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# *COMMENTS ON THE COBB-DOUGLAS PRODUCTION FUNCTION*

PAUL H. DOUGLAS

I AM delighted to offer, at your invitation, some comments on the origin and development of the so-called Cobb-Douglas production function to the second and third generation of production theorists here assembled. The inspiration for this function began nearly forty years ago, in the spring of 1927, when I was temporarily lecturing at Amherst College and working simultaneously on two books which were published some years later, namely, *Real Wages in the United States* and *The Theory of Wages*. I had charted on logarithmic paper (1) the numbers employed in manufacturing in the United States from 1899 to 1922, and (2) the best estimates which I could compute of fixed capital in manufacturing deflated to dollars of constant purchasing power. We, of course, also had (3) the Federal Reserve Board's index of physical production in manufacturing during this period.

Previously I had been working on the theory of wages primarily in terms of relative elasticities of supply. But I was not at all satisfied with this facile approach to the subject, and I realized that we had to obtain some approximation of the slopes or elasticities of the marginal productivity curves of labor and capital, if we were to develop a satisfactory explanation both of wage rates and of the share which labor and capital had in the distribution of the total product.

As I have said, I had charted these three curves logarithmically on a common base and found that, in general, the production curve for manufacturing lay from one-third to one-quarter of the distance between the labor curve, which had the slowest rate of growth, and the capital curve, which had the greatest rate of growth. There were, of course, differences in the rates of growth over the years, and the depression years were characterized by decreases both in production and in labor employed. But since the capital index was one of capital available and not capital actually used, there was a marked deficiency in it.

I am not a mathematician, so I then went to my friend, Charles W. Cobb, who was on the mathematics faculty of Amherst, and asked him if he could devise a mathematical function which could be used to measure the comparative effect of each of the two factors upon the total product. Almost immediately he remembered the Euler theorem of a simple homogeneous function of the first degree and suggested it as a first approximation

$$P = bL^kC^{1-k}$$

Here the exponent for labor would be independently determined, while the assumption would be made that to increase both labor and capital by an equal percentage would increase the product by that percentage. Therefore the exponent for capital would be dependently derived as $1 - k$. We remembered that this simple function had been suggested and indeed laid down over thirty years before by that extraordinary genius, Philip H. Wicksteed, in his *Coordination of the Laws of Production and Distribution* and, later, in his *Common Sense of Political Economy*. To my mind, Wicksteed was the greatest English economist of his day and, incidentally, a most versatile man. He was not only a Unitarian minister of distinction but also the translator of Dante, the author of a series of monographs on the Italian cities of the Renaissance, and he concluded his life with a translation of the Icelandic sagas into English poetry. The same theorem had also been treated very elegantly by Leon Walras.

Cobb and I agreed on this formula and started to find the value of $k$ by the method of least squares. This we discovered to be approximately .75 and, therefore, the exponent of capital $(1 - k)$ was taken as .25. We also found that the computed annual values were very close to the actual values, and that the standard errors of estimate were less than one would expect from the sampling error based on normal distribution. Such deviations as did occur could have been largely explained by business cycle changes and by errors in the estimates of the quantities of capital, because our measurement of capital was, as I have said, one of availability rather than of utilization. This overestimated the amount of capital used during periods of depression and slightly underestimated the relative stock of capital used during prosperity. Consequently, this tended to give a theoretical product in excess of actual product in the depression years and to understate product slightly in the prosperity years.

Assuming that these results were approximately correct, we could then find the slope of the marginal productivity curves. But before I go into that may I say that this follows from the simple logic of the formula: If we assume that an increase of 1 per cent in both labor and capital seems to cause an increase of 1 per cent in total product, and if an increase of 1 per cent in labor alone carries with it an increase of three-quarters of 1 per cent in total product and an increase of 1 per cent in capital brings about an increase of one-fourth of 1 per cent in total product, then from this you can derive the slopes of the marginal productivity curves, or what may be called the elasticity curve of labor of $-4.0$, and of capital of about $1\frac{1}{3}$. From these relationships, under perfect competition, we would expect labor and capital each to receive as its share of the value of product the proportion indicated by its exponent.

We had available the National Bureau of Economic Research studies on income. They seemed to show that for the period 1909–19, labor's share of the value added in manufacturing was 74.1 per cent, or an almost precise coincidence with the value of 75 per cent which we would get from the productivity theory and from the production function under perfectly competitive labor and capital markets. On the basis of all this, Cobb and I prepared a paper for the December 1927 meeting of the American Economic Association. This was forty years after John Bates Clark and Stuart Wood had read their seminal papers on marginal productivity at one of the earliest meetings of the American Economic Association.

Our paper met with a very hostile reception, and the next few years were full of the most caustic criticism. I think no one said a good word about what we had tried to do, and the attacks came from the most diverse quarters. The neoclassicists were very irate at our attempt to quantify the theory which they had contemplated in the abstract as unquantified. The institutionalists were highly indignant that anyone should attempt to find order in what they believed to be an essentially disorderly universe. The econometricians—and there were a few then—were very grieved that one should try to attach specific values to the elaborate models which they were even then constructing. The statisticians resented very much the attempt to say that this rough series had any degree of inner meaning or relationship. So, the views and personal comments for many years were very hostile. Ragnar Frisch and Horst

Mendershausen, as I remember, said that our study should be thrown in the waste basket and all future research on it discontinued. My friends thought that the better part of valor was to ignore the whole subject and never mention it, but others were not so kind.

Very frankly, I was tempted to give up and try to establish scientific respectability in other fields. But a perverse streak led me to decide not to do this, and so I incorporated not only the earlier paper in my book *The Theory of Wages,* which I published in 1934, but included also a later study based on Massachusetts manufacturing, and one on New South Wales made by Aaron Director. They seemed to bear out the general, although not the precise, results of the earlier study on the United States.

After *The Theory of Wages* appeared and my own energies began to be absorbed in other fields, such scientific time as I had available was spent almost entirely on the theory of production. I worked very closely not only with time series but also with cross-section studies of the American censuses from 1889 on to 1919, in which I would take each industry in a census year as an observation and measuring product, not in terms of physical product but value added by manufacturing. I went on to study the best statistics in this field in the world; namely, those of the British Commonwealth countries. Australia had a great statistician then in G. H. Knibbs. Starting in one of the Australian states and later continuing in the Commonwealth, he built up annual censuses of production, employment, and capital. Those series, I believe, continue to the present day and are literally a treasure trove of available material. With assistants, I published studies in the next eight years on a variety of cross-section studies and time series for New South Wales, the Commonwealth of Australia, and for New Zealand. In the process of time the values of $k$ and $(1 - k)$ seemed to settle at about two-thirds and one-third respectively.

In the meantime, David Durand of Cornell published a very able paper in which he pointed out that the original Cobb-Douglas function had assumed what should have been the objective of research to try to prove; namely, that the sum of the exponents of labor and capital is indeed equal to unity—i.e., that constant returns to scale are actually the case. He therefore suggested that the formula be rewritten as

$$P = bL^kC^j$$

so that the exponents would be independently derived. I am deeply indebted to Mr. Durand for this suggestion.

Beginning in 1938 we therefore began to revise our statistical studies and to recompute them on the basis of the Durand modification. We published papers from time to time on the subject, but other engagements occupied me for some years, particularly during the years from 1942 to 1946. I was only able to return to the subject in the latter year when with Miss Grace Gunn we worked for a year, full time, on this matter. Very roughly, what we learned was that in practice the sum of the exponents $k$ and $j$ was found to be very close to one. There seemed to be some statistical evidence to indicate that production was explained by these two variables, at least so far as it was described by the marginal production theory. The values of $k$ tended to be about .65 while those of $j$ tended to be approximately .35 though, of course, some variation from study to study was revealed.

By this time we had accumulated about 2,100 observations for the United States and about 1,400 for countries other than the United States. We found that the deviations of the actual observations from the theoretical values for the United States were relatively minor. They were much less than one would expect from the normal distribution measures of statistical error, and this was true also of the British Commonwealths. We also found that if we analyzed the deviations of more than two standard errors of estimate, there seemed to be good theoretical reasons for them; namely, that for industries characterized by a degree of monopoly the actual value of product exceeded the theoretical values. This, we thought, was an actual corroboration of the theory. For what one could call industries characterized by excessive competition, where the labor was drawn from the most culturally deprived sections of population (which is notably true in the processing of fibers), the actual product was less than we would expect from the theoretical model. We therefore considered the deviations as basically corroborations rather than the reverse.

In the meantime, the National Bureau of Economic Research had been revising its own estimates of the distribution of the national product, and the share of $W/P$ was lower than in their earlier study. Indeed, it was coming down to somewhere around 65 per cent, so it corresponded to our studies. Roughly the same thing was borne out, although with differences in the values, in the Commonwealth countries, and the

Commonwealth figures are much better, of course, than ours. I read a summary of all these investigations in my presidential address to the American Economic Association in December 1947, which was published in the *American Economic Review* for March 1948, and Kelley and Milman later republished it in a revised edition of *The Theory of Wages.*

I have been compelled to drop this work since then. I had in mind a book on the theory of production and was working with Miss Gunn upon it. Just as I started to go down and read the presidential address to the AEA, the telephone rang in the hotel where my wife and I were staying. I was told that I had been nominated by my party as the candidate for United States senator from Illinois. As I went down to the assembly room, I said to my wife, "Emily, this may be the end of the line for my scientific studies," and I quoted to her the line from Othello, "Oh, farewell forever now the tranquil life."

It was not until years later that I discovered that my partial rehabilitation had been started. In Russia they only rehabilitate a man after he has been shot, but ten years later I found that my rehabilitation had commenced. In Oxford, of all places, *The Theory of Wages* was required reading for an honor's degree, although I believe it is still subject to dubiety. I understand it has also been a text at Cambridge. I think all this gave a certain patina of respectability to the idea. As a result, Commonwealth and American students at Oxford and Cambridge read this book as prescribed and discussed by the dons. Others now felt that it was perhaps respectable for them to take up the subject.

I have not been able to keep in touch with all the literature since then, but I make periodic soundings into it as I am greatly interested in what is going on, and I came here to hear something about it.

I'd like to take just a minute or two, if I may, for some observations. I have never maintained that labor and capital were the sole independent variables which influenced production. Certain errors in the capital formula should be modified to get some measurement of capital actually used rather than capital available. I am quite prepared to admit that the introduction of an explicit measure of technological change, which we assumed and perhaps included under $b$, is an improvement. I am not so certain that it has been accurately identified or quantified. Then, my friend and former colleague, Theodore W. Schultz, started to work on education as an independent variable and explained some of the move-

ment on this ground. But I never felt that the production function should be confined to the simple two variables, or at most three variables, which were used in the early studies. I have subscribed to many of the improvements developed in these articles which I can understand. I am also greatly interested in the development of the newer theoretical models. I have nothing to say against the development of these new models, but I would like to tell you a story about two friends of mine.

One friend did not know how to swim and wanted to learn how. He bought books on all the correct strokes, and every morning he would get on a large bed and try these strokes out, and he developed marvelous swimming form, but I could never get him to enter the water. So far as I know, he never actually swam. Another friend wanted to take up tennis, and he developed fine serves and lobs, but I could never get him to a play a game. I have felt the same thing about some of the inquiries into politics by the political scientists. I don't want to discount for one minute the tremendous importance of theoretical concepts and assumptions which one must make, but I do say there is a time in the development of science when one should learn to swim by getting into the water, and to play tennis by playing the game.

It is very difficult to convince some to do this. I therefore hope that building on the Australian and Canadian—possibly the South African —figures, we can erect a structure of studies so that we can progress in this work and actually develop studies utilizing not merely time series but what I call cross-section series with each industry as an observation. You can get many tens of thousands of observations now dating back sixty years, and with modern computers it is relatively easy to do this, whereas in my day we cranked out our limited studies by hand. I hope some—I don't say all—will make experiments in this field.

Let me say also, and I think Dr. Knowles will have some comments on this, that while in the United States we have on the whole the best statistics in the world, we've been very backward in developing capital statistics, and this is a very important point. We did collect capital statistics once upon a time, but in 1921 the Joint Committee of the American Economic Association and the American Statistical Association recommended, unanimously, that all future collection of capital statistics be discontinued by the United States Census. This was a proof of the remarkable foresight of the pundits of the two professions. We have suffered for their mistake ever since. Drs. Kendrick and Goldsmith have

done very valuable work in this field, and I hope very much that we can begin to collect these capital figures again. The Joint Economic Committee, of which I am alternately chairman and vice chairman, recommended this step under the very able stimulus of Senator Proxmire, who is himself an economist. I hope very much that this can be done. Then we may have more material for these empirical studies of the production function.

I personally have faith that there is a fundamental unity in economic as in physical life, and that the world does not move haphazardly, and that we can begin to find the inner laws and tendencies of production and distribution. There is law and relative regularity everywhere else —why not in production and distribution?