The Cobb-Douglas Regression and the Measurement of Economic Growth and its Causes

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This paper is an excerpt from my projected book on the Cobb-Douglas regression. I use the phrase Cobb-Douglas regression to describe the empirical procedure of regressing a measure of output on measures of inputs, under the assumption that the production function takes what is now known as the Cobb-Douglas form. The procedure was introduced in Cobb and Douglas's 1928 paper "A Theory of Production", and Douglas continued to work with it for the next 20 years, producing a stream of studies in which the regression was applied to both cross section and time series data sets. Douglas's work with the Cobb-Douglas regression attracted considerable comment, both positive and negative, from his fellow economists, but prior to the mid 1940s few outside of Douglas's research group at Chicago actually used the regression. It was only over the two decades following the war that the Cobb-Douglas regression became something of a general purpose research tool for empirical economists, who adopted, adapted, and generalized the procedure to explore a variety of questions both micro and macroeconomic in nature.

The material here comes from a segment of the book intended to describe how the Cobb-Douglas regression became a central tool in the effort by American economists to measure, explain, and empirically "account for" economic growth. In the 1928 "Theory of Production" article, Douglas listed as one of the goals motivating his research a desire to estimate the extent to which recent increases in output had been "primarily caused by technique", rather than changes in the quantities of labor and capital employed. The results described by Cobb and

Douglas, however, did not throw any light on this matter, a fact that troubled both Douglas and other economists who commented on his novel empirical procedure. Douglas never adjusted his regression procedure to either measure or control for the impact of technological change on growth, instead sidestepping the problem by developing a cross-section approach to estimating his production function. However, following Solow's articles on growth theory in the late 1950s, it became common to use the Cobb-Douglas regression procedure and generalizations thereof to measure and quantify the sources of economic growth.

In this paper I provide a history of economists' attempts from the 1920s to about 1940 to measure economic growth and its causes, while also recounting the arguments made over the same period by Douglas and others about the potential for the Cobb-Douglas regression procedure to be used to investigate growth and technical change. As Griliches (1996) pointed out, these two streams of research would come together in the late 1950s and beyond to provide the foundation for research on "growth accounting" and productivity measurement, a list to which I would add the empirical literature on economic growth based on cross country regressions. The present paper does not provide an account of this merging of the two research streams, but I do sketch the high points of that story in a concluding section.

Measuring Economic Output and Growth: the 1920s

By the second half of the 1800s, there was broad agreement among economists that steady, if not astounding, growth in productive capacity had become a standard characteristic of modern or "civilized" nations, and that a major factor driving this growth was what would today be called technological change.[1] The view could be found at mid century in both Mill's *Principles* and *The Communist Manifesto*, and, a few decades later, in Marshall's *Principles* as well as the opening pages of Henry George's *Progress and Poverty*.[2] It was not until the early decades of the 20th century, however, that enough statistical material had been accumulated to allow economists to confidently attempt to measure this growth and empirically investigate its causes.

Measuring the growth of a nation's output required consistent and repeated measures of that output, and by the early 1920s, two basic approaches to constructing such measures were being employed. One, which I shall call the physical index approach, proceeded in three steps. The first was to gather several time series measured in physical units, each intended to represent the activity of a well defined industry or sector of the economy. A series might measure one of the outputs of the industry or a key input used by the industry: for example, the leather industry might be represented by both cattle hides consumed and leather gloves and mittens produced. The next step was to express the annual values of each series relative to the value of that series in a common base period. The third step was to average these series of relatives using a set of weights that reflected the relative importance of the industries represented by the series, as measured, for example, by total sales, employment, or value added.

An early example of such measure was the Index of the Physical Volume of Production constructed by E.E. Day and Warren Persons of Harvard's Committee on Economic Research

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[1] Writers of the time used words and phrases such as "invention", "changes in technique", "improvements in the industrial arts", or "increases in man's command over nature."

[2] Mill (1987 [1848]), p. 696-7; Marx and Engels (1992 [1848], pp. 7-8; George, (1981, [1879]), p. 3; Marshall (1948 [1920]), pp. 671, 674-5.

(Day and Persons 1920-21). This index combined series representing the three sectors of agriculture, mining, and manufacturing, with manufacturing further divided into 14 industry groups. The agriculture component of the index, for example, included the annual production of 12 “important” crops, while the manufacturing component included such series as gallons of distilled liquors and tons of pig iron. Coverage extended back to 1899 for the manufacturing and overall indexes, and to 1879 for the agriculture and mining components.

During the 1920s new indexes of this type were being introduced and old ones revised as more and better data became available. In “A Theory of Production” Cobb and Douglas used as the “product” or dependent variable in their regression the manufacturing component of the Day-Persons index (as revised in Matthews 1924), but only because a better index being constructed by Woodlief Thomas was not available in time (Cobb and Douglas 1928, p. 150). In 1922 the Federal Reserve Board began issuing a monthly index of production, with coverage going back to January of 1919 and composed of 22 series. In 1927, the index was broadened and several refinements were introduced into its construction:

> The new index of industrial production . . . is made up of two component indexes, one of manufactures and the other of minerals, and is computed from 60 series of monthly figures representing average output per working day and adjusted for typical seasonal variations. The average of three years 1923, 1924, and 1925, was adopted as the base. The weights for the various industries in the manufactures index were derived from figures showing value added by manufacture, as reported by the Census of Manufactures for 1923 and 1919, and those of the minerals index were derived from the values produced . . . An aggregative formula was

> employed to combine the individual series into a composite index. From 1919 to 1922 the final index was the average of the two separately combined indexes, one with 1919 weights and the other with weights for 1923 (1923-25 in the case of minerals) . . . (Thomas 1927, 316-317).

This passage only begins to hint at the many challenges and decisions that faced economists creating these indexes. The conversion of the data from monthly totals to average output per working day in the revision of Federal Reserve Board index, for example, was a response to spurious fluctuations in the monthly totals caused by changes in the number of working days per month. Numerous options were available in choosing weights, and series could be averaged arithmetically or geometrically, before or after being converted into relative form. Day and Persons developed a method for "anchoring" their annual manufacturing index based on 33 series to a far more comprehensive index built from materials reported in the quinquennial US Censuses of Manufacturing. And the maker of each new index was expected to offer tests and proofs of the credibility, if not the superiority, of his index, such as measures of the share of each industry's output comprised by the series used to represent that industry, or graphical and statistical comparisons of the consistency of the new measure with previously established indexes of production, accompanied by reasonable explanations of any important deviations. But there was fairly broad agreement across the indexes of physical production when it came to the question of economic growth: From 1900 to the mid 1920s, mining and manufacturing output had increased by 5-6% per year on average, while the index that included agriculture grew at a little under 4% per year. With population growing at about 2% over the same period, this implied a healthy annual rate of growth of per capita output.

The work of the National Bureau of Economic Research (NBER) on the measurement of national income in the early 1920s provided a second approach to measuring economic growth. As is well known, the plan pursued by the NBER economists was to construct two alternative but theoretically equivalent annual measures of the national income, one based on statistics of incomes received by individuals and one based on statistics of the value of goods and services produced. In their initial report (Mitchell et al., 1921) the researchers constructed the two measures for each year from 1909 to 1919, expressing them in current year dollars. Both series were also converted into constant dollar terms using a process that divided each current dollar series into subcomponents, and deflated each subcomponent with a different price index (e.g., incomes received by employees were deflated using the BLS cost of living index, while incomes received as interest, dividends, rents, royalties, and profits were deflated using a special cost of living index based on the consumption habits of more affluent households). The two deflated series were averaged to create a third "final" series of what was deemed "real" national income, which was represented as a measure of "the serviceable goods available for use by the population", and "the aggregate of commodities and services which the current money income would buy", that is, a measure of total output (Mitchell et al. 1921, vol. 1, pp. 73, 75). To further underscore the point that real national income represented a measure of the quantity of output, these real income series were compared to four of the then-extent indexes of physical production, including the Day-Persons Index. The NBER "final" real income measure showed an annual growth rate of 2.5% over the years 1910-1918, following, in the words of the NBER researchers, "an intermediate course through the field covered by the fluctuations of the physical production index numbers." (Mitchell et al. 1921, vol. 1, p 80).

Measures of output and growth derived from national income estimates eventually came to supplant those based on physical indexes of production, at least for nations or broad aggregations of industries, but during the 1920s there was not yet consensus upon which measurement approach was superior. The statistical materials on which the real national income measures were based were more extensive and covered a broader range of economic activities, as they could include both series expressed in terms of physical units and those measured in monetary terms. However, the early national income accountants often encountered time periods and industries for which adequate data were not available, and like those constructing the physical indexes, resorted to extrapolation and application of the rule of three to fill in the gaps. More importantly, the fact that each year's national income figure had to be deflated in order to measure changes over time in output gave rise to an additional set of reasons to doubt the accuracy of the "real" national income estimates, as a number of challengeable assumptions and estimation procedures went into the construction of any given price index.

The NBER's first national income study was well enough received, however, that the Bureau continued to produce and refine annual national income estimates throughout the 1920s, and in 1929 another comparison between a real national income measure and a physical index of production appeared in a chapter by Morris Copeland in the NBER's volume on *Recent Economic Changes*.[3] Copeland chose to work with what he called realized income, a national

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[3] *Recent Economic Changes* was the outcome of an extensive research effort to provide a broad-based account of the US economy's development since the world war, with an eye towards understanding the causes and consequences of the major trends thus revealed. The project was commissioned by Herbert Hoover while he was secretary of commerce and coordinated by the NBER , and reflected Hoover's belief (shared by NBER founder Wesley Mitchell) that a key

income measure based on money incomes received, but made it clear that the ultimate goal driving his decisions about what to count and how to count it was the measurement of "production of goods and services or our capacity to produce them." In deflating his realized income series into constant dollar terms Copeland used the NBER approach described above, and he compared the resulting series to a production index similar in coverage to the Day-Persons index. He was disturbed by some obvious divergences between the two series' verdicts regarding year to year changes in output, which is understandable given that the accurate documentation of cyclical fluctuations was a major purpose behind the development of both measures. Copeland noted as a potential weakness of the physical index of production that the industries it represented, being limited to those engaged in commodity production, generated only about a third of the nation's income. The national income measure had a serious weakness as well, however, as "Deflation is a process that requires cautious use . . .(and) the method of deflation employed . . . is not well adapted to the particular purpose of comparing income with output" (Copeland 1929, fn. 8). When it came to longer term economic growth, however, the two series were in remarkable agreement, both indicating that between 1913 and 1926 there had been an increase of about 42% in the nation's output of goods and services.

Studies of Labor Productivity

As time series measures of output were being developed and refined in the 1920s, they became inputs into measures of labor productivity. Woirol (2006) documents this first major

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element of sound governmental economic policy was the acquisition and dissemination of objective knowledge of the economic system to both private and public sector decision makers. See Barber (1985).

productivity research program in the US. He argues convincingly that the most important immediate consequence of the early attempts to measure labor productivity was a concern that rising labor productivity was generating technological unemployment, and that this concern provided a major motivation for pushing the research program into the depression years of the 1930s. However, it is also clear that many of those who wrote and responded to the early labor productivity studies viewed them as a first step towards understanding the causes of the economic growth revealed in the new output measures.

The basic approach to constructing a measure of changes in labor productivity was straightforward. One began with a measure of output at the level of an industry, sector, or economy, expressed in an index form. One then constructed an index number measuring the growth of the labor input, either hours of work or number of employees. For example, in the May 1927 issue of the *Monthly Labor Review*, Bureau of Labor Statistics (BLS) researchers reported that an output index for manufacturing had increased from 100 to 134 between 1919 and 1925, while an index of wage earners employed in manufacturing had declined from 100 to 93.3 over the same period, indicating a growth in labor productivity, measured as output per wage earner, of 43.6%. (US BLS 1927, Woirol 2006). As Woirol describes, a research team headed by Ewan Clague produced a number of influential labor productivity studies in 1926 and 1927, releasing numbers first for individual industries like shoes and automobiles before providing the measure for manufacturing as a whole. Woodlief Thomas produced an even more broadly based measure of productivity growth in 1927, comparing indexes of physical production to indexes of numbers of employees (wage earners and salaried workers) to create measures of labor productivity growth from 1899 to 1925 in manufacturing, agriculture, railways, and mining, as well as a combined measure for all four sectors (Thomas 1927). It is worth noting, however, that while the national income researchers were willing to use real national income per capita as a measure of national "prosperity", neither they nor other economists were yet willing to use an index of real national income as a component in constructing a measure of overall productivity growth, in part due to a lack of reliable annual data on total national employment.

The productivity studies led to broad consensus on one point: labor productivity, whether measured per wage earner, employee, or man hour, was growing dramatically. Put another way, the growth in output revealed by the physical indexes and the real national income was largely due to something other than growth in the labor force. Further, the growth in productivity had been particularly rapid since the end of the war. There was some disagreement over whether the post-war growth was unprecedentedly rapid: productivity certainly increased more quickly in the decade after the war than the decade before, but the always cautious Wesley Mitchell warned that the sensitivity of the growth rate calculations for the 1920s to the choice of base year and the paucity of data from the last decades of the 1800s made it difficult to compare the records of those two periods (Mitchell 1929).

A number of economists proposed explanations for the dramatic increases in productivity, typically in the form of lists of possible causes. The lists often overlapped, but all gave a place of prominence to technological change, or, more accurately, to factors that would later come to be understood as falling under the broad rubric of technological change. In a 1927 article commenting on the productivity studies, Paul Douglas cited three main causes of the measured productivity increases: increased quantities of capital per worker, "the rapid development of American technical methods, including as its most notable feature, the moving conveyor", and economies of scale both internal and external to firms, made possible by the nation's "large internal market in which free trade prevails." Interestingly, Douglas listed as a fourth factor the inability of unorganized workers to resist the introduction of new production methods. As to why productivity growth had markedly accelerated after the war, even though the factors he cited had been operative for some time before, Douglas pointed to changes in the willingness of managers to seek out and employ new and more productive techniques, a change driven, Douglas argued, by the restrictions on immigration and the increase in the real wages of workers, but also by “fifteen years of discussion of scientific management and of efficiency.” Prohibition, Douglas noted, was another recent change to consider in seeking for explanations. (Douglas 1927, pp. 671, 676)

Woodlief Thomas thought it obvious that the main causes of productivity growth were the growing use of mass production, increased mechanization, and scale economies achieved through industrial consolidation. He was also willing to give credit to improved education and literacy, the organization of scientific research in universities, and the increasing use of statistics by businessmen. He, like Douglas, sought particular explanations for the post-war surge in productivity growth, finding them in the war experience, which accelerated the discovery and implementation of new technologies, as well as the “easy availability of new investment funds,” which led to the replacement of old capital equipment with new and better capital equipment.

Wesley Mitchell, writing the summary chapter of the *Recent Economic Changes* volume, sought to place the postwar economic trends in the US in a larger historical context. When it came to explaining economic growth, he too placed technological change at the center of his account, but portrayed the innovations that had revolutionized methods of production and distribution in the developed economies as a product of a more fundamental social trend, speaking of “the purposeful application of scientific knowledge to all aspects of production.” In his view, the postwar spurt in US labor productivity was due mainly to an intensification of this long standing tendency: “Since 1921, Americans have applied intelligence to the days’ work more effectively than ever before. Thus the prime factor in producing the extraordinary changes in the fortunes of the European peoples during the 19th century is the prime factor in producing prosperity in the United States in recent years. The old process of putting science into industry has been followed more intensively than before; it has been supplemented by tentative efforts to put science into business management, trade-union policy, and Government administration.” (Mitchell 1929, pp. 844, 862).

It is not surprising that along with lists of possible causes of increasing labor productivity came attempts to develop a more empirically-based understanding of them, if not quantify their relative importance. One approach to quantifying the importance of those causes that could be at least approximately measured was an extension of the index number method used to originally ascertain the extent of labor productivity growth. Thomas (1928), for example, presented alongside of his indexes of output and labor input an index of “horsepower of installed prime movers”, taken from the censuses of manufacturing. This index number had soared from 100 to 356 while the output per worker index had grown from 100 to 147. L.P. Alford, in his *Recent Economic Changes* chapter on “Technical Changes in Manufacturing Industries”, took this approach further, adding to the conventional indexes of output, labor, and output per worker indexes of a number of other factors potentially related in some way to productivity, including the value of manufacturing buildings and of manufacturing machinery (taken from the original Cobb-Douglas paper), the cost of materials, and the average wage rate. Alford showed, graphically and in tables, how the growth paths and growth rates of these various factors compared with those of output per worker, but it was not made clear what the reader was to take from these comparisons: While dividing output measures by measures of labor input (or comparing the two indexes) led to a quantity of obvious significance, it seemed less obvious how these other series should be compared or processed to gain more insight into the sources of productivity growth.

Alford’s chapter also reported large amounts of information, obtained via questionnaires and on-site investigations, on where and how technological change had been introduced into American manufacturing, making it an illustration of another strategy adopted during the period for gaining a more empirically grounded understanding of the nature and consequences of technical innovation. A survey by the National Research Council, for example, had yielded information from over 500 large firms on their organized, in-house “industrial research” activities. The Census of Manufactures provided detailed information on the types and sizes of non-human power sources used in the nation’s manufacturing plants. The American Engineering Council and Traveler’s Insurance had conducted investigations of safety procedures. Alford himself had sent questionnaires to firms asking about new processes being used, new materials and products being put on the market, and the installation of new manufacturing machines and materials-handling equipment. Results from these and other information gathering efforts were presented by Alford in numerous tables that revealed the incredible variety of cost-saving innovations recently developed or adopted in US manufacturing: Dewey and Almey Chemical had created a new latex shoe cement and a new compound for sealing cans. American Linseed had installed a new process for pressing linseed that saved four cents per bushel. The number of varieties of asphalt produced by the asphalt industry had been reduced, by agreement among producers, from 102 to 10. Manufacturers had begun separating aluminum scrap from other scrap and selling it separately at a higher price. Freeman Dairy Co. installed a coal conveyer and saved $2500 per year. There were literally hundreds of examples like this going on for page after page.

The productivity research program at the BLS also included the collection of detailed information on recent changes in the production methods and processes of various industries under study. In most cases, this information was presented in narrative form, often with photographs, in bulletins and articles such as “Technological Changes in the Cigar Industry and their Effects on Labor” (U.S. BLS 1931). In a few cases, however, the BLS economists attempted to use this sort of detailed descriptive information on specific innovations as a basis for a statistical analysis of the relationship between technological change and labor productivity, at least at the level of individual industries. The study of “Productivity of Labor in Merchant Blast Furnaces” provides a prime example (U.S. BLS 1929). It was based on records from a large number of plants covering the years 1912-1926, with many plants reporting for multiple years. A plant’s productivity was measured in terms of tons per stack (i.e., furnace) per day and output per man-hour. The researchers used their detailed knowledge of the history of and production processes involved in the blast furnace industry to construct meaningful empirical measures of technological and other changes that might have affected productivity, then compared the time series of these measures to the productivity time series. For example, they identified the two most important technological innovations of the period, the mechanization of the process of feeding material into the furnace, and the “machine casting” of the iron coming out of the furnace, then constructed a series of the percentage of plants each year that had adopted each of these innovations. And, after a description of the relationship between the volume of a stack and its productivity, a cross tabulation of stack volume in cubic feet and output of tons per cubic feet was presented, showing that volume affected daily output per cubic foot, but that other factors were clearly also at work. A discussion of the regional distribution of the sample plants and the changing advantages offered by various regions over time accompanied a table of average productivity by region, and provided evidence on the extent to which rising levels of industry productivity had resulted from the changing regional distribution of plants. A look at data from plants that had switched from two twelve hours shifts to three eight hour shifts provided evidence that labor productivity was much higher when the latter system was used. On the whole, the study was a very impressive example of statistical analysis, particularly when viewed in the context of the empirical economics literature of time.

There were other BLS efforts to measure the impact of specific new technologies on an industry's productivity, including one which measured and compared the productivity and unit cost figures for a number of old and new technological methods that still coexisted in the glass industry (Stern, 1927) and another in which similar comparisons were made between past and present processes for printing newspapers using the historical production records of a variety of firms (Kjaer, 1929). The BLS decision to focus on explaining productivity growth at the level of particular industries allowed the researchers to identify a small number of key innovations and develop an understanding of the channels through which these innovations could influence a firm's output and demand for labor. This knowledge could then be used to craft sensible empirical strategies for measuring the impact of the innovations on measured productivity. This is in contrast to the situation faced by Alford, who had on the one hand aggregate productivity measures from the manufacturing sector as a whole, and on the other descriptions of a multitude of innovations from the whole range of manufacturing industries. The task of developing useful aggregated measures of technological change in manufacturing out of the latter body of information certainly must have seemed daunting.

Harry Jerome posed the problem thusly in his 1934 book *The Mechanization of Industry*: "Can we state this tendency towards mechanization in a more generalized form than by describing the development peculiar to each industry, and in a form reasonably comparable from industry to industry and from period to period? . . . Can we measure it or at least delineate its main lines of advance?" (Jerome 1934, p. 205) Jerome's book was concerned with a broad subcategory of innovations which he described as "power mechanization" or "the increasing reliance on equipment driven by generated power, be it steam, electricity, compressed air, or gasoline that furnishes the motive power (Jerome 1934, p. 41)". Like Alford and the BLS researchers, Jerome provided extensive descriptions of a broad range of specific innovations, accounts based on fieldwork and interviews with businessmen. But he also proposed a number of general measures of mechanization that might be used to make comparisons across industries in the level or growth of mechanization, or quantify the increase over time in overall mechanization, including horsepower per worker, proportion of establishments with power, employment in the machine producing sector, and the ratio of wages to value added in manufacturing. Jerome's chief purpose in developing these general indexes of mechanization, along with measures of mechanization specific to particular industries, was to explore more fully the impact of mechanization on unemployment, a matter which by this time, as noted earlier, had become a major focus of productivity research. Although he granted as a matter of course that mechanization had also increased productivity, he was not interested in quantifying that relationship.

Douglas's Time Series Studies and the Problem of Technological Improvement

"A Theory of Production"

In 1926 and 1927, Paul Douglas was at work on his project of compiling and analyzing vast amounts of labor market data, work that would eventuate in *The Theory of Wages* and *Real Wages in the United States, 1890-1926*. The emerging evidence regarding recent growth in output and in labor productivity since the war did not fail to capture his attention, prompting him to mention the increase in production as a possible cause of rising real wages in Douglas (1926), and to speculate at length on the sources of rising labor productivity in the 1927 article discussed above. Understanding the causes of the growth in output and in labor productivity was also on his mind as he contemplated the significance of the statistical procedure that he worked out with his friend Charles Cobb. This is clear from the opening lines of "A Theory of Production", in which the first of his list of questions raised by the new measures of the growth in manufacturing output was "Can we estimate, within limits, whether this increase in production was purely fortuitous, whether it was primarily caused by technique, and the degree, if any, to which it responded to changes in the quantity of labor and capital?" (Cobb and Douglas 1928, p. 139).

Douglas believed that his construction of measures of "changes in the quantity of labor and capital" employed in manufacturing from 1899 to 1922 represented an important step towards providing an affirmative answer to this question, particularly the index of physical capital, which may have been the first such time series constructed by an economist. The raw material for Douglas's capital index came from the US Censuses of Manufactures, which periodically reported the dollar value of various categories of "capital" employed in manufacturing. For four of the years in Douglas's period, dollar values of two types of capital, factory buildings and “machinery, implements, and equipment” were broken out from a “total capital” category that also included the value of items like raw materials, goods in progress, and unsold goods in warehouses. Douglas argued that the two categories represented “fixed capital”, which was the appropriate capital concept for his purposes, as it was the type of capital that “aids in the production of goods.” He then used the share of total capital represented by these categories in the census years for which they were reported to estimate trends in those shares, and used the estimated trends to assign values for the two fixed capital categories for census years in which they were not separately reported. A tougher problem was created by the fact that dollar values of capital reported to the census probably (according to an expert Douglas queried at the Census Bureau) represented original cost rather than current value, requiring Douglas to (i) estimate what proportion of a given year’s fixed capital stock had been added in each of a number of preceding years using an index of production of basic commodities (e.g., pig iron and coke) and (ii) reduce each year’s estimated increment to the value of the fixed capital stock to a constant dollar value, using a capital price index based on a money wage index and price indexes for metals and building materials. This brief description of Douglas’s method of producing his capital series considerably under-represents its complexity and the number of heroic assumptions upon which the series rested, but should help to indicate the large number of potential points of contention it presented to any critic of Douglas’s work. Douglas’s construction of an index of wage earners employed in manufacturing, while still requiring interpolation between census years and various applications of the rule of three, was more straightforward.

The Cobb-Douglas equation of 1928, $P = bL^{k}C^{1-k}$, provided a vehicle for using these input indexes to determine statistically how output (as measured by the Day-Persons index of manufacturing production) responded to changes in the quantity of labor and capital, but no means of attributing some of the growth of output to changes in “technique.” If Douglas considered this a major problem in 1927, however, it is not apparent from “A Theory of Production”, in which the issue is mentioned only briefly in the conclusion. There, Douglas suggests that future research should including “divis(ing) formulas which . . . will eliminate so far as possible the time element from the process”, a remark that does not speak to the potential goal of measuring the extent to which growth in output is due to “technique”, but only to the worry that the historical reality of technical change might bias the estimated marginal productivities of capital and labor (Cobb and Douglas 1928, p. 165). However, the fact that the Cobb-Douglas procedure did not measure or account for technical progress did bother a number of economists who commented on the original Cobb-Douglas study over the next several years.

Sumner Slichter was the discussant of the Cobb-Douglas paper at the 1927 AEA meetings, and a very critical discussant at that. He had numerous complaints about Douglas’s index of capital, showing in particular that it was very sensitive to the base year chosen for the deflating process. Indeed, he seemed dubious of the very idea of measuring the quantity of capital, quipping that “This peculiar substance, abstract physical capital, which changes in relative as well as absolute magnitude as one applies now this and now that deflation factor, appears to be the stuff that dreams are made of.” (Slichter 1928, p. 167). Slichter pointed to ways in which the empirical strategy adopted by Cobb and Douglas was ill-suited to the theoretical framework (marginal productivity theory) underlying their project, and that, as a general matter, this theoretical framework, being static in conception, was ill-suited to the study of real economic activity. The contrast between the static assumptions embodied in the regression equation and the reality of technical change meant that the regression’s accuracy was at best “ephemeral”, and that Cobb and Douglas’s results were unlikely to hold for the twenties, when labor productivity had increased rapidly. In fact, he noted, the largest deviation between the output predicted by the equation and actual output was for 1922, the last year of Cobb and Douglas's sample. Nor did the results shed light on questions of distribution, as "the same technical innovations that destroy the accuracy of the equation also create opportunities for bargaining by causing the joint output of productive instruments to be worth more than the sum of their prices in the open market." (Slichter 1928, p. 170)

A few months after the appearance of "A Theory of Production", J.M. Clark published an article devoted solely to discussing issues raised by the Cobb-Douglas paper (Clark 1928). His criticisms were numerous, but most were constructive, aimed at improving the Cobb-Douglas analysis of marginal productivity rather than discrediting it. Overall, he found the Cobb-Douglas study to be "a bold and significant piece of pioneer work in a hitherto neglected field." (Clark 1928, p. 467) However, he was troubled by the inability of the procedure to account for any impact of technical change on the quantity of output:

> One of the striking things in this study as presented is the fact that it seems to allow no room for the natural effect of advances in the "state of the arts." To one accustomed to crediting our increase in per capita output to the triumphs of inventive genius, it must be a rude shock to see the whole increase calmly attributed to increased capital . . .. What then has become of our boasted progress? Has it totally evaporated? This question places the student in an interesting dilemma. For the purposes of isolating the marginal yields of the factors of production, we should like to get rid of it, or at least isolate it. But this scientific interest is hardly enough to make us glad to see progress eliminated from real life. (Clark, 1928, pp. 463-4)

Like Douglas in the conclusion to the Cobb-Douglas paper, Clark understood that technological progress hindered the regression procedure's ability to accurately measure the marginal productivities of labor and capital, and should ideally be "gotten rid of", at least in a statistical sense. To this end, he suggested "comparative studies of the simultaneous costs of different methods of production in representative industries," an idea similar in spirit the statistical strategy later adopted by Douglas of estimating the regression on cross section industry level samples. But like Douglas at the beginning of the Cobb-Douglas paper, he also saw at least the possibility that the procedure introduced by Cobb and Douglas could be a part of a larger effort to estimate the impact of technical progress on economic growth, and he offered some calculations to suggest how this effort might proceed (Clark 1928, p. 467).

Clark began by explaining that some of what was estimated by the Cobb-Douglas regression as the marginal product of capital was actually a result of technological improvement, and properly so: The growth of capital necessarily involved putting that capital into new forms and adopting new production processes, while the adoption of new processes and forms of capital necessarily involved a growth of the quantity of capital. This was an argument that had also been made by Clark's father, John Bates Clark. When the elder Clark, in his *Distribution of Wealth*, had presented a graph illustrating the diminishing marginal product of capital, he had commented that the additions to the fund of capital represented by movements along the horizontal axis were "mainly new qualities infused into the capital-goods already in the working outfit. If we were to try the experiment of making a capital grow from a small beginning to the size which, in view of the amount of labor that was to use it, it was naturally to take, we should need to have a magical power of transforming and improving every instrument of production; and we should have to exercise this power with every addition that we might make to the productive fund", and later that "More capital, as has been shown, means better capital goods." (Clark, 1908 [1899], chapt. 18, paragraphs 1, 16).[4] The younger Clark now explained that at any given time, there was a "frontier" of known but not yet economical new devices which will come into use if "relative costs should become more favorable". A very rapid growth of the quantity of capital, such as that shown by Douglas's figures, will promote a vigorous search for still more new devices and processes that will be profitable given changing relative factor prices. So, "the product attributable to added capital is also attributable to progress. Both are necessary to it" (Clark 1928, p. 464). However, Clark warned, while many innovations are marginal in this sense,

> others are extremely profitable, creating uses for capital well above the existing margin. To the extent that this happens, the historical increase in aggregate product is not an accurate index of the marginal product of capital either before or after the introduction of the improvement. The change makes the marginal product higher than it would otherwise have been, and also adds to the supramarginal product; and the mass statistics we are studying cannot segregate these two components. . . .new

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[4] J. B. Clark's drawing of a marginal product of capital curve that assumes improving technology as the quantity of capital grows may seem to contradict his use of an assumed "static society" , in which economic activity takes place but technology (among other things) does not change, to elucidate how marginal productivity determines the levels of wages and interest (e.g., Clark 1908, Chapt. 5, par. 16). However, the contradiction is resolved if one assumes that Clark's graph is an exercise in comparative static analysis, in which each level of the quantity of capital is associated with an equilibrium, and equilibrium states with more capital are also necessarily equilibrium states with better capital.

> supramarginal uses of capital may perhaps be called "pure progress", to distinguish them from those whose effects are merged into the marginal yield of added capital. (Clark 1928, p. 465)

It was this "pure progress", then, that led to bias in Cobb and Douglas's estimates of the marginal products of capital and labor, and the measurement of the impact of this pure progress on growth required one to go beyond the Cobb-Douglas procedure.

Clark next argued that if one omitted the "abnormal" years of 1917-1919 (years in which the fit of Cobb and Douglas's regression was particularly bad), and made a reasonable adjustment for the fact that the length of the average work week declined over Cobb and Douglas's period (recall that their labor input was measured as number of workers, not hours of work) the estimated Cobb-Douglas production function would be $C^{.4}L^{.6}$. This improved estimate still implied a marginal product of capital that was too high, "suggesting strongly that capital is getting credit properly due to some other agency." Hazarding a guess that the cost of fixed capital to the enterprise was less than 25% of the total value added to materials by the enterprise, and reasoning that this capital must produce slightly more than it costs, Clark proposed that a production function representing the actual marginal products of capital and labor would be $C^{1/4}L^{3/4}$. Over the period covered by the Cobb-Douglas data, this production function would have implied a growth in manufacturing output 15% lower than the growth generated by the estimated production function $C^{.4}L^{.6}$. This 15% difference, Clark concluded, was "one out of many possible estimates" of the effect of pure progress over the period.[5]

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[5] It was unclear how Clark arrived at some of the components that went into this calculation, such as the cost of capital being 25% of the net income of the enterprise, and he frankly admitted

Importantly, Clark explicitly noted one of the key steps involved in producing his estimate of the effect of technological progress: in order to produce a production function formula that reflected the actual marginal product of capital and labor, he had to assume the correctness of the marginal productivity theory. As he phrased it,

> In coming to this conclusion we have, it will be noted, shifted from the statistical gauge of marginal productivity, in order to have some basis for judging the plausibility of the results; and have fallen back on the tautological device of judging the marginal contribution of the factors by what they receive, or by what they cost. In other words, to the extent that we attempt to take account of the disturbing factor of progress, the index of marginal productivity derived from the historical trends of mass statistics fails us. (Clark 1928, p. 467)

Henry Schultz (1929) also offered an assessment of the Cobb-Douglas procedure at the end of a long article explicating the Walrasian version of the marginal productivity theory. While not explicitly mentioning technological change or economic growth, he argued that the procedure could not be used to verify the marginal productivity theory or measure its relationships because that theory was static, while the statistical data employed by Cobb and Douglas reflected secular changes. However, Schultz pointed out, there were numerous statistical techniques available for adjusting time series for secular change. Many, such as the use of trend ratios and first differences, had been employed by researchers, like Schultz himself, engaged in

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the speculative nature of others, which is presumably why he spoke of the result as “one of many possible estimates.”

the empirical estimation of the supply and demand curves of static theory. And Schultz noted approvingly Douglas's announced intention to explore approaches that would "eliminate the time element" from the data. But Schultz also saw possibilities for using the procedure as a tool for the empirical analysis of long run growth, describing the potential interest in comparative estimates of the "long run productivity curves" of various nations. The Cobb-Douglas procedure would have to be modified for this, and considerable experimentation with old and new statistical techniques for separating short term from long term movements would doubtless be necessary, but the procedure should neither be abandoned nor ignored simply because, in its initial version, it failed to deal properly with secular trends (Schultz 1929, p. 541).

The Theory of Wages

In 1934 Douglas published *The Theory of Wages*, which included as a central feature a description of the statistical data, methods and results from "A Theory of Production," accompanied by results of estimating the Cobb-Douglas regression with time series data from Massachusetts and New South Wales. Whereas the marginal productivity theory had not been explicitly mentioned in "A Theory of Production", the empirical analyses in *The Theory of Wages* were 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 Theory of Wages* also included Douglas's responses to concerns of Clark, Schultz, and others about the implications of secular growth and technological progress for the validity of the estimates produced by the regression.

In the discussion of the results of the original Cobb-Douglas study, Douglas inserted an account of a procedure implemented by two former students to "meet the objection that the time factor has not been eliminated". In the spirit of Schultz's suggestions, they had estimated linear trends for the labor series, the output series, and the (logged) capital series, then used those estimated trends to express the original values of the series as ratios to trend. When the Cobb-Douglas regression was estimated using these trend ratios in place of the original values for the series, the coefficient for labor was .84, "only nine points or 12 per cent" more than the estimate from the original study. Douglas had little to say about what conclusions one might draw from this finding, however (Douglas 1934, pp. 143-44).

Douglas also included a section entitled "Progress and the Equation of Production", in which he grappled with the implications of technological progress for his new empirical procedure, noting that "One of the disconcerting features of the analyses of production . . . in the previous chapters is that it (sic) seems to eliminate "progress" or dynamic improvements in the quality of capital, labor, and the industrial arts from the industrial history of the periods studied" (Douglas 1934, p. 209). Douglas began by explaining how his procedure actually represented a move forward in attempts to understand economic progress. In the past, he noted, some had viewed the increase of total production as a measure of progress, but this was clearly wrong, as it ignored the fact that rising output accompanied by rising population could mean that labor productivity and average consumption were both declining. Output per worker, the measure of progress most commonly used by modern economists, was also flawed in that it could increase solely because of increases in the quantity of capital per worker with no change in "technical efficiency". The Cobb-Douglas procedure corrected this flaw by explicitly taking into account the quantity of capital as well as the quantity of labor.

However, in each set of data to which the procedure was actually applied, the whole of the increase in total production over the sample period was accounted for, with a seemingly high level of accuracy, by mere quantitative increases in labor and capital. In the face of the obvious revolution in manufacturing technique in these periods, such a conclusion was “incredible”; it was a paradox demanding a reconciliation between “the reality of qualitative progress and the validity of the formula” (Douglas 1934, pp. 210-211).

Douglas admitted that he had no such reconciliation to offer, only some suggestions on this “tangled question.” He proposed that when the equation was estimated for a particular time period, one might suspect the existence of technical progress in shorter periods within or adjacent to that period during which the growth of output exceeded what one would predict using the estimated equation, or which led to different estimated coefficients for the equation than those produced by the entire period. Douglas pointed to the period 1921-26 as one that showed progress by this metric. Douglas also suggested that the some of the progress in the US manufacturing from 1899 to 1922 was “concealed in and made possible” the reduction in the average work week and the falling ratio of production to non-production workers. Further, Douglas argued, there was reason to believe that quality of the average worker had been increasing along with the quality of capital, and he cited with approval and quoted at length J.M. Clark’s argument that part of the estimated productivity of capital was due to the improved quality of capital. Douglas did not, however, develop the statistical implications of possible improvements in the quality of labor and capital for his method of estimating the marginal productivity of these two factors, beyond saying that if the qualitative improvement of workers balanced the qualitative improvement of capital, progress could have affected the size of the total product without being reflected in his marginal productivity estimates.

Douglas reported having had useful conversations on the topic of technical progress with William Ogburn and S.C Gilfillan, two colleagues with demonstrated expertise on the subject.[6] Douglas was particularly interested in Gilfillan's classification of the 120 inventions of the last generation "with the most important social effects" into the categories of labor saving, land saving, capital saving, and developments of consumer goods (Douglas 1934, p. 214; Gilfillan 1932). Douglas noted the ratio of capital and land saving inventions to labor saving inventions (1 to 1.5), and related Gilfillan's opinion, conveyed in an unpublished communication to Douglas, that while labor saving inventions tend to raise the capital labor ratio, and capital saving inventions to lower it, the second effect is offset somewhat by the necessary investment in new types of capital and the reduced need for labor to operate the reduced quantity of capital. In reading these passages one senses that Douglas believed that these observations were very relevant to the question of how technological change affected the meaning of the estimates produced by his regression, but also understood that he was not yet seeing all the necessary connections.

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[6] Ogburn, a prominent sociologist, was on the University of Chicago faculty and was known for his work on the social impact of technological change. Sociologist S. Colum Gilfillan also wrote copiously on the topic of inventions, spent much of his career in the Chicago area, and was in the early 1930s working for the President's Research Committee on Social Trends. This committee, established by President Hoover in service to the same general vision that motivated the *Recent Economic Changes* project, produced a volume entitled *Recent Social Trends in the United States* (1933), with a chapter coauthored by Ogburn and Gilfillan and entitled "The Influence of Invention and Discovery".

Another communication received by Douglas bearing on the relationship between his regression and technological change came from Morris Copeland, in which Copeland reported the results of fitting a straight line trend to the logarithm of Douglas's output per worker series. The resulting regression predicted actual output just as well as the Cobb-Douglas regression, leading Copeland to conclude that the hypothesis that all the growth in labor productivity was due to technological change was as firmly supported by Douglas's data as the hypothesis that it was all due to a growing quantity of capital. This finding clearly troubled Douglas[7], as it would others who later encountered it in *The Theory of Wages*, and Douglas closed his section on "Progress and the Equation of Production" by admitting that "the whole question needs to be gone into more thoroughly" (Douglas 1934, p. 215).

Douglas's *The Theory of Wages* gave rise to four article-length reactions from young, mathematically oriented neoclassical economists who were interested in the relationship between Douglas's estimates and the equations of their theoretical systems. Indeed, it was in these articles that the term "production function" was first consistently applied to the relationship Douglas was attempting to estimate, although Douglas quickly adopted the phrase himself. Jacob Marschak (1936) and Wassily Leontief (1934), though each critical of certain aspects of Douglas's effort, obviously believed that Douglas's research represented an admirable first step towards a very important goal, while Horst Mendershausen (1938) saw little of value in what Douglas had done. All three made some reference to the problem of technological progress, but viewed it as something that had to be statistically eliminated if the regression were to produce reliable

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[7] He spoke of Copeland's "weighty criticisms of the theory and the significance of our results that should be recognized" (Douglas 1934, p. 215).

estimates of marginal productivities, rather than something that the technique, with modifications, could potentially measure.

David Durand's (1937) attitude towards Douglas's regression was rather different. After identifying marginal productivity theory with Walrasian general equilibrium theory via a mathematical exposition of equilibrium of the production side of a Walrasian economy, Durand explained that a time series regression like that of Douglas's really could never either test the marginal productivity theory nor measure its fundamental relationships. The marginal productivity theory was a static theory that applied only to individual firms, while the Douglas's data was aggregated across all manufacturing firms and reflected the action of dynamic forces. (Durand, 1937, p. 745-46). However, Durand was in favor of the idea of inductive "productivity studies" like those conducted by Douglas, although he found many problems with the details of Douglas's analysis and allowed the possibility that they and other problems facing inductive productivity researchers might prove insurmountable. While productivity studies like Douglas's said nothing about the marginal productivity theory, they could potentially reveal something important about long run growth, including "how much industrial progress is due to innovations rather than increases in labor and capital" (Durand 1937, pp. 748ff, 752).

It was in connection with understanding the impact of innovations on growth that Durand suggested that Douglas relax the assumption that the production function was linearly homogeneous by estimating the coefficients of labor and capital separately rather than restricting them to sum to one. Having earlier explained that constant returns at the level of industry as a whole was something entirely different in both meaning and implications from constant returns at the firm level, he asserted that "the number and productivity of new inventions" would determine whether industry as a whole (and implicitly, over time) displayed decreasing, constant, or increasing returns. (Durand, 1937, pp. 747, 749, 755) Thus, the sum of Douglas's two coefficients on capital and labor might in some way help to measure the contribution of technological change to growth. In his subsequent work with the function, Douglas adopted (with attribution) Durand's suggestion, separately estimating the coefficients of capital and labor and pointing to their sum when discussing his results. However, while linking this sum to the idea of returns to scale, Douglas gave no indication that he thought its value indicated anything about technological progress.

Indeed, after *The Theory of Wages*, Douglas put on the back burner the idea that his regression might aid in measuring the extent to which growth in output was "caused by technique," focusing almost completely on its potential for measuring the marginal products of labor and capital and testing the theory of marginal productivity. In Handsaker and Douglas (1937, 1938), his final application of the Cobb-Douglas regression to time series data, he was explicit about the need to assume that "changes in production were the result of changes in the quantities of labor and capital alone, abstracting from changes in the skill of the workers and technique" if one were to interpret the estimated exponents as measures of marginal productivity (Handsaker and Douglas 1938, p. 215). He responded to a number of the criticisms of his earlier work, already described, but was silent regarding those related to technological progress. Bronfenbrenner and Douglas (1939) and Gunn and Douglas (1940) introduced a new 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 or group of related industries. All of his subsequent production studies used this type of data. As Bronfenbrenner and Douglas noted (1939, pp 778-79), unlike the results of his time series studies, the good fits achieved by his cross section studies could not be claimed to reflect the impact of technological change on both the growth of capital and the growth of output. However, Douglas's permanent move to cross section data meant abandoning efforts to introduce modifications into his procedure that would allow it to measure the impact of technical change on growth.

Analytical Frameworks for the Empirical Analysis of Growth in the 1920s and 1930s.

As was described in earlier sections, the development in the 1920s of empirical measures of the growth of output and of labor productivity quickly led to attempts by economists to identify the causes of that growth, and to ascertain empirically the relative importance of those causes. It should also be clear from the earlier discussion that there was no widely accepted, well articulated analytical framework upon which the economists based those efforts. The existence of a number of less than fully developed and sometimes conflicting sets of concepts, hypotheses, and categories for thinking about economic growth is reflected in Douglas's attempts to analyze the phenomenon. For example, early in *The Theory of Wages* Douglas reiterated the basic framework for thinking about the causes of growth initially outlined in the introduction to "A Theory of Production", decomposing the growth in output into that which was due to increases in the quantities of the factors of production, and that which was due to changes in technical knowledge (Douglas 1934, 18-19). This decomposition lined up with his statistical approach of constructing index numbers of the growth of labor and capital and using them to account for the growth in a production index, but had roots classical economics, as in J.S. Mills'(1987 [1848]) chapter on the "Influence of the Progress of Industry and Population on Rents, Profits, and Wages" in which he traced the impact on distributional shares of increases in capital, labor, and "the arts of production", each considered separately while assuming the others constant.

However, as discussed above, later in *The Theory of Wages* Douglas also endorsed the argument of J.M. Clark that the quantitative accumulation of capital necessarily involved qualitative change in capital as well, i.e. technological change, although that idea was incompatible with Douglas's earlier decomposition scheme. He also departed from the basic decomposition framework by acknowledging the possibility that an improvement in the quality of the labor force contributed to growth (Douglas 1934, pp, 212-213). This position, shared by a number of others at the time (including Woodlief Thomas, as noted above) suggested that an attempt to statistically account for the causes of growth should include investigations of trends that might increase worker quality in general, such as improvements in education or nutrition, or even prohibition.[8] In addition, Douglas had argued in 1927 that one of the major causes of increasing productivity (along with increasing capital per worker and improved technology) was the exploitation of economies of scale by producers serving the growing US market. However, in the *Theory of Wages* Douglas asserted, citing the authority of Wicksell, that the relationship between inputs and outputs should by logically necessity be characterized by constant returns to scale,

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[8] The idea that the growth of capital necessarily involved changes in technique could also be found in the classical canon. In Smith's account of growth technological change could be seen as a passive factor that occurred almost automatically as capital accumulation facilitated specialization and the division of labor; John Rae offered an alternative account that made technological change the active factor creating an impetus for capital accumulation, with the growing quantity of capital embodied in increasingly sophisticated and productive implements. (Brewer 1991). The proposition that improved worker quality led to higher labor productivity was present in Mill, more prominent in Marshall, and perhaps gained an added boost in the early twentieth century as a result of the popularity of eugenics.

thus justifying his statistical procedure that forced this restriction on his estimated production function, but drawing criticism from economists like Durand who believed increasing returns to be an important part of the growth process[9]

Pluralism was also in display in economists' attempts to understand technological change and its consequences. Economists had not even settled on a common term for the phenomenon that might be called invention by one economist, changes in technique by another; and various classification schemes for schemes for thinking about the phenomenon could be found. S.C. Gilfillan's four categories of inventions have already been mentioned.[10] Harry Jerome proposed a number of ways of classifying technological change in his *Mechanization in Industry*. Although the book was mainly concerned with "Labor saving changes that take the form of increased mechanization" he acknowledged the importance of non-mechanical changes, which he divided into subcategories including better production control (such as those brought about by time and motion studies) and better knowledge of the order market and forecasting techniques (Jerome 1934, p. 23). Jerome explained that labor saving changes could be classed as "productivity increasing" if they increased the number of units of output that a laborer could produce, or "labor displacing", if they lowered the number of works needed "in a specific operation, plant, or industry", or could be of both types. This was a central analytical distinction in the book, with detailed explanations and hypothetical numerical examples provided to show

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[9] Douglas 1934, pp. 55-56; Durand (1937). Mendersahusen (1938) also criticized Douglas's assumption of constant returns to scale.

[10] Gilfillan's brief reference to Pigou's *Wealth and Welfare* in his *Sociology of Invention* suggest that his categories might have been based on Pigou's definitions (discussed below), but Gilfillan provides no description of how he operationalized those definitions.

how it could be applied in practice. Beyond this, labor displacing changes could be classified by the means by which they reduced the labor requirement (e.g., eliminating hand operations or increasing machine speed) and labor saving machines could be classified by the operation in which they were used (handling materials vs. changing their form), the degree to which they were automatic, and so on. (Jerome 1934, ch. 2). Merton (1935), in an analysis of factors affecting the rate of invention, divided inventions into those embodied in industrial technology, techniques of industrial organization and scientific management, and those embodied in consumer goods, and Dennison (1930) asserted that a similar distinction between what he called process and product inventions was important in understanding the impact of technical change on unemployment.

J.R. Hicks, in *The Theory of Wages*, introduced an analytical framework for thinking about technological change that is of particular importance for our story. It was a modification of a scheme of Pigou's, who had proposed that "inventions and improvements" could be classified as labor saving, capital saving or neutral according to whether they decreased, increased, or left unaltered the capital/labor ratio outside the industry affected by the invention (Pigou 1932 [1920]). Hicks kept the three categories, but redefined them in terms of ratios of marginal products: a labor saving invention increased the ratio of the marginal product of capital to the marginal product of labor, a capital saving invention reduced it, and a neutral invention left it unchanged. In the course of discussing the differences between his and Pigou's definitions, he also introduced a distinction between labor saving and very labor saving inventions, with the former referring to those that lowered labor's relative share of output, and the latter to those that lowered labor's share in absolute terms. Important for our purposes is that Hicks's definitions ran in terms of the concepts of marginal productivity theory, and, like Pigou's, were clearly designed to facilitate the use of a Marshallian/neoclassical theory of production and distribution for analyzing the impact of technical change, as was illustrated by both men's use of their definitions to discuss the impact of inventions on real wages and on labor's share of total output.[11]

There are few references to Hick's labor saving/capital saving classification in the journal literature of the 1930s, although Joan Robinson (1937) made use of it, and an alternative concept of the "neutral" technological change benchmark (still in terms of neoclassical production theory) was proposed by Harrod in a 1937 review of Robinson's book. Still, by 1946, one finds economist Gordon Bloom referring in the *American Economic Review* to Hick's "well known chapter" in *The Theory of Wages*, where one finds "a partial theory of invention which . . . embodies the use of concepts that have so captured the economists' fancy that the theory of invention has achieved a certain fame of its own. Indeed the terms "labor saving" invention, "induced" invention" and "autonomous" invention have become stock in trade to most economists . . ." (Bloom 1946, p. 83).

Measuring Growth and Quantifying its Causes: the 1930s

During the 1930s, economists continued to refine and expand the coverage of physical production indexes, providing ongoing measures of output for narrowly defined industries and

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[11] Hicks also introduced a distinction between induced inventions (those spurred by changes in relative prices) and autonomous inventions (all the rest), with the induced inventions further distinguished by whether they existed before the change in factor prices led them to be adopted, or whether they actually came into being as a response to the change in factor prices. Interestingly, this particular scheme of Hicks's is implicit in J.M. Clark's (1927) remarks on technical progress discussed earlier.

broad sectors of the economy engaged in commodity production. As of 1930, however, the NBER's program of producing annual estimates of national income faced an uncertain future. Willford I. King, who had been charged with producing estimates of the national income up to 1928, had left the Bureau on the completion of that project, and in 1931, Simon Kuznets was given responsibility for the program. He planned to carry the work forward using concepts and methods significantly different from those employed by King, as well as revising King's estimates from earlier years. Special emphasis was to be given to improving the cost indexes used in the deflation process through which time series of nominal national income were converted into a measures of real economic growth. However, the NBER was uncertain at this time whether or how they would fund this ambitious plan, as their revenues had fallen dramatically due to the Depression. This funding problem was solved by a 1932 Senate resolution directing the Department of Commerce to produce national income estimate covering the years 1929-1931 (Gay and Mitchell 1932, 1933). The work was carried out cooperatively by the NBER and Department of Commerce researchers, with Kuznets directing the project. Within a few years, the estimation of national income had been made a permanent function of the Department of Commerce, handled by the Department's Bureau of Foreign and Domestic Commerce (BFDC), and while Kuznets returned to the NBER, the BFDC researchers continue to employ his concepts and methods.[12]

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[12] Willard Thorp, a former NBER researcher, was director of the BFDC at the time, and former Kuznets student Robert Nathan directed the national income program during much of the late 1930s. However, during the war years, Department of Commerce researchers led by Milton Gilbert would part company with Kuznets on several important points as they created the

Other economists were also working on estimating the national income and its major components during the 1930s, including Clark Warburton at the Brookings Institution and Lauchlin Currie at the Federal Reserve (Carson 1975), and not surprisingly, there was disagreement among these scholars on matters ranging from foundational concepts to what data sources to use and how best to use them. It was largely in response to the existence of such unsettled questions that in 1936 the NBER established the Conference on Research in National Income and Wealth, made up of researchers from universities and from federal agencies "actively engaged in research relating to . . . the amount and distribution of national income and wealth", drawn from universities, government agencies, and private research institutions. Most notably, the Conference organized and sponsored regular meetings in which researchers could share ideas and research results, with the proceedings of these conferences published in an annual volume entitled *Studies in Income and Wealth*.

The first meeting of the conference led to an "urgent recommendation" by organizers that a subcommittee be appointed to consider basic matters of definition and terminology, and at the second meeting subcommittee chair Morris Copeland presented a paper outlining areas of agreement and controversy related to "Concepts of National Income" (Copeland 1937). Among the seventeen summary points of Copeland's essay was that what he called the gross value product of a community (the total money value of goods and services produced), if deflated, "would give a broad production index number", i.e., a measure of an economy's physical output. However, he warned, current deflation techniques were "in a very elementary stage, and one might rightly hesitate to describe as 'comprehensive' any existing attempt to correct for price

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conceptual framework that still underlies the modern National Income and Product Accounts of the US (Carson 1975).

changes in the estimates of the national income of any nation for any two years". In addition to endorsing, at least in theory, the idea that national income research could lead to a measure of economic growth, Copeland offered a pregnant suggestion of how the results of national income research could be used to measure technical progress: "Income derived from an area may be deflated to show changes in the physical volume of services of labor and wealth employed by the economic system . . the deflated distributive shares may be compared with the deflated consumed and saved income to show changes in the efficiency of operation of the economic system" (Copeland 1937, pp. 11, 31, 33).

At the following meeting of the conference, in an essay with E.M. Martin more narrowly devoted to examining the use of deflation as "an indirect method of constructing an index of physical volume" this idea was fleshed out (Copeland and Martin 1938, pp. 85, 87):

> . . . more than one deflation of the same dollar volume may be possible. The discussion up to this point has been of the procedures required to deflate national income in its credit aspect as the value of ultimate goods and services. National income may also be treated in its debit aspect as a set of primary distributive shares – payroll, interest, profits, etc. We may deflate national income in its debit aspect in order to measure changes in the physical volume of services of labor and wealth used by the economic system in the productive process. In other words, we may use deflation to measure in physical terms the 'input' that results in the 'output' of our economic system.
>
> In terms of uncorrected prices, total national income in debit terms and in credit terms are necessarily equal. In general, the correction of national income

> for price changes over a series of years will make the two volumes unequal . . . Over a period of years the output curve will ordinarily increase more rapidly than the input curve, and this more rapid increase may be taken to measure the increased efficiency of the economic system." (Copeland and Martin 1938, pp. 103-104).

Milton Friedman, a discussant of the Copeland and Martin paper at the conference, was probably not alone when he questioned the empirical feasibility of this proposal. He pointed out that the well-known biases inherent in the use of price indexes to produce measures of real output would also plague attempts to create measures of real input, and "the divergence of two indices, each of which is subject to a bias, can scarcely provide an accurate measure of changes in technology." Further problems were created by the difficulty of measuring the quantity of capital, or its price, or the quantities of other resources. (Friedman, in Copeland and Martin 1938, pp. 126-127). In response, Copeland and Martin, reflecting a gung ho attitude often found among empirically oriented economists of the time, remarked that:

> It must of course be conceded that measurements of the changes in the physical volumes of social input and output are certain to be rough under present conditions. However, those who insist on a high degree of precision had best choose some field of activity other than estimating national wealth and income.
>
> The measurement difficulties about which Mr. Friedman is concerned do not seem to have deterred other to the same extent. Dr. Kuznets has already provided measures of deflated national income in an output sense. . . . Dr.

> Kuznets' measures of capital formation necessarily involve measurements of the quantities of all kinds of capital . . . (E)stimates of total man years of employment have been developed. Thus, the two main elements for measurements of changes in social input (except for non-reproducible wealth . . .) are admittedly at hand . . .Copeland and Martin 1938, p. 134).

A Sketch of Subsequent Developments

Ten years would pass before the method suggested by Copeland and Martin began to appear in published studies, with one impetus for such studies being the continuation of the upward trend of labor productivity indexes throughout the 1930s and 1940s. With the issue of the employment consequences of labor productivity fading in importance, economists' attention was focused more directly on the causes of this upward trend. Thinking in terms of the basic decomposition that Douglas had proposed between quantitative changes in the inputs and changes in technique, the researchers employing the Copeland/Martin approach all reached the conclusion that even when one accounted for qualitative changes in inputs besides labor, the majority of economic growth in the 20th century was due to what the NBER researchers liked to call "changes in the efficiency of the economic system." However, Moses Abramovitz's (1955) remark that the newly constructed "output per unit of utilized resources" productivity indexes provided a "measure of our ignorance" rather than a measure of technological change reflected the general attitude among these researchers, and they, like the researchers who pondered the evidence of rising labor productivity in the 1920s, readily offered categories for thinking about the causes of growth that went beyond the basic dichotomy, including familiar ones such as improvements in the quality of labor and the exploitation of economies of scale.

Solow's famous 1957 paper was also essentially an exercise in calculating an output over input index. As Griliches (1996) pointed out, its novelty lay not in the actual calculations made, but in the reinterpretation of such calculations. Solow used the mathematical framework of neoclassical production theory to describe what he was doing, with the exposition of his method of constructing the index built on the mathematical manipulation of an aggregate production function Q=F(K, L; t). The weights used in combining input measures were explicitly justified with an appeal to marginal productivity theory, and when discussing the nature of technological change, Solow employed the Hicksian definition of neutral technological change, which, as discussed earlier, was defined in terms of neoclassical production theory. The basic decomposition between growth in the quantities of inputs and technical change become, in Solow's presentation, movements along the production function and shifts in the production function. Griliches (1996) writes that Solow "clarified the meaning of what were heretofore relatively arcane index number calculations", but the word "clarified" may assume too much: it implies that Solow's mathematical-neoclassical framework for constructing a productivity index was what the earlier authors had in mind but did not fully articulate. I think it is more accurate to say that Solow's theoretical arguments offered an additional justification for their general Copeland/Martin approach to constructing productivity indexes, and provided benchmarks for identifying better vs. worse algorithms when it came to the actual construction of such indexes.

Solow did not directly estimate a Cobb-Douglas regression in his 1957 paper, but instead used his productivity index to detrend the data, using the detrended data to estimate production functions using Cobb Douglas and other functional forms. So, he was doing what doing what Schultz and Douglas had called "removing the time element from the data," but instead of throwing the estimated trend away, ascribing major theoretical and empirical significance to it.

A few other economists had, prior to the Solow article, included time trend terms in Cobb-Douglas regressions, but after the Solow article (and in part as a result of it), this came to be recognized as one procedure for estimating what was coming to be termed as “the residual”, that is, the difference between the growth of the index number for real output and growth of the combined index of real inputs.

A look at two significant articles published in the early 1960s gives a sense of where the growth accounting literature headed after Solow, and how the Cobb-Douglas regression and generalizations thereof, such as the CES production function, would be used in that literature. Solow (1962) proposed an approach to operationalizing the old idea that the growth of the quantity of capital necessarily involved an improvement in the quality of capital. He constructed a measure of the stock of capital that was a weighted average of new additions to capital from previous years, with weights determined by an assumption about depreciation and an assumption about the rate at which the quality of capital improved. This quality adjusted capital variable was then included in a Cobb-Douglas regression. Griliches (1963) was an attempt to eliminate the “residual” that was revealed in studies of productivity growth in the US agricultural sector by employing better deflators for the input series, defining the inputs so that quality as well as quantity improvements were accounted for, and allowing for the existence of scale economies. In this paper, Griliches used the Cobb-Douglas regression to identify the appropriate weights to use in constructing the combined index of the various inputs used in agriculture. If one accepted Solow’s neoclassical approach to productivity index construction, as Griliches did, the standard approach of using factor payment shares as weights was only correct if factors received their marginal products. If, as Griliches believed along with most agricultural economists of the time, the agricultural sector was not in a neoclassical equilibrium, the proper weights were instead provided by the coefficients of the agricultural production function. So, Griliches obtained weights by estimating a cross section Cobb-Douglas production function using a sample of 68 US agricultural regions.

I would argue that these two articles are also illustrative of, and contributed to, a process that was going on in the 1950s and 1960s in which empirical research into the causes of growth and the consequences of technological change came to be more standardized and more narrowly focused: more standardized in sense that the neoclassical theory of production and distribution was widely adopted as the framework used to develop and discuss the concepts, methods and measures used in that research; more narrowly focused in that adoption of this framework pointed the research away from some of the questions, hypotheses and methods found in the growth research discussed in the previous sections of this paper. For example, I have described how economists of the 1920s and 1930s developed empirical evidence of growing output and increasing productivity, evidence which some regarded as a challenge to develop a more empirically based understanding of the processes of economic growth and technical change. They responded to this challenge in variety of ways: identifying what they believe to be the most interesting questions raised by the evidence, proposing ways to improve the measures of output and productivity, offering hypotheses about the causes of increasing productivity, suggesting or demonstrating potential research approaches to testing such hypotheses, and so on. As I hope I have demonstrated, there were many ways that economists could and did categorize inventions and talk about their economic consequences; one way, as Pigou and Hicks showed, was to discuss them in terms of the key concepts of neoclassical production and distribution theory. Likewise, there were many defensible approaches to measuring increases in productivity; Solow showed that neoclassical production theory could provide a framework for evaluating competing approaches. After 1960, research on technological change and its consequences was dominated by neoclassical categories and models, and methods for constructing productivity indexes were increasingly explained and defended using arguments from neoclassical theory.

I have also described Douglas's simple decomposition of growth into that which was caused by increases in the quantity of inputs and that which was caused by technological change, one which was also employed by the NBER researchers who implemented the Copeland/Martin suggestion for constructing productivity indexes. But they, like Douglas, understood that this decomposition was an expedient adopted as a starting point for empirical analysis of a complex question, and accepted the commonly made points that output could increase due to improvements in the quality of labor, that growth in the quantity of capital might be the way in which much technological improvement manifested itself, and that scale economies might be important. Griliches (1963), however, offered a method of constructing productivity indexes in a way that captured changes in both the quality and quantity of inputs and that allowed scale economies to influence growth, using a Cobb-Douglas regression and emphasizing at each step the link between his procedures and neoclassical production theory. Solow (1962) showed how the hypothesis that technological change manifests itself through the growth of the capital stock could be tractably captured in by clever adjustments of existing measures of capital formation, and used Cobb-Douglas regressions to test that hypothesis against the hypothesis that technological change was neutral in the Hicksian sense. Both of these papers thus offered tractable ways of operationalizing ideas and hypotheses about possible causes of growth that had been talked about for decades. These operationalizations were attractive because they made use of an empirical procedure, production function estimation, that involved statistical methods that were coming to be the foundation of graduate training in econometrics, and because they were expressed in terms of the mathematical neoclassical theory that was becoming central to graduate training in economic theory. So it is perhaps not surprising that most subsequent empirical exploration of these ideas and hypotheses built on the methods found in the Solow and Griliches papers. However, expressing a concept like “improving input quality” or “capital embodied technical change” in a way that can be measured with existing data using a particular statistical technique means choosing one out of a number of possible meanings of the concept. Thus, after 1960, economic research into causes of growth was more standardized, but also more narrowly focused, than the pre-war research had been.

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