from the same fate? Could it also be a statistical artefact? Although the Kaldor-Verdoorn law says that the rate of technical progress, that is, the growth rate of labour productivity, is a function of the growth rate of output in the manufacturing industry, it is often written as a function of the growth rate of GDP. With g standing once again for the growth rate of overall economic activity, the Kaldor-Verdoorn relationship may be written as:

$$\lambda = \lambda_0 + \lambda_g g \quad (12.4)$$

Combining the Kaldor-Verdoorn relation (Eq. 12.4) with Kaldor's technical progress function (Eq. 12.1), we obtain what Michl (1985) calls the augmented technical progress function:

$$\lambda = \lambda_0 + \lambda_g g + \lambda_k \hat{k} \quad (12.5)$$

Writing the extended form of the national accounts dynamic identity, that is, combining Eq. (12.3A) with the value of its τ component, we obtain something that does look highly similar to Eq. (12.5):

$$\hat{y} = \pi \hat{r} + (1 - \pi) \hat{\omega} + \pi \hat{k} \quad (12.6)$$

When running his regression on the augmented technical progress function, Michl (1985) finds an estimate of the λ_k coefficient, which is very close to the share of profits in manufacturing, that is, around 0.38 and 0.40, thus corresponding to the π value in the national accounts identity. Furthermore, since \hat{r} at the time was close to zero, with no trend in the rate of profit, one would expect the λ_0 parameter to be not significantly different from zero, which is also what Michl (op. cit) obtains. So far, the estimates are in line with the national accounts identity. However, the λ_g coefficient in Eq. (12.5) provides us with a piece of information which is not present in the national accounts identity given by relation (Eq. 12.6). It says that faster rates of growth of output (g) are associated with faster rates of growth of real wages ($\hat{\omega}$). The national accounts do not yield such a prediction. The Kaldor-Verdoorn law says that there is a relationship between the growth rate of output and the growth rate of output per labour and hence by extension that there might be a relationship between the growth rate of real wages and the growth rate of *output*. The national accounts by contrast tell us that there is a relationship between the growth rate of real wages and the growth rate of *output per labour*, not the growth rate of output.

Note, however, that the latter relationship may put in jeopardy another behavioural equation often found in post-Keynesian economics, that is,

the dynamic Webb effect, also called the Marx or the Hicks effect. This causal relationship going from the growth in real wages to the growth in labour productivity is emphasized, for instance, by Hein and Tarassow (2010) and Storm and Naastepad (2012) in their discussion of productivity regimes. However, when calculating their productivity regimes, they suppose that the rate of technical change is influenced by both the Kaldor-Verdoorn effect and the Webb effect. Thus these authors include simultaneously the g and $\hat{\omega}$ variables in their estimates of their effect on the growth rate of labour productivity, so that they have an equation that does not correspond to the national accounts identity. This equation, in the same notations, is given by:

$$\lambda = \lambda_0 + \lambda_g g + \lambda_w \hat{\omega} \quad (12.7)$$

3.2 Another Reductio Ad Absurdum Proof

McCombie himself has recently tackled the possible relationship between the Kaldor-Verdoorn law and the national accounts identity. This is done in the paper of McCombie and Spreafico (2016). The authors start by noting what we just said above, that is, the technical progress function in its linear form can be brought back to a Cobb-Douglas production function and hence to the dynamic version of the national accounts. Hence the technical progress function “also suffers from the criticisms that Kaldor made of the neoclassical production function” (McCombie and Spreafico 2016, p. 1118). But what about the Kaldor-Verdoorn law? McCombie and Spreafico (2016) show that “Verdoorn’s law could be regarded as a specification of the linear technical progress function allowing for the possibility of increasing returns to scale” (p. 1134). Does it mean that the Kaldor-Verdoorn effects are just as spurious as those of the neoclassical production function?

To convince us that they are not, McCombie and Spreafico once more resort to the use of the constructed data of a hypothetical economy. This is a highly useful method, for we know the true data that underlies the estimates that are being calculated. They test the Kaldor-Verdoorn relation

given by Eq. (12.4) on 15 hypothetical regions, over ten years, thus testing the following equation, with the subscript i representing each region:

$$\lambda_i = \lambda_{0i} + \lambda_g g_i \quad (12.8)$$

For expositional ease, as will be shown below, they assume “Kaldor’s stylized fact that the growth rate of the capital stock equals the growth rate of output (i.e., the capital-output ratio is constant). As a consequence of also assuming that factor shares are constant, this implies that the growth in the rate of profit is zero” (McCombie and Spreafico 2016, pp. 1127–1128). This means that in Eq. (12.3) of the national accounts, they assume for simplification that $g = \hat{K}$ (so that $\hat{k} = 0$) and $\hat{r} = 0$, so that by construction they have:

$$g(1 - \pi) = (1 - \pi)\hat{\omega} + (1 - \pi)\hat{L} \quad (12.9)$$

Hence, the national accounts under the above restrictions become:

$$\hat{y} = g - \hat{L} = \hat{\omega} + 0.g \quad (12.10)$$

In this simplified case, on the basis of the national accounts, it is obvious that we ought to find no relationship whatsoever between the growth rate of output g and the growth rate of labour productivity \hat{y} .

What happens when regressions are run? McCombie and Spreafico construct a series of variables that give rise to g , \hat{y} and $\hat{\omega}$ for their 15 hypothetical regions over ten years. In the first experiment, they assume by construction the existence of a Kaldor-Verdoorn effect, that is, they assume that there is a positive relationship between the growth rate of output and the growth rate of labour productivity. Running a regression based on Eq. (12.8), and assuming that the λ_{0i} parameter is allowed to vary for each region, the regression captures the special national accounting identity of Eq. (12.10), as the estimate of λ_{0i} is captured by the growth rate of real wages $\hat{\omega}$, while the estimate of λ_g is indeed statistically no

different from zero. By contrast, when the λ_{0i} parameter is assumed to be the same for all regions, that is, when it is assumed that the exogenous constant of technical progress is the same for all regions, as is usually done in this kind of study, the Kaldor-Verdoorn effect is captured. Indeed the estimate of λ_g is statistically different from zero and around 0.45, which is close to the value with which the data was generated by construction.

McCombie and Spreafico (2016) conduct a second experiment, constructing the data in a similar manner, “with the exception that for any given productivity growth rates of a particular region, the output growth rates were random” (p. 1130). In other words, the Kaldor-Verdoorn effect is absent by construction. Once again, assuming that the λ_{0i} parameter is allowed to vary for each region, the regression has a near perfect fit (a R^2 close to unity) as it captures the national accounting identity of Eq. (12.10), with the estimate of λ_{0i} being captured by the growth rate of real wages $\hat{\omega}$, while the estimate of λ_g is not significant. By contrast, when the λ_{0i} parameter is assumed to be the same for all regions, the regression has a very poor fit, with the R^2 being close to zero, and the estimate of λ_g is not statistically different from zero, as it should be since the Kaldor-Verdoorn effect had been excluded by construction.

3.3 General Considerations

From these experiments, as well as from the arguments offered when comparing Eqs. (12.6) and (12.7), I believe it is safe to conclude that the Kaldor-Verdoorn effects are not an artefact. They do not arise from a specification that reproduces the national accounts identity. This is an important conclusion because the Kaldor-Verdoorn effects provide a possible explanation of the super-hysteresis effects that were empirically measured by León-Ledesma and Thirwall (2002) and that have been rediscovered by Blanchard et al. (2015). Super-hysteresis effects mean that a slowdown in the actual rate of growth of the economy, due, for instance, to a restrictive monetary policy, will have long-ranging effects, not only on the potential level of output but also on the growth rate of potential output. This can be explained, at least in part, by the

Kaldor-Verdoorn effect, as the slow growth in actual output is said to generate a slowdown in the growth rate of labour productivity. The effect can also occur on the upside, although neoclassical authors, seduced by downward hysteresis or super-hysteresis, seem dubious of a possible upward hysteresis effect.

As a conclusion on this section, it may be worth recalling that the Kaldor-Verdoorn effects became a hot topic during the recent primaries of the Democratic Party in the USA, when a controversy erupted between Gerald Friedman (2016a, b)—an economist from the University of Massachusetts in Amherst—and Christina and David Romer (2016), two economists who had held important positions in the federal administration. The controversy arose with regard to Friedman's estimates of the impact of the economic programme of the 2016 Democrat presidential candidate Bernie Sanders. To his dismay, Friedman discovered that the Kaldor-Verdoorn hysteresis effects that he had assumed in his estimates were not part of standard modelling, that is, the kind of models which are used by the Council of Economic Advisers. While Friedman may have overestimated the effects, at least the debate propelled the Kaldor-Verdoorn law into the sight of part of the layman public.

4 Methodological Considerations

4.1 Instrumentalism in Mainstream Economics

At the end of their book, Felipe and McCombie (2013, Chap. 12) wonder why their criticisms (and those of Shaikh, Simon and many others) of the aggregate production function have generally been ignored. In fact, McCombie's first foray in methodology was his 1998 paper on 'paradigms, rhetoric and the relevance of the aggregate production function', where he was already asking a similar question. Why are these demonstrations, and in particular the 'reductio ad absurdum' proofs, unable to convince neoclassical economists? One answer, offered by Solow, as recalled by Felipe and McCombie (2013, Chap. 5 and pp. 320–321), is that 'we knew it all beforehand'. This is dubious, because Solow (1957) in his own early work marvelled 'that the fit is remarkably tight'. But even

if we grant the benefit of the doubt, this kind of answer contradicts the fact that neoclassical economists keep using standard aggregate production functions even today. It must be quite frustrating to realize that a quite straightforward argument just does not seem to have any impact on the profession when the consequences of the argument are so profound for mainstream theory.

I had my own experience when I presented a paper devoted to this issue in the economics department of the University of Ottawa (Lavoie 2008). I followed this up by sending by email two Shaikh and McCombie papers to those of my colleagues that I considered to be most open to dialogue. My colleagues listened politely during the presentation, and there was no contestation. Amazingly, the overall response was an apparent inability to understand the implications of the presentation and of the papers that I had sent. I found that a confusing feature for neoclassical economists is that their theory predicts that with perfect competition and factors paid at the value of their marginal product, the output elasticities will equate the factor shares. This is what the regressions yield when technical progress is properly taken into account. Mainstream economists don't get the point that, because of the identity, the estimate of the output elasticities will always turn out to be equal to factor shares.

The most genuine answer came from a member of the department that had been involved in the government and in advising developing countries: he told me that without the estimates of the output elasticities of the factors of production, there was nothing that he could advise about any more. As a consequence, he had to rely on the elasticity estimates derived from the regressions over deflated values, whatever their true significance. In other words, as Paul Davidson (1984) once put it when describing mainstream economics, he would prefer "to be precisely wrong rather than roughly right" (p. 572).

The reactions of neoclassical or mainstream economists to the findings regarding the tight links between the aggregate neoclassical production function and the national accounts identity, when these links are known, are thus strongly reminiscent of the instrumentalist position held by the majority of these economists. This is pointed out by Felipe and McCombie (2013, p. 314), as they recall that instrumentalism in economics is usually attributed to the (only?) methodological essay of Milton Friedman

(1953), according to whom the realism of assumptions is totally irrelevant and can even be a drawback. Robert Lucas (1981) has no doubt pursued instrumentalism to the hilt, when he claimed that “insistence on the ‘realism’ of an economic model subverts its potential usefulness in thinking about reality”, adding that good models had to “necessarily be artificial, abstract, patently unreal” (p. 270).

Indeed, McCombie and Negru (2014) remark that the New Consensus model, also called the New Neoclassical Synthesis, is based on an instrumentalist approach. They very correctly point out that “the criterion of success is the successful empirical implementation through calibration, rather than econometric testing” (p. 61). They add that “the accuracy of the assumptions, *per se*, is irrelevant. Primacy is given to the construction of artificial models that closely mimic the observed path of the economy (Lucas 1978). Indeed, at times, it seems as if econometric testing is irrelevant. What matters is that there should be a fully-articulated model, based on paradigmatic pseudo-assumptions, that has been shown to be capable of replicating the path of the economy” (*ibid*, p. 62).

While this critique may seem rather harsh, Paul Romer (2016), the new chief economist at the World Bank, has addressed an even more ruthless critique to New Classical economists and their dynamic stochastic general equilibrium (DSGE) models based on real business cycle theory. He complained that these authors were calibrating their models so that they could fit a number of stylized facts, without ever being able, however, to demonstrate that the assumed mechanisms—imaginary shocks to technology or utility functions—had any relationship with reality. Indeed, with the large number of parameters of these DSGE models, with enough patience, it is nearly always possible to provide a fair fit. As a result, Romer (2016) concludes that

in the last three decades, the methods and conclusions of macroeconomics have deteriorated to the point that much of the work in this area no longer qualifies as scientific research. The treatment of identification in macroeconomic models is no more credible than in the first generation large Keynesian models, and is worse because it is far more opaque.... The larger concern is that macroeconomic pseudoscience is undermining the norms of science throughout economics (p. 1).

While Romer focuses his attention to the likes of Lucas, Prescott and Sargent, his critique extends to the New Keynesian version of the New Consensus, as he also questions the way sticky prices are introduced into DSGE models. This is obvious if one recognizes, as do McCombie and Negru (2014), that “the difference between the New Keynesians (but not the post-Keynesians) and the New Classical economists are now a matter of degree, rather than of a fundamental nature” (p. 60). The benchmark model in the New Consensus is the real business cycle model of the New Classical economists; the New Keynesians add lots of rigidities and frictions to this benchmark model, but the logic of their revised DSGE model is no different from that of the benchmark model. New Keynesians integrate some degree of realism through the incorporation of auxiliary hypotheses—asymmetric information, credit rationing, liquidity-constrained households and sticky prices.

The main assumptions, however, based on an all-knowledgeable agent, attempting to maximize some utility function for eternity, defy common sense. The question, then, is whether it is possible to arrive at a model that describes the real world adequately by adding auxiliary realistic characteristics. Nicholas Kaldor (1966), for one, thought it was not possible: in an attempt to relieve the programme of its unrealistic foundations, the whole edifice would crumble. As he put it, removing the scaffolding “is sufficient to cause the whole structure to collapse like a pack of cards” (p. 310). Indeed, Kaldor (1972) used the same argument six years later, saying that “the scaffolding gets thicker and more impenetrable with every successive reformulation of the theory, with a growing uncertainty as to whether there is a solid building underneath” (p. 1239). There is no doubt that the scaffolding has taken gigantic dimensions with the advent of Lucasian economics as well as that of the New Consensus and its DSGE models, which are at the heart of mainstream macroeconomics.

Coming back to the issue of why the critique of the aggregate production function does not seem to have made a dent in the armour of neo-classical macroeconomics, McCombie and Pike (2013, p. 503) recall that econometric results rarely did have an impact on the beliefs of the profession. They give as an example the damning econometric critique by Hendry and Ericsson (1991) of the claims made by Friedman and the monetarists, which came out in 1985 but only got accepted for publication

in 1991: most likely it was not the cause of the downfall of monetarism. McCombie and Negru (2014, p. 60) also mention the empirical proofs, too numerous to be recorded, showing that the uncovered interest parity equation just does not hold in the real world, have had no effect on the theoretical models constructed by both orthodox and heterodox economists. In this regard, the Sonnenschein-Mantel-Debreu theorem which questions from within the principle of scarcity and the Walrasian general equilibrium model has had no impact either, as pointed out again by McCombie and Negru (2014, p. 61) and as I have myself called attention to in Lavoie (1992, pp. 36–41) and Lavoie (2014, pp. 50–53).

4.2 Meta-Regression Analysis

An interesting counter-example is the work of Card and Krueger (1995), who provoked an economic earthquake when they contended, based on their own work and a rudimentary meta-regression analysis that raising the minimum wage had virtually no negative effect on employment and that previous research was flawed by publication bias. This counter-example is interesting on several grounds. First, from the sociological standpoint, the authors were considered to be traitors to the (neoclassical) cause as they came from Ivy League universities and were rejecting what was considered until then as one of the best established facts of neoclassical theory, a fact which had found proud of place in most introductory textbooks. Second, surprisingly, despite their work having been subjected to intense criticisms, a number of US economists seem to be less convinced by the negative impact that an increase in the minimum wage is likely to have on the employment of youths.⁵ The only explanation that I can find for this proposition is that the lack of a negative effect is only incompatible with the pure competition version of neoclassical theory; within a labour-market model based on the confrontation between a monopolist and a monopsonist, anything goes when real wages are raised. Third, the book of Card and Krueger seems to have given a boost to meta-regression analysis in economics. This type of empirical analysis has found room in many different journals and particularly in the *Journal of Economic Surveys*.

I have tried to provide an introduction to meta-regression analysis in my book on post-Keynesian economics as I believe that meta-regression analysis offers an additional tool to overcome the publication bias so common in economics and to dismiss the belief that empirical studies provide support for most if not all of the standard claims of neoclassical textbooks (Lavoie 2014, pp. 64–70). Thus, in a sense, meta-regression analysis is a companion to the works of Shaikh as well as those of Felipe and McCombie, which have dismissed the bogus empirical support for neoclassical production functions. It helps to provide an antidote to the claim that there is no alternative (TINA) and it helps to convince students that there is room for alternatives in economic theory.

At the heart of the identification of publication bias is the notion that investigators who rely on smaller samples, with fewer degrees of freedom, are prone to larger standard errors. This implies that estimates of a parameter are likely to be less precise. In order to obtain statistically significant effects (say t ratios above 1.6), they will need to find large effects since the t statistic is the size of the coefficient divided by the standard error. This may require several tries, with different specifications. By contrast, with large samples, estimates are likely to be more precise, standard errors will be smaller, and hence a statistically significant result can be achieved despite smaller values of the estimated parameter. Thus, an adept of meta-regression analysis needs two things from each past regression: the size of the estimated parameter e , usually some elasticity measure, and a proxy of the precision of the estimate, ideally measured by the inverse of the standard error (SE). The meta-regression will thus be the following:

$$e_i = \beta_1 + \beta_0 SE_i + \varepsilon_i \quad (12.11)$$

β_1 represents the estimated *true* value of the parameter, for if the standard error SE is zero, then the estimate e will be equal to β_1 .⁶ We can then proceed to standard tests and check whether the null hypothesis $H_0 : \beta_1 = 0$ can be rejected or not. A fancier meta-regression analysis, based on a multi-variate approach, can also be pursued. In the case of research on the effect of the minimum wage, Doucouliagos and Stanley

(2009) find that the true effect is -0.009 and that it is statistically significant (the t ratio is 3.15). The true effect however is not economically significant: a 50 per cent increase in the real wage would lead to less than a $\frac{1}{2}$ per cent decrease in teenage employment. If one were to take the average elasticity of the 1474 regressions analysed on this topic, the effect would be 20 times bigger! This shows the importance to rely on meta-regression analysis, as Doucouliagos and Stanley (2009) also find evidence of publication bias in studies devoted to the minimum wage.⁷

McCombie and Negru (2014, p. 62) note that it is not easy for neo-classical economists to reject the natural rate of unemployment hypothesis (or for that matter the NAIRU hypothesis), even when the evidence seems to be unfavourable to the hypothesis. Ray Fair (2012) has long been arguing that “the dynamics behind NAIRU equations are not supported by the data” (p. 9). Meta-regression analysis also concludes that there is no support for the concept of the natural rate of unemployment or the NAIRU. Tom Stanley has done two meta-regression analysis studies that pertain to the natural rate of unemployment story. In Stanley (2004), he looks at the persistence coefficient and finds that the true value appears to be very close to unity, thus implying that one cannot reject the hypothesis of unemployment hysteresis: the natural rate follows the actual rate of unemployment. This result is in line with the more recent work on hysteresis conducted in the conventional manner by Blanchard et al. (2015). In Stanley (2005), the other side of the NAIRU hypothesis is being explored: he looks at the relation between inflation and unemployment. He concludes from it and from his previous study that “the natural rate hypothesis may now be regarded as empirically falsified” (Stanley 2005, p. 626).

Despite all this, Fuller and Geide-Stevenson (2014, p. 135) report that the percentage of US economists that approve or approve with provisos the statement that “there is a natural rate of unemployment to which the economy tends in the long run” has not changed between 1990 and 2010: that percentage remains around 75 per cent. There is indeed resistance, except among heterodox economists and perhaps orthodox dissenters, to the dismissal of the natural rate hypothesis or that of the NAIRU concept, despite all their empirical failings, just as there is resistance to the dismissal of the neoclassical aggregate production function.

There are many more neoclassical key constructs, which have recently been questioned by the results of meta-regression analyses. This is particularly the case in microeconomics, which is usually considered to be the *forte* of neoclassical economics. Doucouliagos and Stanley (2013) look at 87 areas of economic research. They conclude that approximately 60 per cent of these suffer from severe or substantial publication bias. In microeconomics, the price elasticities of demand for residential water, tobacco, beer, spirits and alcohol all suffer from substantial or severe selectivity problems. They also show that these elasticities are very much overestimated, all of them being much below unity, so that the strength of substitution effects, which is at the heart of orthodox economics, is much weaker than usually described. In particular, the true value of the elasticity of CEO pay relative to performance seems to be zero!

Does meta-regression analysis have to say anything on some of the cherished beliefs of heterodox or post-Keynesian economics? Krasso-Peach and Stanley (2009) look at what they call the efficiency-wage hypothesis, that is, in our own terms, what I have called the Webb effect. They conclude that whereas “most previous studies report mixed or ambiguous support for the efficiency-wage hypothesis, we find clear and robust evidence of a positive efficiency-wage effect on production” (p. 267). Indeed, their true estimate of the Webb effect elasticity is around 0.30, a number which is similar to what is found by Storm and Naastepad (2012, p. 103).

And what about the Kaldor-Verdoorn effect? Ludwig List, a PhD student at the University of Paris 13, has just conducted a meta-regression analysis on this effect. On the basis of nearly 120 estimates, List (2017) finds the true value of the Kaldor-Verdoorn effect to be 0.42, with no evidence of publication bias. This finding is consistent with the estimates claimed by post-Keynesians: McCombie (2002, p. 106) argued for robust estimates between 0.30 and 0.60; Hein and Tarassow (2010, pp. 748–749) found estimates between 0.27 and 0.86; and Storm and Naastepad (2012, p. 103) arrived at estimates ranging between 0.39 and 0.47. We may thus conclude from this meta-regression analysis that the Kaldor-Verdoorn effect is genuine and within its usually estimated range.

5 Summary and Conclusions

John McCombie is part of the Cantabrigian school of economics, which has provided an alternative vision of what macroeconomics is all about. He is one of the few who has managed to find a position and keep alive this tradition within the confines of the University of Cambridge, inspired by his predecessors such as Nicholas Kaldor or Bob Rowthorn. He has maintained the use of econometrics in post-Keynesian economics, at a time when abstract considerations were in fashion, and he made several important contributions to the Kaldorian strand of post-Keynesian economics.

In this chapter, I have reappraised his contribution to the Cambridge capital controversies by underlining the main arguments justifying the claim that empirics cannot provide support to the aggregate neoclassical production functions. I have shown that the Kaldor-Verdoorn law is not subjected to this critique. And I have argued that meta-regression analysis can provide additional elements in support of post-Keynesian economics.

Notes

1. It also appeared in my book on the foundations of post-Keynesian economics (Lavoie 1992, pp. 33–36).
2. Shaikh (1980, p. 2005) shows that labour productivity is highly non-linear, nearly sinusoid, and hence cannot be represented by a linear trend. Regressions of the neoclassical production functions in time series will provide bad or even absurd results when technical progress is mishandled (for instance, by assuming a linear trend). A possible remedy is to include the rate of capacity utilization as an additional variable in the regression. This was indeed my personal experience when a graduate student of mine ran production regressions on Canadian data and was getting desperate until he added the rate of utilization in his regressions.
3. They are also discussed in my 2014 book (Lavoie 2014, ch. 1).
4. The authors declined to respond to my critique.

5. Fuller and Geide-Stevenson (2014) report that when surveyed members of the American Economic Association were confronted with the statement “A minimum wage increases unemployment among the young and unskilled workers”, in 1990, 62 per cent agreed, 20 per cent agreed with provisos and 18 per cent disagreed; in 2010, 40 per cent agreed, 34 per cent agreed with provisos and 25 per cent disagreed.
6. In reality, to correct for possible heteroskedasticity, meta-regressions are based on the following equation, with β_1 and β_0 keeping their previous meaning: $(ei/SEi) = ti = \beta_1(1/SEi) + \beta_0 + \varepsilon_2$.
7. The absolute value of the parameter β_0 is a measure of publication bias and the authors find that its t ratio is above 10.

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