The Introduction of the Cobb Douglas Regression and its Adoption by Agricultural Economists

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Introduction

The first Cobb-Douglas regression was estimated in 1927, using aggregate time series data from the US manufacturing sector on labor, capital, and physical output, with the goal of understanding the relationship between the level of output and the quantities of inputs employed in production. This was the beginning of a twenty year research program in which Paul Douglas, working with various collaborators, estimated the regression using a variety of time series and cross section data sets. In the first part of this paper, I describe Douglas's research program, highlighting several features of its evolution. One is Douglas's move to emphasize the marginal productivity theory of distribution as a framework for interpreting his statistical results, so that the regression, which he originally presented as a way of quantifying the action of the law of diminishing returns, came also to be offered as a means of testing the validity of the marginal productivity theory and determining the extent to which competition prevailed in markets. A second is the way in which the interaction between Douglas, his coauthors, and his critics contributed to the emergence of a conception of the research program somewhat different from Douglas's own, as the empirical procedure of regressing a measure of output on measures of inputs came to discussed and evaluated in terms of what it could reveal about the parameters of the firm-specific production functions of a Walrasian version of neoclassical theory. A third is the change over time in the way Douglas understood and presented the statistical procedure he was employing. While he originally treated regression as essentially a mechanical curve fitting technique, vigorous criticism of his methods pushed him to articulate a statistical framework that justified his use of regression as a method for measuring relationships between inputs and output and provided a firmer basis for drawing inferences from his results. Finally, in the course of working out the statistical and theoretical implications of Douglas's empirical production function, Douglas, his collaborators, and his critics found themselves dealing with many of the same econometric issues that were being confronted in the literature on the estimation of supply and demand functions, as chronicled by Morgan (1990), including the identification problem, the challenge of estimating static models with time series data, and the question of how to introduce stochastic elements into econometric models.

Although Douglas's research was widely discussed in the period prior to WWII, few economists outside of Douglas's group actually estimated Cobb-Douglas regressions. This changed after the war. The second part of the paper looks at the work of a group of agricultural economists who successfully established the Cobb-Douglas regression as a research tool in their field. These economists saw the regression as a means of addressing a set of long-standing questions specific to agricultural economics. As a result, their defense and development of the method and the criticisms they attracted from their colleagues, while drawing on the prewar literature surrounding the Cobb-Douglas regression, had noticeably different emphases. In agricultural economics, the method was regarded mainly as means to estimate production relationships, so that questions about its efficacy as a test of the marginal productivity theory or the extent of competition became irrelevant. The agricultural economists were the first to estimate the Cobb-Douglas regression using data generated by individual firms, as opposed to the more highly aggregated data used by Douglas and his coauthors, and, by using the procedure along with the statistical methods developed by Ronald Fisher, they embedded it in a comprehensive, probability-based statistical framework. All of this contributed to the process through which the Cobb-Douglas regression came to be seen as an empirical tool potentially suited to a broad list of applications. Finally, the challenges faced by the agricultural econometricians in adapting the Cobb-Douglas regression to their particular purposes helped stimulate further developments in econometric theory and practice, in particular in the area of panel data techniques.

The Initial Cobb-Douglas Regressions[1]

Paul H. Douglas graduated from Bowdoin College in 1913 and received his Ph.D. in economics from Columbia University in 1920. He took his first college teaching post in 1915, and in 1920 accepted a position at the University of Chicago, where he would remain on the faculty until 1948.

Douglas was a prolific researcher, and began in the late teens to produce a stream of articles and books, usually on topics related to labor legislation and working class living standards, and often reflective of his Progressive political views. In 1921 he entered an ongoing debate on the trend in real wages in the US since 1890 and in 1924 started work on *Real Wages in the United States, 1890-1926*, a comprehensive statistical exploration of recent trends in wages, prices, employment, and unemployment rates (Douglas 1930). While assembling this statistical evidence, he worked to develop a theoretical framework through which to interpret it. In 1926 he submitted a "treatise on the theory of wages" to a competition sponsored by Hart, Schaffner, and Marx, and was awarded the $5000 first prize. The manuscript was too long to be published, and it was while Douglas was distilling it into book form that the first Cobb-Douglas regression was

[1] This section and the next are based on Biddle (2010).

estimated. Douglas recounted the "origin story" of the regression in several places, including this version in his autobiography:

> One spring day in 1927, while lecturing at Amherst, I charted on a logarithmic scale three variables I had laboriously compiled for American manufacturing for the years 1899 to 1922: an index of total fixed capital corrected for the change in the cost of capital goods (C), an index of the total number of wage earners employed in manufacturing (L), and an index of physical production (P). I noticed that the index of production lay between those for capital and labor and that it was from one third to one quarter of the relative distance between the lower index of labor and the higher index of capital. After consulting with my friend Charles W. Cobb, the mathematician, we decided to try to find on the basis of these observations the relative contributions which each of the two factors of production, labor and capital, had upon production itself. We chose the Euler formula of a simple homogeneous function of the first degree . . . (Douglas 1971, 46-47).

Cobb and Douglas estimated the value of k in the hypothesized relationship $P = bL^{k}C^{1-k}$ by using Douglas's "laboriously compiled" data to fit the linear regression Log(P/C) = b + kLog(L/C) by ordinary least squares. They then plugged values of C and L along with the estimated values of b and k into their assumed non-linear function, and calculated a series of predicted or "theoretical" values for P, denoted P'.

Douglas was encouraged by the high correlation between P and P', as well as the fact that the NBER's estimate for the share of manufacturing value added represented by wages and salaries over the period 1909-1918 was almost identical to the estimate of k. He described the research in a paper presented at the AEA meetings of 1927, which was published a few months later as "A Theory of Production" (Cobb and Douglas, 1928). Six years later Douglas published *The Theory of Wages,* which included as a central feature a description of the data, methods and results from "A Theory of Production", as well as the results of estimating the Cobb-Douglas regression with time series data from Massachusetts and New South Wales (Douglas 1934).

The 1928 paper included little in the way of explicit theory.[2] Douglas began with a list of questions that could be addressed if an empirical relationship between capital, labor, and output were discovered, including the rate at which the marginal products of capital and labor diminished, if indeed the common assumption of decreasing marginal productivity were true. There was a reference to the question of whether the "processes of distribution are modeled at all closely upon those of the production of value", but no discussion of the link between the two provided by marginal productivity theory. By contrast, in *The Theory of Wages* the discussion of the empirical estimation of production relationships was embedded in a detailed explication of the marginal productivity theory and a defense of that theory as a framework for inductive study of production and distribution. The estimated elasticities of curves of marginal productivity, which as of 1934 Douglas seemed to regard as the most important quantities revealed by his innovative statistical analysis, could, in light of the marginal productivity theory, also be regarded as elasticities of aggregate demand curves for capital and labor. And, when Douglas made comparisons between estimates of the value of labor's marginal product, derived from his regression results, and measures of real wages or labor's share of the value of output, there was no question as to the theoretical framework motivating such a comparison.

By *The Theory of Wages* Douglas had come to view his production research as being complementary to the work of men like Henry Schultz and Mordecai Ezekial, who in the mid-1920s were using regression analysis to estimate supply and demand

[2] Much of "A Theory of Production" was devoted to describing how the series for capital and labor had been constructed. Judged by the standards of the time, Douglas's construction of these series by itself would have been considered an important contribution to empirical economics.

relationships.[3] He argued that his own estimation of the key relationships governing production and distribution was an obvious extension of their efforts to build a quantitative account of economic activity on the "valuable theoretical scaffolding" provided by neoclassical theory, efforts that would help to make economics a progressive, empirically based science. (Douglas 1934, p. xii). And Douglas's use of regression analysis put his work at the cutting edge of the empirical economic research of the time (Biddle 1999).

Reactions of the Profession

Douglas made bold claims in "A Theory of Production" and *The Theory of Wages*: using generally available data and modern but still accessible statistical techniques, he had shown that the actual relationship between capital, labor and output in the aggregate economy could be closely approximated by a simple function, one which embodied and allowed quantification of the hypothesis of diminishing marginal productivity. He had demonstrated a relationship between the characteristics of this "law of production" and the distribution of income between labor and capital, a relationship posited by a well-known though still controversial theory of distribution. It is thus not surprising that the paper attracted the attention of a number of economists.

Douglas recalled in later years that his initial work with the Cobb-Douglas regression was, in general, poorly received by the profession (Douglas 1976, 905), but several early reviews of *A Theory of Wages* gave almost unqualified praise to the book.[4] There were, to be sure, unfriendly critics who had little good to say about what Douglas

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[3] See Morgan (1990) for a full account of this research.
[4] See, e.g., Dickenson (1934) or Rowe (1934).

had done. Slichter (1927) severely criticized the methods used by Douglas to create his index of capital, then went on to condemn the whole idea behind the Cobb-Douglas study, arguing that researchers' reliance on "static doctrines" like the marginal productivity theory was retarding the progress of quantitative economics. Douglas also found unfriendly critics among his more theoretically oriented colleagues at Chicago, who argued that the key concepts of economic theory were essentially static and abstract, while historical data was dynamic, reflecting the action of forces that were assumed away in static theory. Thus, statistical methods could never quantify theoretical concepts.[5]

However, many who published longer reactions to "A Theory of Production" or *The Theory of Wages* were essentially friendly critics: while expressing reservations about such matters as Douglas's data, his use of least squares regression, or his choice of the specific Cobb-Douglas functional form, their general tone was one of enthusiasm for the general goals of his research program, and they offered constructive suggestions for pushing the program forward.

J.M. Clark (1928), for example, shared many of Slichter's concerns about Douglas's data, but took it for granted that "they will be improved and refined as the authors continue their researches." He also suggested an alternative functional form for the regression, one that would estimate the impact on output of cyclical swings in the utilization of capital and labor as well as the long-run normal production relationships embodied in the original Cobb-Douglas specification. Douglas's colleague at Chicago,

[5] Douglas 1934, p. 107; letter from F. Knight to Douglas, 10/12/1932, Frank Knight papers, Box 58, Folder 16; Douglas, 1976, p. 905. Knight, whose opinion carried considerable weight with a number of younger Chicago faculty members at the time, was generally hostile to empirical work in economics, and was highly critical of Henry Schultz's work as well (Reder 1982). Knight's concern was shared by many other theorists at the time, and represented a major challenge for early econometricians in general. (Morgan 1990)

Henry Schultz (1929), voiced some of the same warnings as the Chicago theorists about the difficulty of measuring the concepts of static theory using time series data, but then, referring to his own approach to estimating supply and demand functions, suggested statistical procedures that Douglas could employ to close the data-theory gap.

After the appearance of *The Theory of Wages*, a significant set of friendly criticisms came from mathematical economists who embraced marginal productivity theory, and who were trying to make sense of the relationship between Douglas's regression equation and the equations of their theoretical systems. These included Wassily Leontief (1934), Jacob Marschak (1935), and David Durand (1937). It was in these articles, as well as a very critical article by Mendershausen discussed below, that the phrase "production function" was first consistently applied to the relationship that Douglas was attempting to estimate, although Douglas quickly adopted it himself. At the time, the phrase was rare in the economics literature, used almost exclusively by those concerned with mathematical formalization of neoclassical theory and/or the statistical estimation of the components of the resulting models. As mentioned earlier, Douglas saw his work as a part of the program to estimate the basic relationships of neoclassical theory, so the young econometricians who referred to Douglas's regression as a production function were affirming this conception of Douglas's.

However, while the neoclassically-oriented econometricians were embracing Douglas's program as complementary to their own, they were also redefining the objectives of the program, and developing criteria for evaluating Douglas's methods and results that Douglas himself would not have accepted. This is partly because these pioneering econometricians thought about marginal productivity theory within the context of Walrasian general equilibrium theory, in which the production function was a characteristic of a firm. As a result, when they considered Douglas's research, the question of whether the estimated coefficients revealed anything useful about the true parameters of firm-level production functions or were averages of those parameters in some sense seemed crucial. I would argue, however, that this question was never crucial for Douglas. As he noted in his autobiography, he was taught theory by J.B. Clark, and received "a thorough drilling in (the marginal productivity) principle, which served me well . . . when I started my own inductive work in the theory." (Douglas 1971, p. 29). But Clark's formal analysis ran in terms of aggregates: the basic wage rate and the interest rate depended on the marginal products of "social" capital and "social" labor (Stigler, 1941, p. 307). A student of Clark would have had no trouble thinking of an aggregate production function as a primal entity to be estimated, and its parameters as significant theoretical quantities.

There was also a methodological difference between Douglas and many who raised the issue of the relationship between Douglas's estimates and the production functions of individual firms. In *The Theory of Wages*, Douglas commented that

> I have treated the marginal productivity and supply curves for labor and capital in society as a whole and not for particular industries and plants. This has been done in part because as Willard Gibbs once remarked 'the whole is simpler than its parts' and because it seemed to me to be the more significant problem . . . (T)he forces at work in society as a whole need to be analyzed. For surely general results are more significant than are those for particular branches of industry and in turn are conditioning forces upon these subgroups (Douglas 1934, xv).

When Durand (1937) provided the first extended treatment of the question of whether Douglas's regression yielded estimates of the production functions of (Walrasian) neoclassical theory, he answered firmly in the negative. But he went on to implicitly endorse Douglas's methodological position concerning the existence and significance of an aggregate production function. Discussion of the issue of whether the parameters of a Cobb-Douglas production function estimated with cross-industry or aggregate time series data could be rigorously related to the parameters of the firm level production functions of theory would continue. It would be taken over, however, by economists more firmly committed to methodological individualism, who believed that the parameters characterizing the preferences and constraints of individual actors, including parameters of firm level production functions, were the holy grail of econometrics, and statistical methods and results were to be judged on the basis of what they revealed about those parameters.

Douglas's Cross Section Studies

Some time in 1937 or 1938, Douglas, working with younger associates at the University of Chicago, developed an approach to estimating the Cobb-Douglas regression using cross section data, in which all observations came from a single economy in a single year, and each observation described an industry. In a series of articles published between 1939 and 1943, Douglas's team reported the results of applying this methodology to manufacturing census data from the US, Australia, and Canada. The labor variable was the number of employees in an industry, while the capital variable would typically be the monetary value of total capital (fixed capital, "working capital", and sometimes the value of land). The product variable was value added by the industry, which was a monetary value, rather than an index of physical output as was used in the time series studies. A less restrictive version of the Cobb-Douglas equation in which the exponents for capital and labor were estimated separately (i.e., $P = bL^kC^j$) was adopted as the standard specification, responding to a common criticism of Douglas's time series studies and allowing for statistical tests of the "returns to scale" of the production function.

Over the course of the cross section period, the claims made by Douglas and his coauthors about the meaning and significance of an estimated Cobb-Douglas regression, and its relationship to neoclassical theory, evolved and never really reached a settled state. For example, in the first cross section study, the Cobb-Douglas production function was presented as a means of "statistical verification of the marginal productivity theory of distribution", the argument being that if the marginal productivity theory were true, and "competition approximately pure", then the estimated k coefficient would be close to the share of value added represented by wages, or labor's share (Bronfenbrenner and Douglas 1939, p. 761, 767-68). In Gunn and Douglas (1940) the research program was described as "an attempt to measure statistically . . . the probable elasticities of the marginal productivity curves of labor and capital", and the near equality of labor's share and the k coefficient was offered as evidence that regression had produced accurate estimates of these elasticities, thus assuming the validity of the marginal productivity theory and the presence of competition rather than testing for it. Bronfenbrenner and Douglas (1939) noted that because the product variable was monetary value added rather than physical marginal product, k would equal labor's share even if product markets were non-competitive, and only monopsonistic exploitation of labor would lead to a deviation of k from labor's share. In Gunn and Douglas (1941c, p. 127), however, more ambiguous language was used in describing what one learned from the comparison of k to labor's share, suggesting that not only was this equality implied by “pure” competition, but also implied competition in both product and factor markets.

Also, there was a gradual change in what Douglas and his coauthors were willing to infer from the sum of the estimated coefficients of labor and capital. At first, this sum (j+k) was simply taken as an indication of the returns to scale in production (e.g., Bronfenbrenner and Douglas 1939, p. 767). In the later studies, however, the comparison of j+k to one was offered as a test of the competitive neoclassical model of the economy, based on a version of that model in which the returns to scale faced by a firm could vary with the level of production, but in long run competitive equilibrium, all firms would operate at a point where production was characterized by constant returns to scale (Daly and Douglas 1943, p. 179).

These and other changing and sometimes inconsistent claims about the Cobb-Douglas regression were in part a result of Douglas’s relationship with his collaborators during this period, most of whom were Chicago Ph.D. students. Douglas gave these junior coauthors considerable latitude in writing into the articles their own views on both theoretical and statistical issues raised by the estimation of the cross section Cobb-Douglas regression, including some that Douglas himself never fully embraced.

The articles contained two distinct responses to the question of the relationship between the coefficients of a Cobb-Douglas regression estimated using aggregate data and the parameters of the production functions of individual firms posited in neoclassical theory. In Bronfenbrenner and Douglas (1939), Bronfenbrenner argued that understanding the relationship between statistically estimated production functions and theoretical production function was essential to establishing the legitimacy of the former, and he presented a theoretical model intended to illustrate the connection between the two. Bronfenbrenner later refined this model in an exchange with Melvin Reder that introduced a new terminology for discussing the cross section Cobb-Douglas regression.[6] Reder proposed that the "ordinary, theoretical concept" of the marginal value productivity of labor, that is, the partial derivative of the value of a single firm's output with respect to that firm's labor input, be called the *intrafirm* marginal value product of labor, while the rate of increase in the value of output "as we pass from one firm to another by varying the quantity of labor employed, but holding the quantity of the other factors . . . constant" be called the *interfirm* marginal value productivity of labor. (Reder 1944, p. 261) For several years to come, users of the Cobb-Douglas regression and their critics would talk about the distinction between intrafirm and interfirm production functions.

Gunn and Douglas (1941c) and Gunn and Douglas (1940), on the other hand, offered a statistical approach to the problem, arguing that the plant level production functions of theory could be estimated directly by using per-plant values of the variables in the regression (dividing each industry's variables through by the number of establishments in the industry). Douglas's own position on the matter probably never changed much from that displayed in *The Theory of Wages*. In a 1947 address summarizing his production research, he argued that production functions estimated with time series data, with cross section industry-level data, and with firm level data simply represented different types of production functions, each one interesting in its own right, and each "worthy of consideration." (Douglas 1948, 22-23).

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[6] See Bronfenbrenner (1944) and Reder (1944).

Econometric Criticiques of the Douglas Program

In addition to facing questions about the theoretical significance of an empirically estimated production function, Douglas and his coauthors had to deal with the statistical critique of Horst Mendershausen (1937, 1941), who doubted that the Cobb-Douglas regression (in either its time series or cross section incarnation) really represented a stable, causal relationship between inputs and output. Mendershausen argued that if such a relationship existed, it was probably not possible to estimate it using the data and statistical methods employed by Douglas.

Mendershausen was a student of Ragnar Frisch, and he criticized Douglas using the stochastic framework of Frisch's confluence analysis. Confluence analysis began by positing the existence of a deterministic linear relationship between several economic variables. Two problems were assumed to complicate the econometrician's task of estimating the coefficients of this relationship: all the variables were potentially measured with error (the errors in the variables problem), and there might be other exact linear relationships between the variables holding simultaneously with the one of interest to the econometrician. A key conclusion of the analysis was that classical regression (the method employed by Douglas) was a very unreliable tool for estimation of economic relationships.[7]

The problem of errors in the variables had earlier been analyzed by Frisch and others in the context of what Morgan (1990) calls the regression choice problem: when using regression to estimate an assumed economic relationship between variables, such errors would lead the estimated values of the theoretical coefficients to differ according to which variable was chosen as the dependent variable. A rule of thumb that developed

[7] See Hendry and Morgan (1989) for a detailed discussion of Frisch's confluence analysis.

was that the best choice for dependent variable in the regression analysis was the variable most subject to random measurement errors. This provided the basis for one of Mendershausen's (1937) tactics in attacking Douglas's work. Mendershausen demonstrated that the estimated values for Douglas's k and j parameters varied widely when different variables were designated as the dependent variable. Douglas's choice of product as the dependent variable was the only choice that produced economically sensible estimates. Mendershausen believed, however, that there was no good justification for this choice, especially since there was likely more measurement error in the capital variable than in the product variable.

The possibility that more than one linear relationship existed between the variables was problematic because it increased the levels of correlation between those variables, and obscured the relationship of interest. Mendershausen showed, graphically and with correlation coefficients, that Douglas's variables were indeed highly collinear, and explained that given the multicollinearity between Douglas's capital and labor variables, it would be impossible to produce credible estimates of the marginal effect of changing one of the inputs, holding constant the other.

Douglas and his associates developed two main responses to Mendershausen's econometric critique. The first was to argue, with their own graphs and correlation statistics, that the problem of multicollinearity identified by Mendershausen in Douglas's time series data did not plague the cross section data.[8] The second was to defend their use of regression analysis, and of a specification in which product was the dependent variable. This involved explicating a stochastic framework for thinking about the

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[8] See, for example, Bronfenbrenner and Douglas (1939), Gunn and Douglas (1940, 1941c). Mendershausen (1941) considered these demonstrations unconvincing.

estimation of a production function that was an alternative to the one provided by confluence analysis, and justifying the choice of dependent variable on the basis of economic as well as statistical considerations.

The Douglas team argued that whatever intercorrelations might exist between the three variables of the Cobb-Douglas function, simple economic logic indicated a causal relationship running from changes in input levels to changes in product: "certainly if there is any common-sense concept in economics it is that product (P) is a function of labor (L) and capital (C)" (Gunn and Douglas 1941b, 566). From there it was argued that when the caused variable (in this case, P) was used as the dependent variable, the failure of the estimated version of the relationship to fit perfectly was due not only to measurement error in the variables but also to unmeasured factors that had a causal influence on the dependent variable. These unobserved causal factors had the same impact on estimation as did measurement error in P – they biased the coefficient estimates if P was used as an independent variable, but not if P was used as the dependent variable. And these unobserved factors were likely to be quantitatively more important than the random measurement error in the C or L variables. By 1943, the Douglas team was linking this way of thinking about their methodology to "the modern theory of causation", in which "causal forces express themselves in 'norms' of results or tendencies accompanied by a more or less symmetrical range of variations" (Daly and Douglas 1943, 181-2).[9]

As the Douglas team moved away from a statistical framework in which the estimand was a deterministic linear relationship, they made more use of formalistic

[9] Thus, Douglas team and Mendershausen were engaged in version of the "errors in the variables" vs. "errors in the equation" debate over how to introduce stochastic el;ements into econometric models.

inferential procedures, for example, calculating standard errors to assess the difference between the k estimate and labor's share, or between j+k and unity. However, they never adopted a formal approach to hypothesis testing, either Fisherian or Neyman-Pearson.

Six years after Mendershausen's attack, another detailed critique of the Douglas program was presented in Marschak and Andrews (1944), this time from the perspective of the Cowles Commission econometric research program. The Cowles approach held that economic models should be expressed as systems of stochastic simultaneous equations representing the optimizing decisions of individual economic agents. Accordingly, Marschak and Andrews began by embedding the production function in a simultaneous equations model of the input and output decisions of individual profit maximizing firms, and explained why the goal of empirical estimation should be the recovery of these firm level production functions. Sounding a central theme of the Cowles researchers of the time, they demonstrated that Douglas's use of traditional regression to estimate production functions was inappropriate when the production function was part of a simultaneous system. Further, they showed that within the simultaneous system they proposed, the parameters of the production function were not identified (although they did not use that term).[10] And even if one set aside these problems, they argued, it was only under very heroic assumptions that the coefficients of "production functions" estimated using cross section industry level data would correspond to the parameters of the individual firm production functions. In another section of the paper, they used their model to argue that the conclusions Douglas and his

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[10] In his 1936 review of *A Theory of Wages*, Marschak had raised issues of identification in a less formal way, comparing the problems confronting attempts to estimate production functions to those being tackled by researchers estimating supply and demand functions.

associates had drawn from their results about competition, the marginal productivity theory, and the existence of constant returns to scale in production were unwarranted.

Marschak and Andrews (1944) can be read, and was read by some, as a devastating criticism of Douglas's work, a laundry list of problems and misunderstandings, both theoretical and empirical, that fatally marred the program. But the authors' tone mitigated against such an interpretation. They proclaimed Douglas a pioneer, and hinted that many of the problems they had identified with the Douglas estimates were due simply to the fact that Douglas had not had access to firm level data. Douglas's results were still important, and the program itself was a worthwhile and promising one (Marschak and Andrews 1944, pp. 179, 182). Marschak was still a friendly critic, captivated by Douglas's vision of statistically estimating the key relationships of neoclassical value and distribution theory, and his article with Andrews was a message to others who were likewise captivated: the vision could still be realized, and the Cowles Commission would show the way.[11]

In 1948, Paul Douglas was elected to the US Senate, effectively ending his academic career. In 1947, however, he served as president of the American Economic Association, and he used his presidential address to summarize and interpret the results of his 20 years of research into "laws of production" (Douglas 1948). He closed the address with an appeal to the rising generation of economists to continue working on the many questions raised by his work, but this process was already underway as he spoke.

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[11] Marschak and Andrews' decision not to speak more harshly of Douglas's work may also have been related to circumstances in the Chicago economics department, where at this time there was an "intense struggle" underway between faculty aligned with Knight and faculty aligned with the Cowles Commission (Reder 1982). Differences over matters like the role of empirical work in economics and the potential for economic research to inform interventionist economic policy led to fights over hiring and resources. In Marschak's eyes, Douglas, with his strong record of empirical economic research and his progressive political views, was clearly on the right side of this struggle. To undermine Douglas would have been to alienate a potential ally.

Economists were estimating the Cobb-Douglas regression in new ways and for new reasons. Klein (1946), for example, alluded to the existence of a number of firm level studies, commenting that "statisticians have found that the Cobb-Douglas function (or simple modifications of this function) fit the output-input data very well." And following Solow's famous 1957 article exploring the sources of economic growth, the Cobb-Douglas production function would become a common tool in the empirical "growth accounting" literature. In the decade following WWII, however, most of the published applications of the Cobb-Douglas production function were the work of agricultural economists, and it is to this literature that I now turn.

The Introduction of the Cobb-Douglas Regression into Agricultural Economics, 1944-1955

In retrospect, it is not surprising that agricultural economists were prominent among those who adopted the Cobb-Douglas regression as an empirical research tool. During the interwar period agricultural economics was a hotbed of empirical research. The US Department of Agriculture and the state supported land-grant colleges employed economists with the expectation that they would conduct research into issues of interest to farmers and agricultural policy makers. These economists had access to agricultural data collected by the government, and to funding for the collection and analysis of experimental data and survey data on the activities of individual farmers. A further bias towards empiricism was created by the principle that their research should help create knowledge that was practically useful for farmers (Fox 1989). As a result, training in statistical methods was emphasized in graduate programs in agricultural economics, and many of the pioneering econometricians of the interwar period came out of the field of agricultural economics (Rutherford 2009, Fox 1986). So, when Douglas's production studies, with their innovative use of regression analysis, began to appear, agricultural economists were in a better position than economists in general to understand them, and were more likely to be intrigued by the statistical issues they raised.

In addition, Douglas's vision of statistically estimating the production function(s) of neoclassical economic theory was salient for two related and long-standing research areas in the field of agricultural economics. Banzhaf (2006) provides an excellent account of the emergence of agricultural economics in the early 20th century, as economists came to dominate the pre-existing research area of farm management.[12] In the early 1900s, farm management encompassed the work of applied scientists from a variety of fields – plant genetics, soil science, entomology, etc. – whose research was intended to help farmers solve practical problems. The early agricultural economists envisioned a central role for themselves in this field, as the experts who could teach farmers how to apply the scientific knowledge from several disciplines to keep their farms profitable in the face of shifting economic forces. Their claim to this role was based on their possession of a general analytical framework for thinking about the business decisions faced by a farmer: the neoclassical theory of the firm. A few basic concepts like opportunity cost and the equation of marginal cost and marginal benefit could be used to analyze the dizzying array of choices faced by a farmer seeking to maximize profits. To a greater extent than the average economist of the time, then, these economists were well versed in and convinced of the usefulness of neoclassical theory, although, as Banzhaf points out, it

[12] This and the next two paragraphs are based on Banzhaf (2006), who describes some of the major figures behind the trends described. Additional references are to papers illustrative of the type of research described in the text.

was a commitment to neoclassical economics as a normative tool to guide resource allocation, not as a positive description of economic activity. Indeed, they viewed the agricultural sector as being rife with inefficiency and misallocation, thus justifying the need for farm management research.

As economists moved into the field of farm management, they found a very empirical, practical, but rather atheoretical literature, in which commonsense notions of economy were applied in haphazard and conflicting ways. Over the interwar decades, they sought to bring order to the field through the application of neoclassical theory, generating a body of research and knowledge they called production economics.[13] Research in production economics included the application of the logic of maximization to a variety of situations arising in farming, such as the allocation of laborers of varying efficiency to cooperating inputs of varying efficiency (Waite 1936), or the "multi-enterprise farm" which produced multiple outputs requiring the same type of input (Benedict 1932a). Empirical research included the development of better empirical counterparts for the cost and return concepts of neoclassical theory, e.g., opportunity costs rather than accounting costs, and marginal rather than average returns (Benedict 1932b). As the neoclassical agricultural economists pursued this agenda through the 1920s and 1930s, there was continual tension between the desire for analytical concepts and empirical measures that were theoretically correct, and what was possible and practical given the resources and tools available for the collection and analysis of data.

A few examples serve to illustrate these tensions. One line of empirical research in the traditional farm management literature involved the collection of detailed accounting records from samples of farms, and then the calculation of input-output ratios

[13] Ezekiel (1926) gives a good sense of the agenda of these economists.

(e.g., pounds of feed per hundredweight of cattle) or efficiency ratios (crop acres per man). The values of such ratios on more profitable farms were offered as standards for farmers in general. Economists objected that such observed ratios on any given farm were a function of particular characteristics of that farm, and thus might be optimal for other farms. Also, the calculation of ratios as guides for farmers often implicitly assumed away the core economic principle of diminishing returns. A good deal of experimental research, often conducted by non-economists, was devoted to finding the most profitable mix of inputs to maximize a particular output, whether it was the mix of grain and hay in feeding cattle or the allocation of labor and machinery to maximize the value of a corn crop. Economists came to argue, however, that such information was potentially misleading to the many farmers on multi-enterprise farms, who had to allocate inputs between competing outputs. They pushed for additional data collection and analysis that focused on measuring the profits of the farm as a whole, and the correlation of those profits with the input and output choices of these multi-enterprise farmers (Black, 1932 pp. 250-262, Hopkins 1930).

It is thus easy to see why economists involved in farm management research in the early 1940s would be excited about Douglas's estimation of production functions. In principle, a properly estimated production function could provide a wealth of theoretically appropriate information to guide farmers in their input and output decisions. The regression would yield not just ratios but functions relating inputs to output, thus quantifying the action of the law of diminishing returns; it would also reveal input substitution relationships. If applied to the financial data typically collected in farm management studies, production function estimation could reveal the value productivity of expenditure on various types of inputs for multi-enterprise farms. Indeed, during the thirties some agricultural economists had already produced estimates of the production relationships implied by neoclassical theory, including two way cross tabulations or bivariate regressions involving one input and one output (Hopkins 1930, Warren 1936, Menze 1942), but Douglas's approach offered clear advantages over these earlier efforts: his functional form easily handled several inputs and parsimoniously captured the key assumption of diminishing returns, and when estimated as a regression produced coefficients that were directly and easily interpretable as elasticities of output with respect to inputs.

The normative neoclassicism that characterized economic research on farm management also found its way into discussions of national agricultural policy, although it was only one perspective among many. A number of agricultural economists believed that misallocation of social resources within agriculture and between agriculture and the rest of the economy was a major contributor to the persistent problem of low farm incomes, which marred the otherwise robust US economy of the 1920s, and was exacerbated by plunging agricultural prices in the early years of the depression. They argued that too much land was being cultivated by too many laborers, and in many areas agricultural resources were being devoted to the wrong outputs. This point of view was prevalent enough that it came to be embodied in the Agricultural Adjustment Act of 1933 (Barber 1994), and among those who shared it, some argued that the neoclassical principles that guided the farmer towards maximum profits also provided the best tools for analyzing the social misallocation of resources and for designing measures to address it (Heady 1948, p. 205, Johnson 1944, pp. 635-636). These agricultural economists saw

Douglas's production function as a tool for diagnosing misallocation of social resources, as it could detect situations in which the marginal productivity of inputs in agriculture differed across regions, or differed from the return those factors might earn in other sectors of the economy.

Prior to 1950, most applications of the Cobb-Douglas production function to agricultural data were the work of a small number of researchers associated with Iowa State College, where T.W. Schultz had assembled a group of economists committed to using neoclassical reasoning and advanced empirical methods to help solve the problems of agriculture.[14] The first two applications appeared in *Econometrica* in 1944, in articles by Gerhard Tintner. In one, Tintner estimated the Cobb-Douglas regression using data from a sample of 609 Iowa farms (Tintner 1944a). He pointed out the advantages of the Cobb-Douglas form, including the elasticity interpretation of its coefficients and its embodiment of the law of diminishing returns. Tintner's "product" variable was gross farm profits, and he grouped inputs into six categories: land, labor, improvements (farm buildings and other structures), liquid assets (e.g., livestock, feed, and supplies), working assets (tractors, other machinery, and work animals), and a residual category of cash operating expenses. Land was measured in acres and labor as months of hired and family labor, while the remaining inputs were measured in monetary terms. In the second article, Tintner used annual time series data to estimate an aggregate production function for the US agricultural sector, including a linear time trend in the regression along with capital and labor variables (Tintner 1944b).

[14] Schultz left Iowa State for the University of Chicago in the fall of 1943. Of the economists whose work is discussed below, Tintner came to Iowa State in 1937 and remained after Schultz's departure, while Johnson and Heady were attracted to Iowa State's graduate program by Schultz, with Johnson accompanying Schultz to Chicago and Heady remaining on the faculty at Iowa State.

Tintner was first and foremost an econometrician, and his interest in Douglas's regression was motivated more by the econometric issues it raised than by its applications for agricultural policy. The "variate difference method" he used to estimate the regression in the time series paper was something he had developed for estimating the coefficients of a deterministic linear relationship between variables when all the variables were measured with error, that is, the same statistical model underlying Frisch's confluence analysis and Mendershausen's attack on Douglas's production studies. Tintner estimated the agricultural production function simply as a means of demonstrating this method, and he had almost nothing to say about the economic implications of his results.

Likewise, Tintner's estimation of a cross section Cobb-Douglas regression using farm records was aimed at addressing two perceived econometric shortcomings of Douglas's program. First, the use of a sample in which the farm was the unit of analysis was responsive to Marschak and Andrews' demonstration that a Cobb-Douglas regression was quite unlikely to recover the parameters of a true theoretical production functions if it was estimated with data aggregated above the level of the individual firm.[15] Also, the paper was the first to explicitly and rigorously apply a theory of statistical inference to the estimates from a Cobb-Douglas regression. T-tests were done on the coefficient estimates, and several pages were devoted to describing and conducting the appropriate statistical test of the hypothesis that the estimated coefficients of the inputs summed to one. Again, having presented his results, Tintner had almost nothing to say about their economic implications for farmers or farm policy.

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[15] Tintner did not explicitly refer to this issue, but he was clearly aware of the relevance of his paper to it, as Marschak and Andrews (1944) referred to Tintner's paper , and Tintner's acknowledgement footnote thanked Marshcak for his advice and criticism.

The role that the Iowa State economists saw for the Cobb-Douglas regression in farm management research became clearer in Tintner and Brownlee (1944). This time, the regression was estimated for a sample 498 Iowa farms, and with five subsamples of distinct farm types. Input and output variables were defined as in Tintner (1944a). However, after reporting the coefficient estimates, the authors presented a table of marginal productivities calculated from the coefficient estimates, indicating "the returns which might be expected from the addition of one dollar's worth of the various productive agents". Fiducial limits for the marginal productivities were also presented.[16] In a section on "economic interpretation of the results" it was noted that most farm types showed decreasing returns to scale, although the authors warned that this might be because no measure of the management input had been included. Marginal productivities were compared within farm types, leading to conclusions like "additional inputs of liquid assets and working assets, and additional cash expenditures on equipment repairs, fuel, oil and feed will probably yield higher returns than additional improvements" (Tintner and Brownlee 1944, p. 571). Indications that the marginal productivity of some inputs differed considerably across farm type were also pointed out, but no firm conclusions were drawn from this.

In both the cross section studies just discussed, the sample data had been compiled by farmers participating in an agricultural extension program that encouraged them to keep and use more detailed financial records. This, as Tintner and Brownlee (1944, p. 571) pointed out, led to a non-representative sample favoring relatively large farms that "may represent the upper part of the production function". Tintner and Brownlee's colleague Earl Heady addressed this problem by estimating the Cobb-

[16] Fiducial limits were a concept taken from R.A. Fisher, serving the same purpose as confidence intervals.

Douglas regression with a random sample of Iowa farms surveyed in 1939 (Heady 1946). Although he followed pretty closely the format and methods of the two previous studies, he gave a more a thorough account of the strengths and weaknesses of the method, and a more extensive discussion of the practical implications of the results for farm management.

Heady's sample had over 700 farms, and he estimated production functions for subsamples based on geographical area, type of farm, and farm size. Generally, the estimates indicated decreasing returns to scale. This surprised Heady, given the prevalent belief that larger farms were more efficient, and he presented a careful analysis of the circumstances under which this finding could be attributed to the omission of a variable measuring management. He observed that the marginal productivity of machinery was higher in those areas of Iowa known to be least mechanized. He cautioned about comparing marginal productivities of inputs within farm types: some input variables were the money values of stocks and others were the money values of flows, so assumptions about depreciation rates would have to be made before comparing the marginal impact of a one dollar increase in a stock variable vs. a flow variable.

Heady voiced several concerns about the limitations of the Cobb-Douglas method, but presented his study as a first step in a better direction for farm management research: with refinement, "the general type of analysis . . . might serve as a useful tool in studies concerned with the productivity and allocation of resources." (Heady 1946, p. 1000) For example, he pointed out, it had become standard to assess the relative efficiency of farms with a measure of net farm profit (or loss) created by subtracting from total revenue a measure of the value of inputs used, including the imputed values of service flows from stocks of land, buildings, and machinery. Unfortunately, the farms ranked as most efficient by this measure years when output prices were high could end up being ranked as least efficient when output prices were low. Heady proposed as an alternative efficiency measure the difference between a farm's actual output and the output predicted, based on its input levels, by a well-specified production function estimated with a sample of similar farms.

The pioneering farm–level production function studies just described all made reference to Douglas's production studies, and evidenced a solid knowledge of the debates surrounding those studies. However, there are several interesting differences in approach and emphasis between the Douglas studies and the cross section agricultural studies of Tintner, Brownlee, Heady and the researchers who would subsequently follow their lead. First, as noted above, the views of Douglas and his coauthors as to the statistical assumptions to make in thinking about their data, and thus the correct procedures for estimation and inference, evolved over time and never really stabilized. By contrast, the agricultural economists who used the Cobb-Douglas regression from the beginning thought about their estimation problem in terms the statistical assumptions and procedures developed and employed by Ronald Fisher.[17] They discussed as a matter of course the extent to which their samples were representative of a population of interest. Their least squares coefficients were regarded as estimates of population parameters, and one drew inferences from the estimates by using standard formulas to calculate

[17] Because agricultural researchers had access to experimental data, and because Fisher, in his widely read textbooks, explicated his methods in the context of agricultural experiments, it is not surprising to find that agricultural economists were trained in and willing to adopt this framework. Rutherford's (2009) account of the USDA graduate school indicates the type of statistical and economic training received by agricultural economists during the period. Fisher (1928) was used in statistics classes by the early 30's, as were his papers on fiducial limits; Fisher also visited the school on more than one occasion, speaking on statistical inference an hypothesis testing in 1936 (Rutherford 2009, pp. 12-14).

confidence intervals or to determine levels of statistical significance. Thus, these agricultural economists brought to the empirical production function a higher level of mathematical formalism as well as what Porter (1995) called “mechanical objectivity”, in the form of a standardized set of inferential procedures. Also, while the agricultural economists shared with Douglas the goal of statistically determining the relationship between inputs and outputs and “the probable elasticities of the curves of marginal productivity”, they had no interest in the Cobb-Douglas regression as a tool for testing the theory of marginal productivity, which they were already certain did not apply in the settings they were studying (e.g., Heady 1946, 1001).

Finally, the problem of actually specifying the Cobb-Douglas regression, in particular defining how inputs were to be categorized and measured, had a much greater significance for the agricultural economists than for Douglas. As noted above, Douglas worked with J. B. Clark’s conception of a law of production linking social capital and social labor to social output, which identified a two-input production function as an important relationship to estimate. He gave lip service to the existence of heterogeneity of capital and labor inputs and the desirability of developing measures to reflect this, but he regarded these issues as matters for future consideration rather than pressing items on his own research agenda (Douglas 1934, 60-61). Agricultural economists involved in farm management research could not comfortably take such a position, however. They were interested in production function estimation as a tool for diagnosing and prescribing actual agricultural practices, and the effectiveness of the method in this role depended on their ability to specify regressions that statistically captured the different production technologies and feasible input combinations found in the many types of farming.

In the same year that the Cobb-Douglas production function was introduced into the farm management literature, D. Gale Johnson (1944) used the regression as a tool for discussing national agricultural policy. It was employed in an article promoting the idea of a “forward price policy”, in which the government would announce guaranteed prices for agricultural output in advance of farmers’ production decisions. One task of the article was to demonstrate empirically the existence of a serious misallocation of resources within agriculture and between the agricultural sector and the rest of the economy, using as a benchmark the neoclassical principle that labor and capital should have the same marginal product in all economic activities. For the most part, Johnson made his case by comparing ratios of output to input across regions. However, to address the question of the allocation of resources between agriculture and the rest of the economy, Johnson estimated a Cobb-Douglas regression. His sample consisted of nine categories of agricultural produce measured in each of four US census regions. Labor was measured as man-years, capital by the value of land and buildings, and product by value added. The estimated coefficient of labor was .88, and that of capital was .77. Johnson understood that his estimates indicated increasing returns to scale in agriculture, and that if factors were paid according to their marginal products the total payments to factors would exceed the value of production. He pointed out, however, that there was no reason to expect factor payments to equal marginal products in the real world. Johnson concluded that if one made reasonable adjustments to the data, the marginal product of labor indicated by his function was very close to the actual average agricultural wage, while the estimated rate of return to capital in agriculture was 12%, well above the economy-wide interest rate that he took as a measure of the general return to capital.

Cobb-Douglas regressions continued to appear sporadically in the agricultural economics literature in the later 1940s. Harries (1947) estimated the function using data from Canadian farms, and despite Harries' emphasis on the limitations of the method, his discussant was enthusiastic about its potential to advance the field of farm management (Sinclair 1947). Lomax (1949) estimated an aggregate production function using time series data from the UK, but unlike Johnson, was unwilling to draw any policy implications from it. During the late 1940s and early 1950s, however, Earl Heady was the major user and promoter of the Cobb-Douglas regression in agricultural economics. "Input output relationships", Heady argued, "are the partial basis for the majority of recommendations by agricultural economists", including those made to farmers about the quantities and combinations of inputs to use and those related to national farm policy. Heady believed that the regression approach to production function estimation represented a considerable improvement over existing methods that estimated univariate input output relationships, or calculated fixed input output coefficients (Heady 1952, p. 775). He thus continued to estimate farm-level functions, using samples from diverse farm types and regions, and also estimated production functions for individual agricultural processes. In Heady (1951) he estimated a Cobb-Douglas regression relating the volume of milk output to the amounts of grain and hay fed to dairy cattle, and used it to draw a three dimensional production surface that provided concrete illustrations of theoretical concepts like the marginal rate of substitution and the marginal productivity of one input holding the other constant. Heady et. al (1953) was intended to demonstrate the possibilities for cooperation between economists and the physical and biological scientists involved in farm management research. The team designed an experiment in which pigs were fed with different levels of grain, protein supplement, and antibiotics, and used the data to estimate Cobb-Douglas and quadratic production functions.

Heady's production function papers displayed a command of statistical technique well beyond that of the average economist publishing empirical research during the period. It was clear that he regarded his efforts as exploratory research, demonstrating a potentially fruitful method that he hoped would be improved by others. He was frank about the limitations of his data and his methods, reviewing the interfirm-intrafirm issue raised by Reder (1944), the problems posed by the need to aggregate heterogenous inputs, the possibility of simultaneous equations bias, and so on (see esp. Heady 1952). But he always kept clearly in the reader's sight the potential for practical applications of the results of the study at hand as well as the method in general. The study of dairy cattle feeding, for example, was relevant to a contemporary interest in devoting more land to grazing and the production of forage crops. Having estimated farm-level production functions for samples of livestock and crop farms in Alabama, Montana and Iowa, he took a cue from Johnson (1944) and looked for evidence of resource misallocation by comparing estimated marginal productivities of inputs to estimates of factor prices, as well as comparing them across different farm types in the same region, and across farms of the same type in different regions. (Heady and Shaw 1954). He concluded that the size of the differences (and the extent to which they were statistically significant) suggested a less serious misallocation problem than was indicated by simpler measures used by previous writers, such as ratios of output per worker.

By the mid-1950s agricultural economists outside of Iowa State were responding to Heady's evangelism, publishing micro-level production function studies in journals and in the agricultural bulletins that were the main vehicle for communication of research results among agricultural economists (Johnson 1955). However, articles criticizing the method also began to appear in the *Journal of Farm Economics*. Some of their authors drew on the literature that had criticized Douglas's work, citing problems of multicollinearity and potential simultaneity (e.g., Redman 1954). Several pointed out the interfirm-intrafirm problem, although some were willing to accept Heady's (1952) argument that the problem could be minimized if farm-level data were used, given a sufficiently homogenous sample of farms. The exclusion of a variable measuring management seemed a serious problem to some of the doubters (e.g. Wheeler 1950). Jones (1952) devoted an entire article to criticizing the theoretical framework, empirical methods, and policy recommendations of the researchers he dubbed the "Schultzeans" or the "Ames School", with his distrust of their production function estimates based on Johnson's (1944) small sample and the poor measurement of the labor input by Tintner, Brownlee, and Heady.

The most common arguments of the critics, however, revolved around a belief that the Cobb-Douglas form and the regression method were insufficiently flexible to capture the variety and complexity of the technological processes involved in agriculture. Older and newer technologies might be simultaneously employed by different farms in the sample or even on the same farm. Inputs were too numerous to all be included in a single regression equation, and too idiosyncratic to aggregate. Plaxico (1955) offered the most thorough and rigorous critique along these lines. Among other things, he showed that whatever functional form was used in a regression study of the production function, different decisions about aggregating some inputs would lead to different estimates of marginal productivity of others. He explained that there were optimal aggregation rules for different classes of inputs (perfect substitutes, perfect complements, imperfect substitutes), should one already know of these relationships, but the assumptions required for the rules to hold seemed unlikely to be met. Likewise, aggregation of a number of farm outputs into a single monetary product variable would bias the marginal productivity estimates under most realistic circumstances. In a summary assessment that took aim at entire range of applications for which the new method was being promoted, Plaxico opined that "extreme caution should be exercised in suggesting intrafarm adjustments on the basis of Cobb-Douglas functions. Also, since it is not clear just how an estimating equation which is not suitable for guiding individual farm decisions can suggest optimum area shifts, the validity of policy recommendation arising from Cobb-Douglas analysis is quite questionable." (Plaxico 1955, p. 675)

Epilogue: Agricultural Production Functions after 1955

Despite all criticism, articles using the Cobb-Douglas and other regression specifications to estimate production functions appeared with increasing frequency in the *Journal of Farm Economics* between 1955 and 1960, written by authors from a variety of institutions, and often referring to agricultural bulletins or Ph.D. theses in which the method was also employed. Some agricultural economists were convinced of the shortcomings of the early production function regressions, but were unwilling to abandon the research goals that had motivated them, and turned to the newer technique of linear programming as better suited for the estimation of agricultural production relationships (e.g., Toussaint 1955). Even Heady came to conclude that, at least for the analysis of activities on a multi-enterprise firm, linear programming was superior to the production function approach (Dillon 1996, p. 54). Another important response to criticisms of the Cobb-Douglas regression as a tool for measuring agricultural production relationships, in particular those concerning unobserved heterogeneity across farms and farm managers, was to estimate the regression using what came to be called panel data, leading to seminal work in the area of panel data econometrics (see DuPont-Kieffer and Pirotte, this volume). By the early 1960s the Cobb-Douglas regression was well on its way to becoming ensconced in the toolbox of agricultural economists involved in farm management research.[18] In this role, the Cobb-Douglas regression was no longer linked to the project of testing the validity of the marginal productivity theory, so that its perceived utility was largely unaffected by a growing debates over whether it could actually be used for such a test, and other doubts about its macroeconomic incarnation (e.g. Phelps-Brown 1957, Simon and Levy 1963).

The use of Cobb-Douglas regressions estimated with aggregate data to diagnose misallocation of social resources, on the other hand, never really caught on with agricultural economists. However, inspired by the growth accounting literature that built on Solow's (1957) paper, Griliches (1963a,b) re-introduced the aggregate Cobb-Douglas production function into the agricultural economics literature as a tool for exploring the nature of productivity growth and technological change in agriculture, a role in which it would come to be widely used (see, e.g., Antle and McGuckin 1993).

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[18] As is clear from this comment in 1986 agricultural economics textbook:"The Cobb-Douglas type of production function has been estimated by agricultural economics for virtually any production process involving the transformation of inputs into outputs in an agricultural setting. To review scientific application of the Cobb-Douglas type of function would be to review a large share of the literature in which empirical attempts have been made to estimate production functions." (Debertin 1986)

It is interesting to consider these developments in light of the hopes that Douglas (1948) expressed for his research program, as he prepared to leave its development in the hands of others. The work of the agricultural economists estimating the function with samples of types of farms responded to the need Douglas had seen for studies of “a large number of specific firms . . . within individual industries” (23), while their willingness to also embrace Cobb-Douglas regressions estimated with more aggregated data as tools for studying the growth of agricultural productivity accorded with his belief that the aggregate production function was different from, but no less interesting than, the firm level production functions of microeconomic theory. I would note finally that as the literature on production function estimation developed, new techniques were introduced for estimating production relationships using cost data rather than data on inputs and outputs, techniques founded on the assumption that factors were receiving payments equal to their marginal productivities. This, of course, was an assumption the Cobb-Douglas regression had been designed to test, an assumption explicitly rejected by the men who introduced the Cobb-Douglas production function into agricultural economics, but an assumption that Douglas, based on his reading of the evidence from his own production studies, would have readily embraced.

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