**Making the function of production a thing of the past?**

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

The question of the aggregate production function remains at the heart of debates in macroeconomics. The theory of the distribution that it is associated with serves as a substrate at the core of the work of Thomas Piketty, *The Capital of the XXIst century* - even if its author seeks to [take its distances from it](http://potemkinreview.com/pikettyinterview/). The aggregate production function also plays an essential role in the models of the "new macroeconomics", that of the *Real Business Cycles* - [presented](http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2004/prescott-lecture.pdf) as an extension of the Solow growth model - and in its variants *DSGE* (for *Dynamic Stochastic General Equilibrium*), models still preponderant in the academic world and that would be used by some central banks.

The aggregate production function has been subject to many criticisms in its first formulations, the emphasis being placed on the problems posed by the aggregation of goods and behaviors - issues that are central in the famous [Cambridge Capital Controversy](http://en.wikipedia.org/wiki/Cambridge_capital_controversy). Although it has lost the battle on the theoretical level, the aggregate production function has only prospered, and that is thanks to an almost miraculous result: the adjustment almost perfect (R²= 0.99) of a Cobb-Douglas function obtained by Robert Solow on data related to the GDP of the United States between 1909 and 1949. Miracle also reproduced in other countries and for other periods. Although it seemed too good to be true - Solow himself did not hide his surprise -, the idea that the aggregate production function "worked" quickly took place, at least empirically. We therefore had an apparently strong and ideologically satisfactory relation, with which we are going to work. What we have is not private.

However, a few voices raised, even within the neoclassical movement - including those of Henry Phelps Brown, Franklin Fisher, Herbert Simon and even, in a certain way, of Paul Samuelson - to point out that behind the "miracle of adjustments" could hide an accounting identity. Although prestigious, these voices were not heard.

In their book, *The Aggregate Production Function and the Measurement of Technical Change: Not Even Wrong*, Jesus Felipe and John S.L. McCombie, recall these criticisms, but go well beyond. They take the data of Cobb, Douglas, Solow, and their successors, and show, using the most recent econometric techniques, that we can obtain their "results" from the accounting identity that binds the aggregates that they use, and sometimes from one or two stylized facts. The "mystery" is solved - and the reason is satisfied. Or it should be, but let’s not delude ourselves: despite the unquestionable knock out brought by Felipe and McCombie, the aggregate production function is not ready to disappear – for as much as it is "practical" for modelers and "ideologically correct".

The book of Felipe and McCombie is so rich in content - it should, at least, be included in all the Economics departments of universities 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 they offer, show that we are in the presence of a seminal work – although, at times, a little tough to read. That is why it seems important to us - given the fundamentality of the subject treated - to recall its essential points, in the hope that it will make you want to go consult it, if not to read it entirely.

1. The production function in theory and in practice

A production function establishes, by definition, a relationship between quantities of goods – the product (output) is obtained from the inputs using the most efficient technique. Even on the microeconomic level, it is basically impossible to find examples of such functions, so much that the production of whatever item involves a variety of different elements, of which a good part is undividable (facilities, tools, equipements). F & M don’t dwell too much on the microeconomic study (aspect) - they even admit the existence of these functions in some of their simulations – since they are interested in the econometrical studies, which only operate at the level of the aggregate function(?). This has the advantage of making economists accept more easily the assumption of substitutability between “factors” whereas, at the company level, common sense would dictate complementary[[1]](#footnote-1). An advantage that spurs more from the ambiguity that comes with working at the aggregate level rather than on real grounds, as shown in this quote of Edmond Malinvaud – one of the few authors that don’t elude the aggregation problem:

At the aggregate level, one has to […] take into account that the same volume of production can have very different composition. For example, to replace natural fabrics with artificial ones can translate into a reduction of workforce and an increase in capital required. The same can happen with the substitution of purely agricultural food products with industrial ones.

The substitution between capital and labor is thus accompanied by a substitution between goods, which makes intervene elements other than technical, such as the tastes of consumers. Added to this are the deadlines for implementation, which make this substitution “play a greater role in the study of the long-term growth than in the one of cyclical phenomena” (p. 124). Assuming that the substitutions are instantaneous, the "overall level" which Malinvaud talked about presupposes nevertheless productions (heterogeneous) and inputs (grouped together in the variable "capital") *in value*. Replacing the quantities with values in the case of some variables may appear harmless. It is nothing of the sort, however, the variables in value, especially when they are of a global nature, macroeconomic, can be related by accounting identities, which make believe in the existence of causal relations where there are not. James Cobb and Paul Douglas, who gave their name to a function famous in economics, are the first to be fallen into the trap.

1. The "divine surprise" of the adjustment by the Cobb-Douglas function

Felipe and McCombie remember how Cobb and Douglas had the idea, in 1928, to adjust a function of the form *F*(K*,L*) = *AL*α*k*ßto the data that they had 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 that are closer to those actually observed. This, as Douglas will tell much later during his speech as president of the *American Economic Association*, "reinforced the competitive theory of the distribution and refuted the marxist theses" (Douglas, 1976)[[2]](#footnote-2). This ideological dimension did not, however, prevent criticism, especially that at the time, skepticism prevailed regarding the interest and scope of econometric studies. One of the main criticisms made at Cobb and Douglas is the role almost absent of technical progress in their function. As a matter of fact, the parameter A is there regarded as constant. The growth pro capita was therefore that of capital pro capita, contrary to what we can observe. Felipe and McCombie resume the data of Cobb and Douglas and show that their results are due in part to luck, in particular on account of the choice of the years considered. Douglas was, in fact, aware of that since he had opted for the study of inter-industries data (not including any temporal dimension), which avoids the problem of technical progress. He then obtains far better results, without however reach to really convince the profession, which had not completely lost its common sense - how can we accept that the industries of a country, in all their diversity, can be described (correctly) by a function of two variables?

In reality it took thirty years for the aggregate production function to become a sort of accepted fact for a large majority of economists. The article by Robert Solow "Technical Change and  the Aggregate Production Function ", published in 1957, seems to have much contributed to disinhibit/freed minds on this point. In this article, Solow, who knows well the (unsolvable) problems posed by the aggregation, adopts a very prudent attitude regarding this function, since it begins by remarking that "it needs a little more than the usual "deliberate suspension of mistrust" to be able to talk seriously about the aggregate production function" (Solow, 1957, p. 312). As the title of the article points out, his purpose is to seek to isolate and then measure the effect of technical progress, identified by the letter A in the function of Cobb and Douglas. 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 - *output*- is due to the single combination of "factors" labor and capital. From the data obtained, he represents the pairs on a graph (product pro capita, capital pro capita).

He gets a scatter plot in the shape of a right very slightly curved, with a correlation coefficient whose square is higher than 0.99!5[[3]](#footnote-3) This seems to surprise him: "Given the number of tampering (*a priori doctoring*) suffered by the data, the adjustment is of a remarkable quality" (*ibid.*). In addition, the estimates of the elasticities α and β are very close to the observed values of the share of labor and capital in the product[[4]](#footnote-4). The marginalist theory of apportionment would be therefore definitively confirmed - and the approximate nature of the results of Cobb and Douglas explained (by a poor assessment of the effects of technical progress).

As Solow noted in his Nobel conference, following the results obtained in his 1957 article, "a small industry", which has "been at the origin of hundreds of theoretical and empirical articles", has established itself around the aggregate production function, the latter having "very quickly found its place in the manuals and in the common fund of knowledge of the profession"[[5]](#footnote-5). This does not prevent it from remembering, thirty years after, its "surprise" before the results obtained. Too good to be true?

1. The doubts

Few were those who questioned Solow’s "surprising" result. Although there was some Hogan who remarked that in Solow’s data the shares of labor and capital are almost constant (0.344 for the capital with a variation coefficient of 0.05 ). If we take that into account, it stems from the way in which Solow "eliminates" the effects of the term *A*within the production function that the term remaining in *L*and *K*is necessarily of the form *LaK*1 - a , where a is the observed share of labor (and 1- *a*the one of capital). We are, consequentially, according to Hogan, in the presence of a tautology: the result obtained is present in the hypothesis (Hogan, 1958). The response of Solow to Hogan is somewhat blurry. He admits that "he should warn the reader that his method leads to a perfect adaptation of the Cobb Douglas function if the observed shares of capital and labor are constant" (Solow, 1958, p. 412). Or, they nearly are... Almost at the same time, Henry Phelps Brown published an article where he proposes another explanation than that of Hogan : the good results obtained with the Cobb-Douglas functional arise from the existence of an accounting identity that binds the variables (in value, for the most part) used during the econometric adjustments (Phelps Brown, 1957). If we identify as *V*the added value of a country (or a company, an industry, a sector, etc. ), *L*the quantity of work, paid at wage *w*, and *J*the value of "capital", paid at the rate *R ,* then these variables are linked by the accounting identity:

1. *V* ≡ *wL + rJ*.

Within these conditions, the production function tested by Cobb, Douglas, Solow, etc. is not the "technical" relation between quantities *Q = ALαKβ*, but the relationship between values:

1. *V = cL*α*J*β.

Phelps Brown remarks that then we have *V ' L =* α*cLα*–1*J*β*= αV/L* et *V ' J =* β*cL*α*J*β–1 *=* β*V/J*. En faisant l’hypothèse « de concurrence parfaite » : *V ' L = w* et *V ' J = r,* il s’ensuit que α *= wL/V* et β *= rJ/V*. Or, il résulte de l’identité (1), divisée par *V*, que : 1 ≡ *wL*  
*V +*  
*rJ*  
*V*, et donc que α + β *=* 1. This is presented as a proof of the marginalist theory of the distribution - constant returns, elasticity of "factors" equal to their share in the product – it is in fact nothing more than an illusion: the result is in the way of calculating the aggregates. Phelps Brown speaks of the "two sides of the same coin" - adjustment by a Cobb-Douglas and accounting identity.

The criticism of Phelps Brown has, it seems, gone unnoticed – despite having been published in a prestigious magazine[[6]](#footnote-6). It is, that is true, a little vague. It thus overlooks the fact that the results obtained by adjusting a Cobb-Douglas can sometimes be mediocre - or even bad -, which is not compatible with an accounting identity. We had to wait another six years for Simon and Levy to go back on the erroneous interpretation to which can reconduct the presence of the accounting identity (Simon and Levy, 1963). In a short note, they show how this identity can be obtained by "linearizing" (by its differential) a Cobb-Douglas function in which the elasticities are equal to the shares of "factors" in the product[[7]](#footnote-7). To do so, they assume not, like Hogan, that these shares are constant, but that the wage and the rate of return are. Two approximations that, therefore, explain why R² is not necessarily equal to 1. Herbert Simon refers to this note in his Nobel conference - although it is only a drop of water in the "sea" of his publications[[8]](#footnote-8)-, when 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[[9]](#footnote-9). Felipe and McCombie provide an illustration of the purpose of Simon by performing a very simple simulation where they show the possibility of achieving results as good as those of Solow from a theory completely different from his[[10]](#footnote-10).

4. A thought-provoking simulation

Felipe and McCombie are located in the case the more favorable to the aggregation, the one where all the companies are represented by the same function of Cobb-Douglas with constant returns. They deliberately choose the coefficients (elasticities) of this function "to the opposite" of those found by Solow at the aggregate level: 0.25 for the labor and 0.75 for the capital (it was 0.75 and 0.25 for Solow). So, for each company: *F(K,L) = AK 0.75 L 0.25 -*with *K* and *L* that can vary from one company to another. If we assume "competitive markets ", the remuneration of labor and capital are given by the marginal productivities in asking *F ' L (K,L) = w and F ' K (K,L) = r*, then 75% of the revenue of the production goes to capital, the remainder goes to work - and this for each of the companies[[11]](#footnote-11). As they have all the same production function, logically we should find this sharing of revenues at the global level. But this is no longer strictly the case if one chooses a different system of evaluation of the product and the return of the capital. Felipe and McCombie assume as well that the price of the single good produced in their simulation is obtained by adding a margin of 1/3 to its cost in work - margin which pays the capital. The work therefore receives 75 per cent (= 1/ (1 +1/3)) of the value *V = wL + 1/3·wL* of the production, the remaining 25 per cent going to the capital. The simulation of Felipe and McCombie is to give aleatory variables (in quantities) to two of the three variables micro, *Q, K* and *L* - the remaining deducible from the relationship *Q = F(K,L).* After aggregation, in value, they adjust the production function macro *V = uLαJβ* on the data thus obtained. They get results at least as good as those of Solow (an *R²* greater than 0.99 ) and the estimated coefficients of α and β very close to his own (0.75 for the labor and 0.25 for the capital). This is not surprising, as these coefficients are in fact determined by the accounting identity *V ≡ wL + J* with, given the rule of price fixing, *rJ = 1/3·wL*. This result invalids as well the marginalist theory of the distribution - which, given the values of α and β at the micro level, predicts the sharing: 25 per cent for the labor and 75 per cent for capital. The need to resort to a system of price completely changes the given "technique" - nonetheless clear in the example chosen.

Felipe and McCombie show as well, like Simon, Levy and Fisher did before them, that the results concerning the functions of aggregated production does not allow to decide between several theories supposed to the explain them[[12]](#footnote-12). But they do not stop there, since they provide a framework of explanation more general than that of Simon, Levy and Fisher, which allows to explain all the results obtained by assuming the existence of production features – where some variables are measured in value - without the need to make this hypothesis. They are inspired in this of the approach of Shaikh who has shown in 1974 - 17 years after Solow’s article! - how the accounting identity can be transformed, in certain conditions, in a relation of Cobb-Douglas (Shaikh, 1974).

5. The mystery (finally) clarified

Shaikh (1974) begins by deriving the two members of the accounting identity *V ≡ wL + rJ*, assuming that all the variables may vary in time - or in the space, depending on the type of study done. Either: *V' ≡ w'L+ wL' +r'J+rJ'*. By dividing by *V* it originates:

1. *V′*

*V ≡*  
*w′L*  
*V + wL′V + r′JV + rJ′V*.

En notant *x*^ le taux de croissance *x′/x* et *a = wL/V* la part du travail (et donc 1 – *a* la part du capital, *rJ/V*) dans le produit, puis en remarquant que *w′L*  
*V =*  
*ww′L*  
*wV =*  
*wL*  
*V*  
*w′*  
*w*  
*= aw*^ , *wL′*  
*V =*  
*wL′L*  
*VL = wLV LL′ = aL^* , etc., l’égalité (3) peut se mettre sous la forme : (4) *V′*  
*V + wL′V + r′JV + rJ′V*.

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*V ≡ aw*^ *+* (1 – *a*) *r*^ *+ aL*^ *+* (1 – *a*) *J*^ *.*

In addition, if it is accepted, the "stylized fact" according to which the shares of labor and capital are constant (in time or space, depending on the case), it is shown by a simple calculation that the accounting identity (4) implies the identity:

*(5)V ≡ BLaJ*1 –*a*

where *B = a* –*a*(1– *a*) – (1–*a*) *wa r*1 –*a* ne dépend ni de *L*, ni de *J [[13]](#footnote-13)*

The identity (5) looks incredibly like the relation of Cobb-Douglas! It may be, therefore, that the one who performs (stupidly …) a regression of *V* on *L* and *J* believing to test a causal relationship gets a "perfect" adjustment (with a *R²* equal to 1) - it is sufficient for that purpose that the elements *a, w* and *r* of *B* are all constant - since what it does is just to "test" an accounting identity. The formula (5), which may be obtained by any first year student of Economics, therefore explains the "mystery" of the aggregate production function. It allows us to understand, for example, why the adjustments according to a Cobb-Douglas can be sometimes excellent and other times mediocre, or even very bad. It all depends on the validity of the "stylized facts": the degree of variability of *a* at the time of the passage from (4) to (5), to which it may be added that of *w* or *r*, which influence, along with *a*, the term *B (= a-a(1-a)- (1-a) wa r 1 -a)* of the relationship (5)[[14]](#footnote-14). Felipe and McCombie also show how in some cases of relatively important variability of these parameters, the transition from (4) to (5) is better done with "more flexible" functions. From which their conclusion :

The use of value data means that it is always possible to obtain, thanks to the underlying accounting identity, a very good adjustment to these data with Cobb-Douglas functions, THESE or more flexible, such as the translog function, the elasticities being equal to the shares of the factors (344).

Felipe and McCombie also review the arguments of the (rare) authors who have tried to deny the role of the accounting identity. They show that they all implicitly assume the existence of an aggregate production function, while leaving to believe - or, even worse, believing - that the (good) adjustments obtained prove this existence. Typical example of circular reasoning.

6. On the "total factor productivity"

The initial comments of Solow’s 1957 article, which was at the origin of the "small industry" that has formed around the aggregate production function, was to distinguish, in the growth, which is relative to the "factors" themselves, and the "rest", which can be attributed to technical progress - in a broad sense - and which has been subsequently designated by the vague term "total factor productivity". Thus, in the case of the Cobb Douglas relation "in quantities":

*Q* = *AL*α*K*1 –α,

in which we take the logarithmic derivative, we have:

(5) *Q*^ = *c*^ + α*L*^ + (1 – α)*K*^ , avec *c*^ = *A′/A*.

If we consider that the term α*L*^ + (1 – α)*K*^ represents the growth rate of the factors (weighted by their "share" in the product), then we can see in the term *c^* the representative of the "other" factors which influence the growth of the product - essentially, the technical progress. If we identify this term as *φ*, it follows from (5) that it is given by the difference, or "the rest":

(6) φ = *Q*^ – α*L*^ – (1 – α)*K*^ .

The measurement of φ is, however, valid only at a company level, assuming that such enterprise can be described by a Cobb-Douglas function in which *K* designates a quantity (of 'machinery', for example) - hypothesis never verified, of course.

"In practice", we resort to variables in value and we prefer to talk about "total factor productivity" (TFG). By analogy with (6), by replacing *Q* with *V* and *K* with *J*, we obtain the equivalent in value of φ:

(7) *PGF*^ = *V*^ – *aL*^ – (1 – *a*) *J*^ .

Although the formulae (6) and (7) are very similar, they may lead to drastically different results. To see this, it is sufficient to resume the simulation described above: the "real" effect of the technical progress *φ* y is given by the formula (6) with α = 0.25 (remember that it has been assumed that all companies have the same production function) but its "observed" value is given by the formula (7) or, because of the price rule adopted (cost in work + margin), it was *a* = 0.75 . The variable *TFG*^ therefore gives a wholly erroneous estimate of the "true value" φ[[15]](#footnote-15).

In the case where it is assumed that the share of "factors" is constant ("stylized fact" ), if we refer back to the identity (5), *V ≡ Blaj1 -a*, proceeding as done for (6) and (7) - passing to logarithms and then derivation - we obtain:

^*V* ≡ *B*^ + *aL*^ + (1 – *a*) *J*^

And therefore :

(8) *B*^ ≡ *V*^ – *aL*^ – (1 – *a*) *J*^ (≡ *PGF*^ ).

Therefore, comparing that with (7), we see that the rate of change of the total factor productivity is given by the logarithmic derivative of *B = a* –*a*(1– *a*)1 –*a wa r*1 –*a*, whether, since we assume *a* is constant:

*PGF*^ = *aw*^ + (1 – *a*) *r*^ .

As Felipe and McCombie said: "what the neoclassical theory called "total factor productivity " is, tautologically, a function of wages and profit rates " (p. 209, their italics), which explains nothing - especially when one compares countries. Anyway, "as far as there is no production function underlying, 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)[[16]](#footnote-16).

It is, of course, always possible to calculate terms such as the ones given by the formula (7), while knowing that it derives from an accounting identity and not from relations which would reflect the technical evolution of the economy. Remains to interpret them, while they have been got rid of their ideological gangue (marginalist theory of distribution).

Conclusion

Among the countless assumptions made by most of the neoclassical theorists, there are three who are a challenge to common sense while being fundamental to them. First, there is the one which states that in perfect competition all agents are price takers - this implies a highly centralized system, the precise antithesis of what the model claims describe. Then comes the hypothesis of the "representative agent" which the intertemporal choice is supposed to reproduce that of the whole economy[[17]](#footnote-17). There is, finally, the aggregate production function, a sort of review of the various techniques available to an economy at a given time. The final argument - in fact, the only one - advanced to justify the hypothesis of perfect competition and of the representative agent is that they allow you to obtain unquestionable (theoretical) results, since products of mathematical deductions - without making it relevant for all that. The situation is different when it comes to the aggregate production function: indefensible on the theoretical level - it doesn’t have, nor can have, any " microeconomic foundation" -, it would draw its legitimacy from its adequacy to the data. The book of Felipe and McCombie shows that this legitimization" by the facts" is an illusion/ decoy. They bring in the evidence both on the theoretical level – the explanation according to the accounting identity and the stylized facts - and on a practical level - simulations and econometric studies. In spite of this demolition in rule, the functions of aggregated production continue, and will surely continue, to populate the manuals as well as theoretical and applied works. In fact, for a long time the issue of the aggregation of goods and functions is no longer on the agenda. It has virtually disappeared from the academic world/ teaching/being taught.

A good part of the profession, like Piketty, seems to think that it has been settled - specifically by Solow. It is nothing, of course. Two reasons can explain such an attitude from of those who do not cease yet to state their commitment to the "rigor" in their analyzes. The first one 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 of each *(what?)* according to its marginal productivity, provided that the markets are "competitive". The other one is of a practical nature: the "industry" which is built around the aggregate production function is so important that questioning it again would be catastrophic for those who live thanks to it, while making it thrive. The book of Felipe and McCombie is not likely to change much in this situation. It will, however, provide to the one questioning the aggregate production function - and on the underlying theory of the distribution - all the answers he seeks, on the theoretical plan as well as practical. That is why it is imperative that, at least, this book is present in all the Economics libraries of France - and the world.

1. A ‘homogeneous” product, well-defined, is composed of inputs combined in fixed proportions, well-defined, by labor and complementary machines - at least if they are fully used, in an effective way, as assumed in the production function. [↑](#footnote-ref-1)
2. Theory mistakenly attributed to Wicksteed. For more details concerning this theory and the debates it has held, including among the neoclassical, see <http://www.bernardguerrien.com/concurrence-et-profit-nul.pdf> . [↑](#footnote-ref-2)
3. See <http://www9.georgetown.edu/faculty/mh5/class/econ489/Solow-Growth-Accounting.pdf> , p. 317 [↑](#footnote-ref-3)
4. In fact, the "scatter plot" gives an R² higher than 0.99 for four types of functions: linear, semilogarithmic, hyperbolic and "Cobb Douglas". But only the latter gives to the parameters values which fit with the marginalist interpretation. [↑](#footnote-ref-4)
5. <http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/solow-lecture.html> [↑](#footnote-ref-5)
6. Including by the explicit target of this criticism, Douglas, who ignores it superbly in all its following publications on "his" function. [↑](#footnote-ref-6)
7. Plus précisément, ils considèrent deux valeurs proches de *V* : *V*1 et *V*2, telles que : *Vi = cLi*α*Ji*β, *i =* 1,2*.* En  
   prenant le logarithme du rapport *V*1/*V*2, il vient ln*V*1/*V*2 =αln*L*1/*L*2 + βln*J*1/*J*2. En supposant que *L*1 > *L*2 et *J*1 > *J*2,  
   avec des écarts faibles, et en se servant de la propriété du logarithme népérien (pour *x* > *y* et un faible écart) :  
   ln*x/y* ≈ *x/y* – 1, on en déduit que : *V*1/*V*2 – 1 ≈ α(*L*1/*L*2 – 1)+ β(*J*1/*J*2 – 1). Soit, après quelques manipulations  
   simples, *V*1 ≈ (*αV*2/*L*2)·*L*1 +(*βV*2/*J*2)·*J*1+(1 – α *–* β)·*V*2. En comparant avec l’identité comptable, *V*1 ≡ *wL*1 + *rJ*1, il  
   vient 1 –α–β ≈ 0 (rendements constants), *w* ≈ α*V*2/*L*2 (donc α ≈ *wL*2/*V*2) et *r* ≈ β*V*2/*J*2 (donc β ≈ *rJ*2/*V*2). [↑](#footnote-ref-7)
8. <http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1978/simon-lecture.pdf> [↑](#footnote-ref-8)
9. Just after obtaining the Prize, Simon published an article where he "examines three sets of macroscopic facts which can be used to test the classical theory of production". It shows that "none of them provides support to the classical theory. The adequacy to the data of the Cobb-Douglas functions and CES is misleading - the data in fact reflect accounting identity between the value of the inputs and outputs" (Simon, 1979). [↑](#footnote-ref-9)
10. They are inspired by it in a simulation made in 1970 by Franklin Fisher, director of thesis of Jesus Felipe (<http://dspace.mit.edu/bitstream/handle/1721.1/63262/aggregateproduct00fish.pdf?sequence=1> ). [↑](#footnote-ref-10)
11. On obtient ce résultat en remarquant que *wL=F ' L*(*K,L*)·*L=*(0,25·*A·K*0,75*L*–0,75)·*L=*0,25·*A·K*0,75*L*0,25 = 0,25*F*(*K,L*)*.* De même pour le capital. [↑](#footnote-ref-11)
12. The simulation of Felipe and McCombie tests in fact jointed / attached hypothesis, on the form of production functions micro and on that of the markets. [↑](#footnote-ref-12)
13. En remarquant que *x*^ *=x*′*/x =* (ln *x*)′, l’identité (4) peut s’écrire (ln *V*)′*≡ a*(ln *w*)′+*a*(ln *L*)′+(1 –*a*)(ln *r*)′ + (1 – *a*)  
    (ln *J*)′. En prenant les primitives des deux membres, il vient : ln *V ≡ a*ln *w* + *a*ln *L* + (1 –*a*)ln *r* +(1 – *a*)ln *J +*  
    *constante* et donc, en passant aux exponentielles et en réarrangeant les termes, *V ≡ Cwa r*1–*aLaJ*1–*a* avec *C =*  
    *e*  
    *constante.* Si, dans cette dernière égalité, on divise les deux membres par *V*, il vient 1 = *C*(*wL/V*)*a*(*rJ/V*)1-*a= Caa*(1 –  
    *a*)1–*a*, d’où *C = a*–*a*(1 – *a*) – (1–*a*). [↑](#footnote-ref-13)
14. The greater variability in time and space of *w* and *r* (term war1-a in the B of (4) ) explains why Douglas found better results with the inter-industrial data rather than with the chronological series. The evolution of the term war1-a also allows to explain the (slightly) increasing returns that Malinvaud believes to detect in the estimates of the (so-called) aggregate production function. [↑](#footnote-ref-14)
15. The formula (7) can be also put in the form PGF^ = a(V^ - L^ ) + (1 - a) (V^ - J^ ), the growth of "total factor productivity" is therefore given by the weighted mean of the growth in labor and capital productivity - but with the weighting coefficients which do not correspond to what was stipulated in the "micro" level. Not to mention the fact that we assume that there are functions of production at this level and that *J* is not *K*... [↑](#footnote-ref-15)
16. Solow does not go as far - it was pointed out his hesitation waltz on the existence of the aggregate production function. In his Nobel conference, however, he expressed reservations on the use that may be made of the TFP: " 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." (<http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/solow-lecture.html> ). [↑](#footnote-ref-16)
17. Some neoclassical theorists – beginning with Solow - reject this hypothesis. Others, such as Krugman, use it "to tell stories" which are supposed to "help to reflect", nothing more. On the other hand none of them, not even Stiglitz, questions the idea that the perfect competition represents the case of an ideal “decentralized” market. [↑](#footnote-ref-17)