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## Article

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## **‘I think there’s absolutely no way out for them: an aggregate production function does not make any sense at all!’**



Interview with John McCombie

John McCombie is Professor of Regional and Applied Economics in the Department of Land Economy at the University of Cambridge, UK. He is the Director of the Cambridge Centre for Economic and Public Policy (CCEPP), and a Fellow in Economics at Downing College, Cambridge. He obtained an MA in geography at McMaster University, Canada, and an MA and PhD in economics at the University of Cambridge. He is a Fellow of the Regional Studies Association and a Member of the Academy of Social Sciences. With Jesus Felipe, he has recently published a book, *The Aggregate Production Function and the Measurement of Technical Change: ‘Not Even Wrong’* (Edward Elgar, 2013), which is the culmination of many years of joint research on the topic.

*How did you come to economics and to Keynesian or post-Keynesian economics in particular?*

I came by a very indirect route because I started off as an economic geographer, and I went to Cambridge University where I initially read geography for 2 years. I thought that if I wanted to take economic geography seriously, I needed some formal economics training. So I changed to do 2 years of economics to complete my undergraduate degree.

*When was this?*

This was 1969–1973. Joan Robinson had just retired, and Nicky Kaldor was about to retire, but he was still lecturing in his inimitable fashion. At this time there was a distinctive ‘Cambridge economics’ that was very different from what was taught in other British (and US) universities. After graduating I went on a Commonwealth Scholarship to McMaster University in Canada and reverted back to geography to do my Masters. This was in 1974–1975. I went there because at the time economic geography was going through what was seen as a quantitative revolution. It was beginning to use extensively statistical and mathematical methods. McMaster had a couple of leading scholars there. I then had a very difficult decision: I had the choice of coming back either to the geography department at Cambridge or the economics department. In the end, I saw myself more as an economist, so I returned to do my PhD in economics. Bob Rowthorn agreed to be my supervisor, and I undertook research on the Verdoorn Law for my PhD, which was on the postwar economic growth in the advanced countries. I think my interest in post-Keynesian macroeconomics developed from that point. I should, however, say that as an undergraduate I had been fortunate enough to have listened to a thought-provoking series of lectures by Kaldor on growth. This also stimulated my interest in the subject. So I

straddled the area between regional economics and growth economics. My first appointment was actually as an urban and regional economist at the University of Hull.

*Then you went to Australia?*

Yes, I moved to Hull University (UK) in 1977 and then in 1981–1982 there was a major economic crisis in the UK economy. A result of this was that the Thatcher government started severely cutting funding for the universities. Hull was one of the three or four universities that were most heavily cut, and mobility between universities froze completely. So when the opportunity arose, I took leave of absence from Hull and went to the University of Melbourne on a short-term contract of 3 years. That move was also productive because I met Robert Dixon there, which further stimulated my interest in post-Keynesian economics. I returned to the University of Hull, but only stayed there for a further couple of years before taking up a post at the University of Cambridge in the Department of Land Economy in 1989. Land Economy is an interdisciplinary department, which is separate from the Faculty of Economics, and it covers law, economics, real estate finance, urban and regional economics, and environmental law.

*What about the institute of which you are now the director – how did the Cambridge Centre for Economic and Public Policy come about?*

This was initially funded by an American financier, who at the time was developing his ideas on money and monetary policy and was very sympathetic to post-Keynesian economics. I met him through Philip Arestis and Paul Davidson because he had attended many post-Keynesian conferences. Without this funding the Centre probably would not have come into existence. It is a fairly small centre in the sense that we have six or seven members and several associate members, and it has proved very useful as a focal point for heterodox economists around Cambridge. We have also been able to help host a number of visiting scholars over the years.

*In 1987 you published a paper on the production function. How did you get interested in this issue?*

I studied at Cambridge at the time of the capital controversies, and it seemed, at least for a period of time, that those at Cambridge said ‘well, this is the end of production function’. Using data I had collected for my PhD, out of curiosity I estimated a Cobb–Douglas production function and to my surprise got a very good fit with the output elasticities close to the factor shares. The question arose, why? Serendipity was at hand and was provided by Anwar Shaikh’s 1974 paper on the Humbug production function, which I came across by chance. It quickly led me to papers by Henry Phelps Brown and Herbert Simon which convinced me that there was a fundamental problem with the aggregate production function.

*For the benefit of our readers, perhaps you can summarize what you believe to be the main points of the book on the production function that you just published with Jesus Felipe?*

Yes, Jesus Felipe got interested in the same topic and working with him over the years has been very productive. It was useful that when we kept getting negative responses from neoclassical economists, we could actually convince ourselves we were not talking (or rather writing) nonsense.

So where to begin? Let me clear up a potential misconception from the very start. Nobody, least of all me, denies that there is a production relationship between the output of a firm, or an organization, and its various inputs using *physical* data. In practice, however, there is virtually no physical data available to estimate satisfactorily what may be termed an ‘engineering’ production function. So, ever since Cobb and Douglas’s initial paper in

1928, constant price value data is taken as a proxy for physical data. But once we use value data, we encounter an underlying identity. This is that value added is definitionally equal to the sum of labour's total compensation and total profits. (The arguments also follow through for gross output.) It is probably easiest to show the problem if I use a couple of simple equations. The value added accounting identity is simply given by  $V \equiv wL + rJ$ , where  $V$ ,  $w$ ,  $r$  and  $J$  are constant price value added, the wage rate, the number of workers, the rate of profit and the constant price value of the capital stock. Now, as this is an identity, it is compatible with *any* structure of production, constant or increasing returns at the plant level, any degree of competition, and the complete absence of an aggregate production function.

If we differentiate this identity and then integrate it, we obtain  $V \equiv Cw^a r^{(1-a)} L^a J^{(1-a)} \equiv AL^a J^{(1-a)}$ , where  $C$  is the constant of integration. You can see that this is none other than the Cobb–Douglas relationship, where  $a$  and  $(1-a)$  are the shares of labour and capital in aggregate output. Let me stress an important point. Under these circumstances, the Cobb–Douglas power function is definitionally identical to the identity; it is an exact mathematical isomorphism and not an approximation. Of course, if we wish to estimate the identity using cross-regional data, we will find that  $w$ ,  $r$  and the factor shares differ, but this variation is relatively small compared with those of  $V$ ,  $J$  and  $L$ . This explains why Cobb and Douglas and, later, I could not have failed to find a good fit to the Cobb–Douglas. A further irony is that neoclassical production theory predicts that if there is perfect competition, factors are paid their marginal products and, of course, if there is a well-defined production function, then the value of the output elasticities will equal their factor shares. But the identity shows that this must empirically always be the case!

Does the aggregate production function theoretically exist? The Cambridge capital theory controversies suggest not. Even from a quintessentially neoclassical viewpoint, Frank Fisher's work at MIT has shown that the aggregation theorems demonstrate the aggregate production cannot theoretically exist, even as an approximation. It's a 'pervasive, but unconvincing, fairytale', as he pithily puts it in the title of one of his papers.

But how is it, if all we are doing is estimating an identity, that we often do get poor regression fits to production functions, especially when using time-series data? This is the point, I think, Paolo Sylos Labini made some time ago, when he said we should abandon the production function, because of the statistically poor results. How do we explain that?

*It's because of the technical progress?*

Yes, because when we use time-series data the weighted logarithm of the wage rate and rate of profit is not explicitly included in the regression. In fact, it is generally proxied by a linear time trend which is interpreted, within the neoclassical framework, as capturing the rate of exogenous technical progress. However, the path of the logarithm of the weighted wage rate and rate of profit has a pronounced cyclical trend. Once we allow for this, by, for example, including a more flexible non-linear time trend, this brings us back to the identity. (As Solow himself pointed out, there is nothing in neoclassical production theory that says the path of technical change must be smooth.)

*In your work in the early 2000s, either alone or with Jesus Felipe, you show that the estimates of the output elasticities are in fact measures of the wage and profit shares. So what are the implications for people who make use of these output elasticities to provide policy advice? Is there a way out for them or is there absolutely nothing?*

I think there's absolutely no way out for them: an aggregate production function does not make any sense at all! The implications of the accounting identity critique affect all models

that use the aggregate production function. With just the knowledge of a few Kaldorian stylized facts that do not depend upon the existence of an aggregate production function, such as a constant capital–output ratio, constant factor shares, we can predict many of the regression outcomes even before a single regression has been run. (It should be stressed though that the critique does not depend upon these assumptions.) This includes Robert Hall’s attempts to estimate the size of the mark-up, Mankiw–Romer–Weil’s supposed test of the augmented Solow growth model, the neoclassical marginal productivity theory of distribution, and the demand for labour function.

This critique is essentially one of logic: it is either right or wrong. It is not about the plausibility of the assumptions or the interpretations of the parameters of a model. Yet it has been almost totally ignored by the profession. On the rare occasions the critique has itself been criticized, the arguments are confused and not convincing. You can find a detailed discussion of all this in the last chapter of our book.

*Shall we come to your second main area of research, which is the concept of a balance-of-payments-constrained growth rate? Could you explain what it is and how you got into it and in particular into your collaboration with Tony Thirlwall?*

Yes, it is somewhat ironical that when I first read Tony’s paper, I thought the argument was wrong and I wrote a comment on this. I considered that it was fundamentally based on two identities, namely the definitions of the income elasticity of demand for imports and exports and hence largely tautological.

*That comment was published in 1981 and Thirlwall’s main paper came out in 1979?*

Yes, both appeared in issues of the *Banca Nazionale del Lavoro Quarterly Review*. Tony wrote a very generous reply to my comment and invited me down to the University of Kent, where he was based, to discuss the issue. The point that I had overlooked in the article was that when you are estimating import or export demand functions, there is a relative price term included, so it is not an identity. The approach is based on the idea that most countries over the long run cannot run a substantial current account deficit, as the accommodating net financial inflows cannot continue indefinitely. Tony Thirlwall’s argument is that this can constrain a country’s growth to below that of its potential GDP. If this occurs, the rate of capital accumulation of the economy will be lower than what it might have otherwise been, the rate of induced technical progress will be smaller, and disguised unemployment will increase, etc. The simplest reduced form of the model has come to be known as Thirlwall’s law and the balance-of-payments equilibrium growth rate is given by  $y_b = \varepsilon z / \pi = x / \pi$ , where  $\varepsilon$  is the world income elasticity of demand for the country’s exports,  $z$  is the growth of world income (strictly speaking, the weighted growth of the country’s export markets),  $x$  is the growth of exports and  $\pi$  is the country’s income elasticity of demand for imports. The subsequent large literature has elaborated the theoretical model and more sophisticated econometric techniques have been used to test the model. This literature, to my way of thinking, has largely confirmed Tony’s insights.

*But if we are trying to explain the rate of growth of the world economy, what would be the explanation for this?*

One criticism is that as the world does not export anything, the export-led growth model cannot explain the growth of the world economy. I think this rather misses the point. All countries, of course, cannot be simultaneously balance-of-payments-constrained, except under circumstances to do with dynamics. However, in an early model of mine,

I demonstrated how the growth of one set of countries (or a trading bloc), whose growth is either policy-constrained (because of, say, fear of inflation) or capacity-constrained, limits the growth of another trading bloc through the balance-of-payments constraint. Of course, what determines the growth of the first trading bloc is an interesting question, but I don't think neoclassical growth theory is particularly illuminating here. It could be, as in the case of Japan in the early postwar period, that a country's rapid growth, although due to its export growth, was actually held back by the speed of the intersectoral transfer of labour and its rate of capacity accumulation. I don't think it contradicts anything other post-Keynesians say about economic growth.

*Applying your approach to the present economic problems and crisis we are facing, in particular the eurozone crisis, what are the implications?*

The eurozone crisis is essentially a banking and financial crisis arising partly as a contagion effect from the subprime crisis. It is also compounded by national debt crises in some of the EU countries. The balance-of-payments growth theory has implications for the path to recovery. If Germany concentrates on an export-led growth strategy by restraining wage growth, and for whatever reasons it succeeds and runs huge trade surpluses, then her trading partners, including the peripheral EU countries, are going to have to grow at a lower rate than they would otherwise have done. Even in the eurozone, with a common currency, countries cannot escape from balance-of-payments problems, although they can be postponed. In fact, it can be argued that if Greece had not been in the eurozone, it would not have been able to borrow so much, and for so long, at an interest rate barely above that charged to Germany.

*So, what are the ways out of the crisis?*

A short-term answer is of course that Germany with an export-led growth policy should not run trade surpluses; they should reflate internal demand as such. That will simultaneously allow Spain, Italy and the other countries to export more to Germany. Otherwise, the adjustment process tends to fall on the deficit countries, not the surplus countries. There is no pressure on a surplus country to stop running substantial surpluses. But perhaps more important is a recourse to coordinated Keynesian demand management policies that are so bitterly resisted by the ECB and the EU Austerians.

*This interview was conducted by Eckhard Hein and Marc Lavoie in August 2013. We thank Natalia Budyldina for the transcription.*

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