On the Motion of the Planets and Temple's "Aggregate Production Functions and Growth Economics"

JESUS FELIPE* AND J.S.L. McCOMBIE**

** Asian Development Bank, Manila, Philippines; ** Cambridge Centre for Economic and Public Policy, University of Cambridge, UK.*

ABSTRACT *In an article in the 2006 volume of this Review, 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.*

KEY WORDS: Aggregate production function, growth econometrics, aggregation problems, accounting identity.

JEL CLASSIFICATION: O11, O16, O47, O53

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Introduction

In this Review, Temple (2006) presented an assessment of what has been termed the accounting identity critique of aggregate production functions. This critique 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 it (i.e., the production function) actually exists. All aggregate production functions (estimated using country or sector level data) must necessarily use value data. Disaggregation does not solve the problem if value data, as opposed to physical quantities, are used.[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, p. 307).

However, and 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. In our view, the last 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, to coin Solow's term, or approximations. It shows unambiguously that any estimates will indeed be spurious in the sense mentioned above. But Temple is more equivocal. For example, he comments that

> the most common motivation for estimating these [production function] relationships is to obtain information abut 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*

[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. Disaggregation does not solve the problem unless it is possible to obtain data in physical units, an almost impossible requirement considering the wide variety of capital goods and structures.

and some estimation methods that can accommodate unobserved differences in technology will be discussed later. (Temple 2006, p.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, namely from the omission of TFP.[2]

The aggregate production function is one of the most widely used concepts in macroeconomics. Since Cobb and Douglas (1928) first published their first estimates over eighty years ago, there have been studies, too numerous to mention, estimating production functions using cross-sectional and time-series data. Based on his theoretical model of 1956, Solow (1957) formally linked the empirical study of total factor productivity growth and the neoclassical growth model, and provided what is widely seen as a very powerful approach for the study of economic growth. This work led to a plethora of empirical studies calculating the separate contributions of technical change and of the growth of factor inputs to the growth of output (the "growth accounting approach"). All neoclassical growth models, including the endogenous growth models, have an aggregate production function at their core.

Yet, the aggregate production function is also one of the most controversial concepts in economics. The literature on aggregation and the Cambridge Capital Theory Controversies have shown 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, and certainly not in the form of the conventional specifications of the Cobb-Douglas, CES, translog, etc. 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.

It is in this context that Temple (2006) asks the extremely pertinent question "Is there any 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), they have subsequently simply been either ignored or forgotten (e.g., Jones, 1998; Barro and Sala-i-Martin, 1998; and Weil, 2005).[3] This is despite the fact that no convincing counter-

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[2] This problem goes back to the very first estimates of production functions by Cobb and Douglas (1928). It was Tinbergen (1942) who first suggested proxying technological progress by a time trend in time-series regressions.

[3] A notable exception is Temple (1999, p.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

argument or rebuttal of the U.K. side of the Cambridge Capital Theory Controversies, much less of the damaging implications of the aggregation literature, has ever been advanced.

Nevertheless, Temple argues that models using the aggregate production do, in fact, tell us something: "the very real possibility of error is not enough to make silence the best research strategy".[4] Any model is an abstraction from reality and the aggregate production function is best regarded as a Solovian (1966) parable. So long as the researcher knows what the key assumptions are and how sensitive they are to minor modifications, the simple, at least conceptually if not mathematically, neoclassical growth models do tell us something important that we did not know before.

Temple considers both the aggregation and a separate problem, which we have termed the "accounting identity" (and Temple calls the "economic") critique.[5,6] 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.[7] They will also have to determine the likely labour requirements, both skilled and unskilled, for the operation of the plant. Nevertheless, in practice, when aggregate production functions are estimated, because prices are needed to aggregate the heterogeneous output and capital, constant price value (monetary) data have to be used. This is the core of the problem. (It should be noted that this has nothing to do with the "aggregation problem" as

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essential to an understanding of national income levels and growth rates". However, apart from noting the problems that structural change pose for the concept, he does not elaborate further.

[4] This is reminiscent of the Quine-Duhem thesis with respect to the desirability of persisting with a theory in spite of evidence to the contrary. It 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. See McCombie (1998) for 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.

[5] See Simon and Levy (1963); Simon (1979a & b); Shaikh (1974, 1980); McCombie (1987); Felipe and McCombie (2003, 2005a & b, 2006). The critique may be traced back to Reder (1942), Marshak and Andrews (1944), and Phelps Brown (1957).

[6] Temple (2006, p.305) terms the standard econometric, including specification problems, the "statistical" critique.

[7] 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.

commonly understood, except in the very limited sense that prices have to be used to sum different physical commodities and machinery and structures.)[8] There have been a few studies that estimate production functions using physical data, but such “engineering production function” studies are few and far between (see Wibe, 1984, for a survey.)

This implies that output (gross output or value added)[9] 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 we have 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, 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.[10] We agree that as the aggregation problems are so severe, the use of an aggregate production function poses serious methodological questions. Temple’s defence of the use of theoretical models relies implicitly on Friedman’s (1953) instrumentalist methodology. As, by definition, all models must have simplifying assumptions, 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?

Consequently, 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*

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[8] 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.”

[9] 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.

[10] We hope that we are “extreme” in the same way that Galileo was regarded as extreme by the Catholic Church in 1633.

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 in all probability 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.[11]

The rest of this paper elaborates upon the key issues on which Temple (2006) bases his case for the (partial) defence of the aggregate production function. First, he argues that growth models are "parables" that can be useful even when their assumptions are unrealistic and cannot be formally justified. We summarize the results of the theoretical literature that shows that the concept of aggregate production function is more than just suspect. Secondly, we explain briefly the accounting identity critique and clarify what we see as Temple's misunderstandings. We consider explicitly the growth accounting approach and show how disaggregation does not rescue the aggregate production function. Thirdly, 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 it was originally presented by Phelps-Brown (1957) and Simon and Levy (1963). Fourthly, we discuss the usefulness of growth econometrics without production functions. We then summarize the results of a number of simulation exercises that question the notion of total factor productivity as a measure of technical progress, as well as econometric estimation. 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.

We show that on all counts, Temple's arguments fall short of providing a compelling justification and defence of the concept of an aggregate production function. The 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 two thousand years. The major difference, though, is that the Ptolemaic theory could be, and was eventually, empirically refuted, whereas this cannot be the case with the aggregate production function.

[11] In what follows, we draw on, in particular, Felipe and McCombie (2003, 2006) where the issues are discussed more fully. We make no apology for repeating some of the arguments which have been ignored.

On Models and Parables, Aggregation and All That

Paradoxically, in justifying the use of the aggregate production function, Temple explicitly appeals to the "parable" defence associated with Samuelson (1961-1962) and Solow (1966). This is puzzling because what became clear from the symposium "Paradoxes in Capital Theory" in the November 1966 edition of the *Quarterly Journal of Economics* was that Samuelson's parable provided no defence of the aggregate production function, as it never really moved out of the one-sector world. Samuelson (1966) himself agreed with these conclusions.

Samuelson (1961-62) originally claimed that even in cases with heterogeneous capital goods, some rationalization could be provided for the validity of the neoclassical parable, which assumes that there is a homogenous factor referred to as capital, and whose marginal product equals the rate of profit. Samuelson showed that if there were a number of techniques, each technique consisting of a consumer-good sector and a capital-good sector and both subject to fixed coefficients, the continuum of these techniques could be treated as if they came from a well-behaved, constant returns-to-scale production function, the so-called surrogate production function.

Next, Samuelson showed that the inverse relation between the wage rate and the profit rate would be the same as that obtained from an appropriately defined surrogate aggregate production function, with surrogate capital as a single factor of production. In competitive equilibrium, the wage rate is determined by the marginal productivity of labor. The latter is a ratio of two physical quantities, independent of prices (i.e., independent of distribution). The same is true for the rate of profit: it is determined by the marginal productivity of capital and is also measured in physical quantities. Under these circumstances, since there is a well-behaved pseudo production function, there is a unique inverse relation between the intensity of the factors and their relative prices, and thus, as a factor of production becomes scarcer, its price increases. Consequently, Samuelson showed that an economy with a number of separate fixed coefficients production functions could be viewed as one where the relationship between output and the inputs could be approximated by a well-behaved aggregate production function.

However, it was quickly shown that Samuelson's surrogate production function relied on the crucial assumption that the same ratio of inputs is used in the consumption and capital goods industries. This means that while the micro production functions required for the different techniques comprising the surrogate aggregate production function are different with respect to engineering specifications, for each technique, the ratio of labor to machines in both the capital and consumer goods sectors of any one technique had to be identical. Consequently, the cost of capital is determined solely by the amount of labor embodied in the machines required for each technique (there are shades of Marx here). [12]

The limitations of Samuelson's surrogate production function were exposed in the *Quarterly Journal of Economics* symposium in 1966 and most comprehensively by Garegnani (1970).[13] For the surrogate function to yield the correct total product, the "surrogate capital" would have to coincide with the value in terms of consumption of the capital in use. (See Garegnani, 1970, pp. 414-416.) But in these circumstances, the resulting marginal products would not be the traditional ones (i.e., derived assuming a physical measurement of all the factors) and therefore would not be equal to the rates of remuneration of the corresponding factors. This implies that the surrogate production function cannot be generally defined, since no such function can exist, i.e., one that would give both the correct rates of remuneration and the correct product. Burmeister (1980, p. 423) in a comment on Brown's (1980) succinct survey of the issues, concluded "I fully agree with Brown's stated conclusion that 'the neoclassical parable and its implications are generally untenable'. … Freak cases such as Samuelson's surrogate production function example are of little comfort."

Therefore, even though the Capital Controversies relate to comparisons of different steady-state positions, they give us persuasive reasons why the aggregate production function shouldn't work in practice.

This is not all, however, for in addition to the arguments in the Cambridge Capital Theory Controversies, the aggregation literature also provides another set of powerful arguments that totally question this concept. The position has been summarised by Franklin Fisher (2005, p.490), who has done more work than most on this issue, in the following terms:[14]

- Except under constant returns, aggregate production functions are unlikely to exist at all.
- Even under constant returns, the conditions for aggregation are so very stringent as to make the existence of aggregate production functions in real

[12] Samuelson, apart from working with a model where there is only one consumption good, and where input coefficients are fixed at the micro level, also assumed constant returns to scale, perfect competition, that only the n-th capital good is used to produce the n-th capital good, and that depreciation of a capital good is independent of its age.

[13] Garegnani had in fact pointed this out to Samuelson prior to the publication of the surrogate production function article in 1961-62.

[14] For more detailed discussions see Felipe and Fisher (2003, 2006).

economies a non-event. This is true not only for the existence of an aggregate capital stock but also for the existence of such constructs as aggregate labour or even aggregate output.

- One cannot escape the force of these results by arguing that aggregate production functions are only approximations. While, over some restricted range of the data, approximations may appear to fit, good approximations to the true underlying technical relations require close approximation to the stringent aggregation conditions, and this is not a sensible thing to suppose.

The last point is especially important, as one of the defences of the neoclassical position, and one invoked by Temple, is that any theory is of necessity an approximation to reality. As Fisher himself agrees - the theoretical conditions for successful aggregation are not of great interest. What matters "is whether aggregate production functions provide an adequate approximation to reality over the values of the variables that occur in practice" (Fisher, 1969, p.569, omitting a footnote). Of course, much depends upon what one means by "an adequate approximation". Fisher (1969, p. 571, omitting a footnote) continues that "without going into great detail, it turns out that the only way in which such approximations could result would be if we were willing to accept production functions which were irregular in a well-defined sense. … In less technical language, the derivatives would have to wiggle violently up and down all the time. There is nothing wrong with functions having such properties, but we do not ordinarily expect production functions to exhibit this kind of behaviour. Certainly it is not exhibited by aggregate production functions in practice."

So we have a conundrum, namely, that production functions seem to work very well in a large number of cases in terms of the usual statistical diagnostics when theoretically they shouldn't, even as an approximation (Fisher, 1971). We shall show below why this occurs and why this finding implies that the putative aggregate production function should not be interpreted in the standard sense as reflecting the technical conditions of production.

On Accounting Identities and Production Functions

The answer as to why remarkably good fits are obtained to 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 in applied work, these variables have to be expressed in terms of constant price values. 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 we can always get, with a little ingenuity, a good statistical fit to a form that resembles an aggregate production function. Temple acknowledges the force of the accounting identity criticism of aggregate production functions. However, his exposition of the arguments is misleading and incorrect in a number of respects. Hence, at the risk of some repetition, we need to clarify a few points.

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

$$Y \equiv W + \Pi \tag{1}$$

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

$$Y \equiv wL + rK \tag{2}$$

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.[15] *This 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 ^ 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 putative production $Y = F(L, K, A)$ 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 that there is perfect competition (and hence constant returns to scale) and factors are paid their marginal products, then equation (4) can be expressed as:

[15] 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.

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

This is formally equivalent to equation (3) in the sense that estimation of equation (5) faces the problem that the data used must satisfy the identity (3). This automatically implies that $\hat{A}_t \equiv a_t\hat{w}_t + (1-a_t)\hat{r}_t$. 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 equations (3) and (5). This has been interpreted as confirming 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, putative technical change is assumed to occur at a constant rate (this is discussed further below).

In analyzing the economic implications of the critique, Temple (2006, p. 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, $Y = Be^{\lambda t}L^{\alpha}K^{\beta}$, or a more general specification of $Y = F(L,K,t)$. 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 simply spurious, determined by the value added accounting identity? Our answer is unequivocally that the answer is yes and it is that the results are always spurious.

Let us elaborate. If we integrate equation (3) with respect to time, assuming the stylised facts that $\lambda_t = a_t\hat{w}_t + (1-a_t)\hat{r}_t$ and factor shares are both constant (assumptions that do not depend upon the existence of an aggregate production function)[16], we obtain:

$$Y \equiv Be^{\lambda t}L^{a}K^{(1-a)} \qquad (6)$$

[16] Indeed, Kaldor (1962) who first put forward the "stylised facts" of economic growth was dismissive of the neoclassical aggregate production function.

which is indistinguishable from the Cobb-Douglas production function, and assuming that the neoclassical conditions necessary for the output elasticities to be equal to the factor shares hold. Equation (6) is, however, merely the accounting identity, equation (2), rewritten 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. More generally, equation (3) can be written as (only assuming that factor shares are constant):

$$Y \equiv a^{-a}(1-a)^{-(1-a)} w^{a} r^{(1-a)} L^{a} K^{(1-a)} \qquad (7)$$

It should be noted that at any particular time (for example, using data for one year from the national accounts) equation (6) is an *exact* "approximation" to the identity, equation (2) in that the right hand side of both equations (2) and (7) equal the value of output Y.[17] This explains why the Cobb-Douglas production empirically often seems to be the most parsimonious specification.[18] Equation (6) will only give a perfect fit to time-series data if the factor shares do not change over time and λ is also constant. However, for there to be substantial yearly changes in labour's factor share it is necessary that the annual growth of the real wage differs substantially from that of labour productivity. This is not generally the case. Consequently, estimating the Cobb-Douglas, even using data over long periods, is generally likely to give a good fit, especially in the light of Bowley's law.[19]

The fact that time-series data sometimes fail to give a good fit to equation (6) is due to the fact that there is often a cyclical variation in the weighted growth of the wage rate and the rate of profit. Consequently, a linear time trend (in the log-level specification) provides a poor proxy for λ_t. However, as there is nothing in neoclassical production theory to suggest that the rate of technical change must be exactly constant, it is always possible to find a non-linear time trend that rescues the aggregate production function (i.e., the identity). Alternatively, it is always possible to adjust the factors of production for changes in utilisation rates in such a way as to always get a perfect fit to the form estimated.

[17] The reader may easily check this numerically.
[18] If we only use data for just one year, the shares, by definition, are constant.
[19] Equation (7) can be written for labour (expressed in logarithms) as follows:
$ln\,L \equiv -\frac{1}{a} ln\,B + \frac{1}{a} ln\,Y - ln\,w - \frac{(1-a)}{a} ln\,r - \frac{(1-a)}{a} ln\,K$, where $B \equiv a^{-a}(1-a)^{-(1-a)}$. This *identity* resembles the neoclassical labour demand function (if *lnK* is omitted) and so the negative relationship between the logarithm of the supposed demand for labour and the logarithm of the wage rate is a statistical artefact. On this see Felipe and McCombie (2008).

McCombie (1998) 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 a standard error of 0.02 (so 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.

What makes the Cobb-Douglas with a linear time trend yield poor results is the cyclical variation in the weighted average of the logarithm of the wage and profit rates. If their combined growth rates are not constant (as the linear time trend assumes), then the Cobb-Douglas in logarithmic form with a linear time trend will yield poor results (even with negative coefficients). In most cases, the identity is closely approximated by using a more complex form of time trend that better captures the weighted average of the logarithm of the wage and profit rates. See, for example, Shaikh (1974) for the case of the Humbug production function and Felipe and Adams (2005), who used the original data of Cobb and Douglas (1928). Alternatively, the fluctuation in the profit rate can be reduced by adjusting the capital stock for capacity utilization, making a linear time trend a good approximation for the sum of the weighted logarithm of the wage rate and the rate of profit. McCombie (2000-2001) showed that Solow’s (1957) original data gave a statistically insignificant fit when estimated as a Cobb-Douglas with a linear time-trend. It has to be rescued by a non-linear time trend (which is tantamount to what Solow’s original estimation procedure involved) or by an adjustment for “capacity utilization”.

Temple (2006, p 306, omitting a footnote) argues as follows with respect to an equation derived from the accounting identity and resembling the Cobb-Douglas.

> Some interpretations of this result become overenthusiastic and suggest that a Cobb-Douglas will always fit the data well, simply because of the identity. This should make us pause: for example, if the underlying technology were translog, could we really expect the 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 growth rates.
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> 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,[20] 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 "statistical" and "economic" critiques are closer together than is usually acknowledged.

Temple, if we interpret him correctly, 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. This is erroneous. If there is sufficient variation in the shares and in the inputs then Temple claims that we can, in principle, identify the underlying aggregate production function, which must exist for his argument to hold. This completely 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. In other words, under this interpretation the constancy of the factor shares must be due to the elasticity of substitution being unity. It is not clear why, if we accept Temple's argument, 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 putative aggregate Cobb-Douglas production function (which even Temple accepts may not exist), otherwise there will be perfectly 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, the answer is that this is a meaningless question. *The argument concerning the accounting identity holds whether or not factor shares vary.* The assumptions that factor shares and the weighted sum of the rates of growth of the real wage rate and the profit rate are constant are not necessary assumptions (structures) for the critique to hold. The critique merely shows that *if* these stylised facts hold, a form that resembles a Cobb-Douglas production function (but that is simply the identity written in a different way) will provide the best fit to the data, regardless of the form of the underlying micro-production functions. If shares vary, say, over the cycle because of variations in labour's bargaining power, the critique says it will always be possible to get a good fit to the data by using a more flexible function, such as the translog. But, the changing values of factor shares are not

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20 This problem is more serious than may be gathered from Temple. The Diamond-McFadden (1978) 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.

caused by changes in the output elasticities; the causation runs the other way around. It should be clear that the critique is not limited to the case of the Cobb-Douglas form. The argument is normally couched in terms of the Cobb-Douglas because this is the simplest case and because the Cobb-Douglas is widely used in theoretical modelling. But the argument applies to any other specification. See, for example, Felipe and McCombie (2000 and 2003) for discussions in terms of the CES and translog respectively. 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 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. The discussion is then about correctly specifying TFP (itself a theory-dependent construct) to produce unbiased estimates of the technological parameters such as the elasticity of substitution. The latter is indeed a problem if we use physical data, but it is very much of a second order compared with that raised by the accounting identity and the use of constant price value data. Temple (in his footnote 7, p.314) argues that this point is obscured by those who proceed on the assumption that no production function exists. In fact, we would argue that the failure to see the importance of the critique rests on the failure to appreciate that is not possible to statistically refute *any* supposed aggregate production function.

At the risk of some repetition, compare once again equations (3) and (6). Equation (6) is the general form of the neoclassical production function (with the usual assumptions) expressed in growth rates and this is formally equivalent to the identity. But this does not affect the argument. Suppose there is no aggregate production function and we asked a mathematician, who knows nothing of economics or production functions, to suggest a mathematical approximation that would give a good fit to equations (2) or (3) but only using L and J, and a time trend when time-series data are used.. He might suggest rewriting them using a Box-Cox transformation (which is not necessarily related to production functions). If shares are constant, then a log-linear function will produce the best statistical fit, misleadingly implying a Cobb-Douglas; but with varying shares a non-linear specification (the equivalent of a CES function) will give a better fit. But note all we are doing is finding the best approximation to the underlying identity given by equation (1). (See McCombie, 2000).

For the identity to give a good fit to the Cobb-Douglas, we need some explanation as to why factor shares are reasonably constant. One reason, 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, 1999). As Solow (1958) first remarked, the stability of factor shares is likely to be increased as we sum over industries. Thus, ironically, as we do this arithmetically across the different industries, 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 further asked if it is ever sensible to estimate production functions using data at the national, regional, industrial, or even firm or establishment level. The clear answer is that, with value data, all one gets is an approximation to the accounting identity. Thus, in our opinion, it is not sensible. Estimation of production functions with physical data is possible as they are not subject to this critique (Felipe and McCombie 2006), but they do face the problem of specifying the correct variation of TFP between the units.

Temple argues that the most common motivation for estimating a production function is to obtain information about the technology or TFP. This misses, once again, one of the points of the accounting identity critique. As the identity shows, what neoclassical economics refers to as the rate of TFP in, say, the 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 remark.

First, as argued above, this cannot necessarily be interpreted as the rate of technical progress because it is derived solely from an accounting identity. In our simulations discussed below using hypothetical data, the rate of TFP growth calculated using value data was very different from the “true” rate (i.e., using physical quantities). There is no way that the researcher using only value data could determine the latter, which we as creators of the hypothetical data know.

Secondly, and as an implication, estimates of TFP growth derived from estimating putative aggregate production functions and using a linear time trend are approximations to this weighted average. However, in most cases they are misspecified 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, as we saw in the citation above, 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 it 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). This is not *the* problem because we know what TFP growth under neoclassical assumptions is and looks like: it is the weighted average of the growth rates of the wage and profit rates.

Hence, it is not an issue 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 says that this is not an econometric problem and that it does not have any solution. It is not a case of, for example, that there is 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. (Bronfenbrenner erroneously considered that the fact that the production function shifts over time because of technical progress is sufficient to identify it.)

Temple refers to recent 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 correctly estimate it by the appropriate estimating technique. We have discussed this in Felipe *et al*., (2008). Likewise, the issues raised by Griliches and Mairesse (1998) have no bearing upon the problem.

On Growth Accounting and Disaggregation

Temple (2006, p.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). 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 a production function and the usual neoclassical assumption (e.g. see Jorgenson and Griliches, 1967). This means that the 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. There is no such connection in our arguments.

The problem is that if the aggregate production function does not exist (as Temple at some point acknowledges may be a possibility), 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, as in Jorgenson and Griliches (1967) and cited by Temple, rests on the assumption that an aggregate production function exists, there is perfect competition and factors are paid their marginal products. None of these is required to write equation (2) as an accounting identity.

Related to this is Temple's (2006, p.308) comment that "if aggregation is not possible, the obvious solution is to disaggregate". He continues: "in the case of growth accounting, there is nothing to stop the researcher writing down

$$Y = F(K_1, K_2, \ldots K_M, L_1, L_2 \ldots\ldots, L_N) \qquad (8)$$

where there are M types of capital input and N types of labour input." He cites Jorgenson and Griliches as operationalising this approach. He points out that production function and growth theory does not in principle need aggregation. "Instead it is lack of data that will typically restrict the applied researcher to simpler methods". This unfortunately confuses aggregation with the accounting identity. 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 the minute it is necessary to aggregate different types of output and capital using constant price data then the identity is simply written as

$$Y \equiv w_1L_1 + w_2L_2 + \ldots\ldots w_NL_N + r_1K_1 + r_2K_2 + \ldots\ldots r_MK_M \qquad (9)$$

where Y and the K's are constant price value data. The argument follows through, even though there are several categories of labour and capital.[21] Aggregation poses a problem not for the reasons that Fisher (1992) has set out (important though these are) but because suitable physical data is not

[21] The use of two-sector production functions models to disaggregate the economy into agricultural and non-agricultural sectors (see Temple, 2006, p.309) does not escape the critique.

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.

Ironically, Jorgenson and Griliches are well aware that they start their analysis using an accounting identity (a "system of social accounts" to use their term). All their theoretical analysis determining the growth of total factor productivity and the dual is in terms of the accounting identity. (See Jorgenson and Griliches, 1967, pp. 251-252) and their empirical measurement of these entities is made using constant price value data, not physical data. The analysis is carried out for the US private domestic economy over the period 1945-65. The legerdemain occurs at the end of page 252 in their paper where they assume that there is a production function characterized by constant returns to scale, namely, $F(Y_1, Y_2, Y_m; X_1, X_{2, \ldots,} X_n) = 0$.[22] (It is interesting to note that they rely on Samuelson's (1962) concept of the factor price frontier which Samuelson had already conceded was flawed (Samuelson, 1966). As we have shown there can be never be independent evidence that such a production function actually exists or that factors are paid their marginal products.

Cross-Sectional Production Functions

It is ironical that Temple claims "it is overall much harder to apply the 'humbug' argument in the context of cross-sections". It is ironical because the whole argument can be traced back to Phelps-Brown's (1957) criticism of the myriad of cross-sectional regressions by Douglas and his associates in the 1930s.[23] 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).

For cross-section data, the labour share can be written as $a_i = (w_i L_i / Y_i)$; and similarly the capital share as $(1 - a_i) = (r_i K_i / Y_i)$ (where i denotes the units of the cross section). For a low dispersion in factor shares, the approximation $\bar{a} \cong (\bar{w}\,\bar{\mathrm{L}}/\bar{\mathrm{Y}})$, where a bar denotes the average value of the variable, holds. Then the following also holds:

$$(a_i / \bar{a}) \cong (w_i / \bar{w})(L_i / \bar{L}) / (\mathrm{Y_i} / \bar{Y}) \qquad (10)$$

[22] They further assume that factor prices are determined by the neoclassical marginal productivity theory of distribution.

[23] 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).

and a similar expression follows for the capital share $(1-a_i)$:

$$(1-\overline{a}_i)/(1-\overline{a}) \cong (r_i/\overline{r})(K_i/\overline{K})/(\mathrm{Y_i}/\overline{Y}) \qquad (11)$$

For small deviations of a variable X_i from its mean $\overline{X}$, it follows that $ln(X_i/\overline{X}) \cong (X_i/\overline{X})-1$. Thus, taking logs of equations (10) and (11) and using this approximation we can write:

$$ln(w_i/\overline{w})+ln(L_i/\overline{L})-ln(Y_i/\overline{Y}) \cong (a_i/\overline{a})-1 \qquad (12)$$

and

$$ln(r_i/\overline{r})+ln(K_i/\overline{K})-ln(Y_i/\overline{Y}) \cong [(1-a_i)/(1-\overline{a})]-1 \qquad (13)$$

Multiplying equations (9) and (10) by $\overline{a}$ and $1-\overline{a}$, respectively, adding them, rearranging the result and noting that $ln\,\overline{Y}-\overline{a}\,ln\,\overline{w}-(1-\overline{a})\,ln\,\overline{r}-\overline{a}\,ln\,\overline{L}-(1-\overline{a})\,ln\,\overline{K}=0$ yields

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

$$\cong A_i+\overline{a}\,ln\,L_i+(1-\overline{a})\,ln\,K_i \qquad (14)$$

Obviously, equation (14) resembles a Cobb-Douglas production function in logarithms. This derivation indicates that for a cross-section that displays low variation in the factor shares, estimation of the Cobb-Douglas form will yield very good results. But, as in the case of time-series data discussed above, the critique does not rest on this assumption and so nothing hangs by whether or not it is true. If the empirical 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.

It must be noted that, in general, it is easier to obtain plausible results with cross-sectional data than with time series. The reason is that the wage and profit rates in a cross-section (e.g., regions in a country, firms in a sector) often vary relatively little.[24] This implies that the term A_i in equation (14) will be accurately approximated by a constant, so that A_i will be effectively a constant term, A. This

[24] The obvious exception to this is the use of international data comprising the advanced and less developed countries. However, as discussed in the text, even here a good fit to the identity can be obtained.

means that the cross-sectional regression $Y_i = AL_i^{\alpha} K_i^{\beta}$ should work very well provided, with $\alpha = a$ and $\beta = (1\text{-}a)$, only that factor shares in the cross-section do not vary excessively. It should be noted that if shares show some variation then a more flexible function form might give a better fit to the cross-sectional data. (See McCombie, 2000, for an analysis using cross-regional US data.)

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). It is difficult to understand Temple's rationale when he argues "the Felipe and McCombie argument has to proceed under restrictive assumptions". It is, therefore, useful quickly to sketch the criticism.

The growth of the capital stock is given by

$$\frac{\Delta K_i}{K_i} \equiv \hat{K}_i \equiv \frac{I_i}{K_i} - \delta_i \equiv \frac{s_i Y_i}{K_i} - \delta_i \,, \qquad (15)$$

where I is gross investment, δ is the rate of depreciation and s is the gross investment-output ratio. Given the accounting identity (written as a Cobb-Douglas production function; hence, assuming that factor shares are constant) and the definition of the growth of the capital stock above, and with the further assumption (in Temple's view, our "restrictive assumptions") that there is no growth in the capital-output ratio, i.e., $\hat{Y}_i = \hat{K}_i$, it is possible to derive an equation for labour productivity (see Felipe and McCombie, 2005b, for the derivation) as:

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

or, alternatively, if w_i and r_i grow at constant rates $\hat{w}_i$ and $\hat{r}_i$,[25] this may be expressed as:

$$ln\left(\frac{Y_i}{L_i}\right) \equiv \frac{1}{a} ln\, C_{0i} + (\hat{w}_i + \frac{1-a}{a} ln\, \hat{r}_i)t + \frac{1-a}{a} ln\, s_i - \frac{1-a}{a} ln\left(n_i + \delta_i + \frac{a\hat{w}_i + (1-a)\hat{r}_i}{a}\right) \qquad (16b)$$

[25] In practice, $\hat{r}_i$ is likely to be approximately zero.

where n is the growth of population (strictly speaking, employment) a is labour's share, and (1-a) is labour's share. In equation (16b), $C_{0i} = B_0 w_{0i}^{a} r_{0i}^{(1-a)}$ and therefore varies between countries. The goodness of fit in estimating equations (16a) and (16b) depends only on how accurate the stylised facts are.

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

$$ln\left[\frac{Y_i}{L_i}\right] = ln\,\overline{A}_0 + \overline{g}t + \frac{(1-\alpha)}{\alpha} ln\, s_i - \frac{(1-\alpha)}{\alpha} ln(n_i + \overline{\delta} + \overline{g}) \qquad (17)$$

where α is the elasticity of output with respect to capital, and α is the elasticity of output with respect to labour. A bar over a variable serves to emphasise that MRW assume it is *identical* for all countries. It is immediately apparent that equation (17) resembles the identity given by equations (16a) and, especially (16b). The exception is that, as noted above, both the level and the growth rates of the factor prices (i.e. lnA_0 and g) are implicitly assumed by MRW to be constant across countries in equation (17).[26] These are interpreted by MRW as the level of technology and the rate of technical progress. (MRW also assume a constant rate of depreciation.) MRW found a reasonably good fit with an R^2 of 59 percent (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 – although it was in the right ballpark, being about 0.6. Including human capital in the model improved the parameter estimates and the R^2 increased to 79 per cent.[27] "Put simply, most international differences in living standards can be explained by differences in accumulation of both human and physical capital" (Mankiw, 1995, p.295).

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. If this is

[26] Note that, under neoclassical assumptions and using the aggregate marginal productivity theory of factor pricing, the following two equations hold: $g_i = \frac{1}{(1-a)}(a\hat{w}_i + (1-a)\hat{r}_i)$ and

$$ln\, A_i(t) = \frac{1}{a} ln\, B_0 + \frac{1}{a}(a\, ln\, w_i + (1-a) ln\, r_i).$$

[27] 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. As this is likely to be higher, the more advanced is the country and, without entering into a discussion of the direction of causality, the log of this variable is likely to be collinear with lnw_{0i} in lnC_{0i} in equation (16b). Consequently, its inclusion will improve the goodness of fit of equation (17).

relaxed, then we get the result that the fit should be perfect and the output elasticities equal the factor shares, provided that the stylized facts are reasonably good approximations. 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).

Easterly and Levine (2001) and Islam (1995) used dummies to allow *lnA* to differ among the major regions of the world, hence capturing differences in *lnw*. It is obvious *a priori* why this would lead to a substantial improvement in the fit, as indeed it did. The dummies are closely proxying the variation of the term *lnA* (predominantly due to disparities in *lnw*) in equation (17), bringing the estimated regression coefficients closer to those of the identity.

Let us turn to the important question of the supposedly “restrictive assumptions” that Temple claims we need, and which, in his view, diminish the strength of our argument. What is more restrictive? To assume that the technology of all the countries can be represented by an aggregate Cobb-Douglas production function (which implies constant output elasticities), in spite of the severe aggregation problems that this entails[28], or to assume that for some reason unrelated to the existence of an aggregate production function, the stylised facts hold, and the observed results are due to this fact together with the underlying accounting identity? (The existence of the identity is, of course, not a behavioural assumption, but a fact.).

The important point to note, *pace* Temple, is that it is not correct that the assumptions of constant factor shares and a constant capital-output ratio are necessary for the identity to pose problems for MRW’s interpretation of their results. It is simply that given the identity, we know immediately that if MRW get reasonable results (or not) estimating equation (17), it is simply because these “stylized facts” hold (or not). These “stylized facts” can hold irrespective of whether or not there exists an aggregate production function.

Not only do MRW’s results require each country to have identical constant returns to scale Cobb-Douglas aggregate production functions but all enterprises, from those in the US to those in Gambia, must be both technically and allocatively efficient; factors must be paid their marginal products; and

[28] What precisely does it mean to aggregate, say, oil refining with textile manufacturing and talk about “the” elasticity of substitution of these two industries? In fact, the problem is far worse than this as we are aggregating agriculture (which is highly efficient in the developed countries but with substantial disguised unemployment in the developing countries) with the manufacturing and the service sectors (where, even in the advanced countries, output growth is often measured as the growth of the inputs, with an arbitrary allowance for productivity growth).

perfect competition must prevail. When put in these terms we contend that there is no contest. What grounds do we have for assuming that a well-behaved aggregate production function exists when the aggregation theory and the Cambridge Capital Theory Controversies suggest that it doesn't, especially when we can explain the results without recourse to the production function? What the accounting critique shows is that one can't appeal to the fact that the data gives a good statistical fit to the putative production function in support of the latter's existence.

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 (absolute convergence), 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.[29] 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 fact, 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 is often suspect; they over-simplify complex relationships; and most of the regressor 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 productivity growth in terms of variables that are not part of the definition of value added, but it cannot be denied that growth of the *physical* capital stocks per worker is an important determinant. Therefore, the minute capital is included in the regression as a value measure, the problem of the accounting identity arises. Convergence could be nothing more than regression to the mean, i.e., a stochastic process that does *not* require the assumption of the existence of an aggregate production function and diminishing returns to capital. Moreover, if this broad interpretation of the convergence regressions were the standard one, we wonder why neoclassical researchers waste time

[29] The number of possible explanatory variables presently runs at over 30, and no doubt more will be included in the near future.

with pages of theoretical work involving aggregate production functions. Therefore, it is difficult within the neoclassical paradigm to defend growth econometrics without production functions. To this we add that in Felipe and McCombie (2005b) we showed that the converge regression is also affected by the accounting identity problem and that the coefficient interpreted as the speed of convergence tends to unity as the specification of the convergence regression improves and approaches the identity.

Some Simulation Exercises

As the aggregate production function cannot be refuted empirically, we are faced with yet another conundrum – does this necessarily mean that the aggregate production function gives misleading, not to say meaningless, empirical results concerning the elasticity of substitution, the rate of technical change, etc.? Or, can we regard it as a useful *approximation* of the technological structure of the economy, notwithstanding Fisher's (1969) remarks to the contrary? Temple suggests that, given the existence of the aggregation problem, one could always construct simulation models such as computable general equilibrium models. The problem with this approach is that nearly all these models still rely on the existence of an aggregate production function and use value data, although with their parameters calibrated rather than estimated. Consequently, the simulated predictions are open to the objections made earlier. They sometimes use optimisation assumptions and cost functions, but not surprisingly, the latter are equally vitiated by the existence of the accounting identity (see the appendix).

However, simulations are useful, but in another way. The advantage of simulation experiments is that the exact underlying technological structure is known to the researcher, as its creator, with certainty. If the Cobb-Douglas (or translog) production function gives a good fit to the aggregated data generated by the simulation exercise, with, say, its output elasticities equalling the factor shares, when we know that the underlying technology in no way resembles a Cobb-Douglas, this should certainly give us reason to pause for thought.

In our contribution to the conference volume where Temple's paper was published (Felipe and McCombie, 2006), and not mentioned by Temple, we undertook a simple simulation experiment that illustrated the problems posed by the accounting identity. We assumed that the underlying micro-production functions were given by identical Cobb-Douglas production functions in physical terms, but where the elasticity of labour was 0.25 (instead of the usual 0.75) and the elasticity of capital was 0.75 (instead of 0.25). For expositional ease, we assumed that firms employed a constant mark-up of

1.333 on unit labour costs and we used the resulting constant price value data (as this is the only data assumed to be available to the researcher) to estimate a production function. We found that the estimate of the output elasticities of labour and capital were 0.75 and 0.25, respectively, and not statistically significantly different from the relevant factor shares. What is happening is that the "causation" runs from the accounting identity and the values of the factor shares to the estimated "output elasticities". It is not from the estimates of underlying micro-production functions and the marginal productivity of factor pricing to the factor shares. (See Felipe and McCombie, 2005a, Table 1, for a further elaboration.)[30]

It is important to emphasise that we get the same result even though (i) the micro production functions exhibit increasing returns to scale ; (ii) the true micro-production functions are, say, a CES with an elasticity of substitution different from unity; and (iii) there is no underlying micro-production function at all.[31]

There have been a number of other studies that have made the point. The earliest is perhaps Fisher's (1971) well-known experiment. He constructed a number of hypothetical economies comprising a small number of "firms" each with a constant-returns-to-scale Cobb-Douglas production function. The growth of the "firms" was simulated over a number of periods and the generated values for output, employment, and capital summed. This summation deliberately violated the conditions for successful aggregation, especially with regard to the construction of the capital stock. These data were then used to estimate an aggregate Cobb-Douglas function and to see how well the Cobb-Douglas predicted the wage data. To his evident surprise, Fisher found that the aggregate production function performed very well. However, he came to the conclusion that "the point of our results, however, is not that any aggregate Cobb-Douglas fails to work well when labour's share ceases to be roughly constant, *it is that an aggregate Cobb-Douglas will continue to work well so long as labour's share continues to be roughly constant, even though that rough constancy is not a consequence of the economy having a technology that is truly summarised by an aggregate Cobb-Douglas* (Fisher, 1971,

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[30] Franklin Fisher (1971, p. 305) mentioned that Solow remarked to him that "had Douglas found labor's share to be 25 per cent and capital's 75 per cent instead of the other way around, we would not now be discussing aggregate production functions." What we have shown is that with value data, the supposed output elasticities must always equal the factor shares.

[31] Output, capital stock and employment series were generated as random numbers. We interpret this not as implying there is no physical relationship between outputs and inputs but they are so complex that they are not captured by output being a simple function of the inputs. In other words, parts of the production process are subject to fixed coefficients, others to differing elasticities of substitutions, there are inter-firm differences in managerial efficiency and so on.

p.307, emphasis added). It was left to Shaikh (1980) to point out, not surprisingly, that the reason for this was to be found in the underlying accounting identity.[32]

Fisher's simulation exercise postulate that there are well-defined production function functions at the micro-level and firms are optimising in perfectly competitive markets. Nelson and Winter's (1982) simulation model is one where a hypothetical economy is made up of a number of firms producing a homogeneous good. The technology, however, available to each firm is one of fixed-coefficients, but with a large number of possible ways of producing the good given by different input coefficients of differing efficiencies. The firm does not know the complete set of the input-output coefficients that are available to it, and so cannot immediately chose the best-practice technology. It only learns about the different techniques by engaging in a search procedure. Nelson and Winter (1982, p.227) point out, "while the explanation has a neoclassical ring, it is not based on neoclassical premises". The firms are not maximizing profits. "The observed constellations of inputs and outputs cannot be regarded as optimal in the Paretian sense: there are always better techniques not being used because they have not yet been found and always laggard firms using technologies less economical than current best practice." They also observe "the fact that there is *no* production function in the simulated economy is clearly no barrier to a high degree of success in using such a function to describe the aggregate series it generates" (p.227, emphasis added).

Hartley (2000) used the recursive dynamic equilibrium model of Hansen and Sargent (1990, 1991), which forms the framework of the real business cycle models. The actual production processes are not Cobb-Douglas but are quite restrictive in their specifications. Hartley used the model to generate a number of different types of shocks of varying intensity, including changes in labour productivity, changes in the depreciation rate of capital and changes in technology. The latter is the mechanism that largely drives the typical real business cycle model. Because the model is a simulation, the intensity and type of these shocks is known precisely. What is interesting is that for a large range of plausible values the correlation between the "true" technological shock and the Solow residual (calculated in the usual way) was low or even negative.

Although Hartley puts forward a number of reasons as to why the Solow residual sometimes acts perversely, the main reason would seem to reflect the underlying identity. The factor shares in the simulation data are roughly constant and the calculation of the Solow residual and its interpretation as a measure of technical change is only legitimate if these shares are the output elasticities of the

[32] In a follow up study, Fisher *et al.,* (1977) undertook a similar simulation exercise but this time assuming that the individual firms had CES production functions.

aggregate production function. In the case of constant factor shares, this would suggest an aggregate Cobb-Douglas production function. However, from the underlying structure of the model we know that the technology cannot be represented by an aggregate Cobb-Douglas and so the factor shares do not represent the output elasticities. However, an observer, noting only the constant factor shares and without knowing the true underlying production functions, would be inclined to assume that the aggregate production function was a Cobb Douglas. There is nothing to indicate otherwise.

Shaikh (2005) provides further evidence of the difficulty of estimating an aggregate production function by elaborating on his 1987 entry in the *New Palgrave*. He generates data by simulating a slightly modified version of the Goodwin (1967) model, which is based on a fixed-coefficients production function with Harrod neutral technical change. However, as the data set has the property that factor shares are roughly constant, not surprisingly, he is able, eventually, with a judicious choice of a time path for technical change, to show that the Cobb-Douglas production function gives a good fit to the data. Shaikh (2005, p. 451, italics in the original) emphasises, *"the technological structure of this control group [Goodwin] model is entirely distinct from that of neoclassical production theory and associated marginal productivity rules*".

What do these studies show? First, they confirm that even if the estimation of the aggregate data gives a very good fit to the Cobb-Douglas aggregate production this is no guarantee that underlying micro-production functions bear any resemblance to the Cobb-Douglas. Secondly, even when they do, but the aggregation conditions are violated so no aggregate production function exists, the data may still give the impression that this is not the case. Thirdly, the data may give a close fit to an aggregate Cobb-Douglas when there is an aggregate production function, even though the estimated aggregate output elasticities and the rate of technical change bear no resemblance to the true values.

And so to Lucas on Development

Near the beginning of his paper, Temple (2006, P.3040 cites as an example of the usefulness of the aggregate production function a paper by 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. Instrumentalism appears again here as what matters is that Lucas's model makes some predictions. The assumptions underlying the model are, once again, sidestepped.

Let us illustrate what we mean. 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, then it is not surprising that the data will show that the 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:

$$\frac{(Y_{US}/L_{US})}{(Y_i/L_i)} \equiv \frac{w_{US} + r_{US}(K_{US}/L_{US})}{w_i \quad + \quad r_i(K_i/L_i)} \qquad (18)$$

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

$$\frac{(Y_{US}/L_{US})}{(Y_i/L_i)} \equiv \frac{a^{-a}(1-a)^{-(1-a)} w_{US}^{a} r_{US}^{(1-a)} (K_{US}/L_{US})^{(1-a)}}{a^{-a}(1-a)^{-(1-a)} w_i^{a} r_i^{(1-a)} (K_i/L_i)^{(1-a)}} \qquad (19)$$

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

$$\frac{(Y_{US}/L_{US})}{(Y_i/L_i)} \equiv \frac{(Y_{US}/L_{US})^a}{(Y_i/L_i)^a} \cdot \frac{(K_{US}/L_{US})^{(1-a)}}{(K_i/L_i)^{(1-a)}} \tag{20}$$

The relative contributions, in a purely mathematical and not economic sense, of the two expressions on the right-hand-side of equation (20) are reported in Table 1.

Table 1. The contributions of the ratio of real wage and of the capital-labour ratio to the ratio of productivity levels.

(i) $\frac{(Y_{US}/L_{US})}{(Y_i/L_i)}$	*(ii)* $\left(\frac{Y_{US}/L_{US}}{Y_i/L_i}\right)^a$	*(iii)* $\left(\frac{K_{US}/L_{US}}{K_i/L_1}\right)^{(1-a)}$	Ratio *(ii)/(iii)*
1	1.00	1.00	1.00
10	5.62	1.78	3.16
50	18.80	2.66	7.07
100	31.62	3.16	10.00

Note: a is 0.75; column *(ii)* is equal to $(w_{US}/w_i)^a$

Of course, if the values of labour's share a differ 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 differences in the productivity ratios are largely explained by the increasing value of the ratio of the wage rates. For example, when the productivity of the US is a hundred 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 ten times larger than that provided by the (values of the) capital-labour ratios. These results do *not* require the assumption of a production function with diminishing returns to capital, all factors used technically efficiently and factors paid their marginal products, all highly dubious assumptions especially for the less developed countries. The production function approach assumes that column (*ii*) is the measure of relative level of technology. However, by using the accounting identity, it is not necessary to assume diminishing returns to get these results.

Lucas's argument is analogous to Solow's (1957) "surprising" (as Solow, 1988 described it) finding that less than 20 percent of the growth of US productivity in the private non-farm business sector could be ascribed to the growth of the capital-labour ratio. But this is hardly a "surprising result" in that, as with the previous result, it can be determined by the stylized facts, the accounting identity and the back of an envelope. The growth of labour productivity is defined from the identity as:

$$\hat{Y} - \hat{L} \equiv a\hat{w} + (1-a)\hat{r} + (1-a)(\hat{K} - \hat{L}) \qquad (21)$$

As factor shares are constant, the growth of the wage rate is equal to the growth of productivity. In other words, $\hat{w} = \hat{Y} - \hat{L}$ and $a\hat{w} = 0.75(\hat{Y} - \hat{L})$, as a = 0.75. Consequently, if $\hat{r} = 0$, the weighted growth of the real wage rate and the rate of profit (what would equal the growth of TFP or technical progress) must equal three quarters of the growth of observed labour productivity and the growth of the capital intensity the remaining one quarter.

Within the neoclassical paradigm, the contrary view has been to deny the prevalence of diminishing returns. One approach, although now largely out of favour, is the so-called *AK* model (where *A* is some unspecified constant). As it is assumed that there are externalities to capital accumulation, diminishing returns to scale are eliminated by aggregation, so that the putative production function is given by $Y = AK$. From the accounting identity, we can simply show that, again without having to run a single regression, the data cannot refute this hypothesis.

The identity can be written as (see equation (7)):

$$Y \equiv Bw^{a} r^{(1-a)} L^{a} K^{(1-a)} \qquad (22)$$

but $a \equiv wL/Y$, substituting into equation (22) gives

$$\begin{aligned} Y &\equiv Ba^{a}\left(\frac{Y}{L}\right)^{a} r^{(1-a)} L^{a} K^{(1-a)} \\ &\equiv AK \qquad (23) \end{aligned}$$

where $A = Ba^{a/(1-a)}r$

Thus, the data are equally compatible with both neoclassical growth theories (the endogenous and semi-endogenous growth theories are merely variants within the neoclassical growth paradigm) and also with the assumption that the concept of the aggregate production function is literally meaningless.

The problem is that the use of the aggregate production function, even as a theoretical concept, leads to posing wrong questions and severely limits the types of questions that can be studied. Consider again Lucas's position that differences in the capital-labour ratio cannot explain differences in productivity. This assumes that there is an aggregate production function where factors of production are substitutes and that it is meaningful to partition the level of output into that due to labour and that due to capital (however measured) and to measure technical change as a shift in the production function, albeit determined by economic factors as in the semi-endogenous growth theories.

Conclusions

In this paper, we have clarified a number of misunderstandings that seem to be prevalent in the literature about the theoretical foundations of the concept of 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 theoretically the aggregate production function, for all practical purposes, does not exist.

- The use of value data means that 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 aggregate production function nor do the estimates necessarily reflect the underlying aggregate technology of the economy.

- The accounting identity critique does *not* depend upon factor shares being constant or the weighted growth of the wage rate and the rate of profit being 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. Even then, there is the problem of determining the correct path of technical progress and its correct modeling.

- Any 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. In fact, we share Nelson's (1981, 1998) view that nothing has come out of these exercises that was not already well known. Moreover, these models restrict the search for the explanation as to why some countries are rich and others are poor to a very limited approach.

- Temple concludes by arguing that there are various problems inherent in general equilibrium theory that may be more serious than aggregation problems. There are indeed problems with general equilibrium theory as serious to those of the accounting identity that have largely gone unrecognized by the profession. (See Kirman (1989) for an incisive review of the former.) This does not mean that one critique is more important than the other; it is just that both mainstream micro and macro economic theory lies on less secure foundations than many of the profession think. In the case of the aggregate production function, the problem has no solution as value data has to be used. Disaggregating to the plant level where possibly physical data can be used to enable production relations to be estimated (the so-called "engineering" production functions) but these results can shed no light on macroeconomic relationships.

The reason why the title of this paper is rather cryptic is that the present state of neoclassical growth theory is very similar to that of the theories of the motion of the planets in the seventeenth century. Although the correct geocentric theory was articulated in a convincing fashion by Aristarchus, circa 250 B.C., for the next two millennia the Ptolemaic theory persisted, with many astronomers devoting their life's work to it. Based on the then plausible assumption that the planets circled the earth in perfect cycles, the theory gave very accurate predictions through the use of epicycles. Any anomalies could be dealt with by the addition of yet further epicycles. It is instructive that a theory that is now so obviously wrong could have held sway for so long, but its excellent predictive ability may have had something to do with it (Koestler, 1958, Feyeraband, 1975). At least in the natural sciences there is a set of generally agreed criteria against which individual theories can eventually be judged and refuted. There are no longer any adherents to the Ptolemaic theory. The same cannot be said for the aggregate production function as the problem is that it can never be empirically refuted by way of the tests its defenders use. Consequently, its uncritical use is likely to persist for some time to come, although there are welcome signs that useful insights into the growth process are becoming available without recourse to both the aggregate production function and cross-country regressions using highly aggregative variables. Hidalgo, *et al.* (2007) for example, introduce the concept of product space to understand by analyzing the products that countries produce and export, their constraints to growth.. Different export products have different implications for future growth. The consequence is that the production of textiles, for example, requires capabilities very different from those required to produce electronics. The problem is that the capabilities required to produce textile cannot be easily redeployed to produce other products, and therefore, moving out of textiles is not easy.

As Samuelson (1966, p.583) eventually conceded in summing up the implications of the *Quarterly Journal of Economics* symposium on reswitching and capital reversing: “If all this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence. We must respect, and appraise, the facts of life”. We consider that this comment on the aggregate production function applies equally, if not more so, today as it did forty years ago.

Appendix: Cost functions and the Accounting Identity

We have shown how the existence of the underlying accounting identity means that we can always get a perfect fit to an aggregate production function and it is not surprising that the same applies to the cost function. The cost function shows how total costs vary as output varies i.e. $C = f(Q, w, r)$ were C is total costs. As the derivation is usually carried out at a particular point in time, the distinction between nominal costs and real costs is not normally made. The cost function is derived on the assumption that the firm chooses the optimum combination of the factors of productions given the relative factor prices. It is perhaps easiest to demonstrate the argument with respect to the Cobb-Douglas and its cost function.

As any standard microeconomics textbooks shows, the total cost function is obtained by maximising the output given by the production function, namely,

$$Y = AL^{\alpha}K^{\beta} \qquad \text{(A1)}$$

subject to the cost equation or accounting identity:

$$C = wL + rK \qquad \text{(A2)}$$

where C is assumed to be constant, i.e. the firm has a fixed budget to spend on both factors of production. (It can be argued that the optimisation is carried out with respect to the competitive rental price of capital rather than the *ex post* rate of profit. This does not significantly affect the argument. (See Felipe and McCombie, 2007.) This procedure is not seen as tautological because Y is assumed to be a homogeneous quantity, independent of the costs of production, although, of course, $Y = C$. Obtaining the first order conditions from the constrained maximisation, setting them equal to zero, and using equations (A1) and (A2) and some straightforward algebra, it may shown that the cost equation is given by:

$$C = A^{-1/(\alpha+\beta)}\left[\left(\frac{\alpha}{\beta}\right)^{\beta} + \left(\frac{\beta}{\alpha}\right)^{\alpha}\right]^{1/(\alpha+\beta)} w^{\alpha/(\alpha+\beta)} r^{\beta/(\alpha+\beta)} Y^{1/(\alpha+\beta)} \qquad \text{(A3)}$$

Thus, total costs depend upon the volume of output, the production coefficients, α, β and A and the prices of the factors of production, w and r. Equation (A3) is interpreted as a behavioural relationship as it can be used to estimate the degree of returns to scale. Moreover, like the aggregate production it is erroneously seen as a testable hypothesis because, according to this interpretation, if firms were not productively efficient, even if the production function were a Cobb-Douglas, the estimation could give a very poor statistical fit.

However, if value data is used for output, then it is straightforward to show that we have a tautology again. To see this, let us assume constant returns to scale so that $\alpha + \beta = 1$. Under these circumstances, the expression $[(\alpha/\beta)^{\beta}+(\beta/\alpha)^{\alpha}]$ in (A3) , is equal to $\alpha^{-\alpha}(1-\alpha)^{-(1-\alpha)}$ and equation (A3) becomes:

$$C = \alpha^{-\alpha}(1-\alpha)^{-(1-\alpha)}w^{\alpha}r^{(1-\alpha)}\ Y/A \qquad \text{(A4)}$$

As $Y/A = L^{\alpha}K^{(1-\alpha)}$, it follows that equation (A4) becomes

$$C \equiv Y \equiv \alpha^{-\alpha}(1-\alpha)^{-(1-\alpha)}w^{\alpha}r^{(1-\alpha)}L^{\alpha}K^{(1-\alpha)} \qquad \text{(A5)}$$

and $A \equiv \alpha^{-\alpha}(1-\alpha)^{-(1-\alpha)}w^{\alpha}r^{(1-\alpha)}$.

Equation (A5) is identical to the accounting identity given by equation (7) in the main text. Thus, if we use value data, equation (A4) is definitionally true and does not need to be derived by the optimising procedure outlined above. The reason why equation (A5), in logarithm form, is seen as a behavioural equation is that if time-series data are used and *A(t)*is proxied by a linear time trend, the statistical fit may be poor for reasons set out earlier in the paper. That is to say, it does not adequately capture the path of *alnw + (1-a)lnr.*

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