Putting an end to the aggregate function of production... forever?

Review of Jesus Felipe and John McCombie's book: *The Aggregate Production Function and the Measurement of Technical Change: Not Even Wrong* (2013) Cheltenham, UK and Massachusetts, USA: Edward Elgar Publishing. 400 pages. ISBN: 978-1-84064-255-1.

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Abstract
Since the publication of a famous article by Solow in 1957 in which he seems to give empirical evidence to the aggregate production function, different respected neoclassical authors have shared their doubts about the results obtained.

For some it is nothing but a tautology, for others it is simply the result of an accounting identity – the two criticisms sometimes overlapping. As a matter of fact, both are right. In their book, *The Aggregate Production Function and the Measurement of Technical Change: Not Even Wrong*, Felipe and McCombie give a detailed account of these criticisms. They show – using both (elementary) mathematics and econometrics – why the "empirical results" obtained based on the alleged existence of an aggregate production function are absolutely misleading.

In this article we provide an overview of their main arguments – which are very simple and clear, contrary to the obscure "Cambridge controversies" –, with the hope to convince everyone to definitively abandon the aggregated production functions, both in theory and practice.

The aggregate production function plays a central role in macroeconomics. It is very often identified with the work of Cobb-Douglas and Robert Solow's model of growth. In the recent debate about "mathiness", Paul Romer opposes "science" and "politics". He gives the example of Robert Solow:

> "…*who was engaged in science* when he developed his mathematical theory of growth… [and] mapped the word "capital" onto a variable in his mathematical equations, and onto both data from national income accounts and objects like machines or structures that someone could observe directly."

and of Joan Robinson:

> "…*who was engaged in academic politics* when she waged her campaign against capital and the aggregated production function" (Romer P. 2015, our emphasis)[2].

The aggregate production function, and Solow's model, is also the point of departure of the *Real Business Cycle* model and its siblings, the *DSGE* models (Prescott, 1988). The same can be said about Thomas Piketty's *The Capital of the 21st century*.

For a sensible person, it is obvious that quantities of different kind of goods cannot be aggregated in a quantity of "a good" – even imaginary. If asked, no economist would say that

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[2] Commenting Romer, even the very cleaver Robert Waldmann approve the choice of Solow's model as an example of science as it "fits the data surprisingly well" (Waldman, 2015)

it is possible to describe all of society's possible combinations of productions and inputs with one function – regardless of the number of its variables. Why, then, does the community of economists accept the idea that a country's economy can be "described", or characterized, by a function of two variables ("labour" and "capital")? Well, because for some mysterious reason, "it works" – at least in some important instances. In fact, it is only since 1957, when Solow published an article where a Cobb-Douglas function gave a "remarkable fit" with US GDP data, that the aggregated production function became very popular – not theoretically founded, but empirically true. As Solow once remarked to Franklin Fisher:

> "Had Douglas found labor's share to be 25 per cent and capital's 75 per cent instead of the other way around, we would not now be discussing aggregate production functions" (Fisher, 1971, p 307).

Facts rather than theory or, simply, common sense.

But common sense was not completely lost, even among neoclassical economists, as a few raised their voices – including those of Henry Phelps Brown, Franklin Fisher, Herbert Simon and even, in a way, of Paul Samuelson[3] – to point out that behind Solow's "miraculous adjustment" there is a statistical test of an accounting identity (which is by definition always true). Although prestigious, these voices went unheard.

Those criticisms were totally justified. Even Solow, who first tried to answer them, didn't really insist much. Perhaps because, as he noticed in his Nobel lecture, following the results obtained in his 1957 article, "a small industry", which "stimulated hundreds of theoretical and empirical articles", has established itself around the aggregate production function, which has "very quickly found its way into textbooks and in the fund of common knowledge of the profession" (Solow, 1987a).

This "industry" has gained such a predominant role that nearly no one wants to see it fall down under the pressure of those criticisms. The various flaws related to the problem of aggregation or the relevance of the production function are hence totally ignored by the academic world – teaching included.

Luckily, everybody hasn't given up. First Anwar Shaikh showed how the "remarkable fit" of an aggregate Cobb Douglas function, whose variables are measured in value, can be explained by the accounting identity relating its variables, provided that the factors of production shares are (almost) constant – a "stylized fact" is largely accepted (Shaikh, 1974). What was presented as a *consequence* of the theory – the fact that the factors shares are constant – is actually coming from the data. This invalidates the empirical tests of the production function, as they are simply tests of an accounting identity. But, above all, there is the work of Jesus Felipe and John McCombie who have shown how the combination of an accounting identity – inevitable as aggregates are not measured in quantity but in value terms – and a few "stylized facts" suffice to reproduce, or explain, the supposedly "miraculous" results of Solow but also of Cobb, Douglas and many others – without any need of a fictitious production function. Felipe and McCombie have published, on their own or together, more than 30 articles on this

[3] In his tribute to Douglas after his death, he raises some criticism about "across-industry fitting" which have not received "the attention it deserves". He even says that "on examination, I find that results tend to follow purely as a cross-sectional *tautology* based on the *residual* computation of the non-wage share" (Samuelson, 1979, emphasis in the original). For more details, see Felipe and Adams (2005).

question, where they have mobilized the most recent econometric techniques and built several simulations which support their views.

All their results are recalled in their book, published in 2013's last quarter, *The Aggregate Production Function and the Measurement of Technical Change: Not Even Wrong* (Edward Elgar). This book is so rich in content that it should, at least, be present in libraries of all economics departments around the world. Its exhaustive nature, the recollection of the debates around the aggregate production function, the review and thorough refutation of all the objections which might be made to the explanation offered, show that we are in the presence of a seminal work – although, at times, a little tough to read. This is why we will restrain our review to the essential point of the book, the relation between the production function and the accounting identity.

The "divine surprise" of the Cobb-Douglas adjustment

Felipe and McCombie remember how Cobb and Douglas had the idea, in 1928, to adjust a function of the form $F(K,L) = AL^{\alpha}K^{\beta}$ to the data on the GDP of the United States between 1899 and 1922.

To their great satisfaction, they found estimates of α and β whose sum is little different from 1, with the values concerning the share of income going to labor and capital quite close to those actually observed. One of the main criticisms made at Cobb and Douglas by their contemporaries is the role almost absent of technical progress in their function. Douglas was, actually, aware of it since he had opted for the study of inter-industries data (not including any temporal dimension), which avoids this problem. He then obtains far better results, without however managing to really convince the profession, which had not completely lost its common sense – how can it be accepted that the industries of a country, in all their diversity, can be described (correctly) by a function of (only) two variables?

In reality it took thirty years for the aggregate production function to be accepted by a large majority of economists. Robert Solow's article "Technical Change and the Aggregate Production Function", published in 1957, seems to have much contributed to ease the judgments on this point. In this article, Solow, who knows well the (unsolvable) problems posed by the aggregation, adopts a very prudent attitude, as he begins by remarking that:

> "…it takes something more than the usual 'willing suspension of disbelief' to talk seriously of the aggregate production function" (Solow, 1957, p. 312).

As the title of the article points out, his purpose is to try to isolate and then measure the effect of technical progress, identified by the letter *A* in the Cobb-Douglas function. Unlike them, Solow considers that this effect is not constant and proposes a method to measure it. This allows him to isolate and "eliminate" it, so as to keep only what in the product is given by the single combination of labor and capital "factors". He then obtains, despite "the amount of *a priori doctoring* which the raw data had undergone", a "fit remarkably tight", with a correlation coefficient higher than 0.99 (Solow,1957, p 317). In addition, the estimates of the elasticities α and β are very close to the shares of labor and capital in the product observed values.

Is this too good to be true?

The doubts

Very few questioned Solow's "surprising" results. Warren Hogan was, seemingly, the first to remark that in Solow's data, the shares of labor and capital are almost constant – 0.344 for the capital, the rest for labour, with a variation coefficient of 0.05 (Hogan, 1958). Taking that into account, it stems from the way in which Solow "eliminates" the effects of the term A within the aggregated production function that the term remaining in L and K is necessarily of the form $L^a K^{1-a}$, where a is the observed share of labor (and $1 - a$ the capital's one). According to Hogan, we are therefore in the presence of a tautology: the result obtained is present in the hypothesis.[4]

Almost at the same time, Henry Phelps Brown published an article where he suggested that Cobb and Douglas good fitting may be the result of "a mere statistical artifice" (Phelps Brown, 1957). He observes that it is not the "technical" relation between *quantities* $Q = AL^{\alpha}K^{\beta}$ – that is, a production function – that is tested but the relation, $V = cL^{\alpha}J^{\beta}$, between *values* which are related by the accounting identity:

$$V \equiv wL + rJ,$$

where w and r are, respectively, the wage and the capital rate of return, and V and J, respectively, the value of product and capital.

Six years later, Herbert Simon and Ferdinand Levy gave a more precise content to Phelps Brown criticism (Simon and Levy, 1963). In "A Note on the Cobb-Douglas function", they showed how the accounting identity can explain the data's "remarkable fit" by a Cobb-Douglas function, provided that the wage and the rate of return are constant across industries or over time. The fact that – like Phelps Brown – Simon and Levy use the relation between *values*, and not quantities, plays a central role in their demonstration.[5]

Simon thought these criticisms serious enough to mention them in his Nobel Memorial Lecture. He recalls that "the empirical results" related to the aggregate production functions "do not allow to draw a conclusion on the relative plausibility" of different theories that are at the origin of these functions (Simon, 1978). A year after the lecture, he published an article, "On Parsimonious Explanations of Production Relations", where he "examines three sets of macroscopic facts which can be used to test the classical theory of production". He concludes that:

> "…none of them provides support to the classical theory. The adequacy to the data of the Cobb-Douglas and CES functions is misleading – *the data in*

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[4] In his reply to Hogan, Solow concedes that he should have "warned the reader explicitly that the method would automatically produce a perfect Cobb-Douglas function fit if the observed shares where constant" (Solow, 1958). But he argues that, as shares "wiggles", his reasoning is a "good tautology", not the "bad one" suggested by Hogan (Felipe and McCombie, pp. 167-168).

[5] Taking the logarithm of the ratio of $V_0 = AL_0^{\alpha}J_0^{\beta}$ and $V_1 = AL_1^{\alpha}J_1^{\beta}$ and using the approximation:

$\ln(x/y) \approx x/y - 1$, provided that x and y are not too much different, we obtain the approximation:

$V_1/V_0 - 1 \approx \alpha(L_1/L_0)+\beta(J_1/J_0) - \alpha - \beta$. Multiplying both sides by V_0 and rearranging, we get:

$V_1 \approx (\alpha V_0/L_0)L_1+(\beta V_0/J_0)J_1+ V_0(1 - \alpha - \beta)$. This look like the accounting identity $V \equiv wL + rJ$ with the constant term equal to 0. Somebody testing data verifying this identity will believe that he have "proved" the marginal distribution theory ($\alpha + \beta = 1$, $\alpha = wL_0/V_0$, $\beta = rJ_0/V_0$). Note that the relations are not exact because of the log approximation and because w and r generally vary across data.

> *fact reflect accounting identity between the value of the inputs and outputs*" (Simon, 1979, our emphasis).

In their book, Felipe and McCombie confirm Simon's conclusion with a "simple simulation exercise". They construct an "artificial data set of 25 observations", assuming that the shares of wages "vary as if drawn from a normal distribution with a standard error of 2%, which is plausible when compared with actual values of labour's shares". They obtain a "very good fit" with the Cobb-Douglas function, notwithstanding the fact that data are generated by the accounting identity (Felipe and McCombie, p 57).

The mystery (finally) resolved

Hogan, Phelps Brown, Levy and Simon felt that that behind the "remarkable fits" obtained with the Cobb-Douglas function (at least in some important cases), there is simply a trick. Each of them advanced a plausible explanation. In an article entitled "Laws of production and laws of algebra : the Humbug Production Function", published in 1974 in *The Review of Economics and Statistics,* Anwar Shaikh goes further and shows that the:

> "…puzzling results of the empirical results is (…) the mathematical consequence of constant [factors'] shares (…) and not to some mysterious law of production" (Shaikh A., 1974, p 116).

The proof is so simple that it is astonishing that nobody found it before. Shaikh begins by taking the derivative of the two members of the accounting identity, $V \equiv wL + rJ$, assuming that all the variables may vary in time or in the space, depending on the type of study done. Dividing by *V* and doing some elementary manipulations, the following linear relation between growth rates (marked by the symbol ^) would appear:

$$\hat{V} \equiv a\hat{w} + (1-a)\hat{r} + a\hat{L} + (1-a)\hat{J},$$

where *a* is the labour share, *wL/V*, of the product (therefore 1 – *a* is the capital part, *rJ/V*)[6].
If, in addition, we accept the "stylized fact" according to which the shares of labour and capital are constant (in time or space, depending on the case), we can show by a simple calculation that the accounting identity (1) implies the identity:

$$V \equiv BL^{a}J^{1-a}.$$

The identity (2) looks incredibly like the Cobb-Douglas relation! It may be, therefore, that the one who performs (foolishly…) a regression of the *V* on the L and *J* willing to test a causal relationship gets a "perfect" adjustment (with a R^2 equal to 1) – after "extracting", as Solow does, the effect of the "residual" *B*. The only condition is the constancy of *a*. The formula (2),

[6] Dividing by *V* the derivative of the accounting identity: $V' \equiv w'L + wL' + r'J + rJ'$, it comes:

$$\frac{V'}{V} \equiv \frac{w'L}{V} + \frac{wL'}{V} + \frac{r'J}{V} + \frac{rJ'}{V}.$$

Noting $\hat{x}$ the rate of growth *x′/x* of the variable *x* and observing that:

$$\frac{w'L}{V} = \frac{ww'L}{wV} = \frac{wL}{V}\frac{w'}{w} = a\hat{w},$$

$$\frac{wL'}{V} = \frac{wL'L}{VL} = \frac{wL}{V}\frac{L'}{L} = a\hat{L},$$ etc., the identity (1) follows.

which may be obtained by any first year student in economics, therefore explains the "puzzle" of the "remarkable fit" obtained by Solow and others with an aggregate production function.

More generally, it allows us to understand why statistical adjustments with a Cobb-Douglas function can sometime lead to astonishing results and other times to mediocre ones. It mainly depends on the constancy of *a* and of the variability of $B\ (= a^{-a}(1-a)^{-(1-a)}w^{a}r^{1-a})$ – that is, of wage and profit rates.[7]

This brings us to the question of the "total factor productivity", so prevalent in empirical studies.

On "total factor productivity"

Solow's 1957 article's title is *Technical Change and the Aggregate Production Function.* Its goal was to distinguish, in the growth rate, what is relative to the "factors" themselves and what is relative to the "residual"– particularly, technical change.

This distinction can be shown easily with homogenous functions of degree 1 – as the Cobb-Douglas function. For instance, taking the logarithmic derivative of both members of the relation "in quantities":

$$Q = AL^{\alpha}K^{1-\alpha},$$

gives:

$$\hat{Q} = \hat{c} + \alpha\hat{L} + (1-\alpha)\hat{K}, \quad with\ \hat{c} = A'/A.$$

The growth rate of *Q* is given by the sum of the factors growth rate, $\alpha\hat{L} + (1-\alpha)\hat{K}$, and of the term $\hat{c}$ which represents the other factors which influence the growth of the product– essentially, the technical progress. This "residual" influence on growth is thus given by:

$$\hat{c} = \hat{Q} - \alpha\hat{L} - (1-\alpha)\hat{K}.$$

If we proceed in the same way with the function in value terms, $V = BL^{\alpha}J^{1-\alpha}$, we obtain the equivalent in value of $\hat{c}$, the so-called "total factor productivity" rate of growth:

$$\widehat{TFG} = \hat{V} - a\hat{L} - (1-a)\hat{J}.$$

Although the formulae (5) and (6) are very similar, they may lead to drastically different results. Felipe and McCombie give an example where they use hypothetical data to calculate the growth of total factor productivity for an industry which consist of ten firms, with *the same* Cobb-Douglas function and *the same rate* of technical progress of 0.5% per annum – then, it is possible to talk about the rate of technical progress being 0.5% per annum *in the industry* (Felipe and McCombie, p. 107). They value each individual firm's product and constant price capital stock assuming the mark-up: *wL* for labour, *wL*/3 for capital, the product playing the role of *numéraire* (see note 2). Industry values are obtained by adding individual firms' values.

[7] The greater variability in time than in space of *w* and *r* –and then of *B* – explains why Douglas found better results with the inter-industrial data than with the chronological series.

The rate of total factor productivity growth obtained by using the aggregated value data came to 1.48% per annum – almost three times the “real” or “technical” rate. The reason for the notable difference between these growth’s rates is, obviously, due to the choice of prices – here, the mark-up rule – used to compute the aggregated output and “factors”.

This is an illustration of why, as Felipe and McCombie explain,

> “…what the neoclassical theory called ‘total factor productivity’ is, *tautologically*, a function of wages and profit rates” (p. 209, the authors’ emphasis).

More generally:

> “…as far as there is no underlying production function, it is not possible to calculate separately the contribution to the growth of technical progress (growth of TFP) and the growth of each factor” (ibid).[8]

It is then not a surprise if Felipe and McCombie explain at the end of the two chapters (5 and 6) of their book dedicated to the total factor productivity that:

> “…in our opinion, the concepts of TFP and the aggregate production function serve more to obfuscate than to illuminate the important problem ‘why growth rates differ’” (p 209).

A comment on Solow’ attitude

Robert Solow is one of the most open minded neoclassical economists[9]. We have noticed that from the beginning he was very cautious about the existence of aggregated production functions (“the ‘willing suspension of disbelief’ to talk seriously of the aggregate production function”).

In his *Nobel Memorial Lecture*, he expressed reservations on the use that may be made of the total factor productivity, a subproduct of his model:

> “these total-factor-productivity calculations require not only that market prices can serve as a rough and ready approximation of marginal products, but that aggregation does not hopelessly distort these relationships. (…) So I would be happy if you were to accept that the results I have been quoting point to a qualitative truth and give perhaps some guide to orders of magnitude. To ask for much more than that is to ask for trouble” (Solow, 1987a).

He more or less accepted Hogan’s critique, even if he tried to escape from it by playing with the world “tautology”. He didn’t publicly answered to Phelps Brown, Simon and Fisher papers – even he had private discussions with them.

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[8] Felipe and McCombie show the relationship between the accounting identity and the *Constant Elasticity of Substitution* and the *translog* production functions, which can sometimes better fit the data that the Cobb-Douglas function – all depends of the variability of factors’ shares, wages and profits (pp. 84-89).

[9] He is among the very few mainstream economists who clearly state that the “representative agent” is a non sensical assumption.

By contrast, he didn't like at all Shaikh's criticism – even if it was not fundamentally different from Simon and Fisher's ones. His riposte was brief and along the lines of his reply to Hogan – confusing –, but also unfair. Confusing, when he argues that the intention of his 1957 paper was to "yield an exact Cobb-Douglas and tuck everything else into the shift factor" (Solow, 1974), while in the 1957 paper he expressly stated his intention to "reconstruct the (underlying) aggregate production function". In his reply to Solow[10], Shaikh asks then:

> "If, as Solow now claims, he knew all along that the underlying production function would be a Cobb-Douglas, then why bother "reconstructing" it? Why the surprise at the tightness of fit and the "inescapable impression of curvature"? Why does Solow need regression analysis to 'confirm the visual impression of diminishing returns"[11] (Shaikh, 1980).

Along the same line, we, in turn, could ask: why, in his Nobel lecture, Solow reminds that before his "surprising results", he was "very skeptical about this device [the introduction of some technological flexibility]" (Solow, 1987)?

But that's not all: Solow added bad faith to confusion. To give a proof that his approach is not tautological, he estimated the relation $V = AL^{\alpha}J^{1-\alpha}$ using Shaikh's data but *arbitrarily assuming* that the "residual" $A(t)$ is approximated by an exponential function (or equivalently, that its logarithm, $lnA(t)$, is a linear function of t). He obtained a very bad fitting ($R^2 = 0{,}0052$!) and thought that he had delivered *le coup de grâce* to Shaikh, ignoring – or feigning to ignore – the fact that the latter chose deliberately a fictional data set which draws (ironically) the word HUMBUG (assuming only approximate constancy of factor shares). The values of $A(t)$ corresponding to this set oscillate rather than grow smoothly (as it is shown in the figure 2 of Shaikh's paper). In his reply to Solow, Shaikh proposed another proxy of $A(t)$, which allowed him to deduce from his HUMBUG data set a Cobb-Douglas function that "remarkably fitted" the data ($R^2 = 0{,}82$ and "realistic" factor shares) – as in Solow's 1957 paper.

In turn, Felipe and McCombie take Solow at its words and "test" the Cobb-Douglas production function using Solow's own data but they *assume a linear trend* for $\ln A(t)$ – as he has done with Shaikh HUMBUG example. They ascertain that the results of the regressions "differs markedly from factor shares and, indeed, the coefficient of the capital term is not significant". They wonder:

> "…whether Solow's 1957 paper would have had such a dramatic impact if these regressions had also been reported" Felipe and McCombie, 2013, p 172).

Solow's didn't reply. Indeed, thirteen years later – after winning the "Nobel Prize" – he tried again to answer Shaikh with a different method, which consists of examining the model at the micro ("technical") level (Solow, 1987b). But as Felipe and McCombie reminds us:

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[10] In fact, Shaikh was not allowed to reply to Solow in the same review and could only publish its response 4 years later, in the "postscript" of a book chapter – with a (very) limited diffusion.

[11] The "curvature" and the "impression of diminishing returns" concern figures where the relation $Q = A\,F(L,K)$ is expressed in the form $q/A = f(k)$, where $q = Q/L$, $k = K/L$ and $f(\cdot) = F(\cdot,1)$ (remember that F is homogeneous of degree 1).

> "It is possible for Shaikh's critique not to apply at the engineering production functions, but still be applicable to the use of value data" (Felipe and McCombie, 2013, p 181).

It is quite fascinating to see how a person as intelligent as Solow tried, in vain, to fight against the "laws of algebra": Shaikh's argument is exclusively based on two algebraic relations, the identity relation and the constancy of factors shares.

Solow's name is closely linked with the so-called "neo classical growth model", with the aggregated production function and the marginal distribution theory. Maybe, it is impossible for him to admit that behind all that there is banal identity account – even if he agrees that the results of the model "point at most to a qualitative truth".

Conclusion

Since their introduction, aggregation and production functions problems have been at the heart of particularly heated discussions. Sometimes, though, the debates got lost in secondary details, as with the questions of the " reswitching" and the "reverse capital deepening", that are, in fact, missing the point, as Franklin Fisher rightly notes in an article entitled "The Aggregated Productive Function – a Pervasive, but Unpersuasive, Fairytale":

> "The Sraffians consider the existence of reswitching and reverse capital deepening to be a decisive criticism of neoclassical theory. They believe that this was the deciding factor in the Cambridge debates over capital theory. But that view fails to realize the following. Reswitching and reverse capital deepening only appear paradoxical if one supposes that aggregates should behave the way intuition suggests they should behave – the way that factors of production and outputs behave at the micro level. *But the non-existence of aggregate production functions means that such intuition simply does not apply*. No further consequence can be read from its failure" (Fisher, 2005, our emphasis)."

The defense of the existence of aggregated production functions is exclusively based on empirical grounds – no sensible person can (or at least should) accept its existence on a theoretical one. In their book, Felipe and McCombie examine some of the models, often given as examples in textbooks, which are supposed to "confirm" empirically the existence of an aggregate production. They prove, in each case, that the observed fit can be explained by the accounting identity and the factors' shares, wages and profits variability. They therefore solve definitively the mystery of the (empirical) aggregated production function that had puzzled – or should had puzzled… – economists for near a century. By the way, it provides the means to demolish anyone who pretends to prove, "on empirical grounds", the existence of an aggregated production function – or who assume its existence to "prove" some "result" or "theorem".[12]

[12] In the last chapter of their book, Felipe and McCombie answer to the objections made by a handful of macroeconomists who still try to defend the existence of aggregated production functions, often by a circular reasoning – they first accept their existence and then see a "proof" of that in the data's good fitting.

Despite all that, aggregate production functions continue to populate textbooks as well as theoretical and applied works. In fact, it has been a long time since the issue of the aggregation of goods and functions has stopped being on the agenda. It has virtually disappeared from the university. Two reasons can explain such an attitude from those who do not cease yet to state their commitment to rigorous analyzes.

The first reason is ideological. It is reassuring to be able to affirm that the (delicate) issue of income distribution has been solved in such a simple – and effective for the society – way by the retribution to each according to their marginal productivity, provided that the markets are "competitive". The other reason is of a practical nature: the "industry" which is built around the aggregate production function is so important that questioning it would be catastrophic for those who benefit from it, while making it thrive.

We hope that Felipe and McCombie's book will help to change this situation. It will, at least, provide to those questioning the aggregate production function – and the underlying theory of the distribution – all the answers they seek, both on the theoretical and practical levels. It is why this book is so important, in particular for students and researchers in economics.

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