

# On the survival of a flawed theory of capital: mainstream economics and the Cambridge capital controversies

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The Cambridge controversies on capital theory opposed heterodox economists, mainly from the University of Cambridge, UK, to mainstream economists, mostly based at the Massachusetts Institute of Technology, Cambridge, USA. The controversies started in the 1950s and occupied the pages of some of the most influential journals. Their primary outcome was the broad acknowledgement of flaws, which we retrieve, in the concept of aggregate capital. Despite that acknowledgement, aggregate, homogeneous capital remains a staple of contemporary macroeconomics, as if the Cambridge controversies had never existed. To account for this apparent paradox is the aim of this article. We examine the arguments seeking to justify the enduring commitment to the aggregate capital approach and argue that they indicate an implicit commitment to instrumentalism. The indifference to the results of the Cambridge controversies is a consequence of methodological conformism and has shaky foundations.

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## 1. Introduction

The Cambridge controversies on capital theory opposed heterodox economists, mainly from the University of Cambridge, UK, to mainstream economists, mostly based at the Massachusetts Institute of Technology, Cambridge, USA. The main articles appeared in very influential journals, such as *The Review of Economic Studies*, *The Quarterly Journal of Economics* and *The Economic Journal*. The debate was initiated by Joan Robinson, whose 1953 article ‘The Production Function and the Theory of Capital’ contested the consistency of the concept of aggregate capital and the associated use of

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production functions. The Cambridge side was so effective with this specific criticism and its ramifications that in 1966 MIT's figurehead, Paul Samuelson, conceded there were serious problems in the neoclassical aggregate theory of capital. Subsequently, a second phase began, focussed on the merits of the neo-Walrasian approach to capital in avoiding the problems identified in the first phase of the debate (see [Hahn, 1975](#); [Cohen and Harcourt, 2003](#); [Garegnani, 2012](#); [Fratini, 2019](#)).

The explicit admission of flaws in the neoclassical aggregate theory of capital had no enduring effect on mainstream capital theory. Several decades later, the bulk of mainstream theorising still relies on the production function, with aggregate, homogenous capital as one of its arguments. Empirical approaches concerned, for example, with the factors behind economic growth or inequality resort to a similar production function.

Why has a discredited theory of capital survived? Why don't its acknowledged flaws matter? This paper seeks to explain why the results accepted by [Samuelson \(1966\)](#) have been ignored by those on his side in the controversies or, more generally, by mainstream economists. We argue that the most obvious rationale for the arguments presented in defence of the ongoing use of production functions is an implicit commitment to instrumentalism.

There exists, of course, a large stream of literature on the Cambridge controversies, including in recent years and by economists sympathetic to the Cambridge side ([Pasinetti et al., 2003](#); [Garegnani, 2012](#); [Harcourt, 2015](#); [Lazzarini, 2015](#); [Petri, 2015](#)). Some attribute the fading interest in the controversies and their lack of impact to ideological biases, or to misunderstandings that punctuated the debate ([Cohen and Harcourt, 2003](#)). But the focus tends to be on the post-1966 stage. The neglect of the acclaimed results of the first phase is our sole concern here.

Our article is organised as follows. Section 2 portrays how capital is treated in most modern mainstream macroeconomics, underscoring the dominance of the production function approach with aggregate, homogeneous capital. This section also connects the now dominant treatment of capital with its neoclassical predecessors. This allows us to portray the theoretical environment in which the Cambridge controversies emerged. Section 3 examines the first phase of the controversies, articulating the pitfalls of the neoclassical aggregate theory of capital and of the concept of a production function. Section 4 surveys the most common arguments for the ongoing use of production functions, viewing them as implicit explanations for the neglect of the controversies. In Section 5 we argue that these arguments rest on an implicit commitment to instrumentalism and, moreover, that they have shaky foundations. Concluding comments appear in Section 6.

## 2. Alternative neoclassical approaches to capital

In the aftermath of the marginalist revolution, the theory of production and the theory of capital developed in two strands. One of these strands has its roots in Léon Walras. In the Walrasian system, capital is first and foremost a set of heterogeneous goods that can be deployed in the production of other goods. Accordingly, capital is not aggregated into a single index. For reasons beyond our scope, Walras' approach to capital and production had to be refined before enjoying significant influence. This refinement appears in [Hicks \(1939\)](#) and, most importantly, in [Arrow and Debreu \(1954\)](#). This strand of capital theory we shall call the *neo-Walrasian theory of capital*. Capital, to repeat, is conceived as a set of heterogeneous goods.

Irving Fisher, John Bates Clark, Frank Knight and others developed an alternative way of dealing with capital.<sup>1</sup> In their approach, capital—or pure capital—is conceived as a *single factor of production*, corresponding to the amount of value dwelling in the myriad of heterogenous capital goods or, equivalently, to the quantity of financial capital available. Put otherwise, *capital is a quantity*, a quantity of value. Let us refer to this theory as *the neoclassical aggregate theory of capital*. Robert Solow and his followers regard the 1950s developments in the theory of capital, specifically the theory of capital accumulation and economic growth (i.e. one-sector/one-commodity models), as essentially a development of this branch of the theory of capital.

The neoclassical aggregate theory of capital takes the preferences of economic agents, the endowment of factors of production (i.e. capital in value terms—pure capital—and labour), and the techniques of production (i.e. specific combinations of capital and labour) as given. They remain unchanged as the economy adjusts towards equilibrium—a set of relative prices (including the wage and the interest rate) and the associated level of output/income. Concrete capital goods are not among the data of the theory; only capital, the single factor of production, is. Adjustment to equilibrium involves adjusting the bundle of concrete, physically distinct capital goods. As pure capital is perfectly mobile, it flows towards more profitable sectors. Free competition eventually guarantees that the system features a uniform interest rate over the supply price of capital goods. If that were not so, profit opportunities would remain unexplored and supply and demand would not be equal in every market.<sup>2</sup> In other words, a uniform rate of profit is reached because less profitable capital goods are less demanded (hence, less produced) than more profitable ones.<sup>3</sup> Capital thus changes its form: its physical composition adjusts. Since new capital goods are produced using the economic resources that had produced the old ones, this change in form entails no change in value—as required by the assumption of a given amount of pure capital.

In short, a uniform rate of profit—which can be taken as an equilibrium condition—implies that the specific capital goods in existence are the most profitable ones given the available technology and the preferences of consumers, and allows the theorist to reason about capital in terms of a single, homogeneous quantity of value, that is, as a single factor of production:

In attaining a simple formula that governs the rent, not only of land, but of every concrete instrument of production, we have incidentally attained an equally simple rule that applies to the earnings of all pure capital, whether it be invested in ordinary instruments of production or in land itself. It is based on the equalizing action of pure capital; its earnings tend toward a universal level. (Clark, 1888, pp. 134–35)

By reasoning in terms of pure capital (i.e. capital as a quantity of value)—as he does in his fundamental arguments—Clark renders the notion of heterogeneous capital goods

<sup>1</sup> The main references are Fisher (1906, 1907, 1930), Clark (1891, 1894, 1899) and Knight (1933, 1934, 1936A, 1936B). See also Hobson (1891).

<sup>2</sup> Similarly, the earnings of labour tend towards a uniform wage rate: ‘the earnings of capital tend toward equality; and, with certain important reservations, those of labor do the same’ (Clark, 1888, pp. 125–26).

<sup>3</sup> Classical authors defined profits as the revenue left after paying all costs of production, thus comprising interest payments and a compensation for risk. In neoclassical theory, profits are the revenue left after paying all costs of production, including the interest rate, which is the cost of capital. Perfect competition drives profits to zero, whilst the rate of return on capital *net of risk* tends to equal the interest rate. In the absence of risk, the classical concept of ‘rate of profit’ and the neoclassical concept of ‘rate of interest’ refer to the same thing.

superfluous to the results of the theory. Capital is, in effect, conceived as a homogeneous factor of production. Under marginalist principles, the higher the endowment of capital, the lower the interest rate; more capital-intensive production techniques will be used in equilibrium.

Five propositions summarise the main tenets of the neoclassical aggregate theory of capital (see [Samuelson, 1962](#), pp. 200–01; [Harcourt, 1972](#), p. 122; [Cohen and Harcourt, 2005](#)):

1. The real rate of return on capital is the rate of interest.
2. The rate of interest is determined technically by the diminishing marginal productivity of capital.
3. There is an inverse, monotonic relation between the quantity of capital and the rate of interest.
4. The wage rate varies inversely with the rate of interest.
5. Relative factor scarcities and marginal products determine the distribution of income.

Note that these propositions are articulated in terms of capital as a single factor of production, or as pure capital. This legitimises one-commodity models, where capital is homogenous and identical to the single final good produced, as in [Ramsey \(1928\)](#) or [Solow \(1956\)](#).

The latter model, as is well known, encapsulates the standard neoclassical theory of capital ([King \*et al.\*, 1988](#); [Rebelo, 2005](#)). Capital is a single factor of production, a homogenous set of productive resources that, together with labour, are used in production. This is operationalised through a production function aggregated over the available techniques of production (i.e. different capital-labour ratios). Dynamic stochastic general equilibrium models, prevalent in current macroeconomics, build on real business cycles theory, which in turn builds on a version of Solow's model of capital accumulation.

### 3. On the Cambridge capital controversies

'One can only wonder', writes [Sraffa \(1962, p. 479\)](#), 'what is the good of a quantity of capital ... which, since it depends on the rate of interest, cannot be used for its traditional purpose, which is to determine the rate of interest'. In a nutshell, this is the logical problem in the neoclassical aggregate theory of capital. The quantity of pure capital is, recall, among the data of the theory. As Sraffa points out, complaints arise when one reflects on what the 'quantity of capital' refers to. This is what Robinson did in her 1953 paper on the neoclassical production function.

Three intertwined layers can be identified in the Cambridge controversies: a technical layer, focussed on the coherence of notions of capital and the use of production functions; a non-formal layer, concerned *inter alia* with time, expectations and uncertainty; and a political-ideological layer, turning on income distribution. Moreover, two phases can be distinguished. In the first, Cambridge economists attacked production functions with capital (i.e. its value) treated as a single factor of production. A second phase began after their neoclassical opponents conceded the difficulties associated with the idea of a quantity of capital and turned to the neo-Walrasian approach. They contended that the theory of capital attacked during the first phase was not representative of the most recent neoclassical theory of capital but merely a textbook,

stripped-down version meant for undergraduate teaching (see [Hahn, 1975](#); [Harcourt, 2015](#); [Lazzarini, 2015](#)). Only the technical layer and the first phase of the debate are relevant to us here.

### 3.1 *Errors in the neoclassical approach to capital*

Since capital goods are heterogenous along multiple dimensions—‘shapes, sizes and specifications’, as [Robinson \(1953, p. 81\)](#) puts it—the notion of a quantity of capital inevitably entails the need for a value measure for its stock. But, Robinson asks, ‘should capital be valued according to its future earning power or its past costs?’ ([1953, p. 81](#)). One must deal with time, and thus the interest rate, in both alternatives. The future earning power of capital emerges as a stream of returns over time, the addition of which involves the interest rate. On the other hand, the production of capital goods requires both labour and other capital goods, which were, in turn, produced earlier in time. Hence, ‘the cost of capital includes the cost of capital goods’ ([Robinson, 1953, p. 82](#)). Since these costs are incurred in different periods, the interest rate becomes essential for their aggregation. Hence, a value measure for the stock of capital requires the interest rate, which is precisely what the quantity of capital is supposed to determine in the first place.

This circularity in the notion of the value of capital entails that it is impossible to deploy the concept of aggregate capital—a quantity of capital—in settings with many distinct capital goods. Measuring the quantity of (aggregate) capital requires something that cannot be determined without that quantity. Second, aggregation over the techniques of production—a single function expressing the entire ‘book of blueprints’—is untenable, that is, production functions relating output levels and different techniques of production are a misconception. As [Robinson \(1953\)](#) argues, different techniques of production require the use of different (types of) capital goods. This criticism remains valid even if two techniques of production differ only in the amount of a particular capital item they require, rather than in the type of capital. In what sense could one say that a given technique uses more capital than another when a change in the value of capital is, according to marginal productivity theory, necessarily associated with a change in the interest rate, which itself changes the value of capital? Any statement comparing two or more techniques in terms of capital intensity requires comparing the quantity of capital in those techniques, and the latter comparison is meaningless. Unless there is a measure for the quantity of capital independent of the interest rate, the concept of aggregate capital is unwarranted and so is aggregation over the production techniques.

Ultimately, the problem Robinson highlights is whether it is possible to construct a novel measure for capital, avoiding the logical circularity that she and [Sraffa \(1960\)](#) identified. As Harcourt observes ([1969](#)), rather than a specific author, Robinson is questioning *an approach* encompassing the empirical studies of [Cobb and Douglas \(1928\)](#) and others, like [Clark \(1888\)](#), whose work rests on the concept of capital as a quantity.

### 3.2 *The neoclassical reaction*

Neoclassical economists engaged with Robinson by trying to determine the conditions under which a measure of the stock of capital could be consistently obtained. Amongst the responses, there are papers by [Champernowne \(1953\)](#) and [Swan \(1956\)](#),

and Solow's (1955) argument on the necessary and sufficient conditions for capital aggregation. Solow (1955, p. 102) shows that if 'the marginal rate of substitution of one kind of capital good for another [is] independent of the amount of labor in use', then, mathematically, it is possible to aggregate the various capital goods into a single index. A simple way to guarantee that condition for aggregation is to assume capital to be homogenous, in which case capital goods are perfect substitutes.<sup>4</sup> Once capital is assumed to be homogenous, it can be measured (i.e. aggregated) using its own physical units and then play its role in determining the interest rate. No circularity exists. Conceiving capital in such a way leads to the models often referred to as one-sector/one-commodity models, of which Solow (1956) is the most influential example. Here, the aggregate production function is central, and the model's economy displays the main tenets of the neoclassical aggregate theory of capital.<sup>5</sup>

Samuelson offers another defence of the neoclassical results, which would be more relevant during the debate. Drawing on the mathematics of linear programming to investigate the properties of heterogeneous capital models assuming equal factor proportions in the production of every good, he argues that the results deduced by Clark and others—including Solow (1956)—using the concept of aggregate capital follow even without deploying this concept:

Repeatedly in writings and lectures I have insisted that capital theory can be rigorously developed without using any Clark-like concept of aggregate 'capital', instead relying upon a complete analysis of a great variety of heterogeneous physical capital goods and processes through time. (Samuelson, 1962, p. 193)

In other words, the concept of a quantity of capital is ultimately superfluous. Solow's aggregate production function is consistent both with Clark's verbal analysis and Samuelson's own linear programming models. This alleged consistency may be termed the *equivalence or continuity* between one-sector/one-commodity models and the neoclassical aggregate theory of capital. Or, as Samuelson (1962, p. 194) puts it:

[W]e can sometimes predict exactly how certain quite complicated heterogeneous capital models will behave by treating them as if they had come from a simple generating production function (even when we know they did not really come from such a function)

A theory treating capital *as if* it were a homogeneous set of goods would, so long as it reached the same results, be compatible with or equivalent to, the neoclassical aggregate theory of capital. It would also eliminate the problem identified by Robinson. However, the Cambridge economists would demonstrate that Samuelson's equivalence claim cannot be upheld.

### 3.3 Examining the equivalence claim

Capital heterogeneity has implications on the relationship between the interest rate and capital intensity. Most contributions to the Cambridge controversies concern

<sup>4</sup> If capital goods are homogenous, the marginal rate of substitution between any two capital goods is always equal to 1, so it does not depend on the amount of labour in use.

<sup>5</sup> Beyond dispensing with capital heterogeneity, Solow's model differs from the neoclassical aggregate theory of capital on another point: the steady-state equilibrium, for which the data of the economy are the rate of growth of labour, the book of blueprints—the production function—and the savings rate. In the Solow economy, convergence to equilibrium takes place whilst the amount of capital adjusts. In the neoclassical aggregate theory of capital, the quantity of factors, including capital, is given whilst the economy adjusts towards its long-period equilibrium.



the discussion of these implications (see [Pasinetti and Scazzieri, 1990](#)). The implications in question became known as *capital reverse deepening* and *reswitching* (Sraffa, 1960; Pasinetti, 1966; Harcourt, 1969). In a setting of heterogenous capital, changes in the interest rate do not yield unambiguously signed changes in the economy's capital intensity. An increase in the interest rate may move the economy to an equilibrium in which the prevailing technique of production is more capital intensive, not less. This is reverse capital deepening. Also, a given technique may be the profit-maximising technique at two different ranges of values of the interest rate, while in between another technique is the optimal one. This is the reswitching of techniques.

Capital reverse deepening and reswitching may obtain because changes in capital intensity associated with a change in the technique of production are, so to speak, the joint outcome of (i) a change in the value of capital brought about by a change in the interest rate—the *Wicksell price effect*—which causes a change in prices, and thus a re-evaluation, of capital goods; and (ii) a change in the physical quantities and types of capital goods—the *Wicksell real effect*. Therefore, whether an increase in the interest rate leads the economy to a higher or lower capital intensity depends on the relative size of the Wicksell effects.<sup>6</sup> These effects are the manifestation, at the theory level, of the conceptual complication associated with the idea of a quantity of capital (see [Robinson, 1958](#); [Bruno et al., 1966](#); [Ferguson and Hooks, 1971](#)).

Up until [Robinson \(1953\)](#), it had been taken for granted that, ‘when a change in the physical capital goods themselves takes place, as an effect of a change in the rate of profit ... the ‘quantity of capital’ per man required by the new method of production will change in an inverse monotonic relationship to the rate of profit’ ([Pasinetti, 1966](#), p. 513). The circularity in the notion of a quantity of capital and the associated Wicksell effects entail that the supposedly well-established results that Pasinetti is referring to are not generally valid after all ([Sraffa, 1960](#)).<sup>7</sup> In truth,

The theoretical implications [of the Wicksell effects] are rather far-reaching, particularly with reference to one of the most vexed questions in capital theory: the question of whether—at any given state of technical knowledge—there is any relationship between changes in the rate of profit and changes in the ‘quantity of capital’ per unit of labor ([Pasinetti, 1966](#), pp. 512–13).

The relation between capital intensity and the interest rate, it turns out, may contradict the standard tenets of the neoclassical aggregate theory of capital, replicated in the one-commodity/one-sector models. Only under very special conditions will there be a monotonic relation between capital intensity and the interest rate. There is no equivalence or continuity between the neoclassical aggregate theory of capital and one-sector/one-commodity models.

<sup>6</sup> Assuming, for the sake of the argument, that the production of capital goods is more capital intensive than the production of final goods, a lower interest rate entails a lower relative price of capital goods, in other words, a lower value of existing capital goods. This may lead to the adoption of techniques deploying more physical capital, the increase of which may not compensate its lower relative price. Hence, the quantity of capital may be lower at a lower level of the interest rate.

<sup>7</sup> Recently, [Han and Schefold \(2006\)](#) and [Schefold \(2017, 2020\)](#) have emphasised the importance of the Wicksell effects and the limited degree of substitutability as the most significant criticisms of production functions.

## 3.4 Neoclassical economists fight back and lose

The neoclassical reaction came from [Levhari \(1965\)](#), who argued that, at the macro level, paradoxical behaviour would not happen. He showed this for a specific type of economy, where every commodity is used in the production of every other commodity, directly or indirectly.<sup>8</sup> His demonstration apparently vindicated Samuelson's claim that Solow's homogenous capital model is analytically equivalent to a heterogenous capital model.<sup>9</sup> However, in the first world congress of the Econometric Society, also in 1965, Pasinetti presented a paper disproving Levhari's theorem, published in its final form in a 1966 symposium in the *Quarterly Journal of Economics* that included other criticisms of [Levhari \(1965\)](#).

Yet the most relevant article, as far as the fate of the debate is concerned, is [Samuelson's \(1966\)](#), which is significantly titled 'A summing-up'. In this article, Samuelson surveys the main arguments put forward—and, after [Pasinetti \(1966\)](#), accepted—to the effect that paradoxical behaviour in the neoclassical aggregate theory of capital *was* possible:

The phenomenon of switching back at a very low interest rate to a set of techniques that had seemed viable only at a very high interest rate involves more than esoteric technicalities. It shows that the simple tale told by Jevons, Böhm-Bawerk, Wicksell, and other neoclassical writers - alleging that, as the interest rate falls in consequence of abstention from present consumption in favor of future, technology must become in some sense more 'roundabout,' more 'mechanized,' and 'more productive'—cannot be universally valid. ([Samuelson, 1966](#), p. 568)

[Samuelson \(1966, pp. 568–74\)](#) even shows why paradoxical behaviour might occur using a simple numerical example. He concedes that the traditional version of the neo-classical theory of capital is flawed: 'If all this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence. We must respect, and appraise, the facts of life' ([Samuelson, 1966](#), p. 583).

Shortly afterwards, [Garegnani \(1970\)](#) would show that [Samuelson's \(1962\)](#) model, despite paying lip service to capital heterogeneity, requires homogeneous capital. In that model, 'heterogeneity of commodities can ... be properly defined only as a difference in their conditions of production' ([Garegnani, 1970](#), p. 415), which is precluded by assumption. Samuelson's analytical equivalence argument is simply the demonstration that equal assumptions deliver equal results, rather than proof that a one-commodity model solves the logical circularity problem of the concept of capital as a quantity.

Still, this first phase of the Cambridge controversies had no lasting impact whatsoever:

[M]ainstream economics goes on as if the controversy had never occurred. Macroeconomics textbooks discuss 'capital' as if it were a well-defined concept—which it is not, except in a very special one-capital-good world (or under other unrealistically restrictive conditions). ([Burmeister, 2000](#), p. 310)

Why is this the case? Let us have a look at the most influential mainstream arguments in support of their stance.

<sup>8</sup> Mathematically, the input–output matrices for every technique of such an economy are assumed to be indecomposable.

<sup>9</sup> '[Levhari's] theorem, if true, would have falsified Sraffa and would have given full generality to Samuelson's "surrogate production function"' ([Pasinetti et al., 2003](#), p. 227).



#### 4. Mainstream arguments defending their approach

Ignoring the results established in the Cambridge controversies, many economists continue to defend the use of aggregate, homogenous capital. In this section, we review their best arguments, focussing on mainstream theorists or sympathetic historians.

A few years after Samuelson conceded that ‘Pasinetti, Morishima, Bruno-Burmeister-Sheshinski, Garegnani merit ... gratitude for demonstrating that reswitching is a logical possibility in any technology, indecomposable or decomposable’ (1966, p. 582), Solow observes:

I have to insist again that anyone who reads my 1955 article [Solow (1955)] will see that I invoke the formal conditions for rigorous aggregation not in the hope that they would be applicable ... but rather to suggest the hopelessness of any formal justification of an aggregate production function in capital and labor. (Solow, 1976, p. 138)

But how can a neoclassical economist rationalise the use of production functions in the absence of a formal justification? Somewhat earlier, Solow closed his review of Hicks’ *Capital and Growth* (1965) noting that:

I have never thought of the macroeconomic production function as a rigorously justifiable concept. In my mind it is either an illuminating parable, or else a mere device for handling data, to be used so long as it gives good empirical results, and to be abandoned as soon as it doesn’t, or as soon as something better comes along. (Solow, 1966, pp. 1259–60)

This comment was prompted by Hicks’ study of the mathematical conditions that would have to hold for a production function to exist in a multi-goods economy. However, in the context of Samuelson’s acceptance of defeat, Solow may have been commenting on Hicks’ book with the Cambridge controversies on the back of his mind. Be that as it may, the aggregate production function is deemed either an ‘illuminating’ parable or a mere instrument for empirical research.

Backhouse (2014, p. 279) argues similarly. Referring to Samuelson on the Cambridge critique, he notes that ‘it marked MIT’s recognition of Robinson’s technical point, but, to the frustration of their critics, Samuelson and Solow did not stop using aggregate production functions. The reason was that they had never considered Ramsey production functions any more than a useful heuristic device that was almost essential in doing empirical work, where it was necessary to work with aggregate data’. Blaug (1997, p. 472), in turn, observes that ‘[i]f we refuse to aggregate in any way, we get the Walrasian general equilibrium theory, which is sterile for empirical research. Some aggregation is necessary for empirical work’. And Solow (1975, p. 277) refers to the neoclassical aggregate theory of capital as ‘only a crude simplification made for the purpose of applying the theory to real numbers’, which ‘has to be judged pragmatically and not by standards of rigorous analysis’. In short, the production function is a convenient data handler. Pragmatic considerations eclipse concerns with formal rigour.

Ferguson (1969, p. 266) is even more straightforward, noting that the question ‘is not whether the Cambridge criticism is theoretically valid. It is. Rather, the question is an empirical or an econometric one: is there sufficient substitutability within the system to establish the neoclassical results?’ Whereas Sato (1974, p. 383) remarks that ‘there is another world in which the neoclassical postulate may not fare well or is contradicted. An empirical question is which of the two worlds is more probable’. Accordingly, ‘[w]hen someone claims that aggregate production functions work he means (a) that they give a good fit to input-output data without the intervention of

data deriving from factor shares; and (b) that the function so fitted has partial derivatives that closely mimic observed factor prices' (Solow, 1974, p. 121).<sup>10</sup> Apparently, then, the claim is that the aggregate production function could be, but has not been, statistically refuted (Wan, 1971, p. 71). This view is prominent in textbooks:

Our strategy will be to start with a conjecture that the economy can be described by a particular production function [the Cobb-Douglas function], one that shares important properties with microeconomic production functions. We will then test our conjecture empirically. If it seems to describe the data well, we shall be satisfied that it provides a useful approximation (Hoover, 2012, p. 326).

In short, Solow and others imply that the problems identified by Cambridge economists are *immaterial*. The validity of neoclassical theory is an empirical, rather than theoretical, question (see Ferguson, 1969, p. 258).<sup>11</sup> As Solow (quoted in Fisher, 1971, p. 305) puts it, 'had Douglas found labor's share to be 25% and capital's 75% instead of the other way around, we would not now be discussing aggregate production functions'. Mankiw (1997, p. 104) similarly writes that he has 'always found the high  $R^2$  reassuring when I teach the Solow growth model. Surely, a low  $R^2$  in this regression would have shaken my faith that this model has much to teach us about international differences in income'.<sup>12</sup> In sum, the view that the production function performs well remains widely held.

If we move on to theoretical economics, arguments are somewhat different—but not *that* different. The 1980s quest for micro-foundations led to the revival of aggregate production functions. The theoretical developments most relevant to their revival are the real business cycles paradigm on the one hand, and the new growth theory on the other. Both postulate a representative firm operating under a neoclassical production function with aggregate capital. Two arguments for favouring this postulate are worth underscoring. First, '[t]he reason aggregate production functions are routinely used is that disaggregation is not always possible, and even where possible, may not lead to a tractable model' (Temple, 2006, p. 310). In the same vein,

[m]uch of macroeconomics—and an even larger fraction of the growth literature—makes strong assumptions about the shape of the production function and the direction of technical change. In particular, it is well-known that for a neoclassical growth model to exhibit steady-state growth, either the production function must be Cobb-Douglas or technical change must be labor-augmenting in the long run. But apart from analytic convenience, is there any justification for these assumptions? (Jones, 2005, p. 517)

In short, postulating an aggregate production function is analytically convenient, since it facilitates the deduction of macroeconomic results from the choice theoretic framework of microeconomics, precluding many mathematical complications. Both Jones (2005)

<sup>10</sup> Much later, when the 1928 seminal article by Cobb and Douglas was selected as one of the top 20 articles published in the first 100 years of the *American Economic Review*, the committee, which included Solow, remarked that 'Cobb and Douglas explored the elementary properties and implications of the functional form, and pointed to the approximate constancy of the relative shares of labor and capital in total income as the validating empirical fact' (Arrow *et al.*, 2011, p. 2).

<sup>11</sup> Blaug (1992, p. 182) observes that '[t]he history of both the physical and the social sciences is replete with such examples of "faith," that is, a determination to ignore logical anomalies in a theory until they are shown to be empirically important, rather than to leave whole areas of intellectual endeavor devoid of any theoretical framework'.

<sup>12</sup> Some of the arguments above suppose a Cobb-Douglas function. But the same arguments are invoked to legitimise other aggregate production functions, like the Constant Elasticity of Substitution (CES) production function, explicitly introduced by Arrow *et al.* (1961). The swift adoption of the CES function is attributed to its empirical performance (see Biddle, 2020).

and Temple's (2006) line of argument echo Blaug's (1997) about the relative advantage of an aggregate production function setting over the neo-Walrasian alternative. The use of aggregate production functions contributes to more tractable models, which usually means facilitating the (mathematical) deduction of results (see Cherrier, 2023), later compared with data. So, we are back to empirical tractability and empirical usefulness.

A second argument is worth highlighting: the requirement that growth and cycle theories cohere with each other, which is a feature of the real business cycles paradigm (Rebelo, 2005, p. 2). This requirement pushes the production function to the centre of macromodelling: 'the most basic model of economic dynamics is the neoclassical model of capital accumulation ... it is natural to consider it as the benchmark model for our understanding of economic fluctuations as well as growth' (Plosser, 1989, p. 54). Real business cycles theory '[unifies] business cycle and growth theory by insisting that business cycle models must be consistent with the empirical regularities of long-run growth' (Rebelo, 2005, p. 2). In other words, predictions from cycles theory must cohere with the regularities of growth, which, so the reasoning goes, are well approximated by growth models built around aggregate production functions. Once again, empirical considerations turn out to be at the core of the use of production functions.

## 5. Assessing the mainstream arguments

As shown in the previous section, the ongoing use of production functions rests overwhelmingly on the claims that they facilitate, or enable, empirical research and deliver good empirical, including econometric, fits.<sup>13</sup> This sort of argument also guides choices on how to develop mainstream theory. If we accept these arguments at face value, we must conclude that the alleged empirical merits of aggregate production functions supersede their theoretical shortcomings. But why do mainstream economists apparently think so?

### 5.1 On the common foundations of the mainstream arguments

The underlying unity of the explanations we have considered lies ultimately at the methodological (or, more generally, meta-theoretical) level. Mainstream economists, unlike neoclassical authors of older generations, do not pay much attention to methodology and especially to philosophical reflection, which they tend to see as a waste of time. This is self-contradictory, as has been repeatedly shown, not least by Tony Lawson for over 25 years (see Lawson, 1997). Any method is accepted because of epistemological grounds, regardless of whether these grounds are articulated, implicit or not reflected upon at all. And, since knowledge is knowledge *of something*, those epistemological grounds point to underlying ontological conceptions. The arguments used in defence of the aggregate production function signal an implicit meta-theoretical orientation, which reveals, at a more fundamental level, the nature of those arguments.

All arguments listed above show an implicit commitment to a variant of instrumentalism. Instrumentalism, as is well known, is the meta-theory according to which the truth status of a substantive theory can be ignored. Only its usefulness for specific purposes matters. Either theories are not regarded as true or false, or it is not considered

<sup>13</sup> Of course, other mainstream arguments exist defending the use of production functions. However, either they are not materially different from those presented here, even if they mention 'economic poetry' (McCloskey, 1998), or they are insufficiently elaborated, notably when they invoke 'ideology' (Romer, 2015).

relevant that they rest on acknowledged fictions. They just need to perform according to some criterion, often related to the ability to reproduce some empirical results.

Friedman's (1953) apparently immortal argument is often regarded as the main (implicit) defence of instrumentalism in economics. Friedman states that making accurate predictions is the only criterion for assessing the validity of a theory. In fact, his position is not exempt from ambiguities—he sometimes seems to be defending falsity, rather than its irrelevance—and his reasoning is not philosophically grounded. For our present purposes, however, Friedman's well-known position will do.<sup>14</sup>

Even instrumentalism cannot entirely avoid ontological issues, of course. In Friedman's version, as we interpret it here, there is an implicit assumption that the world is so constituted that successful empirical predictions are achievable—as Lawson (1992) puts it, that it is a closed system. This is why nothing else matters.

Most arguments in the preceding section are transparently connected to Friedman's single criterion for the validity of a theory. Again and again, we find the claim that production functions enable predictions, or that their econometric basis is solid. Prediction—and, relatedly, empirical tractability—overrides any other concerns: it is apparently the chosen criterion to settle the value of the neoclassical theory of capital. What matters is whether the predictions of this theory fit empirical evidence, and it is claimed that they do.

A few arguments are (implicitly) instrumentalist in a sense that differs from Friedman's but does not preclude the label: their meta-theoretical foundation is equivalent. This is the case of Solow's 'illuminating parable' claim, which must mean that the theory in question is a mere construct, useful for certain purposes other than prediction, the truth status of which is irrelevant. Or of the claims that the aggregate production function is analytically convenient, since it avoids mathematical complications and facilitates the deduction of macroeconomic results, which can afterwards be compared with data. Or of the postulate that growth and cycles theories must be coherent, which brought the production function to the centre of macromodelling in the 1980s. What this means is that predictions from cycle theories need to match those empirical facts that are invoked to justify growth models based on the aggregate production function.<sup>15</sup>

<sup>14</sup> There is of course a huge literature on instrumentalism and on Friedman as an instrumentalist (or perhaps not), on which we draw at this juncture. See, for example, Boland (1979), Caldwell (1980, 1994), Lawson (1992, 2001) and Mäki (1992).

<sup>15</sup> In identifying the mainstream arguments in support of their flawed capital theory as (implicitly) instrumentalist we are *not* claiming that the proponents of those arguments would necessarily endorse instrumentalism as a meta-theory. As an anonymous referee has rightly observed, Samuelson (1963) criticises Friedman, having coined the term 'F-Twist'. In turn, Solow sometimes reads like a realist, for example, when he dismisses the dynamic stochastic general equilibrium framework:

[A] macroeconomics ... deduced from a model in which a single immortal consumer-worker-owner maximizes a perfectly conventional time-additive utility function over an infinite horizon, under perfect foresight or rational expectations, and in an institutional and technological environment that favors universal price-taking behavior. ... No one would be driven to accept this story because of its obvious 'rightness'. After all, a modern economy is populated by consumers, workers, pensioners, owners, managers, investors, entrepreneurs, bankers, and others, with different and sometimes conflicting desires, information, expectations, capacities, beliefs, and rules of behavior ... To ignore all this *in principle* does not seem to qualify as mere abstraction—that is setting aside inessential details. It seems more like the arbitrary suppression of clues merely because they are inconvenient for cherished preconceptions. (Solow, 2008, pp. 243–44)

In short, in claiming that mainstream defences of their theory of capital are instrumentalist, we are not implying that the methodological *statements* of those economists are clear-cut, consistent or that they (always) cohere with their substantive work. As shown elsewhere (see Graça Moura, 2002, 2017), this is not so even in the case of authors whose meta-theoretical contributions are in a different league, like Joseph Schumpeter—Samuelson's teacher, incidentally.

### 5.2 The mainstream arguments on their own terms

The justifications mainstream economists provide for their ongoing use of aggregate production functions must be persuasive, or acceptable, *for them*. Whether these justifications are persuasive or not *for others* is immaterial to our claim that they are implicitly instrumentalist arguments. Let us now assess the merits of these arguments *on their own terms*.

Production functions are argued to facilitate empirical studies and econometric results are claimed to be satisfactory. But, to begin with, there is no real-world referent to capital as defined at the theory level. No such entity as a homogeneous physical good exists. In empirical exercises, the measure of capital is taken from statistical tables: it is the monetary measure of the stock of physically heterogeneous capital goods (Petri, 2017).

Moreover, the belief that data do not disprove, but could have disproved, the aggregate production function, has itself been disproved. The literature on this topic has its roots in Brown (1957), Simon and Levy (1963), Shaikh (1974), Simon (1979) and even Samuelson (1979). These authors study Cobb and Douglas (1928) and articulate various versions of an argument to the effect that the empirical estimates of the parameters of production functions are predetermined by an accounting identity. Output and inputs in statistical data sets are expressed in monetary terms. Using these data to estimate a production function amounts to estimating an identity because the exponents of the production function will necessarily be the factor shares.<sup>16</sup> No such estimation can possibly contradict the proposition that factor shares are determined by the contribution of each factor to total output (i.e. by marginal productivity).<sup>17</sup>

Finally, the argument that theoretical consistency between growth theory and cycles theory justifies the use of production functions reveals an arbitrary, or at least mysterious, consistency hierarchy. Why is it so important that cycles theory be consistent with growth theory whilst the theory of capital deployed is inconsistent with a setting with distinct capital goods?

In short, the arguments offered by the mainstream are shaky on their own terms. Some additional comments are therefore in order. They appear in the concluding section.

## 6. Concluding remarks: re-assessing the mainstream arguments

From time to time, the topics of the Cambridge controversies resurface, or arguments first articulated then are reframed. There are interesting discussions of the underlying methodological wrangle and of specific methodological pronouncements of the Cambridge side (see Cohen, 1984; Birner, 2002; Cohen and Harcourt, 2003). Mostly, these discussions are concerned with the trajectory of the controversies, especially after 1966, and their eventual demise.

<sup>16</sup> Consider the fundamental equation  $\widehat{Y}_t = \lambda + \alpha \widehat{L}_t + \beta \widehat{K}_t + \widehat{u}_t$ , where  $\lambda$  is total factor productivity growth. The following identity holds for variables in data series, expressed in monetary terms:  $\widehat{Y}_t \equiv \lambda + a_t \widehat{L}_t + (1 - a_t) \widehat{K}_t + \widehat{u}_t$ , where  $\lambda$  is a weighted average of the rate of growth of factor incomes. Thus, an estimation of the former equation will yield  $a_t$  and  $1 - a_t$  as estimates of  $\alpha$  and  $\beta$ , respectively, regardless of whether perfect competition prevails (which it never does, of course).

<sup>17</sup> More recent work has shown that this problem is not exclusive to Cobb–Douglas functions. Instead, it applies to any aggregate production function, such as CES production functions, where factor shares need not be constant over time (see Felipe and Fisher, 2003; Felipe and McCombie, 2014, 2020).



The problem we address here is a distinct one. Independently of methodological differences, which by then may not have been fully thought out by the participants in the debate, Samuelson accepted in 1966 that the Cambridge economists' theoretical argument *was* right. We collect and assess the arguments mainstream economists offer for their continuous reliance on the concept of an aggregate production function, despite its acknowledged technical problems. We find, or conclude, that their arguments are very similar, meta-theoretically and indeed substantively as well. Implicitly, mainstream economists tend to subscribe to a view of the production function as a mere instrument of analysis, an instrument that is alleged to perform well in empirical exercises, connecting theory and data. Sometimes 'empirical validity' or 'predictive success' takes a backseat to 'analytical convenience'. In either case, an implicitly instrumentalist argument is used to render the results reached in the controversies irrelevant and preserve the production function as a centrepiece of mainstream macroeconomics.

Instrumentalism seems to legitimise a neglect of ontological inquiry; that is, in part, its rationale. If theoretical categories have only an instrumental value, why bother about their truth status? It is irrelevant whether these categories have real-world referents. Correspondingly, attributing false properties (such as homogeneity) to capital does not matter for mainstream economists, provided that empirical performance is considered acceptable.

It is *prima facie* more surprising that the empirically based arguments recruited to support the use of production functions are unsustainable *on their own terms*—if you will, shaky—and that this obviously serious problem does not seem to matter very much to a multitude of very competent researchers. In our view, this suggests an unhealthy *conformism*. Despite their pronouncements on empirical relevance and the like, mainstream economists regard their approach *a priori* as the right, if not the only scientific, approach to economic problems. Methodologically, economics is identified with, and accordingly taught as, mainstream economics. Substantively, the relevant fragments of previous economic literature have, or so it is alleged, already been incorporated into the mainstream framework: witness, for instance, Arrow and Hahn's (1971) patently wrong claim to the effect that general equilibrium theory refines Adam Smith's insights.

For this conformism to be conceivable and subsist, it is a prerequisite that mainstream economists be a hugely dominant, self-reproducing majority, which of course they are, a majority so powerful whose stance it is more expedient not to question. This self-reproducing majority and its conformist position are, of course, the counterpart of the ongoing neglect of ontological inquiry. Ontology *is* unavoidable (see Lawson, 1997, 2015). To repeat, even instrumentalists viewing theories as predictive tools implicitly assume that the world is so constituted as to be predictable. In the absence of explicit and sustained ontological reflection, which the institutional context of academic economics most certainly does not favour or reward, it is unsurprising that methods and concepts at odds with what we know about the world linger on. Their meta-theoretical grounds may be fragile, to put it mildly, but these grounds are not even thoroughly articulated by the mainstream, much less scrutinised. This is where we are and have been for a long time. Which is why wrong concepts of capital live on.

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