Editors' Summary

THE BROOKINGS PANEL on Economic Activity held its seventieth conference in Washington, D.C., on September 7 and 8, 2000. This issue of *Brookings Papers on Economic Activity* includes the papers and discussions presented at the conference. The first paper analyzes the effects on national saving of alternative proposals for Social Security reform and how these effects depend on what policy rule governs the overall budget. The second paper proposes that the sharp rise in the stock market in recent years reflects increases in intangible and unobserved "e-capital." The third paper reviews recent developments in real estate markets and assesses the risk of a crisis in the sector. The next three papers address various implications of the unexpected emergence of federal budget surpluses and the consequences of sharply declining government debt. The first of these examines the effect of improving fiscal balances in most major industrial countries on the level and term structure of interest rates. The next considers the effects of declining government debt on the efficiency and liquidity of financial markets and the future conduct of monetary policy. The third asks whether a reduced stock of debt will impair the government's ability to implement optimal fiscal policy. Finally, the seventh paper in this issue examines to what extent official corruption inhibits foreign direct investment in developing countries.

THE BRIGHT FISCAL PICTURE of the federal government today stands in sharp contrast to that drawn by economists and policymakers a decade ago. Between 1992 and 2000, the federal budget balance improved from a deficit of nearly 5 percent of GDP to a surplus of nearly 2½ percent. By raising the base for economic growth, this performance has also greatly

improved the outlook for the future. Official projections in 1993 foresaw deficits of the unified budget (the broad budget concept that includes Social Security) in excess of 10 percent of GDP by 2010, and worsening rapidly thereafter. Today official projections show surpluses for the next half century. In contrast to this rosy prospect for the unified budget, there has been little change in the funding difficulties projected for Social Security and Medicare. However, the changed fiscal outlook has shifted the debate about reforming those programs away from benefit cuts and tax increases to using the emerging budget surpluses to prefund future obligations. In the first paper of this issue, Douglas Elmendorf and Jeffrey Liebman integrate budget politics with the economics of the Social Security problem. After reviewing the dramatic improvement in the federal budget and discussing the importance of prefunding Social Security, they present an analysis of reform proposals that explicitly incorporates the political process. They then use this framework to show how various reform proposals are likely to differ in their effects on national saving and capital accumulation.

Elmendorf and Liebman begin by describing the emergence of federal budget surpluses in the 1990s, which reflect faster productivity growth, more moderate increases in spending, and higher revenue relative to GDP than had been expected early in the decade. By mid-2000 the Office of Management and Budget (OMB) was projecting that surpluses in the unified budget between 2001 and 2010 would total $4.2 trillion absent any changes in policy. The authors note that this current projection, which represents a dramatic change from projections made only a few years ago, although undoubtedly good news, is itself evidence of the unreliability of projections far into the future. Rates of long-term productivity growth and the projected cost of health care are both highly uncertain. If tax receipts return to their average share of GDP before the late-1990s surge, the surplus would be smaller by 1 percent of GDP. And if real discretionary spending remains a constant share of GDP, rather than a constant real dollar amount as in current projections, the surplus would fall by a like amount. But although the authors perceive substantial uncertainty about future budget outcomes, they have little doubt that the outlook has improved dramatically and that the improvement has changed the terms of the public debate about Social Security reform.

Population aging is the primary reason for Social Security's problems. The retirement of the baby-boom generation is projected to push Social Security expenditure from its current 10.3 percent of payroll to 17.4 per-

cent by 2030 and to 19.5 percent in 2075 as the population ages further. Absent policy changes, the payroll tax and taxes on future benefits together are projected to remain at roughly their current rate of 13 percent of payroll. This emerging problem could be addressed in any of a variety of ways, including cuts in benefits or increases in taxes, transfers from general revenue, and changes in the investment policies of the trust fund. The timing of these changes has major distributional consequences. Tax increases or benefit cuts could be imposed today, placing the burden on either current workers or retirees, or postponed until the distant future, placing the burden on future generations of workers or retirees. Projections that assume perpetuation of the largely pay-as-you-go approach in place today imply that a one-third cut in benefits or a one-half increase in payroll taxes would eventually be required to achieve balance in the Social Security system in 2075. These changes would place most of the burden on future generations. Alternatively, solvency could be maintained for the next seventy-five years by an immediate increase in payroll taxes of roughly 2 percentage points or an equivalent immediate cut in benefits.

The authors see the intergenerational distribution of benefits and burdens as the central issue in Social Security reform. This leads them to evaluate reform proposals by their effect on national saving, which, by changing the future capital stock, is the primary way current generations can change the resources available to future generations. The authors recognize that the optimal level of national saving is not self-evident. Many economists have observed that the United States saves less than most other developed countries. A variety of reasons for this undersaving have been cited: myopic or time-inconsistent preferences of households, capital taxes that discourage saving, the pay-as-you-go nature of the current Social Security system, and the possibility that the social return to saving exceeds the private return. On the other hand, the life-cycle model of household behavior, a common framework for analysis, does not lead to definite conclusions about how the aging of the population will or should affect national saving. Nonetheless it is widely concluded that U.S. saving is too low. Elmendorf and Liebman proceed on the assumption that building a larger capital stock in the immediate future will help maintain consumption as the population ages, and therefore they take the effect on saving as the primary measure by which to compare reform proposals.

Prefunding of benefits, achieved by some combination of payroll tax increases, transfers from general revenue into Social Security, and benefit cuts, is an important feature of most reform proposals. The 1994–96 Advi-

sory Council on Social Security emphasized prefunding and moved individual retirement saving accounts and investment in equities to the center of the debate. The authors note that prefunding is a natural feature of proposals that seek to give individuals control over their own retirement saving through individual accounts. More generally, they see the current emphasis on prefunding as reflecting the emergence of large projected budget surpluses that offer a new funding source at relatively low political cost. The authors briefly discuss the main political and economic arguments for and against prefunding. Proponents have argued that prefunding would increase national saving, minimize tax distortions by smoothing tax rates in the face of anticipated increases in government outlays, and reduce the political likelihood that benefits will be cut in the future, which some feel would place an inordinate burden on the elderly. On the other side, if one regards the current level of benefits as too high, or thinks that maintaining flexibility in future benefit levels is important in light of the uncertainty about the future, protecting benefits through prefunding may be undesirable. The authors also point out that prefunding might not actually increase national saving once the effects on the rest of the government budget and on the behavior of private agents are taken into account.

The effect of Social Security reform on national saving is the sum of its effects on government saving and private saving. The authors first discuss how reforms are likely to affect government saving. From 1967 until very recently, public and official discussion focused on the unified budget, which includes all revenue and expenditure of the federal government. Surpluses or deficits of the unified budget equal government saving or dissaving and are reflected in a decrease or increase in the stock of government debt held by the public. As a result of the 1983 Social Security reform, the Social Security trust funds were officially taken off budget, but the budgetary discussion continued to focus on the unified budget. A dramatic change in policy discussions took place in the spring and summer of 1999, when policymakers suddenly shifted to the objective of balancing the "on-budget" account—the unified budget excluding Social Security. This year the Clinton administration proposed taking Medicare Part A off budget as well.

The political dynamics that led to this shift are undoubtedly complex, reflecting among other things the desire of each party not to let the other use up the emerging unified surplus in its own preferred way. The shift was also motivated by the political argument that an on-budget deficit consti-

tutes a "raid" on Social Security, even though the trust fund receives Treasury securities equal to the Social Security surplus regardless of the condition of the unified budget. In this regard the authors suspect that, historically, the buildup of the trust fund surpluses led to incremental government saving, although less than dollar for dollar because of induced changes in the rest of the budget. Hence the authors believe that the likely interactions between on-budget decisions and Social Security reforms need to be a central feature of the analysis of those reforms.

Forecasting the effect of various possible Social Security reforms involves taking into account the potential responses of other government taxes and expenditures and the responses of the private sector—a complicated matter. It requires numerous assumptions about the behavior of policymakers and private agents, as well as judgments about the allocation of additional saving to different kinds of investment and about the equilibrium response of rates of return to changes in real capital supply. The magnitude and complexity of the enterprise are reflected in the number of combinations of elements the authors examine. They consider five canonical characteristics of current reform proposals: cuts in benefits or increases in taxes, transfers from on-budget revenue to Social Security, trust fund purchases of equities, on-budget contributions to individual accounts, and contributions from Social Security to individual accounts ("carve-out" accounts). They also consider three main political economy "rules" describing alternative ways that budgetary policy might respond to Social Security reform: the first rule assumes no response in the on-budget account, the second assumes that policy adjusts to achieve unified budget balance, and the third assumes that policy adjusts to achieve on-budget balance. The authors also examine the effect of a fourth rule, which assumes asymmetric responses to deficits and surpluses in the unified budget. And they consider alternative assumptions about the way households respond to different aspects of Social Security reform.

The authors begin by illustrating the importance of political economy rules in determining the response of national saving to various reforms. They consider the special case where there are no induced changes in household saving relative to income (other than from individual accounts), in capital tax revenue, and in the return on incremental saving in previous years. Under these assumptions the effect of reform on national saving is simply its effect on government saving plus any effect on withdrawals from individual accounts. In this special case, it is straightforward to deter-

mine how each of the budget rules affects the impact of different reforms. For example, under the rule that balances the unified budget, moving resources from the on-budget account to Social Security has no effect on national saving, nor does investing part of the trust fund in equities. Making contributions from either the on-budget account or the trust fund to individual accounts, in contrast, does raise saving. On the other hand, under the rule that balances the on-budget account, transfers from that account to Social Security raise saving, as does cutting Social Security benefits or increasing payroll taxes. Similarly, if the political process balances the on-budget account, the effect of moving Social Security contributions to individual accounts is opposite from the effect if politics leads to balancing the unified budget.

The authors proceed to the much more ambitious task of making quantitative estimates of the effects of reform, taking into account the main endogenous responses that might be expected. These include the response of household saving and capital tax revenue as well as the general-equilibrium effects on rates of return. They consider seven reforms that represent the range of proposals that have been introduced in Congress or have been the object of serious discussion in the past several years. To make the proposals comparable, they adjust each proposal so that it achieves trust fund solvency through 2075. The authors use prereform projections of trust fund finances and other parts of the budget, and they adopt a demographic model for tracking individual account balances and withdrawals for each cohort of participants. They choose values for various parameters—household behavior, the allocation of capital, the financing of corporate investment, and rates of return on different assets—that correspond to the results of previous research or historical experience. For example, they assume James Poterba's estimate of a 7.6 percent real annual return after taxes on corporate capital, the Social Security actuaries' projection of a of 3.1 percent real interest rate on government bonds, and premiums for corporate debt, equities, and overseas investment based on historical averages. Finally, they adopt separate rules of thumb for the response of household saving to incremental capital income, changes in government saving, cuts in future Social Security benefits, and increases in individual account balances.

To capture the general-equilibrium effects, whereby increases in the domestic capital stock reduce the returns to capital, the authors assume a Cobb-Douglas production function, with capital's share and the initial

capital-output ratio taken at their average levels for the period from 1960 to 1997. The authors assume that all asset returns decline by the same proportion, they ignore any induced change in labor supply, and they assume that changes in the return to inframarginal capital and labor are offsetting for on-budget and household saving. However, they keep track of these variables for the Social Security system, since they affect payroll receipts and benefit levels. They note that the effect of capital deepening on rates of return can be substantial: an increase in the capital-output ratio from 3 to 4 reduces the marginal product of capital by roughly 1¾ percent.

The authors report the quantitative effect on capital accumulation in 2070 of each of the seven Social Security reform proposals under each of the three main political economy rules described above. In each case they separately calculate the effects arising from benefit cuts or tax increases, from consumption out of individual accounts, and from changes in on-budget spending. And they report results both with and without the effects that come through capital taxation, household saving, and general-equilibrium effects on rates of return, providing a rich menu of results for the serious student of Social Security reform. The most salient, but not unexpected, feature of the results is the enormous difference the choice of political economy rule makes. Only three of the seven proposals increase national saving if there is no change in other taxes and spending in response to Social Security reform, but all seven raise saving under either of the two other political economy rules. For example, transferring surplus funds from the on-budget account to Social Security, as advocated by President Clinton in his 1999 State of the Union address (but modified by the authors to make sufficient transfers to achieve seventy-five-year solvency) has different effects on capital accumulation under different rules. It has no effect if there is no attempt to restore the on-budget balance, and only a small effect if the political process balances the unified budget. But under the rule that balances the on-budget account, capital deepening under this proposal increases the capital-output ratio in 2070 from 3 to 4½, even after taking into account the dampening effect of the decline in the rate of return on assets.

Under all the proposals considered, reform significantly increases capital accumulation under this rule of maintaining on-budget balance, although by substantially different amounts under different proposals. The smallest effect comes from the proposal that the authors refer to as "carve-out with transfers." This proposal would divert 2 percent of payroll taxes

from the trust fund to individual accounts, together with a cut in traditional benefits phased in to roughly offset the growing withdrawals from individual accounts, and coupled with a transfer from general revenue to the trust fund sufficient to maintain solvency. In the absence of the endogenous responses to saving, taxes, and rates of return that the authors allow for, this proposal has only half the effect of simply transferring surplus funds to the trust fund. But this figure rises to 70 percent once these endogenous responses are taken into account. Many results using the other political economy rules are less obvious. Investing part of the trust fund in equities while balancing the unified budget decreases the capital-output ratio in 2070 by roughly as much as transferring half of the surplus to the fund increases it while balancing the on-budget account. In every case, taking account of endogenous rate changes lowers the terminal capital-output ratio, thus dampening increases and magnifying decreases in the ratio.

Most of the authors' analysis focuses on the effect of reforms in 2070, when the reforms are fully phased in and almost all retirees have made full contributions to any individual accounts. But the authors caution that there can be great political and economic change over seventy years. Thus the short-run impact of reform on capital accumulation may be as important a criterion for evaluating reform proposals as the long-run impact. The authors calculate the evolution of the capital stock for each of the proposals under each of the political economy rules. Although most of the capital accumulation occurs only after many years, even after two decades many of the proposals increase the capital stock by 20 percent of GDP.

The authors extend their analysis in several ways, showing how the proposals differ in the level and composition of retirement benefits and in the level of trust fund and individual account balances in 2070, and they examine in detail the role of equity investments in the reform plans. Most striking is the similarity of the combined retirement benefits from Social Security and individual accounts for five of seven of the proposals. However, the authors observe that the division of benefits between these two sources is quite different across proposals, and they suggest that this division makes for important differences in both the economic and political risk confronting beneficiaries and in the level of administrative costs.

The authors believe that budget accounting conventions help frame decisions about spending and taxes, and they take the central lesson of their paper to be that the effect of Social Security reform on national saving depends critically on the political economy of the budget process. They

also identify several other general lessons from their analysis. Reform proposals that seem very different can have nearly identical effects on saving, because many proposals have similar total retirement benefits, and because budget responses offset some differences in proposals. Programmatic prefunding ensures economic prefunding only if the political process balances the budget excluding Social Security. And reform plans that move funds out of the government into individual accounts boost saving only if policymakers balance the unified budget. Finally, the authors believe that a fundamental question for the future is whether the present, new consensus to balance the on-budget account will be maintained.

THE HISTORIC RISE OF the U.S. stock market in the second half of the 1990s increased the market value of firms traded there by nearly $10 trillion. Although investment in plant and equipment was high during the decade, increases in the physical capital stock explain only a small fraction of this rise. As a consequence, conventional measures of Tobin's *q,* the ratio of market value to the replacement cost of physical capital, have reached historic heights, more than doubling since 1990. Price-earnings ratios have shown similar dramatic increases. Many explanations have been offered for the rise in stock prices. Some ascribe it to a substantial reduction in the risk premium required by investors, others to a technological revolution that will one day generate extraordinary growth in profits, and still others to irrational exuberance. In the second paper of this issue, Robert Hall provides another, innovative explanation. He argues that the rise in stock market values reflects not an increase in the valuation of measured physical capital, but rather the accumulation over the last decade of vast amounts of what he calls e-capital. E-capital consists of intangible capital, mostly in the form of more efficient business methods that make intensive use of computers, that was not recorded as output in the national accounts and does not appear as an asset on firms' balance sheets under conventional accounting standards.

Hall's view has major implications for how we think of the economy of the 1990s. It implies that measured output and profits understated economic performance during the decade, and that conventional measures of capital inputs, which ignore the accumulation of this intangible capital, increasingly understated total capital as the decade progressed. At the same time, labor inputs used in the production of measured output were significantly overstated, because a substantial fraction of educated labor,

according to Hall, has been devoted not to measured output but to the production of e-capital. This view calls for substantial revisions in our accounting for measured output, altering our estimates of both the timing and the magnitude of total factor productivity growth. Moreover, because technological progress in forming e-capital itself increased the demand for educated workers, his model would help explain the growing wage premium for these workers during the period, despite their relative increase in supply.

A central premise of Hall's analysis is that the stock market is rational: the market valuation of a firm faithfully reflects the sum of the values of physical capital and intangible e-capital. This assumption provides a platform from which to address several key questions. How much e-capital is there, and does its accumulation help explain productivity trends over the 1990s? Does investment in e-capital bear a sensible relationship to the input of educated labor? Even with the assumption of market rationality it might seem difficult to get at these questions, because the intangible assets are unobserved and unreported. One could simply assume that e-capital is the difference between the replacement cost of a firm's physical capital—plant and equipment—and its market value. But that seems a poor approximation given the substantial evidence that the market value of assets can exceed their replacement cost for extended periods because of the cost of rapid adjustment of the capital stock.

The conventional investment model based on Tobin's q relates the difference between the market value of firms and the replacement value of capital to the rate of investment in the macroeconomy. Since neither investment in e-capital nor its valuation is observed, Hall devises an ingenious method of inferring their values by assuming that investment in physical capital and investment in e-capital are determined by separate q investment equations. The first step is to find the market value of physical capital. Once he knows how much of the observed market valuation of firms can be attributed to physical capital, he can calculate how much remains to be attributed to e-capital.

Data limitations confine this calculation to the nonfarm, nonfinancial corporate sector. Because direct information is available on the stock of capital at replacement cost for this sector, all that is needed for this first step is a time series for the q of physical capital (q^k). This is inferred by plugging observed rates of investment in plant and equipment into a q investment equation for physical capital. The equation Hall uses draws on

the existing empirical literature only in the most general way. The crucial parameter is the cost of adjustment. The values Hall picks are below most empirical estimates, which he believes are biased upward as a result of severe specification errors, including attributing to physical capital the market's valuation of intangibles. Hall candidly acknowledges that he has chosen his parameter values so as to achieve a reasonable version of the e-capital story, and that errors in his assumed equation will be reflected in his subsequent calculations of the quantity and value of e-capital.

Hall allocates the portion of market value that he attributes to e-capital into the quantity of e-capital and its q value, q^e. For the latter he uses a method parallel to that used to find q^k for physical capital. He assumes that the stock of e-capital is zero before 1990, thus attributing the entire market value of firms before that date to tangible assets. Since all of the value of e-capital at the end of 1990 then reflects new investment, he uses the e-capital investment equation to calculate q^e, and so to divide this value into its price and its quantity. In turn, knowing the new stock enables him to estimate price and quantity in the following year, and in the same way to recursively construct the entire time series for both.

The results of these calculations for the nonfarm, nonfinancial sector are dramatic. Growth in the stock of e-capital at replacement cost is calculated to be the single largest factor in the rise of the stock market in the 1990s. Although investment in physical capital was high by historical standards, its growth was dwarfed by the growth in e-capital. The high rates of investment in both types of capital are associated with high qs. In the case of e-capital, q^e is approximately 1.5 in the latter part of the decade, meaning that almost a third of e-capital's contribution to the market's value reflects its scarcity value. Large as they are, Hall's estimates of the stock of e-capital during recent years are only two-thirds of what they would be if it were assumed that q^e were at its equilibrium value of one. Hall stresses that these results are not a causal explanation of the rise of the stock market but indeed are consistent with a wide variety of possible explanations, including changes in the risk premium or an increased willingness of U.S. residents and foreigners to invest in huge quantities of capital. They do depend, however, on the assumption that the market's rise is not the result of irrational exuberance.

If Hall's estimates of the growth in intangible capital are correct, conventional measures of saving and of productivity growth are quite misleading. According to his model, a large portion of capital gains in the

stock market reflect previously unmeasured capital formation, and a substantial fraction of labor input has been devoted, not to the production of measured output, but to investment in intangible capital. Hall quantifies the implications of his view by making explicit assumptions about the technology for the production of e-capital and of measured output. He assumes that the only factor used in the production of e-capital is college-educated labor, or what he calls c-workers. Assuming that the productivity of c-workers in producing e-capital grows at 3 percent a year, he calculates the implied number of c-workers employed in producing e-capital. Measured output is then assumed to be produced by the measured and estimated inputs of physical capital, e-capital, unskilled labor, and those c-workers not engaged in the production of e-capital.

Using his own calculated rental prices for physical and e-capital, Hall compares total factor productivity growth in the production of measured output as usually calculated (that is, ignoring e-capital and inputs used in its production) with that implied by his analysis. Over the period 1990 to 1998 the contribution of c-workers to growth in measured output almost disappears once the requisite number are reallocated to the production of e-capital. This removes a contribution to measured output of approximately 9 percent. By itself this would imply an increase in total factor productivity growth in producing measured output. But this effect is more than offset by the accumulation of e-capital during the period. According to Hall's estimates, e-capital has a cost share of only 9 percent, but its rapid growth results in a 15 percent increase in output over the period. The net result is that total productivity growth, when measured including e-capital as an input, is near zero over this period.

Hall checks to see whether the e-capital story as he has told it is consistent with standard production theory. Using his calculated rental prices for capital and observed wage rates for unskilled and skilled labor at the beginning and the end of the period, Hall finds no inconsistency. The input bundle actually used in 1990 is cheaper, at 1990 factor prices, than the input bundle used in 1998, but more expensive, at 1998 prices, than the bundle used in 1998. He also conducts a sensitivity analysis showing that doubling or halving any one of the crucial parameters almost always violates the assumption of cost minimization or implies a negative rate of productivity growth. By this test, the parameter values he uses appear to be in the right range.

The e-capital model can be thought of as an elaboration of the argument based on skill-biased technological change that is commonly used to

explain the simultaneous increase in the relative wage and in employment of more skilled and educated workers. In Hall's model the skill bias arises from the fact that e-capital is produced only with educated labor, whose productivity in that sector is growing at 3 percent a year. Hall shows that it is difficult to find a satisfactory skill bias–based explanation of wages and employment of c-workers in a model with only two factors, but he recognizes that a model with more factors could do so. However, he suggests that his model improves on such an explanation by helping to reconcile the observed rapid rate of wage growth for college graduates with the observed low rate of conventionally measured total factor productivity growth early in this period.

The most dramatic examples of companies with enormous market values relative to their book values come from among the dot-coms and other firms in the information and other high-technology industries. Hall reports, for example, that Yahoo! recently had a market value of $37 billion despite only $158 million of physical capital. But e-capital is transforming low-technology sectors, too. Hall cites Wal-Mart, whose recent market value was more than seven times its book value, as a conventional firm that has harnessed modern technology to bring about great improvements in productivity. Firm-level data would provide a more powerful test than aggregate data of the validity of Hall's e-capital model. For example, it would be strong evidence for his model if a substantial fraction of the value of dot-coms could be explained by their employment of c-workers. Unfortunately, data on the educational attainment of employees are not available at the firm level, and this leads Hall to attempt to assess his theory using industry data. From the public-use sample of the 1990 census, he estimates the fraction of workers in each industry who have completed college. He excludes industries where there are large discrepancies between employment reported in Compustat and that reported by the census, which reflect the difference between classifying by industry of establishment and classifying by industry of the owning company. For the remaining twenty-two industries he calculates the number of workers who are college graduates and the value of e-capital. A cross-sectional regression on these data indicates that an extra c-worker is associated with $986,000 in extra e-capital at market value. This is more than double the average compensation for college-graduate workers for the eight-year period. Taking into account that qe is 1.5 at the end of the period, it appears that the quantity of e-capital that has been formed is only modestly more than the quantity predicted by his production function using c-workers. E-capital appears

to have been built in sectors where c-workers are employed and where the value of e-capital is highest.

As he emphasizes, Hall's model of e-capital is consistent with two important developments of the 1990s: it provides a unified explanation for the rise in share prices and the rise in the relative wages of college-educated workers at a time when their relative supply was rising. Yet the crash during 2000 of many stocks that had boomed the year before reminds us that market valuations are volatile and not based solely on fundamentals. Thus values of unobservables such as e-capital that are inferred from them are subject to large error. Hall's view that intangible assets are an important source of value in many firms is surely correct. But its quantitative importance in explaining stock prices, productivity, and relative wages is uncertain. As Hall himself concludes, the argument of the paper, although consistent with the broad facts of the 1990s, is not yet compelled by them.

OWNER-OCCUPIED HOMES ARE the main form of wealth for most U.S. households, and residential and commercial real estate together make up a major fraction of the nation's capital stock. With its sensitivity to monetary conditions, real estate prices, and tax laws, construction is the most volatile sector of the economy. And because much of real estate is highly leveraged, problems in the real estate sector often become problems for banks and other financial institutions invested in mortgages. This combination of size, volatility, and leverage makes real estate important both in initiating and in propagating shocks to the macroeconomy, and in particular to the financial sector. The past two decades have seen real estate collapses lead to crises in several regions, and with real estate prices rising faster than the overall price level in recent years, some observers have begun to fear a new crisis could be in the making. In the third paper in this issue, Karl Case examines the relation of real estate to the larger economy and evaluates the present risks from real estate markets.

One possible connection between real estate prices and the larger economy comes from wealth effects. Rising wealth from stock market gains is often cited as a factor driving the present consumption boom, but gains from real estate have gotten much less attention. Case calculates that rising home prices, after changes in value from renovations are deducted, have added $1.9 trillion to aggregate housing wealth over the past five years, or about one-quarter of the rise in aggregate stock market wealth over the

same period. Federal Reserve Board chairman Alan Greenspan recently suggested that the permanent increase in spending out of gains in housing wealth is typically about 5 percent, somewhat above his estimate for increased spending out of equity gains. Using this figure, Case calculates that the rise in home real estate values explains over 4 percent of GDP growth since 1995. But he notes that there is broad uncertainty about the size of this wealth effect, and that he and Robert Shiller have estimated a smaller, rather than larger, propensity to spend from real estate gains than from equity gains.

The discussion at the Brookings Panel meeting reflected this uncertainty. Some participants reasoned there would be no wealth effect from housing, because the discounted cost of future housing services just offsets the rise in home values, or, equivalently, if a representative homeowner realized the gain, he would have to spend it to acquire an appreciated replacement. In his comment on Case's paper, Jonathan Parker offers a more nuanced argument, suggesting three reasons why the effects of housing wealth on consumption might be large relative to the effects of equity wealth. First, the increase in housing prices will lead consumers to reduce consumption of housing and increase other forms of consumption. Second, homeowners are not all the same. For older homeowners the increase in housing wealth exceeds the cost of future housing services over their remaining lives. And for liquidity-constrained homeowners the increase in housing wealth increases the capacity to consume by increasing the capacity to borrow. Third, the fact that wealth gains from real estate are more uniformly distributed than wealth gains from equities, given a presumed declining marginal propensity to consume out of wealth, makes aggregate spending more responsive to real estate gains than to equity gains.

The distribution of changes in real estate values has been far from even across regions. Case calculates that between 1980 and 2000, a period when the Consumer Price Index rose 106 percent, housing prices rose by 243 percent in New England but by only 60 percent in the West South Central region. Gains in the other eight regions of the country fell between these extremes. He also notes that the several real estate crises during this period were highly concentrated geographically. Relying mainly on transaction-based indexes of real estate prices, he calculates the capital gains from real estate as a ratio to personal income in each of nine states for four periods since the early 1980s. In the first period, 1983–86, these ratios ranged from 26 percent in Massachusetts, which

was then experiencing its high-technology boom, to only 1.3 percent in Texas, which was hit by falling oil prices. In the second period, 1986–90, the boom in defense spending contributed to a gain in California of 21 percent of personal income, with Washington state not far behind. Case shows that the collapse of demand following each of these booms brought about a long and gradual price adjustment rather than an abrupt fall in prices. Even so, these adjustments were enough to jeopardize the solvency of lending institutions. In each case the capital gains and losses for the affected region were large enough to have an important effect on other spending even if the spending propensity out of such wealth is low. Thus the impacts of real estate booms and busts on a region's economy are amplified both through their effects on lending institutions and through these induced effects on consumer spending.

Case assesses the current situation against this historical experience and the institutional changes that have occurred in mortgage lending in the meantime. Between 1989 and 1999, the share of mortgages held by commercial banks stayed around 22 percent, while the share held by thrift institutions declined from 25 percent to 11 percent, and the share held by life insurance companies declined from 7 percent to 4 percent. In contrast, the secondary market has grown in importance, with the combined share held by Fannie Mae and Freddie Mac rising from 17 percent to 30 percent, and with other federal agencies holding a little over 10 percent in both years. Nearly one-quarter of mortgages are now securitized, and that proportion is rising. These changes have been accompanied by increasingly sophisticated risk pricing of mortgages and mortgage insurance. Risks are now widely distributed across well-capitalized mortgage insurers, holders of mortgage-backed securities, and portfolio lenders.

Defaults in the past have been associated with declines in home prices during severe regional recessions, and Case does not believe that the greater sophistication of today's mortgage market precludes losses to mortgage holders in the event of a significant decline in real estate prices. Although housing prices have been rising faster than the consumer price index in every major metropolitan area, with prices in some submarkets rising dramatically, Case does not see this as a bubble, because these rising prices appear consistent with rising employment and income and with rising equity prices. Thus he does not see a problem originating in real estate, but he acknowledges that the real estate and mortgage markets would be vulnerable to a sharp decline in the stock market and a severe recession.

Turning to commercial real estate, Case recalls that a worldwide boom-and-bust cycle hit that sector in the late 1980s and early 1990s. Several developments interacted to produce and intensify that crisis. In the first part of the 1980s, a building boom got under way as tax changes favored real estate investing, bank deregulation encouraged asset-backed lending, and the teachings of modern portfolio theory led institutional investors to diversify their portfolios by adding real estate holdings. The crises in the United States that eventually followed were mirrored in parts of Europe and Asia, suggesting that overbuilding spurred by optimistic investors had spread across national borders. In this country the 1986 tax reform reduced the profitability of real estate, but projects have a long lead time, and capacity kept coming on stream. When sharp recessions hit some regions, vacancy rates there soared and commercial property values collapsed. Case provides a numerical example of how such economic shocks are intensified by the interaction of the four factors affecting commercial real estate values: expected gross rents, vacancies, operating costs, and capitalization rates. (The last of these represents the return a buyer would require to justify purchasing a property; this rate varies with the prevailing interest rate and perceived risk.) In his example, an economic downturn similar to that in Boston in the late 1980s results in a startling 75 percent decline in commercial real estate value.

Case notes that today's commercial real estate markets are strong, with vacancy rates relatively low nationally and very low in some major markets such as San Francisco, Manhattan, and Boston. Construction remains at modest levels compared with boom periods in the past, financial institutions have been noticeably cautious in their lending, and the tax treatment of real estate has remained stable. But although Case sees no excesses in today's commercial real estate markets, he warns that a recession would significantly lower the value of commercial real estate portfolios. He estimates that a major downturn that cut office employment by 1.8 million workers would set in motion interactions among gross rents, vacancies, and capitalization rates that would produce an estimated 37 percent decline in the value of the nation's office stock. Extrapolating this figure to other types of commercial real estate, he estimates a potential total loss of $1.3 trillion. Yet although it would be a significant blow to the industry, even a decline of this magnitude would not be large relative to total wealth in the economy. And given the diversification of ownership and the stable capital base of most market participants, Case sees no economy-wide crisis emerging from such a decline.

THREE PAPERS IN THIS ISSUE look at the implications of the large surpluses now projected for the federal budget. In fiscal year 2000 the unified budget surplus was $237 billion, or 2.4 percent of GDP, and it is projected to increase steadily over the current decade and beyond. Such projections are of course highly uncertain, both because the projections themselves may elicit tax cuts or new expenditures that will reduce the surpluses, and because economic developments may generate different outcomes even under unchanged policies. But if the surpluses materialize, one effect will be a substantial reduction and eventually the elimination of Treasury securities held by the public. Financial markets have already started to respond to the prospect of a declining supply of these securities.

In the fourth paper of this issue, Vincent Reinhart and Brian Sack take a global view, noting that budget deficits have been sharply reduced, if not actually turned into surpluses, in most major industrial countries over the past half decade. In their 2000 fiscal years, Canada and the United Kingdom are expected to have surplus-to-GDP ratios comparable to that in the United States, and only modest deficits remain in Germany, France, and Italy, where, since 1995, budgets have moved toward surplus by 2, 4, and 6 percent of GDP, respectively. Among the Group of Seven (G-7) countries, only Japan with its weak economy has seen a growing deficit over this period. Because of this global trend, the authors ignore the exchange rate effects that many models predict when a single country sharply changes its fiscal stance. They focus instead on the economic effects that operate through long-term interest rates and wealth, and on the financial market effects that may arise directly from the falling supply of Treasury securities.

The authors first examine the forward-looking model that Olivier Blanchard used to analyze the U.S. budget deficits of the 1980s. In that model the prospect of a shift to surpluses creates expectations of lower future short-term interest rates. These expectations, in turn, reduce current long-term interest rates, which are assumed to be a weighted average of current and future short-term rates with an allowance for risk. Because of this response of long-term rates, a shift to larger expected surpluses leads to "expectational crowding in" of private demand, mirroring the "expectational crowding out" identified by Blanchard in the 1980s. As a result, the conventionally expected impact of a change in fiscal policy can be sharply reduced or even reversed.

A main prediction of this model is that expectations of tighter fiscal policy in the future should flatten the yield curve for debt securities. The

authors show that fiscal balances and yield spreads have been correlated as the model predicts in some industrial countries, including the United States, but not in others. Many other factors, including significant cyclicality in both budget positions and interest rates, make simple correlations an unreliable test of the model. The authors therefore turn to regressions across a panel of nineteen developed economies and allow for other factors that may be correlated with budget positions and may affect future interest rates. These include year-ahead forecasts by the Organization for Economic Cooperation and Development of GDP growth, inflation rates, and unemployment rates, and trailing-year short-term interest rates as well as year-ahead surplus-to-GDP ratios.

For their entire sample of countries, in estimates using quarterly data from 1981:1 through 2000:1, all the explanatory variables have the expected signs in regressions explaining the slope of the yield curve, where this slope is defined as the difference between ten-year and three-month interest rates. A 1 percent increase in the surplus-to-GDP ratio is estimated to reduce this slope by 9 basis points. When the sample is confined to the G-7 countries, this effect of the surplus on the yield curve slope rises to 12 basis points. According to this estimate, the 5½-percent-of-GDP swing in the U.S. fiscal balance between 1994 and 2000 should have flattened the U.S. yield curve by 67 basis points, which is about one-quarter of the change that actually occurred. Other factors in the regression account for some more of the observed change. The authors also report regressions that use the same right-hand-side variables but explain changes in real short-term interest rates over the subsequent five years rather than the slope of the current yield curve. In these regressions the surplus-to-GDP ratio is not significant for the entire sample but is significant in the G-7 regressions, with each percentage-point increase in the ratio estimated to reduce future real short-term rates by 45 basis points.

Evidence that the budget balance affects current long-term rates indicates that real economic effects can occur before the projected budget surpluses materialize. In particular, the crowding-in model predicts that the lower rates will stimulate real investment and so add to economic growth in the medium run. But although the observed rise in U.S. business investment in recent years is consistent with this prediction, the authors note that the model fails to account for other major U.S. developments during this period of improving budget balances. The personal saving rate, for example, has fallen sharply, offsetting about two-thirds of

the rise in public saving, and much of the rise in investment has thus been financed by foreign capital inflows, whose counterpart is a growing current account deficit. The acceleration of productivity and the surge in the stock market, only a part of which can be credibly ascribed to budget policies and their interest rate effects, are the most conspicuous candidates for helping account for these developments.

In addition to affecting real investment, budget policy can influence financial markets by prompting changes in the prices of other assets as investors adjust their portfolios to the declining stock of Treasury debt. The portfolio balance approach to asset pricing relates the mix of assets that an investor is willing to hold to their expected returns and to the investor's perception of the risks associated with alternative asset bundles. As the available stock of Treasury debt declines, the average portfolio will have to substitute other assets for Treasuries. Demand for other assets, and hence their relative prices, will change according to how each affects the overall risk and return in the typical portfolio, and these, in turn, depend on the covariance of the returns of these other assets with returns on Treasuries.

To quantify these price effects, Reinhart and Sack rely on the standard portfolio model using historical variances and covariances of returns among broad asset classes. At the end of 1998 the market value of assets in the global financial market totaled $61 trillion, of which $5.2 trillion consisted of money market instruments, $28.0 trillion was in bonds (most of which were issued by governments), and $27.0 trillion was in equities. Of these world totals, assets from U.S. issuers accounted for $1.9 trillion, $12.6 trillion, and $15.4 trillion, respectively. Treasuries accounted for $3.1 trillion of the $12.6 trillion in bonds. These assets, with some further disaggregation by type of bond issuer and region of origin, make up the portfolio of the representative investor in the authors' analysis. From quarterly data on total returns on these assets over the period 1991:1 to 2000:2, they compute the historical mean returns on each and their covariances, treating U.S. money market instruments as the risk-free asset. They then estimate, for various alternative assumptions about the risk aversion of investors and about how the supplies of other assets change as the supply of Treasuries declines, how the relative returns on these assets would change in response to the disappearance of U.S. Treasuries from the market.

With a coefficient of risk aversion of 2, the modest value often assumed in life-cycle models to explain saving, and assuming that the supply of cor-

porate bonds expands to replace all outstanding Treasury bonds, the authors show that only trivial changes are needed in the returns expected on other assets. The return on corporate bonds would rise by only 0.6 basis point, and the return expected on emerging market equities would rise by only 2.7 basis points. Under an assumed coefficient of risk aversion of 23, which the authors calculate is the value needed to explain why investors actually hold the observed portfolio of U.S. assets (but not the observed holdings of foreign equities), the needed changes in returns are still inconsequential. Returns need increase only 7 basis points for U.S. corporate bonds, 18 basis points for U.S. equities, and 32 basis points for emerging market equities to achieve the adjustment. Returns on U.S. corporate and agency bonds are historically so closely correlated with returns on U.S. Treasuries that substituting them for Treasuries makes little difference to returns on any of the assets. If it is assumed instead that Treasuries are replaced by foreign government bonds or by a proportional increase in all other assets, the needed changes in returns, while still trivial for other U.S. bonds, are noticeably larger for some other assets. Under various of these scenarios, expected returns rise by as much as 98 basis points on emerging market bonds, 233 basis points on emerging market equities, and 95 basis points on Japanese government bonds.

The authors recognize that the small changes in required returns on other assets calculated from the portfolio balance model seem inconsistent with the concern that market participants have expressed over the shrinking supply of Treasury debt. They are also aware that these results seem inconsistent with the recent widening of yield spreads between Treasuries and U.S. agency and corporate bonds that Michael Fleming examines in his paper. The authors note that Treasuries have special characteristics that may make demand for them on the part of some investors highly inelastic, and that the portfolio model, which implicitly assumes that risky assets are held in the same proportions by all investors, may not adequately capture these differing demands. The authors point out that foreign official institutions have, in the past five years, nearly doubled the share of Treasuries that they hold. Together with other foreign holders, these institutions (which include foreign central banks) now own 42 percent of all Treasury securities held outside the Federal Reserve. If these or other holders are especially unwilling to substitute other assets, demand for Treasuries will be increasingly inelastic as their supply shrinks.

The authors reason that portfolio adjustments by foreigners could have major direct effects on the U.S. economy through the exchange rate. At the

end of 1999, the net external U.S. position at market value was already a negative $1.5 trillion, and with the current account now in deficit at an annual rate of more than $400 billion, this net indebtedness is rising rapidly. If, as Treasury debt disappears, foreigners choose to invest in the debt of other governments, financing potentially large current account deficits could force a substantial depreciation of the dollar over a short period.

Finally, Reinhart and Sack consider whether other instruments could supplant Treasury securities as a benchmark for many financial market activities. They note that the liquidity of Treasuries may already have lessened slightly and that Fannie Mae and Freddie Mac have been issuing securities on a regular basis in a manner designed to encourage their adoption as alternative benchmarks. Using a stylized model of trading activity, the authors illustrate how the willingness of investors to accept a particular asset for this purpose depends on their belief about the willingness of other investors to do the same. They also demonstrate how modest changes in that belief can have large effects on the liquidity of a given asset's market. The authors suggest that the large increase in trading volume of agency securities and the parallel decline in that of Treasury bills in the past few years are consistent with such a model. Given the self-reinforcing nature of liquidity, they reason that any security that manages to win benchmark status is likely to become entrenched in that role. Thus, if policymakers want to influence the choice of a new benchmark, they should do so without delay.

IN THE FIFTH PAPER of this issue, Michael Fleming looks for early evidence about how financial markets might react to a diminishing supply of Treasury securities, and how this diminished supply might affect the future conduct of monetary policy. He begins by describing how the liquidity of Treasury securities, together with their freedom from default risk, has given them a special place in markets. Treasury yields are widely used as a proxy for risk-free interest rates across the spectrum of maturities. They are a reference benchmark for other fixed-income securities and are widely traded in derivatives markets, where they are used to hedge interest rate risk in other securities. And they are used as a reserve asset by a wide range of financial institutions, including the Federal Reserve. Fleming also notes that awareness of declining supplies had grown and market participants and other observers had begun to discuss what assets might effec-

tively substitute for Treasuries, even before the Treasury introduced a debt buyback program in January 2000.

Fleming carefully analyzes the recent market behavior of Treasury securities, looking for signs of anomalies that might reflect reduced supply. He finds evidence of temporary price distortions during 2000 in the unusually high cost of borrowing particular issues of some short-term Treasury instruments. The most striking example occurred in the spring, when the cost of borrowing the Treasury bill maturing March 31, 2001, briefly rose 100 to 400 basis points above the general collateral rate. No similar scarcity value appeared in the ten- or the thirty-year bond market. Fleming attributes this fact either to increased willingness of the Federal Reserve and other investors to lend out those longer-term securities, making up for their reduced issuance, or to the market's use of other bonds as substitutes for Treasuries. Although these events demonstrate the distortions that reduced issuance can produce, it is unclear whether such distortions would disappear once participants fully adjusted to reduced supply. However, Fleming notes that trading volume in the May 31, 2001, one-year bill has remained low, suggesting that dealers have been unwilling to take short positions after the sharp rise in the price of the March bill last spring.

To address the issue of liquidity, Fleming examines daily trading volume and bid-asked spreads in detail. The volume of trading in Treasuries increased steadily between the mid-1970s and the mid-1990s as the federal debt grew and as markets developed, making special use of Treasuries for hedging and for creating strips (securities in which principal and interest payments are separated) and other derivative instruments. Volume peaked in the spring of 1997, roughly the same time that the stock of outstanding Treasuries peaked, and both measures have declined since then. Fleming shows that bid-asked spreads, which may provide a better gauge of liquidity than does trading volume, have risen since the middle of 1998, although they exhibit some substantial variations associated with periodic financial market turmoil.

As further evidence of the effect of reduced supply, Fleming shows that the yield spread between ten- and thirty-year Treasuries widened sharply early in 2000 amid speculation that issuance of the latter instrument might soon end. He also finds that the relationship between the entire Treasury market and that for closely related securities has altered. In the past two years, yield spreads between ten-year Treasuries and comparable corporate bonds, agency bonds, and swaps have widened, and that the

historically high correlations between yield changes in Treasuries and in these other securities have fallen. He sees all these changes as evidence of increased scarcity value and suggests that the increasingly idiosyncratic behavior of Treasuries helps explain some dissatisfaction with them as a reference and hedging security.

Market participants have considered three main alternatives to Treasuries as reference and hedging benchmarks: agency debt, corporate debt, and interest rate swaps. Fleming notes that some agencies, starting with Fannie Mae, have already introduced benchmark debt issuance programs to try to stake out an advantage in this role. Such programs provide for the regular issuance of large, noncallable coupon securities in a range of maturities, paralleling the Treasury's historical practice. Fleming observes that yields on agency securities, corporate securities, and swaps move largely in concert over long periods, reflecting a credit risk component not present in Treasuries, and he sees both an upside and a downside to this characteristic. The upside is that the performance of agency securities will correlate more closely with that of other securities, contributing to their attractiveness as a reference. The downside is that these securities will have idiosyncratic risk, especially if the government removes some of the privileges they currently enjoy.

Fleming reports that a futures market now exists for agency securities and that an active market has developed using particular issues of these securities for hedging and trading purposes. These agency issues still display more idiosyncratic price behavior than do Treasuries, and their liquidity remains inferior to that of Treasuries: their daily trading volume is still only one-tenth Treasuries' volume, and their bid-asked spreads are wider as well. But if issuance of agency securities continues to expand while the supply of Treasuries declines, they may become more attractive. Some large corporate debt issues and interest rate swaps have also taken on some benchmark characteristics, but as Fleming explains, they appear less useful than agency debt for this purpose.

The Federal Reserve now holds 17 percent of marketable Treasury securities, and this share would increase rapidly if Fed holdings continue to rise and the supply of Treasuries continues to decline. Fleming calculates that, under present trends for Fed holdings and assuming no change in the current federal budget projections of the Congressional Budget Office, the Fed's share would reach 50 percent in 2005 and approach 100 percent in 2007. This raises concerns about the future ability to con-

duct monetary policy through purchases and sales of Treasuries, and indeed the Fed has endorsed a study to consider alternative asset classes that it might hold and use for this purpose. The Fed already conducts limited operations in other securities, but Fleming points to two concerns that would arise if the scale of such operations increased substantially. One is that the Fed could be seen as favoring some issuers over others, and the other is that it would be assuming some amount of credit risk. Nonetheless, Fleming judges that the conduct of monetary policy would not be importantly impaired by such a change.

THE TWO PAPERS just described explore the implications of the projected reduction in the stock of federal debt for financial markets, asset prices, and the conduct of monetary policy. In the third paper on the declining federal debt, George Hall and Stefan Krieger take quite a different tack, focusing on the role debt plays in enabling fiscal shocks to be shared among taxpayers, bondholders, and beneficiaries of federal expenditures. When an adverse fiscal shock occurs—a war, for example—someone has to pay. The government can satisfy its net-present-value budget constraint, now or in the future, by raising taxes, cutting spending in other areas, or imposing a capital loss on existing bondholders through inflation or higher interest rates. Thus debt allows the government not only to smooth taxes over time, but also to spread the risks of fiscal shocks. As the federal debt shrinks, this ability of the government to shift the risks of adverse or favorable fiscal outcomes onto bondholders is reduced. It must then resort to larger and presumably more costly fluctuations in the returns to bondholders to achieve the same effect, or allow greater variation in government spending or taxes. Hall and Krieger use a calibrated general-equilibrium model to examine the potential importance of this risk sharing role of government debt and its dependence on the amount of that debt. They also compare the optimal fluctuations in the debt calculated from their model with the historical experience in the late 1800s and after World War II.

The idea that government debt can be used to spread the effects of fiscal shocks across individuals and across time is not new. By issuing debt to be repaid later rather than raising taxes, a government facing an adverse shock can shift the added burden of the shock from present to future taxpayers. Even if these present and future taxpayers are the same people, as in representative agent models, so that no distributional issues are

involved, the timing of the tax increases necessary to satisfy the long-run government budget constraint is not a matter of indifference. As Robert Barro showed in the 1970s, if taxes distort individual choices, optimal fiscal policy involves the smoothing of tax rates. In a partial-equilibrium setting in which government expenditure is stochastic and the interest rate is a constant, the optimal tax rate follows a random walk, independent of the persistence of the shocks themselves. Following Barro, various authors have formulated general-equilibrium models in the same spirit, but allowing interest rates or the capital stock to vary endogenously. Typically these models also assume the existence of a complete set of markets in contingent claims; such markets make it possible for agents and the government to optimally share risks. In such models tax smoothing continues to be important, but now, unlike in Barro's model, the government can insure against fiscal shocks. If the government issues state-dependent debt, with the return to bondholders depending on the stochastic realization of its expenditures, it can hedge against unexpected fiscal shocks and need not ever adjust taxes when shocks occur. In this sense the government can buy insurance from bondholders.

To illustrate how state-contingent returns on debt can act as a fiscal shock absorber, and to characterize optimal tax and debt management and evaluate the importance of debt's role, Hall and Krieger present and solve a simple complete-markets model of this type. The government's objective is to pursue tax and debt policies that maximize the welfare of a representative household that receives utility from private consumption and disutility from providing labor services. In doing so, the government must satisfy a present-value budget constraint, financing the stochastic stream of expenditure by imposing taxes on labor and by issuing debt. In the authors' model, government expenditure is the only source of uncertainty and follows a Markov process, so that the current level of expenditure contains all available information about the distribution of future expenditure. Both debt and taxes are allowed to depend on the state of nature. Goods are produced by a constant-returns-to-scale production function involving only labor. Since there is no capital, in equilibrium, current production must be consumed in each period by either households or the government.

Although gauging the quantitative importance of the risk sharing role of government debt requires calibration of the model to the real world, certain qualitative features of optimal tax and debt policy are inherent in the

model's specification. In the model, the government's choices of taxes and debt for each state of the world (given by the level of government expenditure) can be thought of as implicitly choosing household consumption and leisure for that state, given the economy's resource constraint and households' optimizing behavior. The authors show that because of the government's present-discounted-value budget constraint, which gives the common shadow price for the discounted effects of tax changes in different periods, and given the Markov character of expenditure, optimal consumption, leisure, and taxes depend only on the current level of government expenditure and the level of debt at time zero. Given the initial debt, any state of the world with the same present and future expected levels of government purchases will be associated with the same level of outstanding debt, independent of calendar time and history. Hence the optimal policy tends neither to pay down nor to increase outstanding debt simply with the passage of time. Although expected changes in the level of expenditure lead to variations in debt, as would be expected from tax smoothing, systematically reducing or increasing the debt over time does not equate the marginal cost of raising revenue across time and across states.

Going beyond these qualitative implications of the model requires specifying the parameters for preferences, technology, and the government expenditure process. The authors choose parameter values that provide a stylized characterization of the U.S. economy. Output is taken to be simply the sum of consumption c, measured by personal consumption expenditure for nondurable goods and services, and government purchases g, measured by federal government consumption expenditure and gross investment. The authors assume that a household's utility is described by a standard expected discounted utility function, which is additively separable in goods and leisure. They assume log utility in consumption, giving a modest level of relative risk aversion, and choose values for the level of labor productivity and leisure preferences that imply that households devote about one-third of their time to working. The authors specify a two-state (high or low) government spending process, with expenditure values roughly corresponding to those in two different periods. One is a "large-shock" period, 1941–49, corresponding to World War II and the early postwar years, and the other a "small-shock" period, 1950–99. For the large-shock period they set the low level of g at 17 percent of output and the high level at 57 percent. For the more normal, small-shock, period they

set the two values at 20.5 percent and 28 percent, respectively. In both cases they assume a probability of 0.75 of remaining in the same state from one year to the next, and a probability of 0.25 of switching to the other state. This gives an average stay in each state of four years. These processes do a good job of matching the actual mean shares of *g* (and the complementary mean shares of *c*) for both periods and the standard deviations of the actual shares for the small-shock period. For the large-shock period the assumed process understates the actual standard deviations of *g* and *c* shares by about a third. The actual share of *g* varied from 25 percent in 1941, to 59 percent in 1944, to 17 percent in 1947, with the actual standard deviation a dramatic 18 percent. The initial level of debt is set to 110 percent of output at the beginning of each period.

Given these parameter choices, the authors compute the behavior of the variables of primary interest—the means and standard deviations of government debt, the real return on that debt, the unexpected return on that debt, and the marginal tax rate—assuming that the government pursues a policy that maximizes households' welfare. They note several interesting features of the results. They find it striking that, for both the large- and the small-shock periods, the optimal labor tax rate is essentially constant, at a level roughly sufficient to cover the average level of *g* plus the real interest costs on average debt. Hence the calculations show, as they should, that optimal policy does not lead to systematic changes in the level of debt over time. Historically, marginal tax rates were both lower and more volatile. Despite the fact that during World War II and the immediate postwar period the actual real interest rate on government debt was negative, government debt grew substantially during the period.

In the authors' model, government debt is the medium through which risk sharing takes place. Debt pays different real returns depending on the state of nature, which, in this model, is the level of government spending that materializes. Fluctuations in the real returns on debt simply reflect this contractual agreement. These fluctuations, with optimal, state-dependent fiscal policy, turn out to be enormous. Only a trivial portion of this variation reflects the fact that, when government purchases are high, they are expected to decline and consumption is expected to grow, so that market clearing requires a higher expected return than when purchases are low. Most of the variation reflects the insurance element in government debt in the model, with a very large fraction of the absorption of shocks to the net present value of government purchases falling on bondholders rather

than taxpayers. In the large-shock case, when purchases shift from low to high, the return on government debt is a dramatically negative –61 percent. Conversely, when "peace breaks out," bondholders receive a large share of the bounty, with a real return of 135 percent. Even in the small-shock case the returns on government debt vary from –10 percent to +18 percent. These results are in sharp contrast to the actual variation in returns. With optimal policy the standard deviation of real returns in the large-shock case is 54.7 percent, compared with an actual standard deviation of returns of 4.5 percent in the 1941–49 period. Since, for a given amount of variation in government purchases, the amount of insurance required for optimal risk sharing is given, the variation in the rate of return required on government debt to provide this insurance is smaller, the larger the stock of debt. Hence the optimal variation in the rate of return on government debt would be roughly half as great if the initial level of debt were doubled.

The authors recognize that the optimal policy implied in their simple, frictionless model overstates the amount of real interest rate variability that would be optimal in the real world. Almost all U.S. government debt carries a nominal return that is not state dependent. In the context of their simple model, such debt can serve the same role as debt with state-dependent real returns if the government uses unexpected changes in inflation and interest rates to deliver the same real returns. But in the real world there are costs to achieving state dependence in this way. Inflation itself is costly, and creating large fluctuations in the real rates of return on government debt might well impose costs on capital markets, creating fluctuations in the returns on capital and on private financial assets and liabilities not included in the model. Since the government would need to weigh the benefits from mimicking state-dependent debt against these costs, it would be useful to know something about the magnitude of the potential gains.

To get some idea of this magnitude, the authors perform three sets of experiments in which they compute the permanent increase in consumption that would be needed to compensate for a particular deviation from optimal policy in their model. The first deviation allows state-dependent taxes and returns on debt but requires the government to run a balanced budget. In contrast with the (unconstrained) optimal rule, this experiment results in substantial variation in consumption. The permanent increase in consumption required to compensate for this restriction is only 0.47 percent of consumption in the small-shock case but a quite substantial 7.9 percent of consumption in the large-shock case. The second experiment adds

to the balanced-budget rule the restriction that debt is risk free. The incremental cost of this additional restriction is trivial in the small-shock case but increases the compensation required by about 40 percent in the large-shock case. The third experiment estimates the cost of systematically setting the tax rate above or below the optimal rate for a period of time, thus building up or running down the level of government debt. The results suggest that the cumulative deficits of the 1980s and 1990s had a very modest cost, equivalent to 0.3 percent of consumption. However, the cost of raising taxes sufficiently to reduce the debt from its 1998 level to 5 percent of income by 2010, and then returning to the optimal tax rate, is equivalent to 1.7 percent of consumption.

Running down the debt, of course, is not suboptimal if it reflects anticipated future growth of government expenditure. Optimal taxation in that situation will reflect future revenue needs and create current budgetary surpluses. Setting taxes below their optimal level would require increasing taxes in the future. The authors note that, according to their model, the current funding shortfall in Social Security, estimated at about 2 percent of GDP a year, is roughly comparable to the shortfall following the 1980s tax cuts and hence has similarly modest welfare costs.

Having illustrated with their theoretical policy experiments the role that state-dependent debt can play in hedging against fiscal shocks, the authors examine the late nineteenth century and the post–World War II period to see whether government debt has actually played such a role historically. The late nineteenth century, like the turn of the twenty-first century, was a period when the federal government appeared to be on a path that would pay off its outstanding debt. The federal government achieved a steady stream of primary surpluses from 1866 to 1893. The authors provide a fascinating description of the process by which the Treasury repurchased debt during this period, reducing the national debt from a high of $2.8 billion in 1866 to just under $1 billion in 1893. Combined with the steady deflation of the period, these repurchases delivered substantial capital gains to existing bondholders. However, the repurchase of public debt at a premium was politically unpopular and presented Treasury officials with a dilemma. If they accumulated surpluses without repurchasing debt, they would reduce the money supply, making it difficult for firms and households to obtain loans. In 1890 taxes were reduced, by 1892 the Treasury had stopped repurchasing its debt, and by 1894 it had resumed issuing new debt, although in modest amounts. From 1890 to the start of World War I, the ratio of public debt to GNP remained less than 10 percent. The authors

see the fact that bondholders were given high rates of return during the first part of this period, in response to positive fiscal shocks, as consistent with optimal policy as given by their model. However, the delay in adjusting taxes in the face of apparently permanent primary surpluses does not accord with the model so well. The authors leave to future research the interesting question of whether the low level of debt that persisted from 1890 to World War I resulted in more volatile taxes, inflation, or government expenditure than optimal policy called for.

Inadequate data make it difficult to compute the real returns, expected and unexpected, on government debt during the nineteenth century, but it is possible to do so with some precision for the post–World War II period. This is far from a trivial exercise, since it involves calculating, for every period t, the sum of all the nominal principal and coupon payments the government has promised to deliver at all future dates $t + j$, and then converting these nominal quantities into real terms. As they are based on a comprehensive definition of real returns on government debt, the authors' constructed numbers are consistent with the government budget constraint. The real value of government debt, including the monetary base, at the end of each period reflects the real value of debt, including money, at the beginning of the period, the real rates of return paid during the period including negative seigniorage, and the primary deficit. The authors decompose the real rate of return itself into an expected and an unexpected component, using a vector autoregression with the consumer price index, industrial production, and the AAA bond rate as explanatory variables.

The authors' computed real return—the cost of funds to the Treasury—is markedly different from the Treasury's officially reported interest cost. As expected, their computed cost of funds is lower, on average, by roughly the inflation rate over the period. The government's real cost of borrowing has been remarkably low. The real annual return on government debt has averaged only 0.8 percent since 1941, and only 1.2 percent since 1947. Moreover, the authors' real cost of funds is substantially more volatile than the official series, with much of the variation reflecting changes in nominal rates rather than the price level. The authors regard the large capital loss imposed on bondholders right after World War II as the most striking feature of their time series. But they note that the government's real borrowing cost was also negative in 1950 with the outbreak of the Korean War, during the high inflation of the 1970s, and in 1994 with the steep fall in bond prices.

It would be difficult to believe that large negative real returns on government debt are anticipated. And indeed, the authors show that a large

fraction of the capital losses incurred in 1950, the low real rates of return during the Vietnam era and the OPEC years, and the decline in the bond market in 1994 were all unexpected. They also find a number of positive surprises during the 1980s and 1990s. Although these calculations suggest that bondholders absorbed a substantial fraction of the unexpected fiscal shocks, the authors note that these outcomes are not in accord with the optimal timing of gains and losses given by their model. The model indicates that bondholders should have suffered large losses at the beginning of World War II and reaped large gains at its end. This inconsistency with the model's prescription is evident throughout the postwar period, where the correlation of unanticipated real returns and the annual percentage change in government expenditure is positive rather than negative.

The authors' model omits several features of reality that should caution against taking its results too literally. It ignores the costs of inflation, which would argue against using inflation to deliver unexpected gains or losses to bondholders. It does not include capital, and so paying down government debt cannot add to the stock of capital bequeathed to future generations. Nor does it allow for the fact that varying the returns on government bonds would be expected to spill over to private capital markets, with adverse consequences. The authors recognize these limitations. But they believe that the model's central message remains, even taking these caveats into account. Paying down the government debt will require larger fluctuations in taxes or discretionary government expenditures in response to fiscal shocks, or larger fluctuations in the returns on the debt, than would otherwise be the case.

DEVELOPING COUNTRIES VARY in their degree of openness to different forms of foreign investment. One of these, foreign direct investment (FDI), can bring with it essential technical expertise, management skill, and access to global markets. But it also involves a degree of foreign influence in the economy that some countries find unacceptable. Therefore FDI is often the object of official measures either to encourage or to restrict it. Other forms of capital inflow, such as loans from foreign banks, carry neither these special advantages nor these drawbacks. But as the currency crises of the past decade demonstrated, such flows can be volatile and can increase a country's exposure to a currency crisis. Whereas official measures aimed at influencing capital flows can be taken as reflecting the intentions of governments, FDI may also be affected by various forms of

corruption in the host country, whose costs and effects may not be intended. In the final paper in this issue, Shang-Jin Wei examines the effect of corruption on the level of FDI and on the composition of foreign investment that a host country receives.

Wei begins by presenting an econometric analysis aimed at explaining FDI flows between source and host countries in the mid-1990s, using corruption in the host country as one of the explanatory variables. To measure corruption he uses indexes from two recent publications: *The Global Competitiveness Report 1997,* produced by the World Economic Forum and the Harvard Institute for International Development, and the World Bank's *World Development Report 1997*. The indexes are based on surveys, covering fifty-three and sixty-nine countries, respectively, which asked business executives and other individuals with experience in developing countries to indicate whether they need to make irregular, additional payments to get things done. Because corruption may be correlated with official host-country policies to attract or deter FDI, Wei includes indicators of such policies as additional explanatory variables. For this purpose he constructs indexes of FDI restrictions and incentives from detailed descriptions of policies in each country compiled by the international consulting firm PricewaterhouseCoopers. Policies contributing to the restrictions index include controls on foreign exchange transactions, exclusion of foreign firms from some sectors, and restrictions on the share of foreign ownership. Policies contributing to the incentives index include incentives for foreign investment in certain industries or geographic areas, tax concessions for foreign firms, subsidies to foreign firms, and special promotions for exports.

Wei's other main explanatory variables are those often found to be important in models of interaction between developed and developing economies. These include distance (measured logarithmically) between source and host country, the presence of linguistic ties, host-country GDP and GDP per capita (in logarithms), exchange rate volatility, and the corporate tax rate and the government deficit in the host country. The regressions also include source-country dummy variables. The dependent variable is the logarithm of bilateral FDI flows, averaged over 1994–96, and the sample includes up to 658 such observations, the exact number depending on which independent variables are included in the regression.

Wei finds corruption, along with most of the other main variables, to have a substantial and significant effect on FDI in each of several regres-

sions, but with magnitudes that are sensitive to which other variables are included. In a regression that omits exchange rate volatility and the government deficit, a rise in host-country corruption from the level in Singapore to that in Russia, the two extremes in Wei's index, is estimated to reduce FDI by 65 percent. In this regression, FDI restrictions and incentives, distance, the presence of a linguistic tie, and GDP are all significant and important. However, when exchange rate variability and the government deficit are added as explanatory variables, the estimated effect of corruption is only about half as large, and the corporate tax rate loses significance. Wei also adds a variable to test whether the U.S. law against bribing foreign government officials tilts that country's foreign investment away from corrupt countries. The coefficient on this variable is negative, as expected, but insignificant. In other variations on his basic regression, Wei finds a significant positive effect on FDI from a variable measuring host-country accounting standards, but an insignificant effect from an index that measures the arbitrariness of corruption.

Because of its size and growing openness to the world, China has become a major recipient of FDI. Wei reports earlier work that has indicated that China receives significantly more FDI than its size or level of development predicts it should. Yet China is also widely regarded as corrupt. Wei therefore uses his present model to examine more carefully whether China's FDI is more or less than should be expected, given the characteristics that he has found important in explaining FDI to developing countries generally. To do this, he adds to the regressions a dummy variable for bilateral FDI flows to China. The coefficient on this dummy variable is uniformly negative and significant, indicating that China actually receives less foreign investment than one would predict from the present model. Adding the square of host-country population to the regression, to allow for the possibility that the regression penalizes very large countries, leaves the dummy variable negative but makes it insignificant.

A key feature of foreign investment into China in recent years is that more than half of that investment is recorded as coming from Hong Kong. The bilateral FDI flows used in Wei's regressions reflect this. However, the nature of business activity between Hong Kong and the mainland is not transparent, and Wei conjectures that some FDI into Hong Kong may reflect use of the former British colony as a stepping-stone for investing in mainland China. As a rough way of allowing for this possibility, he reas-

signs half of measured FDI into Hong Kong to China and repeats the regressions with the China dummy with this new data set. Although there is little effect on the other explanatory variables, the China dummy is now estimated as positive but insignificant. Wei concludes there is inevitable ambiguity about the estimates for China, but no evidence that it gets more FDI than one would expect from his model.

Finally, Wei addresses the effect of corruption on the division of capital inflows between FDI and bank lending, the two categories for which bilateral flow data are available. Although corruption may make a country less attractive to foreign creditors and portfolio investors as well as to FDI, Wei reasons that the disadvantages to foreign direct investors are likely greater. Not only does corruption make FDI initially more costly than other investment, but the need for direct investors to operate in the country subjects them to continued corruption costs, and the illiquidity of their investment contributes to this vulnerability. For four countries near the extremes of his corruption index the composition of foreign investment is as expected. Argentina and Thailand, among the most corrupt, have much less FDI relative to bank loans than do Singapore and New Zealand, which are among the least corrupt.

Can this relation be generalized across developing countries, and does it remain after other factors affecting FDI and bank lending are allowed for? To answer these questions, Wei turns to regressions similar to those explaining FDI but explaining flows of bank lending as well as FDI from thirteen developed countries to thirty developing and transition economies for which data are available. With bilateral bank lending flows as the dependent variable, the main variables that explained FDI remain significant, but FDI restrictions and incentives do not. Interestingly, corruption has a positive sign in these regressions, suggesting substitution of bank loans for FDI. When the logarithm of the ratio of bank lending to FDI flows is used as the dependent variable, the estimates again support Wei's hypothesis that corruption discourages FDI relative to bank loans. These results are qualitatively the same whether the estimations use fixed effects or random effects. They are also supported by instrumental variables regressions designed to account for the possibility that the corruption measures themselves are endogenous.

Wei concludes that although countries receive less FDI to the extent they are corrupt, corruption may not put them at a similar disadvantage in

obtaining bank loans. Corrupt capital-importing countries thus see the composition of their capital imports distorted, shifted away from FDI and toward bank loans. And given the instability of bank loans compared with FDI, Wei infers that corruption in the end leaves a country more exposed to a currency crisis.