Editors' Summary

THIS ISSUE of the *Brookings Papers on Economic Activity* contains articles, a report, and discussions presented at the fifty-sixth conference of the Brookings Panel on Economic Activity, which was held in Washington, D.C., on September 9 and 10, 1993. The first article presents a new model of the way in which distorted incentives adversely affect financial behavior, focusing especially on how deposit insurance coupled with inadequate regulation led to the thrift industry crisis. The second examines the risk premium on equities—the spread between expected real returns on bonds and stocks—and presents evidence that it has decreased in recent years. The third presents fresh data and analysis of the Russian program to privatize firms. The fourth examines the importance of the credit channel between monetary policy and real economic activity. The fifth provides new empirical evidence on the connection between economic expansion and poverty. The report in this issue questions the accepted wisdom about how U.S. exporters price in foreign markets.

AT LEAST since the savings and loan crisis of the 1980s, there has been a general awareness that deposit insurance, together with inadequate supervision by regulators, could be very expensive to taxpayers. The prevailing model of this process—often referred to as "heads I win, tails you lose" or "fourth-quarter football"—stresses that an insured institution would be tempted to take excessive risks, particularly if it was already nearing insolvency so that the equity in the firm was small. If the risky strategy paid off, the owners would profit handsomely; if it failed, their loss would be limited to the remaining equity. Although such excessive risk-taking was no doubt one characteristic of thrifts in the past, in the first paper of this issue, George A. Akerlof and Paul M. Romer offer a new and different model of distorted behavior generated by discrepancies between private and social returns, reflecting, in the case of thrifts, deposit insurance and inadequate regulation.

The authors show that owners can have an incentive "to go broke for profit" or "loot"—their names for actions that destroy the properly calculated net worth of the firm while allowing owners to increase the amounts they personally take out of it—and show how behavior underlying the thrift crisis of the 1980s fits their model. Although the looting they identify from this period in some cases involved illegal activities, the authors stress that much of what they call looting was perfectly legal. They also show that their model applies in areas other than the thrifts, and offer examples from other recent financial crises.

The authors' theoretical model, incorporating the main features of the regulated thrift industry, develops the conditions under which the looting strategy dominates behavior. It assumes owners of firms act so as to maximize their own net worth. Under most conditions, this will lead them to invest in the normal way, maximizing their firm's profits. But if they can get more from the firm by taking out funds until the regulators close it, they will have an incentive to follow that strategy instead. For such looting to be feasible requires both deposit insurance by the government and the ability to overstate the true economic value of the firm's assets. Deposit insurance permits owners to attract cash with which to pursue the looting strategy. And they will try to attract additional cash so so long as it can be invested in assets that provide a high current cash flow, which the owners can tap, while being carried on the books at a value that helps to meet the firm's capital requirements.

The ability to overstate the true value of the firm's assets so as to circumvent the capital requirements is thus critical to the strategy. The authors identify various accounting rules that help make it possible. One is attaching a value to goodwill in establishing the net worth of a firm. During the 1980s, such a value could be attached to a thrift that was acquired by another thrift, even when the acquired thrift was bankrupt. Another is allowing a long bond to be carried at par. This permits a firm to buy long bonds at a time when interest rates are expected to rise and to generate an immediate cash flow equal to the yield advantage of the long bond over current short rates without having to write down the value of the bond as rates rise. Still another is investing in assets that are difficult to value. Real estate development projects, which are notoriously hard to value, thus offer special opportunities for firms that are aggressively interested in bending accounting rules. The authors show how a thrift

working with an unscrupulous developer can maximize near-term cash flows by lending on projects that have no possibility of eventually paying off but that can nonetheless be carried on the books at full value until they default.

The authors use their model to highlight the difference between their bankruptcy for profit strategy and the more familiar strategy of excessive risk-taking. In the excessive-risk strategy, the owners prefer an outcome in which the gamble pays off and the thrift remains solvent. In the looting strategy, the owners may intentionally pursue a strategy that they know will lead to bankruptcy. In the excessive-risk strategy, owners invest on the basis of the distribution of possible returns, even though they choose more risk than is socially desirable. In the looting strategy, owners invest simply to maximize the cash flows they can extract before bankruptcy.

Akerlof and Romer go on to relate the main features of the savings and loans crisis to their model. They show how changes in regulations and accounting conventions encouraged looting strategies and increased the amounts that a determined looter could hope to extract. From detailed accounts of the crisis, they find evidence of investments designed to yield artificially high accounting profits and strategies designed to pay large sums to officers and shareholders, both signs that looting did in fact take place. And they show that, by their evidence, looting contributed significantly to the overall savings and loan crisis.

Although the looting model applies most obviously to financial institutions, with their insured deposits and ability to attract funds, the authors discuss how it also can help explain other developments of recent years. And they extend their basic model to show how the initial incentive to loot can have effects that spread well beyond the institutions and firms directly involved. The extended model is formulated in terms of land development. In it, if some developers take the movement of prices, which get distorted by the looters, as a signal for underlying demand, they are led to develop on the basis of the false price signals. This leads to overbuilding and, eventually, to bad real estate loans and bankruptcies for both developers and lenders throughout the market. The authors observe that the boom and bust in Dallas real estate during the 1980s seems to fit the model. They also show how the financial crisis in Chile at the start of the last decade can be interpreted as a case of looting,

using dollar liabilities to finance the holding of high-yielding peso assets. And they relate the surge and collapse of parts of the junk bond market to their extended model.

Akerlof and Romer also explore the possibility of a link between the looting of some thrift institutions, the junk bond market, and the wave of corporate takeovers in the 1980s. As they note, thrifts never held more than $13 billion in junk bonds—compared with a $200 billion total of junk bonds outstanding and a $1.3 trillion total of assets changing hands through takeovers during the decade. Yet, they argue, the holdings by thrifts interested in looting may have had a disproportionate influence on the takeover market because they kept the interest rate on junk bonds artificially low. Thus, even though most takeovers were conducted with serious attention to the true economic returns likely from a deal, distorted price signals in the junk bond market could have made debt-financed takeovers seem more attractive to investors than they actually were. The authors assemble evidence that default rates on junk bonds controlled by Michael Milken were manipulated, making it likely that the looting of some thrifts involved with junk bonds had an influence on the takeover wave. However, they emphasize that this could explain, at most, only a small part of the volume of takeovers that took place.

The authors conclude with some cautions about arrangements in the economy that could pose opportunities for looting in the future. They note that the portfolios of life insurance companies can be difficult to monitor, citing the cases of First Executive Life, which was involved with junk bonds, and Coastal States Life, which invested in risky interest-only strips, as recent examples of bankruptcy with major losses to policyholders or taxpayers. And they cite pension funds, backed by pension fund guarantees, and mortgages guaranteed by government as additional areas with high risk of looting. The authors observe that the design of government guarantees and regulatory programs has always had a political dimension. Their paper sharpens our understanding of the potential unintended by-products of such programs and reaffirms the need for adequate regulation and realistic accounting standards.

THE LONG-TERM BOND RATE is frequently regarded as the primary measure of the returns available to savers and the cost of capital to firms. During the early 1980s, real bond rates increased sharply and have remained high since. Some economists saw the increase in the real rate as

a signal of scarcity of saving and an augury of low investment and growth. Others regarded the increase as a sign of rising demands for capital, spurred by antigovernment economic polices in the United States and western Europe during the early 1980s. But while real bond rates remain high, the surge in equity values after 1982 and the decline in the dividend-price ratio that accompanied it suggests that the return expected on equities in the future may have declined, cutting the equity premium over bonds. If so, the environment for saving and investment is quite different than it would appear from looking at the bond rate alone. In the second paper of this volume, Olivier Blanchard examines the historical behavior of the real rates on bonds and equities, and the equity premium itself, in an attempt to evaluate this change and to explore its possible causes.

To place his subsequent, more sophisticated econometric analysis in perspective, Blanchard begins by comparing estimates of expected real returns on bonds and stocks in six major OECD countries for which data are available since 1982. He uses DRI forecasts of inflation over short (one-quarter) and medium (five-year) horizons to measure expected inflation rates, and uses those measures to calculate the implied expected real rates on short-term and medium-term bonds. These show considerable dispersion across countries, but, for most, the major movements are qualitatively similar. Aggregating to "world real rates" by weighing countries by GDP, Blanchard shows that the world short-term rate climbed from -1.0 percent in 1978 to 4.9 percent in 1984, while the medium-term rate rose from 2.1 percent to 6.3 percent. Both rates declined gradually after the mid-1980s. Movements in yields on U.K.-indexed bonds beginning in 1982 are qualitatively similar, but less dramatic.

To calculate the expected real rates of return on equities, Blanchard utilizes the fact that if stock prices are the present discounted value of future expected dividends, the expected real rate of return on stocks is implied by the current dividend yield and the expected real growth in dividends, appropriately discounted. The behavior of the "world" dividend yield is striking. According to Blanchard's calculations, it increased from 4.4 percent in 1978 to 5.0 percent in 1982, then fell to 2.8 percent by 1992. This fall reflects the fact that while real dividends have grown by 34 percent since 1982, real stock prices have increased by more than 90 percent. The decline in dividend yield is a feature of all of the countries except Italy. Could this decline in yield be offset by an in-

crease in the expected rate of dividend growth? Blanchard finds dividend growth closely tracks the evolution of output. In order to believe that the risk premium is the same in 1990 as it was in 1980, expected long-run real dividend growth would have to have increased to a 4.7 percent annual rate, which is well above estimates of long-run output growth for developed economies.

For his formal analysis, Blanchard restricts attention to the United States, where data are available going back to 1927. Instead of using private forecasts of inflation, he estimates, for each year, the "annuitized" expected inflation rate (expected future inflation rates weighted by their importance in the present real value of a bond), using lagged values of inflation, the dividend-price ratio, real capital gain, and the nominal interest rate as predictors. Given the apparent instability of the inflation process, he uses rolling regressions for the later part of the period; expected inflation at time t is inferred from equations estimated over the forty years prior to time t. Blanchard reports results for two horizons, five years and twenty years. In his regressions, past inflation, by itself, explains about 32 percent of the inflation over the next five years and about 10 percent of inflation over the next twenty years. While the sum of coefficients on past inflation tends to be higher at the end of the sample, it remains substantially below one, reaching 0.55 for five-year-ahead and 0.17 for twenty-year-ahead inflation rates.

When all of the variables are included in predicting inflation, several features stand out. The nominal interest rate, which would have a coefficient of 1.0 if the expected real rate were constant and expectations were rational, has a coefficient near -0.3 for both the five- and twenty-year inflation rates, significantly different from 1.0. These results suggest large movements in the real rate and a systematic effect of inflation on real rates. The estimated series for expected inflation over five- and twenty-year horizons smooth out the major peaks in actual inflation—World War II and the two oil shocks—and, appropriately, the twenty-year expectations shows less variability than the five-year rate. Interestingly, the five-year inflation rate expected in 1980, a year with 12.5 percent actual inflation, is 9.7 percent, almost identical with the DRI forecast.

The implied five- and twenty-year real interest rates show substantial variation over the sample. Both are negative from the mid-1930s to the mid-1950s, and relatively low in the mid-1970s. Expected real rates

reached record highs in the early 1980s in spite of the high levels of expected inflation.

Continuing with the assumption that stock prices are the present discounted value of future dividends, Blanchard separately estimates equations for the two ingredients of expected return—the dividend-price ratio and real dividend growth—weighting future dividend growth rates by their contribution to present value. These variables are regressed on the same four variables used in estimating real bond yields, again using rolling regressions. Blanchard stresses these are reduced forms and not easily interpretable. But he finds one result of particular interest. If most movements in the dividend yield reflect changes in the expected growth of dividends, the dividend yield should have a negative coefficient in an equation explaining dividend growth; equity prices should be high and the dividend yield low when future growth is expected to be high. But, in fact, the coefficient on the lagged dividend yield is positive and significant in regressions with or without the inclusion of other explanatory variables. Since high dividend yields are also typically followed by high yields in subsequent years, high dividend yields unambiguously predict high expected equity returns. Another interesting result is that inflation and the nominal interest rates appear to have little to do with explaining either dividend yield or dividend growth.

Blanchard's estimated movements of expected rates of return on bonds and stocks have been strikingly different over the past sixty-five years, frequently moving in opposite directions. From the 1930s to the mid-1950s, expected rates of return on stocks were high, while expected rates of return on medium- and long-term bonds were low and often negative; this pattern was repeated in the 1970s, although with smaller differences. In contrast, the 1960s and 1980s were characterized by relatively high expected bond rates and low expected stock returns. Indeed, in several years, expected bond returns actually exceed expected stock returns.

Blanchard's estimates of expected returns on equity do not utilize information on the capital gains actually realized from holding stocks over the relevant horizon. In his analysis, capital gains are assumed to be either unexpected or captured in changes in current and expected dividends. He examines the sensitivity of his conclusions to this assumption, constructing a realized equity premium using actual capital gains and regressing it on the same variables used to predict his dividend-

based expected return and equity premium. Lagged dividend yield has a significant positive effect on the equity premium in either formulation, but is substantially larger in the regression using actual capital gains. One major difference is in the effect of inflation which, in the period 1954–73, is large and significant in predicting the capital gains–based premium, but small and insignificant in predicting the dividend-based measure. Despite these differences, the movements in the expected premium implied by the two regressions are quite similar. The expected equity premiums rise from 3 to 5 percent in the early 1930s, peak at more than 10 percent in the late 1940s, and drop to 2 to 3 percent today. There are substantial fluctuations in the expected premium from year to year. These fluctuations seem to be correlated primarily with movements in inflation, with a temporary reversal of the postwar downward trend in the premium in the 1970s and a renewed decrease in the premium in the 1980s, when inflation was sharply lower.

Blanchard examines the effect of inflation on the premium more formally by running separate vector autoregressions, one for the joint process of inflation and bond rates, the other for inflation and the components of stock returns—the dividend yield and expected dividend growth. This analysis verifies the importance of innovations in inflation on real rates, but the effects are relatively small and relatively short-lived. A 1 percentage point innovation leads, after a year, to a decline in the five-year real rate of about 0.2 percent, and disappears after two years. The effects on the twenty-year rate are about half as large. The effects of an inflation innovation on expected real stock returns are more complicated, leading to a decrease in stock prices in the initial year, but an increase in expected future real dividend growth. Since the immediate price decline raises the dividend yield, after the first year the expected return on stocks increases unambiguously. According to Blanchard's estimates, a 1 percentage point inflation innovation is associated with a lasting increase in the expected rate of return on stocks of 0.1 to 0.2 percent. While the one-year expected return is dominated by the capital loss, over longer horizons the effects on dividend yield and growth dominate. These results reconcile the results of research on very short holding periods, which typically has found negative effects from inflation, with the positive effects for longer horizons.

Blanchard examines two potential explanations of the trend movements in the equity premium. One is that increases in the relative supply

of government debt increase its relative rate of return and thus lower the premium. But Blanchard notes that such an explanation only works for the 1980s. The 1940s are associated with both a large buildup of debt and a high premium, while in the postwar period until the 1970s, the decline in the premium coincides with a steady decline in the debt-GDP ratio. This rejection from U.S. time series is confirmed by a panel study on twenty countries since 1960 that finds a positive association between debt-GDP ratios and the equity premium.

A second potential explanation, which appears more promising, is that the premium reflects the perceived relative riskiness of stocks and bonds. Blanchard notes that the volatility of stock returns declined in the late 1920s, leading to high stock prices and a low equity premium, and then rose in the early 1930s, with investors shifting to government securities. The gradual trend decline in the premium in the postwar period could be explained by fading memories of the 1930s and the growing importance of institutional investors who, Blanchard suggests, have longer horizons and might be expected to take advantage of the attractive equity premium.

What are the implications of Blanchard's analysis for firms' investment? Blanchard suggests that we should be wary of focusing primarily on the high real bond rate. The cost of capital has both an equity and debt component; the high bond rates in the early 1990s are accompanied by a low equity premium. An equally weighted index of expected bond and equity returns shows much less variation than either return individually, and is essentially trendless. Today it is not far from its average over the last sixty years. While variations in the bond rate may have less importance for firms than usually thought because of associated changes in the risk premium, the variations in the premium have obvious implications for investors in the financial markets who are deciding on their asset portfolio. Blanchard also notes that governments relying on debt finance can take little comfort from the lower equity premium. They will have to continue to live with the painful arithmetic of debt accumulation with low growth and high real bond rates.

IN THE MIDST of the economic and political convulsions that are gripping Russia, the privatization of Russian firms has proceeded at a pace that few observers would have predicted and that is still not widely recognized. By September 1993, more than 20 percent of Russian industrial

workers were employed by privatized firms, and privatization had spread even more widely in the service sector. Yet despite its growing presence, privatization of firms has thus far delivered few of its potential benefits to the Russian economy, in large part because it has not been accompanied by other needed reforms. In the third paper of this volume, Maxim Boycko, Andrei Shleifer, and Robert W. Vishny document the progress that has been made in privatization, analyze the barriers to privatization's effectiveness, and outline what further reforms are needed if Russia is to move to a productive market economy.

The authors' view of what it takes to make privatization work emphasizes the need to depoliticize the management decisions of firms. Public enterprises are inefficient, the authors argue, not because benevolent governments are ill-informed about efficiency and so make well-intentioned mistakes, but rather because governments pursue political objectives rather than economic efficiency. The authors offer examples from the period when the Soviet Union was a pure control economy under communism. Industry produced primarily military goods because the politicians cared about security and not social welfare, firms were overstaffed because politicians wanted full employment to prevent social unrest, and farms were collectivized to control peasants.

In Russia's economy today, even though managers are not tightly controlled by politicians, the authors observe that political influence is still important, if less direct. By using subsidies, soft budget constraints, or access to credit and other rationed inputs, politicians bargain with firms as a way of getting them to pursue political objectives, such as maintaining employment, rather than profit-making objectives, which would satisfy market demands and improve production efficiency. Believing that such influences remain potent, the authors argue that depoliticization of the firm's decision process is the key to creating an efficient market economy. The authors note that the power of politicians over firms is a matter of degree and that, in practice, improving economic efficiency requires increasing the cost to politicians of influencing firms. Privatization is helpful because it increases that cost. Owners care about the forgone profits that come from pursuing political objectives; ministries and other mechanisms of political influence are dismantled when industries are privatized; and a political constituency develops to oppose government interference. But while privatization may have be-

gun the process of reducing political control, the environment is still hostile to a market economy in many ways.

Informed by this overall view of Russia's situation, the authors provide a more detailed account of where things stand and how they might evolve. Using newly assembled data, they first describe and evaluate the Russian privatization program to date. The program treated different categories of firms in different ways. Most small shops and enterprises were assigned to local governments to be sold for cash. At the other extreme, privatization was prohibited in industries such as health and education; and privatization of important strategic industries, such as defense or natural resources, required the approval of the government, which had effectively kept them from being privatized before President Yeltsin dissolved the parliament. The authors therefore focus their attention on those industries that were effectively open to privatization through a voucher system that distributed ownership shares. These included firms subject to mandatory privatization, which included most firms in light manufacturing such as textiles or food processing, and firms subject to privatization with permission of the privatization ministry (the GKI), which included larger firms not operating in what were regarded as strategic industries.

Employees and managers invariably ended up with at least half the outstanding shares in their firms through voucher distribution. Much of the balance of shares was distributed to the public through auctions using vouchers that had been made available to every person in Russia for a small fee. An uncertain portion of the vouchers, which the authors estimate as equivalent to 20 percent of all available shares, was kept by the government; their disposition is unclear. Some might be used to attract foreign investors; others may have been given away to managers, friends, or relatives. Vouchers are tradable, so that managers and some outsiders have acquired large stakes in some firms.

The Russian method of distributing shares had two distinctive effects. It gave workers a more generous share in firms than they received in privatization programs elsewhere, and it gave managers a great deal of control, in part because mutual funds, which might have monitored their performance, are not an important factor, as they are in some eastern European countries. The authors see this outcome as the result of political pressures in Russia that favored the existing managers and

workers but that may have compromised the incentives for achieving economic efficiency.

Some evidence that the system is not working efficiently is provided by data on privatized firms that the authors assemble. By mid-1993, 2,418 firms employing 3.6 million workers had been privatized in voucher auctions, with firms employing more than 1,000 workers accounting for more than 80 percent of privatized assets and employment. At recent rates of privatization, firms employing an additional four million workers will be privatized by the end of the year, at which point private firms would account for about 40 percent of manufacturing employment. This appears to be remarkable progress in a country where no other economic reform has yet succeeded. But the value of these privatized firms tells a different story.

Vouchers are traded both for rubles and shares of firms, enabling the authors to calculate market prices for shares and—using exchange rates—share prices and firm values in dollars. Using prices as of September 1993, they show that ZIL, the truck and limousine manufacturer with a ready market for its product, has a market value of $16 million. Two large firms that roughly correspond in product line and employment to Caterpillar and General Electric have market values of $4 million and $6 million, respectively. Extrapolating from their sample of firms, the authors estimate the total value of Russian manufacturing industry, not including the most valuable raw materials, is about $5 billion. Put another way, they find that Russian manufacturing companies have a market value of about $100 per employee, which compares with a value per employee in U.S. companies one thousand times as great. Making allowances for quality, they still estimate that Russian assets are valued at no more than 1 percent of their U.S. counterparts. Improvement in voucher prices since President Yeltsin dissolved the parliament would not change this qualitative picture much.

The authors suggest three reasons for this low valuation of privatized firms, each of which they characterize as a potential form of expropriation of shareholders by other stakeholders. First, employees have a strong position in firms, in part through their ownership of shares, so that efficiency improvements may be converted to higher wages and other benefits and never pass on to owners. Second, managers are in a position to expropriate wealth from firms by selling assets to their own privately held businesses or through other devices. The high levels of

capital flight from Russia may be one reflection of such activity. Third, the government may expropriate corporate wealth through taxes, regulations, and other interventions, including eventual nationalization.

Although the authors have no specific remedies for these actual or potential problems, they nonetheless see privatization as a useful first step. They present polls showing that privatization is popular and provide regressions showing that regions in which relatively more firms had been privatized had significantly more support for President Yeltsin and for further reforms. This popularity, they reason, suggests that privatization may help tip the political balance in favor of the other structural changes that they believe are crucial to creating a more efficient economy.

The other needed changes highlighted by the authors are competition policy, equity governance, and capital allocation. Exposing firms to competition both at home and from imports could help depoliticize firms by forcing them to respond to market forces to survive, as it has in both Poland and the Czech Republic. Giving equity ownership to active decisionmakers—meaning managers and large shareholders who would put pressure on managers—could provide an important counter to political pressure. And, most important, replacing the political allocation of capital with private allocation would eliminate the dependence on politicians that would otherwise undermine the decisions of even profit-oriented managers and owners. However, there are thus far few signs of progress on any of these fronts.

Product market competition has been resisted by politicians at all levels. Opening up foreign trade has not even gotten onto the political agenda. There have been attempts to resurrect old direct-control ministries in the form of trade associations and financial-industrial groups. And bankruptcy law permits effectively permanent continuation of bankrupt companies, which means that managers lose little. Absent other changes, such as market-oriented capital allocation, the authors are not optimistic about increasing competition in Russia.

The significance of equity governance as a factor controlling managers is as yet unclear in Russia. The authors present data on ownership of shares in individual firms from two separate surveys, each of which, unfortunately, is based on self-reporting by the firms. These show that managers and employees own, on average, between 60 and 70 percent of the shares in a company, with outsiders and the government's prop-

erty fund splitting the rest. The one survey with information on management's share alone shows the management team owning 17 percent, on average. The authors suggest that managers have even more control than their ownership share implies because workers are frequently allied with management, contributing to their entrenchment rather than monitoring their performance.

Of the roughly 15 to 20 percent of shares held by outsiders, half are reportedly held as large blocks by investment funds or individuals and, in rare instances, by foreigners. Anecdotal evidence suggests these large blockholders have occasionally tried to influence company policy, but have generally been successfully rebuffed by managers. Political battles are being waged over how much influence outside shareholders can exert.

The last and, in the authors' analysis, most important barrier to restructuring firms and creating an efficient market economy is the political allocation of capital. There is virtually no private source of credit available to most Russian enterprises, in part because bankruptcy laws do not give lenders access to a firm's assets. Joint ventures with foreign firms are a potential source of capital, but they have not been important thus far because political risks are great and policies toward foreign ownership have been hostile. As a result, the government's subsidy and credit programs largely dictate the allocation of resources, both directly and through the need to keep existing loans from defaulting. In 1992, subsidies from the budget and directed credit from the central bank each amounted to about 21 percent of GDP.

The authors view stabilization policies as complementary to development of the market economy and see special benefits in pursuing both together. They reason that the main force leading to destabilizing money creation is the need to provide cheap credits to subsidized state enterprises and sectors. Privatization helps make stabilization possible to the extent that it removes the demand for subsidies and subsidized credits. In turn, stabilization would limit the ability to allocate capital politically, and so could give private capital markets a chance to develop. Thus the authors believe that a policy pursuing both stabilization and further structural reform would have a much better chance of succeeding than privatization has on its own.

MANY OBSERVERS have suggested that an important reason for the anemic recovery from the most recent recession has been reduced availabil-

ity of finance from banks and other traditional short-term lenders. Several candidates have been advanced to explain why the supply of credit might have been reduced, including the erosion of banks' capital positions, toughening of bank regulation, and the Basle agreements. But it is unclear how much of the slowdown in lending by banks and thrifts reflected decreases in supply, and how much reflects weakness of loan demand by creditworthy borrows—itself a reflection of the recession or other factors. In the fourth paper of this issue, Benjamin M. Friedman and Kenneth N. Kuttner examine, theoretically and empirically, the role that credit markets play in the determination of economic activity, and attempt to assess the role of credit availability in the recent recession.

In order to clarify the distinctive elements of the credit view, Friedman and Kuttner begin with a brief survey of different models of how financial assets and markets affect real nonfinancial activity. At one end of the spectrum are classical models in which financial markets play no role in the determination of real output, and money affects only the price level. The authors label as neoclassical a class of models, including Keynesian and monetarist versions, that have as an essential element some price rigidity so that changes in the nominal supply of any asset or liability, such as money, necessarily change its real supply and, consequently, affect the real economy. The liabilities of households and firms are typically not recognized in any of these models, and, if they are, they are treated as perfect substitutes for nonmoney assets and are also irrelevant to real outcomes. In contrast, credit models have the distinguishing feature that the liabilities of firms and households, and their counterparts, the assets of bank and nonbank institutions, play a role in the determination of nonfinancial activity. When various assets and liabilities are imperfect substitutes, no single rate or quantity adequately captures the links between financial and nonfinancial markets. And in models that allow for credit rationing, real activity is directly constrained by the supply of credit.

Friedman and Kuttner are proponents of the credit view, believing that the imperfect substitution between assets and liabilities of the private sector is an essential feature of the interaction of financial and real markets, regardless of whether there is some form of credit rationing. Using regressions estimated from 1960:1–1990:12 and for the subperiods 1960:1–1979:9 and 1973:6–1992:12, they present evidence that price and quantity variables that would play no role in the standard money-cen-

tered neoclassical model are systematically related to real output. Two variables suggested by the credit view, the paper-bill spread and the quantity of commercial paper, are significant in explaining output for each period, even in the presence of interest rates and aggregate debt and money variables. In contrast, neither aggregate monetary variables nor the volume of bank loans is significant after controlling for the paper-bill spread.

The authors present a theoretical model to better understand the observed relation between credit prices and quantities and real output. They use the model in three ways: to analyze the channels by which monetary policy affects real output when credit as well as money is important; to analyze the effect of various shocks, such as changes in capital requirements, on financial markets and output; and to identify the distinctive patterns of price and quantity response of various potential financial causes of the recent recession. The model has banks, open-market investors, and nonfinancial firms interacting in the markets for Treasury bills, bank loans, and commercial paper. The authors believe this is the minimum number of agents and assets sufficient for their investigation. The rates on credit variables are important to each of the agents. For nonfinancial firms, the financing constraint plays a central role; the difference between investment outlays and net revenues after paying interest and dividends must be financed by increases in loans from banks, increases in commercial paper liabilities, or reductions in holdings of government bills. For the short horizon they consider, the authors take the financing needs as predetermined and the allocation among the three sources to depend on their rates of return, plus two variables corresponding to nonprice cost elements of using loans or commercial paper.

Nonprice costs play an important role in Friedman and Kuttner's explanation of observed price-quantity behavior. For both commercial paper and loans, nonprice costs are simply additional costs of borrowing to consumers or firms. In the case of loans, costs to borrowers are just another way banks earn a profit from lending; a higher cost (to borrowers) is associated with an increased willingness to lend. The authors assume this cost is a function of the loan rate and the volume of loans. In the case of commercial paper, in contrast, these costs are assumed to represent real resource costs accruing to "market makers"—dealers, rating agencies, and the like. Hence, increases in the nonprice cost of commercial paper depress both the supply of paper and households' de-

mand. These costs are assumed to depend on the volume of commercial paper, bearing no necessary relation to the paper rate.

Investment behavior depends on the expected return on investment, the risk of return, and the supply of internal funds, and is the main link between financial and real markets. The model is closed in conventional ways, with aggregate spending depending on investment and firms' revenues and private saving depending on spending. The government supply of reserves and bonds, banks' capital position, and the expected return and risk on investment are taken as exogenous.

As this description makes clear, the authors' model provides a highly articulated description of the financial behavior of households and firms, enabling it to predict a wide range of financial variables and to provide a rich account of their response to monetary policy. How does the model account for the observed relationships between short-term credit markets and real economic activity? To start, a reduction in reserves decreases banks' supply of transactions deposits, leading them to reduce loans and their holdings of bills, and putting direct upward pressure on the bill and loan rates. In response, firms seek credit elsewhere, issuing commercial paper and selling off bills, actions that raise the paper rate and put additional pressure on the bill rate. With plausible lags in investment, this mechanism can account for the empirical regularity emphasized by Anil K. Kashyap, Jeremy C. Stein and David W. Wilcox: that firms' mix of loan and paper finance tends to shift toward paper in advance of a decline in real output. The observed widening in the paper-bill spread could arise because marginal entrants during tightening are less creditworthy than existing borrowers; and it could arise simply because the supply of commercial paper rises relative to the supply of bills. In the authors' model, the nonprice cost of issuing commercial paper, which rises with its volume, provides another explanation. The authors also use the model to help illuminate the impact of bank capital shocks, which have been widely assigned a role in the weakness of lending and sluggish recovery following the 1990–91 recession. And they use it to examine the effect of increases in default risk or reductions in firms' cash flows on financial prices and quantities.

Armed with the insights of their theoretical model, Friedman and Kuttner turn to an empirical analysis of the relationship between real and financial behavior. They proceed by estimating, for the 1960–92 period, a series of four-variable vector autoregressions (VARs), each de-

signed to focus on the effects of shocks to one of seven different financial variables. In each system, three of the variables are the same: the level of real output, the first difference of the implicit price deflator, and the federal funds rate. The fourth variable in each case is one of the financial price or quantity variables. The causal ordering places the financial variable last, crediting it only for fluctuations not explained by the other three variables.

For each system, the authors display the effects of a monetary shock—an innovation in the federal funds rate—on output and the financial variable, and the effect of an innovation in the financial variable itself on output. They also estimate the same series of vector autoregressions including an additional variable: a simple measure of aggregate bank capital holdings, divided by aggregate bank assets. Some results stand out clearly. A positive shock to the federal funds rate always depresses output, a result that is significant statistically and economically. Most of the responses are in accord with the authors' model; for example, a negative monetary shock causes the volume of commercial paper to increase and causes firms to draw down their liquid assets. There are, however, some anomalies. For example, contrary to the model, negative monetary policy shocks appear to increase loan volume. The authors find the empirical evidence broadly supports the importance of monetary shocks not only in affecting real output, but also in accounting for movements of financial variables like the paper-bill spread and the volume of commercial paper.

The authors introduce the spread between the rates on commercial paper of different qualities as a measure of default risk. An increase in the spread between high- and low-risk paper appears to depress output in the short run. It also reduces the volume of commercial paper, the issuance of CDs, and the volume of bank loans, all in accordance with the model's predictions. However, after initially widening as expected, the paper-bill spread subsequently narrows, inconsistent with the theory. The authors note that the predictive power of the paper-bill spread with respect to output disappears when monetary policy and default risk are included and given priority in the causal ordering.

Friedman and Kuttner have less success in estimating the effects of shocks to bank capital. A good measure of capital adequacy would account for the changing ways in which requirements depend on portfolio

composition and would indicate the capital adequacy of those banks that are near those requirements, or in violation of them, not the average condition of banks in general. Lacking such a measure, the authors rely on the difference between aggregate bank assets and liabilities, divided by aggregate banks assets. But the results of including this variable in the vector autoregressions are disappointing.

The authors buttress their VAR analysis by estimating structural equations for several of the key relationships in their model. Equations are estimated by ordinary least squares and two-state least squares for the change in nonfinancial firms' loans and commercial paper, and for bank and household lending. Overall they believe that these results, even though they do not always pin down price effects, provide further support for their analysis. For example, they find that the shocks appear to shift the relationships as they hypothesized, and the relative magnitudes of the effects show that it is important to dissaggregate the financial sector; for example, the responses of firms' commercial paper issuance and bank borrowing are not simply mirror images of one another.

Finally, the authors turn to an analysis of the dramatic slowdown that took place in the U.S. credit markets during 1989–92. Especially for bank loans, the experience from late 1989 onward was quite extraordinary. In the fifteen years before 1990, bank loan contraction was limited to a few isolated quarters; in contrast, the volume of bank loans outstanding declined steadily throughout 1991–92. The authors use a set of vector autoregressions similar to those previously estimated to examine the innovations in monetary policy, bank capital, default risk, and business cash flows for the period 1986–92. Most notably, their results suggest that monetary policy shocks, although small, were mostly contractionary during this period. Bank capital shocks were sharply negative in 1987 and again in late 1989, but positive in between and at the very end of 1992. Default risk shocks were sharply positive in late 1990. The monetary shocks appear to play a major role in accounting for the slow growth beginning in 1989, and the default risk shocks are also especially important during the recession. On the other hand, the bank capital shocks, which helped explain the slowdown in credit, had no discernible independent effect on real output throughout the period.

As in their analysis of the recent recession, the authors believe that a variety of short-term credit markets play a role in the financial-nonfi-

nancial relationship and are worth bringing explicitly into the analysis. They warn against relying heavily on a bank-centered explanation of the impact of monetary policy and other financial shocks.

HISTORICALLY, ECONOMIC EXPANSION has brought with it a reduction of poverty. Typically, expansion has worked through several channels, including an overall increase of jobs and average hours, job upgrading that benefited low-income workers, and a narrowing of the distribution of wages across jobs. Yet poverty rates worsened during the 1980s and improved only slightly even when the overall unemployment rate declined by four percentage points during the period of rapid economic expansion between 1983 and 1989. Related indicators, such as the distribution of income, tell a similar story about the decade. In the fourth paper of this volume, Rebecca M. Blank and David Card attempt to account for the changes in income distribution and poverty during the 1980s and to determine whether the link between economic expansion and these indicators of performance has changed.

For their investigation, Blank and Card assemble longitudinal data on income and poverty for the twenty-five years from 1967 to 1991. In addition to national totals, they disaggregate their data into nine regions, and divide the regional data further into five family types (plus all families) and five income quintiles (plus average income) for a total of 9,000 observations. Because there were significant deviations in income growth and unemployment across the nine regions, the use of regional data provides additional information with which to estimate the effects of economic change on poverty and the income distribution.

The authors first estimate regressions to examine the relation between income distribution and two key economic indicators of the state of the labor market—unemployment and median income. They show that a rise in unemployment raises unemployment of individuals from families at the bottom of the income distribution by twice as much as it raises unemployment of those from families at the top. But they find a substantial difference between the effects on family heads and other family members. The rise in unemployment of family heads is greatest for families at the bottom of the income distribution. But because higher-income households are far more likely to have multiple earners and because many workers affected by unemployment are second and third

earners in a family, the higher the family income quintile, the greater the rise of unemployment for other adult family members.

When the authors go on to examine the effects of unemployment on earnings and income, they find that many offsetting effects are at work. Surprisingly, the elasticity of total income with respect to unemployment is approximately equal for all income quintiles. A higher elasticity of earnings with respect to unemployment in the lower quintiles is almost perfectly offset by a lower share of earning in total income, resulting in approximately equal percentage changes in total income in response to unemployment. Thus variations in unemployment have no important effect on the distribution of income among quintiles of households. Although they find little effect on the distribution of income, the authors emphasize that unemployment has a strong effect on the levels of family income for all income quintiles. For the average family, each percentage point increase in unemployment is associated with a 1.4 percent decline in family income.

Because labor force participation and real wages may change over the cycle independently of unemployment, Blank and Card also explain changes in the income distribution using median family income rather than unemployment. They find the effect of median income growth is not spread evenly across all income groups, with the bottom and top quintiles of families getting less than proportional benefits and families in the second and third quintiles getting the largest relative gains. The top quintile suffers a significant reduction in its income share as median income rises. There is evidence that this redistributional aspect of rising median income growth was weaker during the 1980s than earlier.

The authors next examine the effects of including two additional variables summarizing labor market outcomes—the median wage rate and the dispersion of wage rates—along with the unemployment rate in regressions explaining the distribution of family incomes. They regard regional variations in these variables as reflecting differences in the level and distribution of productivity, as well as in the availability of job opportunities. The general pattern of wage dispersion, which is measured by the standard deviation of log wages, is similar for most regions, with a decline in dispersion from 1973 to 1979 and a rapid rise thereafter. Two regions, New England and the east south central states, depart somewhat from this pattern. Regressions using the three variables summariz-

ing labor market outcomes successfully explain a good part of the variation in mean family earnings, mean family income, and family income shares by quintile.

The main findings can be summarized by the effects on income shares. The growth in earnings and incomes that comes with a rise in median wages leads to some income equalization, while greater wage dispersion clearly raises inequality. Unemployment, as in the results described above, has little effect on distribution. Between 1979 and 1989, median wages grew slightly, unemployment fell slightly, and wage dispersion widened substantially. These changes in labor market developments explain some, but not all, of the widening in the income distribution between 1973 and 1991. In addition, the regressions show that, once the dispersion of wages is allowed for, there is little evidence that income distribution became less responsive to changes in unemployment and median wages during the 1980s.

Blank and Card go on to examine how the dramatic change in the composition of families over the past twenty-five years has affected statistics on family income inequality and poverty. For all families and for families in each income quintile, they calculate a mean income series keeping family composition constant. The calculations show that changes in family composition have led to a 14 percent decline in mean family income during the past two decades. If family composition had not changed, income in the bottom four quintiles would have been higher than actual and income in the top quintile would have been lower than actual. Thus changing family composition has noticeably reduced the level of average family income and somewhat widened the distribution of family incomes. However, they find that composition changes have not contributed to any weakening of the relation between labor market conditions and family income.

Blank and Card next directly examine the poverty rate, which is simply the fraction of family units with real incomes below the poverty line, which depends on family size and composition. The poverty rate thus reflects both average real family incomes and the relative position of the lower tail of the income distribution. Using regressions for regions over the 1967–91 period, and controls for region and year effects and for family characteristics, they find significant connections between poverty and their labor market outcome variables, with increases in unemployment and wage dispersion raising poverty rates and increases in median

wages reducing them. With these labor market outcomes in the regressions, family characteristics are also highly significant; for example, poverty rates are higher for black families and lower the more educated the family head.

While family composition changes affect the level of poverty, the authors find they have much less effect on the responsiveness of poverty to economic changes. When they fit regressions to individual family types, they find median wages and wage dispersion affect all types in the expected way. However, unemployment effects vary, ranging, curiously, from a negative effect on families with elderly heads to a very strong positive effect on single-headed families with children. The authors further show that observed changes in family composition have made poverty rates more responsive to changing labor market outcomes generally.

When they compare actual poverty outcomes to those predicted by their equations, Blank and Card find that the actual poverty rate for all families rose less than predicted. They show that a substantial decline in poverty rates among the elderly accounts for most of this prediction error. Single-person families also did better than predicted. On the other hand, single-headed families with children experienced a larger increase in poverty rates than the regressions based on labor market outcomes predicted, confirming the widely held belief that the plight of such families has worsened.

THE RISE AND FALL of the dollar during the 1980s focused the attention of economists, journalists, and policymakers on the responsiveness of trade flows to changes in the dollar. An important empirical result that emerged and that gained substantial popular currency was that U.S. firms differed from foreign firms in that foreign firms priced to market, but U.S. firms did not. When the dollar fell, foreign firms absorbed most of its effect and let their profit margins shrink to maintain market share. In contrast, throughout much of the 1980s, U.S. firms evidently just allowed exchange rate changes to pass into final prices. Some critics of U.S. firms saw this as evidence that U.S. firms were less fleet-of-foot than foreign firms.

However, theory suggests that it would not be optimal for firms with market power to pass through exchange rate movements. In their report in this issue, Subramanian Rangan and Robert Z. Lawrence focus on the apparent contradiction between this prediction of theory and the evi-

dence on how U.S. firms behaved. If it were optimal for firms with market power to price to market, then why should U.S. exporters—many of them large firms with at least some market power—not do so?

Rangan and Lawrence attack this apparent contradiction by questioning the accepted evidence on how U.S. firms behave. First, they report on a small survey suggesting that exporters did, in fact, price to market during the depreciation of the dollar in the second half of the 1980s. Second, for the question at hand, they raise serious doubts about the usefulness of the export price data that others have relied on to infer the pricing behavior of U.S. firms. Rangan and Lawrence argue that the data from the Bureau of Labor Statistics (BLS) are collected in a way that is likely to capture the U.S. domestic price, rather than the price actually paid by foreign purchasers. If so, export prices would closely track U.S. domestic prices, and it would appear as though U.S. firms directly passed exchange rate movements through to prices, regardless of whether their actual prices to foreigners did so.

The potential problem with data collection is especially apparent for exports from U.S. multinational corporations. Rangan and Lawrence show that the intrafirm exports of U.S. multinationals—products shipped by U.S. parents to foreign affiliates—account for about 40 percent of U.S. manufactured exports. They provide reasons why the prices reported to the BLS for these intrafirm exports are likely mainly to represent U.S. domestic prices. Since the pricing report to the BLS is filled out in the United States by the parent company, it will be easier for the respondent to report the U.S. wholesale price, rather than the price actually paid by the ultimate foreign buyer, especially if the foreign affiliate has independent pricing power. And if tax authorities are monitoring transfer prices, the most acceptable transfer price to report generally will be the price at which arm's-length sales are made in the United States.

Such considerations raise the possibility that U.S. multinationals are reporting domestic prices as export prices, even if the firms are pricing to market abroad. Bolstering this argument, the authors show that official BLS export and domestic prices track more closely in industries in which exports are dominated by multinational firms. This result is just what would be expected if all firms priced to market, but U.S. multinationals reported "export" prices in the way described above.

Rangan and Lawrence go on to estimate directly the extent of exchange rate pass-through in pricing by U.S. firms. They present a standard model of price-setting behavior for a firm with market power selling in a foreign market. For such a firm, the price-cost margins of its foreign affiliate should stay constant if the firm is fully passing through exchange rate movements and costs, and should vary with the exchange rate if the firm is pricing to market. For example, during an appreciation of the dollar, a U.S. firm pricing to market would let its price-cost margin shrink so as to maintain market share.

The authors estimate the actual degree of pass-through by regressing the price-cost margins of foreign affiliates of U.S. firms on the exchange rate, U.S. costs, and foreign costs. Because their data on price-cost margins do not rely on the BLS export price data, their test of pass-through avoids the problems they suspect in the BLS series. According to their estimates, multinationals did not simply pass through exchange rate movements during the 1980s. Depending on the sample period, the estimated coefficients imply that between 14 and 60 percent of exchange rate movements were passed through to prices, far short of the conventional wisdom of full pass-through. Furthermore, the implied pass-through rates are similar to those reported in the literature for foreign firms competing in U.S. markets. Thus, it appears that the pricing behavior of U.S. exporters is not all that different from their foreign rivals.

Rangan and Lawrence observe that pricing is only one of the important ways in which firms respond to exchange rate movements. The other is sourcing, or the share of content produced in different countries. The authors investigate whether the intrafirm exports of U.S. multinational firms—products shipped from U.S. parents to foreign affiliates—respond as much to changes in the exchange rate as arm's-length exports. Simple comparisons suggest that intrafirm exports within U.S. multinationals were less responsive in this dimension than arm's-length trade. For example, as the dollar depreciated between 1985 and 1989, the value of U.S. intrafirm manufacturing exports rose by 47 percent; but this was still less than the 65 percent increase in overall U.S. manufacturing exports, suggesting that U.S. manufacturers took less advantage of improved U.S. competitiveness in their sourcing decisions than changing competitiveness alone would have dictated.

However, more careful analysis by the authors reveals that intrafirm exports of U.S. multinationals responded to exchange rate movements in a manner similar to arm's-length trade. Once industry composition is controlled for, the difference in the growth rates of intrafirm and overall exports disappears. They show that the U.S. content of products sold by U.S. multinationals in twelve out of eighteen countries responded in the expected way to changes in the exchange rate between 1985 and 1989. And their estimated elasticity of substitution between U.S. content and local content is similar to conventional estimates of the export price elasticity.

On the basis of their findings, the authors offer several observations concerning research on trade patterns. They note that to understand trade patterns, it is necessary to understand the pricing and sourcing behavior of multinationals, which play a large role in U.S. exports. And they warn that the export price data published by BLS must be used with care, particularly for studying pass-through. However, they note that the data are probably adequate for broader questions about the relationship between prices and quantities of exports. This is because changes in sourcing—rather than in final sales—appear to be the major channel through which the exchange rate affects intrafirm exports, and the BLS data probably provide a reasonable proxy for the marginal costs that would affect sourcing decisions.