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›Cantabrigian Economics‹ and the aggregate production function

*John S.L. McCombie**

In Cambridge, UK, in the late 1960s and early 1970s, there were some of the most distinguished post-war non-neoclassical economists, which included Nicholas Kaldor, Joan Robinson and Piero Sraffa. The Cambridge capital theory controversies seemed to have been decisively settled in Cambridge, UK's favour, yet no alternative paradigm emerged to challenge the prevailing neoclassical orthodoxy. This paper briefly looks at the reasons for this, including why the Cambridge capital theory debate, despite its important ramifications, is now largely forgotten. The paper concludes by looking at a further problem that vitiates the aggregate production function, resulting from the use of constant-price value data in econometric estimation. This criticism has also been widely ignored.

JEL classifications: B31, B5

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I. Introduction

As this is an article in a series of papers to mark Geoff Harcourt's return from Cambridge to Australia, it is perhaps not inappropriate if I begin with a few remarks about what I term ›Cantabrigian Economics‹ of the 1960s and 1970s of which Geoff Harcourt was such an integral part. Cantabrigian Economics was a Schumpeterian ›vision‹ of economics held by a number of exceptionally original and gifted economists at the University of Cambridge,

* University of Cambridge, UK. I am grateful for the comments of an anonymous referee.

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UK. However, while it ought to be emphasised that these Cambridge economists did not always see eye-to-eye with each other, they nevertheless provided a challenge to the prevailing neoclassical orthodoxy that reached its zenith in the mid-1970s, before unfortunately rapidly fading. I will then discuss an issue with which Geoff Harcourt will always be associated as both commentator and innovator, namely the Cambridge capital theory controversies and I will finish by discussing a further, and to my mind more important, problem with the aggregate production function.

2. *Cantabrigian Economics*

It was one of my great educational benefits that I was able to read for the Economics Tripos at Cambridge when ›Cantabrigian Economics‹ was at its height. There was at that time in Cambridge a collection of outstanding scholars that articulated a view of economics entirely contrary to the dominant neoclassical paradigm. These scholars included, as well as Geoff Harcourt, and in no particular order, Nicky Kaldor, John Eatwell, Wynne Godley and the *Cambridge Economic Policy Group*, Joan Robinson (who had just retired), Brian Reddaway, Richard Goodwin, Mario Nuti, Robin Marris and the reclusive Piero Sraffa. To these one must add the distinguished visiting scholars, largely from Italy and including Pierangelo Garegnani.

Economics at Cambridge was not only totally at variance to that in nearly all other economics departments, but was also extremely idiosyncratic. Textbooks were not used (students had to cope with the original articles) and courses revolved around the interests of the lecturer with only the vaguest reference to the admittedly broad syllabus. If there was a disadvantage, it was that there was not much exposure to mainstream economics. The paradoxical result was that we could fully criticise what we did not fully know!

The second-year microeconomics course was taught by Richard Goodwin, and the whole course involved vector diagram after vector diagram. I think Goodwin was one of the few people who could think accurately in terms of three-dimensional diagrams and draw them on the blackboard. Not surprisingly, few, or none, of us undergraduates could do so and had great difficulty following him. We were not greatly helped by the publication in 1970 of his book *Elementary Economics from the Higher Standpoint*, which was based on his lecture notes. *Elementary Economics* is a misnomer if there ever was one! It is still as difficult a read now as it ever was. Even Bliss (1971: 625) wrote in his sympathetic review,

»so long as one understands and accepts the argument the pace is merely exhilarating, but when it proves hard to follow, or seems incorrect, retracing one's steps may prove of no avail. One typically ends up reading just one sentence over and over again – what did he mean by that?«

That certainly rings true.

I was fortunate to attend Kaldor's last lecture series for the Part II of the Tripos on economic growth, and still have memories of him vainly trying to demonstrate the neoclassical

aggregate production function in three dimensions with a sheet of A4 paper, giving up in disgust and saying in his Hungarian accent, »well, don't worry, production functions are of no use to anyone«. Joan Robinson had retired by then and was not around much, except for the occasional seminar. I do, however, recall attending Frank Hahn's inaugural lecture in 1973 and seeing her and Nicky Kaldor in the front row. Frank Hahn was a quintessential neoclassical economist, but one who knew the limitations of general equilibrium theory. His inaugural lecture was called *On the Notion of Equilibrium in Economics*, although a better title could have been »Why Kaldor's »Irrelevance of Equilibrium Economics« is Simply Wrong«, referring to Kaldor's paper that had just come out in the 1972 issue of the *Economic Journal*. At the time, the nuances of the debate were lost on us undergraduates, but both papers are worth carefully reading even after all these years. Looking back, Hahn had two main messages in his lecture.

First, increasing returns to scale *are* compatible with general equilibrium theory, *pace* Kaldor, provided that they are »small« relative to the size of the economy. This was a direct challenge to Kaldor's (1972: 1240) view that

»in fact equilibrium theory has reached the stage where the pure theorist has successfully (though perhaps inadvertently) demonstrated that the main implications of this theory cannot possibly hold in reality, but has not yet managed to pass his message down the line to the textbook writer and to the classroom.«

In the course of his paper Hahn cited a paper in the *Journal of Political Economy* by Michael Farrell¹ (1959) who was also a member of the Faculty and the paper was »accessible.«² This clarified what was meant by »small«. The increasing returns had to be small, relative to the size of the economy, such that the number of firms was large and the fiction of perfectly competitive markets could still be maintained. This was, of course, *not* Kaldor's view of the magnitude of increasing returns to scale which was similar to, and influenced, by Allyn Young's (1928) path-breaking paper.

The second message of Hahn was that general equilibrium theory is *not* a testable theory about how the real economy works, and in that sense it is not »scientific«. Its use is in providing the counterfactual. One example he gives is that if anyone argues that there is no need to be worried about exhaustible resources because the price mechanism will take care of the problem, then general equilibrium theory shows just how untenable the assumptions underlying such a proposition are; an infinite number of future contingent markets, etc.

However, Cantabrigian Economics as a distinct alternative to the neoclassical paradigm, while so promising and exciting for a short period, faltered, and then died. This raises the question, why?

One of the insights that Kuhn (1970) gives in his discussion of the role of competing paradigms is the importance of the role of the textbooks. The new generation of scholars are

1 Farrell (1957) was also the author of a paper on the notion of measuring technical efficiency that led to the development of Data Envelopment Analysis.

2 A much less accessible paper by Starr (1969) in *Econometrica* was also cited by Hahn.

not explicitly taught the appropriate methodology to follow; it is acquired by tacit learning and ostentation, through the worked exemplars and questions at the back of the chapters of the textbooks. There is a certain irony in this, given the low status textbooks are given in the evaluation of an economist's output for promotion, etc. The second lesson from Kuhn is that the only effective way to challenge and overthrow a paradigm is through the ascendancy of another competing paradigm. How, of course, one paradigm comes to replace another is a crucial question and involves the sociology of knowledge, not the philosophy of science.³ According to Kuhn, there is a degree of local incommensurability between competing paradigms which precludes any logical or objective comparison between them. I will return to this below, but the point I wish to make is that this was probably the reason for the demise of Cantabrigian Economics. While there were many profound critiques of the neoclassical paradigm, it was perhaps too much to expect such a small group of scholars, no matter how brilliant, could develop an alternative paradigm to rival the hegemony of the neoclassical paradigm. Because of the local incommensurability, they could be safely ignored by the mainstream.

At the very least, there is the need for non-orthodox textbooks to be on the economics reading lists alongside the traditional textbooks. This is not to say there were no attempts. Joan Robinson and John Eatwell wrote *An Introduction to Modern Economics* which was published in 1973. The hope at the time was that the *Introduction* would provide a major challenge to the prevailing neoclassical first year textbooks, notably Samuelson's *Economics*. But it proved a ›brilliant failure‹, in spite of its heavy promotion by the publishers in both the United States and the United Kingdom.

The story of this textbook has been analysed in detail by King and Millmow (2003). Why was the volume not a success? The timing could not have been better. The 1970s was a time when the orthodox neoclassical approach was being so heavily criticized from all quarters that it seemed as if it was not likely to survive.⁴ The phrase ›paradigmatic crisis‹ was often heard at this time. The main thrust of these critiques was that economics was becoming too formalised and divorced from real world problems.

The time was thus ripe for another textbook to present an alternative view, if only as a supplement to existing textbooks. Yet the *Introduction* did not, in the end, fill this gap. Although extensive feedback was received from Cambridge students, their advice was not always taken and there had never been a tradition of using, let alone writing, textbooks at Cambridge. For a detailed discussion of the reason for the failure of the *Introduction*, see King and Millmow (2003).

Problems included the focus on an abstract Ricardian model and the complete absence of data; the tables in the book were merely hypothetical, illustrating the theoretical arguments. There were no end-of-chapter summaries or exercises. I have described it as a ›brilliant failure‹ because it was an original textbook (perhaps too original?) that merged the micro-macro divide, and emphasised the importance of understanding the history of the discipline. And

3 On this, see Katouzian's (1980) excellent but much neglected *Ideology and Method in Economics*.

4 See Leontief (1971), Ward (1972), Worswick, (1972), and Robinson (1972).

for the Cambridge students it provided a welcome introduction to the importance of Sraffa's *Production of Commodities by Means of Commodities*.⁵

3. The Cambridge Capital Theory Controversies and the aggregation problem

This was the period when the Cambridge capital theory controversies were at their height. Geoff Harcourt's *Some Cambridge Controversies in the Theory of Capital* was published in 1972 and was a substantial elaboration of his 1969 *Journal of Economic Literature* survey. This debate involved Cambridge, UK, (with its Italian allies) and Cambridge, Massachusetts. The controversy was concerned with the theoretical problems of aggregating heterogeneous individual capital goods into a single index that could be taken as a measure of 'capital' as a factor input. The outcome was that it was generally agreed that no such index could be constructed (Harcourt 1972, Cohen/Harcourt 2003). The debate further showed that, when comparing steady-state economies, there is no necessary inverse monotonic relationship between the rate of profit and the (physical) capital-labour ratio, as in the neoclassical schema, outside of the restrictive one-sector model. It was shown to be theoretically possible for a given production technique to be the most profitable at both a high and low rate of interest but with some other technique dominating in between (a phenomenon known as capital reswitching).

I am not even going to try to summarise the Cambridge capital theory controversies. Excellent detailed overviews can be obtained from Harcourt (1972), Birner (2002) and Cohen and Harcourt (2003, 2005). The controversies encompass Joan Robinson's (1956) *Accumulation of Capital*, Sraffa's (1960) *Production of Commodities by Means of Commodities*, (Chapter XII, Switch in Methods of Production), the catalyst of Paul Samuelson's (1962) 'Surrogate production function', Lehavi's (1965) mistake, first pointed out by Pasinetti in 1965 and published in the resulting 1966 *Quarterly Journal of Economics* symposium, and Bliss's (1975) neoclassical intertemporal general equilibrium interpretation. It should, however, be emphasised that in the early 1970s there was a somewhat acrimonious split in the Cambridge UK camp. The post-Keynesians such as Joan Robinson argued that the failure to incorporate historical (as opposed to logical) time was the most important failing of neoclassical economics. Reswitching was fundamentally unimportant (Robinson 1975). The neo-Ricardians, such as Garegnani, profoundly disagreed and sought to build an alternative framework of value and distribution along Sraffian lines. But as we have noted above, both these approaches came to nought. Why?

⁵ A similar fate befell Wynne Godley and Francis Cripps's *Macroeconomics* (1983). It was so original that it bore no relationship to any of the other macroeconomic textbooks and consequently did not appear on many, if any, student reading lists. Sadly, Wynne Godley died in 2010, but at least he lived to see the publication of his magnum opus written with Marc Lavoie (Godley/Lavoie 2007).

There are two views. The first accepts that basically Cambridge UK effectively demolished the aggregate marginal productivity theory of factor pricing and the aggregate production function, but failed to provide an alternative paradigm (Dow 1980, Cohen 1984). It takes a theory to replace a theory, however logically inconsistent the prevailing paradigm may be.

The second view is that the debate effectively came to an end with Bliss's (1975) *Capital Theory and the Distribution of Income*, succinctly summarised by Dixit (1977).⁶ Bliss showed, to the satisfaction of some, that within an intertemporal general equilibrium framework, neoclassical marginalism was unassailable and Sraffa was merely a restrictive special case of the former.⁷ Samuelson (1966) had capitulated too readily and the whole dispute was nothing more than a storm in a teacup.

Pasinetti and Scazzieri (2008: 368) conclude their entry on the paradoxes in capital theory on an entirely different note.

»The controversy had also a number of less striking but perhaps longer-term consequences. The consideration of paradoxes has alerted economists to the richness and complexity of economic relationships, and to the need to avoid a process of generalization from the consideration of special cases. In any case the debate seems to have compelled theoretical economists to be more rigorous about the nature and limits of their assumptions. In many important cases, it has also brought about a change in the main focus of their analysis. All this leads one reasonably to expect as unlikely that the next generation of economists will leave the issue of capital theory at rest.«

Unfortunately, it is difficult to find any support for this view by considering papers in the top neoclassical journals (such as the *American Economic Review*, *Quarterly Journal of Economics*, and the *Journal of Political Economy*) over the last two decades or so where capital theory and its implications are rarely, if ever, even mentioned.

A second criticism that is related to the Cambridge capital theory controversies is the ›aggregation problem‹. This shows that the conditions under which it is possible to sum micro-production functions to give an aggregate relationship are so restrictive as to make the concept of the aggregate production function untenable (Fisher 1992, Felipe/Fisher 2003). The technical literature on this is quite complicated, but the empirical problem is intuitively very straightforward. Consider, say, the manufacturing sector. This consists of such diverse industries as (to take as random examples) SIC 204, Grain Mill Products, and SIC 281, Industrial Organic Chemicals. Does it make any sense to combine the values of each of the outputs and the inputs of the two industries and estimate a production function that purportedly represents the underlying combined technology of these industries? How do we even interpret the ›average‹ elasticity of substitution? In fact, the actual position is even

6 As Geoff Harcourt (2003: 207, fn 6) deprecatingly put it, »Dixit (1977) said in effect that Bliss's arguments made the quasi-rents of most previous writing on capital theory either zero or, with regard to those of Cambridge England, negative«.

7 The Sonnenschein-Mantel-Debreu theorem about the lack of stability in general equilibrium has been aired from time to time (see Kirman 1984), but has been widely ignored.

worse than this, as estimating an aggregate production function for, say, manufacturing or the whole economy combines many more disparate industries.⁸

Franklin Fisher (2005: 409), who over the years has done more than most to determine the technical conditions under which one can aggregate micro-production functions into an aggregate production function, has summarised the conclusion to be drawn from this literature as follows: »the conditions for aggregation are so very stringent as to make the existence of aggregate production functions in real economies a non-event«. He further argues that the problem applies equally to output and to employment – there is nothing special about capital.

In addition to Fisher's comments, consider the following definition of the *value* of gross output (Q) for i outputs:

$$Q \equiv \sum_{i=1}^n p_{0i} \tilde{Q}_i ,$$

where \tilde{Q} denotes a physical quantity and p_{0i} denotes some base-year price.

To make matters simple, suppose we differentiate Q with respect to the labour and assume that factors are paid their marginal product. But the individual prices, p_{0i} , are a function of the wage rate and the distribution of income, not only between labour and capital, but also between wage earners. Thus the differentiation will affect the distribution of demand and hence relative prices (the p_{0i} 's). Thus, the »value« of gross output is *not* independent of factor prices and cannot be simply differentiated with respect to the factor inputs to give their remuneration.⁹

Fisher (2005) is somewhat dismissive of the Cambridge controversies arguing that they are really only part of the much wider aggregation problem, although Harcourt (1976) sees the former as a much more fundamental disagreement over the »vision« of the way the capitalist system operates. Although the same concepts were used and the debate was one of logic, both Fisher and the other neoclassicists did not view the Cambridge controversies in the same light as Cambridge, UK.

Yet it is ironical that a consideration of both these serious problems has all but totally disappeared from the textbooks and the Cambridge capital theory controversies have been relegated to the history of economic thought, which few economists bother with. Consequently, a whole new generation of economists uncritically uses the aggregate production function with no appreciation of how tenuous its foundations are (Sylos Labini 1995). It is indicative that Cohen and Harcourt felt compelled to write a reminder for the economics profession in the 2003 issue of the *Journal of Economic Perspectives* in the »Retrospectives« section entitled »Whatever happened to the Cambridge capital theory controversies?« and that Birner's 2002

8 In the case of the service sector (e.g., health, education, defence) where the output is often measured as just the deflated total wage bill with an arbitrary allowance for productivity, it makes little sense to talk of a production function even in neoclassical terms.

9 Of course, the realization that prices of output are not independent of distribution goes back to the Classical economists.

volume, *The Cambridge Controversies in Capital Theory*, is part of the *Routledge Studies in the History of Economics*.¹⁰ The aggregation problem has fared little better. In spite of Fisher's persistent warnings of its damaging implications for the aggregate production function, virtually none of the plethora of recent applied and theoretical papers on, for example, economic growth, pay even lip service to the aggregation problem.

It is instructive to look at the way the Cambridge controversies and the aggregation problem have been covered in the textbooks and survey articles on economic growth over the last thirty years or so. Table 1 reports a representative sample of such publications since 1971. This was chosen as the initial year as by that date the main conclusions and implications of the Cambridge capital theory controversies had become well known. Also, Fisher's (1969) accessible critique of the aggregate production function had been available for a couple of years. The damaging problems for the aggregate production function posed by the required aggregation conditions also should have been widely appreciated by this time.¹¹

Wan (1971) was, for its time, a highly mathematical postgraduate textbook that comprehensively covered the state of neoclassical growth theory at that date: the Solow model, vintage capital goods growth models, optimal growth models, etc. Nevertheless, it also found space to include a chapter on the Robinson and Kaldor growth models, which have now entirely disappeared from the modern growth textbooks. Chapter 4 of Wan's book presents a concise introduction to both the Cambridge controversies and the aggregation problems and the damaging implications are clearly set out on page 110 of the volume. Indeed, it is ironical that Wan notes that »Mrs Robinson originally was not pessimistic enough. She still maintained the hope that techniques can generally be ranked by their ›real‹ capital/labour ratio« (Wan 1971: 110). Jones (1975) and Hacche (1979) were popular and clearly written third-year undergraduate and/or postgraduate textbooks. Both dealt with the Cambridge controversies, but only the former with the aggregation problem. Both spent a considerable portion of their books elaborating the Kaldorian or neo-Keynesian theory of economic growth.

Nadiri's (1970: 1146) article was a survey of the more applied aspects of growth theory, including the ›growth accounting approach«, but ended with the warning that »the aggregate production function does not have a conceptual reality of its own«. Such caveats are absent from the later literature, although, to be fair, Temple (1999: 150) in his survey of the new growth theory evidence notes briefly that

»arguably the aggregate production function is the least satisfactory element of macroeconomics, yet many economists seem to regard this clumsy device as essential to an understanding of national income levels and growth rates.«¹²

10 Birner's book, while predominantly examining the Cambridge controversies from a methodological perspective, also contains a clear exposition of some of the developments in capital theory subsequent to Harcourt's (1972) survey.

11 Fisher (1992: xiii) indicates that as far back as 1970 he had already called »into question the use of aggregate production functions in macroeconomic applications such as Solow's famous 1957 paper«.

12 Temple is more concerned about the importance of structural change, which a one-sector model tends to abstract from, rather than whether the concept of the aggregate production, *per se*, is legitimate.

*Table 1: References to aggregation problems and the capital controversies:
Selected textbooks and surveys*

Publication	Date	Capital controversies	Aggregation problems
H.Y. Wan	1971	Yes	Yes
M. Nadiri	1970	Yes	Yes
H. Jones	1974	Yes	Yes, but only briefly
M.D. Intriligator	1978	Yes	No
G. Hacche	1979	Yes	No
A. Maddison	1987	No	No
R.J. Barro and X. Sala-i-Martin	1995 / 2004*	No	No
C.S. Jones	1998 / 2002*	No	No
P. Aghion and P. Howitt	1998	No	No
B. Valdés	1999	Fleeting mention	Fleeting mention
D.K. Foley and T.R. Michl	1999	Yes	No
J. Temple	1999	No	Passing mention
D.N. Weil	2005	No	No
D. Acemoglu	2009	No	No
P. Aghion and P. Howitt	2009	No	No

Note: * Second edition

Valdés (1999: xii) in the preface to his book mentions he hated, for example, the »exaggeratedly heated »capital controversies« but there is literally no further elaboration. He also mentions the need to »accept that an aggregate production exists« (Valdés 1999: 63), but there is no further discussion on why it is necessary to accept this.

Hodgson (1977) has looked at the reception of Sraffa's (1960) *Production of Commodities by Means of Commodities* by way of an analysis of citations to the book. While the book is undoubtedly a powerful critique of neoclassical theory and it must take much of the credit for initially exposing the problems with the concept of the aggregate capital stock, it was the work of others such as Pasinetti, Robinson, and Garegnani that featured prominently in the debate. Consequently, the citations to Sraffa (1960) remained low (less than half a dozen a year) until 1970 and then rose until they peaked at 49 in 1982. As Hodgson (1997: 97)

In Temple (2006), he presents a defence of the use of the aggregate production function. See Felipe and McCombie (2010) for a rebuttal.

commented, this »is itself relatively low for a work of this stature and importance«. The citations declined steadily from 1989. Hodgson attributes this pattern to the publicity given to Sraffa's volume by Harcourt's (1969) *Journal of Economic Literature* review, his 1972 book and Garegnani (1970). But some saw Sraffa's work not just an internal critique of neoclassical theory, but the basis for a new paradigm of the working of the capitalist system. It is, however, a matter of record that the progress made on the latter has been very small, although this is not to deny its importance.¹³

4. *Why is the aggregate production function still widely used?*

The neoclassical production function is still used widely today not as a mere pedagogical tool but as a serious explanation of the way the capitalist system works. It forms, for example, the heart of the New Neoclassical Synthesis (Goodfriend 2004), and neoclassical growth theory of both the augmented Solow and endogenous growth variety.

A standard defence of the production function compares reswitching to the anomalous case of the Giffen good in consumer theory; the existence of which has not led to the abandonment of the law of demand. In other words, reswitching is a possibility, but this does not immediately imply that is likely to occur.¹⁴ Ferguson (1969: xvii, emphasis added) argues that

»[i]ts [reswitching's] validity is unquestionable, *but its importance is an empirical or an econometric matter* that depends upon the amount of substitution there in the system. Until the econometricians have the answer for us, placing reliance upon [aggregate] neoclassical economic theory is a matter of faith. I personally have faith.«

This methodological stance, Blaug (1974), for one, does not consider unreasonable.

The answer as to why the production function continues to be widely used today is related to this point and seems to be that its estimation, ever since Douglas's work in 1928 with Cobb and subsequently in the 1930s with other colleagues, generally, but not always, gives good statistical fits. Furthermore, the estimated output elasticities are often very close to the factor shares obtained from the national accounts, as predicted by the aggregate marginal productivity theory of factor pricing. As Solow once remarked to Fisher, »had Douglas found labor's share to be 25 percent and capital's 75 per cent instead of the other way around, we would not now be discussing aggregate production function« (cited by Fisher 1971: 305).

A study by McCombie (2000) using US cross-state per establishment data for total manufacturing for the year 1987 seems at first sight to give support to this methodological stance. His regression gave the following result:

13 See, for example, the interchange between Blaug (2009) and Kurz and Salvadori (2010).

14 However, this largely begs the question whether reswitching is the rule or the exception. Moreover, others, such as Sraffa, take this to be irrelevant – the problem is that one cannot work with a model, such as the aggregate production function, that is *logically* flawed. The Giffen good is not a logical inconsistency in consumer theory.

$$\ln Y_i = 3.059 + 0.820 \ln L_i + 0.235 \ln K_i \quad \nu = 1.05 (1.18) \quad \bar{R}^2 = 0.911$$

$$(9.85) \quad (12.67) \quad (3.70) \quad \text{SER} = 0.1319$$

Y is output, L is labour, K is capital, ν is the degree of returns to scale and t -values are in parentheses. The subscript i denotes the state. The t -value for ν is based on the null hypothesis that ν is statistically greater than unity. This is rejected.

Taking this estimation at face value would seem to give support for the existence of the aggregate production function. Given all the problems noted above concerning aggregation, etc., the statistical fit is remarkably good with over 90 per cent of the variation explained. The estimated coefficients take plausible values and are statistically significant. This could be taken as good evidence for the existence of an aggregate production function (or alternatively in Popperian terms, of not refuting its existence). This result confirms the earlier cross-sectional results of Paul Douglas and his colleagues (see Douglas (1976)).

Consequently, the defence of the use of the aggregate production function rests largely on a methodological instrumental argument. All models involve unrealistic assumptions; after all, as Joan Robinson once remarked, a map on a scale of one-to-one is of no use to anyone. What matters is the explanatory power of the model, which is taken to be synonymous with its predictive power – the symmetry thesis (Friedman 1953). Wan (1971: 71), for example, views the aggregate production function as an empirical law in its own right which is capable of statistical refutation, a view shared by Solow (1974).

But all this does not explain *why* aggregate production functions generally give such good statistical results, especially in the light of Fisher's (2005: 490) warning that

»one cannot escape the force of these results [of the aggregation literature] 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 answer to this question is surprisingly simple and is again a matter of *logic*, not interpretation and yet again there has been a deafening silence. So all I am going to do here is to present the argument in its simplest form, ignoring any qualifications and elaborations.

5. *On identities and aggregate production functions*

The reason for the good statistical fits that the aggregate production function gives is that essentially all that is being estimated is a (misspecified) identity, not a behavioural relationship. This is an area with Jesus Felipe and I have been doing a lot of work, extending the work, *inter alios*, of Phelps Brown (1957), Shaikh (1974) and Simon (1979). This demolishes the

standard instrumental argument that what matters is the empirical testing of the aggregate production function.¹⁵

Let us consider the Cobb-Douglas which is still the most widely used production function, but the argument follows through to more flexible production functions.

Consider the definition of value added:

$$Y \equiv wL + rK \quad , \quad (1)$$

which is an identity; w is the real wage rate, L is employment, r is the rate of profit and K is the constant price value of the capital stock. This can be for either time-series or cross sectional data. If we differentiate equation (1) with respect to time (time-series data) or totally using cross-section data and then integrate the result, we obtain:

$$Y \equiv a^{-a} (1-a)^{-(1-a)} w^a r^{(1-a)} L^a K^{(1-a)} \quad (2)$$

or

$$Y = AL^a K^{(1-a)} \quad , \quad (3)$$

where $a^{-a} (1-a)^{-(1-a)}$ is the constant of integration and a and $(1-a)$ are the shares of total wages together with salaries and profits in output respectively. This result is purely a result of the mathematics and makes *no* economic assumptions.

Let us assume that w and r are either constant (in cross-sectional data) or can be accurately approximated by a time trend (time-series data). If we were to estimate equation (3) we would find that the coefficients of $\ln L$ and $\ln K$ were exactly equal to the factor shares.

Notice that if we assume that there is a well-behaved Cobb-Douglas production function with all the usual unrealistic assumptions including the marginal productivity theory of factor pricing, constant returns to scale, and perfect competition then the result is that exponents of the Cobb-Douglas equal the factor shares. But we have shown that this *must* occur because of the underlying identity.

Let us put some real figures to this argument from the UK economy (Table 2). As we are merely illustrating a point, the exact date (1990) does not matter.

From the data in Table 2, equation (1), $Y \equiv wL + rK$ (the accounting identity), gives a value for value added of:

$$£519,089m \equiv £13,017.72 * 28,189m + 0.0988 * £1,540,000m$$

where m denotes a million. Thus, value added takes a value of £519,089 million.

The mathematical transformation of equation (1) given by equation (2), namely,

$$Y \equiv \left[a^{-a} (1-a)^{-(1-a)} w^a r^{(1-a)} \right] L^a K^{(1-a)} \equiv AL^a K^{(1-a)}$$

gives exactly the same figure for value added, *viz.*,

$$£519,089m \equiv 1.28 * 1.43 * £810.34 * 0.51 * 184,774.58 * £3,731.35$$

15 Our research on this topic over the last decade is due to be published in a book entitled the *Aggregate Production Function and the Measurement of Technical Change: A Critical Appraisal*.

Table 2: UK total industry, selected macroeconomic variables for 1990 in current prices

Value added (Y)	£ 519,089 million
Wage rate (w)	£ 13017.72
Total persons employed (L)	28.189 million
Rate of profit (r)	0.0988
Capital Stock (K)	£1,540,000 million
Capital-output ratio (K/Y)	2.9667
Labour's share (a)	0.7069
Capital's share ($1-a$)	0.2931
a^{-a}	1.2779
$(1-a)^{-(1-a)}$	1.4329

Sources: OECD Database, Flows and Stocks of Fixed Capital, 1971-1996, OECD, author's estimates.

For any one year, the Cobb-Douglas expression gives an *exact* fit to the accounting identity. As factor shares change slowly over time and the average wage and profit rates differ marginally between regions and industries, then the Cobb-Douglas will give a good fit to time-series or cross-sectional data.

In the case of the US data used above, we may see how the results depend on the identity from the four following equations.

$$Y_i \equiv 1.000 (wL)_i + 1.000 (rK)_i \quad (\text{i})$$

$$\ln Y_i = 0.578 + 0.500 \ln w_i + 0.530 \ln r_i + 0.467 \ln L_i + 0.535 \ln K_i, \quad \bar{R}^2 = 0.9993 \quad (\text{ii})$$

(11.45) (39.17) (66.72) (58.31) (68.45) SER = 0.0118

$$\ln Y_i = 3.059 + 0.820 \ln L_i + 0.235 \ln K_i \quad \bar{R}^2 = 0.911 \quad (\text{iii})$$

(9.85) (12.67) (3.70) SER = 0.1319

The auxiliary equation is:

$$[a_i \ln w_i + (1-a_i) \ln r_i] = 2.481 + 0.353 \ln L_i - 0.301 \ln K_i, \quad \bar{R}^2 = 0.369 \quad (\text{iv})$$

(8.01) (5.48) (-4.78)

Equation (i) is simply the identity while equation (ii) is the transformation of the identity into its logarithmic form. This would be an exact identity if factor shares were constant, but inevitably they show some variation. Labour's share takes a mean value of 0.475 with a coefficient of variation of 0.137. Capital's share is 0.525 with a coefficient of variation of 0.324. It can be seen that the estimates of equation (ii) simply reflect the identity. The sum

of the coefficients is 2.033 which is near the expected 2.000. Equation (iii) is the Cobb-Douglas, repeated here for convenience. It can be seen that omitting $\ln w$ and $\ln r$ from the identity does bias the coefficients and this is confirmed by the auxiliary equation (iv). It transpires from other results, not reported here, that the bias is almost entirely due to the correlation of $\ln r$ with $\ln L$ and $\ln K$ and this appears to be entirely coincidental. Douglas's many regression studies in the 1930s, using, in particular, Australian cross-sectoral firm data, found that the coefficients of $\ln L$ and $\ln K$ did not significantly differ from the factor shares (Douglas 1948). Hence, for his results $a \ln w + (1-a) \ln r$ was orthogonal to $\ln L$ and $\ln K$. The same argument concerning the identity holds for cost functions, but this will not be discussed here.

We can extend the argument further. Let us assume that there are well-defined micro-production functions of the form $Y_i = AL_i^\alpha K_i^{(1-\alpha)}$, where Y , L and K are all measured in homogeneous units and $\alpha = 0.25$ and $(1-\alpha) = 0.75$. Note that these figures are deliberately chosen to differ from the empirically observed values of the factor shares, namely $a = 0.75$ and $(1-a) = 0.25$. Suppose that we need to aggregate the micro-production functions, we need relative prices, p_i , so $\sum p_i Y_i = Y$ (value added in constant price monetary units). Let us assume that these are determined by a simple mark-up on the exogenously given average unit costs, so $p_i = (1 + \pi)wL_i/Y_i$, where the nominal wage w is the same for all firms. If the mark-up is 1.333 then it can be shown that when we estimate the aggregate production function (if even there are no aggregation and capital measurement problems), the estimated coefficients will be:

$$(1 - \hat{\alpha}) = 0.25 \text{ and not the »true« value of } 0.75$$

$$\hat{\alpha} = 0.75 \text{ and not the »true« value of } 0.25$$

It is worth emphasising this point. Even in the absence of any aggregation problems or issues arising from the Cambridge capital theory controversies, the fact that we have to use constant price monetary data in the estimation of production functions means that the estimated coefficients will, only under the most unlikely of coincidences, be equal to the true micro-production function parameters. It also means that we can get a perfect statistically fit to an aggregate production function even when there is *no* statistically significant underlying relationship of the micro-production functions (Felipe/McCombie 2006).

6. Summary and conclusions

In this paper we have briefly looked at the reasons why Cantabrigian Economics has had relatively little success since reaching its zenith in the early 1970s. The fact that neoclassical concepts, especially the aggregate production function and the marginal productivity theory of factor pricing, were shown to be logically faulty seems to have no long-run effect on the economics profession. Although many influential ideas and alternative approaches to analysing capitalist economies were generated over this period, they never were sufficient to establish a successful alternative paradigm, although they did contribute to the development

of post-Keynesian economics. The latter, at the macroeconomic level, eschews the use of the representative agent which leads to the fallacy of composition. It emphasizes the role of uncertainty over risk (the world is non-ergodic), bounded rationality over optimization, and therefore dismisses *tout court* the rational expectations hypothesis. Factor returns are seen as being influenced by the relative bargaining power of labour and capital and not determined by the technical conditions of production as in the marginal productivity theory of productivity (see, for example, King (2002), and McCombie (2010)).

We closed by considering another criticism of the aggregate production function, namely that all the estimates of the parameters are driven by an underlying accounting identity. The data will always give a good statistical fit *even though no aggregate production function exists*. This argument is one of logic, or rather elementary mathematics, but notwithstanding its importance it has, like the Cambridge capital theory controversies, been totally ignored by the neoclassical economists.

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