

To Measure or Not To Measure TFP Growth? A Reply to Mahadevan

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In a recent paper in this journal, Mahadevan (2003) discusses the pros and cons of estimating total factor productivity (TFP) growth.¹ She concludes that “the general contention is that past [total factor] productivity work is not completely futile” (Mahadevan, 2003, p. 375), and agrees with this sentiment, notwithstanding the various criticisms that have been raised about the whole notion of TFP. In the course of her discussion, she briefly summarizes what may be termed the “accounting identity” criticisms of the notions of aggregate production function and TFP growth put forward and elaborated by Felipe & McCombie (2003a).² Mahadevan seemingly dismisses our arguments on three grounds; none of them, we argue, valid.

First, she cites Jorgenson (1966), who explains that the “accounting identity is important in defining an appropriate method of measuring TFP and it provides a useful check on the consistency of any proposed definitions of total output and total input” (Mahadevan, 2003, p. 374). In other words, Mahadevan draws the inference that not only is our argument well known, but also that it does not say much.

Second, Mahadevan quotes with apparent approval Denison (1972) in claiming that the accounting identity from which we begin our argument does not hold in constant prices. Moreover, Denison postulates that TFP change is precisely a measure of the degree to which the identity does not hold. She does not, however, reconcile this with Jorgenson’s seemingly contradictory view cited above (i.e. both arguments cannot be used simultaneously as a critique of our comment). Nevertheless, the implication is that not only is our argument well known, but it is also incorrect.

Third, as a putative *coup de grâce*, she quotes Griliches & Mairesse (1997), who argue “that in most cases, the production function is estimated as a tool for answering questions which are too interesting to give up even though the framework used may be problematic”. In other words, all the problems surrounding the notion of aggregate production function must be brushed aside because the viability of the aggregate neo-classical research programme depends on this construct.

We believe that Mahadevan’s distorted account and evaluation of our arguments result from a series of serious misunderstandings. Moreover, her alleged critique does not address in any way the central issues we discuss in our (2003a) paper and their implications, and completely disregards the empirical evidence that we provide. Hence, we feel that is necessary to clarify the issues involved by way of a rebuttal.

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TFP growth is, by definition, the difference between the growth of output and that of the factor inputs, each weighted by its factor share. As a simple matter of definition, this is uncontroversial. Neo-classical economics relates this definition to the neo-classical theory of factor pricing and production. In the neo-classical growth accounting approach the assumption of perfectly competitive markets is usually invoked, with the implication that factors are paid their *aggregate* marginal products. This is done in order to provide a theory to justify the weighted growth of the factor inputs as a measure of their contribution to output growth. Thus, in these circumstances, TFP growth is the growth of output less the growth of the factor inputs weighted by their output elasticities, where the latter are equal to their factor shares.³ Mahadevan (2003) has an imprecise definition of TFP growth in her equation (1), namely “TFP Growth = Output Growth – Input Growth”.⁴ If there is more than one input, then the growth of the factor inputs must be weighted in some way. Neo-classical theory draws on production theory to provide a rationale for the choice of weights.

The *sine qua non* of this procedure at the aggregate level is the existence of a *well-behaved aggregate production function*. The growth accounting approach, unlike the direct estimation of an aggregate production function, does not require an explicit functional form to be specified. The need to postulate an aggregate production function (what is putatively being estimated) is where the problem with the neo-classical approach begins.

Two strands of literature, namely, the Cambridge Capital Theory Controversies and the aggregation literature, showed long ago that, for all practical (and, indeed, theoretical) purposes, aggregate production functions do not exist. This work has recently been summarized and discussed by Cohen & Harcourt (2003) and Felipe & Fisher (2003). On the latter work, Mahadevan (2003, p. 375) seems to believe (otherwise why mention it) that Nataf’s aggregation condition over sectors, i.e. micro production functions additively separable in capital and labour, solves or lessens the aggregation problem. This is a misunderstanding of the aggregation literature. In their recent survey, Felipe & Fisher (2003, p. 227) indicate: “Taken at face value, Nataf’s theorem essentially indicates that aggregate production functions almost never exist”. Indeed, this condition is extremely restrictive. The reason is that it fails to impose an efficiency condition, which is what is needed to derive an aggregate production function with neo-classical properties.⁵

In a series of papers (see references below) we have discussed why economists continue to use aggregate production functions despite the fact that they lack any theoretical foundation. We concluded that the only reason is that, when estimated econometrically, they seem to work (at least at times) in the sense that they yield estimates that are plausible (e.g. the estimated output elasticities are close to the factor shares in national income).⁶ But it is a *non sequitur* to argue that just because the putative aggregate production function works empirically it must necessarily exist. Hence, we considered whether or not there is any explanation for why aggregate production functions seem to work in empirical work, but which does *not* presuppose the existence of the aggregate production function. The answer lies in the existence of an income accounting identity that relates real value added (V) to the sum of the wage bill (W) plus total profits (R). This is to say, $V \equiv W + R \equiv wL + rK$, where w is the average real wage rate, L is the labour input, r is the average profit rate and K is the stock of capital.⁷ This can always be rewritten as $V = F(K, L, t)$, where t is time, a proxy in the neo-classical schema for the level of technology, or, more restrictively, as $V = A(t)f(K, L)$, where $A(t)$ is a function of time, not necessarily an exponential time trend. For example, let us assume that factor shares are roughly constant because firms

adopt a constant mark-up pricing policy, and that the sum of the growth of the wage rate and the rate of profit, each weighed by its factor share, is constant. Both of these may be regarded as “stylized facts”. It may then be simply shown that using these data the Cobb–Douglas relationship gives a perfect approximation to the accounting identity, *even though no aggregate production function actually exists*. The result is, in fact, more general than this. The function $F(K, L, t)$ can take any of the standard forms (CES, translog, etc.), depending on the empirical path of the data.⁸

The important point of our argument is that the expression $V = F(K, L, t)$ is simply another way of writing the accounting identity. This does not mean that we argue that aggregate production functions “are derived from an income accounting identity” as Mahadevan (2003, p. 374) erroneously interprets the argument. The expression $V = F(K, L, t)$, when estimated statistically, will have a very high fit (potentially an R^2 of unity and standard errors tending to zero) and will yield estimates of the elasticities equal to the factor shares, which may incorrectly be interpreted as excellent results from an economic and a statistical point of view. The problem with most empirical estimations, as we explain in our paper (Felipe & McCombie, 2003a), is that the rate of technical progress or, more accurately, the growth of TFP or the “residual”, is proxied by a linear time trend when the “production function” is estimated in log-level form.⁹ This often gives implausible time-series estimates of the output elasticities, with capital’s elasticity sometimes taking a negative value. We show that this is due to the fact that the linear time trend provides a poor proxy for the weighted log of the wage and rate of profit, and suggest ways to overcome this problem. However, once it is solved, every time an aggregate production function is estimated, the result will be the same as stated above, namely, that the putative output elasticities will equal the relevant factor shares (which would be interpreted as validating or, strictly speaking, not refuting, the marginal productivity theory of factor pricing). Consequently, the shares will always add up to unity, implying constant returns to scale. Surely there is something wrong here?

It is well known that neo-classical growth accounting studies consider the income accounting identity, as we indicate in Felipe & McCombie (2003a, p. 700). But the neo-classical argument is very different from the argument that we have advanced. Starting with the assumption of a well-behaved aggregate production function, this approach further assumes that the marginal productivity theory of factor pricing (with all the other associated assumptions) holds. Through the use of Euler’s theorem, it is shown that there will be an associated accounting identity where the wage and profit rates are equal to the marginal products of labour and capital. This is what provides the justification for the assumption that the factor shares equal the output elasticities, and that the growth of TFP is measured by the sum of the growth rates of the wage and profit rates, each weighted by its factor share (the neo-classical dual measure of TFP growth).

Mahadevan misunderstands our arguments at this point on two counts. First, we do not claim, as she implicitly indicates, that TFP growth cannot be derived from aggregate production functions, should they exist, or that TFP growth measurements are not valid because they are simultaneously derived from identities. What we question is that as neo-classical aggregate production functions cannot theoretically exist, on what grounds is the difference between the growth of value added and the growth of the factor inputs, weighted by their factor shares, a measure of the rate of technological progress? Therefore, the link that exists in neo-classical economics between the aggregate production function and the income accounting identity at the aggregate level is fictitious, and we question it. The neo-classical measure of TFP growth is equal to the weighted average of the wage and profit rates. The neo-classical exercise is misleading in the sense that the same result can be obtained through the identity, *without recourse*

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to any theory or set of assumptions (Felipe & McCombie, 2003a, pp. 704–705). The results follow tautologically. Neo-classical economics, as we noted above, in its derivation of TFP growth needs to assume that the aggregate production function exists, that firms are profit maximizers and that markets are competitive; but these hypotheses cannot be tested because they cannot be refuted (on this see the empirical evidence with data for Singapore discussed in Felipe & McCombie (2003a), and for China in Felipe & McCombie (2002a)). What then is the point of the neo-classical exercise?

Second, what the neo-classical approach purports to do, and this is the crux of the disagreement, is to argue that the aggregate production function, together with the conditions for producer equilibrium, provides a theory of national accounts, i.e. the identity. This is what underlies Jorgenson's (1966) reference to the accounting identity. This theory is based on an aggregate production function with constant returns to scale and the assumption that each factor is paid its marginal product. We have shown that this is problematic because such a theory cannot be tested and potentially refuted. A theory that cannot be refuted cannot be viewed as a scientific theory. It is self-evident that the neo-classical reference to the identity has nothing to do with transforming the accounting identity into a form that resembles an aggregate production function (with the usual neo-classical properties), which is the heart of our argument. Therefore, the fact that neo-classical economists are aware of the existence of the accounting identity does not imply that our argument is taken into account or well known, much less refuted.

Mahadevan's paper contains another important misunderstanding. She quotes with approval Denison (1972, p. 100) in claiming that the accounting identity from which we begin our argument, that is, $V = wL + rK$, does not hold in constant prices at factor cost.¹⁰ However, Mahadevan provides no explanation as to why this is the case. Moreover, to argue this is effectively to invalidate the neo-classical growth accounting methodology, as both the primal and the dual approaches require consistent data sets to hold in constant prices. This is implicit in Jorgensen & Griliches's (1967) paper. Hsieh (2002), for example, calculates the dual directly from the accounting identity in constant prices.¹¹ An introductory discussion to the use of the accounting identity in constant prices is to be found in Barro (1999). In our theoretical discussion, we use the GDP deflator as other neo-classical economists do (see, e.g. Fernald & Neiman, 2003, pp. 3–4).¹²

The identity does hold in constant prices. We argue that Denison had, in fact, another argument in mind. To infer from this that the accounting identity in constant prices, as used by us and by the growth accounting practitioners, does not exist, rests on simple confusion. As this is one of the central tenets in Mahadevan's critique of our argument, it is worth discussing it further.

In our view, Denison chose an unorthodox way to consider the accounting identity, one where he treats the wage and profit rates similar to output prices. Consequently, his interpretation of the identity at constant prices is to hold wages and the profit rate constant at their initial values, that is, $V_t = w_0^c L_t + r_0^c K_t$, where the subscripts 0 and 1 denote the base year and period 1, respectively, and the superscript c denotes current values. The conventional identity for value added, on the other hand, is given by $V_t = (V_t^c/P_t) = \sum p_{i0} Q_{it} = (w_t^c/P_t)L_t + (r_t^c/P_t)K_t$, where P_t is the value of the price deflator at time 1 and p_{i0} denotes the base year prices of the physical outputs given by Q_{i0} . Consequently, it is readily apparent that $V_t = (V_t^c/P_t) = \sum p_{i0} Q_{it} \neq V_t = w_0^c L_t + r_0^c K_t$.

It is true that in Denison's identity, when expressed in growth rates, there is no residual, i.e. "his" identity, in growth rates is given by $\hat{V} = a\hat{L} + (1-a)\hat{K}$, where the circumflex denotes a proportional growth rate and a and $(1-a)$ are the shares of labor and capital in total output, respectively. In the conventional identity,

however, the residual is the weighted average of the growth rates of the wage and profit rates, as the identity is, in these circumstances, given by $\hat{V} \equiv a(\hat{w}^e - \hat{P}) + (1-a)(\hat{r}^e - \hat{P}) + a\hat{L} + (1-a)\hat{K}$. The expression $a(\hat{w}^e - \hat{P}) + (1-a)(\hat{r}^e - \hat{P})$ is the degree to which Denison's "identity" and the conventional identity do not coincide. Consequently, Denison's assertion that the identity does not hold in constant prices, while correct in his own terms, has no relevance for either the growth accounting approach or our critique. It is simply due to the unconventional way that Denison defines the identity in constant prices.

Finally, appealing to Griliches & Mairesse's (1997) argument that the "production function is estimated as a tool for answering questions which are too interesting to give up even though the framework used may be problematic" (Mahadevan, 2003, p. 374) is simply an exercise in bad methodology. On this argument, Felipe & Fisher (2003, p. 250) observed that it is not true that mermaids exist simply because one insists on studying them! The theory and questions Griliches and Mairesse refer to are interlinked and are part of the Kuhnian neo-classical paradigm. But while there are logical reasons for initially persisting with a theory in the face of empirical difficulties, there is no justification for the use of a theory that cannot be refuted. Moreover, persisting with a theory that is false or unverifiable can have damaging consequences. For example, in another context, this attitude delayed the understanding of the motions of the planets for over 2000 years. It resulted in the perpetuation of the Ptolemaic geocentric model of the movement of the planets long after Aristarchus, circa 250 BC, first questioned this system and put forward the alternative and correct heliocentric theory. This was partly due to the fact that by the use of epicycle after epicycle to describe the motions of the planets (although the epicycle is a totally artificial construct), the Ptolemaic system produced very good predictions. But prediction is not the same as explanation.

To conclude, we trust that this clarifies our argument concerning the measurement of TFP growth. We remain convinced that estimating aggregate production functions does not tell us anything about the underlying technology of the economy; hence the concept of TFP growth, which rests on the notion of aggregate production function for its justification, is problematic. Mahadevan concludes that today "we know more about the nature of *productivity* than we did a decade ago. The use of TFP is appealing in that evaluating TFP growth often has policy formulation as the ultimate objective" (Mahadevan, 2003, p. 375, italics added). The fact that a myriad of studies have been published on TFP growth does not mean that we know more today about "productivity" than a decade ago.¹³ In fact, the confusion about TFP growth and its alleged determinants (e.g. in the discussion about the sources of growth in East Asia; see Felipe (1999)) remains today as much as 15 years ago when Griliches (1988, p. 363) claimed that "despite all this work [on TFP], there is still no general agreement on what the computed productivity measures actually measure, how they are to be interpreted and what are the major sources of their fluctuations and growth". Hence, using TFP for policy purposes is dangerous.

In our opinion, the answer to the presumably rhetorical question posed in the title of Mahadevan's paper, namely, "To measure or not to measure TFP growth?", remains an unequivocal "No".

Notes

1. We have benefited from seeing a response from Mahadevan to our first draft of this comment.
2. Felipe (1999, 2000, 2001a, b), Felipe & Holz (2001), Felipe & McCombie (1999, 2001, 2002a, b, 2003b, c) and McCombie (1998, 2000-2001, 2001) offer comprehensive discussions of the issues involved.

3. The rate of TFP growth can be determined by the time trend in the estimation of a production function. This method provides estimates of the output elasticities rather than assuming they are equal to the factor shares. However, this approach suffers from the same limitations as the neo-classical growth accounting approach because of the underlying accounting identity (Felipe & McCombie, 2003a). This is true of any approach that uses value data to determine the putative technological relationship between inputs and outputs at the aggregate level, such as data envelopment analysis. Thus, our critique applies to any procedure that uses value data and that attempts to measure TFP and give it an interpretation using aggregate production theory. The early productivity studies did calculate indices of TFP without any theoretical rationale. However, since Solow's (1957) seminal paper, the neo-classical approach has based the interpretation of TFP within the realm of production theory and the use of the aggregate production function.
4. Labour productivity does not require any weighting, and it is not a controversial concept of productivity, but this is not the concept being discussed here.
5. The same goes for the reference to Star's paper (Mahadevan, 2003, p. 375). Star does not provide any aggregation theorem. He mentions, though not with precision, some condition regarding the marginal rate of substitution. On this, the closest thing that comes to our mind is Leontief's theorem. See Felipe & Fisher (2003, pp. 223–224).
6. This is the instrumentalist justification for the use of the aggregate production function put forward especially by Solow in the 1950s and 1960s.
7. The argument applies equally to the use of gross output.
8. Mahadevan correctly points out that we have less of a problem with the estimation of production functions using physical data, where output and labour and capital are measured in homogeneous physical units; but such data are exceedingly scarce (just think of the vast data requirements for specifying capital goods and structures as different homogeneous units). The estimation of the production function with physical data is still not without its problems (Felipe, 2001a; McCombie, 2001).
9. The neo-classical approach also includes other factors, such as measurement errors, etc. Hence, the preferred use of the term TFP growth or "residual".
10. The identity that appears in the neo-classical theory of production is $V = wL + p_K K + \pi$, where p_K is the rental price of capital and π denotes economic profits. The latter will be zero if markets are competitive. This has led to a discussion in the growth accounting literature about the circumstances when estimates of TFP growth from the primal and the dual differ, as well as about the estimation of TFP growth when markets are imperfect (e.g. Fernald & Neiman, 2003). This does not affect our critique, and since Mahadevan has not discussed it, we shall not pursue it here. On this see Felipe (2001a) and Felipe & McCombie (2002b, 2003c).
11. See, e.g. Jorgenson's KLEM data set (<http://post.economics.harvard.edu/faculty/jorgenson/data/35klem.html>) for a consistent set of data in constant prices.
12. That we mention these authors does not mean that we agree with their arguments. These references are provided simply to stress the point that the identity does hold in constant prices, and that it is the starting point of the neo-classical analysis of TFP. The use of the same or different deflators on both sides goes back to the old discussions in the literature about the single versus double deflation methods to derive value added. This is a non-issue and certainly it was not Denison's argument.
13. Mahadevan is clearly associating here productivity with TFP, and it is erroneous. In our papers we have argued that labour productivity is a completely different notion of productivity from TFP, and that the former is the concept that should be measured, studied and discussed. It should also be the object of economic policy.

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