Editors' Summary

THE BROOKINGS PANEL on Economic Activity held its sixty-eighth conference in Washington, D.C., on September 2 and 3, 1999. This issue of *Brookings Papers on Economic Activity* includes the papers, reports, and discussions presented at that conference. The first paper uses a number of historical tax reforms to examine the response of taxable income to tax rates and concludes, unlike some earlier studies, that the response is small. The second paper asks whether today's working households are saving enough for retirement, by using a stochastic life-cycle model to simulate a distribution of wealth accumulation against which to compare actual household savings. The third paper argues that Europe's persistently high unemployment primarily reflects a rise in the noninflationary unemployment rate caused by prolonged tight monetary policy, and develops a model relating wage inflation to short- and long-term unemployment to explain this hysteresis effect. The fourth paper reviews the nineteenth-century history of international financial crises for lessons about the need for financial reform, the advisability of financial rescues, and the choice of exchange rate regimes in today's crisis-prone global economy. The issue concludes with a report on the first months of the European Monetary Union and on how it has so far delivered on expectations.

FROM THE EARLIEST days of the federal income tax, critics have argued that high marginal tax rates have damaging effects on incentives. "Supply-side" arguments were prominent in the efforts to reduce the high surtax that came out of World War I, and Andrew W. Mellon, who served as President Warren G. Harding's secretary of the Treasury, stated clearly the argument that six decades later, in the 1980s, was popularized as the Laffer curve. At that time a prominent group of economists promoted the

supply-side idea that high marginal tax rates result in large deadweight losses and may even be revenue reducing.

The idea had lost momentum by the end of the 1980s, as econometric studies failed to find much responsiveness of labor supply to tax rates. But it was given new plausibility in the 1990s by a new literature that argued that inelastic labor supply does not prove that deadweight losses are low, or that the economy is on the side of the Laffer curve where higher tax rates produce higher revenue. Most notably, analysis of the Tax Reform Act of 1986 suggested that cutting tax rates at the upper end of the income distribution may substantially raise both revenue and welfare. In the first article of this issue, Austan Goolsbee reexamines what he calls the New Tax Responsiveness (NTR) literature and its analysis of the 1986 tax reform, and applies its methodology to estimate the response to five earlier large tax reductions.

The central premise of the NTR literature is that what matters for the deadweight loss from taxation of labor income is not the elasticity of labor supply but the elasticity of taxable income. If certain forms of income or consumption, such as fringe benefits or nontaxed perquisites, are not taxable, high-income taxpayers may shift "income" to these nontaxed forms without significantly changing their labor supply. In NTR models it is such switching that gives rise to deadweight losses from high taxation even when labor supply is quite inelastic. Goolsbee provides a succinct summary of the NTR theory as it has been laid out by Martin Feldstein. Feldstein augments the standard static tax model, which considers only consumption or income and leisure, by including nontaxable consumption and nontaxable income as additional choice variables directly affecting utility. In this setup the income tax is effectively a sales tax on taxable consumption and does not directly affect the relative prices of the other three "goods": leisure, nontaxable consumption, and nontaxable income. As long as the taxpayer is at an interior solution—that is, not already choosing the maximum allowable amount of any of the untaxed alternatives—the deadweight loss from an increase in the tax depends on how much taxable consumption falls, and not on which of the nontaxable activities is increased. Motivated by this observation, the NTR literature has set out to estimate the elasticity of taxable income with respect to the tax rate, making use of the natural experiments generated by changes in the progressivity of the income tax in the 1980s and 1990s.

Goolsbee recognizes difficulties that various critics have raised about using natural experiments to analyze the behavior of high-income individuals. However, his aim is not to add to that debate, but rather to apply the methodology that others have used for 1986 to five other major tax reforms between 1920 and 1975 in order to see what they suggest about the magnitude of the taxable income elasticity. The advantages of using these historical data are not only that they provide five additional natural experiments, but also that some factors that may have biased the results in the 1980s were different in the earlier periods. The main disadvantage is that, except for some special surveys, panel data are not available, and Goolsbee has to use statistical interpolations to calculate the relevant incomes and tax rates.

The natural experiment approach to estimating tax elasticities requires observing two groups of taxpayers who experienced tax changes of different magnitudes. To control for unobservables, the approach assumes that the two groups' reported taxable incomes would have grown at identical rates were it not for changes in their relative taxation. With this assumption, differing changes in taxable incomes can be attributed to the different changes in tax rates. Hence the technique is often called the method of "differences in differences." Applying this methodology to the 1986 tax reform, Feldstein estimated an elasticity of taxable income with respect to the after-tax share of income (one minus the tax rate) of more than one. Goolsbee reports that a number of other studies of the 1980s and 1990s that used a similar methodology produced a substantial range of elasticity estimates. Critics have cited two important flaws in these analyses of the 1986 tax reform. That law expanded the tax base by eliminating many of the largest loopholes in the tax law. Thus taxable income would have risen even if there had been no response to the lower rates that were introduced at the same time. Also, the 1980s were a period of steadily increasing income inequality for reasons presumably independent of tax changes. Hence some of the relative increase in taxable income of high-income households should be attributed to this underlying trend rather than to the cuts in their relative tax rates.

Since marginal tax rates on high-income taxpayers were increased in 1993, while the secular trend in inequality continued, the difference-in-differences estimates should have the opposite bias in that experiment. Preliminary analysis by Feldstein and Daniel Feenberg of the change in

incomes of the rich from 1992 to 1993 also indicates a large elasticity, but Goolsbee notes that this work does not distinguish between temporary and permanent changes in income. Higher-income individuals had ample opportunity to realize income late in 1992 so as to avoid the higher tax. Goolsbee's own prior work showed that as much as 20 percent of the decline in top-bracket income reported for 1993 came from just 10,000 corporate executives, driven almost exclusively by a one-time cash-out of stock options in late 1992. Goolsbee found that the short-run elasticity in this episode exceeded one, but that the elasticity after one year was one-third or less. He reports that other studies also find lower elasticities in the 1990s than in the 1980s, with sharply different implications for estimates of deadweight loss.

The ambiguities inherent in using any one tax reform to estimate elasticities, and the differences between the results for the 1980s and the 1990s, suggest there could be great value in studying additional episodes. One reason why econometric work has not been done on earlier periods is lack of data. The only data available are the annual income histograms published in the Internal Revenue Service's *Statistics of Income,* which reports the number of returns and total income for several tax rate brackets but gives no individual-level tax data. This gives rise to several problems. To interpret changes in the incomes of individuals in the top two income brackets, for example, as indicative of the changes in earnings of individuals in those brackets requires the assumption that there are no rank reversals of individuals by income. Another difficulty is that income brackets in these tables are fixed in nominal dollars over time. Even assuming that there are no rank reversals, the number of individuals in each reporting group changes over time, with growth in the number in the higher brackets simply reflecting growth in incomes over time. Goolsbee has no way of dealing with rank reversals. However, making use of the fact that the Pareto distribution is known to fit the top of the income distribution quite well, he devises a method of calculating the mean income of those taxpayers in each of the top brackets who were in that bracket in the prior year. This provides estimates of the income changes needed for the difference-in-differences estimation. He estimates changes in tax rates for the various groups by increasing each group's base-year income by the rate of nominal GDP growth and applying the changed tax law.

To provide a check on the reliability of this procedure, Goolsbee compares the results of applying it to the 1986 reform with the results of the

previous studies of that episode that used panel data. He produces two kinds of estimates. First, to parallel the results of Feldstein and Lawrence Lindsey, he breaks the income distribution into three groups and calculates the elasticities from the three pairwise comparisons. These estimates reveal the sensitivity of the results to the income groups chosen: the elasticities range from 2 to –0.2. Second, he produces regression estimates, which he regards as more reliable, using eight income groups reported in the Internal Revenue Service tables. For a range of values for the Pareto parameters, he obtains elasticities near one. Goolsbee regards these results utilizing histograms as reassuring, since they closely match those in the existing NTR literature for the 1986 reform.

Goolsbee then applies the same methodology to five major tax reforms from the period 1920 to 1966, ignoring only the tax changes from World War II. The episodes chosen include the tax cut of 1924–25; tax increases in 1932, 1935, and 1950–51; and the tax cut of 1964. He briefly describes each of these episodes and their unique environments, noting features of each episode that might bias the results one way or the other. Taken as a whole, the regression results suggest that the elasticity of taxable income is not nearly as large as others have estimated from the 1980s. The largest elasticity is less than 0.6, and the average is much smaller. Results for the 1935 tax increase appear anomalous: estimated elasticities are negative, which may result from increased enforcement of the tax law in that period and from an increase in the corporate tax rate that may have led to income shifting out of corporate form. Goolsbee also does a back-of-the-envelope calculation of what the revenue-maximizing tax rate would be if there were only one rate in the tax code. The estimates from the 1980s data imply a revenue-maximizing rate of only 42 percent; the historical data imply rates ranging from 63 percent to near 100 percent.

Goolsbee buttresses these results from aggregate cross-sectional data with analysis of two panel data sets on the incomes of corporate executives. The first comes from the *Forbes* magazine survey of executive compensation that began in 1970, just prior to the cuts in the top tax rate that occurred in 1971 and 1972. The second, previously used by Charles Hadlock and Gerald Lumer, includes data from the *Survey of American Listed Corporations* for the years 1934–38. That survey was supervised by the Securities and Exchange Commission to report compensation and balance sheet information for publicly traded firms. Goolsbee enumerates some advantages and disadvantages of using these compensation data.

Advantages are that they provide information on a large number of high-income individuals, use income measures that are consistent over time, and make it possible to control for characteristics of the firms employing the executives. An important disadvantage is that the data provide no information about deductions or other forms of income, including stock or stock options. Goolsbee suggests this is less of a problem for these earlier periods than it would be today, when options have become an important element in executive compensation. He also notes that the tax cut in the 1970s was atypical, only lowering the marginal rate on earned income.

Using these data, Goolsbee runs regressions which, by including individual and year effects or GDP growth, are equivalent to the difference-in-differences natural experiments. His regressions include both tax rates and specific characteristics of firms and individuals. For both of the episodes, separated by roughly thirty-five years, the estimated elasticities of taxable income are low. The highest are roughly 0.25, and some estimates for the 1970s tax changes are actually negative, as are three out of four estimates for the 1930s tax change.

All in all, Goolsbee's results suggest that the evidence from the 1980s on which the NTR literature is based is atypical of historical experience. The largest estimates of the elasticity from any of the previous historical periods are lower than the smallest estimates based on the 1980s. His results suggest that the results from the 1986 tax reform do indeed reflect some of the biases that have been suggested, and he concludes that it is implausible that cutting today's marginal tax rates will raise revenue.

THE LOW AND DECLINING national saving rate of the last decade, along with a general consensus that demographic changes will eventually force some combination of cuts in social security benefits or tax increases, has stimulated research on whether households' provisions for retirement are adequate. There is no clear consensus, but a number of economists have concluded that they are not. In the second paper of this issue, Eric Engen, William Gale, and Cori Uccello address this issue by comparing the wealth accumulation implied by optimal behavior in a life-cycle model with actual household saving information reported in two comprehensive surveys of household behavior.

One reason for the difference of opinion about the adequacy of households' saving for retirement is that adequacy can be defined in different ways. Some mean by adequacy a level of saving sufficient to maintain con-

sumption during retirement at its preretirement level; others mean enough to support living standards of future elderly at the levels enjoyed by the current elderly; still others mean enough to keep the elderly out of poverty. Although the authors acknowledge that each of these definitions is valid for some purposes, their meaning of "adequate" is different. They regard saving as adequate if observed saving and wealth accumulation are consistent with the optimizing behavior of agents in a standard life-cycle model. By this measure, if a household has little wealth relative to its income as a result of inattention to the future or some other deviation from optimal life-cycle behavior, its saving has been inadequate, whereas if it has responded as best it could to unexpectedly low income in the past, its saving is defined to be adequate.

Various researchers who have used life-cycle models to examine saving behavior typically ask the question the other way around: whether the life-cycle model can explain observed behavior, not whether observed behavior is consistent with optimal life-cycle behavior. Posing the latter question places an extra burden on Engen, Gale, and Uccello. They must specify key parameters of the model on the basis of a priori beliefs and plausibility, without reference to whether they result in a good fit.

A second distinguishing feature of the authors' analysis is its emphasis on differences in the earnings experience of individual households. Rather than asking whether the wealth-income ratio of a whole class of households of a given age and educational status appears optimal, they ask whether the distribution of wealth-income ratios of such households is consistent with optimizing behavior, once it is recognized that individual households experience idiosyncratic shocks to their incomes. Looking at the entire distribution of outcomes recognizes the fact that even if every household behaves optimally, households of the same age, education, and current earnings are likely to find themselves in quite different circumstances, depending on their history. It also enables the authors to compare the predictions of a model of optimizing behavior not just for the average or the median household, but for households at various places in the distribution.

The authors' elaborate stochastic life-cycle model requires a number of assumptions. Like other studies, theirs assumes that household lifetime utility is the discounted sum of period-by-period utilities, where each period's utility is a constant relative risk-averse function of consumption, with a coefficient of relative risk aversion of 3.

A crucial preference parameter in the life-cycle model is the pure rate of time preference, and the authors provide an extensive discussion of plausible values. Lower rates of time preference lower optimal consumption early in life and raise it later, thereby increasing the amount of wealth that an optimizing household would accumulate by the eve of retirement. Since the authors define saving adequacy by whether it matches optimal accumulation, the choice of the time preference parameter significantly affects their conclusions about the adequacy of households' saving. The authors recognize that using rates from empirical studies that explain actual household saving over time would beg the issue, since such studies typically choose values that assume actual behavior is optimal. They also believe that surveys that estimate the rate by asking households for their preferred consumption profiles suffer from being purely hypothetical or involving very small stakes. The authors end up choosing two values. One is a rate of time preference of 3 percent, which they suggest is lower than the rate used in most previous studies. The other is a rate of zero, which leads households to plan for very rapid consumption growth and, in the authors' view, errs on the side of finding household saving to be inadequate.

To get realistic estimates of the mean age-earnings profile, the authors use 1980–92 data on earnings of employed heads of households and their spouses from the Panel Survey of Income Dynamics. Age-income profiles are estimated for groups with high (sixteen or more years) and low (less than sixteen years) educational attainment. The estimates use a fixed-effects model with earnings as a function of age, age squared, and yearly dummies to control for macroeconomic developments. Although the macroeconomic effects are removed when estimating the profiles, the authors assume that the wages of all age groups are expected to rise by 1 percent per year. Individuals are assumed to retire at age sixty-two. On the basis of an earlier study by one of the authors, earnings shocks (in logarithms) are specified as a first-order autoregressive process with a persistence parameter of 0.85 and a variance of 0.05. Under this specification, about half of a shock to earnings remains after five years. Households face a progressive tax structure, with marginal tax rates, deductions, and exemptions similar to those prevailing in the United States in 1998. In retirement each household receives social security and employer-provided pension benefits based on the average earnings profile of its education class, not on its actual wage profile (in the simulations households are distinguished by whether or not they have pensions). Although house-

holds do not plan on leaving bequests or receiving inheritances, the model is closed by unexpected inheritances that households receive from the estates of households that die.

The timing and magnitude of wealth accumulation depend crucially on a household's life-cycle earnings profile, family formation, and life expectancy. The authors' stylized household begins life with two adults, both aged twenty-one, who have one child four years later and a second three years after that. Children become financially independent at age twenty-one and leave home. Adults in the household face uncertain life spans. Annual survival probabilities, estimated from the 1994 life tables used by the Social Security Administration, are near 1.0 until about age fifty but decline steadily thereafter and become quantitatively important as the household ages further. By age sixty-five the annual survival rate for males is down to 0.98, and by age eighty it is 0.92; both rates are slightly higher for females. Life ends for certain in the model at age 110.

The age profile of income and family size, and uncertainty about life span and labor income—together with the terms on which households borrow and lend—are the central determinants of the household's wealth accumulation. If there were no uncertainties, and if households could borrow and lend at the same risk-free rate, optimal consumption per capita would follow a smooth path. That path would be constant, growing, or declining, depending on whether the interest rate was equal to, greater than, or less than the pure rate of time preference, respectively.

Two additional features of the model change the nature of the optimal path. Households cannot insure against uncertainties in life span, earnings, or inheritances, and are not allowed to borrow. As a consequence, households save not only to try to smooth consumption per capita over the life cycle but, early in their life, for precautionary motives as well. As a result, in simulations of the model, optimal consumption per capita for the median household follows a hump-shaped curve. Even when the interest rate equals the rate of time preference, consumption is low when the household is young and grows as wealth accumulates, reflecting the decreasing need for precautionary balances. The importance of the accumulation of precautionary balances early in the life cycle is evident in the fact that wealth accumulation (relative to income) is essentially the same for households with different future income prospects and with widely differing rates of time preference. In the simulations, wealth accumulation begins to diverge substantially for households with different

income profiles and rates of time preference somewhere around age forty, by which time, for most households, the no-borrowing constraint is relatively unimportant. For the next twenty years or so, consumption per capita behaves much as it would in the absence of uncertainty and borrowing constraints. It is relatively flat or slowly growing, depending on the rate of time preference.

To sustain their optimal consumption during retirement, college-educated households without pensions need much higher rates of wealth accumulation than do non-college-educated households. Pensions, which are scaled to preretirement income, moderate this difference. And given smooth consumption per capita, the changes in family size with the arrival and departure of children significantly shift total household consumption and saving. With children present for twenty-four of the household's first thirty years, total household consumption is significantly higher during this part of the life cycle than later. In the later years of life, increasing mortality probability makes households less patient, leading them to consume their wealth at a more rapid rate than if they had a constant mortality rate, and resulting in a declining consumption path.

The authors show that, even with a rate of time preference equal to the risk-free interest rate, their model results in median wealth-earnings ratios higher than the targets used by other authors. For example, the median target for households in their sixties exceeds that calculated by Douglas Bernheim and John Karl Scholz by amounts ranging from 45 percent for households without pensions to 12 percent for college-educated households with pensions. These results confirm the authors' view that their assumptions give a conservative basis on which to judge the adequacy of saving.

A distinctive feature of the analysis is its attention to the distributional consequences of individual households' earnings experience. Optimizing households that have identical age and family size, current earnings, life expectancy, education, and pension status will have sharply different wealth-earnings ratios as a consequence of past earnings surprises. For example, for the population of sixty- to sixty-two-year-old college graduates with pensions, optimal wealth-earnings ratios vary by a factor of almost twenty, from 0.37 at the 5th percentile to 7.07 at the 95th. At a zero rate of time preference, these ratios are higher but the range is almost as wide. The authors stress that these differences are not due to myopic or irrational or ill-informed behavior. Households in the model

not only respond optimally to shocks, but also make optimal decisions about precautionary saving, fully aware of the stochastic nature of their future earnings.

How does this simulated distribution of wealth-earnings ratios of optimizing agents compare with the observed distribution of households with the same observable characteristics? Weighting survey observations to represent the national population, the authors compute wealth-earnings ratios for households in the 1992 Health and Retirement Survey (HRS) and in the 1983, 1989, 1992, and 1995 Survey of Consumer Finances (SCF). They develop three measures of wealth: a broad measure that includes essentially all net worth other than vehicles, an intermediate measure (which they prefer) defined as broad wealth less half of equity in the primary residence, and a narrow measure that excludes all equity in the primary residence. All the wealth measures include balances in defined contribution pension plans; in 401(k) plans, Individual Retirement Accounts, and Keogh plans; and, in the HRS data, the value of defined benefit plans. In the SCF such plans are treated, along with social security, as a flow of benefits during retirement. The authors therefore compare SCF data with the results of the optimizing model for households with pensions.

The authors compare the distribution of wealth-earnings ratios generated by their optimizing model with the ratios in their samples for households of the same age, education, and pension status. For the authors' preferred assumption regarding the rate of time preference (3 percent) and the broad or the intermediate measure of wealth, they find that between 52 and 60 percent of the HRS sample exceed the median target wealth-earnings ratio. Reducing the time preference rate to zero lowers these percentages substantially; nevertheless, 46 percent of the HRS sample still have broad wealth–earnings ratios that exceed the median, and 39 percent have intermediate wealth–earnings ratios that do so. They also find that households with pensions and with more education appear to be saving significantly more relative to the simulations than other households.

Several other features of the distributions are worth noting. For all ages and wealth definitions and for both rates of time preference, the observed wealth-earnings ratios in the HRS at the 95th percentile greatly exceed the model's estimates based on optimal behavior. The authors suggest that this is not surprising given that bequest motives are absent from the model yet may be important for the relatively wealthy, and because the observed ratios at the upper end of the distribution may reflect high rates

of return on some entrepreneurial investments. On the other hand, actual wealth-earnings ratios fall unambiguously below the simulated ratios for the bottom 25 percent of households, and for the 5th percentile dramatically so. The authors observe that although these results are consistent with systematic undersaving, they could also be explained by the omission from the model of a government-provided consumption floor. They note that the results are significantly less optimistic if a zero rate of time preference is assumed. In this case the data suggest that a significant portion of the population is undersaving by substantial amounts.

The authors use probit regressions to investigate the characteristics of high and low savers (those with wealth-earnings ratios above or below the simulated median, respectively, given a 3 percent time preference rate). Most of the results seem sensible. For example, high savers have fewer children than low savers and are more likely to have received an inheritance, to be self-employed, to believe they will live to age seventy-five, to have thought a lot about retirement, and to expect to retire earlier. The probit regressions do, however, find an anomalous result for income: for households nearing retirement, the likelihood of having a high wealth–income ratio falls as income rises. The authors presume that this reflects transitory income shocks that depress the ratio. However, it could also be that most of the presumed variance in the ratio reflects inheritances and capital gains that are not closely related to income.

The SCF data allow consideration of a number of additional items, because this survey spans a wider age group and longer period of time than the HRS. The characteristics of high and low savers are quite similar to those found with the HRS, but the observed wealth-earnings ratios are generally higher relative to the simulation than those using the HRS. The percentage of households that exceed the simulated median is somewhat larger, and the ratios are nearer the simulation results at the bottom of the distribution.

The authors recognize that, elaborate as it is, their model still abstracts from a number of important features of the actual environment facing households, and that the parameter values they have assumed, although plausible, are still somewhat arbitrary. They discuss the sensitivity of their analysis to alternative parameter values and to some extensions of the model. Although changes to assumptions about risk aversion, interest rates, and the persistence of earnings shocks have substantial effects on optimal wealth accumulation, in most of their simulations close to half of

all households are still above the simulated median wealth-earnings ratio. Other changes they consider have larger effects. They quantify the consequences of cutting social security benefits by 30 percent, of raising the retirement age by three years, of anticipating inheritances, of increased health care costs for retirees, and of increasing the expected life span by 10 percent. In many of these cases they find noticeable effects on simulated wealth-earnings ratios. Taken singly or together, these alternatives could qualitatively alter the impression of saving adequacy. The authors also discuss the likely qualitative effects of a number of other factors. They suggest that uncertainty about the rate of return on investments, about retirement income, and about preretirement health care expenditure are much less important than earnings uncertainty during the working years. They also reason that the omission from their model of private and social insurance, of private annuity markets, and of the possibility of partial or delayed retirement all bias their results toward concluding that actual saving is below the simulated optimal values.

The authors' conclusion that the distribution of households' wealth accumulation is in rough accord with optimal behavior appears to contradict the conclusions drawn by a number of previous authors using similar methodologies. Yet the authors suggest that the results of many previous studies may be consistent with the general tenor of their own findings. They note that studies that find that a substantial fraction of households have wealth below the mean or the median estimate of optimal wealth should not be taken to imply that those households have behaved suboptimally, but rather that roughly half of the households have had below-average earnings draws. They also observe that the way saving shortfalls are reported may misstate the magnitude of the problem. Bernheim's finding that baby boomers' retirement saving averages only about one-third of that needed to maintain preretirement living standards calculates the one-third on the shortfall between the total resources needed and those provided by social security and pensions. For example, for a relatively low income household with a pension, the combination of social security and the pension might supply, say, 91 percent of what is required. A household whose saving covers "only" one-third of the shortfall would actually have 94 percent of what it needs to maintain its living standard in retirement. The authors are also critical of studies that compare households' preretirement consumption with the consumption that would be allowed by converting the households' wealth into a hypothetical annuity. And they

criticize a number of studies that interpret declines in consumption after retirement as evidence of undersaving, arguing that these declines may well reflect reduced consumption needs.

Although their results leave them more optimistic than many other researchers about the adequacy of household saving, the authors caution that some important uncertainties about the preferences of individuals and about real-world contingencies point to difficulties for particular groups. They note that their study does not include single workers or unemployed couples, both of which are more at risk than married couples who work full time. And they find worrisome reports that one-third of households in their fifties appear not to have thought much about retirement, and that approximately 20 percent of households, including 45 percent of black households and 37 percent of fifty-one- to sixty-one-year-olds who have not graduated from high school, do not even have a checking or a savings account.

IN THE EARLY POSTWAR decades, the economies of Western Europe regularly operated with unemployment rates well below those in the United States. Today that position has dramatically reversed: Europe's lead in keeping unemployment low gradually narrowed during the 1960s and 1970s and was gone by the end of the 1980s. During that decade the United States fully recovered from the deep recession associated with the second OPEC price shock; by contrast, in most European economies unemployment was much higher at the end of the decade than it had been at the start. In the 1990s the U.S. expansion has reduced unemployment to thirty-year lows, while the experience of the European economies has been mixed. In France, Germany, and Italy, which constitute the core of the new European Monetary Union, unemployment rates at the end of the decade are higher than they were at the worst point of the 1980s recessions.

Because most analyses assume that monetary policies have only short-term effects on real economic outcomes, attempts to understand this era of historically high unemployment in Europe have focused on such supply-side factors as safety net policies, labor market institutions, and productivity trends. In the third paper of this issue, Laurence Ball builds on the idea, first proposed by Olivier Blanchard and Lawrence Summers, that unemployment is subject to hysteresis, which could give rise to lasting effects of monetary policies on employment levels. Indeed, Ball's analysis

points to tight monetary policies as the principal source of the chronically high unemployment in many European economies.

Ball amends the familiar NAIRU framework by treating the value of the NAIRU as endogenous in the medium run, rather than as a constant or only slowly moving structural parameter of the economy. He first examines how policy responses affected the NAIRU in the six of the Group of Seven large industrial countries that experienced recessions in the early 1980s: Canada, France, Germany, Italy, the United Kingdom, and the United States. He shows that the two North American countries pursued prompt and aggressive countercyclical policies when their economies fell into recession, whereas the four European countries did not. The North American central banks cut nominal interest rates sharply while their European counterparts held them steady or even raised them, with the result that real interest rates fell an average of 3.4 percentage points in North America and actually rose slightly in Europe. Ball supports this evidence on the stance of policy with accounts of central bankers' policy deliberations from the period.

Ball sees the immediate consequences of these differing responses to recession in the strength of the recoveries that followed. He models the canonical cyclical pattern as one in which easing monetary policy in a recession produces an initially rapid recovery that brings the economy back to near its long-run growth path fairly promptly. After the initial recovery phase, expansions typically return to near trend growth rates. In the North American countries, the quarters following monetary easings followed this pattern. By contrast, France, Germany, and Italy had much weaker expansions in their first two years of recovery. Since he relates the strength of early expansions to the degree of previous monetary easing, Ball identifies monetary policy as the reason these three economies never returned to their previous growth paths. Only the United Kingdom fails to fit this pattern well: the initial pace of its recovery after 1984 was in between those in North America and the other European economies, but growth slowed in subsequent quarters before recovery was complete. Ten years after their recession troughs, output in each of the four European economies was still well below the level predicted by prerecession growth trends. These patterns of output relative to trend produced corresponding patterns of high unemployment relative to prerecession levels in each of the economies.

Ball expands his analysis to a sample of seventeen country members of the Organization for Economic Cooperation and Development (OECD). For this analysis he uses annual rather than quarterly data, since the latter are unavailable for some countries. Although this causes some precision to be lost in measuring recessions, the larger sample allows him both to examine the role of monetary policy econometrically and to investigate the role of unemployment insurance in the rise in European unemployment. Ball makes use of the OECD's estimates of time-varying country NAIRUs. For each country, he calculates a "degree of hysteresis," defined as the ratio of the change in the NAIRU over the five years since the previous cyclical peak to the maximum rise in actual unemployment over that same period. He then uses cross-country regression analysis to examine how the change in the NAIRU and the degree of hysteresis, both measured over the years following the early 1980s recessions, are related both to monetary policy during the recessions and to the duration of unemployment benefits. He measures monetary easing or tightening by the largest change in real interest rates in the first year of a recession; benefit duration is the measure identified in earlier work as the most significant labor market variable.

Ball's cross-country regressions find statistically significant effects from both variables. In the regression explaining hysteresis, the coefficient on monetary policy implies that reducing real interest rates by 6 percentage points, the largest decline in his sample, reduces the degree of hysteresis by 0.54. Since this measure can in principle range between 0 and 1.0, the estimated effect of an aggressive antirecessionary monetary policy in preventing a rise in cyclical unemployment from becoming a rise in the NAIRU is substantial. In the same regression, reducing the duration of benefits from indefinite to half a year reduces the degree of hysteresis by 0.35, also a substantial effect. From a separate regression using the same sample of seventeen countries, Ball reports that the response of monetary policy to recession has no effect on the trend rate of inflation between the cyclical peak and five years later.

The regressions that successfully explain the rise in NAIRUs following the 1980s recessions do not explain the rise in NAIRUs following the generally milder recessions of the early 1990s. The reason, Ball speculates, may be that NAIRUs do not rise indefinitely and were already high in many countries by the start of the decade, or that policies switched so erratically in the early 1990s that measures of the degree of policy easing are unreliable.

After the mid-1980s, some countries succeeded in reducing unemployment while others did not, and Ball examines whether monetary policies and labor market reforms can explain this divergence. He focuses on ten countries that had OECD NAIRUs above 8 percent in 1985, dividing them into two groups according to whether they succeeded in reducing their NAIRUs between then and 1997. Four of the countries—Ireland, the Netherlands, Portugal, and the United Kingdom—did reduce their NAIRUs, whereas the other six did not. Ball first considers whether labor market reforms have been important in explaining this difference in performance. Using the *OECD Jobs Study,* which provides time series on labor market distortions, he finds that some measures, such as the stringency of employment protection, do not change at all over the period, and others change very little. None are correlated with failure or success in reducing NAIRUs. Ball reports that two of the four successful economies used incomes policies—the 1982 Dutch accord on wage restraint and Ireland's 1988 national pay agreements—but that two of the six failures, Spain and Italy, also did so. He also considers less quantitative anecdotal evidence about reforms in particular countries but finds no consistent pattern to the effects of reforms.

To examine potential macroeconomic explanations, Ball relies mainly on indirect evidence about demand-side shocks. Given that there were no important adverse supply shocks in the late 1980s, he reasons that rising inflation represents a demand expansion that pushes unemployment below the current NAIRU. He hypothesizes that hysteresis during such periods reduces the NAIRU just as it increased the NAIRU in periods when unemployment stayed high. Ball shows that the three largest increases in inflation occurred in three of the four economies that succeeded in reducing their NAIRUs. When these countries subsequently slowed their inflations, their NAIRUs stayed at their lower levels or continued declining. The fourth successful economy, Ireland, does not fit Ball's explanation because it experienced no initial large inflation runup. Nonetheless, a scatter diagram comparing the experience with inflation and NAIRU changes in all ten countries further supports the idea that strong demand expansion was important in the success stories and that inadequate demand expansion helps explain the failures.

Ball goes on to construct a formal model that explains hysteresis in unemployment by distinguishing the impact of the short-term unemployed from that of the long-term unemployed on wage inflation. The model

builds on earlier empirical results that suggest, first, that the long-term unemployed have little effect on wages, and second, that firms prefer to fill vacancies from among the short-term unemployed because the long-term unemployed require more costly retraining or are more costly to recruit. Employment in the model is driven by demand. If a rise in unemployment is not met with expansionary policies sufficient to reverse it, some workers become long-term unemployed, raising the NAIRU because their unemployment no longer has an impact on wage setting. But unlike some hysteresis models that see such unemployment as very hard to reverse, in Ball's model firms will incur the recruiting and retraining costs that come with hiring the long-term unemployed once they cannot find other workers to fill vacancies. As such workers are rehired, the NAIRU falls. A demand expansion strong enough to create these conditions will also create some inflation, but in Ball's model that inflation can subsequently be eliminated without undoing the improvement in the NAIRU. As an example of recent experience that fits the broad predictions of his model, Ball points to the United Kingdom. As unemployment there declined from 11.6 percent in 1985 to 6.9 percent in 1997, the fall in long-term unemployment accounted for 85 percent of the decline in total unemployment, and the NAIRU fell from 9.5 percent to 7.2 percent.

FINANCIAL CRISES ARE a recurring theme of economic history books. Although the international financial landscape has evolved dramatically over the past two hundred years, and institutions have changed in response to past crises, in period after period significant new problems have emerged. The generally prosperous 1990s have been no exception. The breakdown of the European Exchange Rate Mechanism (ERM) in 1992 was followed two years later by a run on the Mexican peso and the subsequent abandonment of that currency's peg to the dollar. That was followed in turn by the 1997 crisis that enveloped several East Asian countries and by the 1998 crises in Brazil and Russia. The economic consequences of these events have varied in severity. The ERM breakdown was readily contained by floating the currencies that had come under pressure and adjusting monetary policies. The other crises were followed by sharp downturns in several economies and brought controversial financial support and direction from the United States and the International Monetary Fund. Even with the benefit of hindsight, there is still disagreement about what caused all these crises, what should have been done at the time, and what

can be done to reduce the risk of recurrence. In the fourth paper of this issue, Bradford DeLong provides a historical perspective on these current issues by reviewing crises of earlier eras.

The years immediately preceding World War I were a high point of globalization. That period was followed by a quarter century of war and depression, during which barriers to trade and capital flows were raised. With the gradual removal of these restraints, and with the rise of multinational enterprises able to control and integrate production around the world, the global integration of commerce has now surpassed its earlier peak. However, DeLong sees international financial integration, by some measures, as having been even greater in the past than it is today. He shows that, in the pre–World War I period, flows of net international investment from the United Kingdom had built up that country's foreign holdings relative to national income, to many times those of any country today. He also shows that annual flows of international investment fluctuated widely in the earlier period. Thus both their magnitude and their variability made international financial flows a potential source of financial instability.

DeLong focuses mainly on the developing U.S. economy, which was a major recipient of foreign investment in the nineteenth century. Crises of varying severity emerged frequently—by some counts, the United States averaged one crisis every eight years in the period after the Civil War. Although he identifies no common cause of these episodes, DeLong notes a rough regularity in their aftermaths: foreign investment flows declined after each crisis but generally resumed in force several years later. For example, after many U.S. states defaulted on their debts in 1830, foreign investors shied away from lending to any level of U.S. government for about a decade, but then came back. Not long after the panic of 1857, foreign investment was important in financing the railroad boom of the late 1860s and early 1870s. And although corruption scandals and major bankruptcies dried up capital inflows in 1873, they were again rising sharply by the end of the decade.

Turning to the 1990s, DeLong applies the lessons he derives from these earlier episodes to the ongoing debate about the recent crises. He first considers the significance of what has been called crony capitalism and of related forms of corruption, both as a cause of the recent crises in emerging economies and as an impediment to recovery from them. From a colorful recounting of past U.S. financial scandals, he concludes that crony capitalism was as pervasive in the United States before World War I as it

is in parts of the developing world today. DeLong grants that the resulting deadweight loss from misallocation of capital could have been large both then and now. But from the fact that capital kept returning to U.S. investments even without adequate financial reforms, he infers that financial market reforms are not a precondition for resumption of large-scale capital flows today. That, in turn, is a mixed blessing. He observes that, if reforms are not needed to attract foreign capital, governments of emerging market economies will have little incentive to undertake them on their own. Their incentive is further dulled by two facts. First, the systems that have been so sharply criticized since the crises had supported historically unprecedented rates of growth in East Asia for decades. And second, the design, let alone the installation, of optimal financial systems for developing countries is no easy matter.

DeLong next considers the role of the International Monetary Fund (IMF) and the rescue packages it provides. Rescue packages are traditionally advocated on the grounds that they provide time for needed adjustments to be made when a crisis strikes, and that they provide the resources that countries need to ride out irrational panics on the part of lenders. But are they also a source of moral hazard, contributing to lending excesses and thus increasing the likelihood and severity of a crisis? DeLong notes that there were international financial crises long before the IMF came into being. But he notes that the moral hazard that results from IMF rescues could increase the severity of such crises. DeLong poses the question, "Why hesitate to lend to a banking system in trouble if one is confident that the IMF will bail out the government, the government will bail out the banking system, and the banking system will repay you . . . ?" His own answer, based on historical comparisons, is ambiguous. He reports that crises since 1972 have, on average, been associated with somewhat sharper output declines than crises before 1914. On the other hand, the most severe of the earlier crises had worse outcomes than any from the recent period.

DeLong finally considers the core questions relating to exchange rate regimes. In the half century before World War I, the classical gold standard defined the relationships among the major currencies. Under this regime, investors expected that the exchange rates of well-run countries would be fixed indefinitely or would quickly return to their fixed parities after a crisis. In recent years, however, institutional arrangements have been vastly different: a few major currencies float freely, with only rare interventions

by their monetary authorities, whereas most other countries operate a variety of currency pegs, bands, and dirty floats. DeLong observes that the viability of the middle range of options within which most countries have managed their currencies is now being questioned, and that the preferred alternatives today center around two extremes: pure floating, or tying more firmly to the dollar or one of the other major currencies.

Under the gold standard, the original system for tying currencies firmly together, a country's monetary policy was dedicated to maintaining the gold parity of the currency, or restoring it if the currency temporarily fell below par in a crisis. With expectations fixed by this commitment, international capital flows would help stabilize a currency once the resolution of a crisis was in sight. On the other hand, DeLong observes that doubt about the government's commitment becomes a potential source of disorder, as it did for the United States in the early 1890s. At that time, foreigners feared the Populist movement might gain power and force a depreciation. The high real interest rates required to keep the dollar at its gold standard parity in the face of this perceived risk caused a severe and prolonged depression.

From this history, DeLong concludes that a strong and credible commitment to a fixed exchange rate parity can convey substantial benefits. But he sees problems in making such a commitment fully credible, particularly in the modern world. He notes that even the United Kingdom under a conservative government could not keep foreign exchange traders at bay in 1992 and that, earlier, the disastrous interwar experience of the industrial countries showed the dangers of trying to maintain parities come what may. In contrast to beliefs during the gold standard era, today it is generally recognized that government policies have important short-run effects on economic performance, and elections are decided by the mass of voters who have a strong interest in prosperity and jobs—and little financial interest in hard money. Thus, DeLong reasons, a government's claim that it will support a fixed parity regardless of the macroeconomic consequences cannot be made believable. And without the ability to make such a commitment believable, a fixed parity is vulnerable whenever it is stressed. Thus, on this question, as on those of how corruption and the IMF's role impinge on financial crises, DeLong's historical perspective enriches our understanding of recent events but does not lead him to strong conclusions about how the international financial system might be improved.

EVER SINCE European Economic and Monetary Union (EMU) was launched at the start of 1999, with the euro as its new currency, observers have watched closely to see what changes it would bring to Europe's economic, financial, and business environment. EMU's proponents had seen monetary union as a historic step in the long endeavor to bring financial stability and further economic integration to Europe's economies. Although it is far too early to evaluate the new monetary union's performance against this promise, some early returns are in. The new European Central Bank (ECB) has already both lowered and raised interest rates, the financial and nonfinancial business sectors have responded vigorously to the opportunities provided by the new unified currency, and the value of the euro has declined markedly against other currencies. In a report in this issue, Giancarlo Corsetti and Paolo Pesenti provide a comprehensive review of the early days of monetary union and offer their interpretation of the major developments to date.

The move to monetary union had suffered a setback in the currency crisis of 1992. The crisis caused some countries to leave the Exchange Rate Mechanism (ERM), which had closely linked the exchange rates of most of the currencies of the EU countries, and led to much wider exchange rate bands linking the currencies of those that remained. At the time, these developments cast doubt on whether full monetary union could be achieved. However, from their current perspective, Corsetti and Pesenti argue that, for those countries that remained in the ERM, the crisis strengthened the commitment to the conditions and timetable for EMU that had been laid out the year before in the Maastricht Treaty. By 1998 these conditions, which included convergence of inflation rates, interest rates, and fiscal policies, as well as central banking reforms, were near enough to being met to permit a seamless conversion to EMU at the start of 1999.

Bond markets have historically been much less important for corporate finance in Europe than in the United States. The authors report that bank loans are about three times the volume of bond financing for European corporations, just the inverse of the ratio for U.S. corporations. They note that EMU was expected to make bonds more attractive for all issuers, by removing currency risk and creating a broader and deeper pan-European market. In line with these expectations, total bond issuance surged from an average of $150 billion per quarter in 1998 to an average of $240 billion in the first half of 1999. However, Corsetti and Pesenti offer

several reasons for being cautious about whether a new pattern for European corporate finance has already emerged. They note that bonds issued by nonfinancial corporations in the first two quarters of 1999 were only a small, although rising, portion of these totals. Moreover, much of the growth in issuance has come from U.S. firms and from those European firms that had traditionally financed with bonds. Some issuers had delayed coming to market until after the launch of the euro. Part of the growth in issuance has been driven by the current wave of mergers and acquisitions rather than by conventional financing needs. And bond issuance in other currencies also rose during this period, suggesting that part of the European surge might have occurred without EMU. The authors also discuss the good start of the market for euro-denominated government bonds and are optimistic about the development of a strong secondary bond market. They also believe that the euro will promote cross-country diversification of European equity portfolios, but they suggest this change will occur only gradually.

The new European Central Bank has been the object of intense scrutiny since it became responsible for stabilization policy at the start of the year. Although maintaining price stability is clearly its principal mandate, uncertainty still surrounds how the ECB will operate in practice. One set of issues has to do with how it conducts its deliberations and communicates them to the public. Another has to do with how it will interpret its mandate to focus on price stability if economic conditions in individual economies diverge or if the exchange rate of the euro moves markedly. Corsetti and Pesenti observe that price stability is specified as a range of 0 to 2 percent, as measured by a European index of consumer prices that excludes interest costs, and that it is identified as a medium-run target. They note that both the range and the time horizon provide some leeway for short-term variations in inflation, although the need to establish credibility is likely to keep the ECB from using this leeway in a permissive direction any time soon. When, with signs that the major European economies were slowing, the ECB reduced short-term interest rates by 50 basis points in the spring of 1999, it caught markets by surprise and signaled that the bank would be responsive to prospects for output growth. It also suggested that stabilizing the foreign exchange value of the euro, which had fallen since the start of the year, was not in itself a factor in its decisionmaking. In an addendum, the authors interpret the 50-basis-point increase in rates in the fall as a response to improving prospects

for output growth along with some concern about inflation arising from higher oil prices.

On the broader questions about ECB conduct and targets, Corsetti and Pesenti note that, taken together, the two large rate changes reveal an activist bent at the new central bank. They also see some significance in the fact that, after surprising the markets the first time, the bank signaled its intentions well before raising rates in the fall. This and other announcements concerning the governing board's deliberations may indicate that the governors are sensitive to demands for greater transparency. The actions taken thus far offer little clue about how the ECB will respond to divergences in the economic performance of member countries, which were apparent before the start of EMU and have continued since.

The authors see the decline in the international value of the euro during its first year as consistent with fluctuations in the "synthetic" euro—the bundle of individual currencies that the euro has now replaced—in the years before EMU. Hence they do not regard the euro's decline as a sign of weakness attributable to the euro itself. The authors see the ECB's willingness to cut interest rates in the spring, when the euro was falling, as a clear sign that it attaches importance to cyclical stabilization. And they find no basis for judging under what, if any, circumstances the exchange rate could acquire a more prominent place in the ECB's decisions.