Comments and Discussion

Martin Neil Baily: It is unfortunate that some issues that surfaced in the economics of growth in the 1950s and 1960s and that appear in productivity studies today have never been resolved. The main problem for growth economics is that variations in the medium-term growth rate of the U.S. economy are not well explained by measured factors of production when these factors are entered into a freely estimated production equation.

Analysts of growth have avoided this problem by blaming collinearity and imposing strong assumptions on the data. Accounting for growth by using relative-share weights is one such approach. With important differences from earlier work, it is the one used by J. R. Norsworthy, Michael Harper, and Kent Kunze in this paper. It is a perfectly good and sensible approach, but it is limited. It assumes many of the phenomena we would like to test or understand. The value of the approach is in establishing a general outline within which to work, and in demolishing myths. Many pet theories do not get off the ground once one looks seriously at the evidence.

Let me turn to specific aspects of the paper. There are many compliments I could give to it. The authors present an interesting and important story. To avoid platitudes let me leave it at that and proceed to areas of possible disagreement or different emphasis.

First, I mistrust the assumption of slope discontinuity. The authors divide the data into three phases. Other studies use the equivalent technique of dummy variables. After one has explained output and productivity movements it may be helpful to distinguish different phases. But imposing them ex ante may prejudice the conclusions.

Second, at a disaggregated level, special factors abound. Public utilities

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overestimated demand for electricity substantially. This led to cutbacks in investment and a stalling of productivity growth. Retail stores began to stay open much longer hours in the seventies. This caused a decline in the capital-labor ratio and a decline in measured productivity growth. One presumes that there was an unmeasured gain in convenience. One can find special factors for most industries, and although the special factors may mask the big picture, the reverse is also true.

Third, adjustments that are made to the quality of the labor force here and in other related analyses trouble me because they assume that relative wages reflect relative productivities. The increase in the educational level of the labor force in the seventies may not have contributed much to productivity growth. Correcting for education may make the puzzle of that decade more perplexing than it actually is. People may demand education as a means of signaling relative ability. My own undergraduate years were mostly a consumption good. The increase in the number of high school graduates may stem as much from a lowering of graduation standards as from an increase in quality.

In addition to the adjustment for education, the authors' table 8 also reports an adjustment attributed to changes in the sex composition of the labor force. Does the difference between the wage rates for males and females really reflect productivity differences? There is little direct evidence on this issue. I know of two items. Martin Feldstein found no significant productivity differences between men and women in a cross-sectional study of manufacturing in the United Kingdom (in the October 1967 *Review of Economic Studies).* And some evidence—which I am sure no one will take seriously—comes from a follow-up survey of persons who had actually changed sex. One hundred seventy follow-up interviews were conducted by a Stanford psychiatrist. *The New York Times* of October 2, 1979, reports that all those changing from female to male earned more after the change. Most of those changing from male to female earned less. Three males who changed to females decided to resume living as males. Two of the three cited an inability to do well economically as the reason. The third had a religious experience.

A fourth aspect of the paper is related to the intriguing Stafford-Duncan study. I wish the Bureau of Labor Statistics had collected more data measuring changes in work intensity. Differences in work practices are alleged to cause important productivity differences across countries and may be a major factor in changes over time.

Fifth, Edward Denison, in his book, *Accounting for Slower Economic Growth: The United States in the 1970s,* describes the slowdown as a mystery. The conclusions in this paper reinforce the sense of mystery. The two successive slowdowns in 1965–73 and 1973–78 have reduced the growth of labor productivity by 2.12 percentage points in the private business sector in comparison with the 1948–65 base period. I refer to table 11. Of this total, 35 percent is attributed to capital effects, 13 percent to labor effects, and 52 percent to unknown factors. For manufacturing, where output figures are relatively more reliable, of a total slowdown of 1.43 percentage points, 18 percent is attributed to capital, −5 percent to labor, and 87 percent to other factors.

The authors place their greatest emphasis on the role of capital in the slowdown. Because capital accumulation has been slow in the seventies, this does allow them to cite capital effects as the principal cause of the slowdown in 1973–78. But, as the authors admit, the price of this is a magnification of the puzzle of 1965–73. During this previous slowdown period, unknown factors account for 95 percent of the slowdown in the private business sector and 103 percent in manufacturing.

In fact, with the perspective of the entire 1948–78 period, capital accumulation appears to have proceeded rapidly immediately after World War II, slowed during the late fifties and early sixties, sped up in the late sixties, and slowed again in the seventies. The correlation between this pattern and that of medium-term productivity trends is not very strong.

Even the capital slowdown in the seventies needs to be viewed with caution. The authors use a very aggregated measure of the capital stock. The separate components have moved relative to each other. I looked at the capital-labor ratio for the nonfinancial corporate business sector defined as the ratio of equipment to total employee-hours. This ratio grew at 2.94 percent a year from 1965–73 and 2.42 percent a year from 1973–77. That is not a big slowdown. Most of the movement in the authors' capital-labor ratio is from structures and inventories. While I do understand the argument that capital is capital and marginal productivities should equate, there are many assumptions required to make the "should" into "will." I suspect that new vintage machine tools, new vintage computers and new electronic typewriters are the items that really enhance productivity.

Finally, the word "vintage" raises another query. If the argument that increased energy prices caused labor-capital substitution is to be taken

seriously, I would like to see some direct evidence that such substitution is in fact possible in a way that saves significant amounts of energy for capital that was designed and put in place before the energy crisis.

One wonders, after all this, why it is that capital does not do better as an explanatory variable. Most writers since the nineteenth century have put capital accumulation at the center of their theoretical growth models. Common sense tells one that it must be important. From investment equations it is apparent that capital *follows* output closely, but one might ask why it does not do more when it arrives.

The problem must lie with the measurement—or lack of measurement—of other factors. There is no good measure of innovation. Patent figures and expenditures on research and development are weak proxies. New products and changes in product quality are hardly counted at all. There is no good measure of labor-force quality. Analysts rely heavily on demographic proxies and formal education or training. With better measurement of innovation and labor quality, the role of capital would probably fall into place. But saying that and showing that are two different matters.

Edward F. Denison: Norsworthy, Harper, and Kunze have carefully integrated into their study of productivity a great deal of data relating to the composition of the capital stock and the labor force. Their results speak for themselves and I shall not attempt to summarize or analyze them. What I can do most usefully is compare some of their estimates with my own and try to identify sources of disagreement.

I shall refer only to the estimates by the authors for private business and compare them with mine for nonresidential business.[1] They show, in addition, results for private nonfarm business and manufacturing; I show government, households and institutions, services of dwellings, international assets, and the entire economy.

The authors' estimates for private business differ from mine for nonresidential business in scope (with their inclusion of tenant-occupied housing and exclusion of government enterprises), measurement of out-

[1] My estimates are provided and fully described in Edward F. Denison, *Accounting for Slower Economic Growth: The United States in the 1970s* (Brookings Institution, 1979).

put (with their use of gross product instead of national income), and to some extent data (but any statistical differences are almost certainly unimportant). Their analysis is organized around output per hour, mine around total output and output per person employed, and there is no simple way to convert. Our analyses are also organized around different time periods (theirs, 1948–65, 1965–73, and 1973–78; mine, 1948–53, 1953–64, 1964–69, 1969–73, and 1973–76; I give some results for 1973–78 but not a detailed analysis of sources of growth). For all these reasons any close comparison would be a major undertaking. In addition, the determinants of productivity change that they estimate directly are not the same as mine and, when they are, the classification is different.

Despite all this, the main findings are easily compared. We agree on three main points and disagree on one, and the reason for that disagreement is readily identified.

We agree, first, that a sharp reduction in productivity growth has occurred and that it has happened in two stages, the first beginning after about 1965 and the second after 1973.

Second, we agree that the post-1973 drop in the growth rate of productivity is quite general. On an (approximately) one-digit industrial classification, the 1973–78 growth rate is much lower than the 1948–73 industry rate for all sectors except communications.

Third, a large part of the difference between the growth rate of productivity in 1948–65 or 1948–73 and the growth rate in 1973–78 is not accounted for by directly estimated determinants. (See the authors' table 11.)

The major disagreement is about the period when the "mystery" developed. I have written that until 1973 the slackening of productivity growth was neither mysterious nor particularly disturbing from the standpoint of long-term growth. The authors, however, find the drop from 1948–65 to 1965–73 to be largely unexplained, while the further drop from 1965–73 to 1973–78 is almost entirely explained.

The reason for this difference is my inclusion of an estimate for the effect of fluctuations in intensity of demand as a determinant of output per unit of input. The growth rate of this series is 0.06 percent in 1948–65, −0.70 percent in 1965–73, and 0.12 percent in 1973–76. I do not have an estimate for 1978, but I think it would be quite close to 1973 so that the 1973–78 growth rate would be close to zero. Assuming it to be

zero, use of my estimates for this determinant with the authors' residual from table 10 would yield the following results:

Period	*Authors' residual*	*Intensity of utilization*	*Other factors*
1948–65	2.16	0.06	2.10
1965–73	1.21	−0.70	1.91
1973–78	1.06	0.00	1.06
Change			
1948–65 to 1965–73	−0.95	−0.76	−0.19
1965–73 to 1973–78	−0.15	0.70	−0.85
1948–65 to 1973–78	−1.10	−0.06	−1.04

Most of the unexplained drop would then appear between the 1965–73 and the 1973–78 period, as in my estimates.

Of course, I do not suggest that the authors would necessarily accept my estimates for the effect of changes in intensity of utilization. For anyone interested, they are fully explained in *Accounting for Slower Economic Growth,* pp. 76–77 and 176–89.

Of the many differences in our procedures, I shall examine only our treatment of capital, which is likely to be of most interest.

One important point is that capital should play a larger role in the authors' scheme than in mine for two reasons. First, the authors measure gross output, and I focus on net output. Because the change in depreciation can only be counted as a contribution of capital to GNP growth, their capital contribution should exceed mine by the value of depreciation. Corresponding to the difference in output measures, I use net income as the weight for structures and equipment, and they use gross income. Also, the authors include the contribution of tenant-occupied dwellings because they are part of the private business sector.

A second point is that I believe the authors inadvertently omit hotels, motels, and other nonhousekeeping residential structures from the capital stock. In other respects our capital stock estimates appear to be consistent.

Third, they deduct from capital input the capital needed for pollution abatement. This classification has the effect of making capital contribute more to the productivity drop than it does in my classification, where the corresponding drop is classified as the result of requirements for pollution abatement. Their capital deduction may be incomplete insofar as invest-

ment is made by a firm other than the one whose pollution is being abated. They apparently neglect to make a similar deduction from labor input.

Fourth, the authors use net stock to measure capital input whereas I use a modified gross stock series (gross stock with a weight of 0.75, net with a weight of 0.25). This difference gives the authors a sharper drop in the capital contribution from 1965–73 to 1973–78.

In my calculations, the growth rates of nonresidential structures and equipment per person employed were as follows:

	Capital stock measure		
Period	*Gross stock*	*Modified gross stock*	*Net stock*
1948–65	2.55	2.70	3.15
1965–73	2.18	2.30	2.63
1973–78	1.33	1.23	0.89
Change			
1948–65 to 1965–73	−0.37	−0.40	−0.52
1965–73 to 1973–78	−0.85	−1.07	−1.74

The drop from 1965–73 to 1973–78 is only three-fifths as large on my basis as on their net stock basis. It would be only one-half as large as theirs if gross stock were used—the most common practice. In their paper, the authors generously state that my measure may be more reasonable. It is unusual for the choice among these measures to make so much difference.

And fifth, because I analyze output per person—and the authors, output per hour—it is natural that I divide capital by employment and they by total hours. However, I believe that dividing by employment is more appropriate. Under most circumstances, it seems doubtful that shortening average hours of work reduces requirements for fixed capital and land, or even reduces inventory capital requirements proportionally. It is more commonly held that the capital required per work station (for example, one's office and desk) is typically constant regardless of the length of the workweek.

Finally, I can compare our main results on the effects of capital. I estimate a drop of 0.31 percentage point from 1948–73 to 1973–78 in the contribution of capital and land. Combining the authors' first two periods in their table 10 to obtain 1948–73 estimates shows they obtain

a drop from 1948–73 to 1973–78 of 0.57 point in the combined contributions of what they call the "capital-labor ratio" and "asset composition," the combination that seems to come closest in concept to my estimate. It seems likely that the difference of 0.26 point between the drops could easily be explained by the difference in measuring fixed capital input (my fourth point above) and their use of gross weights alone (my first point). The authors have additional drops of 0.08 point due to pollution abatement capital, which I count elsewhere (my third point), and 0.11 point ascribed to "intersectoral shifts."

In both the authors' calculations and in mine, the big drop in the capital contribution comes between the 1965–73 and 1973–78 periods.

Michael L. Wachter: The paper by Norsworthy, Harper, and Kunze is an excellent and fully documented contribution to the literature on the productivity slowdown. Its importance to the productivity debate is as much due to its lack of success in explaining the decline as to its positive findings. That is, it argues that some key elements are unimportant—for example, labor quality and the differential between hours worked and hours paid, which have been used by other researchers to explain productivity changes. Indeed, these results are likely to be the most controversial aspect of the paper.

The authors' approach is to divide the postwar years into three periods: 1948–65, 1965–73, and 1973–78. They argue for this division on the basis of the hypothesis that the productivity slowdown began in 1965, but that the underlying causes changed around 1973. For the 1965–73 period, their findings are completely negative; the "residual" or what they call "other factors" captures the entire change. For the later years, they attribute an important part of the decline in productivity to the slowdown in the growth rate of the capital-labor ratio.

The fact that the residual category accounts for the entire productivity slowdown in 1965–73 creates an incentive to identify the residual with some term that has economic significance. Some researchers, especially in the related field of estimation of the production function, tend to identify the residual with changes in knowledge or technological change. The authors avoid this temptation; they argue against the significance of improvements embodied in capital (which would appear in their residual term) and take an agnostic stance toward the importance of disembodied technological change.

The authors' conclusions, especially for the 1965–73 episode, are surprising because most researchers find some evidence that labor quality or interindustry shifts are important. For example, in some of my work with Jeffrey Perloff we find that an adjustment for age and sex in the labor force plus a cyclical adjustment explains over half of the decline for the overall economy. Indeed, productivity growth, adjusted for demographic and cyclical factors and disaggregated into the eight major nonfarm sectors, even increased slightly in a few sectors.

The difference in results is largely because of the nature of the authors' labor quality adjustment. They include years of schooling as well as age and sex to adjust for demographic shifts. They do find a strong negative effect from age and sex, but it is erased by a strong positive effect from education. That is, they argue that knowledge improvements may not be embodied in capital, but embodied knowledge is important in measuring labor input.

That education has a strong empirical positive effect is not surprising because years of schooling have been increasing and schooling is associated with higher wage rates. But it is also recognized that years of schooling is an imperfect measure of labor quality. I believe that the education correction in the paper may be inappropriate. A basic question in this type of analysis, as in empirical work in general, is what variables to include in the productivity accounting mechanism. Although there is a precedent for adjusting for age, sex, and education, the addition of other variables might "wash out" the effect of years of schooling. The notion that the quality of elementary and high schools is deteriorating may be too widely accepted in the conventional wisdom to be believable, but there may be something to that story for the 1965–73 period. Unfortunately, there are no obvious variables to include to capture this quality effect exactly. The tendency to remain in school to avoid the Vietnam War or to avoid the labor market that is overcrowded with youth may have contributed to increased schooling but not increased productivity. Other factors, such as the decline in expenditure per pupil (after adjusting for the increase in relative teacher salaries), racial and regional composition effects, and the decline in Scholastic Aptitude Test scores may also indicate reduced quality of education per year of schooling on the aggregate level. Although school quality itself cannot be included, the other variables mentioned above can be measured and used in the productivity calculations.

More generally, once years of schooling is used as an explanatory variable, balance suggests including other potential personal characteristics that may be related to productivity changes. If the additional variables are unimportant, they will presumably have a quantitatively small effect. Omitting them only means leaving their potential influence in the "other factors" category.

For the second productivity decline, 1973–78, the authors find that the capital effect, primarily the decrease in the growth rate of the capital-labor ratio, is the main contributing factor. In the nonfarm business sector it explains over 75 percent, and in manufacturing about 50 percent of the slowdown. Because the capital stock data are best in manufacturing, the latter figure may be more reliable. This is especially the case given relative capital intensities across sectors.

I should add that, to the extent that one adjusts labor quality for age and sex but not for education, the 1973–78 slowdown appears to be even more dramatic than that isolated in the paper. That is, the age-sex adjustment helps explain the pre-1973, but not the post-1973 slowdown. The published data suggest a gradual decline from 1965 to 1978. The speed of the decline is slightly faster after 1973 than before 1973. If the age-sex adjustment "explains" the 1965–73 developments but not the post-1973 events, then the demographically adjusted productivity series has a sharper decline after 1973 than the published or the authors' adjusted productivity series.

As the authors make clear, their approach does not explain the productivity slowdown; rather it attributes to the production inputs that part of the slowdown paralleling changes in input growth rates. As a result, although the slowdown in the capital-labor ratio is the key variable in the last period, explaining the changes in capital and labor are beyond the scope of their paper. Capital-labor ratios that grow more slowly than in earlier years are compatible in a general equilibrium context with either capital that grows "too slowly" or labor that grows "too fast." They seem to argue that the former view is correct; that is, the problem is a slowdown in capital accumulation. This conclusion, however, cannot be tested using their approach. The issue of causality between labor and capital effects requires an analysis of input demand functions in a dynamic context. The underlying question is what factors have caused the deviation in the growth rates of the production inputs from previous rates.

In their conclusion the authors suggest that energy prices may be a

key factor, but this is largely speculative. No rigorous test of the energy hypothesis is offered and, indeed, the evidence in the literature is mixed. Although energy prices increased, the quantity of energy used declined only slightly. In their input imputation, the quantity of energy is the correct variable. The 1979 energy crunch will at least provide a second data point to test the hypothesis.

A problem in analyzing the 1973–78 productivity slowdown is that it coincides with the major cyclical downturn of 1974, and therefore the cyclical correction factor becomes an important part of the productivity story. On this question, the key is which notion of the equilibrium unemployment rate is important. If the correct rate is the demographically adjusted rate, which is approximately 5.5 percent, then the recovery has been incomplete. If it is the rate that results from inverting the Phillips curve, which is approximately 6.3 percent, then the economy has had a complete recovery. My view is that for productivity analysis, the equilibrium unemployment rate from the inverted Phillips curve is not relevant. Indeed, it may well be a function of the same elements that contribute to the slowdown in productivity. Inflation, productivity, and equilibrium unemployment rates are jointly determined dependent variables. As a result of the importance of cyclical forces between 1973 and 1978, the productivity slowdown is best evaluated in a dynamic cyclical model.

The productivity problem is conceptually and empirically complicated. Because of the inherent data limitations in testing certain hypotheses (for example, the effects of pollution abatement), it remains necessary to investigate this question using several different techniques. The paper is an important contribution to the input imputation approach to the productivity slowdown. It obviously cannot be expected to deal with all the major, extant hypotheses because of the varied empirical approaches that would be necessary.

General Discussion

Several participants discussed the cyclical adjustments that made so much difference between Edward Denison's results, as reflected in his comments, and the results of Norsworthy, Harper, and Kunze. William Nordhaus questioned the subperiod results of Norsworthy, Harper, and Kunze because they ignored any such cyclical adjustments at their end-

points. He suspected that a downward trend in productivity growth may have characterized most of the postwar period and that the process had none of the distinct breaks that a subperiod analysis seemed to show.

Robert Gordon pointed out that a cyclical adjustment based on factor shares would be misleading if a permanent change in shares was taking place during the latest interval. Studies such as Jeffrey Sachs' paper in this issue raised the possibility that such a change in the trend of shares was part of the inflation process. A cyclical adjustment based on tightness in the labor market would give a very different adjustment from the one Denison uses based on income shares, making 1965 and 1973 years of roughly comparable cyclical performance and making 1978 a year of considerably lower utilization.

George Perry stated that either basis for adjustment would require a benchmark. Although some analysts might feel more confident about benchmarking on some unemployment concept, Denison's method of benchmarking to an income-shares concept had the virtue of adjusting appropriately for productivity residuals around the benchmark. In a year when the residual made productivity exceptionally high given the level of output, the nonlabor share would be exceptionally high and so would unemployment. Denison's adjustment would correctly scale back productivity in estimating its trend value, while the unemployment adjustment would raise it, adding further to the residual in that year.

Norsworthy expressed two specific doubts about the trend in the nonlabor share used in Denison's adjustment. The shift in the composition of the capital stock toward more equipment substantially increased the annual depreciation of the capital stock and put a downward bias on Denison's measure of nonlabor payments. An additional bias in the same direction occurred from the correction to profits for depreciation toward a replacement cost basis. All in all, he found Denison's 0.7 percentage point adjustment to annual productivity growth between 1965 and 1973 implausibly large, implying a 6 percent adjustment between the productivity levels in the two years. Norsworthy reasoned that it was impossible to derive a cyclical adjustment with which one could feel confident. But the adjustment he would have made would have been so small in the endpoint years of the Norsworthy-Harper-Kunze study that it would have no appreciable effect on the average annual growth rate over the subperiods.

The uncertainty about the causes of any productivity slowdown that remains from aggregate studies led some participants to urge disaggre-

gated analysis as the only way to get closer to what is actually happening. Robert Hall noted that studies of the coal industry had isolated weak productivity performance to the unionized part of the industry and found little effect on productivity from federal health and safety standards. William Nordhaus added several examples from other industries: productivity growth in electrical utilities slowed as that industry exhausted economies of scale. Productivity in the mining of oil and natural gas slowed as increasingly less productive fields were developed and put into production. If adhered to, the 55 mph speed limit substantially reduces productivity in the trucking industry. And environmental regulations made it difficult if not impossible to build entirely new facilities in the steel industry, eliminating investments that might have offered substantial productivity improvements. In a different vein, James Duesenberry observed that rapid productivity growth at the industry level was commonly correlated with rapid growth in output. If the proportion of industries growing rapidly had declined, the instances of exceptional productivity growth would be missing from the averages.

Out of the range of estimates that Peter Clark provided for the effect of capital formation on productivity, Nordhaus found the lower end of the range most plausible because it was based on gross stock measures of capital, which he believed corresponded to the way capital actually entered the production process. He also noted that the slowdown in the growth of total demand in the latest subperiod might fully explain the slower growth of the gross capital stock.