International Review of Applied Economics
Vol. 24, No. 6, November 2010, 665–684

Routledge
Taylor & Francis Group

What is wrong with aggregate production functions. On Temple's 'aggregate production functions and growth economics'

Jesus Felipe[a,b] and J.S.L. McCombie[b,*]

[a]*Centre of Full Employment and Equity, University of Newcastle, Australia;* [b]*Cambridge Centre for Economic and Public Policy, University of Cambridge, UK*

In an article in the 2006 volume of this journal, Jonathan Temple presented a defence of the use of the aggregate production function in growth theory in the light of various criticisms that have been levelled at it. These criticisms include the Cambridge Capital Theory Controversies, various aggregation problems, and the problems posed by the use of value data and the underlying accounting identity. We show that Temple has underestimated the seriousness of these criticisms, especially the last one, which vitiates the concept of the aggregate production function. Because of the identity, estimates of putative aggregate production functions, such as the aggregate elasticity of substitution, cannot be interpreted as reflecting the underlying technology, and hence the use of the aggregate production function is extremely problematical.

Keywords: aggregate production function; growth econometrics; aggregation problems; accounting identity

JEL Classifications: O11; O16; O47; O53

Introduction

Temple (2006) presented in this *Review* an assessment of what has been termed the accounting identity critique of aggregate production functions. This criticism shows that the estimation of production functions using constant-price monetary data (at any level of aggregation) can shed no light on the underlying technological parameters (e.g., the elasticity of substitution), or indeed whether or not the production function actually exists. The estimation of aggregate production functions, using country or sector-level statistics, must necessarily use value data, as opposed to true physical quantities. Disaggregation does not solve the problem if value data, as opposed to physical quantities, still have to be used. This is an almost universal requirement considering the wide variety of capital goods and structures used in the production process.[1]

Temple's contribution is important on two counts. First, he is one of the few economists working in growth theory and with production functions who seems to be aware of the critique. Indeed, he has some sympathy for the implications of the argument. 'Overall the critique has some force. It deserves to be more widely known among researchers estimating production relationships using time series or panel data, including researchers who never doubted the existence of a well-behaved underlying relationship' (Temple 2006, 307).

*Corresponding author. Email: jslm2@cam.ac.uk

ISSN 0269-2171 print/ISSN 1465-3486 online

DOI: 10.1080/02692171.2010.512146
http://www.informaworld.com

Nevertheless, secondly, he seems to misunderstand certain aspects of the critique, as he believes that it relies on 'restrictive assumptions', especially that it requires constant factor shares. He consequently underestimates the full extent of the damaging implications for the aggregate production function. There seems to be agreement between us that the heart of the critique is whether a researcher using value data can ever establish whether the estimated relationship has any behavioural implications or is merely spurious. The latter would be the case if the estimates were merely driven by the underlying accounting identity and, in our view, this is the correct position. The accounting identity critique is one of logic and has nothing to do with whether or not one is willing to work with parables or approximations. It shows unambiguously that any estimates will indeed be spurious in the sense mentioned above. But Temple is more equivocal. He comments that:

> The most common motivation for estimating these [production function] relationships is to obtain information about technology or TFP [total factor productivity]; but without that information to start with, and hence without the possibility of controlling for TFP, any effort at estimation is on dangerous ground. There is a sense in which the estimation of production technologies is required on precisely the occasions when it is guaranteed not to work. *That is perhaps a little too strong and some estimation methods that can accommodate unobserved differences in technology will be discussed later.* (Temple 2006, 307, emphasis added)

This is where we part company with Temple. The inference from the above quotation is that he believes that there are certain circumstances where, in principle, one can estimate an aggregate production function albeit with problems of omitted variable bias, especially from the omission of TFP.

We contend that Temple's arguments fall far short of providing a compelling justification and defence of the concept of the aggregate production function. Our conclusion is that neoclassical growth theory is reminiscent of the Ptolemaic geocentric theory of the explanation of the motions of the planets, the predictive accuracy of which allowed a completely erroneous theory to persist for 2000 years. This was despite that the correct geocentric theory had been articulated convincingly by Aristarchus, circa 250 BC. The major difference, though, is that while the Ptolemaic theory could be (and was eventually) empirically refuted, the aggregate production function cannot be.

The rest of the paper elaborates briefly on the key issues upon which Temple (2006) bases his case for the (partial) defence of the aggregate production function. Some of our work on these issues, where the arguments are discussed more fully, is listed in the references. First, Temple argues that growth models are 'parables' that can be useful even when their assumptions are unrealistic and cannot be formally justified. We disagree with this methodological position for it negates the ramifications of the Cambridge Capital Theory Controversies and the aggregation problem. Second, we explain briefly the accounting identity critique and clarify what we see as Temple's misunderstandings. Third, we consider explicitly the growth accounting approach and show how disaggregation does not rescue the aggregate production function. Fourth, we show that Temple's claim that the argument does not apply to cross-sectional work cannot be substantiated. In fact, ironically, this is how the critique was originally presented by Phelps Brown (1957) and Simon and Levy (1963). Fifth, we discuss the usefulness of growth econometrics without production functions. We close the paper with a discussion of Lucas (1990), which Temple cites as an example of a valuable use of the aggregate production function.

Problems with aggregate production functions

The aggregate production function is undoubtedly one of the most controversial concepts in economics. The literature on aggregation and the Cambridge Capital Theory Controversies showed formally that aggregate production functions do not exist, in the sense that their theoretical derivation requires assumptions that are very difficult to justify. The conditions for successful aggregation are so stringent that it is very unlikely that aggregate production functions exist, even as approximations (Fisher 1969, 571). Consequently, confidence in the aggregate production function even as a 'parable' (Samuelson, 1961-1962; Solow, 1966) is misplaced (Garegnani, 1966; Samuelson, 1966). See also Felipe and Fisher (2003) and Fisher (1992, 2005) on the aggregation problem. This makes theoretical and applied work that requires an aggregate production function questionable endeavours, no matter how entrenched this concept is within the economics profession.

In this context, Temple (2006, 301) asks the extremely pertinent question 'Is there anything more to be said?' While in the 1970s the reservations about the existence of the aggregate production function were widely discussed, even at the textbook level (see, for example, Wan 1971; Jones 1975; and Hacche 1979), the debates have subsequently been either ignored or forgotten (e.g. Jones, 1998; Barro and Sala-i-Martin, 1998; Weil, 2005).[2] This is despite the fact that no convincing counter-argument or rebuttal of the UK side of the Cambridge Capital Theory Controversies, much less of the damaging implications of the aggregation literature, has ever been advanced.

Nevertheless, Temple (2006, 303) argues that models using aggregate production functions do tell us something: 'the very real possibility of error is not enough to make silence the best research strategy'. Notwithstanding our comments above, according to Temple, any model is an abstraction from reality and the aggregate production function is best regarded as a Samuelson/Solow parable. So long as the researcher knows what the key assumptions are and how sensitive they are to modifications, the simple, at least conceptually if not mathematically, neoclassical growth models do tell us something important that we did not know before. Temple's defence of the use of theoretical models relies implicitly on Friedman's (1953) instrumentalist methodology. By definition, all models must have simplifying assumptions, but, according to Friedman, the realism of these assumptions is irrelevant – what matters is the predictive ability of the model. By the symmetry thesis, prediction is taken to be equivalent to explanation. While Temple does not explicitly invoke the necessity of a model to stand up to empirical scrutiny, this must be an important element of his defence; otherwise how is one to judge the degree to which the model is capturing any aspect of reality?

This position is reminiscent of the Quine-Duhem thesis, which suggests that empirical refutation may not be sufficient to reject a theory. This is because we can never be certain whether we are refuting the hypotheses or the auxiliary statements. The Quine-Duhem thesis is also central to the role of anomalies in both Kuhn's (1962) notion of the paradigm and Lakatos's (1970) *Methodology of the Scientific Research Programme*. However, Temple does not explicitly draw on any methodological literature and thus his argument is best considered as the viewpoint of a practitioner.

However, as we shall show, the problem is that this defence is not open to proponents of the use of the aggregate production function. Because of the underlying accounting identity, we can always find a functional form where the data will give a perfect fit to a production function, even though it does not exist. Thus, the problem is not one of erroneously refuting the model, but the complete opposite – it is one of always being able to specify a particular 'production function' that will give a statistically significant (indeed perfect) fit to the data. McCombie (1998a) provides a methodological discussion of why the aggregate production function is still so widely used today, notwithstanding the various criticisms that have been levelled at it.

Temple considers both the aggregation and a separate problem, which we have termed the 'accounting identity' (and Temple calls the 'economic') critique. See Simon and Levy (1963); Simon (1979a,b); Shaikh (1974, 1980); McCombie (1987); Felipe and McCombie (2001, 2003, 2005a,b, 2006) for discussions of the problems raised by the accounting identity. The critique may be traced back in rudimentary form to Reder (1943), Marshak and Andrews (1944), and Phelps Brown (1957). Temple terms the standard econometric problems, including specification issues, as the 'statistical' critique, which if our arguments are accepted becomes largely irrelevant.

Ideally, a production function should be estimated using physical data. It is a technological relationship and its parameters, such as the elasticity of substitution, are derived from the form of the physical production process. Nobody would deny that a 'production function' in this sense exists. After all, engineers designing, say, an oil refinery need to know, and do have a good idea of, the relationship between volume of crude oil used as an input and the volume of output of refined oil. They will also have to determine the likely labour requirements, both skilled and unskilled, for the operation of the plant. Needless to say, the design plans of an oil refinery and the relationships between the various types of capital equipment in the draughtsman's plans are far more complex than the relatively simple relationship given by, say, the translog function. There have been a few studies that estimate production functions using physical data, but such 'engineering production function' studies are few and far between. Even with the use of physical data, it may be difficult to get robust estimates of the various parameters of engineering production functions. There may be substantial differences between firms in managerial efficiency, the commitment of workers and flows of effective labour time and capital services, etc. See Wibe (1984) for a survey.

However, when aggregate production functions are estimated, constant price value (monetary) data have to be used because prices are needed to aggregate the individual (heterogeneous) outputs and capital goods. This is the core of the problem. This has nothing to do with the 'aggregation problem' as commonly understood, except in the very limited sense that prices have to be used to sum different physical commodities and machinery and structures. It is important to note that the deflation of monetary or value data in nominal terms to give constant-price series does not lead to data in physical terms, even though they are commonly referred to as 'quantities.'

The use of value data implies that output (gross output or value added) is related to the inputs through an underlying accounting identity. This precludes the possibility of statistically refuting the basic assumptions underlying a neoclassical aggregate production function or of interpreting any of its estimated coefficients as technological parameters. Temple, as noted above, not only considers that there are some circumstances where production functions can be estimated, but also that growth econometrics, at least as usually carried out, does not necessarily rely on aggregate production functions. Consequently, for him, the 'economic' critique does not necessarily pose a serious impediment to macroeconomic growth analysis.

We take a more nihilistic or, in Temple's words, 'extreme' view. While we know that the aggregate production function is a concept that lacks sound theoretical foundations, the accounting identity critique rules out ab initio the possibility of refuting the hypothesis, using the standard econometric methods, that a functional form identical to a neoclassical production function (derived as an algebraic transformation of the accounting identity) represents the underlying technological relationships. Thus, the interpretation of the estimates of all putative aggregate production functions as if they were those of a technological relationship can lead to very misleading conclusions. The implication is that it is not possible to estimate such technological parameters as the aggregate elasticity of substitution, which does not exist. We shall also show that the fact that it is possible, supposedly, to test the neoclassical growth models without estimating the production function directly does not mean that the critique has no bearing. We next outline the problem that the accounting identity poses for the aggregate production function.

On accounting identities and production functions

The answer as to why remarkably good fits are obtained by the estimation of the Cobb-Douglas production function and other more flexible functional forms such as the CES and the translog, is ultimately due to the fact that because of the heterogeneity of output and capital, these variables have to be expressed in terms of constant price values in applied work when they are aggregated. The existence of an underlying accounting identity, where value added is simply the sum of the total wage bill and the gross operating surplus (total profits), means that it is always possible to get, with a little ingenuity, a good statistical fit to a form that resembles an aggregate production function.

For any firm, industry, or economy, value added is defined by the identity:[3]

$$Y_t \equiv W_t + \Pi_t \tag{1}$$

where Y is value added, W is the labour's total compensation and Π is the gross operating surplus or total profits. The identity may be equivalently written as

$$Y_t \equiv w_t L_t + r_t K_t \tag{2}$$

where w is the average wage rate, L is the labour input, r is the average ex post rate of profit and K is the constant price value of the capital stock.[4] Equation (2) holds for any economy, regardless of the state of competition, the degree of returns to scale and whether or not a well-defined production function actually exists.

Equation (2) may be expressed in growth rates as:

$$\hat{Y}_t \equiv a_t \hat{w}_t + (1-a_t)\hat{r}_t + a_t \hat{L}_t + (1-a_t)\hat{K}_t \tag{3}$$

where the symbol $\wedge$ denotes the growth rate of the corresponding variable and a_t and $(1-a_t)$ are the shares of labour and capital in total output, respectively. The shares have t subscripts as they can change over time.

If we express the general form of a supposed production function $Y_t = F(A_t, L_t, K_t)$ where A is the level of technology, in growth rates, we obtain:

$$\hat{Y}_t = \hat{A}_t + \alpha_t \hat{L}_t + \beta_t \hat{K}_t \tag{4}$$

where α and β are the respective output elasticities. If the usual neoclassical assumptions are made (perfect competition and constant returns to scale) and factors are paid their marginal products, then equation (4) can be expressed as:

$$\hat{Y}_t = \hat{A}_t + a_t\hat{L}_t + (1 - a_t)\hat{K}_t \tag{5}$$

This is formally equivalent to equation (3) in the sense that estimation of a functional form underlying equation (5) faces the problem that the data used must also satisfy the identity (3). This further implies that $\hat{Y} - a_t\hat{L}_t - (1-a_t)\hat{K}_t \equiv \hat{A}_t \equiv a_t\hat{w}_t + (1-a_t)\hat{r}_t$. Estimations of production functions specify some specific functional form such as the Cobb-Douglas, CES or translog that will give a good fit to the data given by equation (5). The fact that the estimated output elasticities are often close to the relevant factor shares has been interpreted as confirming, or – strictly speaking – not refuting, the underlying neoclassical assumptions (Douglas 1976). But all that such estimations are, in fact, accomplishing is to estimate a mathematical transformation, with no economic content, of the identity given by equations (2) and (3). It should be noted that almost invariably $\hat{A}_t$ is assumed to be a constant, say, λ. In other words, 'technical change' is assumed to occur at a constant rate (this is discussed further below).

Analysing the economic implications of the critique, Temple (2006, 306) asks: 'if we assume there is no production function, or perhaps a relationship of complex and unknown form, what happens when we estimate a production function from time series data?' In other words, suppose there is no aggregate production function and the researcher gathers aggregate data on output, employment and capital stock and estimates a form such as the Cobb-Douglas relationship, $Y_t = A_0e^{\lambda t}L_t^{\alpha}K_t^{\beta}$, or a more general specification of $Y_t = F(L_t, K_t, t)$. Is there any alternative interpretation of the estimates, no matter how well determined, that does not rely on the existence of the aggregate production function? The question can be posed in a different way: can a researcher using value data ever establish whether or not the estimated coefficients reflect a production function, or are they simply predetermined by the value added accounting identity? Our answer is that unequivocally the results are always determined by the identity.

To see this, if we integrate equation (3) with respect to time, assuming the stylised facts that both $\lambda_t = a_t\hat{w}_t + (1-a_t)\hat{r}_t$ and factor shares are constant, assumptions that do not depend upon the existence of an aggregate production function yield:[5]

$$Y_t \equiv B_0e^{\lambda t}L_t^{a}K_t^{(1-a)} \tag{6}$$

Equation (6) is indistinguishable from the Cobb-Douglas production function with a linear time trend when all the neoclassical conditions necessary for the output elasticities to equal the factor shares hold. Equation (6) is, however, merely the accounting identity, equations (1) or (2), rewritten solely under the assumption that factor shares and λ are constant. This is in spite of the fact that no assumptions have been made about diminishing returns to each factor, the state of competition or the degree of returns to scale.[6]

McCombie (1998b) showed, using simulated data, that estimating a Cobb-Douglas 'production function' where labour's shares for each year were a random draw from a normal distribution with mean 0.75 and a standard error of 0.02 (labour's share varied between 0.80 and 0.72) gave what would generally be accepted as a very good statistical fit. Using Monte Carlo simulations, Felipe and Holz (2001) also showed that significant variations in factor shares still lead to results that most researchers would accept as good. What this simply means is that, empirically and for the data sets that most researchers use, the variation in factors shares is not large enough to make the Cobb-Douglas form yield poor results.[7] When a poor statistical result is obtained it is almost invariably because a linear time trend does not adequately proxy ln A (the weighted average of the logarithms of the wage and profit rates), which empirically has a strong cyclical component.

For the identity to give a good fit to the Cobb-Douglas form, all that is required is that factor shares are reasonably constant compared with the variation of the other data. One reason for this relative stability, although there may be others, is that, generally, firms pursue a constant mark-up pricing policy on unit total costs, for which there is a great deal of empirical evidence (Lee 1998). As Solow (1958) first remarked, the degree of stability of factor shares is likely to increase as shares are summed over industries. Thus, ironically, as we do this, the goodness of fit of the Cobb-Douglas 'production function' is likely to increase, contrary to what aggregation theory would suggest if we were aggregating micro-production functions.

Temple (2006, 306–7, omitting footnotes) goes on to argue as follows:

> Some interpretations of this result become overenthusiastic and suggest that a Cobb-Douglas technology will always fit the data well, simply because of an identity. This should make us pause: for example, if the underlying technology were translog, could we really expect a Cobb-Douglas to fit the data well? Given sufficient variation in the input ratios, movements in factor shares would immediately reveal that Cobb-Douglas is not the right specification. The argument that Cobb-Douglas results are spurious uses not only the value added identity, but also some additional structure: namely constant factor shares and the constancy of the weighted average of the wage and profit rate growth rates.
>
> The need for this extra structure points to the heart of the problem in estimating production relationships. Estimation must usually treat the level or growth rate of technology (TFP) as unobservable and it is this omitted variable that poses the fundamental difficulty. If the data were generated by a translog, and the researcher had identified a good proxy for TFP,[8] a suitably specified regression would accurately recover the parameters of that translog production function, and reject the Cobb-Douglas specification given sufficient variation in the data. It is the inability to control for the TFP term that causes problems and this means that the 'statistical' and 'economic' critiques are closer together than is usually acknowledged.

Temple is arguing here that the problem of the accounting identity only occurs if the two stylised facts (i.e. constant factor shares and constancy over time of the weighted average of the growth rates of the wage and profit rates) hold. If there is sufficient variation in the shares and in the inputs, then Temple claims that in principle the underlying aggregate production function can be identified, which must exist for his argument to hold. This is erroneous and misunderstands the problem and two comments are in order here.

The first is that if, in principle, the data could identify an aggregate production function when the shares vary, then they must similarly identify it when they are constant. It is not clear why the accounting identity precludes estimating the true production function when shares are constant, but not when they vary. In practice, we need some variation in the factor shares (or rather the supposed output elasticities) to estimate the aggregate Cobb-Douglas production function, otherwise there will be perfect multicollinearity (Felipe and Holz 2001). Consequently, how great must the variation in the shares be before we can be confident that we are estimating the true aggregate production function that he mentions? Of course, more fundamentally, the answer is that this is not a sensible question. The argument concerning the accounting identity holds whether factor shares vary or not. The argument applies to any specification of the production function. See, for example, Felipe and McCombie (2001) for the case of the CES, and Felipe and McCombie (2003) for that of the translog. All these more flexible functional forms do is to provide better approximations to the underlying identity given by equation (1) (see McCombie 2000). More generally, compare equations (3) and (5). This shows the equivalence between the accounting identity and the general form of the production function.

The second point is that it is notable that, in especially the second paragraph of the passage from Temple cited above, the existence of the aggregate production function is taken for granted (the aggregation problems are simply assumed away) and is a maintained hypothesis. We argue that the failure to see the importance of the critique rests on the failure to appreciate that it is not possible to statistically refute any supposed aggregate production function.

Temple argues that the most common motivation for estimating a production function is to obtain information about the technology or TFP. This misses one of the points of the accounting identity critique. As the identity shows, what neoclassical economics refers to as the rate of TFP in growth accounting studies is always just a weighted average of the growth rates of the wage and profit rates. There are three important things to note.

First, this cannot necessarily be interpreted as the rate of technical progress because it is derived solely from an accounting identity. In our contribution (Felipe and McCombie 2006) to the conference volume where Temple's paper was published, we undertook a series of simple simulation experiments that illustrated the problems posed by the accounting identity. In particular, we showed that the rate of TFP growth calculated using value data was very different from the 'true' rate (i.e. using physical quantities). The hypothetical output elasticities for labour and capital in the underlying physical micro-production functions were assumed to be 0.25 and 0.75; i.e., they were deliberately chosen to be different from the values of the factor shares, 0.75 and 0.25, respectively. If the data are aggregated where the price is determined by a mark-up of 1.333 on unit labour costs, and an aggregate Cobb-Douglas production function is estimated, the estimates of the output elasticities turn out to be equal to the factor shares, different from the true values. Therefore, if factor shares are used in a growth accounting exercise, they give a very misleading picture of the rate of TFP growth.

Secondly, estimates of TFP growth derived from estimating putative aggregate production functions are simply capturing the weighted growth of the wage and profit rates. Using a linear time trend gives an approximation to this weighted average. However, in most cases this is a misspecification as the weighted growth of the real wage rate and the rate of profit are not exactly constant over time, but show some cyclical variation.

Thirdly, Temple concludes that what he terms the 'statistical' critique and the 'economic' or accounting identity critique (the identity) are close. While these two critiques share some elements, they are not, however, the same thing. One implication of the 'economic' argument is that this is not an econometric problem, i.e. it is not about how to identify a good proxy for TFP (given the difficulty to control for it). The problem is neither one of finding appropriate econometric instruments to estimate the production function. The basis of the 'statistical' critique is that this is an econometric problem that has a solution. The 'economic' critique, however, says that this is not an econometric problem and that it does not have any solution. It is not an identification problem between two separate equations, one the identity (and, thus, not a behavioural relationship) and the other the production function, as Bronfenbrenner (1971), for example, seemed to think. There is no way that the supposed aggregate production function can be identified as distinct from the identity. Temple refers to work by Olley and Pakes (1996) and Levinsohn and Petrin (2003). These papers claim to offer solutions to the problem of estimating production functions when technical efficiency is unobserved. He also cites Griliches and Mairesse (1998) who provide an accessible summary of the problems inherent in estimating aggregate production functions. However, these first two studies are irrelevant as they are based upon the assumption that the aggregate production exists and all that is needed is to estimate it correctly by the appropriate estimating technique. We have discussed this in Felipe, McCombie, and Hasan (2008). Likewise, the issues raised by Griliches and Mairesse (1998) have no bearing upon the problem.

On growth accounting and disaggregation

Temple (2006, 306) notes that equation (3) is simply an illustration of the 'dual' growth accounting results, namely that TFP growth can be calculated either from quantities (the primal) or from factor prices (the dual). The rationale for the dual interpretation is established from the cost function.[9] This is correct, but the explanation is incomplete. While neoclassical economists are aware of the accounting identity (3), the interpretation of primal and dual estimates of TFP growth takes place in the context of neoclassical production function theory and the usual neoclassical assumptions (see, for example, Jorgenson and Griliches 1967). This means that the neoclassical interpretation of $a_t\hat{w}_t + (1-a_t)\hat{r}_t$ as measuring technical change (or, more generally, the growth of TFP) does require the assumptions of constant returns, perfect competition and that factors are paid their marginal products. Temple does not emphasise perhaps the most important assumption of the growth accounting approach, namely, that an aggregate production function must also exist. In the discussion of the relationship between the aggregate production function and the dual, Temple implicitly makes use of Euler's theorem and the usual neoclassical assumptions.

The problem is that if the aggregate production function does not exist, then the only possible interpretation of the accounting identity is that it is just an identity, and thus there is no way that any element of it can be necessarily interpreted in terms of technical progress. The expressions $a_t\hat{L}_t$ and $(1-a_t)\hat{K}_t$ do not measure the contribution of the growth of labour and capital to the growth of output in any causal sense. The neoclassical argument about duality in Jorgenson and Griliches (1967), and cited by Temple, does start by explicitly considering the accounting identity (Jorgenson and Griliches 1967, 251). But the argument depends on the assumption that an aggregate production function exists, there is perfect competition and factors are paid their marginal products (Jorgenson and Griliches 1967, 253). None of these is required to derive equation (3).

Related to this is Temple's (2006, 308) comment that 'if aggregation is not possible, the obvious solution must be to disaggregate'. He continues: 'In the case of growth accounting, there is nothing to stop the researcher writing down $Y = F(K_1, K_2, \ldots K_M, L_1, L_2 \ldots\ldots, L_N)$ where there are M different types of capital input and N different types of labour input.' He points out that production functions and growth theory do not, in principle, need aggregation. 'Instead, it is lack of data that will typically restrict the applied researcher to simpler methods'. Unfortunately, this confuses the aggregation and the accounting identity problem. First, if the researcher has physical data for output and all the different types of inputs, individual capital goods and structures, then it might be possible to estimate a production function. But as soon as it is necessary to use different types of output and capital measured using constant price data because of heterogeneity, then the identity is simply written as

$$Y \equiv w_1 L_1 + w_2 L_2 + \cdots w_N L_N + r_1 K_1 + r_2 K_2 + \cdots r_M K_M \qquad (7)$$

where Y and K's are constant-price value data. The accounting identity argument follows through, even though there are several categories of labour and capital. The use of two-sector production functions models to disaggregate the economy into agricultural and non-agricultural sectors (see Temple 2006, 309) does not escape the critique.

Aggregation poses a problem, not for the reasons that Fisher (1992) set out (important though these are), but because suitable physical data are not normally available to the researcher, who then has to resort to value data. It should be noted that this is true even for industries at the level of the three and four-digit SIC. Disaggregating by industry rather than by input does not prevent the problem.

Cross-section production functions

Ironically, Temple claims that it is much harder to apply the 'humbug' argument in the context of cross-sections' (Temple 2006, 307). It is ironic because the whole argument can be traced back to Phelps-Brown's (1957) criticism of the myriad of cross-section regressions by Douglas and his associates in the 1930s.[10] His argument was a little obscure but was subsequently formalised by Simon and Levy (1963) and Simon (1979a) (who also showed that the criticism applied to the CES function).

The identity is given using cross-section data as $Y_i = w_i L_i + r_i K_i$. Labour's share is defined as $a_i = (w_i L_i / Y_i)$; and similarly capital's share as $(1-a_i) = (r_i K_i / Y_i)$ (where i denotes the units of the cross section). Moreover, we define the baseline shares as $\bar{a} \equiv (\bar{w}\bar{L}/\bar{Y})$ and $(1-\bar{a}) \equiv (\bar{r}\bar{K}/\bar{y})$, where a bar over w, L, Y, r and K denotes the average value of the variable. Then the following also holds:

$$\frac{a_i}{\bar{a}} \equiv \frac{(w_i/\bar{w})(L_i/\bar{L})}{Y_i/\bar{Y}} \qquad (8)$$

and a similar expression follows for the capital share:

$$\frac{1-a_i}{1-\bar{a}} \equiv \frac{(r_i/\bar{r})(K_i/\bar{K})}{Y_i/\bar{Y}} \qquad (9)$$

For small deviations of a variable X_i from the reference point or baseline, , the approximation $\ln(X_i/\bar{X}) \cong (X_i/\bar{X}) - 1$ holds. Thus, taking logs of equation (8) and using this approximation, we can write:

$$\ln\frac{w_i}{\overline{w}}+\ln\frac{L_i}{\overline{L}}-\ln\frac{Y_i}{\overline{Y}}\cong\frac{a_i}{\overline{a}}-1 \tag{10}$$

and

$$a_i\cong\overline{a}\ln\frac{w_i}{\overline{w}}+\overline{a}\ln\frac{L_i}{\overline{L}}-\overline{a}\ln\frac{Y_i}{\overline{Y}}+\overline{a} \tag{11}$$

Similarly, equation (9) can be written as

$$\ln\frac{r_i}{\overline{r}}+\ln\frac{K_i}{\overline{K}}-\ln\frac{Y_i}{\overline{Y}}\cong\frac{1-a_i}{1-\overline{a}}-1 \tag{12}$$

and

$$(1-a_i)\cong(1-\overline{a})\ln\frac{r_i}{\overline{r}}+(1-\overline{a})\ln\frac{K_i}{\overline{K}}-(1-\overline{a})\ln\frac{Y_i}{\overline{Y}}+(1-\overline{a}) \tag{13}$$

We know from the accounting identity that $a_i + (1 - a_i) \equiv 1$. Substituting equations (11) and (13) into this equation gives:

$$\ln Y_i\cong\ln B+\overline{a}\ln w_i+(1-\overline{a})\ln r_i+\overline{a}\ln L_i+(1-\overline{a})\ln K_i \tag{14}$$

where $\ln B\equiv\ln\overline{Y}-\overline{a}\ln\overline{w}-(1-\overline{a})\ln\overline{r}-\overline{a}\ln\overline{L}-(1-\overline{a})\ln\overline{K}\equiv-\overline{a}\ln\overline{a}-(1-\overline{a})\ln(1-\overline{a})$ and

$$\ln Y_i\cong\ln A_i+\overline{a}\ln L_i+(1-\overline{a})\ln K_i \tag{15}$$

where $\ln A_i \cong \ln B + \bar{a}\ln w_i + (1-\bar{a})\ln r_i$

Equation (15) resembles a Cobb-Douglas production function in logarithms, but has been derived from the accounting identity and the definition of factor shares.[11] This derivation indicates that for a cross-section that displays small variation in the factor shares and in $\ln w$ and $\ln r$, estimation of the Cobb-Douglas form will yield very good results ($\ln A_i$ becomes a constant). But, as in the case of time-series data discussed earlier, the critique does not rest on this assumption and so nothing depends on whether or not it is correct. If the actual data do not have this property, then researchers who estimate the Cobb-Douglas form under the impression that they are estimating a production function will not obtain a very good statistical fit.

In Felipe and McCombie (2005b), we applied this critique to the well-known cross-sectional study of the augmented Solow growth model of Mankiw, Romer, and Weil (MRW) (1992). There are substantial differences in the wage rate between the developed and less developed countries that comprise the full sample. Consequently, the statistical fit should not be perfect, as indeed proves to be the case. It is difficult to understand Temple's rationale when he argues that in this case 'the Felipe and McCombie argument has to proceed under restrictive assumptions' (Temple 2006, 307). It is, therefore, useful to sketch our criticism of MRW.

Solely from the accounting identity, the definition of the growth of the capital stock as $\frac{\Delta K_i}{K_i} \equiv \hat{K}_i \equiv \frac{I_i}{K_i} - \delta_i \equiv \frac{s_i Y_i}{K_i} - \delta_i$, and the two 'stylised facts' that factor shares are constant and there is no growth in the capital-output ratio, i.e. $\hat{Y}_i = \hat{K}_i$ (in Temple's view, our 'restrictive assumptions'), it is possible to derive an equation for labour productivity (see Felipe and McCombie 2005b) as:

$$\ln\left(\frac{Y_i}{L_i}\right) \equiv \frac{1}{a}\ln B_0 + 1.0\ \ln w_i + \frac{1-a}{a}\ln r_i + \frac{1-a}{a}\ln s_i - \frac{1-a}{a}\ln\left(n_i + \delta_i + \frac{a\hat{w}_i + (1-a)\hat{r}_i}{a}\right) \quad (16)$$

where s is the share of investment in output, n is the growth of population (strictly speaking employment) and δ is the rate of depreciation. It is assumed that equation (16) refers to cross-section data where the growth rates are calculated over a single period. It is straightforward to generalise it to more than one period (Islam 1995).

Equation (16) may be expressed as:

$$\ln\left(\frac{Y_i}{L_i}\right) \equiv \frac{1}{a}\ln A_{0i} + \left(\hat{w}_i + \frac{1-a}{a}\hat{r}_i\right)t + \frac{1-a}{a}\ln s_i - \frac{1-a}{a}\ln\left(n_i + \delta_i + \frac{a\hat{w}_i + (1-a)\hat{r}_i}{a}\right) \quad (17)$$

As the cross-country growth rates are calculated over one period, time t is set equal to unity. In equation (17), $A_{0i} = B_0 w_{0i}^{a} r_{0i}^{(1-a)}$ and therefore varies across countries. The goodness of fit of equations (16) and (17) depends only on the degree of constancy of the factor shares.

Using a Cobb-Douglas aggregate production function with labour-augmenting technical change, $Y_i = (A(t)L_i)^{\alpha} K_i^{(1-a)}$, MRW showed that the logarithm of the level of productivity can be expressed as:

$$\ln\left(\frac{Y_i}{L_i}\right) = \ln\tilde{A}_0 + \tilde{g}t + \frac{(1-\alpha)}{\alpha}\ln s_i - \frac{(1-\alpha)}{\alpha}\ln(n_i + \tilde{\delta} + \tilde{g}) \quad (18)$$

A tilde over a variable serves to emphasise that MRW assume it is identical for all countries. It is immediately apparent that equation (18) resembles the identity given by equations (16) and, especially, (17). However, $\ln A$ and g are interpreted by MRW as the level of technology and the rate of technical progress, respectively. (In equation (18) t again equals unity.) The exception is that both the level and the growth rates of the factor prices (i.e. $\ln A_0$ and g) are implicitly assumed by MRW to be constant across countries in equation (18). Note that, under neoclassical assumptions and using the aggregate marginal productivity theory of factor pricing, the following two equations hold: $g_i = \frac{1}{a}(a\hat{w}_i + (1-a)\hat{r}_i)$ and $\ln A_i(t) = \frac{1}{a}\ln B_0 + \frac{1}{a}(a\ln w_i + (1-a)\ln r_i)$, so even within the neoclassical framework, the assumption that the level and the growth rates of the factor prices is constant is implausible.

Notwithstanding this, MRW found a reasonably good fit with an R^2 of 59% (except for the data using just the OECD countries), but the implied share of capital in their version of the model exceeded its factor share, being about 0.6. Including human capital in the model improved the parameter estimates and the R^2 increased to 79%. 'Put simply, most international differences in living standards can be explained by differences in accumulation of both human and physical capital' (Mankiw 1995, 295). It is easy to see why this will be the case. The proxy for human capital is the percentage of the working age population that is in secondary school. This is likely to be higher the more advanced the country is and, without entering into a discussion of the direction of causality, the log of this variable is likely to be collinear with $\ln w_{0i}$, which is part of the expression $\ln A_{0i}$ in equation (17). Consequently, the inclusion of the proxy for human capital will improve the goodness of fit of equation (18).

We, however, interpret these results merely as being due to the estimation of an identity with 'omitted variable bias', resulting from imposing the constraint that certain variables are constants (i.e. g, δ and A_{0i}). If this constraint is relaxed, then equation (17) should give an excellent fit, with the output elasticities equal to the factor shares, provided that the two stylised facts are reasonably good approximations. In fact, from a neoclassical viewpoint, the most unrealistic assumption that MRW impose is that A_0 is constant across countries. Easterly and Levine (2001) used dummies for the major regions of the world to allow it to vary. From equation (18) we can see that this would improve the goodness of fit and bring the estimated coefficients closer to the relevant factor shares, which indeed was the result. Islam (1995) also allowed $\ln A$ to vary in panel-data estimation and found, not surprisingly, that the estimated elasticities were closer to the factor shares than those obtained by MRW (see Islam 1995, 1147). (He also included the logarithm of productivity lagged to estimate the speed of catch-up.) Indeed, we can correctly predict these changes in the regression results before a single regression has been run. Consequently, it is doubtful whether this 'model' actually tells us anything useful about why some countries are rich and others poor (Felipe and McCombie 2005b).

The important point to note, however, *pace* Temple, is that the assumptions of constant factor shares and a constant capital-output ratio are not necessary for the identity to pose problems for the interpretation of MRW's results. The point is simply that, given the identity, we know immediately that if MRW get reasonable results (or not) when estimating equation (17), it is simply because these 'stylised facts' hold (or do not), together with the assumption of the (rough) constancy of $\ln A_{0i}$, g and δ. These 'stylised facts' can hold irrespective of whether or not there exists an aggregate production function. They are not 'restrictive assumptions' in the sense that the whole critique depends on them. If they do not hold, then we need a more flexible functional form (such as a translog) that gives a better approximation to the accounting identity, and hence a better statistical fit. We also know that, because of this, MRW are bound to get a poor fit to their estimating equation which is based on the assumption that the 'production function' is a Cobb-Douglas function.

Finally, we also showed in Felipe and McCombie (2005b) that the much-discussed convergence regression is also affected by the accounting identity problem. By simply subtracting the logarithm of the initial level of productivity from both sides of equation (17), it can be seen that, as the specification of this regression improves and approaches the identity, the coefficient of initial income tends to minus unity and, therefore, the speed of convergence tends to infinity.

Growth econometrics without production functions

Temple notes that while the inclusion of the initial level of productivity in a regression with productivity growth as the regressand can be given an interpretation based on the aggregate production function (i.e. the absolute convergence regression), this need not be the case. Regressions explaining disparities can include variables that are not related to the aggregate production function and it is not necessary to rely on this as a justification for the regression. A large number of such variables can, and are, included in such Barro-type regressions. These 'everything but the kitchen sink' regressions have become popular in some quarters. But as such models, according to Temple, have nothing to do with the aggregate production function, it is clear that they cannot represent a test of the neoclassical growth model, which is the rationale for MRW's exercise (e.g. the interpretation of the coefficients in terms of output elasticities) and the debate that we raise.

In our view, such regressions represent little more than 'measurement without theory'. Whether they really tell us anything about the causes of growth and why some countries have never developed is debatable (see Rodrik 2005). The regressions assume ergodicity and thereby exclude any form of path dependence; they assume homogeneity of parameters; the data are often suspect; they over-simplify complex relationships; and most of the regressors are fragile, etc (Kenny and Williams 2001; Levine and Renelt 1992).

It is of course possible to run regressions that attempt to explain differences of growth rates in terms of variables that are not part of the definition of value added. This is the case of, for example, growth regressions whose objective is to test the statistical significance and sign of variables such as the abundance in natural resources, the effect of being coastal versus landlocked, or neighbourhood effects (e.g., Collier and O'Connell 2007). Clearly, these regressions are not derived from a production function and they are not the subject of this debate.

Our argument is that when capital is included in the regression as a value measure, together with employment, the problem of the accounting identity arises. Moreover, if Temple's broad interpretation of growth regressions were the standard one, we wonder why neoclassical researchers waste time with pages of theoretical work involving aggregate production functions. Therefore, it is difficult within the neoclassical paradigm to defend growth econometrics without production functions.

And so to Lucas on development

Temple (2006, 304) cites as an example of the usefulness of the aggregate production function Lucas (1990):

> In a classic paper, Lucas (1990) showed that, under conventional assumptions about the extent of diminishing returns, the vast differences we observe in labour productivity across countries cannot be explained by differences in capital intensity, without a counterfactual implication. If differences in capital intensity account for underdevelopment, the returns to investment in poor countries would have to be many times the returns in rich countries – to a far greater extent than is usually thought plausible.

> One response to the Lucas paper is to say that, because his conclusions are derived from an aggregate production function, it is of no value. I think that is clearly wrong: Lucas has shifted the burden of proof away from one side of the debate and towards another.

This quotation raises a number of interesting points, not least because it shows that the neoclassical paradigm, with an aggregate production function with diminishing returns, generates puzzles that have to be answered within that paradigm, while in fact the paradigm may be irrelevant.

Lucas's observation, which is hardly novel, comes from postulating a neoclassical production function. With the supposed output elasticities of labour and capital equalling 0.75 and 0.25, it is not surprising that the data will show that capital-intensity can explain little in the way of differences in labour productivity, as a simple back-of-the envelope calculation will demonstrate.

For any year, the ratio of the accounting identities of the most advanced country, the US, and a less developed country, i, can be written exactly as:

$$\frac{(Y_{US}/L_{US})}{(Y_i/L_i)} \equiv \frac{w_{US} + r_{US}(K_{US}/L_{US})}{w_i + r_i(K_i/L_i)} \tag{19}$$

or equivalently as (assuming that factor shares are constant, and the same in the US and country i):

$$\frac{(Y_{US}/L_{US})}{(Y_i/L_i)} \equiv \frac{a^{-a}(1-a)^{-(1-a)}\, w_{US}^{a} r_{US}^{(1-a)} (K_{US}/L_{US})^{(1-a)}}{a^{-a}(1-a)^{-(1-a)}\, w_i^{a} r_i^{(1-a)} (K_i/L_i)^{(1-a)}} \tag{20}$$

Using the stylized fact that the capital–output ratio is constant (or, what comes to the same thing, that the rate of profit does not differ between countries) and the definition $w \equiv a(Y/L)$ the ratio may be written as

$$\frac{(Y_{US}/L_{US})}{(Y_i/L_i)} \equiv \frac{w_{US}^{a}}{w_i^{a}} \cdot \frac{(K_{US}/L_{US})^{(1-a)}}{(K_i/L_i)^{(1-a)}} \equiv \frac{(Y_{US}/L_{US})^{a}}{(Y_i/L_i)^{a}} \cdot \frac{(K_{US}/L_{US})^{(1-a)}}{(K_i/L_i)^{(1-a)}} \tag{21}$$

The relative contributions, in a purely mathematical and not economic sense, of the two expressions on the right-hand-side of equation (21) are reported in Table 1.

Of course, if labour's share a differs from 0.75, or if there are disparities in this value between countries, this will affect the contribution of columns (ii) and (iii). This is also true if we allow the rate of profit to vary. Nevertheless, the picture is clear. From the accounting identity, the increasing difference in the ratio of productivity levels is largely explained by the increasing value of the ratio of the wage rates. For example, when the productivity of the US is 50 times greater than that of the less developed country, the 'explanation' of the differential in productivity provided by the ratio of the wage rates is seven times larger than that provided by the value of the capital–labour ratio. These results do not require the assumption of a production function with diminishing returns to capital or that all factors are used efficiently and they are paid their marginal products. All these are highly dubious assumptions, especially for the less developed countries. The production function approach assumes that column

Table 1. Contributions of the ratio of the real wage rate and of the capital–labour ratio to the ratio of productivity levels.

(i) $\left(\frac{Y_{US}/L_{US}}{Y_i/L_i}\right)$	(ii) $\left(\frac{w_{US}}{w_i}\right)^a$	(iii) $\left(\frac{K_{US}/L_{US}}{K_i/L_i}\right)^{(1-a)}$	Ratio of (ii)/(iii)
1	1.00	1.00	1.00
10	5.62	1.78	3.16
20	9.46	2.11	4.47
30	12.82	2.34	5.48
40	15.91	2.51	6.32
50	18.80	2.66	7.07

Notes: The figures are calculated using equation (21).

The value of a is 0.75 and column (ii) is equal to $\left(\frac{Y_{US}/L_{US}}{Y_i/L_i}\right)^a$

(ii) is the measure of relative level of technology or TFP. However, through the accounting identity it becomes obvious that it is not necessary to assume diminishing returns to show the relative unimportance of the relative capital–labour ratio.

Conclusions

We have clarified a number of misunderstandings that seem to be prevalent in the literature about the theoretical foundations of the concept of the aggregate production function and its use for theoretical and applied analyses. We have also clarified what we consider to be Temple's misinterpretation of the accounting identity critique of aggregate production functions. We may summarise the position as follows.

- The aggregation literature and the Cambridge Capital Theory Controversies have shown that, for all practical purposes, aggregate production functions do not exist, even as approximations.
- The use of value data means that, because of the underlying accounting identity, it is always possible to obtain a close statistical fit to the Cobb-Douglas, CES and other more flexible functions, such as the translog, with the output elasticities equal to the factor shares.
- These results cannot be interpreted as a test of the existence of the aggregate production function. And the estimates obtained are not the underlying technological parameters of the economy.
- Therefore, theoretical models that use the aggregate production function are untestable in the sense that they cannot be statistically refuted (at least using conventional methods) and therefore the results tell us nothing relevant.
- The accounting identity critique does not depend upon the assumption that factor shares are constant, or that the weighted growth of the wage rate and the rate of profit is also constant.
- Disaggregation, per se, does not invalidate the critique unless physical units are used in measuring output and the various items of machinery and structures.

Acknowledgements

We are grateful to Jonathan Temple for his helpful detailed comments on an earlier version of this paper and for clarifying some of his arguments – he has convinced us that there are greater areas of agreement between us than we had previously thought, although, as can be seen, substantial differences of opinion remain. We also benefited from the comments of two anonymous referees.

Notes

1. We somewhat unconventionally define an aggregate production function as one that uses value data. Consequently, estimating production functions using value data at, for example, the four-digit SIC level is equally subject to the problems posed by the identity as estimating an aggregate production function for the whole economy or for manufacturing.
2. A notable exception is Temple (1999, 150) who states that 'arguably the aggregate production function is the least satisfactory element of macroeconomics, yet many economists seem to regard this clumsy device as essential to an understanding of national income levels and growth rates'. However, apart from noting the problems that structural change poses for the concept, he does not elaborate further.
3. For expositional ease, we shall confine our discussion to value added, although the same arguments hold when gross output is used and the inputs include intermediate goods and materials.
4. Land may be included as another factor of production without affecting the conclusions of the argument. Land is not included in any estimations of production functions for industry or the whole economy (as opposed to the agricultural sector) due to the unavailability of separate data in the national accounts.
5. Indeed, Kaldor (1961) who first put forward the 'stylised facts' of economic growth was dismissive of the neoclassical aggregate production function.
6. Felipe and McCombie (2009) demonstrate that neoclassical labour demand functions are also merely capturing the identity.
7. See also Shaikh (1974, 1980) for the case of the Humbug production function, and Felipe and Adams (2005), who subjected the original data of Cobb and Douglas (1928) to the accounting identity critique. McCombie (2000-2001) showed that Solow's (1957) original data gave a statistically insignificant fit when estimated as a Cobb-Douglas relationship with a linear time-trend.
8. This problem is more serious than may be gathered from Temple. The Diamond-McFadden impossibility theorem has shown that with labour- and capital-augmenting technical change growing at different rates over time, it is not possible to identify the technological parameters of the aggregate production function, even when the latter exists (Diamond, McFadden, and Rodriguez 1978).
9. It is also straightforward to show that the interpretation of the dual derived from the cost function suffers from the same problem, namely, that it is determined solely by that the accounting identity. The proof is available on request from the authors.
10. In fact, the criticism can be traced back to the 1940s (see McCombie 1998b, for a discussion of the history of the aggregate production function).
11. Alternatively, we can assume a continuum of firms, totally differentiate the accounting identity and then integrate the result. If factor shares are constant we will obtain a Cobb-Douglas relationship.

References

Barro, R. J., and X. Sala-i-Martin, X. 1998. *Economic growth*. Cambridge MA: MIT Press (2nd ed, 2003).

Bronfenbrenner, M. 1971. *Income distribution theory*. London: Macmillan.

Cobb, C.W., and Douglas, P.H. 1928. A theory of production. *American Economic Review* (Supplement) 18, no. 1: 139–65.

Collier, P., and O'Connell, S.A. 2007 Opportunities and choices. In *The political economy of economic growth in Africa, 1960–2000,* Vol. 1, ed. B. Ndulu, S.A. O'Connell, R. Bates, P. Collier, and C. Soludo. Cambridge: Cambridge University Press.

Diamond, P., D. McFadden, and M. Rodriguez. 1978. Measurement of the elasticity of factor substitution and bias of technical change. In *Production economics: A dual approach to theory and applications, Vol. II, applications of the theory of production,* ed. M. Fuss, and D. McFadden. Amsterdam: North-Holland.

Douglas, P.H. 1976. The Cobb-Douglas production function once again: Its history, its testing, and some new empirical values. *Journal of Political Economy* 84, no. 5: 903–15.

Easterly, W., and R. Levine. 2001. What have we learned from a decade of empirical research on growth? It's not factor accumulation: Stylized facts and growth models. *World Bank Economic Review* 15, no. 2: 177–219.

Felipe, J., and F.G. Adams. 2005. 'A theory of production'. The estimation of the Cobb-Douglas function: A retrospective view. *Eastern Economic Journal* 31, no. 3: 427–46.

Felipe, J., and F.M. Fisher. 2003. Aggregation in production functions: What applied economists should know. *Metroeconomica* 54, May: 208–62.

Felipe, J., and C. Holz. 2001. Why do aggregate production functions work? Fisher's simulations, Shaikh's identity, and some new results. *International Review of Applied Economics* 15, no. 3: 261–85.

Felipe, J., and J.S.L. McCombie. 2001 The CES production function, the accounting identity, and Occam's Razor. *Applied Economics* 33, no. 10: 1221–32.

Felipe, J., and J.S.L. McCombie. 2003. Methodological problems with neoclassical analyses of the East Asian miracle. *Cambridge Journal of Economics* 27, no. 5: 695–721.

Felipe, J., and J.S.L. McCombie. 2005a. How sound are the foundations of the aggregate production function? *Eastern Economic Journal* 31, no. 3: 467–88.

Felipe, J., and J.S.L. McCombie. 2005b. Why are some countries richer than others? A sceptical view of Mankiw-Romer-Weil's test of the neoclassical growth model. *Metroeconomica* 56, no. 3: 360–92.

Felipe, J., and J.S.L. McCombie. 2006. The tyranny of the identity: Growth accounting revisited. *International Review of Applied Economics* 20, no. 3: 283–99.

Felipe, J., and J.S.L. McCombie. 2009. Are estimates of labour demand functions mere statistical artefacts. *International Review of Applied Economics* 23, no. 2: 147–68.

Felipe, J., J.S.L. McCombie, and R. Hasan. 2008. Correcting for biases when estimating production functions: An illusion of the laws of algebra? *Cambridge Journal of Economics* 32: 441–59.

Fisher, F.M. 1969. The existence of aggregate production functions. *Econometrica* 37, no. 4: 553–77.

Fisher, F.M. 1992. *Aggregation. Aggregate production functions and related topics,* ed. J. Monz. London: Harvester Wheatsheaf.

Fisher, F.M. 2005. Aggregate production functions: A pervasive, but unpersuasive, fairytale. *Eastern Economic Journal* 31, no. 3: 489–91.

Friedman, M. 1953. The methodology of positive economics. In *Essays in positive economics,* ed. M. Friedman. Chicago: Chicago University Press.

Garegnani, P. 1966. Switching of techniques. *Quarterly Journal of Economics* 80, no. 4: 554–67.

Griliches, Z., and Mairesse, J. 1998. Production functions: The search for identification. In *Econometrics and economic theory in the 20th century,* ed. S. Strom, 169–203. Cambridge: Cambridge University Press.

Hacche, G. 1979. *The theory of economic growth: An introduction.* London: Macmillan.

Islam, N. 1995. Growth empirics: A panel data approach. *Quarterly Journal of Economics* 110, no. 4: 1127–270.

Jones, H.G. 1975. *An introduction to modern theories of economic growth.* Middlesex: Nelson.

Jones, C.I. 1998. *Introduction to economic growth.* New York: WW Norton (2nd edn, 2002).

Jorgenson, D.W., and Z. Griliches. 1967. The explanation of productivity change. *Review of Economic Studies* 34, no. 99: 249–83.

Kaldor, N. 1961. Capital accumulation and economic growth. In *The theory of capital,* ed. F.A. Lutz and D.C. Hague, 177–222. London: Macmillan.

Kenny, C., and D. Williams. 2001. What do we know about economic growth? Or, why don't we know very much? *World Development* 29, no. 1: 1–22.

Kuhn, T.S. 1962. *The structure of scientific revolutions.* Chicago: Chicago University Press (3rd edn, 1996).

Lakatos, I. 1970. Falsification and the methodology of scientific research programmes. In *Criticism and the growth of knowledge,* ed. I. Lakatos, and A. Musgrave. Cambridge: Cambridge University Press.

Levine, R., and Renelt, D. 1992. A sensitivity analysis of cross-country growth regressions. *American Economic Review* 82, no. 4: 942–63.

Levinsohn, J., and A. Petrin. 2003. Estimating production functions using inputs to control for unobservables. *Review of Economic Studies* 70, no. 2: 317–41.

Lucas, R.E. 1990. Why doesn't capital flow from rich to poor countries? *American Economic Review, Papers and Proceedings* 80, no. 2: 92–6

Mankiw, G. 1995. The growth of nations. *Brookings Papers on Economic Activity* 26, no. 1: 275–326.

Mankiw, N.G., D. Romer, and D.N. Weil. 1992. A contribution to the empirics of economic growth. *Quarterly Journal of Economics* 107, no. 2: 407–37.

Marshak J., and W.H. Andrews. 1944. Random simultaneous equations and the theory of production. *Econometrica* 12, no. 3/4: 143–205.

McCombie J.S.L. 1987. Does the aggregate production function imply anything about the laws of production? A note on the Simon and Shaikh critiques. *Applied Economics* 19, no. 8: 1121–36.

McCombie, J.S.L. 1998a. Paradigms, rhetoric, and the relevance of the aggregate production function. In *Method, theory and policy in Keynes. Essays in honour of Paul Davidson, vol. III,* ed. P. Arestis. Aldershot: Edward Elgar.

McCombie, J.S.L. 1998b. 'Are there laws of production?': An assessment of the early criticisms of the Cobb–Douglas production function. *Review of Political Economy* 10, no. 2: 141–73.

McCombie, J.S.L. 2000. Regional production functions and the accounting identity: A problem of interpretation. *Australasian Journal of Regional Studies* 6: 133–56.

McCombie, J.S.L. 2000–2001. The Solow residual, technical change, and aggregate production functions. *Journal of Post Keynesian Economics* 23, no. 2: 267–97 (Errata, 23, no. 3: 544).

Olley, S., and A. Pakes. 1996. The dynamics of productivity in the telecommunications equipment industry. *Econometrica* 64, no. 6: 1263–98.

Phelps Brown E.H. 1957. The meaning of the fitted Cobb–Douglas function. *Quarterly Journal of Economics* 71, no. 4: 546–60.

Reder, M.W. 1943. An alternative interpretation of the Cobb–Douglas function. *Econometrica* 11, no. 3/4: 259–64.

Rodrik, D. 2005. Why we learn nothing from regressing economic growth on policies. (Unpublished manuscript) Harvard University.

Samuelson, P.A. 1961–1962. Parable and realism in capital theory: The surrogate production function. *Review of Economic Studies* 29, no. 3: 193–206

Samuelson, P.A. 1966. A summing up. *Quarterly Journal of Economics* 80, no. 4: 568–83.

Shaikh, A. 1974. Laws of production and laws of algebra, the Humbug production function. *Review of Economics and Statistics* 56, no. 1: 115–20.

Shaikh, A. 1980. Laws of production and laws of algebra: Humbug II. In *Growth, profits and property,* ed. E.J. Nell. Cambridge: Cambridge University Press.

Simon, H.A. 1979a. On parsimonious explanations of production relations. *Scandinavian Journal of Economics* 81, no. 4: 459–74.

Simon, H.A. 1979b. Rational decision-making in business organizations. *American Economic Review* 69, no. 4: 493–513 (Nobel Memorial Lecture, 8 December, 1997).

Simon, H.A., and F.K. Levy. 1963. A note on the Cobb–Douglas function. *Review of Economic Studies* 30, no. 2: 93–4.

Solow, R.M. 1957. Technical change and aggregate production function. *Review of Economics and Statistics* 39, no. 3: 312–20.

Solow, R.M. 1958. A skeptical note on the constancy of relative shares. *American Economic Review* 48, no. 4: 618–63.

Solow, R.M. 1966. Review of J.R. Hicks. *Capital and Growth, American Economic Review* 56, no. 5: 1257–60.

Temple, J.R.W. 1999. The new growth evidence. *Journal of Economic Literature* 37, no. 1: 112–56.
Temple, J.R.W. 2006. Aggregate production functions and growth economics. *International Review of Applied Economics* 20, no. 3: 301–17.
Wan, H.Y. 1971. *Economic growth.* New York: Harcourt Brace Jovanovich.
Wibe, S. 1984. Engineering production functions: A survey. *Economica* 51, no. 204: 401–11
Weil, D.N. 2005. *Economic growth.* Boston: Addison Wesley.