Editors' Summary

THIS ISSUE of the *Brookings Papers on Economic Activity* contains articles and discussions presented at the fifty-eighth conference of the Brookings Panel on Economic Activity, which was held in Washington, D.C., on September 8 and 9, 1994. The articles span a range of topics: the effects of tax changes on business investment; liquidity constraints as a determinant of inventory fluctuations; the effects of not coordinating fiscal and monetary policies; the increasing transitory variance of earnings; and the likely contribution of computers and related equipment to productivity.

BUSINESS FIXED investment, although accounting for only about 10 percent of gross domestic product, is highly volatile and plays a central role in the economy's cyclical fluctuations. The share of business investment in GDP is also a major factor in explanations of long-run growth in output and productivity. It is little wonder that no other component of aggregate demand has received more attention from theorists, econometricians, and policymakers. Structural models of investment are essential for economists and policymakers to predict the effect of policy changes or other economic events. Yet the most prominent structural models of investment demand, the neoclassical user cost model developed by Dale Jorgenson and the q model proposed by James Tobin, have not been notably successful in explaining actual investment behavior, arguably doing no better than ad hoc models emphasizing cash flow or sales. Many explanations have been offered for this lack of success. Some center on measurement problems, especially difficulties of measuring current and expected future values of variables used in constructing user cost and of distinguishing between marginal and average valuations in calculating q. Others center on the difficulty of identifying exogenous shifts in the marginal profitability of investment, user cost, or market valuation.

In the first paper of this issue, Jason E. Cummins, Kevin A. Hassett, and R. Glenn Hubbard attempt to deal with these difficulties by exploiting the cross-sectional variation in the impact of tax reforms on investment incentives. By focusing on periods of major tax changes, the

authors hope to reduce the importance of measurement error by increasing the signal-to-noise ratio in the data. And, by using cross-sectional data, they expect to avoid the problems of endogeneity of interest rates, market values, and, indeed, tax changes themselves, all of which contaminate time-series studies of aggregate investment.

The authors begin by identifying the major changes in the tax code affecting investment between 1962 and 1988, a period in which there were 13 significant changes in either the statutory rate, the investment tax credit, or depreciation allowances. With the exception of surcharges from 1968 to 1970, the statutory tax rate was reduced steadily from 52 percent in 1962 to 34 percent in 1988. The investment tax credit, in effect for most of the 1962–86 period, was increased three times but was also suspended from October 1966 to March 1967 and from April 1969 to August 1971. Depreciation allowances were first made more generous and then again limited.

The authors summarize these changes in the tax code by calculating their effects on the tax wedge relevant to the user cost of capital and q. Since the authors are most interested in changes in the code that have different effects across firms, they focus on the present value of tax savings from depreciation allowances and the investment tax credit, making separate calculations for structures and equipment. The variations in the tax wedge are substantial. Using a 10 percent change in the calculated tax wedge as a cutoff, the authors identify as major reforms the legislation enacted in 1962, 1971, 1982, and 1986. They assume these changes were largely unanticipated in the year prior to the change. At the cross-sectional level, the key variation comes from the taxation of different asset classes. The authors calculate the tax wedge for 22 classes of equipment and 14 types of structures. They find significant variation in the tax wedge across the asset categories through most of the sample period, but a substantial reduction in this variation, a "leveling of the playing field," following the Tax Reform Act of 1986.

As a preliminary test, the authors examine how investment by asset class has responded to tax-induced changes in the cost of capital. Focusing on the years of significant tax reform, they expect and find that surprises in investment are negatively correlated with surprises in user cost for the 22 equipment and the 14 structure categories of assets. They then turn to the formal user cost and q models of investment, following the same general strategy of estimating the relationship between surprises in investment and surprises in user cost. The authors use the theoretical

models to specify precisely the way in which taxes should enter the investment equation, and they employ more sophisticated econometric methods to distinguish expected from unexpected changes in investment and user cost and to control for other variables that may affect investment.

To estimate the investment surprise for each firm, for both total investment and investment in equipment, they use a first-stage equation to forecast investment in the year following a change in the tax code using as instruments only information available the year before the change—time and lags of the firm's investment and cash flow. The difference between a firm's actual investment and this forecast is the variable to be explained by tax surprises. To construct the surprises in user cost and tax-adjusted q for each firm, the authors use their own estimates of an individual firm's depreciation rates and construct a firm-specific required rate of return using Compustat data on a firm's interest expenses and debt structure. These surprises are typically much larger for years that bracket a major tax reform but are nonzero in other years, both because of tax code changes and because of changes in firms' tax status. While recognizing that their calculations involve substantial measurement error, the authors believe that they better capture the firm-specific investment incentives than would alternatives, such as industry averages.

The authors' preferred equation relates these surprises in investment and tax effects for each year in their sample. In accordance with their expectations, q and the user cost are highly significant in tax reform years. Also as expected, the coefficients tend to be insignificant in years without reform. The q equation explaining total investment yields estimates of the speed of adjustment, and hence investment costs, that are an order of magnitude different from previous studies and, arguably, far more plausible. At the mean investment rate, the implied adjustment costs are roughly 5 percent to 12 percent of the cost of an investment. The q equation results for manufacturing alone are similar. The inclusion of cash flow lagged twice, although it is significant and reduces the magnitude of the q coefficients, does not qualitatively change the results.

Although, in principle, it is possible to calculate tax-adjusted q separately for plant and for equipment, in practice such calculations are likely to involve significant measurement error. So when the authors disaggregate to equipment alone, they only estimate a user cost equation;

this not only provides estimates that are of intrinsic interest but enables them to check the consistency of the two models. They find that the mean of the user cost coefficient over the 13 tax reform years is much larger than in previous studies, corresponding to a coefficient on tax-adjusted q of about 0.16. This is less than half as large as their estimates for the effect of q on total investment and implies larger adjustment costs than the total investment equation.

The authors recognize potential problems in using cross-sectional results to explain aggregate behavior. Nonetheless, they show what their results imply about the likely effect of tax reform on aggregate investment. Their estimates suggest an elasticity of aggregate equipment investment with respect to user cost of approximately -0.7. This implies that the 1962 tax reform increased investment by about $7 billion in 1987 dollars. While the authors' findings suggest that long-lasting changes in corporate taxation can significantly affect the level of business fixed investment, they note that the standard deviation of investment is roughly four times the mean predicted effect of the major tax reforms during the postwar period. Other factors obviously play a major role in determining investment, and explaining the remaining variation in investment is likely to prove more challenging than estimating the effect of taxes.

THE BUFFER STOCK model of inventory investment, based on the idea that firms vary inventories so as to smooth production in the face of surprises to their sales, has not fared well in explaining the broad facts of aggregate inventory behavior. The model predicts inventory investment will be negatively correlated with sales surprises and that production will vary less than sales. Yet empirical studies find that aggregate inventory investment is positively correlated with contemporaneous sales shocks and that, over the business cycle, production varies more than sales. In the second paper in this volume, Robert E. Carpenter, Steven M. Fazzari, and Bruce C. Petersen hypothesize that imperfect capital markets, which prevent firms from borrowing and force them to allocate scarce internal funds among competing uses, are a feature of the economy that helps explain observed inventory fluctuations. Their empirical analysis helps resuscitate the buffer stock model of inventories and illuminates how imperfections in capital markets affect real economic activity.

While all investments should be affected by cash flows in an environment of capital market imperfections, the authors reason that inventory

investment will be affected relatively more than fixed investment or other uses of funds, such as research and development, because the cost of adjusting inventories is relatively low. Using data on individual firms, they test for such a linkage by augmenting a standard inventory investment model with measures of each firm's cash flow. The data are from Compustat's quarterly "full coverage" files for manufacturing firms and cover the years from 1981 to 1992, during which three episodes of pronounced fluctuation in inventory investment took place.

The authors work with a basic inventory model in which inventory investment is the sum of anticipated and unanticipated components. The anticipated component corresponds to adjusting the actual stock to a desired stock that, apart from financial effects, depends on the expected level of sales. The unanticipated component corresponds to the change in inventory stocks that occurs when actual sales differ from expected sales. Higher expected sales raise the desired level of inventories while higher actual sales relative to expectations lower the level of inventories. The authors model expected sales as a function of sales in the previous two quarters and, to allow for expectations that are adjusted within a quarter, sales in the current quarter. Current sales thus enter into both the anticipated and unanticipated components of inventory investment. Since these components have opposite predicted effects on inventory investment, the net effect of current sales on inventory investment expected in empirical estimates is ambiguous. However, this ambiguity does not affect the authors' primary task of identifying the effect of cash flows on inventory demand.

To test the importance of cash flows as a financial determinant of inventory investment, the authors add them to the estimation model, with the same lags as sales. Since sales are an important determinant of cash flows, using the same lags on each guards against the possibility that effects estimated for cash flows merely reflect the effects of sales that are correlated with cash flows. At the same time, the firm-level data provide ample variation in cash flows relative to sales to permit estimating their separate effects on inventories. The data also permit the authors to include dummy variables for fixed time effects at the four-digit industry level as a way of controlling for industry cost and technological shocks as well as for seasonal variation. To properly scale the individual firm's data for estimation, sales, cash flows, and inventory changes are all divided by the total assets of the firm.

The authors split their data into three panels, each of which starts at

a peak in aggregate inventory investment and includes a pronounced inventory cycle: 1981:1–1983:4, 1984:1–1988:3, and 1988:4–1992:4. They also divide their sample into small and large firms, with a dividing line of $300 million in assets, as a way of testing whether financing constraints are more important for smaller firms, as most models of capital market imperfections would predict. There is a pronounced difference in the characteristics of small and large firms defined this way, with the median large firm more than ten times the size of the median small firm measured by either employment, total assets, sales, or inventories. However, even the small-firm subsample has median firm sales of $19 million and median firm assets of $63 million in the latest period, both in 1987 dollars. Thus, the sample does not include very small firms, such as venture startups, which have often been thought to have especially limited access to external finance.

The regression results clearly support the authors' hypothesis that inventory investment is affected by cash flows; there is also evidence that the importance of cash flows is greater for small firms. In the basic regressions, cash flow effects are sizable and significantly positive for small firms in all three subperiods; they are consistently smaller for large firms, and not always significantly positive. For small firms, but not always for large firms, the regressions including cash flows produce a negative coefficient on current-period sales. Thus, the effect of sales surprises on inventories predicted by the buffer stock model comes through in these estimates with quarterly data.

All these findings are repeated when the basic regressions are modified by using a separate dummy for each possible year-quarter combination in each four-digit industry, thus effectively estimating the coefficients from idiosyncratic firm variation alone; the differences between large and small firms are more pronounced in these regressions. The estimated effects of cash flows are not affected when stock price growth and leads on sales—additional variables that should reflect expectations of future sales—are added to the regressions. This further supports the interpretation of cash flows as financing constraints rather than reflections of sales expectations. The importance of cash flows remains when the equations are estimated using instrumental variables and when changes in short-term debt and in cash and equivalents are added to reflect other dimensions of finance availability. Finally, when the firms in the data set are divided according to bond ratings, the results parallel those obtained with the large firm–small firm split; cash flows are more

important for firms with lower ratings, presumably because such firms are more likely to be credit constrained.

Armed with this evidence, the authors go on to offer some rough judgments about the possible importance of internal finance for aggregate inventory investment. They estimate the departure of cash flows from trend for both large and small firms in the recessions of the early 1980s and early 1990s. Applying these shortfalls to the coefficient estimates for each period, they calculate that internal-finance constraints could account for nearly half the inventory investment shortfall in the early 1980s and about a quarter of the shortfall in the early 1990s. But they point out that if inventories were somehow better insulated from financing constraints, so long as these constraints were still present, they would affect other activities of the firm, possibly having the same effect on aggregate economic activity through different channels.

MONETARY AND FISCAL policies, the main instruments for affecting the macroeconomy, are conducted independently in many countries, including the United States. Many economists endorse this separation as a way of optimizing economic performance. At the same time, most economists in countries with such separated authority believe their countries suffer from a policy mix that produces budget deficits and real interest rates that are too high for optimal long-term growth. In the third paper in this volume, William Nordhaus examines how economic outcomes depend on the way the fiscal and monetary authorities interact and how these interactions may help explain the present policy mixes observed in most countries. He shows analytically how the cost of reducing large structural deficits depends importantly on whether policy changes are well coordinated. And he quantifies these costs, making use of empirical models of the economy. He shows that reducing the deficit requires consumption sacrifices for extended periods, especially if policies are not coordinated, while the benefits are deferred even with perfect coordination.

Nordhaus observes that independent monetary and fiscal authorities generally have quite different preferences with respect to inflation, employment, government expenditures, and taxes, though nowhere does he make a presumption about which policymaker's preferences are more appropriate for society. He notes that, in countries such as the United States and Germany that have chosen to establish a largely independent central bank, the monetary authority cares primarily, and at times exclu-

sively, about price stability. By comparison with central bankers, elected officials care relatively more about unemployment and about the taxes and expenditure programs that affect their constituents directly. Although they are also concerned about inflation, the fact that inflation typically lags behind reductions in unemployment leads them to attach relatively more importance to reducing unemployment when elections are on the horizon.

Nordhaus uses game theory to analyze the implications of differences in preferences between the policymakers in the context of a simple economic model. In the model, both fiscal policy, through the budget surplus, and monetary policy, through the real interest rate, affect the levels of demand, output, and unemployment, while inflation depends on unemployment, with a lag, and on the expected rate of inflation. In the longer run, output is higher with a policy mix of lower budget deficits and interest rates. How the two authorities interact is crucial to what outcomes emerge in the game-theoretic framework.

In a cooperative equilibrium, implying coordinated policies, the possible outcomes would be efficient in the sense that neither authority could move to a more preferred set of outcomes without the other moving to a less preferred set. Such efficient outcomes lie on the "contract curve," a line connecting the bliss points, or most preferred positions, of each. Where on the contract curve the economy settles depends on which authority has the greatest say in the cooperative decision. If, for example, the government dominated the cooperative decision, the outcome would be near its bliss point, implying a tendency to counter recessions aggressively in the short run and to experience relatively more inflation and larger deficits, on average.

Noncooperative outcomes are inefficient and span a wider range of possibilities. If each authority sets its policies in response to the state of the economy while ignoring the other authority, the result is a Nash equilibrium in which the deficit and interest rates are both higher than necessary, in that the two authorities would each prefer an achievable coordinated reduction in both.

Nordhaus also considers a number of other possible noncooperative outcomes that arise with alternative specifications of preferences, policy responses, and economic models. Of these, a rule equilibrium is of particular interest. A rule equilibrium can emerge when one authority convinces the other that it will not deviate from a well-defined policy path. Nordhaus shows that, if the monetary authority adopts such a

strategy and the fiscal authority believes it and responds accordingly, the rule equilibrium will be nearer the bliss points of both authorities than is the Nash equilibrium. With his specification, the surplus will be at the level preferred by the fiscal authority, but neither inflation nor unemployment will be affected since monetary policy controls aggregate demand.

Nordhaus suggests that noncooperative models help in understanding the evolution of budget deficits and real interest rates in the United States. In the 1960s, first the Kennedy-Johnson tax cuts, which aimed at spurring expansion, and then the Vietnam war changed the structural budget surplus of the 1950s into a small deficit. Monetary policy eventually raised interest rates to slow inflation. The Nixon and Carter stimulus packages, which again aimed at spurring expansions, added a little more to the deficits in the 1970s. And monetary policy again eventually raised rates to slow inflation. Finally, the Reagan supply-side tax cuts sharply increased the structural deficit in the early 1980s. And monetary policies aimed at slowing inflation led to the highest real interest rates of the postwar period.

Turning from purely analytic models, Nordhaus examines the data for evidence on how the economy has responded to policy changes and how policymakers have responded to the economy in the past. Using vector autoregressions, he finds that shocks to either unemployment or inflation are eliminated only slowly. The federal-funds rate has the expected stabilizing sign, rising in response to inflation and falling in response to unemployment, but these reactions have historically been slow. Monetary policy appears to have no systematic response to fiscal policy, leading Nordhaus to characterize it as results oriented, responding only to actual economic conditions. Fiscal policy shows no systematic response either to monetary policy or to economic conditions in these VARs.

Nordhaus finally uses the Clinton administration's 1993 deficit reduction package to provide a concrete illustration of the difference between cooperative and noncooperative behavior by the Federal Reserve. With noncooperative behavior, the Fed is assumed simply to respond to economic developments the way it has historically. By contrast, under cooperative behavior, it is assumed to take steps to keep the economy on the same path it would have been on without the deficit-reduction package. The author examines the short-run economic consequences of each assumed behavior using alternative equations describing monetary pol-

icy and three different models of the macroeconomy—a "minimodel" he constructs based on plausible assumed parameter estimates, the compact model developed by Ray Fair, and the large DRI model. The costs of noncooperation are shown to be large in all models. Comparing cooperative and noncooperative outcomes, by the third year, failure to cooperate results, on average, in a 0.5 percent higher unemployment rate, a $160 billion cumulative loss in real GDP, and a higher debt-GDP ratio as a result of cyclically higher deficits.

Nordhaus observes that the purpose of deficit reduction is, ultimately, to spur investment so as to raise the sustainable level of consumption, which is measured by real national income—the total of private and public consumption plus the net accumulation of capital, all measured at domestic purchasing power. An important feature of the adjustment to lower deficits is that some part of higher national saving results in higher foreign investment, an initial exchange rate depreciation, and a smaller trade deficit. Earnings on this foreign investment increase consumption in the future, just as domestic investment does. But the depreciation of the exchange rate reduces the U.S. terms of trade, thus lowering the real value of national income.

In light of the importance of these international effects for assessing long-run changes in real national income, Nordhaus supplements projections using the DRI model, by examining the effects of government expenditure reduction using two explicitly global macroeconomic models—the Brook-I version of the MULTIMOD model developed by the staff of the International Monetary Fund and modified by Ralph Bryant and Charles Soludo and the McKibbin-Sachs model. Besides providing a more elaborate treatment of the foreign sector than the DRI model, these models embody many classical assumptions, including flexible-price market clearing and rational expectations. They also include endogenous monetary policies, which the author regards as somewhere between the cooperative and noncooperative cases from the DRI model.

In both these models, total consumption is depressed by deficit reduction for an extended period. In the MULTIMOD model, total consumption declines over the entire 1992–2030 simulation period, as the gradual reductions in public consumption are never overtaken by higher private consumption. In the McKibbin-Sachs model, which can be simulated further into the future, total annual consumption finally begins to rise about 30 years after the deficit reduction. Total undiscounted cumulative consumption exceeds baseline for the first time in 2057.

Nordhaus does not attempt to compare cooperative and noncooperative monetary policies in the MULTIMOD and McKibbin-Sachs models. For that purpose, he uses the DRI model to evaluate the 1993 deficit-reduction package out to 2004. He compares outcomes under the package and alternative assumptions about Fed behavior with outcomes under a baseline case in which fiscal policy does not change. Averaged over 1993–2004, deficit reduction reduces real national income under noncooperative Fed behavior and increases it under a cooperative Fed strategy. The difference is $40 billion to $50 billion a year, depending on how noncooperative Fed behavior is characterized. Total consumption, private and public, is below baseline, even with coordinated policies.

In his concluding reflections on the several parts of his analysis, Nordhaus offers some strong messages. "The monetary-fiscal game combines fiscal authorities who are soft-hearted, work in unstable jobs, and are extremely averse to short-run economic downturns with monetary authorities who are hard-headed, have considerable job security, and are highly averse to inflation. Deficit reduction must be initiated by the group with the shortest time horizons, yet it is likely to produce immediate if temporary unemployment if it is badly timed, and it will definitely incur the wrath of the antitax lobby and other affected interest groups. . . . Given the tastes of the players and the meager and uncertain returns to deficit reduction, it is hardly surprising that major deficit reductions like that of 1993 are a rare and endangered species in today's political economy."

AFTER TWO DECADES of stability following World War II, many observers came to regard the U.S. income distribution as immutable, unresponsive to major shifts in the labor markets or government policy. Beginning in the 1970s, however, the annual earnings distribution began to widen and this process appears to have accelerated in the 1980s. It has been implicitly assumed that these changes in the distribution of income or wages reflect differences in permanent income rather than an increase in transitory movements of individuals within the income distribution. And quite naturally, studies attempting to explain these trends have focused on slow-moving changes in the returns to education, skills, and job experience, with these returns themselves explained by a variety of factors including technological change, international trade, and the decline of unions. In the fourth paper of this issue Peter Gottschalk and Robert Moffitt show that the increased dispersion of incomes reflects

important transitory as well as permanent changes in incomes and argue that the list of candidates for explaining increased earnings inequality needs to be expanded.

One reason most existing studies of the changing income distribution have not distinguished between the distribution of permanent and transitory income is empirical. Typically, these studies have utilized successive cross-sections from the Current Population Survey (CPS), which do not include observations on the same individual through time. In order to disentangle the two sources of dispersion, Gottschalk and Moffitt utilize the Michigan Panel Study on Income Dynamics (PSID), which began annual interviews of about 5,000 U.S. families in 1968. This study follows not only individuals who were in the original households but also individuals who have joined these households. The authors focus on white males, the group most frequently examined in the past, noting that this group has relatively stable labor market experience so that, if anything, their results likely underestimate the importance of transitory fluctuations for the entire population.

The authors begin by documenting that increases in the variance of log (real) earnings in the PSID are similar to those in the CPS. Then, using the PSID longitudinal information on individual's earnings, they decompose the variance of log earnings into permanent and transitory components for two nine-year time periods, 1970–78 and 1979–87. They do so by first estimating a common age-earnings profile for each period and then computing, for each individual, the mean and variance of log earnings around the age profile. The authors measure permanent income variance by the variance of individual means and transitory income variance by the average of individuals' income variance around their permanent income. Both permanent and transitory variances are important, with roughly two-thirds of the variance of log earnings within each subperiod attributed to permanent changes and one-third to transitory earnings. The fraction of men with income in the middle of the distribution has fallen, and the fraction of men in each tail of the distribution has risen. Changes in the distribution of transitory variances have also added to income inequality. The proportion of the white male work force experiencing relatively small transitory shifts in earnings has sharply declined, and the proportion with large transitory fluctuations has increased. Both the permanent and the transitory components of earnings variance increase by approximately 40 percent from the 1970s to the

1980s, so that increases in transitory income variance, measuring individuals' income instability, account for about one-third of the increased dispersion in earnings.

The authors show that the importance of transitory income variation, and the increase in both permanent and transitory sources of earning variance, are important for subsets of individuals classified by age, education, or earnings percentile. Transitory variance is largest, both absolutely and relative to the variance of permanent incomes, for the least educated, the youngest, and the lowest paid. The most dramatic increase in income instability is for individuals with fewer than 12 years of education, for whom the average transitory variance approximately doubled.

The authors recognize that their measure of transitory earnings variance may be biased upward because of measurement error and positive serial correlation. They report that other studies with similar data show that measurement errors are substantial; but they can think of no reason why the importance of measurement errors should have grown so as to bias their estimates of the increase of transitory variance. They also report that the serial correlation in their estimates of transitory earnings is small; only 2 percent of transitory earnings is explained by a regression of transitory earnings lagged one period. The authors find, however, that the allocation of earnings differences to the permanent or transitory category is sensitive to the length of the period over which permanent earnings are estimated. If permanent earnings are estimated by a five-year moving average, differences in permanent earnings account for four-fifths rather than two-thirds of the variance in earnings. Although the relative size of transitory variance is smaller using a shorter time period, its increase is more dramatic, more than doubling over the period 1974–86.

Having demonstrated that transitory earnings fluctuations are both important and growing in their importance to the earnings distribution, the authors explore some potential proximate sources for these developments. Calculating variances year by year, they show that the transitory variances are indeed highly countercyclical, rising sharply from 1974 to 1975 and from 1980 to 1983. However, after 1984, transitory variances continued to rise during the recovery and by 1985 were higher than during the 1980–83 recession. The continued rise of the transitory variance after 1983, at the same time the dispersion of permanent earnings was

increasing, suggests that the same forces may be at work on both sources of inequality. For example, it might be imagined that "good" jobs have both high pay and stable employment and wages, while bad jobs have low and unstable pay. However, the authors note that their evidence is not fully consistent with this hypothesis, since the variance of transitory income has increased even for the highest paid individuals.

To further illuminate the increase in transitory variance, the authors refine their estimates in a variety of ways. They find that the transitory variances of both real weekly wages and annual weeks of work not only increased between the periods but that, after 1984, the increase in the transitory variance is even sharper for weekly wages than for annual earnings. They also explore the importance of changes in industrial composition and unionization. Not surprisingly, the transitory variances were lower for unionized workers than for workers who are not unionized, so that a shift toward nonunion jobs has increased overall variation. Similarly they find, as expected, that the shift from manufacturing jobs into services and trade also accounts for part of the overall increase in transistory variance. In addition, transitory income variances increased within each category—for both unionized and nonunionized workers, and for each broad industrial sector. The authors show that shifts in industrial composition and unionization are relatively minor factors in the increase in earnings instability; 88 percent of the increase is accounted for by changes in the variance within such "cells."

The authors also examine the extent to which increased job changing is responsible for the increased earnings variance. While cautioning that their data provide a noisy measure of transitory income changes for job changers, they report that not only is transitory variance higher for job changers than for nonchangers but that the difference grew over the sample period. Data from the PSID also show an increase in the proportion of respondents reporting a change in job, so that increased job turnover would help explain increased earnings variability. However, the authors report that the CPS data do not show this trend, and they reserve judgment on the importance of this effect.

The authors show that the increase in transitory earnings instability is a general phenomenon, not limited to labor market entrants, to men who change jobs, or to particular educational or age categories. Regressions allowing for age, education, employment status, industry, and the unemployment rate, for the entire sample of individuals, and separately

for those who did not change jobs and those that changed jobs voluntarily, all show a significant increase in variance after 1979. Only involuntary job changers did not suffer this worsening instability.

The authors briefly consider other potential explanations for increased earnings instability. They note that, although temporary jobs have grown and are associated with high earnings fluctuations, the temporary help industry still makes up only slightly more than 1 percent of aggregate employment. It cannot, therefore, be an importance source of the general increase in instability. Similarly, increased job creation and destruction, which may be hidden in the industry and aggregate figures, could be an explanation. However, researchers have not discovered any significant increase in this source of turnover. The authors, while confident that increases in the variance of transitory earnings are important in explaining the increased dispersion of earnings, find the sources of this increase unresolved.

THE SLOWDOWN in productivity growth that started in the 1970s is one of the most puzzling and important economic events in postwar U.S. history. And the continued sluggish pace of productivity growth in the midst of the 1980s revolution in information technology only deepened the puzzle in the minds of many observers. More recently, popular press accounts, along with some academic literature, suggest that information technology is finally spurring productivity growth. In the fifth paper of this volume, Stephen Oliner and Daniel Sichel revisit these issues, examining how much the new information technology could plausibly have contributed to economic growth in the past two decades and what the prospects are looking ahead.

The authors first focus on computers, where most previous analysis has been concentrated, rather than on the broader sector of information technology. They start with a Denison-style growth accounting to calculate the contribution of computing equipment to aggregate economic growth under various assumptions about rates of return. As a base case, the authors assume the net return to computers is equal to that earned by other nonresidential equipment and structures, which averaged 12.3 percent a year from 1970 to 1992. Computers lose value quickly through depreciation and obsolescence. Using data on useful lives from the Bureau of Economic Analysis, the authors estimate that the gross return for computers must be 36.6 percent to achieve the average net return of

12.3 percent. Using these rates of return, they calculate that computer investment contributed only 0.15 percentage point to the annual growth in gross output between 1970 and 1992. The annual contribution to net output, which provides a better measure of economic welfare, averaged less than 0.1 percentage point for the same period.

Given the assumption of normal rates of return in these calculations, the contribution of computer investment has been so small because computing equipment has been only a small share of the total capital stock. As recently as 1993, computers and peripheral equipment accounted for only 2 percent of the net stock of nonresidential equipment and structures. The authors note by way of historical comparison that in 1890, railroads, which some regard as the dominant factor improving productivity in the nineteenth century, accounted for about 18 percent of the U.S. net capital stock.

The authors consider two potential objections to their type of analysis. The first questions the base case assumption that computers earn the same return as other capital, reflecting the belief that computer technology is special and likely to earn a supernormal return. If it does, applying conventional growth accounting and assuming normal rates of return would understate the true contribution of computers to output and productivity. However, the authors calculate that, even with supernormal returns of the magnitude suggested by some recent studies, computers would not have generated a sizable contribution to growth. The second objection asserts that part of the output of information technology is intangible and difficult to measure, leading to an understatement of output from computers. Oliner and Sichel demonstrate, however, that even with generous assumptions about unmeasured output—for example, in the form of unmeasured consumption benefits to users that enjoy their personal computers or the network activities they make possible—the contribution of computers to output growth would still not be huge. The reason that neither supernormal returns nor unmeasured output, under generous but plausible assumptions, lead to large effects on productivity is that the share of computers in the capital stock is simply too small. The authors conclude that there is no computer productivity paradox; computing equipment should not have been expected to have made a large contribution to growth in the past two decades.

The authors go on to expand their inquiry beyond computing equipment, which, they note, misses important aspects of the computer revo-

lution. Hardware must be combined with software and skilled labor inputs to produce output of value. They refer to this joint output as computing services and extend the basic growth-accounting framework to assess its impact. Their basic data include software sales and prices, employment in computer-services industries, and budget shares of information systems departments at U.S. companies. For prices, they construct matched-model indexes for spreadsheet, word processing, and data base programs, using prices from computer magazines. From 1987 to 1993, the authors estimate that prices of PC software (spreadsheet, word processing, and data base programs) fell by nearly 3 percent a year. They note that this is much slower than the decline in BEA's deflator for computer hardware, though they acknowledge that their estimate may underestimate the true rate of decline. Recognizing this and other measurement uncertainties, the authors present rough estimates showing that the growth contribution of computing services averaged about 0.4 percentage point annually over 1987–93, about double the figure for hardware alone over this short period.

Oliner and Sichel next broaden the scope of their paper by considering investment in the broad category of information processing equipment as defined by BEA. This extension recognizes that the information revolution has affected equipment other than computers, but also necessarily brings into the analysis more conventional equipment not associated with any frontier technologies. The broader category accounted for nearly 12 percent of the total nominal net stock of nonresidential equipment and structures in 1993. Under their neoclassical assumptions, this broader aggregate contributes about 0.3 percentage point to growth from 1970 to 1992, about double that of computers alone over this long period. If all the pieces are added together, including computers and other information processing equipment, software, and computer-services labor, the total growth contribution over the 1987–93 period for which data are available comes to a noticeable 0.50 percentage point. The traditional focus on computer hardware alone has been too narrow.

Oliner and Sichel go on to ask how much computing services may have contributed to the most recent pick-up in productivity, which most observers associate with downsizing and cyclical recovery, and whether faster gains in productivity may be in store in the future. Because the share of computer services is still not large, growth accounting still assigns it a modest contribution to the economy's performance in 1992 and

1993. And, looking ahead, the authors show that, if computer hardware and software earn the same net return as other assets, the contribution of computing services to growth does not increase dramatically in the next decade, even with very rapid growth in hardware, software, and labor inputs.

The authors note that some analysts, such as Paul David, believe that the big productivity gains from information technology are still to come as people are only now learning to use this equipment effectively, and, as radically new applications are developed, rates of return to computer investment in the future are likely to be much higher than in the past. To assess such a scenario, the authors assume that the net return earned by hardware and software rises rapidly from about 13 percent in 1993 to 50 percent by 2003. Under such a "takeoff" scenario, computer services would generate a substantial contribution to productivity over the next decade. However, Oliner and Sichel remain skeptical of such an optimistic view. They note that there is simply no direct evidence that the returns to computer services have jumped, and there is no speedup in the trend growth rate of the computer capital stock such as would be expected if firms had finally discovered how to achieve exceptional returns from investing in the information technology.