"On the Cobb-Douglas and all that…": The Solow – Simon correspondence over the aggregate neoclassical production function[1]

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Abstract

The debate over the Cobb-Douglas production function has been raging ever since the mathematician Charles Cobb teamed up in 1928 with the economist Paul Douglas and developed this famous model of aggregate production and distribution. This essay presents a heretofore unpublished exchange in 1971 over the efficacy of the Cobb-Douglas by two future Nobel Laureates, Robert Solow and Herbert Simon. Solow emerges as the defender of the Cobb-Douglas while Simon the engaging critic. The correspondence demonstrates that the logical and empirical problems with the Cobb-Douglas were well-known by the most advanced minds of mainstream economics. This calls into question the rationale for its continued use as an empirical corroboration of marginal productivity theory.

Keywords: Cobb-Douglas Production Function, Aggregate Production Function, Robert Solow, Herbert Simon

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1. Introduction

The debate over the Cobb-Douglas production function and aggregate production functions generally has been raging ever since Cobb and Douglas first published their original article in 1928. The debate still continues today, as evidenced in the Symposium on the aggregate production function in the summer 2005 issue of *The Eastern Economic Journal*. What is particularly interesting regarding the history of the debate over the Cobb-Douglas function is not only did neoclassical economists disagree and debate with non-neoclassical Post-Keynesians over the efficacy of the Cobb-Douglas, but also that mainstream economists disagreed amongst themselves as well. This note brings to light an interesting correspondence between at that time (1971) two future Nobel Laureates, Herbert Simon and Robert Solow, over the relevance of the Cobb-Douglas function. The archival evidence was uncovered in the Robert Solow Papers at the Economics Papers Project of Duke University. The correspondence itself took place in April through June 1971, precisely at that time when neoclassical theory was on its heels as regards the logical consistency of their notion of capital that the Cambridge Controversies were bringing out. Simon of course came out very critical of the Cobb-Douglas function in a short note he co-authored with Ferdinand Levy in 1963 (Simon and Levy, 1963), **and although in the archives presented below (see letter dated June 30, 1971) he seems to have been sympathetic to marginal productivity**, by 1979 he will have solo-authored a devastating critique of neoclassical theoretic interpretations of the Cobb-Douglas function, arguing there that the "criterion of parsimony" alone forced one to reject the Cobb-Douglas as a "test" of marginal productivity theory (Simon 1979). His 1979 paper closely follows his correspondence with Solow some 8 years earlier.[2]

This short essay is organized as follows. In section 2 the nature of the debates on the Cobb-Douglas function are briefly presented in historical context. It is shown there that almost all critiques of that function, beginning with Horst Mendershausen in 1938, lament that what's really being measured is either some proxy for technical and/or evolutionary change; further he was the first to demonstrate the poor econometrics in Cobb and Douglas as reflected by the lack of a time trend**.** What is clear in these critiques is that what it is *not* a measure of is the marginal productivity theory of distribution. It is this common idea of what the Cobb-Douglas is *not* – namely it is not a test of marginal productivity theory - that ties the various critiques of that function together (there is less agreement about what the Cobb-Douglas is actually a measure of). In Section 3 the Solow-Simon correspondence is presented. The methodological issues raised in Section 2 are given sharp relief in this exchange. Also presented here is a summation of the exchange by Martin Bronfenbrenner dated July 1971. Bronfenbrenner had been given a copy of both sides of the correspondence by Herbert Simon and who writes to him on these issues. The latter was found in the Bronfenbrenner Papers, also from the Duke Archive; note that Bronfenbrenner's Papers also included copies of the letters reproduced in this note. The final section concludes.

2. The Cobb-Douglas Production Function

The Cobb-Douglas aggregate production function can be simply expressed as:

$$Y = AL^{\alpha} K^{\beta}$$

Where: A = shift/scale parameter

Y = homogenous output

L = homogenous aggregate labor input

K = homogenous aggregate capital input

Where in the original Cobb-Douglas $\alpha + \beta = 1$ by assumption.[3]

Since it made its debut in the late1920's (Cobb and Douglas 1928), the Cobb-Douglas function has long been the subject of debate and controversy.[4] One expressed purpose of the Cobb-Douglas function from its inception was to reinforce the neoclassical vision embodied in one-commodity parables, as Murray Brown in 1968 candidly admits:

> "Professor Douglas's procedure, in brief, was to estimate α and β from the production function and then compare the estimated values with the shares of labour and capital in income; if the shares agreed with the respective estimated elasticities, Professor Douglas concluded that factors were receiving their marginal products" (Brown, 1968, 36-37, note).

The Cobb-Douglas has long been known to *not* be a production function in a precise sense but rather either an income distribution function (identity) under the assumption of constant returns to scale ($\alpha + \beta = 1$) and/or some measure of technological/evolutionary change. One of the first sustained critiques of the original 1928 article and Douglas's (1934) *Theory of Wages* where the function is meticulously applied is made in 1938 by Horst Mendershausen (1938, 1939). Mendershausen argued that the Cobb-Douglas

function measured a "confluent relation" and at best can be interpreted as an (ex post) "expression for the trend in…technical development (Mendershausen 1938, 153) or an (ex post) "indicator in the volume of evolutionary change in the volume of output per unit labor" (Mendershausen 1939, 362; see also A. A. Walter's important survey of production and cost function, 1963, 24).[5] This critique of the specification of the functional form of the Cobb-Douglas in terms of what it actually measures reappears in all subsequent critiques. Phelps Brown (1957) advances the thesis that the Cobb-Douglas is an *ex post* accounting relation that "describe[s]…the historical rates of growth of labor, capital, and the product" and cautions against making any inferences for marginal productivity theory (Phelps Brown 1957, 551). Simon and Levy (1963) have a very concise demonstration that the share of wages in the income identity $Y = wL + rK$, or $\omega = \frac{wL}{Y}$, assumes the exact same functional form as the exponent of the labor input in the Cobb-Douglas (α). That the Cobb-Douglas is simply a transformation of the income identity is ***also*** shown in Anwar Shaikh's (1974; 1980; 1990; 2005)[6] HUMBUG production function. Addressing the purported empirical success of the function, Shaikh demonstrated that the Cobb-Douglas is simply an (anti-) logarithmic transformation of the income identity under the assumption that relative income shares are constant. Felipe and Holz (2001) call this *Shaikh's Identity*.[7] In the original article, Shaikh fit arbitrary data that spelled out the word "HUMBUG" to a Cobb-Douglas and found that it too was consistent with that function. This exercise was influenced by the work a few years earlier of Franklin Fisher (1969; 1971) who openly questioned the existence of aggregate production functions and through simulation experiments was able to establish that the constancy of labor's share in the data accounted for the nice fit, not the purported

inferences (corroborations?) made about marginal productivity theory. However, the micro production functions Fisher used were indeed neoclassical and constructed on the basis that labor was paid its marginal product.[8]

Herbert Simon in 1979 once again addressed these problems. Simon's basic point is that the goodness of fit of the Cobb-Douglas and CES functions to data amounts to a "statistical artifact" in that the data being fit correspond to the income identity and hence is readily explainable without resort to marginalist assumptions:

> "[G]rave questions of econometric method have been raised (and not answered) about the legitimacy of regarding the fitted functions as genuine production functions, in the meaning that [neo-] classical theory attaches to that term. Since the observed phenomena can as readily be explained on the weaker assumption that what is being observed is simply the accounting relation equating value of output to the sum of factor costs, the criterion of parsimony would lead us to prefer the latter explanation to the [neo-] classical one" (Simon, 1979, 472-3).

All of these critiques of the Cobb-Douglas refer to the inability of being able to interpret the statistical fit of that functional form as a "test" of marginal productivity theory in the sense a true production function would evidence. Yet despite all that has been written that seriously questions the underlying marginalist assumptions that are implicit in aggregate neoclassical production functions, economics experienced in the 1980's and 1990's a resurgence in aggregate production and growth theory (see Romer, 2001, Barro and Sala-i-Martin 2005; Jones 2002; Mankiw 1997). Somewhat embarrassing, Mankiw especially reveals his ignorance of these questions in the following quote:

> "I have 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…" (Mankiw 1997, 104; quoted in Felipe and Holz, 281, note 3).

Obviously Mankiw has little awareness – or at least does not demonstrate that he does have this knowledge - that the Cobb-Douglas function underlying the Solow model is simply an (anti-)logarithmic transformation of an income identity. Such ignorance did not exist among the first and second generations of neoclassical economists interested in using aggregate functions.[9] This is an interesting example of "how conclusions become firmer as they filter from secondary to tertiary sources" (Simon, 1979, p. 460). All of this serves as a point of departure for the recent criticisms of the Cobb-Douglas in large part spurred by Sylos Labini (1995) and picked up in the field of applied economics by Felipe, Holz, McCombie, and Adams and now includes original contributors Anwar Shaikh and Franklin Fisher. Sylos Labini (1995) openly questioned *why* the Cobb-Douglas function was getting so much life in recent growth models. He argued that *the very assumption of the exponents summing to unity is itself an "act of faith"*[10]

> "[O]n the basis of an act of faith in the marginalist theory of distribution, the constraint $\alpha + \beta = 1$ was introduced, sometimes implicitly, and the value of β was even introduced exogenously. The result is that neither the values of α and β nor the so-called 'residual factor' can be considered theoretically significant" (Sylos Labini, 1995, 497-498).

Recent critiques of the Cobb-Douglas have been expounded in the late 1980's through the early 2000's, especially in McCombie (1987, 1998a, 1998b, 2000-2001, 2001), McCombie and Dixon (1991) Felipe (2001), Felipe and McCombie (2001, 2005), Felipe

and Holz (2001), Felipe and Fisher (2003), Felipe and Adams (2005), Shaikh (2005), Pressman (2005), and Fisher (2005).[11] This recent literature openly targets the new generation of neoclassical macroeconomists to whom problems with the aggregate production function are unknown and asks the question:

> "[I]n the light of the negative conclusions derived from the Cambridge debates and from the aggregation literature…*why* [do neoclassical economists] continue using aggregate production functions [?] ...The younger generation of economists remains ignorant of these problems, with the consequence that bad habits and bad science breed bad economics and bad policy advice" (Felipe and Fisher 2003, 211).

Of course the answer to this question often given by the neoclassicals is that all theoretical models are abstractions from reality or simplifications and what matters is how they stand up to empirical testing a la Friedman's instrumentalism. Generally (but not always), estimates of production functions give good statistical fits with "plausible" estimates of the coefficients. But as repeatedly demonstrated in the critical literature, the reason for this is that they are merely tracking an underlying accounting identity! This certainly echoes Joan Robinson (1953) in the article that started it all: "sloppy habits of thought are handed down from one generation to the next" (Robinson 1953, 81)

3. The Solow-Simon Exchange

"5th April, 1971

Dear Herb,

…

On the Cobb-Douglas and all that, here are my thoughts. There seem to be two separable questions that the literature has not always distinguished. One is whether the Cobb-Douglas is a good approximation to the 'true' production function, supposing that there is one. The second is – again supposing that there is a 'true' production function to be approximated – whether the marginal productivity theory of distribution holds. Literally interpreted, you[r] note with Ferd Levy is a contribution to the first question. I have no quarrel with it. I agree that if the 'true' function were linear and one fit a Cobb-Douglas, things would turn out as you say; and agreement of the estimated elasticities with observed distributive shares is not a good test of the Cobb-Douglas against a linear or other form.

But the more interesting conclusion drawn from the statistical results is that observed shares behave as if the marginal productivity rules hold. This raises a deeper question. Suppose actual distribution violated marginal productivity rules. It would still be true that $P_t = w_t L_t + M_t K_t$ where w_t and r_t are simply real wage rates and the observed yield on capital; in fact the equation simply defines M_t. Now how does one interpret the statistical results? (I remember a similar point being made by Tibor Barna about cross-section data, but, as your note really says, the same is true of time series.) Now I think it's not so simple. Suppose w_t and r_t don't behave like random drawings from a stable

population with fixed mean. Suppose K and L change systematically and *w* and *r* appear to change in a way related to K and L. That would seem to contain information both about the relative appropriateness of the linear and Cobb-Douglas forms and about the validity of marginal productivity rules, or at least about the combination of those two things. If K rises faster than *r*, in just such a way to keep $\frac{wL}{rK}$ constant, that at least rules out the combination of a linear function and marginal productivity, but doesn't rule out the combination of Cobb-Douglas and marginal productivity. If *w* rises faster than *r* but still $\frac{wL}{rK}$ rises or falls, that might be consistent with marginal productivity and CES, with elasticity of substitution less than or greater than one. That kind of evidence is hardly definitive, because something quite different could be at work. But I think that's the nature of the problem.

Does this make sense to you? Of course none of this touches the problem of using an aggregative production function to interpret data where no logically rigorous function exists.

Best wishes,

Yours,

Robert M. Solow"

*

"May 4, 1971

Dear Bob:

I am grateful to you again for your very instructive letter. From that letter, and from additional reading I have been doing, it appears that I have been mostly reinventing the wheel. However, I often find that a good way of educating myself – if at the expense of my friends. I hope that you will bear with me a little longer, because I really want to nail down what I think about Cobb-Douglas, CES, and all that.

The letter stimulated me to try once more – this time laxing the strong assumption about the fixed activity ray that bothered you and me in my last model; and trying to be more precise about my definition of capital. It appears that if I am careful on the latter point, I can dispense with the restrictive assumptions about technology, and also that the interest rate does the equilibrating! Let's see if you believe it.

Let me restate my goal: to construct a believable model of an economy that is based on the assumption of a markup or average cost pricing rather than maximization, but that explains the relative stability of labor's share in the face of technological change that may or may not be neutral.

We need to make clear the distinction between (a) amount of capital services, and (b) capital renewal requirements. Perhaps this distinction is already made in the literature, but I must confess that I was unclear about it until a few days ago. Suppose we have

capital in just two forms: flimsy tractors and sturdy tractors. I can do just as much work with a flimsy tractor as with a sturdy tractor – the former merely wears out sooner. Hence both tractors represent the same amount of capital services, but the annual renewal requirement is larger for the flimsy tractor than for the sturdy one. Now, it is the amount of capital services, proportional to the stock of capital, that enters into the production function; but it is the renewal requirements that enter into the accounting relation that determines prices. But the renewal requirement is the product of the stock of capital by a 'depreciation' coefficient. Assuming both average-cost pricing and a completely general production function, I would now write:

(1) $qP = wL + npK$, and

(2) $P = LF\left(K, L\right)$

Where P, L, and K are (physical) product, quantity of labor, and stock f capital, respectively; q, w, and p are their respective prices, n is the annual charge for use of capital – mostly depreciation – and F is a function. (I'll later assume diminishing returns for K with L fixed.)

If we let q = 1 be the numeraire, and assume $\frac{p}{q} = b$, constant, we can rewrite (1) as:

(1') $P = wL + mK, \quad m = nb$

From (1'),

(3) $w = \frac{P}{L} - m\left(\frac{K}{L}\right)$

Next suppose a desire for wealth proportional to P. For equilibrium (I'll put the dynamics of the savings rate aside for the moment), we then have:

(4) $K = aP, \quad a$ constant

Substituting for K in the second term of (3), we get:

(5) $w = \frac{P}{L} - ma\left(\frac{P}{L}\right) = \left(1 - ma\right)\left(\frac{P}{L}\right)$, and

(6) $\frac{wL}{P} = \left(1 - ma\right)$

Now we are out of the Harrod-Domar box, and home free, without any over-determination of the savings rate, and without any dependence on an interest rate for adjustment. The desire-for-wealth parameter, a, is exogenous, as is the sturdiness parameter, n. As far as I can see, I have not made any marginalist assumptions. Labor's share depends on the parameters a and n, but not on the marginal productivity of labor (as can be seen by observing that (2) can be Cobb-Douglas or CES with any exponent or coefficient whatsoever). Wages do depend, of course, on $\frac{P}{L}$, since you can't give away what you don't have, but this ratio, again, has nothing in particular to do with marginal productivities. Technical change, neutral or otherwise, can be introduced in the model in all the usual ways, plus the unusual way of changing n. Change in the technical coefficients through change in demand can also, of course, be accommodated.

It is easy to dynamicize the whole system by introducing a savings rate, with $\frac{dL}{dt} = bL$, say, and $\frac{dK}{dt} = g\left(aP - K\right)$, b and g constant. Then we get a steady state, with $\frac{dK}{dt} = dK$ when $\frac{aP}{K} = \left(1 + \frac{b}{g}\right)$, provided that the partial of F with respect to K is monotone decreasing.

Now if this system doesn't have any obvious flaws, then someone else must have invented it previously. Can you give me a reference? (See Postscript below.) Does it have any interest, and is it worth developing? Since I have plenty of other things to keep me out of mischief, do not feel you have to humor me on this one.

Among the papers this foray has taken me back to is the original ACMS (or is it SMAC?) one. I have lots of difficulties swallowing everything in that – e.g., the remarkably low Japanese efficiencies you estimate – and I am probably going to try reworking parts of it, along the lines suggested here. Do you suppose the raw data are still lying about somewhere?

With best regards,

Cordially yours,

Herbert A. Simon

P.S. I have now looked at the Modigliani piece to which you referred me – and for which my thanks – and to the Scitovsky paper on which Franco Comments.[12] These are very close to my model (which is not surprising, since I am sure that my equations came originally from Franco). However, there is an important difference, in that I make none of the usual marginalist assumptions, and do not have to assume neutral technological change to boot. Perhaps my pricing equation is the same as Phelps-Brown's, but it really is a little more sophisticated

than a simple mark-up of direct costs. Do the distinctions sound like genuine differences to you?

H.A.S."

*

"25th May, 1971

Dear Herb:

I'm still not happy with what you're doing, but maybe that's just because I don't want to do what you want to do. As it seems to me, wherever there is a possible economic choice, you assume some ratio to be an exogenous constant. I understand that you want to avoid assuming that each such choice is resolved by optimization, and I have some sympathy with that. But I'd rather you find something intermediate between optimization and constant ratios. You can almost certainly account for the 'stylized facts' with your model, because those ratios don't change fast. What I have to hope is that there are some slow changes that my kind of model can explain whereas yours would have to rely on an exogenous parameter change. It might be interesting to speculate about intermediate models.

Here are some specific comments on this version. You want to be careful about the relation between flow of capital services and size of the stock. In your tractor example, the flow of services depends only on the number of tractors. But most measures of the stock (market value, replacement cost) will depend also on the composition. So will $\frac{p}{q}$.

Moreover, in my way of thinking, the durability parameter n, which depends on the

composition of the stock of tractors, is open to economic choice; but you simply make it exogenous.

Second (4) should really be $\frac{pK}{qP} =$ constant . That leads to (4) as your assumptions, namely that $\frac{p}{q}$ is exogenous. But if the price ratio changes endogenously, then so does what you call a.

Third, your n should contain a healthy slug of profit in addition to depreciation (for the corporate sector the two are the same order of magnitude). So you need something to determine the rate of profit too, which you can take to be a pure mark-up, but that's another economic choice turned into an exogenous constant.

On the literature, I don't know that I believe everything that ACMS says. I doubt the data worksheets have been preserved. Arrow or Chenery might know. It seems to me that the nearest thing to your approach is Kaldor's (there are lots of versions but REVIEW OF ECONOMIC STUDIES, 1956, "Alternative Theories of Distribution" will give you the flavor). The saving details are different, and, come to think of it, so is he rest, because Kaldor gets the ratio of capital to output from the production side (whereas you get it from the saving side) and has the saving function doing what you do by taking the gross profit rate constant. The spirit is similar, though. If I simplify your approach by saying that you fix gross profit rate and the capital-output ratio exogenously (which, after all, does button up the share of gross profits in output!) then maybe you're closest to Joan Robinson. She never says anything precise, but I think there are some remarks on her ESSAYS ON ECONOMIC GROWTH – or something like that.

Most people aren't that interested in my prejudices! Best regards.

Yours,

Robert M. Solow"

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"June 30, 1971

Dear Bob:

Your second letter was, again, very instructive, and pinpoints our differences very clearly. As you say, it's all a matter of what you want to take as exogenous and what you want to explain. In your model, economic processes determine wages, interest rates, and the longevity of capital. Fine. But you get long-term stability of labor's share only by assuming neutral technological change.

The latter assumption sticks in my craw – seems to me entirely gratuitous. Therefore, I have been willing to take the markup of direct costs as exogenous, a la Phelps-Brown and others, and then get the stability of labor's share directly – but somewhat inelegantly. De gustibus[14]…

Of course, what we both would like is your derivation (I do believe that in the long run there is some rough relation between marginals and relative prices of labor and capital, at least) without the arbitrary requirement of neutral technological change. I have no idea more than you where that magic is to come from, but I'll keep trying, as I hope you will. One must never be seduced by the convenience of mathematical functions like Cobb-Douglas or CES into believing the world really is that way.[14]

Of course, it is also possible that the phenomenon we are trying to explain – the stability of shares – is illusory. Nevertheless, when a number can range, theoretically, from zero to one, but persists within an interval of about one tenth, there seems to be something that needs explaining.

Well, I'll write you when I have a blinding flash, and I hope you will do the same.

Your book on growth is excellent. But I am sure you have been told that before.

Again, many thanks,

Cordially,

Herbert A. Simon"

*

"Herbert Simon

M. Bronfenbrenner

July 27, 1971

Production Functions et. al.

Thanks for showing me your correspondence with Bob Solow on this subject. I find little to add, and a good deal of that is blowing my own horn.

1. The problem of distinguishing between a 'production' and an 'accounting' function is at least as old as Paul Samuelson's days at Chicago, which overlapped with ours. Paul fit

$$P = P_0 + aL + bC$$

to Douglas' data, and found it fit about as well as the Cobb-Douglas function.[15] In the literature, I think you'll find it spelled out in a long article by Marschak and Andrews (Econometrica, 1945).

2. The most 'behavioral' theory of distribution simply, that firms price so as to make 'payrolls' *x* percept of value added, or *y* per cent of sales. I touched on the first of these notions, in a piece called "Imperfect Competition on a Long Run Basis (Chicago Journal of Business, 1950). Sidney Weintraub has done more than anyone else with the second, especially in the 'wage-cost mark-up equation' of his General Theory of the Price Level, Output, Income Distribution, and Economic Growth (1959).
3. I think Chapter 16 of my Distribution Theory manuscript is a useful introduction to a lot of the literature in both English and German, though Krelle and Rothschild are fuller on the Continental materials.
4. As you've doubtless discovered by this time, most macroeconomics is 'behavioral' in the sense of independence form 'maximizing' assumptions. That's also what a lot of people (Friedman, Machlup, Phelps) object about it, and why they worry about the 'micro-foundations' problem, which strikes me as relatively unimportant.

I assume I can keep your discussion with Solow"

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4. Conclusion

An underlying theme of the above correspondence addresses the "pin-pointed differences" in exactly *how* to interpret the "good empirical fit" of the Cobb-Douglas aggregate production function. Whereas Solow wants to import an "economic choice" into various aspects of the model, and thus laments against "exogenous constants" Simon erects in its place – with "economic choice" meaning of course some variant of neoclassical optimization and the primacy of the marginal method – Simon wishes to avoid what he sees as unnecessary inferences (hence implicit/explicit corroborations) of standard assumptions of marginal productivity theories, especially when the results of the model can be explained without need to resort to marginal theory, **although admittedly his statement from June 30 does make the caveat that at least in the long run Simon was sympathetic to marginal productivity**. This caveat notwithstanding, clearly from the overall exchange Simon was not convinced by Solow's comments and the arguments Simon raised in the above correspondence were more fully elaborated in Simon (1979). Simon was convinced that all that statistical estimates of the Cobb Douglas and CES "production functions" were picking up was a transformation of the accounting identity and did not reflect the underlying technology of the economy.

This of course is Simon's "criterion of parsimony", a criterion that is *not* synonymous with "simplicity". To this latter distinction between parsimony and simplicity Simon would return to in an entry published the year of his death (2001) in the edited book *Simplicity, Inference, and Modelling* (Zellner, Keuzenkamp, and McAleer, eds. 2001). In this entry Simon returns to the question of parsimony – as distinct from simplicity – as a necessary methodological condition for *all* scientific activity:

> "The basic desideratum in science…is parsimony…We aim to discover pattern in observed facts that can be used to describe and explain these facts parsimoniously.
>
> Parsimony brings simplicity in its wake; but simplicity in theory without parsimony in the relation between the theory and the data is bought only at the price of weakening the goodness of approximation of our descriptions, narrowing the range of phenomena over which they extend, and impoverishing our understanding of the phenomena" (Simon, 2001, 69).

The Cobb-Douglas indeed is a simple approximation of aggregate production and distribution, however the inadequate addressing of the criterion of parsimony has resulted, new recent literature would certainly attest to, with an increasing "impoverishment of the understanding" of macroeconomic production and distribution both in theory and in practice. This all renders clear to in increasing number of especially applied economists that the resurgence of the Cobb-Douglas and aggregate production function generally is the one of the most blatant "don't ask – don't tell" theoretical questions in modern economic theory.

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[2] In the beginning footnote of Simon (1979) we read:

> "This paper has been in gestation for a little more than a decade. During that time, in discussions and correspondence n these topics, I have incurred numerous intellectual debts to Martin Bronfenbrenner, Richard Cyert, Yuji Ijiri, Dale Jorgenson, Robert Lucas, Timothy McGuire, and Robert Solow. None of them, or course, are responsible for the positions I take here" (Simon, 1979, 459, note 1).

Obviously reproduced here is a portion of the correspondence with Solow and Bronfenbrenner.

[3] More sophisticated models such as those of the constant elasticity of substitution hold the sum $\alpha + \beta$ as constant but not necessarily unity. The original article that develops the CES model was the joint effort of Arrow, Chernery, Minhas, and Solow, and (ACMS) published in 1961. The basic CES function takes the following well-known form:

$$Y = \gamma\left[\delta K^{-\rho} + (1-\delta) L^{-\rho}\right]^{-\frac{1}{\rho}}$$

$$\rho = \frac{1}{\sigma} - 1$$

$$\sigma = \frac{1}{1+\rho}$$

Where Y, K, L are value added per man year, capital, and man-years or labor time and γ, δ, ρ are the efficiency, distribution, and substitution parameters respectively and σ is the elasticity of substitution. The CES function is more general than the Cobb-Douglas since it reduces to a Cobb-Douglas as the substitution parameter (ρ) approaches zero and the elasticity of substitution (σ) approaches one. The CES function is often used at the "frontiers" of modern growth theory. See Barro and Sala-i-Martin 2004, 68-71 for an account of modern growth theory in terms of the CES function. In the earliest studies Douglas and his colleagues did constrain the estimates to unity but in the light of subsequent criticisms he estimated the Cobb Douglas in unconstrained form in the 1930s. He found it made no difference and that the sum of the coefficients was not statistically significantly different from unity.

[4] In his important tome surveying and developing extant income distribution theory, Bronfenbrenner (1971) notes that:

> "Controversy about the meaning and usefulness of the Cobb-Douglas function, and for that matter, of aggregate production functions generally, was inaugurated by the discussion of the original Cobb-Douglas paper. It has never died out" (Bronfenbrenner, 1971, 389).

[5] Mendershausen (1939) is a "Correction" published a few months after the original article (1938). The correction is very interesting because he distances himself from the earlier statement that the Cobb-Douglas "express[es]…the trend in the technical development" (1938, 153) and now feels that the function "may be taken as an indicator of an evolutionary change in the volume of output per unit labor" (1939, 362). Mendershasusen seems to have come to the idea that the economic system is more socio-biological than technical and experiences evolution and change in a more organic manner than (exogenous) changes in technology.

[6] Throughout Shaikh's articles on the HUMBUG function has been the subtext of a debate with Robert Solow. The original 1974 *RES* article, published as a "Note", was followed by a curt dismissal of Shaikh's argument by Solow. Marjorie Turner (1989, 195-6) has an account of this from which the following remark by Joan Robinson to Alfred Eichner is taken:

> "I suppose you know that Shaikh's Humbug article was published in Review of Economic and Statistics as a note not an article, and that Solow's reply was not shown to Shaikh…nor was he given the usual right of replying. This is a clear case of bias in the journals and I think you should make the maximum fuss about it. Solow's reply is evasive, silly and abusive as usual" (Robinson to Eichner, 21 June 1974; quoted in Turner 1989, 196).

Shaikh (1980) contains both an extension of the original model as well as a reply to Solow's 1974 critique. Solow (1987) is a response to Shaikh (1980), to which Shaikh (2005) responds**,** although Shaikh does not directly address many of the issues in Solow (1987). The latter is much more thoroughly done in McCombie (2001).

[7] Leontief's theorem of separable functions specifies that "valid aggregation" is possible only under the condition that labor and machines are "additively separable", which is what the logarithmic transformation of the Cobb-Douglas accomplishes (Walters 1963; Fisher 1969; Blaug 1974: 13). What Felipe and Holz (2001: 263-266) show is that this is tantamount to assuming Shaikh's Identity.

[8] See the interchange between Joan Robinson (who was confused on this point) and Franklin Fisher in *Econometrica,* 1972.

[9] Ferguson (1962) in an interesting "pedagogical note" cites Samuelson's (1947, 84) *Foundations of Economic Analysis* on this score:

> "As Samuelson has indicated, it is probably best to avoid [the] expression ['factors of production'], because 'factors' tend to get identified with social classes" (Ferguson 1962, 101).

And in another paper published the following year which Bell (1965) notes was one of the first applications of the then-recent ACMS CES function, Ferguson (1963) is ambivalent as to whether the function itself was a production or a distribution function:

> "[S]uch functions get their names *only by analogy* with microeconomics; the underlying concepts do not transfer. It is a matter of personal choice whether [the CES function] is called a 'production function' or an 'aggregate distribution function'" (Ferguson, 1963, 313; emphasis added).

These sentiments are among other places explicit in Solow's review of Hicks's (1965) *Capital and Growth*:

> "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 something better comes along" (Solow 1966, 1259-60);

in the Preface of Hahn's (1972) re-issue of his dissertation:

> "Except under absurdly unrealistic assumptions such aggregate [neoclassical] theory cannot be shown to follow from the proper theory and in general is therefore open to severe logical objections" (Hahn, 1972, 8);

and in Blaug's (1974) polemic against the "Cambridge Revolution":

> "[T]he concept of an aggregate production function is fraught with enormous difficulties that have been neglected by all but a handful of orthodox economists, and it is not too much to say that empirical work in production functions has come dangerously close on occasions to 'measurement without theory'" (Blaug 1974, 6).

[10] Some methodological questions of "faith" in economic theory have been explored in Cohen and Harcourt (2005) and Carter (2011, forthcoming). McCombie in private correspondence on an earlier version of the present paper cautions that Sylos Labini's paper did not really contribute to the debate as his major point was that estimates of the Cobb-Douglas give poor statistical fits and he nowhere mentions the problem of the identity.

[11] The references for 2005 refer to the above-mentioned Symposium on the Aggregate Production Function that appeared in the *Eastern Economic Journal*.

[12] Tibor Scitovsky (1964) "A survey of some theories of income distribution", and Franco Modigliani (1964) "Comment", *Behavior of Income Shares: Selected Theoretical and Empirical Issues*, NBER.

[13] Latin for "there is no disputing about tastes".

[14] In preparatory notes entitled "Metaphysics" written in November 1927 for his Lectures on the Advanced Theory of Value given at Cambridge in 1928-31, Sraffa defines "metaphysics" as "the emotions that are associated with our terminology and names (schema mentali)"; this seems to us to be the cognitive structure of analysis. He discusses "metaphysics" in general and sketches out the particular "metaphysics" of Marx, Marshall, and J.B. Clark. Of "metaphysics" in general, Sraffa warns us:

> "Our metaphysics is in fact embodied in our technique; the danger lies in this, that when we have succeeded in thoroughly mastering a technique, we are very liable to be mastered by her" (D3/12/4/16).

[15] See Samuelson, P. A. (1979). In many ways Samuelson rediscovered the wheel – note that he does not refer to any of the literature on the accounting identity**.**