Aggregate Production Functions and the Accounting Identity Critique: Criticisms and Misunderstandings with Special Reference to Temple

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Abstract The aggregate production function continues to be widely used, notwithstanding the fact that the Cambridge capital theory controversies and aggregation problems have shown that its foundations are tenuous in the extreme. The main reason for its use seems to be the instrumentalist position according to which aggregate production functions "work", in that they usually, but not always, give close statistical fits with plausible estimates of the parameters. However, Anwar Shaikh was one of the first to show that this is simply the result of the existence of an underlying accounting identity together with the use of value data. This interpretation with reference to the subsequent work of Felipe and McCombie has been challenged by Temple in two articles. While Temple concedes the argument has some validity, he erroneously believes, *inter alia*, that the critique only holds under certain *ad hoc* assumptions. As a consequence, he argues that the criticism only works "part time". This paper further discusses Temple's arguments and demonstrates that none of them is compelling and the critique, in fact, works "full time". Because of the underlying accounting identity, estimates of the parameters of aggregate production functions cannot be regarded as reflecting the underlying technology of the industry or economy and Anwar Shaikh's original insights remain valid.

Key words: Accounting identity, aggregate production function, aggregation problems, value-added identity, value data

JEL classification: C43, O11, O16, O47, O53

> "I am informed ... that attempts to explain the impossibility of using aggregate production functions in practice are often met with great hostility, even outright anger. To that I say ... that the moral is: 'Don't interfere with fairytales if you want live happily ever after'." (Franklin Fisher, 2005, p.491).

> "[Neoclassical economists] don't like the true and simple; they like fairy tales and humbug." (With apologies to Edmond de Goncourt, 1822-1896).

Introduction

It is nearly ninety years ago since Cobb and Douglas (1928) first published the results of their estimations of the now familiar multiplicative aggregate production function that bears their name. In that paper, they also set out the conditions for the aggregate marginal productivity theory of factor pricing. Douglas (1976) subsequently argued that the close approximation that the estimated output elasticities bore to their respective factor shares confirmed the assumption of perfectively competitive markets and disproved the Marxian explanation of the distribution of income. It is sometimes forgotten now that Cobb and Douglas's original study was so heavily criticized by the econometricians and Douglas's *a priorist* colleagues at Chicago that he nearly gave up the whole endeavor (Douglas, 1967). Nevertheless, he persevered with his colleagues and felt eventually vindicated by being elected President of the American Economic Association in 1947. The title of his Presidential address, "Are there Laws of Production?" (Douglas, 1948), was largely rhetorical. The ultimate accolade came in 2011 when the article Cobb and Douglas (1928) was nominated as one of the 20 most "admirable and important articles" published in the *American Economic Review* in the last hundred years (Arrow, 2011). By implication, all doubts about the validity of the neoclassical one-sector aggregate production function and the associated aggregate marginal productivity conditions as a meaningful representation of the technical conditions of production of individual industries, or the whole economy, had been banished. The unanswered serious reservations about aggregation problems (Fisher, 1992, 2005) or the Cambridge capital theory controversies (Harcourt, 1972, Birner, 2002, and Cohen and Harcourt, 2003) have disappeared from

the collective memory of mainstream economists or been relegated to the history of economic thought.[1]

While the Cambridge capital theory controversies have attracted more notoriety, the more general aggregation problems have proved even more damaging. Franklin Fisher (2005, p.490), who has probably done most work on this topic was forced to conclude that "even under constant returns, the conditions for aggregation are so very stringent as to make the existence of aggregate production functions in real economies a non-event. … *One cannot escape the force of these arguments by arguing that aggregate production functions are only approximations*." (Emphasis added.)

It was half a century ago that Samuelson (1962) tried, and failed, to provide a theoretical foundation for the use of the aggregate production function as a "parable" or useful approximation reflecting the myriad underlying complex micro-production function relations. Samuelson attempted to show that a series of two-sector production techniques (a pair of fixed-coefficient techniques to produce a consumer and a capital good) could give rise to an aggregate relationship with all the standard properties of, say, an aggregate Cobb-Douglas production function. While problems of the measurement of capital and more general aggregation problems can be traced back to the Classical economists and had been repeatedly emphasized in the 1950s by Joan Robinson (1953-54, 1956), this paper galvanized a debate in capital theory between Cambridge, Massachusetts and Cambridge, UK.[2]

The outcome was that unless the capital-labour ratios of the two sectors were identical, the phenomenon of reswitching could occur. A technique could be the most profitable at both a high rate and a low rate of interest, but not at an intervening rate of interest. There was no necessary inverse monotonic relationship between the rate of profit and the capital-labour ratio, as implied by the standard aggregate neoclassical production function. For reswitching to be ruled out meant, in

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[1] It is indicative that Cohen and Harcourt felt compelled to write a reminder for the economics profession in the 2003 issue of the *Journal of Economic Perspectives* in the "Retrospectives" section entitled "Whatever Happened to the Cambridge Capital Theory Controversies?" and that Birner's 2002 volume, *The Cambridge Controversies in Capital Theory*, is part of the *Routledge Studies in the History of Economics.*

[2] Strictly speaking, it was the erroneous attempt of Lehavari (1965) to remove limiting conditions in Samuelson's paper that crystallized the debate.

effect, any technique in Samuelson's model had to have identical capital-labour ratios. This effectively meant Samuelson had not escaped from assuming that there was a one-sector model. This phenomenon of reswitching had been known for some time, being regarded by Joan Robinson as merely the "Ruth Cohen curiosum" and had been discussed by Champernowne (1953-54) and Sraffa (1960), but assumed greater prominence as a result of Samuelson's (1962) paper. Indeed, Samuelson's (1966) summing up of the debate gave a simple example as to why reswitching occurs. The ensuing debate was published as a symposium in the November issue of the 1966 *Quarterly Journal of Economics.* For a time, the outcome was seen as the death knell for the aggregate production function. In the immediate aftermath, advanced undergraduate textbooks, especially on growth theory, carried discussions of the debate (Wan, 1971, Jones, 1975, Haache, 1979). But, surprisingly, by the early 1990s all references to it had disappeared (Barro and Sala-i-Martin, 1995, Aghion and Howitt, 2009, Weil, 2005, Acemoglu, 2009).

Given that it was a debate involving formal logic, rather than the plausibility about certain assumptions (such as the relevance of the assumption of perfect competition), the reason for this collective amnesia remains an important question for the history of the subject. In this paper, we focus on what we see as the most convincing reason, namely the instrumentalist defence (Friedman, 1953) that, not withstanding the aggregation problems and the implications of the Cambridge capital theory controversies, the aggregate production function "works". In other words, estimations of the aggregate production function often give close statistical fits with "plausible" estimates of the parameters.[3]

However, this methodological stance was demolished in important articles by Phelps Brown (1957), Simon and Levy (1963) (both for estimation using cross-section data) and especially the influential papers of Anwar Shaikh (1974, 1980).

Shaikh (1974) made two separate critiques of Solow's (1957) famous paper on the aggregate production function and technical change. First, he showed that Solow's method of 'deflating' the time-series data to remove the influence of the growth of technology when estimating the

[3] Han and Schefold (2006) use input-output tables for the OECD countries to test empirically the possibility of reswitching. They find that the existence of at least three switch points between two wage curves is negligible; it occurs only 0.73% of the time. However, the input–output coefficients used are *value* measures, not physical technical coefficients, and so it is perhaps not surprising that these results come to the same conclusion as the direct estimation of the aggregate productions and are subject to the same critique. (See Gandolfo (2008) for a more general critique of this approach.)

production function reduced the latter procedure to a tautology, as Solow (1974) conceded. To drive the point home, Shaikh constructed a data set where the scattergram for productivity on the vertical axis and the capital-labour ratio on the horizontal axis spelt out the word HUMBUG and this likewise gave a near perfect fit to the Cobb-Douglas production when Solow's estimation method was used. Secondly, and more importantly, Shaikh showed how the underlying accounting identity would ensure a near perfect statistical fit to the aggregate production, even though the latter did not exist. Shaikh (1980) is a further elaboration of this argument and also a devastating refutation of Solow's (1974) critique of Shaikh (1974). Mention must also be made of Shaikh's excellent simulation paper that showed how, because of the accounting identity, data generated by Goodwin's (1967) fixed-coefficient growth model will also give a good statistical fit to the Cobb-Douglas 'production function', because of the stability of the factor shares.

The baton was then picked up by the present authors who have extended and generalized the critique and shown how the results of some seminal papers and models are dependent upon the accounting identity. These include the augmented Solow model of Mankiw *et al.*, (1992) (Felipe and McCombie 2005a); Hall's (1988) estimates of the mark-up (Felipe and McCombie, 2002), and labour demand functions (Felipe and McCombie, 2009).

Surprisingly, this critique has been almost totally ignored by both post-Keynesians and mainstream economists alike. There is no entry in the *Elgar Companion to Post Keynesian Economics* and literally two sentences in King's (2002) history of post-Keynesian economics. This is not withstanding Joan Robinson's comment (1970, p. 317, omitting a footnote) that the "statistical defence" of aggregate production function "must have needed an even tougher hide to survive Phelps Brown's article on "The Meaning of the Fitted Cobb-Douglas Function" than to ward off Cambridge Criticism of the marginal productivity theory of distribution".

An exception is Temple (2006, 2010), who in two articles commented specifically on the accounting identity critique and authors' work in this area. While conceding some points, he argues that the critique only "works part-time" Temple (2010). This was in spite of a rebuttal to Temple (2006) by Felipe and McCombie (2010a), which Temple largely ignored. Temple's papers are important if only to the extent that they demonstrate what may be common fundamental misunderstandings of the critique by mainstream economists and hence the neglect of its damaging implications. Consequently, in this paper we return to this debate and show that the critique works "full time", *pace* Temple. Felipe and McCombie (2012) focus on some broader methodological issues concerned with the critique.

The Instrumental Defence of the Aggregate Production Function

A common defence of the use of unrealistic assumptions is Friedman's (1953) methodological stance that a "theory is to be judged by its predictive power of the class of phenomena which it is intended to 'explain' … the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience" (pp. 8-9). The realism of its assumptions is irrelevant. The only problem is "whether they are sufficiently good approximations for the purpose at hand. And this question can only be answered by seeing whether a theory works, that is, whether it yields sufficiently accurate predictions" (p. 15). While Friedman's methodological stance has been heavily criticized (Samuelson, 1963, Kincaid, 1996, pp. 227-228), his approach is still widely accepted by economists.

As far as the aggregate production function is concerned, Wan (1971, p.71) views it as an empirical law in its own right, which is capable of statistical refutation. The instrumental defence is also implicit in Solow's remark to Fisher, that "had Douglas found labor's share to be 25 percent and capital's 75 per cent instead of the other way around, we would not now be discussing aggregate production function" (cited by Fisher, 1971, p.305). Ferguson (1969, p.xvii) explicitly made this instrumental defence with respect to the criticism about the measurement of capital as a single index in Cambridge capital theory controversies. "Its validity is unquestionable, *but its importance is an empirical or an econometric matter* that depends upon the amount of substitution there is the system. Until the econometricians have the answer for us, placing reliance upon [aggregate] neoclassical economic theory is a matter of faith. I personally have faith".

The citation in the introduction to this paper as to why the Cobb-Douglas (1928) article should be regarded as one of the twenty most influential articles in the last hundred years published in the *American Economic Review* states: "Cobb and Douglas explored the elementary properties and implications of the functional form, and pointed to the approximate constancy of the relative shares of labor and capital in total income as the *validating empirical fact* (Arrow *et al*., 2011, p.2). Hoover (2012, p. 326) also adopts an instrumental position in his intermediate macroeconomic textbook.[4] He briefly notes the aggregation problems which "are well beyond the scope of this book". So

[4] As Kuhn (1970) points out, textbooks are crucial in that they are generally taken by students as being correct and they set the legitimate methods and assumptions for "puzzle solving" within the paradigm.

instead, "our strategy will be to start with a conjecture that the economy can be described by a particular production function [the Cobb-Douglas], one that shares important properties with microeconomic production functions. We will then test our conjecture empirically. *If it seems to describe the data well, we shall be satisfied that it provides a useful approximation*" (emphasis added)." What is this test? It is simply the approximate constancy of shares and "provides a good reason to take the Cobb-Douglas production function as a reasonable approximation of aggregate supply in the U.S. economy" (p. 330).

The problem with this line of reasoning is that a production function is theoretically a relationship between *physical* units of homogeneous output (say, widgets) and the flow of physical inputs, for example, efficiency-adjusted labour services and the flow of services of homogeneous physical capital inputs. [5] However, in practice, the use of constant-price *value* data has to be used for both output and capital. The existence of an underlying accounting identity that definitionally relates these to variables means that any regression of an "aggregate production function" will simply be an estimate of a mathematical transformation of this accounting identity. Hence, with a little ingenuity it should be always possible to obtain a perfect or, at least, a very close statistical fit. This argument can be made a number of ways, and so we will only give the case with respect to time-series (Felipe and McCombie, 2005b). The definition of value added is given by the accounting identity:

$$V_t \equiv W_t + \Pi_t \equiv w_t L_t + r_t J_t \qquad (1)$$

where V is constant price value added, W is the total wage bill, Π is total profits, w is the average wage rate, L is the employment, r is the *ex post* or earned rate of profit and J is the constant price value of the capital stock, usually calculated by the perpetual inventory method (the argument also holds for gross output where the value of output also includes the cost of materials). Expressing equation (1) in growth rate form gives:

$$\hat{V}_t \equiv a_t \hat{w}_t + (1 - a_t)\hat{r}_t + a_t \hat{L}_t + (1 - a_t)\hat{J}_t$$

[5] We leave aside the problems as how to measure these flows. In practice, the levels of employment and the capital stock are used, sometimes adjusted for changes in capacity utilisation.

$$\equiv \lambda_t + a_t \hat{L}_t + (1 - a_t)\hat{J}_t \qquad (2)$$

which is compatible with *any* state of competition and whether or not an aggregate production function actually exists.

A general form for the aggregate production function is $V = f(L, K, t)$ which, expressed in growth rates, gives:

$$\hat{V}_t = \lambda_t + \alpha_t \hat{L}_t + (1 - \alpha_t)\hat{J}_t \qquad (3)$$

If the usual neoclassical assumptions hold, including that factors are paid their marginal products, equation (3) can be written as:

$$\hat{V}_t \equiv \lambda_t + a_t \hat{L}_t + (1 - a_t)\hat{J}_t \qquad (4)$$

which is formally equivalent to the accounting identity, and where the rate of technical progress or, strictly speaking, the growth of total factor productivity is given from the dual by $\lambda_t = a\hat{w}_t + (1 - a_t)\hat{r}_t$. Neoclassical production theory generally estimates a specific form of equation (3), often assuming that the rate of technical progress is a constant with a random error.

A Common Misunderstanding of the Critique

The critique has often been dismissed out of hand by some neoclassical economists as simply an elementary failure to understand the optimization conditions underlying producer equilibrium. A firm will produce the optimal volume (and we use this word advisedly) of output where the cost curve is tangential to the highest isoquant. A corollary is that if we write the production function in intensive form $Q/L = f(K/L)$, then the equilibrium level of output will be given by Euler's theorem where the cost function expressed in the form $Q/L = f_L + f_K K/L$ is tangent to the production function. In this specification Q and K are homogeneous physical units (numbers of widgets and units of leets respectively). f_L and f_K are the marginal products expressed in physical units. This may simply be transformed into monetary terms by expressing the cost function as $pQ/L = f_L + pf_K K/L = w + \rho K/L$, where p is the price (in £s), w is the wage rate and ρ is the rental price of capital, again both measured in £s. Consequently, $pQ/L = w + \rho K/L$ is also an identity as the total factor payments must exactly equal the value of output. Thus, in terms, of figure 1, this occurs at point A where the cost equation and the production function (in intensive form) are at a tangent. All, for

example, it is (erroneously) argued that Simon and Levy (1962) accomplished with their Taylor series expansion of the cost and the Cobb-Douglas production function is to prove that at the point of tangency (or strictly speaking in the neighbourhood of *A*), the optimal level of output per worker is given by two equivalent equations. This, so the argument continues, does not imply that there is no underlying production function.

Unfortunately, this argument is a classic *petitio principii* or a case of 'begging the question'. This is the fallacy of assuming in the premise of an argument (namely, the existence of an aggregate production function) that which one wishes to prove in the conclusion (namely, the existence of aggregate production function).

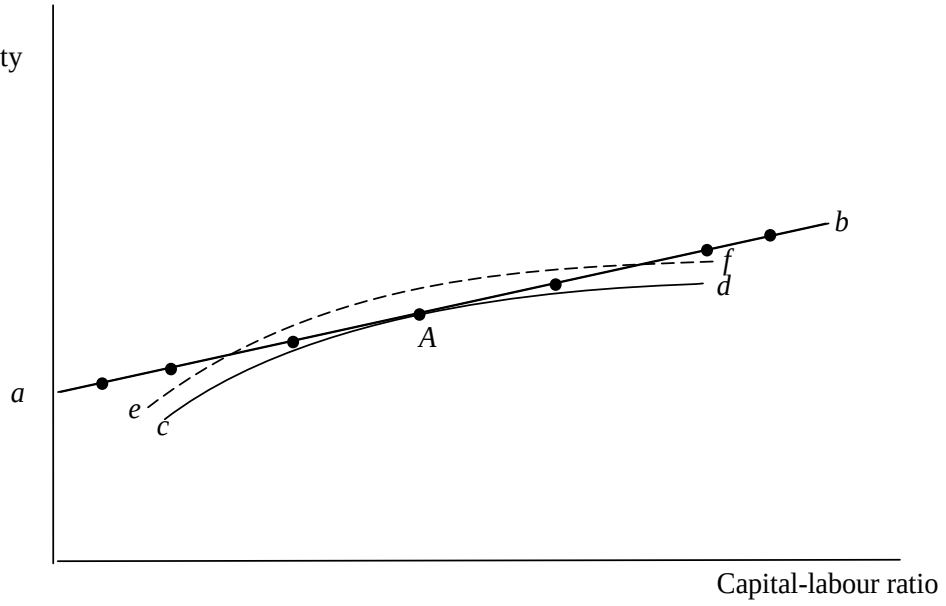

Figure 1. The Cobb-Douglas Approximation to the Linear Accounting Identity

Although the analysis above has been conducted in terms of value terms, as the price (in, say, £'s) is known, the aggregate production function can simply be written in physical terms and apart from the usual econometric specification problems, this can be estimated and reflects the underlying technology of the economy. This implicitly assumes that $PV = V \equiv pQ$ where P is the price deflator (normalised for convenience to unity) and V is value added. Hence the use of value data (value added and the constant price value of the capital stock) can be used as proxies in the estimating of an aggregate production function. Hence, it can be inferred from the findings that, say, a Cobb-Douglas gives a good fit to the data implies that the aggregate elasticity of substation is a meaningful technological concept and takes a value of unity.

A moment's reflection will show there is a serious error in this argument. Value added is actually defined as $V_{t(0)} \equiv \sum_i p_{i0}Q_{it}$. In other words, value added at time *t* in base-year *0* prices equals the sum of all the physical units of output (chairs, personal computers, aircraft, etc.) multiplied by their unit prices. The total value of the capital stock is calculated by the perpetual inventory method using the cumulative deflated *values* of the investment goods, minus an assumed rate of depreciation of the value of the existing capital stock. Hence, we know that the linear accounting identity must hold be definition and the direction of 'causation' is directly from this to the form of the aggregate production function which in all probability does not exist. This is not to say that there is no relationship at the micro-level between quantities of inputs and outputs measured in *physical* terms; obviously there is a relationship between output and the inputs. However, the form of even these micro-production functions cannot be recovered from the data unless physical data are used and even then the relationship may be very complex, obscured by differences in x-efficiency, and change over time, etc.

Hence, in terms of figure 1, if we have several observations for the accounting identity (and for expositional purposes we assume that the wage rate and the profit rate is constant), and fit, say, a Cobb-Douglas production function, then the best statistical fit will be given by the dotted line *ef* even though no aggregate production function exists. Simon (1979) has shown that the degree of error induced by fitting a Cobb-Douglas production function to the linear accounting identity in these circumstances for plausible ranges in the capital-labour ratio is very small, around 5%. Simon and Levy (1963, p. 94) are quite clear about what they see as the chain of reasoning: "Thus, the existence of a fitted Cobb-Douglas function with a value of k [the output elasticity of labour] in agreement with the actual k [labour's share in output] does not imply that the underlying production

function is truly Cobb Douglas. In fact, we expect this agreement when the true function is given by [the accounting identity]."

The fact that distinction between value added data and the value of homogeneous output is not explicitly made accounts for this above confusion over the argument.

Temple's (2006, 2010) Criticisms of the Accounting Identity Critique[6]

Temple (2006) has raised a number of objections to our arguments (the latter have been brought together in the book by Felipe and McCombie (2013)) concerning the accounting identity, none of which is compelling (Felipe and McCombie, 2010a). In a rejoinder, Temple (2010) concludes that we make substantially more of the criticism of production functions estimated using value data (at any level of aggregation) than it deserves. Hence, he labels it "the part-time tyranny of the identity". However, he largely repeats the objections of his 2006 paper and generally ignores our counter-arguments. While he concedes that there are some areas of agreement and that the argument deserves to be better known, he contends that we have gone too far and exaggerated the scope and implications of the argument. To do so, he reviews some of our work and disputes a number of our conclusions.

We stand by the full extent of the implication of the argument, namely, that the use of value data (as opposed to physical quantities) in the estimation of *any* specification of an aggregate production function, whether or not it is a Cobb-Douglas, precludes the researcher from interpreting the regression results as the technological parameters (e.g., the factor output elasticities or the elasticity of substitution). However, Temple persists in maintaining that the critique *only* relates to the Cobb-Douglas relationship not withstanding the above argument. We also argue that it is true for any level of aggregation using value data. The aggregate production function is, in fact, unlikely to exist, not least because of the serious aggregation problems and variations in *x*-efficiency, etc. The only certainty is that the regression results and the values of the estimated parameters are determined by the accounting identity. The tyranny of the identity works "full time".

[6] This is taken from Felipe and McCombie (2013, chapter 12)

Temple, nevertheless, agrees with us on two points. The first one is that the aggregation problem should receive more attention in the literature than it does, although he argues that there are other approaches that are not so reliant on aggregation, e.g., the use of multi-sector models, reduced-form regressions, and methods to infer productivity levels from bilateral trade data (Temple 2010, 686). We do not deal with this view here, but concern ourselves solely with the problems posed by the accounting identity for the aggregate production function.

As we have noted above, both the Cambridge capital controversies and the more general aggregation literature suggest that the aggregate production function does not exist. "Indeed, Temple (2005, 438) himself gives the simple example that two Cobb-Douglas production functions with different exponents cannot be aggregated to give a single Cobb-Douglas production function.[7] Surprisingly, later in his comment, he declares himself "agnostic" on this issue (Temple, 2010, 689), although no compelling reasons are given for this. It is not clear why aggregation problems disappear and a true production function can be estimated if "we have no prior reason to believe that output and inputs are badly measured" (Temple, 2010, 689), especially when it is agreed that the accounting identity critique has nothing to do with measurement errors.

The second area where there is agreement is that an applied researcher may appear to obtain meaningful results from estimating a production relationship, even when the researcher is making assumptions that do not hold in the data. "One important instance arises when factors are not paid their marginal products. In that case, although researchers often interpret their results as if the estimated parameters can be used to derive output elasticities, the identity *suggests* that the estimates may be more closely related to the factor shares" (Temple 2010, 686; emphasis added). This would seem to go a long way to conceding our position and poses difficulties for understanding the rationale for his criticisms. We would indeed agree with this statement, except that the identity *shows*, not *suggests*, that the estimated coefficients will take values that are equal to the factor shares, even when no well-defined aggregate production function exists.[8] An implication

[7] Nevertheless, ironically, this does not stop Temple (2005) from assuming that the production functions of agriculture and non-agriculture are each represented by an aggregate Cobb-Douglas, and that factors are paid their aggregate marginal products, as if aggregation problems did not matter for these sectors.

[8] Where this does not prove to be the case, it is because the mathematical transformation of the identity to give a specific functional form (i.e., what the neoclassical economist calls the aggregate production function) does not accurately mirror the identity. In other words, this is when the statistical fit of the transformation is less than perfect. We elaborate on the reasons for this below.

of Temple's statement cited above is that given that the researcher has only access to constant-price value data; it can never be known whether or not the researcher is correctly estimating the parameters of a production function, or, indeed, whether or not the latter exists. This is precisely our critique.

Yet, at times in his reply, Temple (2010) takes the opposite view, and argues erroneously that if factor shares vary to an unspecified extent and the researcher can correctly specify "total factor productivity", all will be well. The aggregate production function can then be estimated and the values of the coefficients will correctly reflect the aggregate technological parameters of the economy, albeit with the necessity of finding the most appropriate statistical estimating technique. (We dealt with the erroneous arguments of Temple (2006) in Felipe and McCombie (2010a).

In the rest of this article, we briefly point out the problems with Temple's (2010) arguments. Broadly speaking, there are two issues that we wish to emphasize. First, Temple erroneously continues to imply that the critique only holds if certain *ad hoc*, or what he terms auxiliary, assumptions are made; typically the "stylized facts" that factor shares are constant and the weighted growth of the wage and profit rates are constant. To this, he incorrectly adds, in the case of our critique of Mankiw *et al.*'s (1992) growth model that we have, of necessity, to assume also a constant capital-output ratio for the criticism to hold. At times, as we noted above, he seems to assume that the critique only applies to the case of the Cobb-Douglas and so, presumably, once there is some variability in factor shares, he implies that we can actually be confident we are estimating a "true" aggregate production function. He nowhere answers the question posed by Felipe and McCombie (2010a) as to how much variability in factor shares is required to suddenly remove the problems posed by the accounting identity and aggregation problems.

Secondly, we show below that his argument at times reduces to *petitio principii*, or circular reasoning. Temple sometimes *assumes* that the aggregate production function exists, and uses this assumption to supposedly counter the argument that the relationship between output and inputs in value terms does not reflect a technological production relationship.

On some Misunderstandings and Misinterpretations of the Accounting Identity Critique

1. Economic rents and the actual and virtual accounting identities.

Temple implies that for the critique to hold, (his discussion is in the context of the Solow residual), rents need to be excluded from the accounting identity (Temple 2010, 688). However, the accounting identity simply shows how value added is measured. As we have seen, this is given by the identity $V \equiv W + \Pi$, where W is the wage bill and Π is the operating surplus. The latter includes all types of profits. All we do is to split the wage bill into the product of the average wage rate (w) multiplied by employment (L); and the surplus into the product of the average rate of return (r) times the value of the stock of capital (J). This implies that $V \equiv W + \Pi = wL + rJ$. This requires no economic assumptions whatsoever and holds true by definition.

Unfortunately, Temple seems to confuse the "actual" accounting identity with what we have termed elsewhere the neoclassical "virtual" identity (Felipe and McCombie, 2007), based upon the assumption of perfectly competitive markets and optimisation. Consequently the latter may, or may not, hold in reality. This is usually derived at the microeconomic level by applying Euler's theorem to the micro production function, together with the assumption that the marginal theory of factor pricing holds, i.e., $pY = p\,F(L, K) = p(\partial Y / \partial L)L + p(\partial Y / \partial K)K = wL + \rho K$ where Y is output and K is capital, both measured in *physica*l terms, and ρ is the rental price of capital. p is the price of a unit of output. This chain of reasoning is then applied seamlessly in neoclassical production theory to the macroeconomic level, regardless of the fact that output is value added (V) - not units of physical output - and capital is not the stock of homogeneous structures and equipment, but the constant-price value of the stock of capital (J).[9] If an estimate of the competitive rate of profit is used in the accounting identity, then the implied economic rents would have to be deducted from value added and the argument follows through exactly (Felipe and McCombie, 2007).

[9] The argument also holds for gross output by adding intermediate materials.

2. The accounting identity critique does not hold only when factor shares are constant.

Temple argues to the contrary that the identity argument only holds with constant factor shares and a constant weighted growth of the wage rate and the rate of profit, which he claims we have to introduce as arbitrary or *ad hoc* assumptions. This is most clearly seen in the following:

> All of their [Felipe and McCombie's] arguments share a common structure which is to manipulate the value added identity, add some auxiliary assumptions, and then show – under these maintained assumptions- that the data will appear to have been generated by a production relationship of a certain type, typically, *but not always*, Cobb-Douglas, even when no such relationship exists. (Emphasis added.)
>
> Here is their main claim stated explicitly: "Can a researcher using value data ever establish whether or not the coefficients reflect a production function, or are the simply predetermined by the value added identity? Our answer is that unequivocally the results are always determined by the identity". *Yet, the very next sentence in their paper assumes that the weighted average of growth of factor prices and factor shares are all constant.* These assumptions are needed to show how the identity leads to estimates that appear to support a Cobb-Douglas production function. Since these assumptions will not always be met, it is clear that the value added identity does not always lead to a spurious Cobb-Douglas result (Temple, 2010, 687-688, emphasis added).

Temple's argument, consequently, is that we assert (correctly in our opinion) that the results are "always determined by the identity", even when these assumptions are not met. But Temple disputes this and argues that if a good proxy for total factor productivity (TFP) can be found "there is no reason why a researcher should not discriminate between, say, a translog and a Cobb-Douglas specification. Say that the data have been generated by a stable production relationship, and the researcher specifies this relationship correctly, including controls for productivity differences such as TFP. In that case, the researcher is estimating a model that corresponds to the data generating process. There is no reason for the estimates to be biased, or for the parameters to be unidentified. In contrast, and for the same reason – the equivalence between the form of the estimated model and the generating process – the dynamic version of the value added identity cannot do better than this. It will certainly do worse, when the auxiliary assumptions introduced by Felipe and McCombie are not a good approximation to the data" (Temple 2010, 688, omitting a footnote).

The circular reasoning of Temple is readily apparent here. He assumes that a "stable production function exists" [i.e., the data is generated by an aggregate production function]. This can be

estimated provided TFP can be correctly specified, another concept dependent on the aggregate production function. Of course, if one adopts this *petitio principii* then the problem is merely one of determining the best specification and estimation techniques, which has been the subject of the numerous articles that have estimated the production function. As the "dynamic value added identity" is an identity, then a better way of putting it is that the estimate of any specification of an "aggregate production function" can do no better than this, rather than *vice versa* as Temple argues in the quotation cited above. And if factor shares vary, then, of course, the functional form that gives the best fit to the identity will not be the Cobb-Douglas. But this ignores (rather than refutes) the criticism that what is driving the results is the identity as the estimates are not of a behavioural equation. We spelt this out in Felipe and McCombie (2010a) immediately prior to our argument in the above citation and ignored by Temple (2010).

Consequently, the argument follows through *whether or not* factor shares and the weighted growth of the wage rate and the rate of profit are constant. In practice, as we have noted, researchers will attempt to find an explicit functional form that will give a good fit to the data generated by equation (1). Thus we have:

$$V_t \equiv w_t L_t + r_t J_t \quad \Rightarrow \quad \hat{V} \equiv a_t \hat{w}_t + (1-a_t)\hat{r}_t + a_t \hat{L}_t + (1-a_t)\hat{J}_t$$

$$\Rightarrow \quad \hat{V}_t \equiv \lambda_t + \alpha_t \hat{L}_t + \beta_t \hat{J}_t \;\Rightarrow\; V_t = f(L_t, J_t, t) \qquad (6)$$

with the arrows showing the direction of causation. This implies that $a_t \equiv \alpha_t$ and $(1\text{-}a_t) \equiv \beta_t \equiv (1\text{-}\alpha_t)$. As we have noted, economists try to find a specific mathematical functional form that will closely fit the data generated equation (3) and hence, by implication, the underlying identity. If, and only if, the weighted average of the growth of the wage and profit rates is constant, and factor shares are also constant, will a conventional Cobb-Douglas relationship fit this criterion. If they are not constant, then a more flexible functional form that contains the Cobb-Douglas as a special case, such as a Box-Cox transformation, or the translog, will be required. But these mathematical isomorphisms should not be regarded as aggregate production functions. Consequently, the argument does not apply solely to the case where the aforementioned assumptions hold. As this has

been quite generally emphasized throughout the literature on the subject, and especially in Felipe and McCombie (2010a), it is surprising that Temple should think otherwise.

The argument is consequently a matter of methodology and logic. What we show are the conditions under which a given form of the production function, say the Cobb-Douglas, would yield good results in terms of the usual statistical diagnostics. This is very different from claiming that specific assumptions or some structure must be imposed for the critique to hold. In fact, an implication of the accounting critique is that unless factor shares and $a_t\hat{w}_t + (1-a_t)\hat{r}_t$ are approximately constant, the estimation of the equation $V = A_o e^{\lambda t} L_t^{\alpha} J_t^{\beta}$ using time-series data will be likely to yield poor results. That is to say, implausible estimates of the factor elasticities that are very different from the values of the factor shares and may even be negative. The identity shows that a better fit can be obtained by both a more flexible functional form and time trend. But if these stylized facts hold, then the goodness of fit will potentially be unity and the estimated elasticities must equal the factor shares.

Given this, why has the Cobb-Douglas proved so durable, and why does it so often give a good statistical fit to the data? If we integrate equation (2), we obtain $V \equiv a^{-a}(1-a)^{(1-a)} w^a r^{(1-a)} L^a J^{(1-a)}$. This is not an approximation, but an isomorphism: it holds *exactly* for any particular year, whether it is for, say, the UK economy or an individual firm. As it is a stylized fact (not an assumption) that factor shares do not change greatly between firms in the same industry, and wages and profits show little variation compared to *L* and *J*, estimating cross-section production functions gives a good fit with a surprisingly high R^2 of over 0.9, and the estimated output elasticities equal the factor shares (Douglas, 1976). Time-series data often yield worse results, not because factor shares change dramatically over time (they do not), but because $a\ln w_t + (1-a)\ln r_t$ is often not well approximated by a linear time trend, or $a\hat{w}_t + (1-a)\hat{r}_t$ by a constant, as they are subject to cyclical fluctuations.

Temple also discusses the distinction between the Solow residual and TFP growth. To interpret the Solow residual as a measure of TFP growth requires equality between factor prices and marginal products. What we demonstrate is that the data will always show this, provided one uses the right functional form. This section of Temple's argument also borders on the *petitio principii*. As is well-known, Solow (1957) came to the "startling" result (Solow, 1988, 313) that the growth of factor inputs for the US explained less than one eighth of the growth of labour productivity, while the rate of technical progress (which is how Solow loosely interpreted the residual) explained the remaining seven-eighths. Far from being startling, a back-of-the-envelope calculation with the identity shows

that this result is inevitable. The growth of total factor productivity is defined as $\hat{TFP} \equiv a\hat{w} + (1-a)\hat{r}$. The neoclassical assumptions are the existence of an aggregate production function and that factor shares are equal to the aggregate output elasticities. If factor shares are roughly constant (with a labour share of about 0.75) and the rate of profit does not vary systematically over time, by using value data the growth of TFP will equate to 75% of the rate of growth of productivity.[10] In fact, Solow found the proportion slightly larger than this, because the rate of profit declined over the period under consideration.

Temple agrees that the growth accounting approach requires the existence of a well-behaved production relationship, but argues that a more general approach would be needed where the equality between marginal products and factor products does not hold. As examples, he cites the work of Basu, Fernald and Kimball (2006), Fernald and Neiman (2010) and Temple and Wöβmann, (2006). It is difficult to see any relevance of these articles to the present debate because all commence by explicitly *assuming* an aggregate production function exists and use value data. Fernald and Neiman (2010) actually specify a Cobb-Douglas production function!

3. The Mankiw, Romer and Weil (1992) growth model is merely a misspecified identity.

Further evidence of Temple's (2010, 689-690) misunderstanding on this point is given by the following:

> As in Simon and Levy (1963) and Simon (1979) they [Felipe and McCombie] examine the cross-section implications of the identity and show that it could lead to a (spurious) production relationship. The argument requires factor shares to be constant and the levels of factor prices to be similar across units. Felipe and McCombie relate this to international data [the Mankiw-Romer–Weil model, 1992] even though the assumption that factor prices are similar across countries is highly implausible. This does not strike Felipe and McCombie as a problem: "the critique does not rest on this assumption and so nothing depends upon whether or not it is correct. If the actual data do not have this property, then researchers who estimate the Cobb Douglas form… will not obtain a very good statistical fit". But their argument has veered off course. In these more general and plausible circumstances, a researcher no longer finds that Cobb-Douglas is a good fit.

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[10] Note that in these circumstances $\hat{TFP} = a\hat{w} = a(\hat{V} - \hat{L})$ where $a \approx 0.75$. The growth accounting approach normally calculates TFP growth over several years and given that factor shares are not constant, uses the average value of the shares (based on the Tornqvist approximation to the Divisia index).

> Instead the researcher concludes appropriately that a Cobb-Douglas technology does not provide a good explanation of the data in question. So what is the problem here? The proposed 'tyranny 'of the accounting identity seems part-time at best.

The quotation above shows that Temple misunderstands our arguments concerning the Mankiw *et al*., (1992) paper. Let us restate Mankiw *et al.*'s procedure. They posit a "world" aggregate production function that is a Cobb-Douglas. They see no problems in, say, aggregating Indian agriculture with the plough and oxen, the highly mechanized agricultural sector of the US and Europe, the aerospace industry of Europe and the US, the retailing sector with the hypermarkets in the developed countries and the bazaars of the less developed countries. They, therefore, assume that the "world elasticity of substitution" is a meaningful concept and that all countries have access to the same level of technology. Commonsense (and a cursory acquaintance with aggregation theory) would suggest that this is not a sensible approach.

But nevertheless, Mankiw *et al.,* find that estimating their specification gives, in these circumstances, a reasonably good, but not perfect, statistical fit in terms of the R^2, and the coefficients are statistically significant. (The results using OECD data were poor.) As they use a (neoclassical) Cobb-Douglas production function and assume initially that growth is at its steady-state rate, they implicitly assume a constant capital-output ratio. Later in the paper they introduce a specification purporting to capture the non steady-state growth behavior of the countries.

The question is why do the data give such a reasonably good fit? Is it that the data has not refuted their assumptions underlying the concept of world production function? The answer is no. The reason is that Mankiw *at al.,* use value data, and cannot escape the fact that the series of value added, employment, and capital are related through the accounting identity. Looking at their data set, it is apparent that factor shares are empirically roughly constant. This is an empirical observation or one of Kaldor's stylized facts, not a "maintained hypothesis" as Temple asserts. In Solow's (2000, 2) words, "the ratio of capital to output shows no systematic trend".

What we show is that the initial less than perfect statistical fits of the Mankiw-Romer-Weil model is not because the capital-output ratio or factor shares show considerable variation, but because the wage rate varies considerably between countries. Mankiw *et al*., assume a constant level of technology, A_t, where, from the identity, we know that $A_t \equiv Bw_t^{\,a} r_t^{\,(1-a)}$. We are fully aware that the "assumption that factor prices are similar across countries is highly implausible" as Temple (2010, 689) notes, implicitly criticizing us. Indeed, we discuss this at length in both our critique of Mankiw

et al. (1992) (see Felipe and McCombie, 2005) and in Felipe and McCombie (2010a, 677). As the accounting identity holds separately for each country (both advanced and less developed nations), then we know immediately that the specification of the model of Mankiw *et al.,* with a constant level of technology, will not lead to a particularly good statistical fit. This indeed proves to be the case. As we point out (Felipe and McCombie 2010, 676), the identity shows that the assumption of both a constant technology and a spatially invariant rate of technical progress (i.e., *alnw + (1-a)lnr* and $a\ln\hat{w}+(1-a)\hat{r}$ respectively) by Mankiw *at al.,* will produce a less than perfect statistical fit. If the capital-output ratio did show considerable variation, then the identity shows that Mankiw *et al*.'s specification is likely to give a poor fit to the data, not that we can suddenly be confident that we can find a specification where the data is actually estimating a "true" production function.

Mankiw *et al*., (1992) improve the fit by including a human capital variable derived from school enrollment rates. As this is likely to be correlated with the wage rate, it acts as a proxy for the latter in the identity. Once the variation in factor prices is allowed for by regional dummies or is explicitly included in the regression, the Cobb-Douglas gives a good fit without, in the latter case, the need to include human capital, which Mankiw *et al.,* are forced to resort to (Felipe and McCombie, 2005a). Moreover, the estimated neoclassical speed of convergence becomes infinite. But our argument does not impose *a priori* the assumptions that factor shares are constant, or of a constant capital-output ratio. We know from the data, given these stylized facts hold, that the accounting identity tells us the Mankiw *et al.,* model is bound to give a good statistical fit to the data (subject to the variability of the real wage rate), before a single regression is run. Felipe and McCombie (2005a) confirm this by regression analysis. It is difficult to see any rationale for Temple's *non sequitur* that at this point our discussion "veers off course" (Temple 2010, 690). Empirically, if factor shares did vary considerably and we found another functional form that provides a better approximation to the identity than the Cobb-Douglas, it does not mean, as we have repeatedly emphasised, that we can now be confident that we are estimating an aggregate production function.

The concluding sentences of the above quotation of Temple (2010, p.690) demonstrate a fundamental confusion and it is worth repeating them:

> In these more general and plausible circumstances, a researcher no longer finds that Cobb-Douglas is a good fit. Instead the researcher concludes appropriately that a *Cobb-Douglas technology* does not provide a good explanation of the data in question. So what is the problem here? The proposed 'tyranny 'of the accounting identity seems part-time at least (emphasis added).

This implies Temple considers that if the data provide a good fit to the Cobb-Douglas, the researcher can conclude that a Cobb-Douglas *technology* does provide a good explanation. The

"problem here" is that the whole point of the critique is that the existence of the accounting identity shows that no such inference can be made. The corollary is that if, for example, factor shares vary, we cannot suddenly be confident that an aggregate production function, *pace* Temple, is being estimated.[11]

Temple continues with this line of circular reasoning when he maintains that a constant capital-output ratio "makes little sense in the context of the Solow model. The Solow model can be seen precisely as a theory of adjustment to an equilibrium capital-output ratio. It makes little sense to reject estimates of the model on the basis of a highly restrictive assumption, even less so when that assumption rules out the central mechanism of the model" (Temple 2010, 690). This is again a case of the *petitio principii* fallacy because, as we have noted, the correct measure in Solow's growth model is the *physical* capital-output ratio. Of course, because of the heterogeneity of physical output and capital goods, there is no such thing as an aggregate physical capital-output ratio. See also Simon (1986, 172-183, Appendix A, "A Constant Long-Run *K/Y* Ratio is a Meaningless Observation") for a discussion of why the constant-price monetary value of the capital-output ratio will always tend to be approximately constant regardless of what is happening to the various individual physical capital-output ratios. If the data cannot show whether or not the aggregate production function exists, then the same applies to the whole Solow growth model, upon which it depends. It is not a case of rejecting the estimates of the parameters of the model – we know exactly what the estimates of the model are; they are the factor shares, but they cannot be interpreted as the physical aggregate output elasticities. Again, Temple misunderstands the argument.

To summarise: all Felipe and McCombie (2005a) does is to show, using the identity, the circumstances under which the augmented Solow model (the specification that Mankiw *et al.,* (1992) estimated) will lead to good results. We do not claim that the assumptions about the

[11] Temple (2010, p.687) argues that "if the data have been generated by a translog, a simpler model such as the Cobb-Douglas will be an imperfect fit. This is because the output elasticities and factor shares will not be constant over time or across production units. Even if the researcher lacks information on factor shares, standard methods can be used to detect and investigate parameter heterogeneity that has been left unmodelled. The only way Felipe and McCombie can get around these arguments is to rule them out, by assuming that production relationships never exist, and factor shares behave in particular ways." The point to notice is that Temple again *assumes* that an aggregate production function exists in the form of a translog, which entirely begs the question under discussion. If the shares do show variability then, of course, the Cobb-Douglas relationship (not "production function") will give an imperfect fit and a more flexible functional form (not "production function") is needed.

constancy of the wage and profit rates, the factor shares, the capital-output ratio, are correct theoretically or empirically (although some of them, as we have argued, are stylized facts in the literature). What we argue is that Mankiw *et al.*'s (1992) regression will work *if and only if* these conditions are met as the estimate a Cobb-Douglas relationship. In fact, what we implied in our discussion about the identity was that their poor initial results derived from the fact that all these stylized facts about the data were not met (especially the constancy of the level and growth of "TFP"). If these assumptions about the data are not correct, then the equation Mankiw *et al.,* (1992) estimated, will give a poor statistical fit. This is exactly what originally happened, and our point was that this can be seen without the need for estimating any regression to explain why.[12] The identity also tells us how to improve the goodness of fit.

4. There is no econometric solution to the implications of the critique.

As we are dealing with a misspecified representation of an accounting identity, the solution is *not* to use instrumental variables, or any other econometric technique (as used by, for example, Olley and Pakes, 1996, Blundell and Bond, 2000, and Levinsohn and Petrin, 2003. Temple also cites Ackerberg *et al.,* (2006) as an example of progress towards solving this identification problem, but again they assume the existence of an aggregate production function. The poor statistical results are not due to standard econometric problems (the identity does not have an error term). Poor statistical fits using time-series data are often found even though factor shares are relatively constant. The

[12] It is difficult to understand the implications of Temple's remark (2010, fn. 7, 691) noting the fact that the model of Mankiw *et al.,* can be expressed as a function of the logarithm of the aggregate labour share. He comments "But that quantity has been assumed constant across countries, so their [Felipe and McCombie's] suggested reinterpretation seems internally inconsistent (Temple 2010, fn 7, 691). This result was actually proved by Felipe and McCombie (2005, 375) and discussed there. Felipe and McCombie prove that if the stylized facts mentioned in their paper hold, then the model of Mankiw *et al.* reduces to the logarithm of the aggregate labour factor share. But note that this share does not have to be constant across countries. This theoretical result arises from the fact that the accounting identity is replicating the model of Mankiw *et al.*, which assumes *identical* output elasticities (and, hence, from the identity, identical factor shares). The same result arises for each country if we start from the case where the elasticities and, hence the shares, *differ* between the individual countries, although this will reduce the goodness of fit of the cross-country regressions. It is difficult to see how this is any way internally inconsistent. (The only interpretation we can make is that Temple is implicitly assuming that if factor shares differ for some unspecified reason, the regressions are estimating a "true" aggregate production function.)

problem, as noted earlier, is that $a_t \hat{w}_t + (1 - a_t)\hat{r}_t$ is not constant, but has a pronounced cyclical component. Consequently, its approximation by the constant term in the regression in growth rates (or a linear time trend when log-levels are used) is responsible for the poor results. In these circumstances, it is necessary to find the correct approximation to this equation (e.g., through a different type of time-trend such as a trigonometric function).[13] If factor shares vary greatly then the Cobb-Douglas form will also give a poor statistical fit. In this case, a more flexible functional form for the identity is required.

5. The problem cannot be solved by disaggregating the value measures of capital (and output).

Disaggregation of labor and capital services as in Jorgenson and Griliches (1967) does not solve the problem. Temple argues that with sufficient disaggregation the aggregate production function may (or presumably may not) exist and all that remains is a statistical problem of correctly specifying its functional form. This is also incorrect. As we clearly stated in Felipe and McCombie (2010a, 673-674), disaggregating the relationship between output, labour, and capital as:

$$V_i = f_i(\ L_{1i}\ , L_{2i},\ \ldots L_{mi};\ J_{1i},\ J_{2i} \ldots J_{ni}\ ) \quad (7)$$

does not solve the problem.[14] The production function can only be estimated if the disaggregation is such that physical quantities are used. The problem is not that the inputs are badly measured in value terms, but that they have to be measured in value terms in the first place.

The question is not so much about disaggregation, but the type of data, value versus physical. As we have argued, although not exempt from problems, with data in physical terms it is possible to estimate the technical parameters. Temple argues that "if the inputs have been disaggregated appropriately, then a production function may well exist, and the only remaining problem is a purely statistical one: can the data be used to establish the form of the relationship?" (Temple 2010, 687). Temple argues that, provided we sufficiently disaggregate the constant-price value data of the capital stock and employment, the resulting aggregate production function exists and therefore can

[13] Adjusting the inputs, especially capital, for differences in capacity utilisation will also have the same effect.

[14] Temple's argument is puzzling, as he accepts that our arguments are not about input mis-measurement, but about the dangers of using value added to measure output, and constant price value data to measure the capital stock.

be estimated using value data.[15] He does have the proviso that the correct measure of total factor productivity is required, which he sees as a difficult, but not insuperable problem. This legerdemain occurs in his 2006 paper and he repeats it in his 2010 comment. Thus, he seems to consider that the critique rests on a "fundamental identification problem" (Temple 2010, 685). However, we have long argued that, *pace* Temple, it is not a statistical identification problem if this implies that it is possible, in principle, to specify a model where the aggregate production function can be statistically refuted. Temple (2010, 687), paradoxically also recognizes that "the argument is not simply one about statistical identification."[16]

To reiterate our 2006 argument: the problem is that, to be meaningful, production functions must be estimated using physical quantities. As these are heterogeneous and have to be summed, constant price value data have to be used. Consequently, no matter how many inputs (and outputs) are specified and measured in value terms, the problem posed by the identity still arises. Jorgenson and Griliches (1967, 253) (see Felipe and McCombie, 2006) start out by *assuming* the existence of an aggregate production function, perfect competition, and that factors are paid their marginal products. They use this approach to disaggregate the constant price value indices of the capital stock in order to try empirically to eliminate the residual. Jorgensen and Griliches (1967, footnote 2) explicitly state that because of their assumptions, their approach cannot be used to *test* the marginal productivity theory of factor pricing.

6. *Simulation results confirm the importance of the critique.*

As part of our 2010a reply to Temple we cite a simulation study of ours where we show that with a constant mark-up pricing the data will give a perfect fit to a Cobb-Douglas production function, where the estimated coefficients of the log of capital and labor are 0.75 and 0.25, respectively; while the true output elasticity were 0.25 and 0.75, respectively. We assume the existence of well-defined physical micro-production functions not because we necessarily believe they exist, but to show the implications of the critique even under these circumstances.

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[15] Temple only concentrates on the disaggregation of inputs, although his argument must logically apply to the different outputs, which means that we have to disaggregate the aggregate production function.

[16] However, from a careful reading of the text, it is not clear if he merely correctly attributes this to us, or whether he accepts that it logically follows from our critique.

Temple argues that as the estimated coefficients of the log of capital and labor using value data differ markedly from the true output elasticities, there must be large differences between the rewards to factors and their marginal products. "Those are not the usual assumptions made in interpreting the results from estimated production functions" (Temple 2010, 690). But any researcher with only access to the value data and interpreting the results of the estimated "production function" would find that the estimated "output elasticities" equal the factor shares. Thus, the neoclassical researcher would erroneously conclude that markets are perfectively competitive, constant returns to scale prevail, and that factors are paid their marginal products. This would also be the case, as we show in our simulations, when the true production function displays increasing returns, or, indeed, there is no well-defined relationship between the outputs and inputs. In the simulation model we use (Felipe and McCombie, 2006), prices are determined by a mark-up on unit costs, which in turn is determined by, for example, the state of competition in the industry and the relative power of labor and capital in the wage bargaining process. It may well differ from the physical marginal productivity of labor if the firm (but not the researcher) knows the true micro production function, but so what?[17] Firms, under neoclassical assumptions, will set the rewards equal to the marginal product measured in value terms and are unlikely to know a worker's physical marginal product. (Moreover, there are vast sectors of the economy where there is no reliable independent measure of output even in constant-price value terms.)

There have been a number of other important simulation studies which demonstrate how the data will give a good fit to a Cobb-Douglas, even though we know by the construction of the hypothetical data this is not reflected in the underlying technology. These include a study where the micro-production functions deliberately violate the conditions for successful aggregation (Fisher, 1971); where the production function has a fixed-coefficients technology (Shaikh, 2005); and where firms satisfice, rather than optimize (Nelson and Winter, 1982). For a discussion of these studies, see Felipe and McCombie (2010b).

[17] Temple argues that some argue that "no firm knows its production function" but he considers it knows its costs and that well-behaved cost functions are mirrored by the existence of production functions. But a cost function is also derived from the accounting identity and will be mirrored by a "spurious" production function. (See Felipe and McCombie 2011-12). A neoclassical cost function does not guarantee the existence of a well-behaved production function.

Conclusions

The Cambridge capital theory controversies and the related aggregation problems have had no bearing on the use of the aggregate production function, which continues to be widely and uncritically used. We suggest that the answer to this conundrum is the instrumental justification that in practice it works. However, the fact that very simple functional forms and two highly aggregate variables (with the constant price value of the capital stock in particular subject to all kinds of statistical measurement errors) can often explain over 90 percent of the variation in output is due simply to the fact that the three variables are definitionally related. This explanation does not depend upon any specific assumptions such as constant factor shares, a constant weighted log-level (or growth) of the average of the wage and profit rates, or a constant capital-output ratio. Allowing these to vary does not mean that all the aggregation problems and the problems posed by the accounting identity disappear, and that we can be confident of estimating a technological relationship.

The key disagreement between Temple and us is that we argue using value data that a researcher can always find a perfect fit to the data, with the estimated coefficients equal to the factor shares (and not only when these are constant), even though no aggregate production function exists. Temple does not share this conclusion. We have shown that the only reason why factor shares and the output elasticities may differ is that the specific functional form estimated does not accurately track the accounting identity. Temple unwittingly concedes our case when he states: "Moreover, the production function *may* appear simple and well-behaved even when no *stable relationship* exists and the true extent of the misspecification may never be detected" (Temple 2010, 689; italics added). This accurately summarises our position, although we argue it is not a case of 'may', but of 'will'; and furthermore, that the statement 'no stable relationship' includes the case when plausibly the aggregate production function does not exist. An implication of the above quotation is that the researcher can never know whether or not the estimates of the aggregate production function mean anything. As we noted above, Temple correctly states, "the argument is not simply one of statistical identification" (Temple 2010, 687), but then inconsistently and erroneously states that "to the extent that a researcher can control for the variation in TFP and takes care over the specification, the simultaneous existence of the value added identity does not invalidate these methods". Unfortunately for the researcher it does and Temple has not demonstrated otherwise.

Anwar Shaikh's (1974) important conclusions about the accounting identity critique have stood the test of time.

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