PERSPECTIVE ON PRODUCTIVITY RESEARCH*

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Radical changes, up and down, have taken place in the estimates of growth in total factor productivity in the U.S. made by different economists, or by the same economists at different times. If such estimates provide "some sort of measure of our ignorance," as Abramovitz once put it, we seemed to be a lot less ignorant in 1927 (when Cobb and Douglas published their famous paper), or in 1967 (when Jorgenson and Griliches published theirs), than we were in the years between (when Schmookler, Abramovitz, Kendrick, and Denison completed their studies), or than we are today (when we have, or will soon have, revised estimates by Denison and by Kendrick, and new estimates by Christensen and Jorgenson). Viewed in this perspective, many questions may be raised about the significance of the current estimates that something like a third or more of the rate of increase in U.S. national output is "due" to increase in productivity, as well as about the concepts, data, and methods that underlie the estimates. A list of particular subjects worth considering for research is given and each is briefly discussed.

Perhaps I should introduce my remarks with Henry George's vivid phrase, "progress and poverty," for that is what I see from where I stand. Those doing research on productivity have indeed made progress. But in the paradoxical manner characteristic of science, this progress has served also to plumb the shallow depth of our understanding of a subject we all feel to be important for economic welfare. Thus—such is man's gratitude—demand is spurred for still further, and faster, progress.

That productivity is important hardly needs to be underscored. In many of the problems of current public concern—economic growth, unemployment, general price inflation, the distribution of income, the competitiveness of the United States in international trade, the high costs of providing governmental and other essential services—in all of these, it is becoming widely recognized, productivity in one sense or another is a major factor.

Recognition of the role of productivity in so many areas of concern is itself a sign that productivity research has made progress. So also, in fact, is the uncertainty surrounding our knowledge, of which productivity research has made us more keenly aware. We have come to realize that there are unanswered questions we had not even thought to ask, and questions unanswered because we had not known enough to ask them properly.

To be more specific about the progress and the poverty and the way to move ahead, let us look back and consider the kind and the pace of some of the productivity research already done. We will then be better able, I think, to appreciate the nature, dimensions, and importance of the tasks that lie before us.

We might begin our story with the late 1930's, when the Bureau of Labor Statistics, the National Bureau of Economic Research, and the National Research Project, here in the United States, and that one-man research institution, Colin

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Clark, overseas, were piling up quite a heap of productivity measurements and related information, and doing analyses of these; or a little earlier, with 1927, when Cobb and Douglas read their paper on the production function; or with 1913, and Mitchell's researches on the relation between productivity and business cycles; or with the late 19th century, when Wicksteed was working out his production and productivity theory; or some decades earlier, when Hearn discussed "the circumstances which determined the extent of invention"; or with the late 18th century, when Adam Smith thought of a multitude of relevant ideas, ranging from the economies of scale down to the basic matter of incentives; or even with the 17th century, when Petty applied his political arithmetic to the measurement of human capital.

But I have said enough to make my initial point. Studies of one or another aspect of productivity go back a long way. What we know about productivity today is based on the labors of generations of economic scientists.

For the present purpose it should be sufficient if we limit our review largely to the post-World War II period, with a glance back to the 1920's. Even for this relatively brief period no one could pretend to cover in a single paper the researches done and the questions opened on a subject as ramified as productivity.[1] We must also limit our review, then, to only a section—a central section, I hope you will agree—of the wide range of subjects that legitimately fall under the rubric of productivity research. This subject is the measurement, and determination of the sources, of change in total factor productivity.

What I want particularly to do is to suggest a little of the flavor of the research done, the difficulties encountered, the alternatives pursued, the mistakes made, the results obtained or not obtained, and some of the subjects that seem worth considering for the research agenda.

Research is not quite like a random walk, though it may often look as if it were. But neither is it going down a straight and narrow path. Consider the radical changes, up and down, that have taken place in the estimates of total factor productivity published by different economists, or by the same economists at different times. When Abramovitz presented his own estimates in 1956, he hastened to add that the indicated importance of total factor productivity in accounting for change in output is "some sort of measure of our ignorance about the causes of economic growth in the United States and some sort of indication of where we need to concentrate our attention," since we know so little about the sources of productivity increase. If the indicated importance of total factor productivity is "some sort of measure of our ignorance," and if the importance indicated by successive estimates is to be taken at its face value, we could say that we were a lot less ignorant in 1927 and 1967, when estimates (implicit or explicit) were published then indicating that change in total factor productivity was zero or close to zero, than we were in the years between or than we are today. Something

[1] By 1963, Walters was able to list several hundred references in his survey of production and cost functions, most of which were postwar studies. Hahn and Matthews added more in their 1964 survey of the theory of economic growth. So did Nadiri in his survey, 1970, of approaches to the theory and measurement of total factor productivity. And so, also, did Kennedy and Thirlwall in their 1972 survey of research on technical progress. Yet these surveys do not begin to list relevant references to the vast literature classified under such headings as labor relations, economic development, income distribution, business (and governmental) organization, national accounting, incomes policy, and regulation.

like a third or more—current estimates still vary—of the average annual rate of increase in national output is now being ascribed to increase in total factor productivity. If these estimates are anywhere near the mark, they point to some of the work cut out for us. For until we know more about the dimensions of the various sources of this productivity increase, we cannot be as confident as we would like to be when choosing policies to promote productivity.

To appreciate what I have just said, it is necessary to recall some of the details of the story.

A good deal about the productivity research of recent decades is indicated by the shift in the concept of productivity viewed as primary by economists—or, at least, by economists who specialize in the subject. When they talk about productivity these days, unless it is otherwise specified, these economists are talking not about labor productivity, as they used to and as most laymen still do, but about total factor productivity. In 1946, when another Conference on Productivity was held here in Washington under the auspices of the Bureau of Labor Statistics and the Bureau of the Budget, there was barely any reference to the idea of total factor productivity. Indexes of total factor productivity did not even exist, except in the implicit form taken by such an index in the empirical production function estimated by Cobb and Douglas.[2]

The shift of interest among economists doing productivity research from labor productivity to total factor productivity reflects the postwar emphasis on the study of economic growth. In thinking about the sources of growth and the problem of measuring these sources, economists found it helpful to classify them into two main groups. One is change in the resources available for use in production—total factor input. The other is change in the efficiency with which available resources are used in production—total factor productivity.

This is not to say that labor productivity—output per man or manhour—was no longer of interest in the study of economic growth. In fact, labor productivity continued to be highly interesting, for it measures the fruitfulness of human labor under varying circumstances. It simply means that differences among countries at various stages of economic growth, in respect of output per man or per manhour, were recognized to reflect differences in total input per man or manhour, as well as in levels of efficiency.

Looked at a little differently, the measurement of efficiency in the use of all resources no longer needed to depend, as before, on labor productivity, which is at best only an approximation to a measure of efficiency. It became possible to take into account also capital productivity—output per unit of capital input.[3] For the year 1946 was also the year that saw the publication of Kuznets' book on *National Product since 1869*, with its important section on reproducible wealth as well as output. These data, along with information on land and labor taken from other sources, made it possible for Schmookler to calculate the estimates he published in

[2] This may seem something of an exaggeration, since Cobb and Douglas *assumed* no change in total factor productivity; but see the later comments on their test of the assumption. Tinbergen had constructed a Cobb–Douglas function that did provide a productivity measure not assumed equal to zero; but because his paper had been published in a German journal in 1942, it was overlooked until a translation was published in 1959.

[3] Total factor productivity is, in effect, a weighted average of labor and capital productivity.

1952 of long-term changes in total factor productivity in the United States.[4] Soon after, in 1956, came Abramovitz's estimates, and also a preliminary version of Kendrick's; then, in 1961, Kendrick's book; Denison's, in 1962; Christensen/Jorgenson's papers, in 1969–70; and most recently, only a year ago, an "official" estimate for nonfinancial corporations, by Gorman at the Bureau of Economic Analysis.

With the help of these measures of total factor productivity, and of tangible and intangible capital per manhour, economists have been learning something about the sources of increase in output, in output per manhour, and thus also in output *per capita.*

Perhaps the most striking finding—to judge from the surprise with which economists greeted it in the 1950's—was the large importance ascribed by the early estimates to total factor productivity (in which little or no allowance was made for improvement in the quality of labor), in accounting for increase in real national product. In the United States, over the preceding three-quarters of a century or so, the rate of growth of total factor productivity had been no smaller than the rate of growth of total factor input. With regard to national product per capita, total factor productivity was altogether the dominant source. Its increase accounted for approximately nine-tenths of the growth of *per capita* output. Something very much like this was found to be largely true also of the trends in other countries, and of differences in international levels of output *per capita,* when attention was turned to them.

Also striking was the fact that the great preponderance of industries in the United States, and in other countries as well, was characterized by an upward trend in total factor productivity even though little or no allowance could be made for significant improvements in the quality of output, as in manufacturing and the service industries, and even when pressure on land and other natural resources might be expected to be an important negative factor, as in agriculture and mining.

It took a while for these surprising results to sink in. There was Schmookler's pathbreaking paper in 1952, a brief note by me in the 1954 Annual Report of the National Bureau of Economic Research, which may have helped, and then the papers by Abramovitz and Kendrick in 1956. These must have had a cumulative effect. Yet I rather suspect that the results were hammered home to economists only in 1957, when Solow presented essentially the same findings in a form that fitted more neatly into the analytical framework of economic theory. Change in total factor productivity was expressed by him as an annual rate of shift ("technical change") in the function connecting output with labor and capital input.

The research of the 1950's stimulated a good deal of thinking and rethinking, and for precisely the same reason that the original Cobb–Douglas production function of 1928, of which we took notice earlier, had stimulated thinking. The results were surprising in both cases. But they were also almost opposite in what

[4] To my knowledge, the first explicit measure of change in total factor productivity was constructed by Stigler in 1946 (for U.S. manufacturing, 1904–1937). Mention should be made also of the work done during 1946–47 in the Department of Agriculture by Cooper, Barton and Brodell; and of the later, 1961, report by Loomis and Barton. The latter presents estimates of total factor productivity in U.S. agriculture that are updated periodically in the *Agricultural Situation.*

they indicated about total factor productivity. The findings in the 1950's seemed to indicate that total factor productivity was of great importance in accounting for the increase in output. We needed, then, to learn more about the sources of increase in total factor productivity. The finding by Cobb and Douglas in 1927 seemed to indicate that total factor productivity was of negligible importance. Was the increase in efficiency that economists and economic historians had been taking for granted really negligible?

Before we continue our story of the postwar research, it is worth going back to 1927 for a moment.

In their famous 1927 paper, "A Theory of Production," Cobb and Douglas had, in effect, compared a weighted index of number of workers and reproducible tangible capital, calculated by them for U.S. manufacturing, 1899–1922, with Day's index of the real output of manufacturing. They found a close correspondence between the trends of the two. Indeed, the ratio of output to the total input estimated by them seemed to fluctuate along a horizontal line. Expressed in our current terminology, the rate of growth in "total factor productivity" was zero, at least for this industry during this period.[5]

This result did not go unchallenged. At the very same meeting of the American Economic Association in which the Cobb–Douglas paper was read, Sumner Slichter, the discussant, expressed his doubts. A few months later he was joined by J. M. Clark in a fuller discussion. Slichter and Clark questioned the accuracy of the basic data and the estimates derived from them—with justice, as it turned out years later. They questioned a calculation that made no allowance for land, working capital, and "entrepreneurship"—but did not mention human capital. They were especially surprised by the absence of technological change. Indeed, wrote J. M. Clark, "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 full increase calmly attributed to increased capital. . . . What, then, has become of our boasted progress?" The assumed absence of economies of scale also troubled him and Slichter. Clark devoted considerable attention also to the effects on the results of cyclical fluctuations in production and hours of work. And he considered the choice between number of workers and number of manhours in measuring labor input posed by secular declines in hours of work. Clark even made a calculation in which labor input was measured by manhours, substantially altering the Cobb–Douglas results. Slichter further noted the assumptions of competition and of "complete and instantaneous adjustment" involved in the use of base-year income shares as weights in combining the labor and capital indexes. Clark alluded, in passing, to the "many-faceted issue of 'social productivity versus private acquisition'." In the list of questions drawn up by Slichter and Clark, and

[5]It is true that the original Cobb–Douglas function *assumed* this to be the case. But the assumption would not have been validated in the eyes of Cobb and Douglas had not two requirements been met. First, the function should fit the data very closely. It did. Second, the percentage change in output in response to a one-per-cent change in labor or in capital (i.e., the elasticity of the product with respect to small changes in labor or capital), as estimated by the function's parameters, should agree with independent evidence on the fraction of income going to labor or to capital. The latter, provided in the National Bureau's first study of national income, did agree with the fractions implied by the fitted Cobb–Douglas function.

later by others on econometric problems, can already be seen many of the subjects that have been the concern of economists ever since.

It tells us something about researchers that their first empirical (implicit) calculation of change in total factor productivity could say that it was negligible in a country and industry and period when technological and other change was widely recognized as advancing rapidly and greatly transforming the economy.[6] Further, Douglas' collaborator, Cobb, obtained puzzling and contradictory results when he later applied the function to the Massachussetts data. Notwithstanding—and this tells us something about the other economists—the results seemed to be widely accepted by many of them despite the doubts and questions raised by Slichter and Clark. At any rate, the results found their way into textbooks.

The lesson here, I think, is two-fold. First, hunger for information on the importance of the factors with which we must deal is so keen that even bad estimates tend to be used when there is nothing better. Second, even research that comes up with misleading results can eventually serve to advance knowledge if it is interesting enough to stimulate constructive efforts to do better, as the work of Cobb and Douglas was.[7]

Those who made the estimates of total factor productivity in the 1950's recognized at the outset that the measures of input failed to take account of important forms of intangible capital, especially education and technology. The estimates of efficiency made by comparing output with these measures of input were therefore defective. Much more research was needed.As Abramovitz noted, in the 1956 paper I have already cited, conventional methods of measuring resource inputs, by following the usual definitions of labor supply and capital, are faulty and probably understate the increase in factor input. There is, he felt, the possibility of a more fundamental concept of investment broad enough to include "any use of resources which helps increase our output in future periods," such as expenditures for health, education, training and research. "These are fairly obvious because one is conscious both of an income motivation and an income effect. But there are other classes of expenditures where motives are mixed or disguised but which have at least the incidental effect of increasing productivity . . . The fact is that, in a thoroughly commercialized economy . . . very few expenditures are wholly without the aim and effect of increasing income."

These and similar reflections by others in the 1950's and early 1960's led to attempts to measure some of the neglected elements of capital and say something

[6]Compare, for example, *Recent Economic Changes*, especially Chapter II.

[7]One of the subjects that absorbed an increasing amount of attention in productivity research during the postwar period was the production function, further mention of which I must relegate to this footnote. (See the Surveys cited above.) The Cobb–Douglas function presented in 1927, and the later work in the 1930's and 1940's carried on by Douglas and his collaborators, stimulated a good deal of criticism and undoubtedly helped to draw attention to the production function, as it did also to various questions of econometrics. So did the Solow paper of 1957. But mathematics and econometrics were becoming popular for other reasons as well, and they lent themselves to the study of production functions. Many, probably most, of the production function studies have a formal rather than an applied character, but even the formalistic studies have on occasion contributed to the progress made—if only by exposing, directly or indirectly, assumptions implicitly made in much of the empirical work on productivity. Whether work on the more formal aspects of production functions belongs on the research agenda we will be suggesting is questionable. However, whatever our answer, I suspect, some economists—or mathematicians—will continue to study them.

about the sources of change in the recalculated measure of total factor productivity.

The procedure followed by Kendrick, in his 1961 study, was to weight the hours of labor in different industries in accordance with their relative wage rates.[8] Denison made a more explicit attack on the estimation of change in the quality of labor, in his 1962 study, by calculating the increase in the average number of years of education received by members of the labor force and shifts in the age–sex mix, and assuming that the quality of labor of a person of given age, sex and education could be measured by his relative income. Each of the approaches had its limitations. Kendrick's, for example, suffers the handicap of a rather gross industrial classification, and in any case cannot deal with shifts in the composition of labor within industries. The same can be said about Kendrick's weighting of the tangible capital of different industries. And Denison's approach raises questions that have constituted the subject matter of many efforts to determine the effects of education on income. Both approaches involve the assumption, in combining different classes of labor, and labor input as a whole with capital input, of competitive equilibrium—something that had troubled Slichter in 1927, and still troubles economists today, as we shall see. Neither approach could take into account changes in the "spirit" of labor and management.

Denison went further, going beyond Kendrick, by estimating (some might say, "guessing," because the necessary information is extremely limited) the contribution to increase in output of the secular decline in the length of the work week, one of the questions that Clark had raised. Denison identified this effect not as a productivity factor but rather as a change in the quality of labor, an input factor—which raises another question.

Kendrick's, and even more, Denison's, efforts to improve the measurement of total factor input had pushed their estimates of its rate of growth up, and thus reduced the estimates of growth of total factor productivity. But even thus reduced, the contribution of total factor productivity to increase in output was still as high as 40 percent (over the period 1950–62, according to Denison).

Denison boldly went on to identify and give measures of the major components of his estimate of total factor productivity. He estimated (or guessed) the contribution to growth of productivity, and thus also output, of the scale economies made possible by the larger size of the U.S. market, the economies associated with shifts of workers and entrepreneurs away from inefficient farming and other small-scale business, and the changes in governmental regulations that influence the efficiency of allocation of resources among industries and firms. The final residual he took to measure increase in the stock and change in the rate of diffusion of knowledge.

We would all agree, it hardly needs to be said, that these are sources, proximate sources, of economic growth, and that most of these sources belong under the heading of productivity—as does also, in my opinion, the effect of the decline in the work week. We would probably agree, further, about the direction of their effects. The crucial question, however, is that of their importance at different times and in different places. Denison's opinions deserve attention; but

[8]In his earlier study, Schmookler was able to do so only in a limited way. He could distinguish only two industries, agriculture and non-agriculture.

they are more in the nature of targets at which to shoot than of estimates to be taken seriously, as Abramovitz noted in his review of Denison's book some ten years ago. Since 1962 very little has been done on assessing the hours effect. There has been an interesting hypothetical calculation by Griliches on R. & D. And he and others have done some work on the economies associated with changes in scale. Measurement of the several sources of change in productivity, and of the contribution of technology and other useful knowledge to economic growth, are still strong candidates for the research agenda.

The measure of increase in total factor productivity, about the sources of which little more was known, in quantitative terms, than could be read in Denison's bold calculations, remained a considerable challenge after 1962.

Something like a bombshell seemed to come in 1967, in response to this challenge, when Jorgenson and Griliches published their paper on "The Explanation of Productivity Change." In it they presented an estimate of change in total input very much higher than Denison's, and therefore even farther above Kendrick's. So far had the rate of growth in input been raised by Jorgenson/Griliches that the resulting estimate of change in total factor productivity was pushed down almost to a negligible level. However, after some corrections of errors and a reconsideration of their estimates on other grounds, in response to a careful examination by Denison, Jorgenson/Griliches restored a good measure of importance to their estimate of total factor productivity. Further revisions by Christensen and Jorgenson restored still more. It now accounts for about 30 percent, for the period 1950–62, of the rise in output. The larger part of the remaining quantitative difference between Jorgenson/Griliches and Denison appears to reflect a correction by the former for change in capacity utilization.[9]

I have mentioned the debate because—as in the case of the Cobb–Douglas production function of 1927—it tells us a good deal about the nature of the research process. As I said earlier, research can sometimes seem to resemble a random walk. But there is a more important reason. I expect that when the air has finally cleared—we are still too close to the debate to be sure—it will be found that some net progress had been made.

At least, some new questions have been raised and old questions brought back to interest us. Surely among the subjects of these questions, all worth considering for the agenda of research, are the following, which I draw from the debate and from previous discussions by the participants and other economists, ranging back at least to Slichter and Clark:

The distinction between the concepts of, and the quantitative difference between changes in, the input and the stock of labor and capital;

The concept and measurement of capital consumption;

The treatment of capital gains and losses in calculating input and output;

The relation between capital consumption—particularly obsolescence—and measurement of the quality of successive vintages of capital goods;

[9]Neither Kendrick nor Denison made any explicit allowance for the effect of fluctuations in the studies they did in the 1960's. The problem created by fluctuations was met by confining the calculations of trends to changes between peaks in business cycles.

The estimation of percentage of capacity utilized, which is only part of the broader question of the pattern and the causes of cyclical fluctuations in output, input, and productivity;

The scope of the economy covered, particularly with regard to family holdings of tangible capital goods (and of the family labor associated with their use), and the family's role in the educational process;

The introduction of the notion of disequilibrium into growth accounting, and the related task of introducing as an explicit source of change in productivity the changes that take place in the industrial composition of output;

The effects of subsidies, and changes in subsidies, on the measurement of the contribution of education and R. & D. to economic growth—the question of the inequality of social and private costs and benefits;

The interrelationships among inputs, and among productivity sources, and between input and productivity.

It is quite a bag of subjects I have emptied out before you.

I can begin to comment a bit further on a few of these subjects by recalling, first, how—in his usual careful way—Denison tells his readers at the outset that he will be determining the sources of "measured" growth. This, of course, is to warn them of the imperfections of his measurements. The limitations apply with equal force to the estimates by others. Doubts about government output and about capital consumption, for example, are sufficiently strong to cause economists to concentrate on the "private" economy, and on measures of output, input, and productivity gross of capital consumption. The estimates of output are questionable also for construction and the service industries, when they are used in the measurement of productivity. And even the measures of manufacturing output are deficient because quality changes are not taken fully into account.

A fair amount of study has gone into these questions.

When the postwar period began, about all we had on the question of quality change was Court's notable 1939 paper on hedonic price indexes. Then came Griliches' paper in 1961, which led to further studies by him and others. By 1971 enough work had been done by various authors to warrant the publication of a volume of collected papers on the methodological issues, which we owe to the editorial enterprise of Griliches. A more immediately useful publication is the regular release by the Bureau of the Census, over a decade, of an hedonic price index for one-family homes. Much remains to be done, especially on the health service industries, where quality change has been very rapid—perhaps excessively rapid when measured on a rational cost-benefit scale.

The situation with regard to output and productivity in government is beginning to show some signs of improvement. Because the costs of governmental services have risen sharply over the postwar period, some efforts have been made to measure, analyze, and raise productivity in the federal government, and more recently in the lower levels of government. Hopefully, the effort will help develop information on the activities, and organizational arrangements and incentives, that influence government productivity and costs. For many decades, government activity has been growing more rapidly than the economy as a whole, and it now employs a substantial fraction of the nation's labor force and its capital resources.

It is interesting how long it has taken for this fact to be realized, and for something substantial to be done—or to begin to be done— to study productivity in government.

Some of the difficulties in measuring capital consumption arise because of obsolescence. The quality of successive "vintages" of capital goods, and also of successive generations of workers, changes with technology. How to measure capital and labor input, in this situation, and how to determine the importance of investment, when it is a vehicle for technological change that could not otherwise be put to use, led to a flurry of research some years ago on the "embodiment" question.

What we have, here, is only one example of a much broader problem. Economists have found it convenient, at the present stage of their research, to distinguish input from productivity and assume them to be independent of one another. It is doubtful, however, that they are independent, and if related, that they are related in any simple way. There are reasons for believing that input and productivity interact in ways more complicated than we can now deal with. So, too, do different kinds of input, as we recognize when we worry about elasticities of substitution. So do various sources of change in efficiency. Scale influences technology, for example, and technology shifts the optimum scale. Further, the size of a market influences its productivity through the specialization and other economies large markets make possible. This we classify as a "proximate" factor. The size of market influences productivity also through the competition—a "basic" factor—that a large market may force on what in a small market would be a dominant, because absolutely large, enterprise.

These are all questions that have been studied for many reasons and deserve to be studied further. Which, if any, of these complicated questions should be picked for the agenda to which our discussion will lead is difficult to say. The data and technical demands some of them would make are, I suspect, beyond our reach in the foreseeable future. In any case, we must avoid the danger, in drawing up an agenda for productivity research, of encompassing the whole universe of economic—and extra-economic—research.

More amenable to analysis at this time, I would judge, are studies of typical patterns of productivity change and of the factors associated with them.

Typical patterns of change in productivity, especially during the business cycle, have commanded attention in some of the National Bureau's researches during the postwar years. Building on the work of Mitchell, to which I alluded earlier, advances in our knowledge have been made by Hultgren, Burns and Moore. We have learned something about the factors that affect productivity, and about the way productivity change affects labor costs and profits, and helps to generate the restrictive forces that bring on recession. The question of cyclical change in productivity is important for the measurement of potential GNP ("Okun's law"), for the so-called "re-entry problem" (mentioned most recently in the Economic Report of the President), and for incomes policies. A further review and study of these questions may be desirable.

Another useful point of view from which to observe patterns of change in productivity, if they exist, and to identify sources of change in productivity, especially technological innovation, is provided by the studies made in the early

1930's by Kuznets and Burns and in the 1960's by Schmookler. The point, here, is that the rate and character of technological innovation in an industry may be a function of the age of the industry. In particular, it is presumed that the rate of innovation in an industry suffers retardation as the industry exploits the technological break-through that gave rise to it in the first place. This relationship of productivity and innovation to age of industry deserves more attention than it has received in recent years.

Reference has already been made more than once to an idea that has been taking hold during the postwar period, that a good deal of what is classified under productivity might better be classified under capital input. This has led to efforts by Juster, the Ruggles, and Kendrick, to shift from the consumption to the investment category of the national accounts such items as educational expenditures. This would also require widening the scope of the accounts to cover the depreciation on the stock of educational capital, for example, and to cover parts of the family economy not yet covered—in this case, the time and energy spent by students in acquiring an education, if not also the contribution made by parents in this arduous enterprise.

A closely related idea, which should at least be mentioned in any discussion of productivity research, is that of productivity in the household. Many years ago, Wesley Mitchell wrote a pioneering essay on "The Backward Art of Spending Money." Only in recent years, however, has the idea of families as producers, who buy commodities and services and combine these purchases with their own labor and the use of their own capital to produce the utilities they desire, spread as a result of the work by Schultz, Becker and others. A very recent example of research along this line is that of Grossman and Michael at the National Bureau on the role of education in efficiency in consumption. Emphasis these days on consumer advisory services, government protection of consumers, etc., has also brought the subject into prominence.

The notion of disequilibrium, mentioned earlier, refers not only to an intriguing theoretical problem, but also to a difficult human problem associated with productivity change.

A major consequence, and also a major source, of productivity increase is change in the way we work and live. The shift out of agriculture and the accompanying process of urbanization has been going on for centuries; Colin Clark referred to these changes as following "Petty's law," because Petty observed their significance. But hardly anywhere is there equilibrium. Industries and jobs within the urban area also become obsolete or change their character. Clark and A. G. B. Fisher, before World War II, and Kuznets after World War II, have traced many of these changes in some detail. What results is an enormous and difficult problem of adjustment for many people. These are the problems people have in mind when they worry about technological unemployment and the diminution of job satisfaction.

One of the lines of research that may be mentioned in this connection consists of statistical correlations, most recently by Kendrick, between trends in productivity in individual industries and corresponding trends in prices, output and employment. These correlations suggest that industries in which labor productivity is rising most rapidly are not necessarily the industries in which employment is declining most rapidly. In fact, within manufacturing industries the correlation between relative trends in productivity and in employment seems to be positive. But the picture is not simple. There are many exceptions, as in the case of agriculture in recent years; and it is in many of the service industries that productivity (as usually measured) has lagged and employment risen most rapidly. More needs to be done on the problems of adjustment associated with these changes, and on policy to ease them—to help lower a major obstacle to technological change and other sources of productivity increase and to avoid imposing on a few the costs of economic progress enjoyed by everybody.

There are many other lines of productivity research that I have not mentioned and cannot mention. But I must conclude, and I do so with some general observations.

First, as with any variable or problem to be investigated, it is easy to make lists of work to be done. Research could be pursued on productivity concepts and measurements, on productivity behavior and on departures from typical patterns of behavior, on the causes and on the consequence of productivity change, and on policy to raise productivity. But these can only be lists of possible lines of research. Anyone planning a research program must be more specific. He must estimate, as well as he can, the amount and the time and the probability of returns from study in each direction. He must estimate the prospective costs that would be incurred by each. Only then can he choose. Even economists sometimes forget the difficult problem of estimating costs and benefits, a problem especially difficult in planning the adventurous and hazardous enterprise of research. Yet it is a problem that all of us assembled here must face up to when we discuss an agenda for economic research on productivity.

Let me return for a moment, next, to 1946. It was the year, you will recall, when the growing belief that something more could and should be done by government to deal with the economic problems that troubled our people led to the passage of the Employment Act of 1946. The Conference on Productivity was one response to the rising demand for economic knowledge that accompanied this concern.

The immediate postwar period is noteworthy also for certain developments on the supply side—developments already in motion that influenced the volume and the character of economic research in general and of productivity research in particular during the post-World War II period. Partly but not entirely in response to the rising demand there came an accelerated improvement in the quantity and quality, frequency and timeliness, of statistical information. We forget that quarterly estimates of national product, income, employment and unemployment, for example, were in their infancy about the time the 1946 Conference on Productivity was held. The electronic computer was still a primitive and costly instrument in 1946. Econometrics was not yet the full-blown subject now "required" of all students of economics. Scientific sampling was only just beginning to be applied to the collection of the kind of survey data we need for micro studies. Longitudinal micro data, on which I suspect that a good deal of our future research progress will depend, could hardly be said to exist at all in 1946.

Today, I dare say, the demand for economic knowledge is greater than it was in 1946. Today, also, I suspect, researchers are better armed than they were in

1946. I am not bold enough to say that our productivity is higher than it was then, but we have better data, better means of getting more data, better ways to analyze them, and a stock of ideas that still another generation of economists has bequeathed us, ideas on what to look for and how to look for it. We can plan ahead with some optimism.

I should add, finally, that no studies, however thorough, could be expected to eliminate differences of opinion on what to do and how to do it, in order to promote productivity. In some part, these differences reflect ambiguities of language, and limited understanding for other reasons on the part of the public, including those in positions of responsibility, of the results of the studies. In part, also, these differences reflect differences in the values placed on alternative national goals, although these differences often are, in fact, differences in judgments on means rather than on ends. In time, education will lessen these obstacles to more effective policy. Economists have some responsibility to contribute to this process. But the main task of the economist, in his role of investigator, is to help lessen the differences of opinion on policy that reflect the limitations of tested knowledge of the basic facts on productivity, of the factors that determine the level and rate of change of productivity, and of policies to promote productivity.

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