

Editors' Summary

THE BROOKINGS PANEL on Economic Activity held its sixtieth conference in Washington, D.C., on September 7 and 8, 1995. This issue of *Brookings Papers on Economic Activity* includes the articles and discussions presented at that conference. The first article uses plant-level data on both expenditures and retirements to show how microeconomic investment decisions lead to aggregate investment dynamics. The second reviews the changes in regulation, technology, and applied finance that underlie the recent transformation of U.S. commercial banking in order to forecast the industry's future under nationwide banking. The third looks for the common elements in the recent collapse of the Mexican peso and several other currency crises of the last twenty years. The fourth investigates the link between stock price movements and real economic activity, in particular, examining whether stock values have a causative effect. And the fifth article examines the existing evidence on the relationship between the size of government and economic performance, in light of the great increase in government involvement in the industrialized economies since World War II.

IN SPITE OF decades of research, aggregate investment equations are far from satisfactory. Few economists have much confidence in the estimates of the effects of changes in the cost of capital on investment, or in estimates of the dynamics of the adjustment of the capital stock to macroeconomic shocks. In recent years, various attempts have been made to gain insight into aggregate investment by examining firm or industry behavior. Several features of the microeconomic data seem to be at variance with the dynamics of adjustment of the capital stock assumed in the standard neoclassical model. Rather than the partial and smoothed adjustment implied, for example, by quadratic costs of adjustment, investment at the plant level appears to be lumpy and the

probability of an investment spike increases with the time elapsed since the previous spike. These features lend support to the view that at the microeconomic level the adjustment technology is nonconvex, reflecting, for example, fixed set-up costs for undertaking an investment project. However, aggregation is likely to smooth such behavior. It is therefore an empirical question whether explicit aggregation of equations estimated at a highly disaggregated level will provide a better explanation of aggregate investment. Recent research using SIC two-digit postwar U.S. manufacturing data suggests that it may be. For example, allowing for time-varying elasticity of aggregate investment helps to explain the high skewness and kurtosis of the time series of aggregate investment.

In the first article of this issue Ricardo Caballero, Eduardo Engel, and John Haltiwanger make an innovative attempt to use plant data both to improve our understanding of investment behavior at the microeconomic level and to develop an aggregate investment equation that makes explicit use of the distribution of firms with respect to the determinants of investment. The authors focus on equipment investment, utilizing data from a sample of seven thousand continuously operating plants in the U.S. manufacturing sector for the period 1972–88. This is an imposing body of data to analyze. To organize it they use a relatively simple framework that allows separate analysis of the desired capital stock and of the investment that adjusts the actual to the desired stock. Each plant's investment is assumed to depend on the authors' estimate of "mandated" investment, the deviation between the desired and actual capital stock. The estimates of mandated investment by plants are used to estimate a common adjustment rate function which specifies, for each value, the fraction of mandated investment that is to be eliminated by actual investment in one period. The large number of observations allows this to be done without restricting the function's shape. In particular, the authors group plants by the value of mandated investment and, for each group, simply calculate the fraction of mandated investment that is, on average, actually undertaken. They then use the adjustment function to calculate aggregate investment for a variety of distributions of mandated investment.

The first step in filling out this framework is to estimate, for each plant and time period, the desired and actual equipment stocks, and their difference, mandated investment. The authors construct the actual

stock by a perpetual inventory method utilizing plant-level retirement information and depreciation rates from the appropriate two-digit industry. The desired capital stock at any time is assumed to depend on the plant's current output and the cost of capital. The elasticity of desired capital with respect to output is inferred from capital's share of costs. However, unlike many earlier investment equations, the elasticity with respect to the cost of capital is separately estimated, but with the restriction that the elasticity is the same across all plants in a given two-digit sector. The desired capital stock is also allowed to differ across plants by a plant-specific constant.

At the aggregate level the sensitivity of equipment investment to the cost of capital is of great interest. It is one of the major channels by which monetary or tax policy may affect the rate of capital formation. In the authors' framework, changes in the cost of capital affect investment by changing the desired capital stock and creating a discrepancy between actual and desired holdings. The cost of capital series is constructed using industry-specific price deflators, depreciation rates, and tax credits, together with an economywide corporate tax rate and an assumed constant real interest rate. These series show substantial variation over time and across industries. The authors show that the changes over time in the cost of capital reveal the effect of tax changes, with a large favorable shock in the early 1980s and a substantial adverse shock following the tax reform of 1986. These aggregate variations in the cost of capital are of the same magnitude as aggregate "profitability shocks" reflecting common shocks to the ratio of output to capital. At the plant-specific level, the relative variation in profitability is much greater, no doubt reflecting, in part, the fact that the cost of capital is taken to be the same for all plants in a given industry. Estimates of the elasticity of desired capital with respect to its cost vary widely across industries, from a low near zero to a high near minus two. The estimates average approximately minus one, the long-run elasticity implied by the standard Cobb-Douglas assumption.

Given estimates of the output and the cost of capital elasticities and the plant-specific factor, it is straightforward to compute a time series of mandated investment and the combined effect of the cost of capital and profitability shocks on desired capital for each plant. The distributions of these variables, and of equipment investment across plants and time, provide a rich picture of the underlying determinants of

aggregate investment. Pooling all of the observations, shocks to desired capital look roughly normal. However, the distribution of actual equipment investment relative to capital is heavily skewed in the positive direction and displays high kurtosis. These two facts taken together are a clear suggestion of the presence of nonconvexities and asymmetries in the adjustment technology. The concentration of mass is consistent with the view that, in a given time period, many firms are not heavily engaged in either the sale or acquisition of equipment; the fat positive tail of the distribution is consistent with adjustments, when they are made, being large. In contrast with the distribution of investment, the pooled distribution of mandated investment, while not noticeably skewed, is less peaked than the normal distribution. Such a distribution would result if firms were inactive when they were near their desired capital stock, rather than always adjusting part-way toward that level. In addition to the pooled distributions, the authors calculate the kurtosis of the distributions period by period. In part because of shifts in the means of the distributions over time, the pooled distributions show less kurtosis than the average of corresponding temporal distributions, but otherwise give a very similar picture.

The significant departures from normality in the distributions of investment and mandated investment call into question the standard quadratic cost of adjustment model. In the quadratic case, investment-to-capital ratios and mandated investments are both linear combinations of previous shocks; since shocks appear roughly normal, these distributions should themselves both be normally distributed. The shape of the adjustment function provides more direct evidence on the nature of adjustment costs. The commonly assumed quadratic costs of adjustment imply partial adjustment at a constant rate; the fraction of the gap between desired and actual capital that is closed in any one period is independent of the level of that gap. By averaging the adjustment rate for firms falling into narrow intervals of mandated investment, the authors create an estimate of the entire adjustment function and find that it is not consistent with the quadratic cost-partial adjustment model. They find that when mandated investment is negative because plants have excess equipment, the function is flat at a low rate. For positive levels of mandated investment the adjustment function is positively sloped, with the rate of adjustment for high values of mandated investment many times that for low values. These properties are to be

expected if adjustment costs are nonconvex and investment is difficult to reverse.

While the estimated adjustment function indicates that investment increases more than proportionally with mandated investment, there is substantial variation in investment across plants for any level of mandated investment. The authors display histograms of the investment-to-capital ratio for plants with high and low levels of mandated investment. At high levels most plants invest, and many invest at rates of over 50 percent of their capital stocks. At low levels of mandated investment, on the other hand, a large number of firms do not invest at all; indeed, the distribution is very similar to that of firms with negative levels of mandated investment.

The authors also estimate an adjustment equation parametrically in the form of a fourth-order polynomial plus a sector-specific constant term. The resulting investment equation therefore depends on the first five moments of the distribution of mandated investment. This equation is estimated by OLS, using the cross-sectional moments as explanatory variables. They also estimate the equation restricting the coefficients on the higher moments to be zero, as implied by partial adjustment. The authors find that, when freely estimated, the parameter values imply an average adjustment equation that is qualitatively similar to the average adjustment equation that they had found nonparametrically from plant observations. The adjustment function is clearly increasing for capital shortages and close to zero for retirement decisions.

How much difference does this deviation from partial adjustment make to predictions of aggregate investment? Is there sufficient variation in the shape and location of the underlying distribution of mandated investment to lead to predictions over the cycle or through time that are substantially different from those made by the simpler model? An increasing adjustment function matters most when a large shock results in a distribution of mandated investment with substantial mass at high levels. The authors find this effect in their data. The authors also illustrate the importance of the adjustment nonlinearity by showing that the marginal response of the aggregate investment-to-capital ratio varies widely over the sample. Using a decomposition of the effect of a shock into a linear portion corresponding to partial adjustment and a nonlinear portion, they find that including the nonlinear adjustment can be quite important. The nonlinear effects are especially important during 1986

when the nonlinear equation predicts a decline in investment 20 percent greater than the prediction from a linear equation.

The authors conclude that the view of investment behavior that emerges from their examination of microeconomic data is not only sensible, but is quite different from that commonly assumed. Although there is substantial noise in their plant-level data, equipment investment appears consistent with the existence of nonconvexities in the cost of adjustment, and there appears to be substantial variation over time in the distribution of discrepancies between desired and actual capital stock. Taken together, these two facts imply that the response of aggregate investment to common shocks, either to profitability or to the cost of capital, is likely to depend on the state of the economy and on the previous history of shocks. The authors recognize their analysis pays too little attention to dynamic factors, including the persistence of shocks and an allowance for the time needed to build. Much remains to be done. But the paper suggests the potentially great value of using microeconomic information in explaining and predicting aggregate investment.

SINCE THE LATE 1970s, the U.S. banking industry has been transformed by changes in regulation, advances in technology, and innovations in finance. Over one-third of independent banking organizations have disappeared during this period, even as the assets of the banking system have been growing. Once the remaining barriers to interstate banking are removed after 1995, observers expect the wave of mergers to continue and nationwide banks to emerge. How far this consolidation will go, how many small banks will survive, and how the historical activities of banks will change are all unresolved questions. In the second paper of this volume, Allen Berger, Anil Kashyap, and Joseph Scalise analyze developments in banking over the past fifteen years and address these and related questions about the future of the banking industry.

The authors first review the key changes that have taken place since the end of the 1970s. They divide regulatory changes into five categories: reduction in reserve requirements, expansion of bank powers, tightening of capital requirements, deregulation of deposit accounts, and relaxation of restraints on geographic diversification. The first two types of change improved the competitive position of the banking in-

dustry. Reserve requirements were reduced three times over the period studied and only a 10 percent requirement on transactions balances now remains. Regulations have been relaxed so that today banks can pursue a number of potentially profitable activities that were previously prohibited. For example, bank holding companies can have separately capitalized subsidiaries that operate mutual funds and offer investment advice, provide brokerage services, and underwrite a range of securities. Changes in capital requirements have probably reduced bank profits. These requirements, which had been largely ad hoc through the 1970s, subsequently underwent two distinct changes. Starting in 1981, banks were required to hold capital equal to a flat percentage of balance sheet assets, regardless of their type. These rules provided incentives both to reorganize balance sheet portfolios and to shift into off-balance sheet activities. The Basle Accord, which called for risk-based capital standards that applied to both on- and off-balance sheet assets, addressed these problems starting in 1990. The authors show that banks, and especially large banks, held much more capital relative to assets at the end of the period than they had at the end of the 1970s, indicating that the new capital standards were probably costly to them.

The authors argue that the other two major regulatory changes—deposit account deregulation and liberalization of geographic expansion rules—had more ambiguous effects on the industry. Banks had historically earned monopsony profits because ceilings on the interest rates that they could pay meant that they acquired deposits at below-market rates. By the 1980s, market innovations such as money market mutual funds had eroded this position by drawing deposit funds to nonbank institutions. The deregulation of deposits between 1981 and 1986 permitted banks to compete for funds, but they now had to pay competitive rates. Without deregulation, they would have held far less in deposits but would have paid less for the deposits that they did hold. The authors observe that the ability to diversify geographically has increased gradually over the past fifteen years and has favored those banking organizations that wanted to expand while hurting those whose markets were invaded. But until now there have still been major barriers to bank consolidation across state lines.

The Riegle-Neal Act of 1994 will remove all barriers to interstate expansion of banks, ushering in an era of nationwide banking. To help project the effects of this change, the authors quantify some key di-

mensions of the banking transformation that has already occurred. First, assembling data from the quarterly Reports of Condition and Income (Call Reports), they track the distribution through time of gross domestic assets by five asset size classes of banking organizations (all expressed in real 1994 dollars): “small banks” with assets under \$100 million; three size classes of “mid-sized banks” with assets ranging between \$100 million and \$100 billion; and “megabanks” with assets greater than \$100 billion. In their classification, all assets controlled directly or indirectly by a bank holding company are aggregated into a single “bank.”

The authors document a striking amount of consolidation in the banking industry over the 1979–94 period. Although total real banking assets rose by nearly one-quarter, the number of banking organizations declined by more than one-third. Nearly all of this decline is explained by the disappearance of over four thousand small banks. The fraction of assets held by small banks dropped from 13.9 percent to 7.0 percent, while the fraction held by megabanks doubled from 9.4 percent to 18.8 percent. The authors attribute this consolidation to the relaxation of geographic restrictions on branching, an easier process for approving mergers, and innovations in information processing and telecommunications. They also identify other changes attributable to technical and financial innovations. The value of derivative positions in megabanks soared, and the share of their noninterest income, which includes fees from such activities, rose from 7 percent to 21 percent over this period. These figures suggest that asset size is an inadequate measure of banks’ economic importance, and that management of market risks has become a crucial skill for large banks. Other innovations have transformed the banking business in different ways. The number of ATMs has multiplied eight times, and the cost of processing electronic pay deposits has fallen to 15 percent of its 1979 level.

External competition, from less-regulated domestic financial institutions, foreign institutions, and direct financing by borrowers, has reduced, by most measures, the market share of U.S. banks. Nonetheless, since overall financial activity has grown, the banking industry has not actually declined but rather has grown more slowly than it would have otherwise. Between 1979 and 1994, real gross assets of the industry grew by 23 percent while real GDP rose by 41 percent. The total credit market debt of individuals, businesses, and governments more

than doubled, while the share of this debt held by U.S. banks fell from 26 percent to 17 percent. Similarly, total non-credit market debt of intermediaries more than doubled, while the share held as deposits at U.S. banks dropped by one-third.

Finally, the authors show that the share of U.S. banks in lending to nonfarm, nonfinancial corporate business fell from 20 percent to 15 percent over the 1979–94 period. The decline was more than offset by increased lending by foreign banks, mostly from offshore sources. But while foreign banks now have nearly half of all bank loans to nonfarm nonfinancial corporations, the authors suggest that this share exaggerates their importance as a source of financing for U.S. business. Much of foreign banks' lending is to home country clients with U.S. operations, replacing a foreign rather than a U.S. bank loan. To a greater degree than domestic banks, they buy loans originated by other large banks. And they are unlikely to lend to small business borrowers.

The evolution of bank lending, especially lending to small business borrowers who typically have trouble raising funds cheaply from alternative sources, are a special focus of the authors because of their potential significance as nationwide banking develops. They show that real bank commercial and industrial (C&I) loans declined by 23 percent from the end of 1989 to the end of 1992. Loans dropped proportionately more at small banks than at megabanks, indicating that the implementation of risk-based capital standards, which impact the largest banks the most, were not responsible. In the following two years, C&I lending recovered only a small part of this decline, suggesting that it was part of a long-term development.

Building from the Federal Reserve's quarterly Survey of the Terms of Bank Lending to Businesses (STBL), the authors estimate the distribution of loans by bank size and borrower size. They confirm that bank size and loan size are highly correlated. In 1994 megabanks devoted only 2.5 percent of their domestic C&I loans to small borrowers. Conversely, banks with less than \$100 million in assets made 82 percent of their loans to small borrowers and made almost no loans to large borrowers. The authors also estimate the lending patterns for different sizes of borrower during 1989–94, a period over which total real C&I loans declined by 19 percent. Their estimates confirm that the lending slowdown of 1989–92 disproportionately affected small borrowers. In the following two years their bank borrowing recovered, but only a little,

so that by the end of 1994 real loans were still down 35 percent to small borrowers and down 42 percent to very small borrowers.

Loans to medium-size borrowers fell 14 percent during the lending slowdown years and were down only 2 percent by 1994. Surprisingly, loans to large borrowers fit neither of these patterns. They fell 26 percent during the lending slowdown years and then declined further, so that by the end of 1994 they were 35 percent below 1989 levels. From these patterns, the authors infer that something more complicated than an aggregate reduction in either the supply of or demand for loans has been at work. They discuss several potential explanations and attach particular importance to the idea that the reallocation of assets from small to large banks accounts for the decline in lending to small business, although this consolidation hypothesis clearly cannot explain the decline in lending to large borrowers.

Armed with these historical patterns, the authors turn to simulations to address two important questions about the future of the banking industry: How much consolidation is likely to occur once the Riegle-Neal Act permits full nationwide banking over the next few years? And what will be the effect on lending, particularly to small businesses? They first explain the within-state distribution of bank assets among bank size classes, using the changing state restrictions on branching and merging and also state demographic characteristics as explanatory variables. They then use these estimated effects of regulatory restrictions to simulate the effect of removing restrictions at the national level.

The authors provide both a simulation that assumes no growth in total bank assets and a simulation that assumes that each state's assets grow by 1.71 percent a year, the national average over 1979–94. In the no-growth case, substantial consolidation of the banking industry occurs quickly, although an important number of small banks remain. Within five years, the number of banks falls by almost 4,000, from 7,926 to 4,106. The share of domestic assets controlled by megabanks more than doubles from 19 percent to 42 percent, while the share of small banks falls from 7 percent to 3.5 percent. There is very little additional change after the first five years, and the authors note that this quick adjustment is consistent with the rapid response to regulatory change that has been observed at the state level. They also note that the predicted distribution of assets resembles the present distribution in California, where statewide branching has been permitted since 1909.

In the simulation that allows for total asset growth, the first five years show only slightly more consolidation, but thereafter the differences widen. After twenty-five years only 1,939 banks remain and fifteen megabanks control 47 percent of all banking assets. The authors believe that these differences from the no-growth simulations primarily reflect the historical positive relation between the amount of bank assets in a state and the proportion of assets in large banks.

The authors apply their simulations of industry consolidation to project the effect on credit flows. They assume that banks in each size category maintain the ratios of loans by borrower size to assets at their 1994 levels. As assets are redistributed among banks of different size, lending to borrowers of different size changes. Over the first five years, lending to large borrowers grows by 33 percent in the simulation with no growth in the economy, and by 50 percent with growth. However, the authors warn that these projections take no account of the decline in lending to large borrowers that has been observed over the past five years, a period when the considerable bank consolidation that occurred might have led to predictions of increase rather than decline in such lending. They attribute the recent decline to external competition and innovations that were relatively more helpful to alternative sources of finance, and they make no attempt to project such effects in the future.

Loans to small borrowers in the no growth projections drop by 32 percent in five years, with little change thereafter. The authors observe that these changes are actually smaller than the decline in small business lending that has occurred over the past five years. In the projections with growth in bank assets the results are more complex. In the early years, the reduced share of assets in smaller banks is about offset by the growth in total assets and small business lending is about the same as in the zero growth case. In later years, asset growth dominates and small business lending is somewhat higher. The authors acknowledge that institutional changes could produce alternative sources of finance for small business if these projections based on past patterns fall short of the economically efficient needs of small borrowers. However, the authors have no basis for anticipating such change. And they reason that some small business lending, which typically has been conducted on the basis of relationships between bankers and borrowers, may not have been efficient and may not be replaced in the future.

NEARLY A YEAR after the event, observers are still divided over the causes and handling of the Mexican peso collapse in late 1994. Some believe it was brought on by the bungling of the new administration in Mexico and argue no devaluation was needed. Others believe the currency collapse was inevitable because the government had for too long maintained the peso at a value that made it increasingly overvalued. Virtually all observers have been surprised by the severity of the problems that have emerged since the currency started to fall. In an earlier Brookings paper (1:1994), Rudiger Dornbusch and Alejandro Werner analyzed the then emerging problems in Mexico and anticipated the peso devaluation. In the third paper of this volume, Dornbusch, Ilan Goldfajn, and Rodrigo O. Valdés revisit the peso crises together with several other currency collapses in an attempt to identify common features of those experiences. They also review why recent devaluations in Europe have been more successful. And they look at the present prospects for Argentina and Brazil.

The authors' central premise is that overvaluation resulting from exchange rate policies has been the central common factor in at least four currency crises during the past fifteen years: Chile, 1978–82; Mexico, 1978–82; Finland, 1988–92; and Mexico, 1990–94. While the details of policy and economic performance vary in these four crises, they see broad similarities in the way the government's attempt to use a nominal exchange rate anchor to fight inflation led eventually to serious financial and real economic difficulties.

In Chile in 1979 the military government, having introduced many promarket reforms but with prices still rising by over 30 percent a year, fixed the peso-dollar exchange rate as a way to break an inflation-devaluation cycle. The authors show that inflation slowed, but not enough to prevent a substantial real appreciation. By August 1981, the real exchange rate was 60 percent higher than its 1970–80 average, and the current account deficit was 14.5 percent of GDP. The external deficit had been easily financed because Chile's reforms, including budget surpluses, and the rapid expansion of the economy after the deep recession of 1975 made Chile newly attractive to foreign lenders. Although inflation was largely conquered and wholesale prices actually declined after the summer of 1981, the economy by then had fallen into recession and pressure on the exchange rate mounted. With reserves dwindling,

the peso was devalued by 18 percent in mid-1982, and by much more in the following years.

The Mexican crisis of 1982 took place against a chaotic economic background. The world was in recession following the OPEC II oil price increases and high interest rates, while the Mexican economy had been growing rapidly under the spur of budget deficits and negative real interest rates. Although the nominal exchange rate was fixed after mid-1978, with only occasional adjustments, the inflation rate was near 20 percent in 1978 and 1979 and exceeded 25 percent in the following two years. By the end of 1981, the real exchange rate was 37 percent higher than it had been at its trough in 1977.

The exchange rate was finally devalued by 68 percent in early 1982, but the authors judge that action to have been too little, too late. The current account deficit had worsened to \$16 billion in 1981. With the currency convertible but overvalued and negative real interest rates, asset holders had been moving investments abroad and shifting to dollar deposits in the Mexican banking system. Inflation soared to near 60 percent during 1982, while budget policy became sharply more expansionary ahead of the election. Capital flight intensified and Mexico was unable to meet its dollar interest obligations. The peso was devalued by almost 100 percent in late 1982, and by much more in subsequent years.

In the Finnish experience of the late 1980s and early 1990s, the authors see another example in which growing overvaluation eventually led to currency crisis. Finland liberalized and deregulated its economy during the 1980s, opening its financial markets both domestically and internationally. During most of the decade it experienced rapid expansion, budget surpluses, and only moderate inflation as the authorities kept interest rates high. However, in 1989, with inflation having crept up to around 6 percent, but with demand already slowing, the central bank appreciated the Finnish markka as an anti-inflation measure. According to the authors, this appreciation, together with the collapse of Russian trade and retrenchment by the banking system which had overlent in the years following deregulation, helped start a severe recession in 1990. An official attempt to peg the markka in mid-1991 as a way to restore credibility and lower interest rates, soon had to be reversed. The authors observe that throughout the crisis, credibility was

the focus of the exchange rate strategy: the way to lower interest rates was to emulate Germany's anti-inflation stance. By contrast, they argue that such a strategy could not achieve the crowding-in that was needed, and believe that a lower currency value was needed and would have permitted lower interest rates and rejuvenated the economy. In fact, growth did not resume until real depreciation helped expand exports in the mid-1990s.

The second Mexican crisis, which is still unresolved, followed a period in which Mexico moved forward under market-oriented reform policies that were widely heralded in the United States and other advanced countries, and that culminated in the North American Free Trade Agreement. International lending had resumed even before the previous Mexican debt had been restructured using Brady bonds and, by the 1990s Mexico was attracting large amounts of portfolio investment from abroad. Yet the authors identify three unsatisfactory developments of this period. Growth was slow, so that per capita income remained below its peak in 1981, before the first Mexican crisis. Inflation continued in the 20 to 30 percent range through 1991, although it slowed thereafter. And the peso again experienced a real appreciation. Real appreciation need not produce overvaluation, and indeed, the Mexican authorities insisted that it had not. With reforms and investment boosting productivity, it could be argued that real appreciation could be sustained. But the authors reason that the widening trade imbalance, at a time when the economy was not booming, showed that the currency had in fact become overvalued and was vulnerable to capital outflows.

During 1994 a series of political events disturbed markets that were already growing skeptical. In response, the government sterilized the capital flight and converted peso debt to dollar debt, development banks supplied credit, and the pressure on the peso was resisted until after the election. When devaluation came, its effects were made worse by the large amount of debt incurred in dollars during the time the government was resisting devaluation, and by the disillusionment of the finance community that had continued investing in Mexico on the assurances that there would be no devaluation. Had it not been for the intervention by the United States and the IMF, the authors conclude that Mexico would again have defaulted on its debts.

The authors contrast the currency collapses in Chile, Finland, and Mexico, each of which was part of a deep and lasting setback in eco-

nomic performance, with the devaluations that took place in Italy, Spain, and the United Kingdom in 1992. In these three European cases, disinflation and possible eventual monetary union was the official objective behind pegging the exchange rate. The policy did reduce inflation, but at the cost of slow growth, high unemployment, and high real interest rates, which cast doubt on the resolve of policymakers to stay the course. Speculators tested that resolve and, one after another, the vulnerable currencies fell. In each country, real currency values and real interest rates declined, and faster growth followed. Yet, surprising many observers, inflation increased only very modestly. The authors attribute the vast difference between these outcomes and the economic disasters that accompanied the currency collapses in Chile, Finland, and Mexico mainly to the repercussions on the respective financial systems. In the latter countries, the large balance sheet impacts of the currency realignments crippled their banking systems, while no such financial fallout followed the European episodes.

The authors summarize three conceptual views that have framed the discussion of real exchange rates: a monetarist view and a classical view, both of which they reject for ignoring key aspects of actual performance, and a disequilibrium view, which they see as encompassing the essentials of the currency problem. In the monetarist and classical views, there is no reason for policymakers to focus attention on the nominal exchange rate. In the disequilibrium view, because there is inertia in wage and price inflation, a policy that constrains the nominal exchange rate appreciates the real exchange rate. The problem is made worse when some reforms, such as trade liberalization, or other shocks, such as budget cutting, require crowding-in and, to achieve this, real depreciation. The relevant policy choice is whether to accomplish desired changes in the real exchange rate by moving nominal exchange rates or through deflation or inflation. Compared with moving the nominal exchange rate, the authors believe that the other routes will generally have high real costs and take a long time.

Many observers of currency crises have emphasized the role of currency speculators and footloose capital as opposed to misguided policies. The authors stress that both are involved and that their roles are interrelated. Changes such as trade liberalization or budget cutting may be viewed favorably in asset markets, especially when they are part of an overall market-oriented reform package, and so may encourage cap-

ital inflows and appreciate a currency when real depreciation is needed. When the conditions for a crisis approach, policymakers are surprised by how the same markets turn on them, and markets are surprised by how little liquidity there is when everyone scrambles for the exits together. Moreover, to the extent that the credibility of policies is tied to the existing exchange rate regime, when that regime is abandoned the effects on credit availability are greatly magnified.

Finally, the authors assess prospects for Brazil and Argentina, both countries that have experienced strong real appreciations. They suggest that Brazil should devalue as part of a comprehensive stabilization program, while Argentina should hold out and foster deflation. In Brazil's case, currency reform brought inflation down from near hyperinflation rates to annual rates of near 20 percent in early 1995. In the process, the currency appreciated by 15 percent in real terms. The authors see problems in this appreciation, since pending reforms and budget cuts will restrain demand, and interest rates cannot be reduced sufficiently to compensate given the aggressively valued currency. But they warn that devaluation would be dangerous without reforms that first reduce the very high degree of indexation in wages and prices that currently exists. In Argentina's case, they argue that devaluation is not an option because the economy is already effectively on a dollar standard, with all money creation fully backed by increases in foreign exchange reserves. If the peso goes, Argentinians will stay with the dollar, which is already the accepted means of payment. Thus they conclude that Argentina has no option but to work its way out of overvaluation through deflation in the domestic economy.

THE RELATION between the stock market and the real economy is a subject of continuing interest to economists and the financial community. Changes in stock prices reflect, among other things, changes in expected future earnings and so should be a leading indicator that predicts real activity. Standard theory also suggests two important avenues through which variations in stock prices should not just predict, but should actually influence real spending in the economy. First, since they affect the cost of capital to firms and Tobin's q , stock prices should help determine investment spending. And second, since stocks are a large share of aggregate household wealth, stock prices should help determine consumer spending. In the fourth paper of this volume, James

M. Poterba and Andrew A. Samwick take a fresh, empirical look at how the stock market may affect the economy through this second avenue, investigating the stock market's effect on consumer spending and attempting to distinguish this from its role simply as an indicator of future economic activity.

The authors start by putting the 1995 stock market rise in perspective. Using data through midyear, they estimate that rising stock prices had already added roughly \$1 trillion to the total real value of U.S. corporate stock. The rise since then has about doubled this increase for the year as a whole, providing a spectacular increase in the wealth of stockholders. The authors also present several commonly used indicators of whether average stock prices are high or low relative to historical norms. As of mid-1995, most such indicators showed stock prices at or near historic highs. These include the dividend payout rate and, adding share repurchases to dividends, the total payout rate; Tobin's q , which compares the market value of shares with the replacement cost of firms' fixed capital; and the ratio of the market value of shares to GDP. By contrast, the price-to-earnings ratio was near the middle of its historic range.

Poterba and Samwick note that although corporate stock is ultimately owned by individuals, the form of ownership—whether it is direct or through some form of intermediary—may affect the impact of price fluctuations on the owners' behavior. For example, stock held in the defined benefit retirement plans of a state or local government has no connection with the wealth of beneficiaries and only an extremely indirect effect on the taxpayers of the community responsible for paying the benefits. Even stocks in retirement accounts that are owned by the beneficiaries, such as IRAs, 401(k)s, or other tax-deferred plans, may be viewed differently by their owners than stocks held in other forms, in part because the tax-sheltered plans have penalties for early withdrawal.

To examine the significance of changes in stock ownership patterns, the authors first examine the postwar history of individual stock ownership. They adjust the "household sector" ownership category in the Flow of Funds accounts in several ways to make it more appropriate to explaining consumption: they remove nonprofit institutions, add stock held by bank personal trusts, add stock held in defined contribution retirement plans and variable annuity reserves at life insurance

companies, and add stock mutual funds owned by households. For 1994, these adjustments raise the share of stocks owned by individuals from the 47.7 percent implied by the Flow of Funds data for "households" to 63.7 percent. Compared with a decline of 42 percentage points since 1952 in the Flow of Funds household category, the adjusted data show a decline of only 15 percentage points in the share held by individuals since 1952, and a stable share since 1982.

Poterba and Samwick also summarize evidence on the concentration of stock ownership using data from the Survey of Consumer Finances. They find the proportion of households with any stock holdings (excluding trusts and variable annuities) has risen substantially, from 19 percent in 1962 to 33 percent in 1992. This broader participation has had little effect on the share of stock owned by the bottom 80 percent of households, though there has been a noticeable change in the distribution of ownership between the very top and the near top households. The distribution of stockholdings changed little between 1962 and 1983. Then between 1983 and 1992, the share of equity held by the top 0.5 percent fell from 55 percent to 37 percent, with nearly all this decline mirrored by increases in the share held by the remainder of the top 20 percent. The holdings of the bottom 80 percent increased only from 1 percent to 2 percent of all equities. Not surprisingly, the distribution of ownership is also concentrated by age, though less so in 1992 than in 1983. Households headed by persons over 44 years old owned 78 percent of stock in 1992 and 88 percent of stock in 1983.

Turning to statistical analysis, the authors first present time-series regressions for several broad consumption categories that summarize the relation between consumption and stock values. For total consumption, a 10 percent rise in stock prices is associated with a 0.64 percent rise in consumption over the next four quarters. The effect is greatest for durable goods, for which the associated consumption increase is 2.9 percent, and weakest for services, for which there is no association. By these estimates, a 35 percent increase in stock prices, such as occurred over the first eleven months of 1995 and which amounts to an increase of about \$2 trillion in wealth, predicts a consumption increase of about \$100 billion, or 5 percent of the wealth increase. This response is very similar to estimates from other studies that assume a structural relation between consumption and wealth. However, as the authors emphasize, it is unclear from regressions such as these whether a causal

wealth effect lies behind the regression results, or whether they simply reflect the stock market's role as a leading indicator.

To try to distinguish between these two possibilities, Poterba and Samwick devise tests exploiting the fact that the distribution of stock ownership is so highly concentrated. They first identify five broad categories of goods and services that are disproportionately consumed by high-income households. Regressions show that only for one of these, new cars, does the stock market help explain spending on that category relative to total consumer spending. And when they separate luxury and upper-luxury cars from total new cars, they find that stock prices do not contribute to explaining the ratio of their sales to total car sales, suggesting that in the equation for new car sales, the stock market was significant as a leading indicator rather than as a causal variable.

Turning to PSID household survey data, Poterba and Samwick find weak support for a causal connection running from stock values to consumption: separating households into three categories according to the amount of stock they hold, and using proxies for total consumption by household, they find the correlation between consumption growth and stock market returns is higher the more stock a household owns.

The authors next examine the effects of changing stock ownership patterns. If stock prices are only a leading indicator, then changing ownership patterns should not matter, while if they are causing consumption through wealth effects, then changing patterns should matter. They compare the explanatory power for consumption of stock prices alone with the explanatory power of a variable that interacts stock prices with the share of stock that is owned by households. Although the interactive term performs slightly better, the collinearity of the alternative explanatory variables is so high that the hypothesis of no effect from the share variable cannot be rejected.

Some empirical studies suggest that changes in stock prices that are associated with changes in earnings or dividends are more likely to be permanent than are changes without this association. Informed by this evidence, the authors examine whether dividends or earnings add to the predictive power of stock prices for consumption. But in a series of equations explaining total consumption and its major components, they find almost no evidence that earnings or dividends have such an effect. Thus stock price increases appear to have similar relations to consumption whatever their source.

On the basis of all their tests, Poterba and Samwick conclude that there is only slight evidence of a wealth effect through which stock prices affect consumption, a finding that challenges a widely used, traditional model of consumption. However, they note that consumption may respond so gradually to stock market wealth that the effect is missed by their tests. Or, since stock prices seem to predict future activity, they may affect consumption through channels such as consumer confidence rather than wealth, an effect that would not be detected by many of the authors' tests.

THE GROWTH of government has been one of the most dramatic changes in the structure of industrialized countries in the post-World War II period. In twenty-four OECD countries, government expenditures as a ratio to GDP rose from less than 35 percent in 1970 to a plateau of over 45 percent by 1982. From the beginning there have been critics as well as supporters of this growth, and in recent years a strong political movement has emerged arguing that much of government activity is unproductive. In the fifth paper of this issue, Joel Slemrod critically reviews the evidence that has been put forth on the effects of government on economic activity. One major difficulty in assessing the costs and benefits of government is that many of the goals of government intervention are not directly related to usual measures of economic activity. The distribution of income, provision of minimum levels of shelter and medical care, condition of the environment, and insurance against social risks may all affect individual or societal welfare but will affect measured output indirectly, if at all. Since they are not valued in markets, assessing the benefits of pursuing these goals inevitably involves value judgments. Slemrod avoids this difficulty by limiting his review to the effects of government on conventional measures of economic prosperity and growth. He reasons that if there is agreement about these net costs or benefits, they can be stacked up against the more subjective benefits of government involvement, thus narrowing the debate. Unfortunately, however, even this more limited objective is difficult to achieve; economists are far from a consensus about the effect of government on measured economic activity.

Slemrod distinguishes between two broad approaches to estimating the economic effects of government, "top-down" and "bottom-up." The top-down approach attempts to infer the net costs of government

from the relationship between aggregate measures of taxes or expenditures and the level or rate of growth of GDP. The bottom-up approach estimates net costs program by program and tax by tax. Slemrod believes that both approaches are fraught with difficulty and that persuasive evidence is hard to find. Simple correlations between the extent of government and the level of GDP per capita illustrate the problem. Both for a given country over time and across countries, such correlations are frequently positive. For example, there is a strong positive association between the size of government and output over the period 1929–82, not only for the United States, but throughout the developed world. The unprecedented growth of government has occurred over the same period as the unprecedented growth of output. However, that this relationship is causal is implausible even to many of those who believe the overall effects of government on welfare are positive; such individuals would not be surprised to find a negative effect of government on measured output, since a large fraction of the benefits of government are not captured in measured output and most costs are. Across countries, the relationship between prosperity and the extent of government is less clear. No obvious correlation exists for either tax or expenditure ratios in 1990, and the positive relationship for a larger sample of countries apparently reflects the difference between the high-tax OECD countries and the rest of the world. Slemrod is not aware of any serious academic study that purports to demonstrate a significant negative causal relationship between the extent of government and the level of prosperity.

Slemrod believes that a fundamental difficulty comes from simultaneity in the determination of GDP and G , measuring the size of government. While a large G , if unproductive, may decrease the level of measured output that can be expected from an economy, it is also true that the demand for G depends on the level of output. In order to clarify the resulting identification problem, and the resulting estimation bias, Slemrod analyses a simple model in which G affects measured output, Y , and in which G itself is determined by an optimizing government. In any empirical application, each of these relationships is subject to error. Variations in costs across countries or time can be expected to bias upward single equation estimates of the effect of G on Y . For example, consider two countries with different but constant marginal costs of G and with the same marginal benefit schedule.

Ceteris paribus, the country with the higher costs will have the lower G , but also the lower Y . A cross-country regression will show a spurious positive relation between G and Y . Ideally the investigator could identify the cost curve by finding variables that influence the benefits of government activity but do not enter in the cost curve; but Slemrod doubts that this can be done in practice.

With this framework as a backdrop, Slemrod gives a broad review of the vast empirical literature investigating the relationship between the extent of government and the level of prosperity. Remarkably, except for a few studies, the literature makes hardly any reference to the simultaneity problem. However, no one appears to have had the temerity to simply regress Y against G , perhaps because it takes courage to assert that no important unmeasured influences on Y would be correlated with G . In contrast, Slemrod notes that scores of empirical studies by economists, political scientists, and sociologists try to explain G , or the growth of G , and some of these include Y as a regressor. The sample of studies that he reviews offers a wide range of possible explanations of the growth of G , ranging from the simple explanation of a high income elasticity to explanations which give no direct causal connection between high Y and high G , taking both to reflect other factors, such as a high level of literacy or political stability. While the studies propose a wide variety of possible reasons for the positive correlation between Y and G , none explicitly allow for G affecting Y .

In recent years there has been an explosion of top-down, cross-country studies which reverse the question, asking about the impact of government taxation and expenditure on output. Slemrod's extensive review shows a striking difference of perspective between these studies and the earlier studies of the determinants of G . First, in the recent studies G is always on the right-hand side of the equation, with little or no attention given to how it is determined. Second, G is related not to the level but to the rate of growth of output. Slemrod attributes the burst of research activity and its emphasis on growth to two events. First, the publication by Robert Summers and Alan Heston of comparable data for a large number of countries provides a convenient and rich source of information for cross-country and panel studies. Second, the emergence of a new theory in which the rate of output growth can be permanently affected by the rate of saving or the level of taxes or government expenditures. In the standard neoclassical model, except

for a period of transition, these variables affect only the level of output, not its long-run growth rate. The need to test the new theory and compare its empirical performance with that of the neoclassical model has provided a strong stimulus to empirical work. Not only do the new theories provide a rationalization for estimating growth equations, they also typically suggest that effects of taxes and expenditures can be quite large.

Slemrod begins his review of this new empirical literature by plotting the relationship between growth rates and both tax and expenditure ratios, for all countries, and for the OECD countries only. These plots reveal no striking relationships, suggesting to Slemrod that it will be difficult for empirical work to provide any clear verdict. Although a few authors have found that taxes depress growth, a large number of other studies show that such associations are by no means robust. Slemrod also finds that, with the exception of work by Robert Barro, these studies, too, are flawed by lack of attention to simultaneity. He concludes that the existing cross-country literature provides no persuasive evidence for either a positive or a negative impact of government on the level or growth rate of income.

Although Slemrod suggests that much of the ambiguity in empirical studies arises because of the fundamental problems of identification, he also discusses other conceptual and practical problems confronting research in this area. Little can be done about some of these problems, such as the limited and poor quality of data, or the fact that some purported mechanisms for an effect from government act by changing social norms, attitudes, ethics, and habits in the very long run and are not susceptible to measurement. However, Slemrod identifies some empirical and conceptual issues that could benefit from greater attention. For example, arbitrary conventions of government budgeting can make economically equivalent programs appear to represent different levels of government involvement in different countries. Both France and the United States have policies that provide net fiscal benefits to families with more children. In France, these appear as direct payments to families, and are recorded as an expenditure; in the United States, they primarily take the form of tax exemptions for each dependent. Even if the policies provided equivalent support, the budgeting rules would portray France as having higher taxes and expenditures.

Slemrod proceeds to catalogue other measurement problems that are

more difficult to correct. Some government activities have substantial measured costs, but also have benefits that do not appear in conventionally measured output. Expenditures on the environment and social insurance programs are examples where the unmeasured benefits reflect an explicit policy goal. The increase in leisure resulting from the disincentive effects of taxes on labor is an example where the unmeasured benefits (or costs) are a byproduct of a government activity undertaken for another reason. Another category of measurement problems results when the government intervention is measured badly, if at all, but the effects of the intervention are likely to show up in measured output. Slemrod lists government regulatory and antitrust policies, trade restrictions, and the enforcement of property rights as examples. If errors in measuring output show up as random noise, errors in measuring G would bias the coefficient of G toward zero. But Slemrod notes that a positive correlation between measured and unmeasured G could actually bias the estimates of the cost of government upward. He makes special note of the research by Jeffrey Sachs and Andrew Warner (*BPEA*, 1:1995) and by David Cameron suggesting that a government's policies toward openness to the world economy are potentially important. Openness can have consequences far beyond its direct effect on the volume of measured trade by affecting the competitiveness of markets, the ability to raise taxes, and the need for government stabilization. To the extent that openness affects the costs and benefits of government policy, it will be a determinant of G , and another example of why it is critical to worry about identification.

All the studies discussed thus far measure the impact of government taxes by the share or per capita level of revenue or expenditures. However, the link between revenue collected and aggregate disincentives is far from direct. Slemrod therefore turns to the issues involved in obtaining a better estimate of disincentives. One issue is the difference between the average and marginal tax rates. In idealized linear income tax systems, this difference could be captured by supplementing the average tax ratio with a measure of progressivity. The task becomes more complicated when combining taxes and transfers, since transfers are typically means-tested and often have a nonlinear structure. Another complication arises when, as in some countries, the benefits of transfers and other government programs are contingent on some level of labor

force participation. This is especially true in countries like Sweden where not only does eligibility for many programs require labor force participation but, in some cases, the benefits are an intrinsic part of the job, tying them to taxes at the individual decisionmaking level. In such systems, the disincentive effects of taxes on participation are substantially muted, although the system also generates a strong incentive to participate at the minimal level of hours needed for eligibility. Even in the United States, these effects may be important. Social security benefits are tied to designated payroll taxes, albeit in a complicated way. For example, Martin Feldstein and Andrew Samwick have calculated that although the statutory marginal payroll tax on employees was 11.2 percent in 1990, the actual effective marginal tax rate, net of benefits, varies from that level to as low as - 6 percent.

The preceding complications suggest the need to take into account the fine structure of tax and expenditure programs. A different kind of problem comes from the fact that marginal statutory rates, even if precisely calculated, do not appropriately capture incentive effects. As is obvious even to laymen, effective tax rates may be quite different than statutory rates. Slemrod gives several examples of "avoidance technology." For long periods, incorporating a business firm and retaining earnings was an effective way to cap the effective tax rate. Because of the deferral of realizations and the revision of basis at death, the effective rate on capital gains in the United States is only a fraction of the statutory rate. Less apparent to the layman is the lesson of tax theory that the magnitude of disincentive effects depends not only on the tax rate, but also on the degree of substitutability or complementarity between the taxed items and untaxed leisure.

An even more subtle point is that almost all economic models of the response to taxation focus on the behavior of atomistic agents when, in fact, nonatomistic institutions exist and may adapt to government activity. Slemrod notes that some authors have suggested that differences in tax levels and tax structures among OECD countries reflect differences in the extent to which labor market institutions are corporatist or centralized. In these circumstances, taxes on labor may be less distortionary than when labor supply is determined individually because the central decisionmakers recognize the linkage between taxes paid and benefits received. This is analogous to tying benefits to costs at the

individual level. But Slemrod notes that although the distortive effect of taxes may be less in countries with corporatist labor market institutions, these same systems may have offsetting efficiency costs.

The existence of nongovernmental institutions and associations creates another complication, both for assessing the incentive effects of the tax and expenditure system within a country and for making cross-country comparisons. Many nongovernmental organizations that provide goods and services charge income-tested or wealth-tested prices. According to Feldstein, for example, need-based college scholarship rules can impose an additional marginal tax rate of between 22 and 47 percent on the incremental labor earnings of students' parents during the years of attendance. A comparison of the U.S. tax system with that of a country where higher education receives greater public support by means of a graduated income tax would overstate the relative disincentive effects of the latter system. Slemrod cites other differences in the division of responsibilities between the private and public sectors and suggests that the explicit tax rates of the welfare state have, to some degree, replaced the implicit tax rates and disincentives of the extended family and private institutions in countries without social welfare.

Slemrod concludes that our current understanding of the link between real government activity and private behavior is far from satisfactory. But he is optimistic about improving this situation, seeing a large and "intellectually challenging research agenda that would refine our understanding of the link between government involvement, prosperity, and growth."