Beyond Shocks: What Causes Business Cycles? An Overview

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In the summer of 1997, when the Federal Reserve Bank of Boston selected the topic for its forty-second annual economic conference, many pundits were asking: "Is the business cycle dead, or at least permanently dampened?" By the time the Bank's conference convened in June 1998, the same pundits queried: "What caused the massive recessions in Asia?" and "Can the United States remain 'an oasis of prosperity,' as Fed Chairman Alan Greenspan termed it, while economies worldwide are under siege from financial crises?" How quickly things change!

Beyond Shocks: What Causes Business Cycles? turned out to be a particularly timely conference. Of course, the answers to the pundits' questions are inextricably tied to an underlying fundamental question: What makes economies rise and fall? To determine whether the business cycle is dead, one must first determine whether economic fluctuations arise from the decisions of governments, financial market participants, and businesses, or simply from unexpected events (that is, "shocks"). To determine why Asian economies plunged into severe recession, it is necessary to understand how external pressures on vulnerable financial markets can lead to a sudden collapse, with severe consequences for nonfinancial sectors. And to determine whether the robust economic expansion in the United States will continue, it is necessary to evaluate how a slew of adverse economic factors, financial and real, could interact to end it.

So, what caused the Asian crisis, the recessions of the 1970s and 1980s, and even the Great Depression? According to many modern macroeconomists, shocks did. This unsatisfying answer lies at the heart of a currently popular framework for analyzing business cycle fluctuations. This framework assumes that the macroeconomy usually obeys simple behavioral relationships but is occasionally disrupted by large "shocks," which force it temporarily away from these relationships and into recession. The behavioral relationships then guide the orderly recovery of the economy back to full employment, where the economy remains until another significant shock upsets it.

Attributing fluctuations to shocks—movements in important economic variables that occur for reasons we do not understand—means we can never predict recessions. Thus, a key goal of the conference was to try to identify *economic* causes of business cycles, rather than attributing cycles to "shocks." The greater the proportion of fluctuations we can classify as the observable and explainable product of purposeful economic decisions, the better chance we have of understanding, predicting, and avoiding recessions.

Several themes emerged during the conference. One was the concept of "vulnerability." It was especially prominent in discussions of the recent Asian crises and bears on the distinction between shocks and systematic economic behavior. Rudiger Dornbusch perhaps put it best in the following analogy. Consider the collapse of a building during an earthquake. While the proximate cause of the collapse was the earthquake, the underlying cause may better be attributed to poor construction techniques. Because of its structural defects, the building was going to collapse when the right "shock" came along. So it goes with financial and real economic collapses, Dornbusch and many others would argue.

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While it will always be difficult to anticipate the particular event that precipitates a collapse, it is important to constantly assess the vulnerability of financial, product, and labor markets to potential shocks. Macroeconomists and forecasters tend to focus primarily on the overall health of the economy as measured by aggregate demand or by the unemployment rate; they may be able to improve their economic models by incorporating vulnerability. Likewise, policymakers should be vigilant against vulnerability. To do so, they will need to develop new tools. In Asia, for example, policymakers should have had a better assessment of the ability of the financial system to absorb shocks to currency valuations.

Developing such an assessment would likely have been hampered, many conference participants pointed out, by the inability to obtain key data on the debt portfolios of financial institutions, the performance of bank loans, and the exposure of the country as a whole to exchange rate risk. Proposals abounded for more accessible banking data and new indexes of risk exposure. Although little agreement was reached on exactly what information would be most useful, most agreed that policymakers and investors need new and more timely measures to adequately assess the vulnerability of economies to severe disruptions.

A second theme of the conference discussion was the role of systematic monetary policy in causing and preventing business cycles. Many have blamed the bulk of recessions on monetary policy. But as pointed out by Peter Temin, Christina Romer, and Christopher Sims, in assigning blame, it is important first to distinguish the systematic response of monetary policy to existing conditions from policy regime shifts and exogenous policy shocks. To take a leading example, did the Fed cause the Great Depression by raising domestic interest rates to maintain the gold standard, or was the outflow of gold from the United States following Great Britain's abandonment of the gold standard the cause, and the response of the Fed a "business as usual" response to that triggering event? Such questions are very difficult to answer, but a careful attempt to do so must be made if we are to understand the role of monetary policy in cycles.

Most participants agreed that the Fed played a significant role in causing many of the recessions of the past century, largely in the pursuit of its goal of long-run price stability. The degree to which monetary policy did or could moderate the effects of cyclical downturns was less clear. Many pointed to the apparent diminution of the amplitude of business cycles in the postwar period as evidence of the Fed's ability to lessen the severity of contractions.

Interestingly, Sims's more formal analysis of this question raised doubts that the systematic component of monetary policy either causes fluctuations or can offset them, at least through interest rate movements. Using econometric substitution of modern interest rate policy back into the Great Depression era, Sims found that modern policy would have had little effect on employment or prices. While this finding met with a good deal of skepticism from participants, one skeptic who tried to prove Sims wrong—discussant Lawrence Christiano—reported that he could not. In any case, the suggestion that conventional interest rate policy is limited in its ability to offset major recessions is thought-provoking. Of course, the limitations of interest rate policy do not preclude alternative policies, such as deposit insurance and acting as lender of

last resort in financial crises. These policies may be at least as important as interest rate policy.

A third conference theme was the importance of a deeper understanding of the contribution of changes in the efficiency and structure of production to business cycle fluctuations. Recently, some macroeconomists have advanced the idea that shocks to these supply-side or "real" factors cause many, if not most, of the ups and downs in the economy. This idea contrasts sharply with the traditional macroeconomic notion that changes in aggregate demand cause most fluctuations, and the two views generate quite different policy implications.

One theme of the conference discussion was the concept of "vulnerability"; another, the role of systematic monetary policy in causing and preventing business cycles. A third was the need for deeper understanding of the contribution of changes in the efficiency and structure of production to business cycle fluctuations.

Two real shocks were evaluated. One is a shock to the technological efficiency of firms' production of goods and services. Technological changes are very positively correlated with output and business cycles, a relationship that has led many observers to conclude that technology shocks cause fluctuations. Susanto Basu, however, demonstrates that more detailed and sophisticated estimates of technological change substantially reduce, if not completely eliminate, the correlation between technology shocks and the business cycle. He also shows how modern macroeconomic models, especially those that rely primarily on technology shocks, have difficulty fitting the data. Proponents of technology-oriented models were predictably skeptical of his results.

The second real shock is a change in the desired distribution or allocation of economic resources across firms, industries, and regions. Restructuring involves the costly and time-consuming reallocation of factors of production, especially workers, between firms, industries, and regions through the processes of job creation and destruction. It also typically involves lower output, higher unemployment, and often even recessions. In fact, job reallocation and job destruction rise sharply during recessions, leading some to surmise that shocks to the process of reallocation itself may be responsible for recessions and should therefore be taken into consideration by macroeconomic models. Scott Schuh and Robert Triest discover strong correlations between job reallocation and the primary determinants of how jobs are allocated across firms and industries: prices, productivity, and investment. Correlations between these determinants and job reallocation suggest that it is not mysterious allocative shocks that cause business cycles, but significant changes in observable economic variables.

Together, the two studies of real shocks reaffirm the fact that the production and employment behavior of firms is subject to substantial variation over the business cycle, but they deepen doubts that the variation is due to real shocks. Instead, the correlations between output and simple measures of real shocks reflect the failure of conventional analyses to incorporate a sufficiently detailed specification of production and market structure. As more and more of firms' behavior is accounted for in macroeconomic models, less and less scope remains for real shocks to generate business cycles. However, much is still to be learned about business cycles from the behavior of factor utilization, investment, prices, productivity, and the like.

Opening Address: History of Thought on the Origins of Business Cycles

Paul Samuelson's opening address begins with the question "Is the business cycle dead?" While the macroeconomy appears to have stabilized over the past 50 years, perhaps owing to successful countercyclical macropolicy, Samuelson sees no evidence of a trend toward the elimination of business cycle fluctuations. He notes that after most periods of extended expansion, especially those accompanied by outstanding performance in asset markets, suggestions of a "new era" of recession-proof prosperity have arisen, and they have been received "with increasing credulity" as the expansion rolls on. Acknowledging this historical association between healthy economies and booming asset markets, Samuelson takes a more realistic view, stressing also the intertwined histories of

business cycle downturns and bubbles and crashes in asset markets.

Samuelson cites Victor Zarnowitz's recent observation that in the seven decades between 1870 and World War II, the United States suffered six major depressions. In the past 50 years, we have had no declines of comparable severity. Samuelson attributes this improved performance to changes in "policy ideology, away from laissez-faire and toward attempted countercyclical macropolicy." But despite the gains in policy's management of the economy, Samuelson sees no "convergence towards the disappearance of non-Pareto-optimal fluctuations. We are not on a path to Nirvana." The scope for improved performance arising from better government policies appears marginal today.

Samuelson notes that when persistent macromarket inefficiencies threaten both employment and price stability and private incentives fail to encourage financial markets back into line, only policymakers can take the systemic view necessary to guide the economy back into balance.

So pronounced fluctuations in production, prices, and employment are here to stay, despite the best efforts of policymakers. But why? In the end, Samuelson argues, fluctuations are usually the product of two factors. First, on the upside, asset price bubbles will always be with us, because individuals have no incentives to eliminate "macromarket inefficiency." While we have made tremendous progress toward "micro-efficiency"—making individual financial markets more efficient through the widespread use of options and other derivatives, for example—little evidence can be found, either in economic history or in economic theory, that "macromarket inefficiency is trending toward extinction." One can make money by correcting any apparent mispricing of a particular security, but one cannot make money attempting to correct apparent macro inefficiencies in the general level of stock market prices.

Economists and financial market participants simply have no theory that can predict when a bubble will end. As a result, an individual investor will be perfectly rational in participating in a bubble, as he will make money from the bubble so long as it continues, which could be indefinitely. As Samuelson puts it, "You don't die of old age. You die of hardening of the arteries, of all the things which are actuarially . . . associated with the process. But that's not the way it is with macro inefficiency." Bubbles go on until they stop, and no one has ever been able to predict when that will be.

Downturns can develop from the asset markets themselves, and they can develop quite quickly. Because asset prices are based on the "prudent ex ante expectations" of market participants, swings in market expectations can produce large and rapid swings in asset prices, causing massive revaluation of asset-holders' wealth. This was in part the cause of the ongoing Asian crisis, according to Samuelson. Market participants reasonably reassessed the valuation of investments (and therefore currencies) in Asia and quickly altered the direction of capital flow, precipitating a currency and banking crisis there.

Given the lack of private incentive to restrain the stimulative effects of this "oldest business cycle mechanism," we come to the second factor that contributes to business cycle fluctuations: government policy. Samuelson noted that he has often said, "When the next recession arrives, you will find written on its bottom, 'Made in Washington.'" This is not, as he points out, because the Fed is a sadistic organization. Rather, "if the central bank and fiscal authorities did not step on the brakes of an overexuberant economy *now*, they might well have to overdo that later." When persistent macromarket inefficiencies threaten both employment and price stability and private incentives fail to encourage financial markets back into line, only policymakers can take the systemic view necessary to guide the economy back into balance.

Historical Evidence on Business Cycles: The U.S. Experience

Peter Temin examines the causes of U.S. business cycles over the past century. In developing his taxonomy of causes, Temin points out three inherent problems with the effort. First, the idea of a "cause" is fraught with ambiguity. In part, this ambiguity arises from the difficulty in distinguishing the endogenous, or "normal response" component of government pol-

icies and private actions, from the exogenous, or out-of-the-ordinary actions of private and public agents. In Temin's view, only exogenous events should be seen as causal. He uses oil prices and the 1973–75 recession to illustrate the dilemma: Was the recession following the oil shock "caused" by the oil shock, or by the monetary policy response to the oil shock? The imputation of causes depends on one's model of economic history, and particularly on the degree to which one makes behavior endogenous or exogenous.

Second, the Great Depression should be treated as a unique event. As Temin notes, output lost during this enormous downturn was almost one-half of the sum of output lost in all other downturns in the past century. The body of writing on the Great Depression is larger than that on all other business cycles combined. Consideration of the causes of the Great Depression provides useful lessons about the causes of the less prominent cycles of the past century. For example, it seems implausible that a single "shock" in 1929 pushed the U.S. economy into massive depression. Instead, Temin argues, the Great Depression was likely the result of a sequence of contractionary influences. Prominent among these were the fear that the hyperinflationary pressures in Eastern Europe following the First World War would spread to the United States, the adoption by industrialized countries of the relatively inflexible gold standard in response to these pressures, and the breakdown of banking and legal systems. The Great Depression was really a sequence of smaller recessions large and persistent enough, given policy responses, to throw the world into depression.

Third, Temin cautions that his assignment of causes relies on the existing literature on the subject. The literature on recessions other than the Great Depression is quite sparse, with earlier recessions receiving considerably less attention than more recent ones. And within this limited set of sources, most authors focus on the *transmission* of cycles, rather than on the causes. Finally, most of the available sources do not highlight expectations and do not clearly distinguish anticipated from unanticipated changes.

Temin classifies the reported causes of recessions as either domestic or foreign, and either real or monetary. Changes in the relative prices of assets, both real and financial, are classified as real phenomena. Temin finds that the preponderance of cycles in the past century may be attributed to domestic causes, with the split between real and monetary causes roughly equal for the entire period. Monetary causes of recessions were more prevalent in the pre-World War I period than during the post-World War II period, however.

Temin focuses on the larger downturns. The cause of the Great Depression of 1931 is classified in Temin's taxonomy as a foreign monetary phenomenon. The action of the Fed to maintain the gold value of the dollar by raising interest rates was to behave as a "traditional and responsible central banker" or, in other words, to follow a normal and expected endogenous policy course. Thus, the Fed's behavior cannot be viewed as an exogenous cause of the Great Depression, in Temin's view. The search for causes then reverts to the question of what produced this monetary policy response. Temin suggests that U.S. monetary policy was responding to the external gold drain that arose from Britain's departure from the gold standard, which threatened to weaken the dollar. The Fed's reaction in increasing interest rates, and the bank panics and failures that followed, were endogenous responses to the gold drain.

Examining the causes of U.S. business cycles over the past century, Temin finds that domestic real shocks—ranging from inventory adjustments to changes in expectations—were the most frequent source of fluctuations, and that monetary shocks have decreased in importance over time.

In assessing the causes of the four largest downturns of the century—the Great Depression, and the recessions of 1920, 1929, and 1937—Temin concludes first that no single cause explains all four downturns. Three of the four possible causes in Temin's taxonomy appear as causes of the downturns. Second, three of the four recessions appear to be responses to domestic shocks. Most often, we cannot blame our downturns on foreign causes.

Taking all of the cycles studied into consideration, Temin offers the following conclusions: (1) "It is not possible to identify a single type of instability as the

source of American business cycles." Thus, Dornbusch's statement, "None of the U.S. expansions of the past 40 years died in bed of old age; every one was murdered by the Federal Reserve," is not supported by Temin's analysis. (2) Domestic real shocks—ranging from inventory adjustments to changes in expectations—were the most frequent source of fluctuations. (3) Other than the two oil shocks of 1973 and 1979, foreign real shocks were not an important source of U.S. cycles. (4) Monetary shocks have decreased in importance over time. (5) When measured by the loss of output, domestic sources have loomed larger than foreign sources; real sources have caused about the same losses as monetary sources.

Christina Romer takes issue both with Temin's classification scheme and with his interpretation of the literature on the causes of recessions. She suggests that an improved classification scheme and a different reading of the literature would yield a more critical role for domestic monetary shocks, particularly in the inter- and postwar periods.

Romer suggests that Temin's methodology is biased toward finding very few monetary causes of recessions. Whereas Temin classifies most Fed behavior as a fairly typical response to prevailing conditions and therefore not the ultimate cause of the recession, Romer would prefer a more practical classification of monetary policy actions. If the monetary policy action was the inevitable or highly likely result of a trigger, then we should consider the policy action endogenous and therefore not a cause. If, however, "a conscious choice was made" or if "alternative policies were . . . discussed at the time," then the policy should be considered at least partly exogenous, and monetary policy should get some blame for the recession.

Romer shows that, using this criterion, many more of the twentieth-century recessions have an important monetary policy aspect. Monetary factors would likely be given an important causal role in the 1931 recession, for example, as "reasonable men *at the time* were urging the Fed to intervene" in the face of financial panics. Thus, the choice not to intervene but to raise the discount rate was not inevitable or even most likely. Romer also questions the extent of the constraint imposed by the gold standard, as U.S. gold reserves in 1931 were probably adequate to have allowed the Fed to pursue expansionary open market operations while maintaining the gold value of the dollar, as in fact it did in 1932.

Turning to the 1973 recession, for which Temin ascribes no monetary role, Romer argues that the central bank was not simply acting as "a respectable central bank [that] resists inflation," and therefore responding only as expected. Romer points out that the decision to tighten in 1974 was not a foregone conclusion but rather a conscious choice, as "the economy was already in a downturn and many were calling for loosening." Thus, "monetary policy and the oil shock share responsibility for the 1973 recession."

Romer also challenges Temin's attribution of the 1957 and 1969 recessions to declines in government spending. She points out that the high-employment budget surplus actually *falls* throughout the late 1950s, suggesting a net stimulative impulse from the federal government for the 1957 recession. For both recessions, Romer asserts that the Federal Reserve made a conscious decision to tighten in order to reduce inflation.

As Romer sees it, the key change in the causes of business cycles has not been from monetary to real shocks or vice versa, but from random shocks from various sources to governmental shocks.

As Romer sees it, "the key change has not been from monetary to real shocks or vice versa, but from random shocks from various sources to governmental shocks." Since the Second World War, the government has been more effective at counteracting most shocks, accounting for the diminished frequency of cycles. However, the combination of a tendency toward overexpansion and a few large supply shocks caused inflation to get out of hand. In sum, Romer would agree with the thrust of Dornbusch's statement, which is that monetary policy has played a vital role in postwar recessions. She might re-cast the role of the Fed, however, as "more like a doctor imposing a painful cure on a patient with an illness than a murderer."

Historical Evidence on Business Cycles: Business Cycles Abroad

Michael Bergman, Michael Bordo, and **Lars Jonung** examine the broad cyclical properties of GDP, using a newly compiled data set of annual observa-

tions for a sample of "advanced" countries. Their data set spans the years 1873 to 1995. The authors show that the duration of business cycles (the calendar time from peak to peak or trough to trough) has been fairly similar across countries and fairly stable over time. The average duration rose from about four years in the pre-World War I period to about five and one-half years during the interwar period, falling back to just under five years in the period following World War II. The most severe recessions appear to have occurred prior to 1946, and the magnitude of all fluctuations in GDP seems to have decreased in the postwar period.

Formal statistical tests of diminished cyclical fluctuations in the postwar period generally confirm the visual evidence. This observation has often been interpreted as evidence that countercyclical policy has been more effective in the postwar period. However, an alternative explanation is that the increased integration of the world economy serves to mitigate the negative influence of any one country's disruptions on other countries.

Bergman, Bordo, and Jonung find that the correlations among real output in the 13 countries studied have increased over time, suggesting a more integrated world economy and possibly a stronger coherence of the business cycle across countries.

Conventional wisdom holds that downswings are sharper and "steeper," whereas upswings are more gradual. Bergman, Bordo, and Jonung test this proposition and find that, for the United States, upswings are indeed more gradual than downswings. The evidence for other countries is more mixed, however, with most exhibiting this asymmetry prior to World War II but only a minority displaying asymmetry in the postwar period.

The authors then attempt to determine the extent to which different components of GDP—including consumption, investment, government expenditures and revenues, exports, and imports—account for its cyclical volatility. For virtually all countries and time periods, all components of GDP except consumption generally are more volatile than GDP. This finding is consistent with the presence of a consumption-smoothing motive, that is, the desire of consumers to maintain a relatively smooth stream of consumption over time in the face of volatility in their income and wealth.

The authors find that larger countries experience deeper recessions; the average decline in GDP below trend is larger for large countries than for small, open European countries. For most countries, the downturn in GDP during a recession is accounted for by declines in consumption, investment, and net exports.

Finally, Bergman, Bordo, and Jonung consider the patterns of international co-movement of output and prices in their data. They find that the correlations among real output in the 13 countries have increased over time, suggesting a more integrated world economy and possibly a stronger coherence of the business cycle across countries. During the gold standard, real GDP for most countries exhibited little or no correlation with real GDP in other countries. During the interwar period, U.S. GDP was significantly correlated with seven other countries, but corresponding correlations between other countries were not evident. The authors suggest that this correlation arises from the role of the United States as the "epicenter" of the Great Depression. Output linkages among European countries strengthened considerably in the postwar period, perhaps the result in part of the establishment of the European common market and in part of the common influence of the oil shocks in the 1970s.

Price levels appear to be much more consistently correlated across countries. Like output, price levels have become increasingly correlated over time, perhaps consistent with "increased global integration of goods markets," the authors suggest.

Richard Cooper offers a different perspective on Bergman, Bordo, and Jonung's conclusion that "the cyclical pattern . . . appears to remain surprisingly stable across time, regimes, and countries" and on the broad question of the international origin and transmission of the business cycle. He examines years in which the raw data for real GDP declined, for a set of nine countries during the periods 1873 to 1913 and 1957 to 1994. Cooper prefers this approach, as the authors' results may depend on the filtering and detrending methods that they used in constructing their data.

The conclusions that he draws for the earlier period are as follows: First, "most downturns are domestic in origin, and are not powerfully transmitted

to the other important trading nations." Second, if one were interested in international transmission, one would focus on 1876, a year in which the Continent and Canada experienced declines in GDP, and on 1879 and 1908, years in which several countries experienced output declines. Third, Belgium exhibits only one downturn during these periods, a suspicious finding given the 12 downturns in neighboring Netherlands and 14 in France. As a result, Cooper calls into question the reliability of the annual data for any of these countries prior to 1914.

Cooper outlines a number of broad changes in industrial economies that lead him to question the stability of the business cycle over long spans of time, including the reduction in the fraction of the labor force required for food production and major technological innovations.

For the period 1960 to 1995, Cooper notes that the few recessions have been concentrated in five years: 1958, 1975, 1981–82, and 1993. This suggests strong international transmission, in contrast to the earlier period. All of the recessions in the United States were accompanied by recessions elsewhere. The greater coherence may be attributed to the importance of the oil price shocks in these recessions, Cooper notes.

Cooper goes on to question the detrending method used by Bergman and his coauthors. Only 60 percent of their recessions match NBER reference dates. The issue of appropriate filtering is important when considering the welfare implications of business cycles, Cooper suggests. A departure of output below its (rising) trend may imply relatively little lost income or underutilized resources, whereas an absolute decline in output would almost surely entail significant welfare losses.

Cooper outlines a number of broad changes in industrial economies that would lead one to question Bergman, Bordo, and Jonung's conclusion about the stability of the business cycle over long spans of time. He suggests that "the most dramatic by far . . . is the reduction in the fraction of the labor force required for food production." The decline in this number from about one-half in 1880 to below 5 percent by 1995 for all of these countries is likely to have altered the dynamics of the business cycle significantly, according to Cooper. Other important secular changes include the increased participation of women in the paid work force, the growth in the importance of government expenditures, and major technological innovations, including electricity, automobiles, and aircraft. "A relatively unchanged economic cycle that survived these dramatic secular changes in modern economies would be robust indeed," Cooper suggests.

Government Policy and Business Cycles

Christopher Sims examines one of the most contentious questions in macroeconomics: the role of monetary policy in twentieth-century business cycles. Sims points out that one cannot determine the influence of monetary policy simply from observed changes in interest rates and output. The observation that a rise in interest rates precedes each postwar recession does not show that policy-induced interest rate movements *caused* the recession. If, for example, rapid expansion of private demand for credit systematically causes all interest rates to rise near the end of an expansion, this rise in interest rates should not be interpreted as the cause of a subsequent slowdown; it is a consequence of previous strong demand. Because such "eyeball" interpretations of the data can lead to confusion about the role of monetary policy, Sims advocates examining the interactions among many economic variables in order to obtain a clear picture of the role of any one of them in economic fluctuations.

Sims employs a methodology that allows each of six variables (industrial production, consumer prices, currency, a monetary aggregate, the discount rate, and commodity prices) to respond to lags of the other variables, and to the contemporaneous values of *some* of the other variables. The restrictions on the contemporaneous interactions among variables reflect common-sense notions about policy, goods market, and financial market behavior. Monetary policy-induced interest rate changes affect prices, output, and monetary aggregates only with a one-month lag; monetary policy responds to output and prices only with a lag, reflecting data availability; and commodity prices respond to everything contemporaneously, reflecting their auction-market, flexible nature.

This simple model is estimated on monthly data for the postwar years 1948 to 1997. Sims uses the model to show that most of the variation in the Fed's discount rate represents systematic policy responses rather than unanticipated shifts in policy. The discount rate responds primarily to movements in production, commodity prices, and M1. These three determinants of interest rate movements in turn cause the largest increases in CPI inflation, suggesting that the Fed responds to these as signals of future inflationary pressures.

When Sims estimates this same model on the interwar period from 1919 to 1939, he finds similarities but also some important differences in monetary policy responses and influences. One key difference is that the effect of interest rate changes in the early period is roughly double the effect in the later period. On the other hand, monetary policy in the early period appears to be more accommodative toward unanticipated increases in output, raising the discount rate less in response to output and thereby allowing greater inflation in commodity and in final goods prices. Interestingly, the model shows that when depositors' worries caused a rush into currency in the interwar period, the Fed typically *raised* the discount rate, accelerating the shrinkage of money.

This first set of exercises establishes that the systematic responses of policy to output and prices represent the dominant source of interest rate fluctuations in Sims's model, and that these interest rate movements are likely the most important source of policy's effects on the rest of the economy. Noting that economic fluctuations have been smaller in the postwar period, Sims proposes using his model to answer a key question: whether better systematic monetary policy is responsible for the improved economic performance of the postwar period.

To answer this question, Sims transplants the estimated monetary policy equation for one period into the other period, then observes the estimated behavior of output, prices, and monetary aggregates under this counterfactual monetary regime. The results from these exercises are remarkable. In the first variant, the (estimated average) policy judgment of Burns, Volcker, and Greenspan is imposed on the 1920s and 1930s. Overall, Sims finds the outcomes—particularly the Great Depression—would have been little changed by this more responsive postwar policy. The drop in production from 1929 to 1933 is "completely unaffected by the altered monetary policy." Postwar policy would have made the 1920–21 and 1929–33 deflations less severe, but not by much. The upheaval of the 1920s and 1930s would have been the same, even if modern monetary policymakers had been at the reins. Sims notes that his methodology leaves the banking runs, panics, and currency speculations that plagued the Depression era as unexplained non-monetary shocks. To the extent that a persistent commitment to monetary ease would have alleviated such disruptions, the drop in output might have been less severe, he suggests.

The effects of substituting interwar monetary policy into the postwar economy are qualitatively the same. Even though the discount rate responds much more slowly to the postwar economic fluctuations, resulting in a markedly different interest rate pattern, the influence of this altered policy on industrial production and consumer prices is quite small at business cycle frequencies. The implications for output and inflation at longer horizons are what one would expect with a more accommodative policy: Output and inflation both rise higher in the 1970s, resulting in a larger recession in the 1980s, although Sims is careful to point out that these findings may well be statistically unreliable. Overall, he reaches the startling conclusion that "the size and timing of postwar U.S. recessions had little to do with either shocks to monetary policy or its systematic component."

Sims reaches the startling conclusion that "the size and timing of postwar U.S. recessions had little to do with either shocks to monetary policy or its systematic component."

Lawrence Christiano focuses on Sims's surprising conclusion that monetary policy played little or no role in the Great Depression. He disagrees with the methodology that Sims uses to reach this conclusion, but upon employing what he considers a superior method, he confirms Sims's results.

One criticism of Sims's methodology revolves around the assumption that private agents behaved the same in the postwar period after the creation of the Federal Deposit Insurance Corporation (FDIC) as they did during the interwar period prior to the FDIC. Christiano suggests that the frequency with which

interwar depositors converted deposits to currency at the slightest sign of bad news, in contrast to the virtual absence of such bank runs in the postwar period, suggests that the presence of the FDIC fundamentally changed private agents' behavior. In particular, they may have viewed the commitment of Federal Reserve policy to maintain banking system liquidity quite differently in the postwar period, and in a way that cannot be captured by the simple "reaction functions" or interest rate equations in Sims's analysis.

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The more important flaw in Sims's analysis, according to Christiano, is the characterization of the postwar monetary policy rule. Under this rule, after all, the Fed would have *contracted* the money supply by 30 percent in the 1930s. Christiano cannot conceive of a sensible policymaker who would pursue a contractionary monetary policy during a widely recognized, worldwide depression. So Christiano proposes instead to use a monetary policy equation that keeps money (M1) from falling during the episode.

Using this more plausible counterfactual policy in Sims's model for the interwar period, Christiano finds that a stable M1 path for the early 1930s would have prevented the dramatic price declines that actually occurred. Surprisingly, however, even under the more realistic policy response, which implies a more realistic path of money growth, "the basic course of the Great Depression would not have been much different," as shown by the similarity between the path of output in Christiano's simulation and the actual path of output.

Benjamin Friedman is also skeptical of the empirical results developed in Sims's paper, stating: "If the model he presents has succeeded in identifying Federal Reserve actions and measuring their economic effects, these findings should force us to reconsider many aspects of economics and economic policy." Friedman finds troubling Sims's result that postwar monetary policy would not have significantly altered the course of the Great Depression, and he views as even more problematic the finding that Depression-era monetary policy would have worked just the same in the postwar period as did actual policy. Friedman notes that the general price level was approximately the same at the onset of World War II as at the onset of the Civil War, while prices since that time have risen approximately tenfold. That the monetary policy that delivered the interwar *de*flation is the same one that delivered the "historically unprecedented phenomenon of a half century of sustained *in*flation" would make inflation, even over periods of several decades, never and nowhere a monetary phenomenon.

Friedman suggests that Sims's model delivers its surprising results because it fails to adequately identify the Fed's monetary policy actions or the effects of those actions on the macroeconomy. If so, then the model's "implied irrelevance of monetary policy" for the postwar inflation translates further into irrelevance for assessing monetary policy's role in causing or cushioning business cycles. One indication that Sims's postwar policy rule does not accurately represent Fed actions, Friedman argues, is the difference between the Sims model's policy prescriptions for the Depression era and John Taylor's policy rule prescriptions for the same period. Friedman finds that Taylor's rule would imply nominal interest rates "an order of magnitude more negative than what Sims reports," casting some doubt on how well Sims's policy rule reflects all of postwar Fed behavior.

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Finally, Friedman notes that the assumption that Fed policy can be characterized by one unchanging rule over the entire postwar period is implausible. He asks, "Are we really to equate Paul Volcker's tough stance against inflation with the see-no-evil regime of

Arthur Burns?" While Friedman recognizes that Sims tests for a shift in monetary policy in 1979, Sims does so by testing for a shift in all 279 of his model's parameters. Friedman notes that Sims could have more narrowly focused this test to detect only shifts in the parameters that summarize monetary policy.

Financial Markets and Business Cycles: Lessons from Around the World

A panel composed of **Rudiger Dornbusch**, **Maurice Obstfeld**, and **Avinash Persaud** analyzed recent financial market crises, most notably the turmoil in Asia, and drew lessons on how to reduce the likelihood and severity of future crises. Generally speaking, the panelists agreed more on why the crises occurred than on what should be done to prevent future crises.

Dornbusch believes that recent financial crises in Asia, Russia, and Mexico differed from most preceding crises because they centered on capital markets rather than on the balance of payments. Both types of crises often are associated with currency crises as well, but the vulnerability or risk imposed on an economy by a capital market crisis is fundamentally different. He explains that financial systems experiencing a capital market crisis exhibit five characteristics: (1) borrowing short and lending long generates a *mismatching of maturities* between liabilities and assets; (2) borrowing in foreign currency units and lending in domestic currency units generates a *mismatching of denominations*; (3) borrowing to carry assets exposed to large fluctuations in price generates *market risk*; (4) high risk exposure throughout a country generates a *national credit risk*; and (5) the central bank is weakened by *gambling away foreign exchange reserves*.

According to Dornbusch, the capital market crisis in Asia made the regional economy vulnerable, or at risk, to adverse external factors. And two such factors happened. First, "Japan went into the tank." Just as the Japanese economy was starting to show signs of emerging from several years of sluggish growth, the Japanese government tightened fiscal policy and the economy slumped again. This time the weakened economy exposed underlying banking problems that exacerbated the situation so much that the Japanese economy eventually began to contract. Because Japan is the largest economy in the region and the leader in regional export and import markets, the Japanese slump put stress on the foreign trade structure of the entire region, which is characterized by extensive export and import linkages.

A second adverse factor was the sharp depreciation of the yen vis-à-vis the U.S. dollar, "leaving the dollar peggers high and dry." Asian economies that were dependent on robust exports to Japan but had pegged their currencies to the dollar suddenly found their exports priced too high, in yen terms. Export demand fell sharply among Asian trading partners, and almost overnight domestic economies throughout the region began experiencing severe contractions. Together these adverse external factors turned vulnerable economies into collapsing economies. Thus, Dornbusch attributes the Asian economic downturn to a confluence of capital market vulnerability and adverse external factors.

Obstfeld also believes that the primary source of economic vulnerability in recent financial crises was capital markets, but he emphasizes shifts in expectations as the central factor driving the economic fluctuations. He notes that "exogenous fluctuations in capital flows have become a dominant business cycle shock" for developing countries in the modern era, and that similar financial crises were quite common prior to World War II.

Obstfeld describes two main types of crises—exchange rate (currency) crises, and national solvency crises—and explains that although they can occur separately, they often "interact in explosive ways." The main linkage between them is self-fulfilling expectations. An economy with a weak and vulnerable capital market can avoid crisis so long as there is no expectation of one. But when expectations change, the desirable but tenuous equilibrium will give way abruptly to a crisis. A sudden new expectation of currency depreciation can start the process rolling, once speculators perceive the threat that public debt will be paid through inflation. He cites Indonesia as an example of this phenomenon.

In Persaud's view, moral hazard and inadequate oversight were key factors in generating the underlying capital market vulnerability. "Moral hazard [induced by International Monetary Fund bailouts] . . . probably played a role in the exponential rise in foreign bank lending to Emerging Asia," and "crony capitalism" may have further "impaired the proper allocation of resources." Furthermore, Asia's economic success was "unbalanced" in the sense that lending went toward overinvestment that was concentrated in a limited number of sectors. Inadequate supervision and unreliable information about this worsening capital situation allowed the rise in risky lending and overinvestment to go unchecked until it was too late.

Persaud also cites the weakened Japanese economy and depreciating yen as important factors, but he identifies the collapse of the Thai baht on July 2, 1997 as the "trigger" that set off the Asian crisis. The effect of this trigger was amplified as investors suddenly realized new or mispriced risks in the region and greatly reduced their "appetites for risk"; this led to widespread and simultaneous capital outflows from the region.

A key factor contributing to this capital flight, says Persaud, was the sudden discovery that domestic corporate investment positions were highly concentrated. When the crisis emerged, heavyweight investors in the region discovered that their peers were also deeply vested in the same small number of collapsing Asian economies. Thus, these influential investors not only wanted to get out of Asia because of the inherent financial problems, they also wanted to get out first, because they knew that a massive capital outflow would dramatically reduce asset prices in the region.

The panelists generally agreed that unwise economic decisions had promoted an environment of vulnerability, and that Japan's economic weakness and other events turned a precarious situation into turmoil. However, their recommendations about how to respond to the current crisis, and how to prevent future crises, were notably different.

Dornbusch believes that the key to preventing future capital market crises is to control financial risk. He proposes using model-based value-at-risk ratings and disseminating "right thinking" within the international financial community regarding controlling and pricing such risk. Controlling capital flows themselves, however, is not appropriate. He advocates International Monetary Fund (IMF) inspections of financial market conditions during country consultations, but he is doubtful the IMF will become sufficiently forward-looking and preemptive, because IMF member countries will resist such changes. For this reason, he particularly opposes an Asian IMF. Dornbusch advocates moving toward regional currencies like the euro. Regarding the appropriate response to current developments, Dornbusch is adamant that tight money policies are required to restore financial stability; debt restructuring can be negotiated later. Fiscal policy is not a viable tool because of the fiscal deterioration associated with the recent crises.

Obstfeld asserts that "policy must counteract the severe capital-account shocks by creating a new expectational climate" that will restore confidence in these economies. He sees no economic prescription for this change "short of infeasibly extensive official financial support from abroad." In contrast to Dornbusch, Obstfeld concludes that fiscal expansion is the least risky policy prescription, particularly in Japan. Monetary expansion in Japan might also help, but it carries the risk of further yen devaluation and is insufficient until Japan resolves its banking problems. He ends by warning that monetary tightening now by the Federal Reserve and the new European Central Bank to fight domestic inflation "would be an error of perhaps historic proportions."

Persaud highlights the need to develop policies that "work with financial markets and not against

Panel Comments

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Persaud wants an international financial system that permits countries access to an international pool of foreign exchange reserves if they meet certain "selectivity criteria" intended to reflect sound and prudent financial operations.

them." He views many actual and proposed policies as counterproductive. Capital controls intended to curb outflows would implicitly curb much-needed inflows. Looking to the IMF for faster and more lucrative assistance is also unwise. He doubts that the IMF loans can keep pace with the magnitude of required private capital flows, and in any case further IMF assistance worsens the moral hazard problem.

Instead, Persaud wants an international financial system that permits countries access to an international pool of foreign exchange reserves if—and only if—they meet certain "selectivity criteria" intended to reflect sound and prudent financial operations. The criteria, which must be "public, clear, and transparent," would consider the extent of external debt, the productivity of capital inflows, the competitiveness of exchange rates, the soundness of government finances, and the openness of governance. Countries or financial institutions that do not meet these criteria should be allowed to fail. Indeed, Persaud believes that selective assistance is a critical requirement for eliminating moral hazard.

Production, Technology, and Business Cycles

Susanto Basu tackles another of the most contentious questions among modern macroeconomists: Do fluctuations in technological change or productivity growth actually cause business cycle fluctuations? Some prominent neoclassical macroeconomists assert not only that the answer is yes, but that technology change is the *primary* determinant of such fluctuations. This assertion is contested by macroeconomists like Basu who adhere to the Keynesian tradition of emphasizing fluctuations in aggregate demand as the primary contributor to business cycles. Because these two views of the sources of business cycles lead to radically different macroeconomic models and prescriptions for government policy, resolution of this debate is critical.

Basu argues that neoclassical economists have misinterpreted the link between technological change and business cycles by misusing the standard measure of technological change: the Solow residual, named after M.I.T. economist Robert Solow. Solow's methodology is simple: measure the growth of output; subtract the appropriately weighted growth of all observable inputs such as labor, capital, and materials; and the difference, or residual, is an estimate of unobserved technological change. Economists use this sensible but indirect measure because they do not have direct data measures of technological change.

Thus far, most attempts to construct Solow residuals with conventional data on inputs yield a measure that is positively correlated with output, giving rise to the claim that technological changes cause business cycles. But Basu argues the Solow residual was only intended to estimate the long-run impact of technology on the economy, not the cyclical impact. He notes that Solow warned long ago that his measure would be spuriously correlated with output and the business cycle because firms adjust to fluctuations in demand by varying the rates at which they utilize capital and labor.

Basu concludes that the defining cyclical feature of technological change is a short-run reduction in inputs and factor utilization, and that business cycle models face the challenge of reproducing that feature.

Basu has developed a new measure of technological change that adjusts for features that could lead to an excessively positive correlation between technological change and output. Basu's methodology, developed in earlier research with John Fernald and Miles Kimball (henceforth the BFK technology measure), adjusts for four factors: (1) variable utilization of capital and labor; (2) variable worker effort; (3) imperfect competition and other special advantages firms may have in production; and (4) different characteristics of firms across industries. In other words, it adjusts for many of the demand-side features Solow was concerned about. The BFK methodology requires relatively few controversial restrictions or assumptions; indeed, previous measures of technological change are special cases of it.

The salient and distinguishing feature of the new BFK technology measure is that it is essentially uncorrelated with output and the business cycle. Unlike the Solow residual, which is positively correlated with output and the business cycle, it exhibits no simple statistical evidence of causing business cycle fluctuations. Moreover, the BFK measure is much less vari-

able than the Solow residual. Together, these features reduce, if not eliminate, the likelihood that unexpected technological changes cause business cycles. Basu shows that this conclusion holds up in simple statistical models of the production process.

Another potentially important characteristic exhibited by the BFK technology measure is that it suggests what all workers fear: that technological improvements reduce employment. At least initially, the BFK measure is very negatively correlated with factor inputs, such as labor and factor utilization. In other words, when firms improve their technical efficiency by installing the latest and greatest machines, they are able to produce the same output with fewer inputs, so they reduce costs by cutting their work force rather than reducing their prices and producing more. Only much later, as profits rise, do they expand their output and hire workers. This interpretation of the data stands in stark contrast to interpretations based on the conventional Solow residual, in which employment and other factor inputs rise with technological improvements.

In the second part of his investigation, Basu uses his technology measure to evaluate whether the dynamic properties of two state-of-the-art macroeconomic models match the postwar data. One is the real business cycle (RBC) model, which features technological change as the main source of business cycle fluctuations. It also assumes complete, competitive markets with fully adjustable prices. The other model is basically similar but introduces slowly adjusting or "sticky" prices. Sticky prices are a common feature of macroeconomic models that emphasize fluctuations in aggregate demand as the main source of business cycles.

The result of Basu's evaluation is quite discouraging for state-of-the-art macroeconomic models. He finds that neither the RBC nor the sticky price model generally fits the data very well. The RBC model, in particular, does not match the dynamic properties of the data, and it cannot reproduce the essentially zero correlation that exists between the BFK technological change and output or the negative correlation between factor inputs and output. These models also fail to reflect the generally sluggish response of output changes in the economy. Basu reports that the sticky price model is qualitatively better because it approximately reproduces these two correlations, although it does not do so well. The prognosis for these models becomes even bleaker when he evaluates the models with both technological change and various specifications of monetary policy.

Basu concludes that the defining cyclical feature of technological change is a short-run reduction in inputs and factor utilization, and that business cycle models face the challenge of reproducing that feature. At present, standard RBC and sticky price models cannot do the job, and variable factor utilization does not impart enough rigidity to generate sufficient sluggishness. He projects that the sticky-price models, modified to include other sources of rigidities, "show some promise of being able to match the data, but clearly have a long way to go."

Mark Bils questions whether Basu's technology measure adjusts *too much* for the positive correlation between factor utilization and output. He hypothesizes that the proportions of capital and labor used in production are likely to be fixed in the very short run. Thus, when capital utilization rises slightly, labor hours will rise in equal proportion. If so, total factor productivity should be positively correlated with output but labor productivity should be approximately uncorrelated with output. Bils finds exactly these correlations in data on detailed manufacturing industries. Because the BFK methodology infers movements in capital utilization from movements in materials prices, and because materials prices are more positively correlated with output than labor costs, Bils believes the BFK measure makes capital utilization more positively correlated with output than labor utilization is.

Bils questions whether Basu's technology measure adjusts too much for the positive correlation between factor utilization and output, and he questions the plausibility of price stickiness.

Other aspects of Basu's methodology make Bils skeptical of the results. He doubts that labor quality (effort) is positively correlated with output, as in the BFK measure, because there is evidence that workers hired during expansions are paid less and therefore of lower quality. Moreover, he thinks the relationship between effort and hours will vary depending on the stickiness of wages and the type of shock. Bils also argues that factor utilization will vary more if shocks are transitory rather than permanent. Basu's

methodology relies more on variables associated with transitory shocks, so it may yield estimates of utilization that are too positively correlated with output.

Finally, Bils assesses the plausibility of price stickiness in two empirical exercises. One exercise is based on the theory that if prices are sticky, then firms with significant inventory holdings should be less likely to reduce inputs and output when technology increases, because they can inventory unsold output. He reports evidence that "labor hours are much less likely to decline for industries that hold significant inventories," but points out that this evidence does not conclusively determine the actual flexibility of prices. So in a second exercise he provides more direct evidence from models of relative prices. Prices are significantly negatively correlated with current total factor and labor productivity but not with past productivity, a relation Bils interprets as evidence that prices are not sticky.

Thomas Cooley is also cautious about interpreting Basu's results as evidence against the idea that technological change is an important source of business cycle fluctuations. Like Bils, Cooley has reservations about the methodology underlying the BFK technology measure, although he embraces Basu's finding that firms do not enjoy market power from technological advantages in production. In particular, he notes that the correlation of the BFK technology measure with output is sensitive to the exact form of the econometric methodology used to construct the measure and to the identifying assumptions of the modeling framework.

However, granting the validity of Basu's results, Cooley directs his critique at the logic of Basu's inferences about the implications for macroeconomic models. First, he questions Basu's conclusion that the results necessarily rule out RBC-type models. He argues that RBC models no longer rely on artificially sluggish technology shocks to obtain sluggish output responses. Sluggishness can arise from factor utilization as well as financial market imperfections, differences among firms, and other features. As for the RBC model's inability to generate a negative correlation between technology and factor inputs, he suspects that this result is not robust.

Cooley also questions whether the evidence should lead one to conclude that prices are sticky. Basu provides no direct evidence of sticky prices, and economic theory does not make clear predictions about the direction in which capital and labor should respond to technology changes. The response will depend, among other things, on the nature of the technology change, market structure, and the sensitivity of demand to prices. This point calls into question Basu's assertion that he does not need to consider the behavior of profits and product markets.

Cooley thinks Basu's results suggest that technological change is embodied in new capital investment—a characteristic absent from the BFK methodology. With technology embodied in capital, the short-run responses of output and factor inputs to technological change are different from those of a standard RBC model and are capable of yielding the patterns Basu finds in the data. Moreover, in this case the nature of depreciation matters for interpreting the effects of cyclical factor utilization.

Cooley is also cautious about interpreting Basu's results as evidence against the idea that technological change is an important source of business cycle fluctuations. He thinks they suggest that technological change is embodied in new capital investment.

Reallocation, Restructuring, and Business Cycles

Scott Schuh and **Robert Triest** investigate the idea that business cycles might be caused by the shuffling of jobs as firms restructure the way they do business. New data produced during the past decade show that firms are continuously changing. Some expand and create jobs while others contract and destroy jobs. The pace of change is rapid; one in 10 jobs is newly created and one in 10 jobs newly destroyed in manufacturing each year. The sources of these ups and downs of particular firms include product demand and innovation, prices and wages, regional economic conditions, technological change, and other factors idiosyncratic to each firm, rather than factors common across all firms. Job creation and destruction together represent job reallocation, a mea-

sure of job turnover or churning in the economy.

Traditionally, macroeconomists looking at the labor market have ignored job reallocation and have focused solely on total employment growth (or the total unemployment rate). However, Schuh and Triest point out that a given rate of employment growth can occur with either low or high rates of job reallocation. More important, the intensity of job reallocation has significant consequences for unemployment, wage growth, and productivity growth.

For example, if changes alter the desired distribution of jobs across firms, industries, and regions, job reallocation must intensify to keep productive efficiency high. More intense reallocation usually means higher job destruction that forces many workers into unemployment. These unemployed workers lose any skills they had that were unique to their previous job (such as knowledge of firm operating procedures), have a hard time finding a comparable new job, and stay unemployed longer. Eventually they may have to accept a job entailing sizable reductions in their wages. Such issues are linked inherently to the determination of aggregate unemployment, wage growth, and productivity.

Schuh and Triest investigate the idea that business cycles might be caused by the shuffling of jobs as firms restructure the way they do business.

Schuh and Triest point out that job reallocation and the pace of restructuring rise markedly during recessions. Traditional macroeconomic models cannot explain why because they do not incorporate the phenomenon of job reallocation. But in light of the potentially negative economic consequences of job reallocation, it is important to know whether an identifiable connection exists between reallocation and business cycles, and whether the correlation between them is of no consequence and can continue to be ignored.

Schuh and Triest ask the following fundamental question: Does job reallocation cause business cycles, or do business cycles cause job reallocation? Evidence on job reallocation has sparked an interest in building theoretical models capable of explaining the observed patterns in the data, and they classify these theories into two types. One type stresses the role of factors that primarily determine the desired allocation of economic resources, such as workers, across firms. The other type stresses the role of aggregate factors, such as monetary policy, that primarily determine the overall level of economic activity. Both types of theories aim to explain why job reallocation rises during recessions. Yet both types of theories tend to rely on vaguely defined aggregate and allocative "shocks" rather than observable variables.

Schuh and Triest argue that these theories do not and cannot answer their fundamental question, for two reasons. First, although the two-way classification of factors may be conceptually sensible, in practice the definitions of allocative and aggregate factors become hopelessly muddled. Second, these theories have little to say about what *causes* business cycles—that is, *why* they occur—because they focus more on *how* they occur.

Schuh and Triest present results from three empirical exercises that extend research by Schuh with Steven Davis and John Haltiwanger on job creation, destruction, and reallocation (henceforth referred to as DHS). One exercise analyzes the behavior of job reallocation during the 1990s using newly available data. A second exercise attempts to learn what kinds of plants destroy and reallocate jobs and how, in hope of discovering clues about the causes of recessions. The third exercise looks for evidence of causal relationships between job reallocation, the fundamental determinants of reallocation, and the business cycle. Each of these exercises uses data from the U.S. Bureau of the Census on individual manufacturing plants (the Longitudinal Research Database (LRD)).

The new data show that the 1990–91 recession was much less severe in manufacturing than preceding recessions, as evidenced by a relatively modest decline in employment. Nevertheless, job destruction and job reallocation both increased in a manner similar to that in previous recessions. The ensuing expansion was unusual in that job destruction and reallocation remained above average, rather than declining quickly after the recession. In addition, job creation experienced two large surges that were *not* preceded by surges in job destruction, as creation surges typically are. The authors interpret these surges as evidence of favorable allocative shocks, in contrast to the unfavorable allocative shocks of the 1970s and 1980s.

Regarding the nature of job creation and destruction, Schuh and Triest take a deeper look at two areas: (1) the magnitude, permanence, concentration, and cyclicality of job flows; and (2) the differences in job

flows between larger, older, and higher-wage plants (henceforth, simply "large") and smaller, younger, lower-wage plants (henceforth, simply "small"). Previous DHS research concluded that job flows are large, permanent, and concentrated in a minority of plants with large employment changes. Also, large plants account for most of the increases in job destruction and reallocation during recessions. Together these DHS findings suggest that during recessions only a small fraction of really large plants experience really large and permanent rates of job destruction, and thus they imply that the cause of job destruction and recessions is related to large plants.

The Schuh and Triest findings significantly refine this DHS view. They find that small plants tend to have much higher rates of job creation and destruction than large plants, and that high rates of job creation and destruction—especially plant start-ups and shutdowns—are much more likely to be permanent. Thus, even though large plants account for most of the increase in job destruction during recessions, these large-plant job destruction rates are likely to be much smaller in percentage terms and less permanent. In fact, Schuh and Triest find that almost one-half of all jobs destroyed by plants experiencing relatively mild contractions are ultimately restored within five years. In other words, all plants are adversely affected by recessions but large plants appear to be more resilient than small plants, which expand and contract more dramatically and permanently.

Finally, Schuh and Triest uncover some evidence that suggests allocative factors cause business cycles. Their evidence is based on the premise that there are observable determinants of the allocation of jobs across firms, industries, and regions—prices, productivity, and investment—and that changes in those determinants cause job reallocation to increase, which in turn causes recessions. One key finding is that when relative prices and productivity growth across detailed industries change dramatically, job destruction and job reallocation also increase dramatically shortly afterward. Another key finding is that increases in job reallocation generally are *not* associated with increases in trend productivity and investment growth, as some recent theoretical models seem to imply.

Ricardo Caballero regards some of the Schuh-Triest results as "potentially promising," but he challenges two fundamental tenets. He questions the central premise that job reallocation is countercyclical, and he doubts that reallocation shocks actually cause fluctuations. In addition, he objects to the authors' characterization and testing of theories of job reallocation.

Caballero contends that the term "job reallocation" is a misnomer. He does not dispute the fact that Schuh and Triest's measure of job reallocation is countercyclical. However, he argues that the main feature of job reallocation over time is a significant fluctuation in total job destruction that is unconnected with the process of total job creation. Thus while individual jobs are destroyed and created at the plant level, thereby generating worker reallocation, it is what he calls a "dynamic fallacy of composition" to infer that a link exists between total job destruction and creation that could be characterized as total job "reallocation." Put another way, job "reallocation" would be higher if job destruction rose now and fell later while job creation stayed constant, but it would not be true in this case that job losers were reallocated to new jobs.

Caballero questions the central premise of Schuh and Triest that job reallocation is countercyclical, and he doubts that reallocation shocks actually cause fluctuations.

Caballero cites evidence from his own research that the surge in total job destruction during recessions is more than offset by a decline in destruction during the subsequent expansion. He calls this latter effect "chill," where job destruction falls below the rate associated with the "normal" underlying level of job turnover in the economy. He argues that it is important to understand that this chill can arise from market imperfections and produce technological sclerosis as a result of insufficient turnover. This argument contrasts with theories earlier this century that suggested that all job turnover is healthy for the economy.

Caballero believes "it is a large leap to claim that reallocation shocks are a substantial *source* of business cycles, at least in the United States," although he thinks they might be important elsewhere such as Eastern Europe, for example. He argues that plausible statistical models show that reallocation shocks are "substantially" less important than aggregate shocks, at least for net employment growth. He also demonstrates that such models can produce confusion about the relative importance of job reallocation, and asks whether the "fragile decomposition" of shocks as

aggregate versus allocative is worthwhile, compared to focusing on observable shocks such as prices or interest rates.

In general, Caballero thinks it is a mistake at this point to focus on trying to discover whether or not reallocation shocks cause business cycles. Instead, effort should be directed toward the less debatable issue of whether "the churn [ongoing processes of creation and destruction] has a significant effect on the economy at *business cycle* frequencies."

Steven Davis shares the ambition of Schuh and Triest to develop new evidence on the connection between job reallocation and the business cycle. Indeed, he devotes a significant portion of his comments to explaining why this endeavor is important. But Davis, too, challenges the claim that reallocation activity is countercyclical, and he argues further that total job reallocation is inappropriate for this analysis. He also suggests a more effective methodology for summarizing the relationship between job flows and plant characteristics.

Davis argues that the amount of job reallocation in excess of the change in total employment is a more suitable measure of reallocation intensity, and he reports that excess job reallocation is uncorrelated with the business cycle.

Davis provides a detailed description of the dynamic nature of job and worker flows and then advances several reasons why it is important to study these flows. First, "the extent to which the reallocation and matching process operates smoothly determines . . . the difference between successful and unsuccessful economic performance," with European unemployment serving as a prime example. Second, successful conduct of policy requires accounting for the reallocation and matching process. Third, recent modeling of reallocation frictions and heterogeneity makes it evident that aggregate shocks have allocative consequences, and shocks to factor demand can drive fluctuations in economic aggregates. Fourth, "models with reallocation frictions also help to address some well-recognized shortcomings in prevailing theories of the business cycle."

Davis believes that Schuh and Triest err in treating gross job reallocation "as equivalent to the intensity of reallocation activity." His criticism is that gross job reallocation does not account for the fact that movements in job creation and destruction merely may be achieving changes in total employment instead of reflecting a fundamental reallocation of labor across plants. Davis argues that the amount of job reallocation in excess of the change in total employment is a more suitable measure of reallocation intensity. He reports evidence that, unlike total job reallocation, excess job reallocation is uncorrelated with the business cycle.

Policy Implications

In the closing session, leading economists from the public and private sectors discussed the implications for government policies of the conference's analysis of the causes of recessions. Panelists focused especially on the important role of vulnerability in setting the stage for unanticipated or adverse events. Each argued that governments should implement policies to reduce the economy's vulnerability and exposure to risk, provide more and accurate information to private agents about the extent of risk, and—if necessary—aid the recovery of economies that plunge into crises.

Henry Kaufman believes that sweeping structural changes to financial markets in recent years have significantly altered the linkages between financial markets and the real economy. Among the developments he identifies are securitization, derivatives, globalization, and leveraged investing. Several themes pervade his analysis. First, global financial markets are becoming increasingly sophisticated and complete. Second, this maturation process increasingly makes financing available to borrowers who would not have been able to obtain it previously. Third, and a consequence of the first two points, financial markets are becoming increasingly volatile, as risk-taking becomes easier while accurate risk assessment becomes more difficult. Altogether, these changes increase the likelihood that financial market turbulence will make economies more vulnerable to shocks and recessions.

Kaufman believes the changes increase the difficulty and reduce the efficacy of monetary policy. Monetary policy is more difficult because traditional monetary factors—monetary aggregates, debt aggregates, and the like—have become less reliable indica-

tors of the stance of monetary policy and the state of money markets. Monetary policy is less effective because increased availability and easier acquisition of credit mean that short-term interest rates must increase more to achieve the same real response. Furthermore, increased volatility in asset prices (wealth) leads to greater volatility in aggregate economic behavior. Thus, he argues, the Federal Reserve should take asset price developments explicitly into account in formulating monetary policy.

Internationally, Kaufman sees a need for increased supervision of financial markets. Paradoxically, he notes, when financial markets become deregulated and "freewheeling," the need for more accurate, timely, and complete information increases, particularly about the risks in which financial entities are engaging. He decries the poor job of oversight and information gathering done by official institutions thus far and proposes several reforms. In particular, he recommends a new body he calls a Board of Overseers of Major Institutions and Markets, which would set a code of conduct, supervise risk-taking, and harmonize capital requirements.

Kaufman also favors reforms to two international economic organizations. First, the IMF should be reorganized to specialize in a narrower set of core functions. The new IMF would continue to facilitate lending to countries in financial distress and to press for reform in government policies in these countries. But it would also be charged with rating the creditworthiness of countries, by assessing economic and financial conditions, reviewing extant government policies, and demanding remedial action where needed. Kaufman also argues that the G-7 must be restructured to account for the European Monetary Union and its euro currency.

Martin Zimmerman provides perspective from one of the largest and most cyclical components of the U.S. economy: the automobile industry. He explains how the auto industry, specifically Ford Motor Company, views the unfolding of a recession—how consumers postpone their car purchases, how auto makers respond to weakening sales, and how interest rate policy is an important determinant of the economic fortunes of the auto industry. But ultimately he argues against the central theme of the conference. That is, Zimmerman believes it is impossible to go "Beyond Shocks."

The economy is always subject to shocks, according to Zimmerman. For the auto industry, a shock is anything that causes consumers to suddenly alter their normal plans to purchase new cars. Zimmerman tells the story of how the 1990–91 recession unfolded. As late as June 1990, economic forecasters were predicting confidently that there would be no recession, only a slowdown. But Iraq's invasion of Kuwait and the U.S. military response caused a precipitous drop in consumer confidence and sales of cars to consumers. The shock of the Kuwait invasion, like all shocks, by definition was not forecastable, says Zimmerman (an assessment that was not well-received by his employers, he adds wryly).

Although shocks are pervasive, the central question is whether the shocks will tip the economy over into recession. Here, he asserts that not all shocks do, in fact, trigger recessions. The economy must already be vulnerable when the shocks hit. Absent this vulnerability, the economy may be able to withstand shocks. Likewise, absent shocks, vulnerability may never result in a recession.

What is the role of policy in a world of vulnerability and inevitable shocks? Zimmerman notes that every precipitous drop in auto sales has been associated with an increase in interest rates, so he tends to associate monetary tightening with the emergence of economic vulnerability (weak growth). But because not every increase in interest rates was followed by a recession, he surmises that a shock is required to turn vulnerability into recession. He asserts that monetary policy cannot prevent shocks because they are inherently unpredictable. Instead, policy should minimize vulnerability of the economy.

Agustin Carstens contributes a view of recessions and policy from the perspective of emerging economies such as Mexico. He identifies five characteristics of business cycles in emerging economies that distinguish them from business cycles in industrialized economies. First, business cycles in emerging countries are closely synchronized with the fortunes of industrialized countries: "When the United States gets a cold, Mexico gets pneumonia." Second, business cycles are more volatile in emerging economies. Third, emerging economies are susceptible to additional sources of volatility, such as terms of trade fluctuations. Fourth, and more recently, increasing globalization of markets has encouraged massive capitals flows into emerging countries like Mexico. But these capital flows are very unstable, so emerging countries can experience sudden and massive capital outflows that devastate their economies. Finally, emerging economies have to deal with exchange-rate regimes and their failures.

These characteristics force emerging economies to adopt very different policies to deal with business

cycles. Industrialized countries, as leaders of the world economic engine, follow policies designed to manage aggregate demand so as to achieve low inflation and full employment. Such policies are countercyclical. In contrast, emerging countries follow policies designed to avoid or mitigate economic crises that break out there, often because industrialized countries are slumping and reducing their demand for emerging country exports. One essential goal of these policies is to reestablish the credibility of emerging economies, especially the credibility of their currencies and financial markets. Often this means reestablishing the credibility of governments that have made bad policy decisions. These types of policies, then, are usually procyclical.

Carstens offers four specific policy recommendations for emerging economies to help them to reduce vulnerability and follow a more stable path. First, they must reduce their vulnerability to changes in the international prices of exports, by adopting more open trade and investment regimes. Second, they should allow market determination of interest and exchange rates so these rates can accomplish their purpose of absorbing shocks. Third, they must ensure the robustness of their financial institutions to macroeconomic fluctuations. Fourth, they should push forward with structural changes in order to achieve central bank autonomy, privatization of production, labor market flexibility, and reduced dependence on foreign saving. In each case, more complex policies are required beyond the traditional demand management schemes followed by industrialized countries, Carstens notes.

Michael Mussa, as a leading official at the International Monetary Fund, offered an informed, practical—and oftentimes contrarian—view of the conference papers, the conventional wisdom about the ongoing global economic crises, and recent criticisms of international policy responses to the crises.

Mussa infers from Sims's paper that systematic monetary policy *does* have a significant, positive effect on the real economy, despite Sims's claim to the contrary. He says Sims understates the effect of monetary policy, citing Sims's own results showing that industrial output would have been nearly one-fifth higher if the Fed had followed modern monetary policies during the Great Depression. He also points out that Sims omits the positive role monetary policy can play in avoiding banking and financial panics by subsidizing and reforming weak banks, and by reassuring depositors that their accounts were safe. Had Sims accounted for this, and for the fact that fiscal policy should have been more aggressive, one-half to three-quarters of the impact of the Great Depression could have been avoided.

Mussa finds the two long historical analyses of business cycles to be inherently valuable. He particularly agrees with Temin's premise that recessions

Panel Comments

Kaufman believes that sweeping structural changes to financial markets in recent years have significantly altered the linkages between financial markets and the real economy, increasing the difficulty and reducing the efficacy of monetary policy.

Zimmerman asserts that monetary policy cannot prevent shocks because they are inherently unpredictable. Instead, policy should minimize vulnerability of the economy.

Carstens points out that emerging economies often are forced to adopt policies to deal with business cycles that are very different from those of industrialized countries. Often the goal is to reestablish the credibility of emerging economies, especially their currencies and financial markets, and these policies are usually procyclical.

Mussa warns that to say that monetary policy has been "as good as it gets" implies that it is better than it is normally expected to be. Ultimately, the monetary authority cannot avoid all recessions; it can only be expected to avoid "big" ones.

"have a multiplicity of causes," although he doubts that it is possible—or useful—to try to quantitatively separate causes into different categories of influence. Like Romer, Mussa believes that Temin underestimates the contribution of monetary policy to recessions. However, Mussa is cautious about the quality of older economic data and what we can reliably infer from them, particularly data for countries other than the United States.

Regarding the paper by Schuh and Triest on labor reallocation and business cycles, Mussa is "skeptical that labor reallocation is itself an independent cause of most U.S. business cycles." He suggests that the authors focus more on the relationship between labor reallocation and the NAIRU (non-accelerating-inflation rate of unemployment). Regarding the central issue addressed in Basu's paper, Mussa believes that "the notion that adverse downward movements in total technology cause recessions [because workers don't work as hard] is just plain silly. This is the theory according to which the 1930s should be known not as the Great Depression but as the Great Vacation."

Mussa then turned to a discussion of current economic developments and the appropriateness of policy. On the domestic economy, Mussa likens recent monetary policy performance to the movie, "As Good As It Gets." Aside from some minor quibbles, Mussa judges U.S. monetary policy management during the last decade to be "remarkable" by any standard. But he notes that it has been "very good management with very good luck." Moreover, he warns, to say that monetary policy has been as good as it gets implies that monetary policy is better than it is normally expected to be—in other words, it is likely to get worse, not better. Ultimately, the monetary authority cannot avoid all recessions; it can only be expected to avoid "big" ones.

On the international situation, Mussa likens catastrophic economic events such as the Great Depression and the current worldwide financial crisis to the movie "Titanic." What caused the Titanic to sink, he asks? Perhaps an exogenous shock (the iceberg), he quips. But it was more than that. Errors in the design and operation of the ship, inadequate preparation for the sinking, and other factors all contributed. In the same way, the current financial crisis has many complex causes and contributing factors.

However, reasons Mussa, the *real* tragedy of the Titanic was not that it sank and 1,500 lives were lost, but that *800 of the Titanic passengers were saved that day*! Clearly this policy mistake discouraged shipbuilders from spending money on improving designs and shipping lines from bearing the cost of conducting safe navigation of future cruises across the Atlantic. The Titanic rescue demonstrated that entrepreneurs in the shipping industry didn't need to worry about safety—they knew that the government would be there to save them from their imprudence!

Mussa employs this tongue-in-cheek argumentation to rebut those who argue that moral hazard problems should prevent the international community from responding to the current financial crisis. Despite moral hazard problems, saving 800 Titantic passengers *was* the right thing to do. And despite clear moral hazard problems, Mussa says the IMF attempts to rescue Korea and other besieged economies *is* the right thing to do. He argues that IMF support is not a gift but a loan, and that the IMF's earlier financial support of Mexico has been validated by Mexico's successful servicing of IMF debt.

Conclusion

In the end, most participants agreed that the business cycle is *not* dead but is likely here to stay. No one championed the ideas that a "new," recession-proof economy has emerged, that unanticipated adverse economic events have stopped buffeting the economy, or that government policy has become so adroit that it can offset every dip in the aggregate economy. If anything, the mere mention of these ideas drew disdainful remarks, and even served as "proof" that the ideas were without merit. Indeed, the general premise among participants was that the right question was *when*, not *if*, the next recession occurs, what will have caused it? The consensus answer is it is likely to be not one but many things, with government policy and vulnerability playing important—but still not fully understood—roles.

Most participants also agreed that policymakers in a world continually subject to business cycles should adopt certain goals to improve their ability to deal with fluctuations. First, policymakers must learn how to recognize and address the economy's vulnerability to disruptions and unanticipated events. Second, policy institutions should conduct and support research that shows the contribution of deliberate actions of economic agents to economic fluctuations. Finally, and most important, policymakers should understand that they cannot prevent every recession, but they should concentrate their efforts on averting The Big Ones, such as the Great Depression.

Beyond Shocks: What Causes Business Cycles?

At its forty-second economic conference in June 1998, the Federal Reserve Bank of Boston brought together national and international policymakers, financial market participants, academics, economists, bankers, and businessmen to consider what makes economies rise and fall. The purpose of the conference was to develop a better understanding of the causes of business cycles in general and recessions in particular, and to assess the inevitability of future fluctuations. A key motivation for investigating the causes of business cycles and recessions was to evaluate the recent and widespread hypothesis that business cycle fluctuations have been permanently dampened. The conference agenda is outlined below.

Opening Address: *History of Thought on the Origins of Business Cycles*
Paul A. Samuelson, Massachusetts Institute of Technology

Historical Evidence on Business Cycles:
The U.S. Experience
Peter Temin, Massachusetts Institute of Technology
Discussant: Christina D. Romer, University of California, Berkeley

Business Cycles Abroad
Michael D. Bordo, Rutgers University
Lars Jonung, Stockholm School of Economics
Michael Bergman, Lund University, Sweden
Discussant: Richard N. Cooper, Harvard University

Government Policy and Business Cycles
Christopher A. Sims, Yale University
Discussants: Lawrence J. Christiano, Northwestern University
Benjamin M. Friedman, Harvard University

Panel: *Financial Markets and Business Cycles: Lessons from Around the World*
Rudiger Dornbusch, Massachusetts Institute of Technology
Maurice Obstfeld, University of California, Berkeley
Avinash Persaud, J. P. Morgan Co., Inc.

Production, Technology, and Business Cycles
Susanto Basu, University of Michigan
Discussants: Mark Bils, University of Rochester
Thomas F. Cooley, University of Rochester

Reallocation, Restructuring, and Business Cycles
Scott Schuh and Robert K. Triest, Federal Reserve Bank of Boston
Discussants: Ricardo J. Caballero, Massachusetts Institute of Technology
Steven J. Davis, University of Chicago

Panel: *Policy Implications*
Agustin G. Carstens, Central Bank of Mexico
Henry Kaufman, Henry Kaufman & Company, Inc.
Michael Mussa, International Monetary Fund
Martin B. Zimmerman, Ford Motor Company

The proceedings, Conference Series No. 42, will be published at the end of the year. Information about ordering this volume will be included in the next issue of this *Review* and will also be available on the Bank's website at http://www.bos.frb.org.