Editors' Summary

THIS ISSUE OF THE Brookings Papers on Economic Activity contains papers and discussions presented at the forty-sixth conference of the Brookings Panel on Economic Activity, which was held in Washington, D.C., on September 15 and 16, 1988. The first major paper focuses on the post-1973 slowdown of U.S. productivity growth and on the extent to which measurement problems may have distorted our perception of the slowdown. The second paper challenges the validity of the widely accepted natural rate hypothesis and argues that demand management *can* affect an economy's long-run average level of output and unemployment. The third major paper presents a new model of labor turnover in the United States and relates it to competing macroeconomic theories. The first of four reports in this issue examines capital gains taxation in the United States, with emphasis on the revenue consequences of cutting the capital gains tax rate. The second challenges prevailing hard-landing scenarios associated with the U.S. budget and trade deficits. Two final reports assess the dangers and benefits of debt buybacks, an increasingly popular debt-reduction strategy of LDC debtors.

THE SLOWDOWN in U.S. productivity growth that began in the early 1970s continues to puzzle analysts and concern policymakers. Despite a marked improvement in productivity growth in manufacturing in the 1980s, aggregate productivity growth in this decade is still far slower than it was in the first quarter century after World War II. Outside manufacturing, growth in labor productivity has averaged near zero in the 1980s, and multifactor productivity, the residual after the contributions of both labor and capital to output growth have been allowed for, has actually declined. In the first paper of this issue, Martin Neil Baily and Robert J. Gordon look at individual industries to see where the productivity growth slowdown has been concentrated and attempt to uncover measurement or conceptual problems that are likely to have distorted measures of output and productivity.

Baily and Gordon show that the post-1973 slowdown in productivity growth has been widespread. Among major industries in the nonfarm business sector, only the nonelectrical machinery industry, which includes computers, showed a speedup in productivity growth. Because of the rapid technical progress in computers, productivity in nonelectrical machinery rose 6.8 percent a year during 1973–87. Since 1979, it has risen 11.5 percent a year. Because of the pervasiveness of the productivity slowdown in all other industries, there is not much scope for explaining the aggregate slowdown through compositional changes. Indeed, the authors show that aggregation problems arising from the changing importance of different sectors of the economy account for only 0.2 percentage point of the total 1.6 point post-1973 slowdown in aggregate productivity growth. They also review studies of the effect on productivity of changes in labor quality. Although these studies suggest a range of plausible adjustments that could be made to a simple aggregation of labor hours, the authors conclude that at most 0.3 percentage point of the slowdown is due to changing labor quality.

In popular discussion, the fact that poor productivity growth has coincided with the computer revolution is a special puzzle, and Baily and Gordon give computers special attention. They review how computers are accounted for in the national income and product accounts (NIPA) and explore some of the consequences of this accounting. In NIPA, real output is calculated by dividing current-year spending by current-year prices relative to 1982 prices. In the case of computers, where technical progress has been so rapid, prices are adjusted for quality change. The enormous improvements in computational power mean that, in effect, the price of today's desktop is compared with the price of a small mainframe some years ago. Because most computers and related equipment are part of the business capital stock, this procedure, as compared with measuring the real value of computers by the resources needed to build them, has the effect of showing the technical progress as a larger capital stock. As Edward Denison has observed, this credits capital accumulation rather than technical progress in accounting for aggregate economic growth.

Whenever the relative price of a commodity declines, the use of base-period prices in calculating real output overweights the importance of the commodity in years following the base year and underweights it in preceding years. The substantial adjustment for quality change means that prices for computers are calculated to have fallen dramatically. Between 1969 and 1986, the share of office computing and accounting machines (OCAM) in total current-dollar spending on equipment did not quite double, while their share of real spending on equipment, measured in 1982 prices, rose fourteenfold, from 1.6 percent to 22.9 percent. This real spending on OCAM has dominated total real investment in producers' durable equipment. While total real PDE spending grew 2.6 percent a year during 1979–87, PDE less OCAM actually fell 0.4 percent a year over this period. Baily and Gordon show that an alternative accounting treatment, based on a chain-weighted price index for investment that weights the growth of computers by their share in current spending each year, produces a noticeably lower estimate of total investment growth in the 1980s. In fact, they show that the recent growth rate of total real GNP would appear 0.8 percent a year slower using a chain-weighted price index to deflate nominal GNP in place of the base-year NIPA method.

None of these calculations shows that the NIPA method is wrong; rather, they show that any index of prices and output has limitations when relative prices change. Baily and Gordon note that the massive decline in the relative price of computers and increase in the use of computer services presumably correspond to a declining marginal productivity of computers in use. It is commonly observed that many machines sit largely idle in offices today while a computer with the same technical characteristics was used intensively a decade or two ago, when it was a scarcer, more expensive resource. Nevertheless one would still expect substantial positive productivity gains in user industries rather than the slowdowns that are recorded. To explore these slowdowns, the authors look closely at several industries outside manufacturing in a search for measurement and conceptual errors that could be distorting productivity data. The search for errors can be thought of as involving two kinds of issues. One is proper accounting for quality changes in output and input. A second is the use of appropriate price indexes for deflating nominal outputs and inputs, quite apart from adjustments for quality.

Baily and Gordon look at several industries within the broad finance, insurance, and real estate (FIRE) sector, which, in total, shows a substantial decline in measured productivity growth during 1973–87. In real estate, which accounts for roughly half the output of the FIRE sector, productivity growth slowed by 4.0 percentage points from its 1948–73 pace, actually falling at a 0.8 percent annual rate during 1973–87. Industry output consists of rental income and realtors' commissions, with nominal output deflated by rental cost indexes. The authors note that these rental indexes do not adjust for quality improvements in the property being rented and so may substantially overstate price increases and understate output and productivity growth in the industry. For the insurance industry, the official data are based on deflators for the industries covered by insurance—auto repair costs for auto insurance, medical costs for medical insurance, and so forth—that bear no relation to the activity of providing insurance. The authors show that the insurance industry has invested heavily in new capital and reason that computerization should have yielded substantial benefits to industry productivity and led to below-average increases in the industry's deflator. The official industry deflator actually rose faster than the deflator for total GNP, which suggests it understates output and productivity growth in insurance. For other financial services industries, output is measured by labor input, so that productivity gains are absent by definition. Baily and Gordon show that, as the insurance industry has, the finance industry has invested heavily in new equipment and should have benefited substantially from computerization. They then offer an assortment of evidence on activity in the financial sector that reinforces their belief that productivity has been improving in the industry.

Although their analysis of these major industries within the FIRE sector convincingly documents the need to improve measures of output, and creates a strong presumption that output and productivity growth have been understated, the authors cannot provide alternative measures or show how much of the recorded productivity slowdown is accounted for by mismeasurement in these industries. However, they provide an illustrative order-of-magnitude calculation suggesting that productivity change in FIRE was understated by 1.1 percent a year pre-1973 and by 2.3 percent a year thereafter, with the difference accounting for two-thirds of the 1.8 point slowdown between the two periods in the official statistics for those industries.

In the official data for construction, after rising 44 percent between 1948 and 1967, productivity declined 20 percent between 1967 and 1972 and 20 percent more between 1977 and 1986. This improbable pattern, Baily and Gordon suggest, makes the construction industry the most obvious candidate for a correction of measurement error. They reason that errors in measuring construction output arise from some combination of three sources: undercount of nominal new construction, overstatement of the construction deflator, and overcount of materials used on construction that are subtracted from the value of construction in calculating value added in the industry. They show further circumstantial evidence from Canadian data in which construction productivity closely parallels that in the United States from 1948 to 1967 but diverges thereafter, rising a further 21 percent to 1986 while U.S. construction productivity declines nearly 40 percent. They also discuss available evidence on the quality of construction that suggests that the existing deflators rise too much, thus contributing to the understatement of output and productivity growth.

In retail trade, Baily and Gordon provide an extensive discussion of factors that may understate quality change in the form of convenience. But they conclude that the greatest increase in convenience came with the development of supermarkets. Because this predates the productivity slowdown, it goes against rather than toward explaining the slowdown in retail trade as a mismeasurement of quality change.

The authors identify a conspicuous source of mismeasurement in the transportation sector. The NIPA deflator implies that air fares almost tripled from 1972 to 1986. By contrast, a direct measure of revenue collected per passenger-mile rose only 60 percent over the same period. Baily and Gordon attribute the difference to the failure of official data to allow for the growing importance of discount fares. However, they caution that so many other changes have taken place in the quality of transportation services that no overall presumptions about bias in output and productivity growth exist for this sector.

Baily and Gordon emphasize that measurement problems, even severe ones, may not contribute to explaining the aggregate productivity growth slowdown. For one thing, if a measurement bias existed both before and after 1973, it would distort measured productivity growth in both periods, but not the slowdown between them. For another, if a measurement problem understates output growth in an industry producing intermediate products, correcting that bias would add to productivity in that industry but reduce it in industries using its output. Aggregate productivity growth would be unaffected. Thus, to help explain the aggregate productivity slowdown, an error must affect the growth, not just the level, of output or input; it must affect final, not just intermediate, output or input; and it must affect growth differently during the period of productivity growth slowdown than before. Although their industry studies point to important problems in the official statistics for several industries and suggest ways in which they can be improved, they do not provide a basis for dismissing, or even substantially reducing, the aggregate productivity slowdown that appears in the official data.

DURING THE quarter century following the Great Depression and Keynes's *General Theory*, the view that demand management can significantly affect macroeonomic performance gradually became the dominant belief of economists and laymen alike. But experience with accelerating inflation in the late 1960s and the stagflation following the 1973 oil shock eroded the confidence that active demand management was up to the task of stabilization. New classical models were developed that challenged the Keynesian orthodoxy and implied that policy would be ineffective in stabilizing the economy. While subsequent research and the experience of the late 1970s and 1980s have revealed deficiencies in these new classical models and a New Keynesian counterrevolution is in full swing, some central propositions of the new classical models have become part of a new orthodoxy and are accepted even by many Keynesian economists. The natural rate hypothesis, with its corollary that demand management cannot affect an economy's long-run average level of unemployment or output, is one such proposition. In the second article of this issue, J. Bradford De Long and Lawrence H. Summers raise questions about the validity of this hypothesis and argue that demand policies can and do affect not just the variance of output, but its average level as well.

De Long and Summers emphasize the importance of distinguishing the view that transitory shocks, coming from policy or elsewhere, simply cause fluctuations around the natural rate of unemployment—the business cycle fluctuates around a policy-invariant average trend of output—from the view that the business cycle consists of repeated transient and potentially avoidable lapses from sustainable levels of output. In the latter view, good policy can fill in cyclical troughs in output without shaving off peaks, yielding first-order gains. According to De Long and Summers, Keynesians' acceptance of the view that business cycles are fluctuations around supply-determined trends traps them into fighting for the "low ground." Once this view is accepted, policy, at best, can merely decrease the variance of output, with welfare gains that are likely to be second order.

The authors recognize that the natural rate hypothesis is built on the plausible idea, attractive to economists who have typically been trained in the competitive equilibrium paradigm, that economies operate at a unique natural level of employment and output, disturbed only temporarily and symmetrically by shocks. But they offer a variety of theoretical reasons why shocks may, in fact, affect the mean level of output. One is that shocks may have asymmetric effects, as, for example, in theories that link cyclical fluctuations to credit problems. Bank failures can have a negative effect on output, with no corresponding possibility on the positive side, and the authors believe other, less dramatic, credit mechanisms could be subject to similar effects. Efficiency wage models provide another avenue for asymmetric responses to monetary events. If an unexpected increase in money initially starts to reduce unemployment and the real wage below equilibrium levels, existing workers and firms will both find it in their interest to raise wages along with prices. On the other hand, the interests of firms and workers will diverge if there is a negative monetary shock, and the adjustments will be slower. Because of such asymmetries, variations in nominal demand can affect the average level of output, and good policy can raise average levels of utilization.

Theoretical models that have multiple equilibriums provide another type of reason for believing policy may have an important role. As an example, the authors cite models that have "thick-market externalities." These models, in which increasing returns to scale often play an important role, not only have multiple equilibriums but often have the property that the optimal equilibrium occurs where the level of production and the rate of resource utilization are highest. Existing versions of such models suggest that policy actions may be able to move the economy from one equilibrium to another.

Having argued that asymmetric responses to monetary shocks provide one reason for thinking that policy may be able to affect the average level of output, De Long and Summers look at the empirical evidence for such asymmetries. They cite a study by James Cover using quarterly data for the postwar period that shows insignificant real effects from positive monetary innovations but large and significant effects from negative innovations. They report some tests of their own using annual data and estimating separate equations for the pre-Depression and postwar periods. One specification relates real output to the expected growth in money and the monetary innovation, distinguishing positive and negative innovations inferred from an auxiliary equation; lagged values of output and time are additional variables. A second specification substitutes the expected growth in nominal income and its innovations, positive and negative, for the monetary variables.

The results from estimating these equations are generally consistent with those of Cover. Positive money growth surprises have relatively small and statistically insignificant effects on real output in all sample periods, whereas negative surprises are large in all regressions and significant, or nearly so, in each regression. The pre-Depression equations show a slightly smaller effect of negative surprises than do the postwar regressions. The coefficient patterns are by and large similar when nominal income is substituted for money growth, but the differential effects of positive and negative shocks are typically smaller and less significant.

Most observers believe that, from the late 1940s until at least the 1970s, and in contrast with earlier periods, economic policy in the West aimed at stabilizing demand at a high level. Not only did governments take on explicit responsibility for high employment and output in their discretionary actions, but there has been an increase in "automatic stabilizers," some introduced as a direct consequence of government action. While the authors believe it is difficult to allocate nominal demand volatility between automatic stabilizers and active policy, they find strong evidence that nominal demand has been more stable since World War II than before the Depression. For example, the standard deviation of annual nominal GNP growth was halved between the two periods. The authors do not see plausible reasons for believing either that the natural stability of the economy changed significantly between the two periods or that technology or factor prices have been subject to smaller shocks in the postwar period. Hence they believe comparison of the behavior of real output in the pre-Depression and postwar periods provides evidence about the ability of demand policies to affect macroeconomic performance.

The authors discuss several stylized facts about the behavior of output. They note that much has been made of the finding, primarily from postwar data, that the time series of output contains a unit root. Shocks to output do not appear to fade away. They criticize the view that this persistence provides strong evidence that all fluctuations are the result of shifts in permanent factors, such as shifts in technology or tastes, or nominal shocks that have hysteresis effects. They note that the power of the tests for unit roots is low and illustrate the difficulty this poses with an example. They analyze a time series made up of a permanent component following a random walk and a transitory component that is a first-order autoregressive process with a lag coefficient of 0.75. Although the transitory component is responsible for 83 percent of the variance in annual output, an econometrician would need 135 years of data to have a 50-50 chance of rejecting the hypothesis that the entire process was simply a random walk. Hence they reason that failure of time series techniques to find transitory fluctuations means only that these fluctuations do not dominate the data.

The authors report regressions of output (measured per person of working age) on lagged output and a time trend for both the pre-Depression and postwar periods for the United States and five western European nations. In the case of the United States, the coefficient on lagged output in the postwar period is large and only slightly below its expected value if output were generated by a random walk. But the pre-Depression sample rejects the random walk hypothesis with ease. The same conclusions hold for the other nations, and, in all countries except the United Kingdom, the persistence of output rises between the pre- and postwar periods. The authors interpret this evidence, together with the earlier evidence on the reduction of nominal demand variation, to show that demand policies and institutions have been responsible for substantially reducing the transitory variations in real output. It follows that the postwar persistence of output, which some suggest indicates no useful role for the management of nominal demand, can instead be regarded as evidence of the success of demand management.

This evidence, while suggesting an important role for policy in stabilizing the economy, does not deal directly with the question of whether policy can change the average level of unemployment or output. To address this question, De Long and Summers first report evidence from previous studies that suggests that several real economic variables have distributions that are skewed downward, consistent with the idea that their response to shocks is asymmetric and that reducing demand fluctuations may increase average output. They also show that the skewness in unemployment was reduced in the postwar period, which suggests that the reduction in transitory fluctuations may have come primarily from reducing the level of unemployment at cyclical troughs. Turning to a more ambitious attempt to examine the gaps between output and capacity, De Long and Summers construct a family of measures of potential output that assume that potential has four important properties: it does not decline over time, it is a limit on actual output, it grows smoothly, and it is actually achieved on a semi-regular basis. The authors recognize that one could quarrel with these assumptions, particularly the assumptions that potential output never falls, even in the face of supply shocks such as the 1973 oil shock, and that output achieves potential on a semi-regular basis. But except for the 1980s in Europe, they believe that the assumptions provide the basis for reasonable estimates of potential output in the pre-Depression and postwar periods.

The output gaps implied by these estimates of potential are consistent with the view that the severity of the U.S. business cycle has been reduced. For each potential output series, the mean output gap is at least 50 percent greater before the Depression than after World War II. The authors calculate that this performance improvement corresponds to $50 billion of output per year in today's economy. The performance of most other Western economies is by and large consistent with the U.S. experience, with average output gaps noticeably reduced in the postwar period.

The magnitude of these reductions in the average gap for the United States is in striking contrast to Christina Romer's conclusion, based on volatility measures, that the severity of business cycles is essentially unchanged. De Long and Summers reason that Romer's volatility measures, which compute deviations from quadratic trends, are probably contaminated by the stochastic nature of long-run potential output growth. They provide auxiliary evidence that their gap estimate is superior to the cycle approach by showing that their gap measure does a better job of explaining the movements of unemployment with output.

The authors note that, in leaving the Great Depression out of their empirical investigation, they have omitted the episode that argues most powerfully for their view of output fluctuations. Thus full recovery of the economy to approximately its pre-Depression growth path favors the hypothesis that the economy can have multiple equilibriums and that a gap-based approach to business cycles is appropriate. They conclude that demand management policies can and should be used to raise the mean level of output by reducing the gaps between actual output and potential.

DISCUSSIONS OF macroeconomic policy focus on a few highly aggregative summaries of economic performance—output, unemployment, and inflation. Most macroeconomic models do likewise. But behind these aggregate series are a variety of important empirical regularities. In the third paper of this issue, George A. Akerlof, Andrew K. Rose, and Janet L. Yellen present and test a new model of job turnover in the U.S. labor market that attempts to explain not only the salient cyclical movements in unemployment, quits, and vacancies, but also a number of important characteristics of job changing among workers. Among these key characteristics are the concentration of quits in low-wage jobs, the reduction in wages that often accompanies quits, the negative relationship between job tenure and the quit rates, and the fact that a large proportion of job switches do not involve a spell of unemployment. Not only are these variables of intrinsic interest, but the authors argue that they provide informative tests of alternative macroeconomic theories. In particular, the authors believe that while the variables can be comfortably explained by a model with nonclearing labor markets, they are hard to reconcile with models that assume that labor markets clear.

Akerlof, Rose, and Yellen's model of labor turnover illuminates the basic mechanisms they believe are at work in the economy. The model makes a number of key assumptions. The total number of jobs is exogenously determined, but some old jobs disappear and others are created each period. Jobs pay different wages, all exceeding whatever pecuniary and nonpecuniary benefits there are to unemployment and leisure. Jobs also have nonpecuniary returns, which can be positive or negative. To make the model tractable, the authors assume that these returns are initially zero for an individual taking a new job, but then vary randomly over time. Thus, some workers become happier in their jobs over time; some become less satisfied.

In Akerlof, Rose, and Yellen's model some workers enter the labor force each period and some leave, but leavers and entrants balance—the size of the labor force is fixed. New entrants to the labor force have to search for a job and they are initially unemployed. They are joined in unemployment by workers who lose their job because of a job disappearance and workers who voluntarily leave their job because of job dissatisfaction or as a result of exogenous voluntary quits. Employed workers are on the lookout for jobs that are superior to their existing job and switch jobs when one is offered, giving rise to employment-employment quits. Although it is inessential to their model, Akerlof, Rose, and Yellen assume that unemployed and employed applicants have equal probabilities of receiving any job offer.

To illustrate a key feature of their model, the authors first consider a simple version with just one type of job, for which it is possible to derive an explicit formula for the steady-state level of employment-to-employment (E-E) quits as a function of the fraction of employed workers who would prefer a new job over their current employment, the unemployment rate, and the number of autonomous vacancies. This last reflects the birth of new jobs, departure of incumbents from the labor force, and voluntary quits to unemployment because of job dissatisfaction or some exogenous factor. They show that the flow of such E-E quits is a simple multiple of the number of autonomous vacancies. This multiple, which they call a "vacancy chain," reflects the fact that when a worker quits to take a better job, his old job is filled by another worker who, if he was employed, leaves a new vacancy in turn. The length of this chain is sensitive to the level of unemployment, since the fraction of vacancies filled by currently employed workers depends on the relative proportions of employed and unemployed workers. An important implication of this model is that average job satisfaction is higher when unemployment is lower. In good times, more workers get to improve their lot by switching jobs so individuals are better matched to jobs.

The authors simulate this basic model for plausible values of the parameters. They have no trouble generating combinations of central variables in rough accord with U.S. data. At an unemployment rate of 10 percent they simulate an E-E quit rate of 3.5 percent of employment per quarter, average unemployment duration of 10 months, and average job tenure of 44 months. A decrease in the fraction of the labor force unemployed from 10 percent to 5 percent increases the E-E quit rate to 4.1 percent per quarter. This increase is much less than proportional to the decline in unemployment, reflecting the much smaller fraction of employed workers dissatisfied with their jobs when labor markets are tight.

The authors stress that this increase in the welfare of employed workers is an important benefit of lower unemployment. It comes on top of the benefit in standard disequilibrium models in which an increase in employment driven by a shift in demand has a direct effect on output and workers' welfare because there are unemployed workers who are glad to work at the going wage. The additional welfare gain in the Akerlof, Rose, and Yellen model reflects the improved allocation of already employed workers among jobs. In their simulations, a decline in the unemployment rate from 10 percent to 5 percent raises the nonpecuniary reward received by the typical worker by a significant amount, the equivalent of 1.5 percent of the average wage.

Although it is less amenable to formal analysis, Akerlof, Rose, and Yellen construct and simulate a multiple-job version of their model that eliminates several unrealistic features of their simpler model. They assume a uniform distribution of jobs, distinguished by their wage rate, which is assumed to range from 0.7 to 1.3 times the economywide average. E-E quits can now be made for pecuniary as well as nonpecuniary gain, and quit rates are obviously likely to be higher for lower-paying jobs. Simulating this model, Akerlof, Rose, and Yellen find a somewhat higher average rate of job switching, with roughly the same cyclical sensitivity as in the one-job model. Most quits, not surprisingly, are from relatively low-paying jobs, and because vacancies are less likely to occur in high-paying jobs, quitters from low-wage jobs move disproportionately to other jobs at the low-wage end of the spectrum. Wages are a more important factor in determining switching behavior, and nonpecuniary gain from reducing unemployment is only approximately half what it was in the one-job model. The multiple-job market also displays declining hazard functions for job tenure: the longer a worker has been employed, the longer he is expected to remain in the same job. The model also generates a small negative correlation between wages and nonpecuniary rewards. But this effect, which appears as a compensating differential, is entirely due to the switching behavior of workers; firm behavior plays no role in their model.

The Akerlof, Rose, and Yellen model of turnover depends crucially on the assumptions that jobs are rationed and that workers receive rents. The authors cite a variety of studies that present evidence supporting that assumption. There appear to be systematic interindustry and occupational wage differentials that cannot be explained by differences in worker characteristics. When workers change industries, their wages tend to change in accord with industry differentials. High-wage industries tend to pay high wages in all occupational classes. These data all suggest that some workers are in "good" jobs for which other workers, in lower-paying jobs, would be qualified. The negative correlation between quits and wages and the fact that quits tend to fall as wage premiums rise are both consistent with the view that workers in high-paying jobs are, on average, receiving rents. The authors offer further evidence of job rationing from studies of applicant data. Even in the relatively tight labor market of 1948, both low- and high-paying plants had many job applicants. Other fragmentary evidence includes a help-wanted study showing "a tidal wave" of applicants for full-time nonskilled positions in 1978, and data for the U.S. labor market as a whole suggesting that over 80 percent of all unemployed workers accept the first job offered.

A novel feature of the Akerlof, Rose, and Yellen model is the prominent role given to nonpecuniary rewards. The authors use the National Longitudinal Survey (NLS) to investigate the importance of nonpecuniary rewards in determining job behavior. They report that nonpecuniary factors, both positive and negative, are mentioned as the most important features of jobs at all levels of job satisfaction. Over 80 percent of those who like their jobs cite a nonpecuniary reason as the primary cause of their satisfaction; similarly, for over 80 percent of those who dislike their jobs the culprit is nonpecuniary. They also find that a significant fraction of the sample population reported a change in job satisfaction from one year to the next, with nonpecuniary factors most often the reason.

Perhaps the most striking data from the NLS suggesting the importance of nonpecuniary rewards are answers to the question "suppose someone in this area offered you a job in the same line of work . . . What would the wage or salary have to be for you to be willing to take it?" Almost half the sample was willing to give a precise numeric response. The median percentage increase required was 25 percent. Ten percent of such respondents indicated a willingness to take a pay cut in order to switch jobs. Another 10 percent indicated that they would require a pay increase in excess of 75 percent. Among those who did not give a numeric response a majority said they would not change jobs for any pay. Akerlof, Rose, and Yellen conclude that, if anything, they underestimate the importance of nonpecuniary rewards in their simulation models.

The authors briefly review a variety of studies of labor mobility surveys done before the NLS data became available that suggest that a large fraction of workers voluntarily changing jobs take wage cuts and that nonpecuniary factors play an important role in the decision to quit. But they recognize that simple tabulations may be misleading, since both wages and the estimates of nonpecuniary rewards may proxy for other factors that are important determinants of quitting. In an effort to deal with this problem, they estimate a quits equation that explains quits by the worker's current wage and estimate of nonpecuniary reward, but also controls for a variety of personal characteristics including education, experience, race, marital status, and location. They also report multinominal logit estimates of the role that wages and nonpecuniary benefits play in determining job satisfaction. The results support Akerlof, Rose, and Yellen's belief that nonpecuniary rewards are a significant factor both in explaining worker satisfaction and in the decision to quit. Indeed the coefficient on nonpecuniary reward (which is calibrated in wage units) is not significantly different from the coefficient on wages in explaining quits.

In Akerlof, Rose, and Yellen's view, the two major market-clearing theories—real business cycle theory and the new classical theory based on search and imperfect information—are inconsistent with the wide variety of evidence suggesting that jobs are rationed. Furthermore, they cannot explain the cyclicality of quits and do not provide an explanation for employment-to-employment quits. According to search theory, unexpected deflation, for example, should lead to high quit rates and high unemployment as individuals become dissatisfied with their current wages relative to their incorrect perceptions of the available alternatives. Similarly, unemployed workers become less likely to accept job offers in those circumstances. Because real business cycle theories do not make clear predictions about the behavior of quits, the authors present their own version of such a model and demonstrate that, under plausible assumptions, it predicts that negative productivity shocks raise quits in the short run, just the opposite of the relation between quits and unemployment actually observed. The long-run effects of such shocks are ambiguous. Of course, the most obvious fact that real business cycle theories do not explain well is the presence of, and cyclical variation in, unemployment itself. Akerlof, Rose, and Yellen do not find compelling the argument that an unemployment spell corresponds simply to search and retooling, and observe that an implication of that view, that mobility of labor is greater during periods of high unemployment, is inconsistent with the data.

How to tax capital gains has long been a source of contention among tax analysts and politicians. The issue resurfaced during the recent presidential campaign when candidate George Bush proposed reducing the capital gains tax to 15 percent, compared with a top rate on regular income of 28 percent. In the first report of this issue, Alan J. Auerbach reexamines the issue of capital gains taxation with particular emphasis on the revenue consequences of changing the tax rate. Although the revenue issue is only one of many considerations in the taxation of gains, it is a particularly timely one because the federal budget deficit is so large and disagreement about how to raise revenues and cut expenditures so as to reduce it is so profound.

The direct revenue consequences of changing capital gains tax rates can be analyzed in two steps. First, how is the volume of capital gains realizations affected by the change in tax rates? Second, how do the revenues from the new level of realizations and the new tax rates compare with the revenues from the old level of realizations and old tax rates? For a cut in the rate to increase revenues, the volume of realizations must rise proportionately more than the tax rate is reduced. Auerbach shows that there have been substantial changes in realizations in years surrounding changes in the taxation of gains, but reasons that much of this effect should be attributed to anticipated changes in the tax rate, rather than to the level of the tax rate. Most recently, with the top rate on long-term gains scheduled to rise from 20 percent to 28 percent in 1987, realizations of capital gains rose to $325 billion in 1986, double their level the previous year. Once final data are available they will show realizations down substantially in 1987, when the tax rate rose to 28 percent, but this drop should not be confused with the permanent effect of the new level of rates. Previous statistical studies of capital gains realizations have failed to make this distinction between levels and expected changes and, Auerbach argues, have produced seriously biased estimates of the permanent effect of tax rates on realizations and revenues.

Auerbach presents equations allowing for just the level of rates alongside his own specification that explicitly allows for the change in the tax rate as an additional factor explaining realizations. The equations allow for the effects of prices, the total value of stocks, and the cyclical position of the economy, as well as the tax rate variables. When no effect is allowed for the change in the tax rate, higher levels of the tax are estimated to result in substantially lower levels of realizations, although the total revenue from the tax is scarcely affected. Once the expected change in the capital gains tax is allowed for, the level of the tax is estimated to have virtually no effect on realizations. The expected change in the tax has a substantial estimated effect, though the standard error of the coefficient is large, and the equation still falls short of explaining all of the increase in realizations that occurred in 1986. Using the point estimate for the effect of tax changes, Auerbach simulates the effects on capital gains realizations and revenues that would result from an anticipated reduction in the marginal tax rate to the 15 percent level proposed by President-elect Bush. In the year before the tax change took effect, both realizations and revenues would decline 28 percent. In the following and subsequent years, with the new lower tax rate in effect, realizations would be up slightly, while revenues would be down nearly 40 percent.

Two main aims of the Tax Reform Act of 1986 were to eliminate many existing avenues for tax avoidance that had eroded the tax base and to reduce incentives for creating new ones. Because many of these schemes relied on converting regular income into capital gains, by creating losses in regular income offset by capital gains in the future, equalizing the tax rate on gains and regular income was an essential feature of the legislation. As to revenues, the incentive to convert ordinary income into capital gains when tax rates on capital gains are much lower is one reason why estimates of revenue loss from cuts in the capital gains rate are likely to be understated. Quite apart from revenue effects, one important reason for not reopening a large discrepancy between the tax rate on capital gains and regular income is to keep tax avoidance schemes and their distorting effects on capital allocation from reemerging.

Auerbach discusses some of the ways in which capital gains are not taxed properly and some of the difficulties in doing it better. Inflation poses special problems for capital gains taxation because, with nominal gains being taxed, the taxation on real gains can be very high and even exceed 100 percent. This problem has led to proposals to index capital gains for inflation so that only the real component of gains would be taxed. However, indexing schemes can be complicated: if only real gains are taxed, then only real interest payments should be deductible against gains. Auerbach notes that indexation also makes the limitation on deduction of capital losses more onerous, since, like a reduction in inflation itself, it lowers gains and increases losses. Insofar as the limitation on losses adversely affects risk taking, it could reduce investment in risky enterprises.

Even if taxed at the same rate as regular income, capital gains have the advantage that they are not taxed until they are realized. This advantage, in turn, gives rise to the "lock-in" effect, which inhibits taxpayers from selling appreciated assets because they want to postpone paying the capital gains tax. The lock-in effect is greatly amplified by the treatment of appreciated assets at the death of the investor: those who inherit the assets use the value at the time of death as the basis for future taxation. As the most direct solution to both the lock-in effect and the disincentives to risk taking that arise from the limitations on deducting losses, Auerbach recommends taxing capital gains on accrual rather than realization, which would permit allowing full deduction of losses. A second-best solution would be to replace the current step-up in basis at death with taxation of capital gains in an estate through constructive realization or, at least, with a carryover of the original price basis of the assets by the heirs. Either change would reduce the lock-in effect while gaining, rather than losing, revenues. In conclusion, Auerbach finds it unlikely that cutting the capital gains tax is a good way to reduce the costs of objectionable features of the current tax law, and warns that such cuts would themselves create new distortions by reopening the wedge between the taxation of capital gains and ordinary income.

As THE U.S. budget and trade deficits grew during the 1980s, and as the dollar's foreign exchange value cycled sharply first up and then down, many observers predicted hard-landing scenarios in which the United States, and in some versions the world, falls into deep recession. Such predictions continue to be made. Curiously, some attribute the trouble to failure to reduce the budget deficit, while others attribute it to success in doing so. In the second report of this issue, Jeffrey D. Sachs explains why he disagrees with such predictions of a hard landing for the economy.

The prospect of continued large deficits in the U.S. budget and current account motivates one of the hard-landing scenarios that Sachs examines. The basic argument is that continuing foreign financing of a \$150 billion annual current account deficit would be historically unprecedented. If the budget and trade deficits continue, foreigners will be increasingly reluctant to finance them by investing capital in the United States. The sharply higher U.S. interest rates necessary to keep foreign capital from fleeing the country will precipitate a U.S. recession that may spread abroad.

Although the assumed budget deficits and the required foreign capital inflows are both large, Sachs argues that history provides little guidance as to whether they are "too large" because, until recently, capital controls were a significant barrier to sustained capital transfers. He shows that the \$150 billion annual current account deficits represent only 2.4 percent of the gross domestic product of the major foreign industrial countries, or less than 10 percent of their gross national saving, and finds it feasible that capital flows of that relative size could continue without requiring ever-higher U.S. interest rates. As evidence against Martin Feldstein's argument that the decline in the dollar already signals a growing reluctance of foreigners to hold dollar assets, he shows that there has been no evidence of a rising risk premium in relative interest rates between the United States and foreign markets.

The other hard-landing scenario asserts that eliminating the U.S. budget deficit would be so contractionary as to cause a worldwide recession. Sachs also finds that prospect unlikely. A U.S. fiscal contraction would lead to dollar depreciation, a fall of U.S. output, and a reduction in world interest rates. Although the first two effects are indeed contractionary abroad, the interest rate effect is expansionary and modifies the first two. On some assumptions, it may even dominate them (Fitoussi and Phelps, *BPEA 2:1986*). Furthermore, if foreign wages respond promptly to foreign currency appreciation, lower real wages would provide another avenue through which the net effect of U.S. fiscal contraction could be expansionary to the rest of the world. Although this net effect is ambiguous, Sachs cites the recent strength of expansions in Europe and Japan during a period of U.S. fiscal retrenchment as evidence that further cuts in the U.S. deficit would not cause economic weakness abroad.

According to Sachs's analysis, the U.S. budget deficit has been only one cause of the U.S. current account deficit. He presents a model developed jointly with Nuriel Roubini that attributes a little more than half of the worsening in the U.S. trade deficit between the late 1970s and 1985 to the growth in the U.S. fiscal deficit. The model attributes the remainder to contractionary fiscal policies in Japan, to monetary policies here and abroad, and to the inability of LDCs to borrow, which forced them to move to trade surpluses. He sees the huge real appreciation of the dollar through 1985 as endogenous, responding to changing fiscal and monetary policies. The outcomes would have been different, according to Sachs, if capital markets had not been liberalized during this period, particularly in Japan. Without the large capital outflows that were permitted from Japan in the 1980s, the yen would not have depreciated as it did, Japan's trade surplus would not have grown as much as it did, and the Japanese economy would have expanded through domestic rather than export demand. Sachs cannot offer as complete an explanation of the years after 1985 because the fall in the U.S. private saving rate contributed to the external deficit in that period, and the private saving rate is not directly explainable by policy changes.

Because he finds changes in the budget deficit reflected only partially in the trade deficit, Sachs projects that the United States would continue to run a current account deficit even if the budget deficit were eliminated entirely. Alternatively, if the budget deficit is not reduced further, the present level of the dollar may continue to improve the current account somewhat and lead to excess demand in the United States. Thus Sachs concludes that the main risk for the U.S. economy is inflation rather than recession. And, he reasons, this risk would increase if the monetary authorities attempted to depreciate the dollar further through monetary expansion.

MANY Latin American economies are overwhelmed by their debt burdens. Debtors, creditors, and interested third parties all agree that most of these countries will never be able to repay fully their current debt obligations. But it is more difficult to find a consensus on how to deal with the debt problem. Attempts by creditors to force debtor nations to undertake economic policies intended to maximize repayments can lead to economic and social disarray. Indeed, too stringent requirements may even reduce the total assets recovered by creditors. Still, the conflicting interests of the parties involved make negotiated restructuring of the debt and repayment obligations extremely difficult, and the process of negotiation is itself costly, distracting policymakers from other pressing problems in their economies. In these circumstances, and with market prices of outstanding debt only a small fraction of face value, some debtor nations have attempted to improve their debt position by buying back their debt. The last two reports in this volume present alternative analyses of such actions.

In the first report, Jeremy Bulow and Kenneth Rogoff examine the likely effect of debt buybacks and equity swaps on the ultimate welfare of creditors and the debtor nations themselves. They argue that, contrary to much popular opinion, a heavily indebted country using the market to retrieve part of its debt in a buyback will, in the absence of significant concessions by creditors, make itself worse off.

The two keys to understanding Bulow and Rogoff's central conclusion are first, the difference between the marginal value of debt reduction and the average value of debt, and second, the special nature of the collateral in the case of sovereign debt, debt issued by a national government, as compared with the collateral of a private debtor. A repurchase of private debt is made with assets that would have been seized by the lender in the event of default. In the case of sovereign debt, the relationship between what lenders can expect to collect in the future and the use of reserves for repurchases is much more tenuous. According to Bulow and Rogoff, how much the creditors can extract ultimately depends on their capacity to threaten a country with cutoffs of trade credits and withdrawal of other benefits of participating in international goods and capital markets. In these circumstances, the amount that can be extracted after a debt repurchase by the remaining creditors may not be much less than the entire group of creditors could have extracted before the debt repurchase.

Consider the extreme case in which the country is sure to default and in which a buyback does not reduce what the creditors can extract in the future. Then the total market value of outstanding debt will be essentially unchanged by a market repurchase that buys back part of the debt. Because it does not reduce future claims, the marginal value of the debt repurchase will be zero, but the country will be paying the market price, or average value, for the debt it buys back. Whatever assets the country uses to repurchase the debt will be wasted. Creditors who are bought out will be no worse off, remaining creditors will be better off because the pie will now be divided among a smaller number of claims, and the country will have fewer resources to use for domestic consumption and investment. If creditors' "collateral" is not reduced, but there is some prospect of the country meeting its contractual obligations, the results are similar but not as extreme. Repurchases will reduce the market value of outstanding debt, but by less than the cost of the repurchases. The situation is quite different in a negotiated agreement in which all debt is repurchased and future claims of creditors are eliminated or reduced by the amount paid for the repurchase. But the authors doubt that actual repurchases have met such conditions.

A crucial assumption behind the Bulow-Rogoff results is that using resources to repurchase debt does not reduce what creditors can expect to recover in the (likely) event of default. If all of the resources used to repurchase debt would eventually have been entirely taken over by creditors, the results would have been reversed, with creditors, in effect, paying for the debt repurchase. Bulow and Rogoff use their theoretical model to examine what fraction of the reserves used for repurchase would have to be at the creditors' expense in order for the repurchase transaction to benefit debtor nations. The critical value depends on the relative magnitudes of the marginal and average values of debt. Whether the critical value is exceeded for the typical Latin American country is an empirical matter. After examining data on secondary market prices and actual repayment rates of Latin American countries, the authors conclude that the actual value is far below the critical value required to make a partial debt repurchase advantageous. In the case of Bolivia, which bought back half its debt, the market price of the half that remained outstanding approximately doubled, just as the authors' basic model predicted for the case in which expected future repayments are not reduced at all by the buyback. Furthermore, the remaining debt still trades at a huge discount from par, which leads the authors to infer that whatever disincentives might exist from debt overhang are still present. Thus they conclude that, viewed in isolation, the Bolivian buyback was a giveaway to creditors. However, they grant that it might be justified if it was a concession by Bolivia for which it received sufficient compensation from its official creditors.

Bulow and Rogoff analyze several other market-based transactions, including debt-equity swaps and the sale of new debt that is senior to the already outstanding debt. They conclude that a debt-equity swap is essentially a combination of a marginal debt repurchase and a conventional direct foreign investment. Since the foreign investor must at least break even, and the creditors as a group benefit from the repurchase, the combined transaction has to be a bad deal for the debtor nation. It would be better off simply selling assets directly to the foreign investors. Issuing senior debt seems to hold out the possibility of benefiting debtor countries. They would receive the proceeds from the new lenders and, in the case of default, most of the costs would be borne by existing creditors. However, the authors reason that, for legal or other reasons, existing creditors would have to agree to the new debt issue and would have to be compensated, leaving little or no surplus to the debtor.

In the final report of this volume, Jeffrey D. Sachs provides a different interpretation of the Bolivian buyback. He suggests that the Bulow-Rogoff model is not germane to the Bolivian case and offers an alternative model of debt repurchase schemes in general. In Sachs's view, an overindebted country that can pay only a fraction of what it owes faces serious difficulties that are not reflected in the market value of its debt. Among the difficulties are an inability to borrow for productive investment, high bargaining costs over existing debt, possible trade sanctions by creditors, and disincentives to economic reforms that could increase its output and capacity to service debt. These problems are not noticeably reduced by a piecemeal buyback of debt. But if all or most of existing debt can be repurchased at market value, the country benefits by eliminating these severe costs of the debt overhang. Therefore, Sachs regards buybacks as a potentially useful and important component of comprehensive arrangements for debt reduction. He argues that the Bolivian debt buyback was part of a highly successful plan for eventually reducing that country's entire debt burden.

If the market value of debt represents the expected value of repayments on the part of lenders and there are substantial additional costs to debtor nations from their debt overhang, there ought to be room for both to gain by a comprehensive buyback of debt. Sachs discusses why such buyback schemes are hard to achieve despite this potential for mutual gains. Bank regulators do not generally require banks to write down their LDC debts to their market value. However, if the debts were repurchased at near market price, the banks would have to record the loss, in many cases forcing intervention of bank regulators and possibly costing bank managers their jobs. According to Sachs, U.S. policy has worked to prevent large-scale buybacks in order to avoid any such problem at major banks and has made servicing the debt its prime aim.

He sees debtor governments acquiescing in this strategy "out of fear of a foreign policy rupture with the United States."

Sachs notes that the U.S. support for the buyback of debt by Bolivia is the only exception to date from this official posture. After experiencing hyperinflation and a massive decline in real GNP during the first half of this decade, Bolivia, as part of an overall stabilization plan, ceased meeting its foreign interest payments. Sachs explains how the International Monetary Fund and U.S. government eventually came to support a negotiated debt buyback, with foreign governments providing $34 billion for that purpose. He reasons that Bolivia and the banks both gained. The banks that sold back debt received nearly double the old market price. Bolivia benefited even more. It made no interest payments during the two years of negotiations and received net transfers from official creditors of about 5 percent of GNP a year. Sachs judges that no more than half of the $34 million provided by foreign governments for the buyback would have come to Bolivia in other forms of aid. Most importantly, Bolivia freed itself to pursue domestic economic policies that ended its hyperinflation, stabilized its economy, and restored its growth. Based on his analysis of the buyback, Sachs interprets the change in the market price of Bolivian debt differently than Bulow and Rogoff do in their paper. In Sachs's view, the present market price reflects an understanding of how much Bolivia will pay to buy back the remainder of its bank debt in a second negotiated settlement, leaving it free of all its old debt burden.