

Editors' Summary

THE BROOKINGS PANEL on Economic Activity celebrated its twenty-fifth anniversary with its fifty-ninth conference, held in Washington, D.C., on April 6 and 7, 1995. This issue of *Brookings Papers on Economic Activity* includes the articles and discussions presented at that conference. The first article analyzes trade liberalization across countries to trace the effects of increasing global integration on economic performance. The second examines the experience of floating exchange rate regimes in the aftermath of the Bretton Woods system. The third examines the role of permanent job loss in labor market dynamics. The fourth assesses the field of growth theory. And the fifth article considers the causes and implications of the late-twentieth-century globalization of the world economy.

THE PAST TEN years have witnessed a remarkable harmonization of the national economies of the world. For many countries the collapse of communism in 1989 was a critical element in this process. But the change that has occurred goes well beyond what can be explained by that historic development. It includes market reforms in the developing third world as well as the transformation of former communist states, the development of compatible legal codes, ownership patterns, and regulatory arrangements in these countries, and progress toward their integration into a global economic system based on open trade and free capital movements. In the first paper of this volume, Jeffrey Sachs and Andrew Warner document the extent of reform and global integration and assess its effects on economic growth in the reforming countries.

Sachs and Warner put recent developments in historical perspective with a review of changes in the world economy since the industrial revolution. After about 1870, a global market system developed, with Western Europe and the United States as the main industrial powers. Russia and the rest of Europe participated as they industrialized, Japan opened to the world economy, and Latin America settled into market-based, export-led growth based on raw materials. Trade barriers remained low among these economies until the outbreak of the First World War.

As in recent times, the interaction of technology and economic insti-

tutions facilitated the emergence of the global economy. Transport costs fell with the opening of the Suez Canal and the development of steamships and railways, and communication was revolutionized with the spread of telegraph lines and transoceanic cables. Basic economic codes governing property and commerce were widely adopted, and almost all countries enjoyed currency convertibility under the gold or silver standards. Recent economic studies have shown that the open international economy was crucial in promoting economic growth in the developing world of that day.

Sachs and Warner document how World War I and its aftermath destroyed the open global economic system. Britain's financial leadership was eroded and was not replaced, and the German economy was impoverished. The Russian revolution and the emergence of fascist states in Italy and Germany drastically changed the political and economic landscape of Europe and influenced political developments in other parts of the world, especially in Latin America where several governments committed to free trade were replaced by inward-looking regimes. Economic hardship in Europe and the Great Depression of the 1930s spurred these upheavals and encouraged political leaders to experiment with alternatives to market-based economies. They also brought intense protectionism in Europe and the United States.

At the end of World War II, there was essentially no international trade, no international capital market, and only the United States, among major industrial nations, had a convertible currency. The major non-socialist economies gradually expanded trade during the 1950s and, after 1958, restored convertibility. But most of the world's population lived in countries which followed fundamentally nonmarket strategies for development—either socialist economies or economies relying on state-led industrialization. The authors provide an extended discussion of the political, economic, and intellectual forces behind experiments with state-led industrialization, and analyze the developments that eventually led to their abandonment in favor of liberalizing economic reforms. Such reforms were generally pervasive, involving privatization, deregulation, and price liberalization along with the opening of trade. But because it is vital to global integration and is so strongly correlated with these other changes, the authors focus on trade liberalization as the litmus test of the overall reform process.

Sachs and Warner categorize developing countries according to the

timing of trade liberalization—defined by the absence of certain characteristic impediments to open trade. Using this timing in a logit model they find two factors that encouraged early liberalization: a higher labor-to-land ratio, presumably because it made governments more responsive to labor than to landowners; and a higher per capita income, presumably because it implied a higher initial level of division of labor and degree of specialization. They also find that postcolonial countries were less likely to liberalize than were countries that had long been independent, a difference they attribute to the “imperatives of nation-building” in the postcolonial countries.

Over the postwar period, there has been little convergence in income levels of the rich and poor countries of the world. The authors review three main explanations that have been offered by other analysts for this lack of convergence. First, technological leadership, because it provides externalities to the workforce, helps the rich grow richer. Second, poor countries lack the educated labor force—human capital—necessary for using modern technology. Third, the long-term potential income of currently poor countries is low, so that while they may grow relative to that potential, their income will never reach that of the rich countries. The first two explanations are profoundly pessimistic for the prospects of poor countries. So is the third, unless long-term potential depends on policies that can be improved. The authors are even more optimistic, arguing that a policy of trade liberalization does, in fact, make a crucial difference in economic growth.

Sachs and Warner reach this conclusion by analyzing how openness relates to economic performance. Defining as open only those developing economies that fulfill their criteria throughout the 1970s and 1980s, they find that the open economies grew at an average annual rate of 4.5 percent over these two decades, while the closed economies grew at only a 0.7 percent rate. They present scatter diagrams relating initial income levels to subsequent growth rates over 1970–89 for 117 countries, both rich and poor. Not only do the open economies grow faster, on average, than the closed economies, but the poor open economies grow faster than the rich ones, while there is no such tendency among the closed economies.

The authors provide further support for their hypothesis with regression analyses that show no tendency for convergence for the entire sample, strong convergence for the subsample of open economies, and no

convergence for the subsample of closed economies. Including variables that have been found by other researchers to influence convergence—educational attainment, the investment-to-GDP ratio, and measures of political instability—along with a dummy variable for openness, they find that the openness variable is highly significant while the effect of educational attainment is not. Based on these and related results, Sachs and Warner draw four conclusions: there is strong evidence of unconditional convergence for open countries and no evidence of unconditional convergence for closed countries; open countries uniformly grow faster than closed countries; the role of trade policy is evident, even after controlling for other growth factors; and poor trade policies affect trade both directly and indirectly by limiting the rate of investment in physical capital. Thus all their evidence supports the idea that openness, and the related reforms that inexorably accompany it, are the keys to growth for developing economies.

Sachs and Warner go on to examine some more specific issues regarding trade policy using their sample of open and closed economies over the 1970–90 period. They reject the idea, first associated with Raul Prebisch, that countries dependent on raw materials exports need protection to allow the development of import-competing industries. The reasoning, accepted widely in Latin American countries after World War II, was that these countries would be unable to industrialize under free trade, and would be vulnerable to long-term adverse movements in their terms of trade. The authors' regressions show that open economies progress more rapidly than closed economies from being primary-intensive to manufactures-intensive exporters. They also show that closed economies have been more vulnerable to financial or inflation crises than have open economies. Of thirteen economies that were open throughout the 1970s, only one succumbed to such a crisis, while fifty-seven of seventy developing economies that were closed in the same period experienced such crises. Finally, turning to recent performance the authors find, although on admittedly preliminary evidence, that among the economies in transition from socialism, those with the most intensive trade reforms have produced the best economic performance. All these findings lead the authors to be optimistic about the possibility of improving the lot of poorer countries and convince them that opening trade with the world economy is the key.

SINCE WORLD WAR II the world's economies have operated under a variety of exchange rate systems as drawbacks to one arrangement after another have become apparent. For the first quarter century after the war, the Bretton Woods system of fixed but adjustable exchange rates served the world economy well. Yet as U.S. economic dominance diminished, a series of needed currency realignments starting in the late 1960s, together with continuing pressure on the dollar, led to the introduction of floating exchange rates in 1973. Floating permitted divergent policy responses to the two oil price shocks of the 1970s. But the substantial movement of exchange rates that resulted from floating eventually led to the desire among many nations to reduce exchange rate variability. The most prominent response to this desire was the creation of the European Monetary System and its Exchange Rate Mechanism (ERM) linking the currencies of several European nations. However, speculative pressures brought down the ERM in the early 1990s and, in late 1994, broke the link of the Mexican peso to the dollar. In the second article of this issue, Maurice Obstfeld analyzes the quarter century of experience since the breakdown of Bretton Woods and draws its implications for the redesign of international monetary arrangements in today's world of integrated capital markets.

Early advocates of floating rates argued that they would offset differentials in inflation trends, accommodate equilibrium movements in real exchange rates, and liberate monetary policy to pursue domestic goals, in the process encouraging an open system of trade and capital flows. Obstfeld shows that many of these benefits have been realized. The move to floating rates in the early 1970s permitted some countries to decouple their own inflation rates from the higher U.S. inflation of that time. Floating rates permitted diverse domestic reactions across countries to the oil price shocks of 1974 and 1979. And floating rates have allowed the long-term real appreciation of the yen over the past twenty years, even in the face of Japan's preference for inflation rates lower than those prevailing in the United States. At a fixed dollar-yen rate, real yen appreciation would have required falling prices in the United States or much faster Japanese inflation than they desired.

Performance in the shorter run raises different issues. Obstfeld shows that real and nominal exchange rates have been closely correlated in the short run, an outcome that is dramatically apparent whenever an indus-

trial country has changed the exchange rate regime under which it operates. Without exception, a change from floating to fixed rates has reduced the variance of both real and nominal exchange rates, and a change from fixed to floating rates has sharply increased the variance of each and produced a very high positive correlation between the two.

Obstfeld attributes this correlation to the sluggish adjustment of domestic prices. He notes that, in a sticky price environment, floating rates permit equilibrating relative price changes that otherwise require costly stretches of unemployment, if real depreciation is needed, or inflation, if real appreciation is needed. He cites the British experience after leaving the ERM in 1992 as a clear example. The British lowered interest rates, the pound devalued, the economy expanded, and inflation remained modest.

The main drawback to floating rates has been their volatility, which has been greater than most observers expected before 1973, and also greater than subsequent models relating exchange rates to fundamentals would predict. Obstfeld notes that the Dornbusch overshooting model explains some of the apparent excess movement of exchange rates, and that, under some conditions, exchange rates can vary more than their fundamental determinants, even if currency speculation is rational. But he emphasizes that much of the short-run volatility of exchange rates is nonetheless left unexplained: at short time horizons, structural exchange rate models have less forecasting power than a naive random walk. But he reasons that the welfare costs of such short-term volatility are small, in part because firms can hedge their exposure. On the other hand, he sees the costs of longer-term currency misalignments as substantial, especially for workers who cannot hedge their exposure, and notes that even floating exchange rates can remain misaligned relative to fundamentals for significant periods of time, with the dollar's rise between 1980 and 1985 a prime example. Looking at other aspects of volatility costs, Obstfeld notes that economists since Ragnar Nurkse have emphasized that real exchange rate movements interfere with trade by encouraging protectionism. He reports that statistical research using cross-country data has identified negative effects of volatility on trade volumes, but has not established whether these effects are economically important.

Obstfeld argues that the expansion of international capital mobility has increased the scope for current account imbalances, and he shows

that current account fluctuations have, in fact, widened for many industrial countries since the end of the Bretton Woods system. He sees these developments as complicating economic policymaking. Under some conditions, large current account imbalances appear to be sustainable for extended periods. But, as developing country debt crises have shown, the willingness of foreign lenders to finance continuing deficits can end abruptly.

Compared with the Bretton Woods framework, which provided explicit exchange rate rules and a discipline on national policies, floating rates permit greater freedom and, hence, provide less discipline. Over time, many countries have sought to impose greater discipline on fiscal and monetary policies by adopting multilateral or unilateral schemes of pegged exchange rates. But in Obstfeld's view, these schemes have met with mixed success, and the recent barrage of currency crises has called into question attempts to peg rates in an environment of open capital markets.

The ERM, which began in 1979, sought to impose anti-inflation discipline from Germany onto other European nations by fixing exchange rates within narrow bands. Through 1987 there were frequent, small adjustments in exchange rates; after that, currency adjustments ceased as ERM members liberalized capital accounts and the movement toward full monetary unification accelerated. Obstfeld shows that inflation rates among ERM members did, in fact, converge toward the German inflation rate in these years; but so did inflation rates in countries that maintained floating rates. Thus anti-inflation policies were successful in this period under both fixed and floating rate regimes.

Obstfeld also presents statistical evidence on the degree of inflation persistence under alternative regimes. Persistence is presumed to depend on price- and wage-setting institutions and the decisionmakers that operate within them, both of which may be influenced by expectations of how policymakers will respond to shocks. Except for the United States, which has had considerable policy freedom throughout, countries uniformly have shown slightly more inflation persistence since Bretton Woods. But Obstfeld notes that persistence has increased for both low and high inflation countries and infers that the monetary discipline needed to keep average inflation low does not preclude using the freedom provided by floating rates to pursue domestic employment objectives. Turning to fiscal policy, Obstfeld finds that the ratio of public debt

to GDP rose sharply between 1978 and 1993 in most industrial countries, but finds no connection between debt growth and the exchange rate regime.

In the post-Bretton Woods period, developing countries have often managed their exchange rates in an attempt to bring down domestic inflation. Obstfeld reviews a number of specific episodes and analyzes the general idea behind this strategy. The typical plan ties the currency to a hard currency like the dollar in order to anchor the price of tradable goods, with the hope that this will also reduce the inflation rate of wages and nontradables. There have been some successes, such as Israel in 1985, Chile since the mid-1980s, and, thus far, Argentina since the late 1980s, although Obstfeld notes that, in the first two cases, the government used the exchange rate flexibly. More often, however, domestic prices and wages did not slow enough to prevent real appreciation, enlarged current account deficits and, eventually, a foreign exchange crisis and devaluation.

Obstfeld draws several lessons from these experiences. A pegged exchange rate may be useful in the early stages of disinflation, both for the signals it sends about policy intent and for its immediate effect on the prices of tradables; but it risks creating an unsustainable real exchange rate if domestic inflation does not slow promptly. He notes that auxiliary policies may be useful: temporary capital controls may facilitate exchange rate management, and temporary incomes policies may help coordinate a slowdown in domestic inflation with the slowdown in the inflation of tradables. However, in the end, pegged exchange rates cannot substitute for discipline in fiscal policy.

The crises that have overtaken both developed and developing economies on fixed exchange rates bring into question whether fixed rates are harder to maintain with the increase in world capital mobility that has evolved. The central issue concerns the susceptibility of currencies to speculative attacks. Obstfeld reviews models of speculation that distinguish between self-fulfilling attacks, in which the attack itself forces the government to abandon its currency peg, and crises justified by fundamentals, in which speculators merely contribute to the adjustment. But he suggests that the line between the two cannot be drawn sharply, because the government preferences help define what constitutes the fundamentals that justify a change. Thus whether a fixed rate can be held depends importantly on how much the government wants fixed rates

compared with other economic goals. If a government is seen to be willing to let the exchange rate go rather than raising interest rates to defend it, the currency will be vulnerable to speculative attack. Only by eliminating that possibility can the fixed rate be made invulnerable. Obstfeld concludes that “until sovereign nations are willing to relinquish economic authority to supranational organizations . . . fixed exchange rates and capital mobility will remain an inherently explosive combination.”

Obstfeld also examines the case for sterilized intervention as a way to moderate exchange rate volatility in a floating rate regime. Starting in 1985, when the G-7 nations mounted a massive intervention to bring down the dollar, concerted interventions have been used on a number of occasions. In principle, intervention could work through the portfolio effect that arises from changing the relative supplies of bonds of different currency denominations; but he judges such effects to be empirically unimportant. Intervention could also work by influencing the expectations of market traders about what outcomes the monetary authorities desire. Some researchers have identified episodes of concerted intervention that have been forceful enough to have some success in altering the trend of exchange rates. However, Obstfeld examines dollar-yen interventions over the past two years and comes to a more guarded conclusion. He grants that intervention can provide a helpful signal when markets are confused about policy, but finds little support for the idea that intervention can halt market trends for long.

Finally, Obstfeld turns to various suggestions that have been made for improving the current exchange rate system. He reasons that a return to fixed rates, even if feasible, would not be an improvement for large areas like Europe, Japan, and the United States, where real shocks requiring real exchange rate adjustments occur frequently. He grants that broad target zones are superior to a peg in that they give policy more room for maneuver. But he questions whether they are better than a floating system since defending the edges of the zone presents the same problems as defending a fixed rate. Obstfeld finds some merit in a Tobin tax—a small tax on all foreign exchange transactions—which would discourage short-term speculation but have only a negligible effect on long-term capital movements. He sees developing countries as the most likely candidates for such a tax since they need to manage exchange rates and have relatively shallow financial markets, and because the cost

of failed stabilization is relatively high for them. But he is skeptical about the wider application of a Tobin tax since enforcement would be difficult, given the highly developed capital markets of the world.

Obstfeld expects that the present flexible rate system between major trading blocks will continue because it is too useful to abandon, even though it produces highly volatile exchange rates. While he does not see any major innovation to the exchange rate system as a promising improvement, he argues for the continued development of complementary institutions. The World Trade Organization can help to ensure that exchange rate swings do not undermine free trade. Policy coordination, although always likely to succumb to national interests, can be useful where there are opportunities for mutual gain. And monetary and fiscal stability at home can eliminate some of the sources of exchange rate volatility.

JOB LOSS can have a range of consequences, both to the individual and to the economy. In many cases, epitomized by the dispersal of workers at the completion of a construction project, job losses are routine. However in many other cases, the loss of a permanent job is a significant, costly economic event for an individual worker. After a permanent discharge, a worker may take many months to find a new job and the new job itself is likely to be inferior in many ways to the job that was lost, paying less and entailing a higher probability of subsequent discharge. A concentration of such individual losses represents an important event in the macroeconomy. In the third paper of this volume, Robert Hall examines a variety of evidence on the dynamics of individuals' experiences following a permanent job loss. He finds that brief, sharp episodes of primary job loss are followed by only gradual rebuilding of employment relationships, and identifies this process as a key characteristic of the business cycle.

Hall begins by modeling how job losses may arise under alternative employment arrangements. He contrasts two polar views of separations. In one, a model of efficient employment relations, a separation occurs because the employer and worker, considered jointly, are better off if the worker leaves his existing job. In this model, the wage is simply the means for the two parties to split the joint value from their relationship; it plays no direct role in terminating the job. In the other, a model that incorporates what Hall calls suppressed renegotiation, contractual

wages do matter. The employer discharges the worker if the discharge raises the employer's own value, which is the case if the worker's marginal product falls below the predetermined wage. A discharge reduces the joint value of the worker and firm when the contract wage exceeds the worker's marginal product, but the best alternative job pays less than that marginal product.

Hall suggests that a labor market with suppressed renegotiation will have larger flows of workers in and out of jobs than a market with efficient employment relationships, and he suspects that this feature helps to explain the high rates of job termination in the United States and elsewhere. However, he notes that suppressed renegotiation, even though it leads to wage rigidity and inefficient separations, may be the most efficient arrangement available given information limitations and the need to provide incentives for workers to make firm-specific investments. This is because, when a worker's investment is not directly observable by the firm, and when only the firm knows the worker's productivity, the worker will not find it profitable to invest unless there is a credible commitment by the firm not to capture the increase in productivity resulting from the investment.

The concept of permanent job loss is central to Hall's analysis of employment dynamics, but choosing an empirical measure of it is difficult. Hall presents seven alternatives which give quite different views of the frequency of such losses. The highest estimated frequency is the 17 percent per quarter rate obtained from state unemployment insurance records. These data include even workers whose jobs last only a single day, and therefore do not provide a meaningful measure of the large losses associated with losing long-term jobs. The Current Population Survey (CPS) on the duration of individuals' current jobs implies a 10 percent quarterly rate of job loss. CPS data on the change in employment status of individuals suggest a quarterly loss rate nearer 8 percent. Data on employment changes at individual manufacturing plants provide another measure, this time showing gross employment reductions averaging approximately 6 percent per quarter. Three other measures of job loss that relate to displacement from jobs rather than total separations (and thus exclude quits) show much lower rates of job loss: The Panel Study on Income Dynamics indicates a quarterly rate of less than 2 percent, and two measures from the Displaced Workers Survey indicate a rate well below 1 percent.

Putting aside the briefest jobs, Hall summarizes the picture of job loss that emerges from these findings. On average around 8 to 10 percent of workers separate from their employers each quarter, with around 4 percent of workers losing their jobs permanently. Something like half of these permanent, involuntary losses are sufficiently burdensome that the worker identifies the experience retrospectively as a displacement, and about 0.6 percent remember it as a displacement as much as three years later. Relating the job loss findings to the more familiar unemployment data, Hall notes that some workers who lose jobs do not show up as unemployed because they move directly to another job or out of the labor force altogether. Of the individuals who lose jobs and experience subsequent unemployment, a little less than half have been permanently laid off; the remainder are either on temporary layoff or had quit their job.

Hall summarizes several studies that indicate that the experiences of displaced workers are much worse than the experiences of other workers. He reports Christopher Ruhm's finding that displaced workers average 17 extra percentage points of unemployment in the year they are displaced, 9 extra percentage points in the year after displacement, and a significant amount of extra unemployment even four years after displacement. Their earnings are similarly depressed, averaging almost 15 percent below the earnings of comparable nondisplaced workers after four years. Taking into account both the higher unemployment and lower weekly earnings of displaced workers, Hall shows that the capitalized value of the lost earnings is substantial. He calculates that the financial cost of such a job loss amounts to about 120 percent of a worker's annual earnings.

The findings just discussed are based on observations over several years, and so represent some sort of average experience. Hall recognizes that they miss the fact that aggregate conditions are likely to affect both the probability of job displacement and the postdisplacement unemployment and earnings losses. Some probabilities, such as the quit rate and the rate of departure from the labor force, clearly vary over the cycle. On the other hand, he notes that the job-finding rate varies only slightly with unemployment.

Hall also recognizes that his description of employment dynamics may be interpreted in different ways, depending on one's view of the macroeconomy. At one extreme, one could view all labor market develop-

ments as the consequence of variations in aggregate demand. In this case the findings about unemployment and earnings in the aftermath of an adverse job loss shock describe how the economy allocates scarce jobs. Hall hypothesizes the opposite extreme, that the persistent movements of employment can be completely accounted for by the dynamics of rebuilding employment relationships after a job loss shock of unspecified origin. He views this analysis as an attempt to find out how far labor market dynamics can be explained with only a single source of cyclical variation, the rate of primary job loss.

Hall suggests that gross employment reductions in manufacturing, measured at the plant level, appear to be the best available measure of the primary job loss resulting from adverse macroeconomic shocks to the labor market. Empirically, recessions start off with large bursts of employment reductions in manufacturing, but these do not persist. Rather, during the extended slump after a sharp contraction, gross employment reductions return to normal levels. Data on economywide flows into unemployment provide another view of the dynamics of job loss. A burst of unemployment resulting from permanent layoffs, for example, is followed by several years of higher levels of unemployment. To reconcile the stories told by these two sources, Hall notes that manufacturing leads the rest of the economy cyclically; laid-off workers may take temporary work, only to be laid off again; and some may leave the labor force and reenter later, rather than becoming unemployed directly.

Since he regards the data on gross employment reductions as the key measure of shocks to the labor market, Hall uses them to explain subsequent inflows to unemployment. He runs regressions for each of five categories of new unemployment—permanent layoffs, temporary layoffs, job quitters, reentrants, and new entrants—with current and seven quarterly lags of gross employment reductions. He finds that the effects of gross job reductions on permanent layoffs are spread over all eight quarters; the effect on temporary layoffs is more heavily concentrated in the current quarter, but with some downstream effects; and the effect on quits is spread relatively evenly over the eight quarters, consistent with the idea that slack in the labor market discourages quits. The effect on entrants is less reliably estimated.

These results indicate that a burst of primary job losses has effects on employment and earnings over a significant period. But the actual dy-

namics are complicated and involve, in addition to the initial effect on new unemployment spells, the rate at which individuals leave the pool of unemployed, and their subsequent experience, either as workers or out of the labor force. Hall first shows that unemployment depends on a longer distributed lag of job losses. He then constructs a probability model of the process that is set in motion by a burst of job losses. The model is designed to capture the idea that when an event breaks a set of long-term employment relationships, the workers released in the labor market will form new relationships, many of which will prove to be short lived, with the worker rejoining the ranks of the unemployed. Workers may take temporary jobs or experiment with other types of work. New matches of employee and employer may not work, and new employees, with little firm-specific human capital, are likely to be the first laid off if the firm has a subsequent adverse shock.

Hall notes that a simple Markov model of transitions is not faithful to even the most conspicuous features of the labor market's dynamics. For example, rates of separation decline sharply with tenure on the job, and job-finding rates fall with the duration of unemployment. To capture the salient features of the dynamics following a shock, he constructs a nineteen-state Markov model. Low-tenure jobs are broken into four tenure categories, high-tenure jobs into eight. The model distinguishes regular job search and displacement search, each broken into three categories. Interim jobs are explicitly recognized. Even with this much detail, the model does not distinguish quits from layoffs. Hall uses data on the cross-section distribution of job tenure to estimate separation rates and makes educated guesses about the other crucial probabilities, choosing parameter values that result in an equilibrium distribution across tenure categories that mirrors the CPS data. In equilibrium Hall's model yields an unemployment rate of 4.3 percent, of which 3.5 percent is displacement unemployment. Since the model does not include unemployment associated with very brief jobs or new entrants, it is not surprising that its equilibrium unemployment rate is somewhat below historical U.S. experience.

To simulate a burst of primary job losses in his model, Hall increases the separation rates in all tenure categories by 10 percent for one period. This is a large shock, corresponding to the shutdown of 10 percent of the economy, and it shifts the balance of job seekers toward those in dis-

placement search and increases the fraction of workers in interim jobs. While the majority of workers, including the high-tenure displaced workers, find new work promptly, the speed with which an individual job seeker finds a job greatly overstates the speed of recovery of the labor market as a whole, and it takes many quarters for the labor market to return to equilibrium. Many spells of unemployment start as late as two years after the initial shock, and the effects on earnings extend for at least four years.

At the individual level, loss of a long-term job leads to episodes of brief employment, periods of job search or time out of the labor market, and lower earnings when working. At the macroeconomic level, a cyclical picture emerges of occasional bursts of primary job loss followed by long periods of rebuilding employment relationships. Employment shocks thus have significant downstream effects. The magnitude of the aggregate compensation losses rises sharply during recessions. Hall calculates that from levels of about \$25 billion per year in 1972 and 1973, the value of job loss peaked at over \$70 billion per year during the sharp recession in late 1974. Losses of similar magnitude occurred in 1980, 1982, and 1987. However, the recovery from these peak levels has been moderate in recent years, and the value of job loss has remained above \$60 billion a year.

While Hall believes that the labor market dynamics he describes help to explain the persistence of high unemployment and low output long after an initial shock has triggered a recession, he concludes that much remains to be explained. Even if we accept that employment relationships may be fragile because their terms cannot be renegotiated, we cannot evaluate the efficiency of these arrangements. Furthermore, in the context of his model, we have no explanation for the periodic shocks that cause such large bursts of primary job loss in the first place.

STIMULATED BOTH BY the availability of new data and new theoretical developments, there has been a resurgence of interest in economic growth in recent years. New models, relaxing restrictive assumptions of traditional theory, have expanded potential explanations of empirical phenomena, and cross-national studies have provided new tests of hypotheses that were difficult to examine with traditional time series data. In the fourth paper of this volume, N. Gregory Mankiw assesses

the adequacy of both old and new modern growth theory for explaining the salient facts about growth, and presents his own view of the prospects for future advances in our understanding.

Economists confronting questions about economic growth look first to the Solow-Swan neoclassical model dating from the 1950s. That model has served for so long because it is simple yet makes predictions about the central features of economic growth. Mankiw begins by reviewing the essential features and implications of this centerpiece of modern growth theory. He lists five of the model's predictions about an economy's long-run steady-state growth path: it is independent of initial conditions; the level of income depends simply on the rates of saving and population growth; the growth rate of per capita income depends only on the rate of technological progress; the marginal product of labor and the real wage grow at the rate of technological progress; and the marginal product of capital is constant. Mankiw notes that most of these predictions are broadly consistent with experience. For example, in the U.S. economy the real wage grows at about the same rate as income per person, and the profit rate and capital-to-income ratio exhibit little trend. Also in accord with the model, across countries income per capita is positively correlated with saving rates and negatively correlated with population growth rates. Contrary to the model's predictions, however, there appears to be a strong correlation between growth and saving across countries. According to the model, this fact, and the apparent lack of convergence of growth rates in general, must be interpreted as reflecting a very slow transition of economies to their steady states.

Mankiw reviews, but does not find compelling, some objections that have been raised to the basic neoclassical model. First, the model does not shed light on the ultimate source of economic growth—in the steady state all growth is due to advances in technology, which itself is taken as exogenous. Second, the model assumes that different countries use roughly the same production function at a given point in time. He believes these assumptions are appropriate since he views the challenge to growth theory as being to explain variations in economic growth across countries and at different times rather than the existence of economic growth *per se*. Indeed, he sees it as a virtue of the theory that it attempts to explain differences in growth without resorting to the possibility of different production functions and different rates of technological change.

Mankiw finds more merit in some attacks on the empirical adequacy of the model. First, the model makes predictions about how steady-state income levels vary with differences in saving ratios and population growth rates. For a capital share of one-third, a country with four times another country's saving rate should have a steady-state income about twice as large. Furthermore, with realistic rates of technological change and depreciation, an additional 2 percentage points in the population growth rate should reduce income per capita by 15 percent. These differences in saving and population growth correspond roughly to the differences between many rich and poor economies. However, in the model they predict that levels of income per capita would differ only by a factor of a little more than two, while income per capita in fact differs by more than a factor of ten.

A second difficulty is in the model's predictions of how quickly the income of an economy converges to its steady-state level. For realistic values of the same key parameters that determine steady-state income, the model predicts convergence at a rate of approximately 4 percent per year. Yet most studies have found convergence at only half this rate.

A third difficulty is in the model's prediction of differences in the rates of return on capital. If poor countries are poor because they have low capital-to-labor ratios, capital invested in these countries would have a high rate of return, and capital would have an incentive to flow from rich countries to poor. According to the model, if the production function is Cobb-Douglas, as is usually assumed, a poor country with an income one-tenth that of a rich country should have a rate of return one hundred times as large. Mankiw observes that it does not take precise data to reject this prediction. He shows that this prediction of the model can be salvaged by assuming an elasticity of substitution greater than the 1.0 of the Cobb-Douglas function. With a moderate elasticity of substitution of four, the rate of return to capital in the poorer country would be three times as great as in the rich country. While still large, this prediction is empirically much more plausible. However, this modification creates another problem for the model because increasing the elasticity of substitution also reduces the predicted differences in wage rates across countries. For an elasticity of one, wages are proportional to per capita income, roughly in accord with the facts; with an elasticity of four, a country with ten times the per capita income of another is predicted to have wage rates less than twice as high.

These difficulties in reconciling the standard Solow model with observation have led to a reexamination of the concept of capital and the return to capital. Traditionally, capital has been thought of as consisting of the economy's tangible assets, primarily the stock of equipment and structures, and the return to capital was the profits received by the owners of those assets. In recent years, a much broader view of capital has emerged that significantly alters the interpretation and success of the neoclassical model in explaining empirical regularities. In particular, many economists have stressed the importance of the acquisition of knowledge and skills as a form of human capital accumulation. The returns to human capital may be large. For example, if the returns to labor in excess of the returns received by unskilled labor are attributed to school and on-the-job training, some two-thirds of labor income, or almost half of national income, can be attributed to human capital.

Mankiw first notes that if the capital share were much larger than the traditional one-quarter or one-third, the three problems with its predictions discussed above could be eliminated. For example, with a capital share of two-thirds, differences in saving rates between rich and poor countries explain the observed differences in steady-state per capita income. Similarly, doubling the capital share roughly halves the predicted rate of convergence, bringing it in line with most estimates, and leads to a prediction of a rate of return in the poorer countries three times, rather than one hundred times, that in the rich. Assuming in addition a somewhat higher elasticity of substitution, the predicted rate of return in poor countries can be made low enough to rationalize by differences in taxes, political risks, and the risk of expropriation. Hence incorporating human capital into the model can reconcile its predictions with many broad facts and, Mankiw notes, helps to explain why capital does not flow more readily from rich to poor countries.

Mankiw finds it less plausible that the model can be salvaged by assuming that the returns to capital greatly exceed the conventionally measured private returns. Paul Romer has suggested this discrepancy between private and social returns could arise, for example, because as capital is built, new ideas may arise and enter the general pool of knowledge. Mankiw finds the idea that capital conveys positive externalities plausible, but notes that if such externalities are to explain cross-country differences, they must be geographically limited in their effect. He is

skeptical that capital conveys local externalities of the magnitude necessary to save the neoclassical model from empirical difficulty.

Mankiw concludes that the role of capital in economic growth is much greater than traditionally assumed, and that it is not necessary to make substantial changes in the model to explain the international differences that initially appeared to counter its predictions. He recognizes, however, that the Solow model does not illuminate the sources of persistent growth, simply assuming exogenous advances to technology. He therefore turns to an assessment of endogenous growth models in which greater saving leads to faster growth forever. The essential element in these models is that, in one way or another, they all assume constant returns to scale in the factor being accumulated through saving. Mankiw notes that a broader concept of capital inherently leads toward constant returns, and that some endogenous growth models can be regarded as a limiting case of the neoclassical model.

Some of the endogenous growth models explicitly provide for the endogenous determination of education and help to clarify the role of human capital formation in the growth process. However, Mankiw finds explanations of endogenous growth in terms of the accretion of knowledge more appealing. He argues that models that incorporate the generation of knowledge need to recognize three microeconomic facts: even though knowledge is largely a public good, much research is done by firms driven by the profit motive; research is profitable because innovations give firms temporary monopolies; and when one firm innovates, other firms build on that innovation in order to produce the next generation of innovations. He commends recent work that incorporates these characteristics of research and development by allowing for monopolistic competition in modeling growth.

An attractive element of recent research on economic growth, in contrast to the work of the 1950s and 1960s, is its empirical emphasis. Much of this work was made possible by the construction by Summers and Heston of international data suitable for cross-sectional analysis. Mankiw provides a selective summary of the research that has utilized these data. Many of the results—for example that growth is fostered by high investment, widespread education, low population growth, political stability, and well-developed financial institutions—accord with most economists' prior beliefs. But Mankiw notes three problems that pervade this entire literature and raise questions about its reliability. The

most obvious problem is simultaneity. Investment, population growth, human capital formation, and almost every other variable used to explain growth can be regarded as caused by, as well as causing, growth. Standard econometric practices do not resolve this problem. A second problem with interpreting cross-country regressions is multicollinearity among the explanatory variables. Together with the presence of measurement errors, multicollinearity can be an important source of bias. He gives as an example the provocative finding of De Long and Summers that the social return to equipment investment is very high, while the effect of education is estimated to be insignificant. Mankiw reasons that equipment investment may be proxying for investment in human capital, which itself is poorly measured, and that equipment investment, not surprisingly, is higher in countries with highly skilled workers. He also argues that there are so many hypotheses to be tested that not even a larger cross-section of countries can provide enough degrees of freedom.

Mankiw regards modern growth theory as one of the triumphs of economics. In his view neoclassical theory, with its emphasis on capital accumulation, broadly defined, is the key to international differences in growth, while endogenous growth theory illuminates the reasons for worldwide advances in knowledge. But he reminds us that little progress has been made in explaining why some countries save and invest so much more than others, and he observes that we have far to go in understanding the creation and dissemination of knowledge. Similarly, he believes that economists have yet to produce persuasive measures of the magnitude and sources of externalities which could justify a role for government policy in stimulating growth. He concludes on a cautionary note, urging economists to heed the first rule for physicians: Do no harm.

THE GLOBALIZATION OF the world economy over the postwar period has significantly altered the economic landscape in which developed economies operate. Today, commentators who discuss the U.S. economy as if it were closed do so at their peril. The share of trade in U.S. GDP has more than doubled since 1960, and it has risen by about 50 percent in the average OECD country. In a number of developing countries, trade has risen much faster; China, which used to be virtually isolated from the world economy, may now export one-quarter of its GDP. And the

growth of manufactured imports from these low-wage developing nations has raised troubling questions about its effects on unskilled labor in the United States and other advanced economies. In the fifth paper of this issue, Paul Krugman provides perspective on these developments and attempts to shed light on the causes and implications of growing world trade.

Krugman begins with a historical overview of the growth of world trade. He notes that world markets achieved an impressive degree of integration during the second half of the nineteenth century. By the eve of the First World War, steamships and railroads had created global markets for standardized commodities like wheat or wool, and even the global flow of information was better than many modern observers realize; by 1900 all of the world's major economic regions could effectively communicate instantaneously. In the major industrial nations, merchandise exports as a share of GDP had risen from 5.1 percent in 1850 to nearly 12 percent on the eve of World War I. However, between then and the early post-World War II years, most of the world's economies turned inward, and the share of world output that entered into international trade declined substantially.

Using numbers assembled by the World Bank, Krugman estimates that merchandise exports did not outstrip 1913 levels as a share of world output until sometime around 1980. The same seems true of the major western powers individually. Germany's trade share was still below its 1913 level in 1970, and Britain's trade share did not reach its 1913 level until the late 1980s. However, although recent trade volumes do not suggest that the present era is different from previous experience, Krugman identifies several novel features of contemporary international trade: the rise of *intra-trade*, trade in similar goods between similar countries; the ability of producers to *slice up the value chain*, breaking the production process into geographically separated steps with resultant increased trade in intermediate goods; the emergence of *supertraders*, countries with extremely high ratios of trade to GDP; and the growing exports of manufactured goods from low-wage to high-wage nations, a feature that provokes great anxiety in developed nations.

Krugman uses data on the major changes in the composition of British trade to illustrate the growth of intra-trade. In 1913 Britain exported three times the volume of manufactured goods it imported, and the majority of its trade was with non-European countries. It was a nation that

exported manufactured goods and imported raw materials. By 1992 roughly 80 percent of both imports and exports were manufactures, and most of the Britain's trade was with other European nations. Strikingly, the bulk of the increase in trade that followed major trade liberalizations among industrial countries after World War II consisted of nearly balanced increases in exports and imports within three-digit industrial categories. The standard explanation for trade in similar products is economies of scale in the production of differentiated products, an idea associated with Krugman himself. He notes, however, that the rise of intra-trade in part reflects the fact that the "typical" manufactured product has become more complex and involves the use of a greater variety of specialized intermediate goods. This feature, which increases the scope for slicing up the value chain, greatly increases the potential volume of international trade. In Krugman's view, a typical consumer good in 1913 could only be exported once; today, a good produced in one country may be assembled from components produced in other countries, which themselves may embody imports. As a result the trade involved in global production of a final good can be several times the value added in all the stages of its production.

This increased potential for specialization helps explain Krugman's third new feature of international trade, the emergence of supertrading nations. He reports that, whereas it is unlikely that any country had exports exceeding 50 percent of GDP in 1913, today there are at least six such countries. In two cases, Singapore and Hong Kong, exports actually exceed GDP. While it is still true that scale plays a major role in determining trade shares, Krugman suggests that slicing up the value chain has reduced the importance of the size of the home market in determining home production, and has therefore weakened the link between a country's population and its trade share.

The rapid growth of manufactured exports from low-wage, newly industrialized economies is the new feature of trade that has created the most controversy and concern. Such trade was virtually nonexistent twenty-five years ago, when manufacturing exports from such countries were of the order of one-quarter of a percent of the GDP of the destination nations. Today they are roughly six times as great. While the numbers are still small, they are now large enough to be a significant feature in the world economy. To many it seemed unlikely that this could ever be the case, and the growth of these exports was initially something of

a surprise. Krugman believes that the slicing up of the value chain has something to do with this phenomenon. It has proven possible to expand niches for labor-intensive products by slicing up the production of goods traditionally viewed as skill-, capital-, or technology-intensive, putting labor-intensive slices in low-wage locations.

What are the major factors which explain why trade has grown faster than world output? Krugman downplays the technological explanations popular with journalists, arguing that the oft-cited reduction in transportation costs and growing speed of communications, while they are important, largely occurred earlier, and that political developments, especially trade policy, have been the dominant force in the growth of world trade since 1950. He notes that large-scale world trade first emerged in the days of Pax Britannica, that politics killed the global economy between the two world wars, and that it was reconstructed with the gradual restoration of a free trade environment under Pax Americana. But he also observes that this broad scenario misses many subtleties of the actual process. The pre-1914 world could hardly be described as one with universal free trade when the United States and Germany were both protectionist. And the progress in the postwar period has not been entirely the result of GATT negotiations and changes in trade policy among the OECD nations. In recent years, the most important moves toward free trade have been unilateral actions by developing countries that have turned away from earlier import-substitution policies.

Krugman also warns that elimination of formal trade barriers by the political process will still leave us far from the economist's idealized view of free trade. He cites a recent study by John McCallum which shows that, even between the United States and Canada, where tariff barriers have been low for some time and where language is not an impediment, common laws, customs, and institutions nonetheless make national boundaries important for trade.

Of course, even if trade were unaffected by the existence of national boundaries, the measured volume of shipments across borders would depend on the relative size of the trading partners. Krugman constructs a simple gravity model that illustrates how the distribution of world output affects measured volume. As might be expected, the more equal the size of national economies, the greater the overall share of trade. In fact, the national distribution of gross product among the world's economies has become somewhat more equal over time, with the relative decline of

the United States, whose share of world output has declined by approximately 20 percent, being the most important source of this change. Hence some portion of the recorded increase in world trade may have nothing to do with either political or technological changes.

In Krugman's view, the most controversial new aspect of world trade arises from the fact that the rise of manufacturing exports from newly industrializing economies (NIEs) has more or less coincided with increases in wage inequality, particularly in the United States, and with a sharp rise in unemployment in Europe. While research to date suggests that the impact of third world exports on first world markets has been small, the literature has largely ignored how this trade and the wages and employment in advanced countries are simultaneously determined. To address this issue, Krugman constructs a general-equilibrium model of a world economy with two countries, one representing the OECD as a whole, the other representing the aggregate of NIEs. The OECD economy has two types of labor, skilled and unskilled, and produces two commodities that differ in their use of these two factors. The model is essentially the standard two-sector model common in the international trade literature, but with capital replaced by a second type of labor, so that Krugman can focus on the interaction of trade, employment, and the wage distribution. Since his purpose is to show how changes in NIE behavior affect the OECD economy, the NIE is represented simply by an offer curve, which shifts outward with growth and increased integration with the world economy.

To breathe life into the model, Krugman makes educated guesses, informed by various data sources, about the crucial parameters—ratios of skilled to unskilled labor supplies and wage rates, shares of each type of worker in the two industries, shares of goods in expenditure and employment. He then considers the effect of a shift in the NIE offer curve in two regimes, a "European" regime in which relative wages are assumed to be rigid, so that reduced demand for less skilled workers is reflected in unemployment; and a more conventional, "American," regime in which wages are flexible and adjust to shifts in the demand for labor. Under the European assumptions, the shift out in the NIE offer curve must be accommodated by a change in both production and employment. With the assumed wage rigidity, the prices of the two goods are unaffected, and both employment and income decline. Interestingly, because of reduced income, the decline in the employment of unskilled la-

bor is greater than the quantity of unskilled labor “imported.” The output of the unskilled-intensive good falls by more than the volume of imports. Similarly, the output of the skill-intensive good rises less than the volume of exports. Using World Bank data and his assumed parameter values, Krugman shows that this demand effect can be quite substantial. He estimates the total employment decline by 1.5 percent, double the amount directly accounted for by the net imports of NIE manufactures.

The results differ substantially if wages are flexible and the effects of trade are manifested in income inequality rather than unemployment. In this case, prices change and OECD production shifts toward more production of the skill-intensive good as a result of the shift in the NIE offer curve. Hence, quantifying the impact of such a shift requires assumptions about the elasticities of substitution in production and demand. Krugman considers the simplest case, Cobb-Douglas for both, which he argues is not too far from most empirical estimates. Armed with these and his earlier assumptions, he solves for the competitive equilibrium with and without NIE trade. He finds that NIE trade of 2.2 percent of OECD gross product, somewhat more than the actual level, raises the relative wage of skilled workers by only 3 percent, and the relative price of skill-intensive goods by only 1 percent. He concludes that it is not surprising that studies of the effects of trade on income have failed to find any clear-cut effects—the changes in relative prices associated with the growth of NIE trade should be well within measurement error.

Krugman concludes that NIE trade can explain only a fraction of the increase in income inequality in the United States since 1970 and in unemployment in Europe for the same period. Thus a careful assessment does not support the view that NIE is the principal cause of these labor market problems, nor does it support apocalyptic predictions about the future effects of such trade.