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The aggregate production function and the measurement of technical change: 'not even wrong', by Jesus Felipe and John S.L. McCombie

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Yet, in spite of some drawbacks, this book is an interesting reading. It convinced me to make sure not to miss the next issue of the Survey; and perhaps this is what its authors ultimately wanted to achieve.

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The aggregate production function and the measurement of technical change: 'not even wrong', by Jesus Felipe and John S.L. McCombie, Cheltenham, Edward Elgar, 2013, vii + 388 pp., ISBN 978 1 84064 255 1

The thesis of this book is startlingly simple – all empirical growth theory based on aggregate production functions (APF hereafter) is flawed because all estimates of parameters such as the elasticity of substitution are determined by an accounting identity. It follows that estimates of concepts derived from the APF, such as total factor productivity (TFP hereafter), are now placed in doubt. Here is the argument in a nutshell: suppose Y, L and K are three variables whose measured values are linked by an accounting identity. Can we engage in some clever econometrics that somehow avoids this identity? If, necessarily,

$$Y \equiv wL + rK \tag{1}$$

for all measured values of Y, L and K, and where $\equiv$ denotes that expression (1) is an identity, then any attempt to estimate $Y = F(L, K)$ can always be written as

$$Y \equiv F(L, K) + [wL + rK - F(L, K)]. \tag{2}$$

Note that Equation (2) is only the identity rewritten. After all, the terms in $F(L, K)$ cancel out in Equation (2); therefore, we are left only with Equation (1). If the term in [] can be made 'small', Equation (2) may well approximate some tractable statistical form, and we will end up with an estimate of $Y = F(L, K)$. Since Equation (2) is an identity, econometric 'estimates' may be just a reflection of the identity and not represent anything real. How do we escape this uncertainty?

Jesus Felipe and John McCombie, hereafter FM, have been irritated by this question for over 25 years. They did not begin the fight, but they do want to finish it. The economics profession can escape the accusation of intellectual hollowness only by denying Equation (1) or the 'statistical form' of Equation (2), or by saying that the formulations involving Equations (1) and (2) do not arise in cross-section data, or in time-series data or panel data. FM have left no stone unturned. They have refused to be deterred by the obduracy of the economics profession and are to be congratulated for having laid out their case completely for the public. For decades, they have been tracking every variant of response by the profession and shown the validity of their accusation for each response.

After the Introduction, which actually summarises the contents of the book, Chapter 1 briefly lays out the reasons not to believe in the existence of an aggregate production function, or APF, i.e., the insurmountable aggregation problems studied in detail by Franklin Fisher and others. This is the starting point of the FM thesis: if APFs cannot really be derived theoretically, how are we to interpret empirical estimates? This is perhaps the least readable chapter in the book; FM would have been better served to state why neoclassical economics, hereafter necon, needs an APF to talk about problems of growth, how every attempt to make its existence theoretically rigorous has led to disappointment, and why the use of an APF to 'justify' an aggregate theory of distribution between wages and profits floundered after the Cambridge controversies of the 1960s. Chapter 2 states the main issues clearly and forcefully and provides a list of questions and answers (Q&A's) to clarify common misconceptions of their thesis, which I will refer to as the accounting identity critique. The argument is then supplemented in Chapter 3 by simulation studies which address some of the objections to FM; these simulations show that all that is being estimated is indeed the identity. Chapters 4, 5 and 12 are somewhat more historically inclined than the others. They discuss the original Cobb–Douglas, hereafter CD, paper on aggregate production functions, the Solow model of TFP and the reception of the accounting critique by critics, especially Temple. Chapter 6 shows why, given that APFs cannot be estimated, TFP calculations cannot be interpreted as measures of true productivity. Chapter 7 shows the problems with the use of the Solow model by Mankiw, Romer and Weil to explain economic growth across the world, as well as estimates of convergence. Chapter 8 questions Feder's attempt to explain export-oriented growth because his model is also a version of the accounting identity. Chapter 9 discusses why estimates of endogenous growth models suffer from the same problems. Chapter 10 questions tests of the hypothesis that markets are competitive, when the tests are derived from estimates of production functions. The reason is that the same regressions can also be derived from the identity. Finally, the estimation of neoclassical labour demand functions is also problematic because the same relationship, i.e., the labour demand function, is embedded in the accounting identity.

One could spend much time on the unfolding history of this question. How Simon and Levy did the right mathematics in 1963 but interpreted the accounting identity as a linear production function. Simon elaborated the point in 1979 and further in his Nobel lecture (how was this missed by the profession?); how Shaikh struck a dagger at the edifice in 1974 and in 1980 but was ignored and Solow's 1974 and 1987 replies never gave a clear response to the critique – indeed, Solow's general pronouncements are full of wisdom, so one can only regret that his most quoted scientific contributions are so questionable; how Samuelson practically washes his hands off the entire program of APF and TFP in his eulogy of Paul Douglas in 1979 ... and so on. However, that will distract from the more important message that something is really wrong with the literatures on APFs and TFP. Therefore, let me state the issues as simply and clearly as I can so that the reader can judge whether the issue is worth a closer investigation.

The object of the exercise is to get from the knowns to the unknowns. The known are Y_i, L_i, K_i, w and r. From these we can compute various ratios such as those for capital output, share of wages, etc. The only 'theory' involved is arithmetic and untutored curiosity. At this point, our computations may show up some unexpected regularities. Kaldor first observed that several such stable numbers did exist and popularised these regularities as the 'stylised facts' that growth theory should try to make sense of. Accepting all this as background how much further can we proceed?

There are two widely used concepts, the production function and TFP. Since they are so ubiquitous, and they are being challenged, the concepts need clarification. Since TFP is usually derived from the production function, the latter will be discussed first. If we use labour and tools to dig up the soil for a garden we are 'producing' a flower bed. What the production function is supposed to do is to tell us how many hours of labour, L , combined with various amounts of tools, K, will produce how many square feet of flower bed, denoted Y . Since we can be wasteful in our work, the production function only tells us what will be produced if all inputs are efficiently used. This is primarily an engineering concept, *not* an economic one, and it can be denoted by $Y = F(L, K)$. If we could get data on Y , L and K in physical units, *and* if we agreed that these represented efficient production, then a scatter plot of $(Y,\ L,\ K)$ or its econometric, equivalent, would produce a production function.

In the world of the policy economist, this is *never* the case. We do not obtain data on physical units; instead, all our data come from the accounting identity which link the value of total output with the value of all inputs. If V denotes value-added, w the wage rate and r the return on capital, then the equation which actually provides the data being used are as follows:

$$V \equiv wL + rK. \tag{3}$$

This is an accounting identity, hereafter denoted as AC, which may be taken as defining V . It is this V that serves as the proxy for the Y that we actually want. Note that, in the data we use, both L and K are not physical measures but aggregate value measures; but this will not be emphasised in what follows. I continue using L and K so as not to complicate the notation, even though FM make this last point clear and denote the value measure of the capital stock as J . The distinction between the numbers we can obtain, and the numbers we wish we could obtain, turns out to be of great importance.

There are now two relations linking (Y, L, K). The production function $Y = F(K, L)$ and the accounting identity $V \equiv wL + rK$. How do we know which we are estimating? If the production function were linear in L and K, say $Y = aL + bK$, then few would doubt that a must equal w and b equal r. After all, an identity should trump an equation.

What if $Y = F(L, K) = AL^aK^b$, the ubiquitous CD? Now it is not so clear how to relate the production function and the identity. Does the different equational form only disguise the accounting identity or does it make the identity disappear? The important, and central, claim of FM is that estimates of CD are just estimates of AC $-$ the production function can be transformed to provide a local approximation to the accounting identity. This is a mathematical point; it is either valid or invalid. Hitherto, no one has challenged the mathematics of FM.

However, if the approximation claimed by FM is valid, we can turn the usual argument, so to speak, on its head. The identity *must* be upheld, the technological relation *may* be upheld. It follows that when we believe we are estimating a production function, we are really only using the production function as an approximation to the accounting identity. As a consequence, all the production estimates provided in the literature may be good for other reasons, but are dubious indications of actual production possibilities.

The issue really is that serious. It was adumbrated as a problem by Phelps-Brown, then proposed more seriously by Simon and then by Samuelson. It has been well said by Whitehead that every good idea was first stated by someone who did not recognise its importance; it has considerable truth even in this case. FM have recognised the fundamental nature of the critique and pressed home the argument against every objection that

has appeared. The comprehensiveness of their argument is due in equal parts to the proliferation of studies which simply assume the validity of the aggregate production function, hence providing FM with additional material, and partly to the their tenacity in seeking clarity on basic questions.

Let AC and CD represent the two equations. We wish to show that when CD is applied to the data, it actually approximates AC. Using $d()$ for the differential of the item in (),

$$d(\text{AC}) = dw.L + dr.K + w.dL + r.dK$$
$$d(\text{CD}) = mpL.dL + mpK.dK, \text{ approximately}$$

if $w = mpL$ and $r = mpK$, then we get

$$d(\text{AC}) = dw.L + dr.K + d(\text{CD}), \text{ or}$$
$$d(\text{AC}) - d(\text{CD}) = dw.L + dr.K,$$

and the approximation will be good whenever dw and dr are 'small'.

Note that the argument does not require that $w = mpL$, etc. It holds as long as $d(\text{output}) = w.dL + r.dK$, remembering that output is measured as values and that the right hand side (RHS) is the value of incremental inputs measured at current factor returns; this is quite likely. FM provide several alternative ways to get this approximation; therefore, the unhappy reader can readily consult the book for a better argument.

The reader will have noticed that the essence of the critique derives from our need to use value data instead of physical data. As such, the questions raised apply to microeconomic as well as macroeconomic production functions estimated with value data. FM in fact provide an econometric illustration with data from the Indian cotton industry. Alternatively, the reader can wonder if using data generated by a production function with wrong values can still generate a 'fit' with economically plausible estimates for a and b. FM provide an example of this too. What about those cases where the estimated aggregate production function provides a poor fit? FM use the approximating formula to predict those cases where the regression is likely to show poor results. The example chosen is the now famous Mankiw−Romer−Weil paper which purported to 'test' the Solow model with a wide span of international data.

Perhaps, the real problem lies on the use of the CD production function. More complicated production functions may well escape this critique. Apart from the fact that this response implicitly dismisses the vast majority of empirical growth theory, the escape to the 'next' complication, the constant elasticity of substitution (CES) and translog production functions, does not get us out of the quicksand because FM show that a Box−Cox transformation of the accounting identity will get us to the CES. Also, the translog is a yet more complicated approximation to the accounting identity. In short, FM have considered all variety of objections to their thesis − that the accounting identity suffices to explain all the plausible results of empirical macro and growth − and shown that these objections are not compelling. What more would one have them do?

In concluding this section of the review, I want to provide two caveats. First, regardless of the equation chosen to represent the APF, if one believes it to be true and one accepts that competition prevails, then any differentiable function will of course have a tangent plane and the slopes along the L and K axes will be w and r, respectively. FM's response to this point on p. 75 is too strong, as it fails to concede that two equivalent

explanations exist and one must look further to discriminate. Second, I want to state that growth theory cannot escape from the necessity of having *some* relationship that expresses next year's output as a function of next year's capital and labour. Today's macroeconomic questions may be able to evade this, but when we look to tomorrow, how can we begin to talk about the problems of consumption and distribution if we have no idea of the magnitude of the output? Economists are faced with the dilemma that the entities they need for coherence, in this case an aggregate 'production function', may have no assignable empirical counterpart.

This last bears on other interesting issues, such as whether macro-concepts have a reality independent of the micro-entities they are named after. Given that we have used our national accounts to produce numbers like Y (V), L and K, what properties can be assumed of them? Just because we call them output, labour and capital, are they obligated to behave like the micro-entities we have named them after? Or are these 'emergent' entities, whose interrelationships may be more or less than those of their namesakes? If we could derive macro-properties from the micro-ones, – deriving a macro-production function for the entire economy from the complex multitude of micro-production functions littering the economy – there would be little argument about this being the best way forward. However, that door has long since closed. No clear path forward emerges.

This leads to TFP, the second major concept attacked by FM – what is TFP? The puzzle it seeks to solve is a very natural one. Given an increase in GDP, ΔY (now measured as ΔV), how can we explain what caused this increase? The obvious way is by deducting from ΔY those parts that seem to need no explanation, such as the increase due to more labour ΔL, or that due to additional capital ΔK (which in reality is also a value measure due to the fact that use aggregate data). The 'explained' parts are thus those due to ΔL and ΔK, and the remainder is what needs investigation. Since ΔY represents more goods, while ΔL and ΔK represent more workers and machines, respectively, we cannot do any arithmetic without converting ΔL and ΔK into units comparable to ΔY. Let a and b represent the coefficients that, respectively, convert labour and capital into output. Therefore, our puzzle is the number $\Delta R = \Delta Y - a\Delta L - b\Delta K$.

The reasoning presented above for macro-entities has its plausibility by transfer. If we were digging ditches with spades and we noticed that more ditches were dug this year, we would want to know why. We did hire more workers and use more spades, denoted by Δw and Δs. After subtracting from the extra volume of ditch, Δd , the output of the additional workers and spades, we are left with $\Delta r = \Delta d - a\Delta w - b\Delta s$, where a and b now indicate the productivity of workers and spades, respectively. Δr may be positive, negative or zero. If it is positive, then we have been unable to account for the additional Δd by claiming that it was due to more workers or more spades. Δr is then a measure of our increased efficiency at digging ditches.

The above is such transparent reasoning that we would like it to apply to every case of interest. The transfer of logic is anything but simple. It is not the explicit reasoning that causes difficulties in such transfer, but the implicit one. In seeking to explain GDP growth with such a calculation, we are assuming that we are manipulating numbers Y, L and K, which have the same reality as d, w and s. However, that is simply not so. Y, L and K are numbers indicating economic aggregates with no palpable goods, labour or machines which correspond. Can there be any transfer of plausibility when the same procedure is applied to notional entities like Y, L and K as to real ones like d, w and s?

The question defies a simple answer. Much depends on the level of abstraction of the notional entities. Are we adding together arranges and applies to form 'fruit', or oranges, apples and broccoli to form 'produce', or produce and meat to form 'meals', or meals,

laundry and furniture to form 'household expenses'; similarly, are we combining the work of ditch digging with spades, carpentry with saws and metal work with hammers, where the combination has elements of skilled labour and simple tools – or are we adding to these teaching, opera and accounting to form a more comprehensive figure for labour? As we move toward the more comprehensive entities, Y, L and K, our intuition fails us and we find the logic of our argument forces us to treat Y, L and K with the same respect that we give apples, sweat and spades.

This is a very uncomfortable intellectual position. Only an overdose of humility can rescue us. It is only by patiently elaborating upon all the conceptual difficulties, noting the extent to which each step up the conceptual ladder makes additional fuzz, which we can honestly face our interlocutors. How can we engaged in this plethora of caveats and still give policy advice with a straight face? That is the question.

Common speech adjusts its precision when we move from 'apples' to 'trust' to 'food' to 'expenses' to 'consumption' – but the numbers we use in every case are still the same old numbers. One would like each integer to acquire some fuzz as we go from 'apples' to 'fruits' and some more tentacles as we denote 'food' and so on, until our numbers are really hairy balls. Language adjusts and we silently follow Aristotle's dictum that it is the work of an educated man not to expect more precision from a subject than it is capable of. But also, no such adjustment occurs in the calculations of macroeconomic entities. Fisher et al. advised us to handle macro-concepts much in the manner of a garbage-man handling suspicious waste – 'very gingerly'. However, what can this mean in practice? Are we to hold our noses and wave a fan when talking to policy-makers?

We have to return to face our misery. We need to use $\Delta R = \Delta Y - a\Delta L - b\Delta K$, which, upon multiplying and dividing by Y, can be rewritten as follows: $sR\%\Delta R = \%\Delta Y - sL\%\Delta L - sK\%\Delta K$, where sG indicates the share of G in Y and $\%\Delta z$ means the per cent change in z. Or, in differential form,

$$\frac{dR}{R}sR = \frac{dY}{Y} - sL\frac{dL}{L} - sK\frac{dK}{K}.$$

Digressing slightly, is there a macro-reality corresponding to this equation? Mathematically, if we take it as a Kaldorian stylised fact that the G 's are constant, the macro-reality can only be some variant of the logarithmic function popularly called a CD. In this important sense, the entire literature surrounding the APF basis of TFP is wedded to the constancy of sR, sL, sK, a and b .

Now that the problem has been posed, we need to ask about any difficulties that may arise in the execution. We need numbers to represent, say V, L and K; these numbers need to be independently derived for L and K, and they also need to be linked to V only through the production function. This point bears emphasis as it is the heart of the objection, noted by several, but fully elaborated upon only by FM. Our standing hypothesis is that L and K combine through the production technology, denoted by $F(K,\ L)$, to produce Y. If the data on L, K and Y are also linked together by some other relation – and the claim is that they are necessarily so linked them we may have no way of extracting the production relation from the other one. FM provide a detailed account of the manner in which AC tracks all measures of TFP. I will not lengthen this review with their details – again, the mathematics has not been questioned. Therefore, the accounting critique still applies.

The second line of attack upon the numbers used to represent Y, L and K lies in the distance our computed Y, L and K have from their original inspiration, d, w and s. We are convinced that there are diamonds in our carbon formation, but the only process available to

'extract' the diamond may be one that is only capable of producing lead. How valid is it to still speak of the output we do obtain as 'diamond'? Zvi Griliches objected, politely but strenuously, to such mutilation of language. Two excellent accounts of the practical limitations of such TFP measures are *Asia-Pacific Economic Literature* (Chen 1997) and a recent National Bureau of Economic Research (NBER) paper of Hulten (2009). Those who insist on engaging in calculations with such numerical constructs – it is an abuse of language to call them numbers, perhaps one should refer to 'numcons', at least it rhymes with 'necons' – should face the obligation to give us the margin of error accompanying each numcon. If we are arguing about, say growth rates that differ by 1%, and the numcons have errors which can be as high as 5%, then our arguments about growth rates are worthless.

Such a pragmatic use of TFP will proceed at several levels. Does the accounting critique overturn any of our procedures? If not, will our numbers withstand all the conceptual measurement issues elaborated upon by Griliches, Chen and Hulten? Are there credible non-numerical factors that could overturn the findings, such as the empirical economics of management, which was once called *X*-efficiency? The TFP framework may be mis-specified in assuming labour and capital to be independently variable; has the displacement of the assembly line by the Toyota process suggests this? Policy-makers will act as they will, but our responsibility is informed advice.

A final caveat on TFP. The concept links conceptually with the APF. However, there is no necessity to this link. We can talk about productivity changes without an APF just by using the common-sense formulation elaborated upon earlier – how much output change can be explained by changed inputs? Also, if we ensure our humility by keeping in mind all the caveats suggested in the literature – dealing fairly with the points discussed in the paragraph just before this one, we can do so honestly. Too much ink has been spent on finding a version of TFP compatible with differentiability. It is extremely convenient to apply the calculus but the world is not obliged to make itself differentiable for our sakes. Thus, for a time series, differentiability simply means that we can seamlessly and smoothly stitch one point of time to the next – but do we need to restrict ourselves to such a reality? It is better to say 'bye' to line integrals and integrating factors.

A brief conclusion. This book carries an important message. It is detailed and thorough. The authors have made every effort to follow the norms of sound intellectual endeavour. It deserves a clear reply. In view of the respect given to 'Western economics' all over the Asia-Pacific, think of the furore caused by Krugman's carefully crafted but worthless article on '*The Myth of Asia's Economic Miracle'*, readers of *Journal of the Asia Pacific Economy (JAPE)* are asked to place this book on their 'must read' list.

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