

## *Editors' Summary*

THIS ISSUE of *Brookings Papers on Economic Activity* contains papers and discussions presented at the fortieth conference of the Brookings Panel on Economic Activity, which was held in Washington, D.C., on September 12 and 13, 1985. The papers cover a wide range of topics of policy and professional interest. One article analyzes and evaluates the floating exchange rate system. Another studies the aggregate consumption function, including the response of consumption spending to the tax changes of the 1980s. One article and a shorter report examine the Latin American debt problem. Two other reports focus on union wage developments and on technological innovation as an explanation for productivity.

IN THE HIGHLY INTEGRATED world economy, major developments in one part of the world have repercussions for nations everywhere. It is widely acknowledged that aggregate demand and interest rates are two important links through which policies in the industrialized nations can worsen or lessen the foreign debt problem besetting many less developed countries. These links and the effects on LDCs of policies and economic performance in the industrialized nations are the subjects addressed by Rudiger Dornbusch in the first paper in the present volume.

Dornbusch stresses at the outset two distinct perspectives on the LDCs' debt problem. From one perspective, the issue is a narrow one of debt servicing, which depends on the flow of foreign currencies available to the debtor nations. The industrialized nations, out of concern for their banks and financial markets, tend to view the debt problem in this light. From the other perspective, the issue is the gain or loss in welfare to the debtor LDCs resulting from the debt crisis and attempts to deal with it. Dornbusch shows why what is good for the industrialized nations is not necessarily good for the debtors.

He illustrates this point using a defense of U.S. policies that President

Reagan made last year. The president acknowledged that recent high interest rates raised the net interest payments of non-oil-producing LDCs, but he suggested that this burden was far outweighed by the increase in LDC exports to the United States. Using actual data for 1983–84, the period referred to by the president, Dornbusch shows that net dollar cash flows to the nonoil LDCs did in fact rise by \$32 billion. But the change in LDC welfare was negligible. The president had ignored two adjustments necessary to infer welfare changes. First, an adjustment must be made for the costs of increased exports. While it is true that welfare is enhanced by greater trade revenues coming from improved terms of trade—the ratio of export prices to import prices—it is not enhanced, in a distortion-free world, by an increased volume of exports, which diverts resources from other uses of comparable value. Second, nominal interest rates must be adjusted for inflation. Welfare is affected by the change in real rather than nominal interest rates, and real interest rates rose sharply in the period because export (and import) prices were declining.

Such calculations are quite sensitive to the period over which they are made. In his discussion of the paper, William Cline shows alternative welfare calculations, based on an adjacent period during which LDCs' export prices rose rather than fell, which show a substantial improvement in LDC welfare. Furthermore, Dornbusch himself shows that when certain distortions exist, the change in the volume of exports can affect welfare. But the general principle remains: LDCs' net cash inflow of foreign exchange and LDC welfare are not equivalent. The dominant effects on welfare come from changes in the terms of trade and in real interest rates, both of which transfer command over real resources to or away from the LDCs.

Dornbusch then turns to analyzing how foreign disturbances—in particular, developments in the industrialized OECD nations—can be expected to affect welfare in an individual LDC. The effects will depend not only on how the disturbances affect world demand and interest rates, but also on the structure of both the LDC's trade and its debt. Before the 1960s, LDCs typically exported primary commodities and imported manufactures. Dornbusch shows that trade in many Latin American countries still resembles this traditional pattern, with primary commodities accounting for over 40 percent of exports and manufactures for nearly 60 percent of imports for the region as a whole. In East Asia, by

contrast, manufactures account for nearly 50 percent of total exports. Korea is an extreme, with over 80 percent of its exports in the form of manufactures. LDCs also differ widely in their debt-export ratios. The major Latin American countries were burdened by debt-export ratios between 300 and 500 percent in 1984, an exposure that makes them exceptionally vulnerable to increases in interest rates. By contrast, major East Asian LDCs had debt-export ratios ranging from 95 to 154 percent in that same year.

Dornbusch provides econometric evidence of the link between industrial countries' performance and the variables that most affect the welfare of LDCs. For most of the debt-ridden Latin American LDCs, he finds that the terms of trade improve with OECD growth and a weaker dollar, both of which raise commodity prices, and that their exports and output rise with OECD growth. Their debt burden is reduced by lower real interest rates, which, in turn, come from lower nominal rates in the United States and rising prices for their exports.

According to Dornbusch, the sustainability of LDC debts, which is the central concern of lenders, depends, roughly, on reducing the debt-export ratio. He shows that this ratio declines when a country's export revenues grow faster than the interest rate on its debts adjusted for the fraction of interest payments that are covered by the country's trade surplus. During the 1970s, LDC export revenues grew at a rate substantially higher than the average interest rate. During 1980–84, however, the annual growth of export earnings fell sharply—from an average of 20 percent to 2.5 percent for all nonoil LDCs—and debt-export ratios in many countries soared, despite current account surpluses.

Dornbusch assesses various policy scenarios and the conflicting interests of OECD and LDC nations. From their own perspective, the best short-run scenario for the debtor LDCs would be an OECD policy of rapid expansion fueled by lower interest rates, along with a large decline in the exchange value of the dollar. Such a scenario would improve their welfare both by improving the debtors' terms of trade and by lowering their real interest rates. It would also improve their cash flow and reduce debt-export ratios. But in Dornbusch's view, the sustainability of such a scenario is doubtful because it would be inflationary in the long run, and, in any case, is far from the intentions of the OECD nations. What Dornbusch describes as the "first-best" policy mix from the perspective of OECD countries involves a transitory

European fiscal expansion, a long-term fiscal tightening in the United States, and an accommodating monetary policy that would reduce interest rates. This policy mix would help the debtor LDCs, but to a much lesser degree than would the more vigorous monetary expansion. However, Dornbusch observes that, at present, OECD governments outside the United States are not inclined to pursue even transitory fiscal expansion. Instead, attention is focused on the U.S. deficit, with the hope that reducing it will be sufficient to improve all the world's problems.

Dornbusch judges that this most likely scenario for OECD policy, which counts mainly on U.S. fiscal tightening, is approximately neutral with respect to the cash-flow debt dynamics of LDCs. With some dollar depreciation and moderate OECD growth, LDCs' export revenues should grow by 10 to 15 percent a year, not far from the rate of interest, thus providing only a little help in reducing debt-export ratios. Consequently, reducing those ratios, which has become a prime objective of U.S. policy toward the Latin LDCs, will require the LDCs to maintain adjustment programs designed to generate substantial trade surpluses.

Dornbusch outlines three difficulties for LDC governments inherent in such programs of "forced debt service" that transfer resources to the industrialized countries. One problem stems from the need to provide in the budget for servicing foreign debt. Because it is politically difficult, if not impossible, to raise taxes, budget deficits rise. This may exacerbate inflation and, in any case, will require high real interest rates to induce the private sector to hold the added government debt. Many Latin countries already find themselves struggling with inflation and high real interest rates. The second difficulty stems from the need to convert domestic revenues into foreign exchange. In the absence of stiffening trade restrictions, this requires a real depreciation in order to improve the trade balance. The amount of depreciation is greater the smaller the foreign trade sector, making the adjustment especially painful for the typical Latin American country. Furthermore, because indexation, which minimizes the political conflicts otherwise inherent in exchange rate movements, is so prevalent in Latin America, devaluation adds still further to inflation. Finally, Dornbusch observes that the needed devaluations are greater when many LDCs try, at the same time, to achieve the necessary trade surpluses. Because of all these difficulties, Dornbusch warns that forced debt service, requiring the build-up of substantial trade surpluses, will become increasingly threatening to domestic political stability in Latin America.

UNTIL RECENTLY most observers of the international scene have considered the floating rate system of exchange rates a success. Even though exchange rates had been somewhat more volatile than was anticipated at the time of the formal adoption of floating rates in 1976, most economists believed that floating rates provided increased freedom of maneuver for domestic monetary policies and that the exchange rates themselves provided appropriate signals for resource allocation. But when the dramatic depreciation of the dollar between 1976 and 1979 was followed by an even larger appreciation between 1980 and 1985, politicians and economists began to question those beliefs. In the second paper of this issue, Maurice Obstfeld examines the performance of floating rates during the past decade and reexamines the theoretical case for floating rates, asking whether a less flexible exchange rate system would have been better.

Large movements in the nominal exchange rate may have effects on financial markets, but it is real exchange rate movements that direct resource reallocations. Spurious movements of real exchange rates would constitute a flaw in the exchange rate regime. Fixed rates, of course, do not guarantee stability of real exchange rates given differential rates of inflation. And real exchange rate movements under whatever system may or may not be appropriate signals for resource movements. Whether, on balance, fixed rates or flexible rates provide more appropriate signals for the allocation of resources is an empirical question, but unfortunately a difficult one to answer.

Obstfeld begins by examining the movements of the dollar's real exchange values. Against a weighted average of other currencies, the dollar depreciated sharply between 1969 and 1975 and again between 1976 and 1979. This depreciation was followed by a massive real appreciation of about 40 percent from 1979 until early 1985. Over this last period, the dollar's real value rose by 60 percent relative to the deutsche mark, but only by 20 percent relative to the yen. These large variations in exchange rates indicate that the purchasing power parity theory, which predicts that movements in exchange rates will exactly offset intercountry differentials in inflation, is not a good characterization of the real world. Indeed, Obstfeld reports that the correlation between real and nominal effective dollar exchange rates during the period was 0.95, suggesting that most nominal exchange rate movements caused rather than eliminated changes in real exchange rates.

Understanding the sources of these dramatic changes in exchange

rates would help in assessing their appropriateness as guides to resource allocation. Obstfeld observes that there have been substantial national differences in output and employment growth during the present recovery from the last recession, with the most rapid growth taking place in the United States, Japan, and Canada. The U.S. trade deficit is obviously the counterpart of surpluses abroad; but the existence of trade surpluses in Japan and Canada suggests that part of their recovery is attributable to growth in U.S. demand. He notes that fiscal policy has been strongly expansionary in the United States and contractionary in most other OECD countries, a development consistent with real appreciation of the dollar. The role of monetary policies is less clear because changes in financial regulation make it difficult to interpret recent U.S. money growth figures. However, it appears that there have been even sharper monetary slowdowns in Japan and Germany than in the United States. This suggests that tight monetary policy cannot explain the dollar's appreciation.

Obstfeld cites the appreciation of the dollar since 1982 as an example of the potential benefits of a flexible rate, arguing that appreciation moderated the potentially inflationary effect of rising demand on U.S. output and prices by shifting demand abroad and thus "exporting" recovery to Japan, Canada, and to a lesser extent, Europe. While most observers would agree that exporting U.S. demand was beneficial on this occasion, it is not obvious that this would typically be the case. At other times, the export of an excessively stimulative budget deficit would be unwelcome abroad.

In order to sharpen our understanding of the way policy actions affect exchange rates and other economic variables in a flexible rate regime, Obstfeld constructs a Keynesian asset-market model for the global economy. The model assumes rapid adjustment of asset markets relative to goods markets, with nominal exchange rates between countries linked by the interest rate parity condition. Exchange rates maintain continuous portfolio balance, given expectations about the future, and hence play a central role in determining equilibrium terms of trade over the longer term. The model assumes that, in the short run, sluggish nominal wage adjustments can give rise to unemployment, whereas, in the medium run, wage flexibility insures full employment. Also in the short run, increased demand for domestic goods causes appreciation, giving a theoretical rationale for the observed association between demand

expansion in the United States and elsewhere and the dollar's real appreciation.

Even when all prices are flexible, floating rates do not provide insulation from changes elsewhere in the world economy. Because the full-employment real exchange rate is expected to remain constant, home and foreign real interest rates coincide, and expansionary demand shocks from fiscal policy or other sources drive up global real rates. They also drive up price levels throughout the world.

Obstfeld's analysis shows that the transmission of real or monetary shocks depends crucially on the exchange rate regime. Real demand shocks are transmitted more readily with flexible exchange rates. If demand shocks are prevalent in both domestic and foreign economies, the sharing of the output variation that occurs under a flexible regime may be preferable to the greater insulation that occurs under a fixed rate regime. The gains from this "pooling" of real shocks will obviously be greater the smaller the correlation of shocks across countries, with the most advantageous situation arising when such shocks are negatively correlated. On the other hand, Obstfeld shows that, with a floating exchange rate, purely monetary shocks may cause output to vary more than it would under fixed rates. Hence countries that desire to share risk are likely to prefer a fixed exchange rate when monetary shocks are dominant, but a floating rate if demand shocks are the major concern. Obstfeld's analysis also provides an explanation for international disagreement about the appropriate exchange rate regime. If one country's shocks are primarily real, whereas another's are primarily monetary, the two will prefer different regimes.

It is one thing to compare, as Obstfeld's model does, how economies would respond to given policy shocks under fixed and flexible exchange rate regimes. It is another to ask whether the policies themselves would have been different in a different exchange regime. Put differently, do fixed rates discipline policymakers into pursuing appropriate budgetary policies? Specifically, would U.S. policymakers have reduced the U.S. budget deficit if they had been operating in a fixed rate regime? Obstfeld finds no reason to presume they would have. The budget deficit would have raised interest rates and prices more under fixed exchange rates. But he does not believe that these developments would have provided powerful incentives for deficit reduction, citing the failure to restrain fiscal policies in the late 1960s as an example. Several discussants at the

meeting disagreed, arguing that the rise in U.S. interest rates that would have been required to avoid overheating the economy in the absence of a rise in the exchange rate would have forced policymakers to reduce the budget deficit.

Obstfeld recognizes that real exchange rate fluctuations may have significant costs as well as benefits, causing inefficient migration out of sectors that are temporarily depressed and creating political pressure for tariffs or quotas that themselves are a source of inefficiency. Hence he recognizes that an argument can sometimes be made for using monetary policy to dampen exchange rate movements that reflect temporary shocks. He believes, however, that it is inappropriate to use monetary policy to resist the adjustment of the exchange rate if the shock is permanent, requiring realignment of the rate sooner or later. Furthermore, Obstfeld observes that it is difficult to know the permanence of a shock in advance. Implicitly, he accepts the pace of adjustment dictated by the foreign exchange market. He concludes that “only when it is known that a . . . shock will be reversed . . . is there a case for resisting its . . . effect.”

Obstfeld’s theoretical argument that the case for flexible rates is stronger when goods-market disturbances are more important than asset-market disturbances leads him to examine the empirical evidence about the sources of exchange rate movements. He is skeptical of attempting to use conventional exchange rate equations to infer the relative importance of monetary or real shocks, both because they tend to perform poorly out of sample and because the monetary and real “explanatory” variables in those equations are themselves almost surely endogenous. Instead, he provides an informal discussion of the major policy actions during the period and examines various other types of evidence about the relative importance of real and monetary shocks. He concludes that U.S. macroeconomic policy contributed significantly to both the dollar’s depreciation early in the period and its appreciation since 1980. He assigns a major role in determining the exchange rate before 1980 to monetary policies in the United States and abroad, but attributes much of the recent appreciation to fiscal policies. Although he believes that it probably overstates the effect, he reports a back-of-the-envelope calculation suggesting that a real appreciation of perhaps 21 percent could be attributed to fiscal policy differences between the United States and other countries.



Obstfeld turns to an analysis of the stock and exchange markets in order to provide a more quantitative insight into the relative importance of goods- and money-market shocks. He notes that the two types of shocks should have different effects on these markets and therefore that the correlation of prices in the two markets may provide some important clues. Monetary expansion in a country should raise both its stock market and the price of foreign currencies, thereby creating a negative correlation between the country's stock prices and the value of its currency in foreign exchange. In contrast, a positive shock to goods markets—such as a fiscal expansion or an upward shift in investment demand—should generally cause appreciation of the home currency while having an ambiguous effect on the stock market, depending on whether the shift raises the productivity of capital more or less than it raises interest rates. Obstfeld finds that the actual correlations are roughly consistent with his earlier description of events. The correlation has been negative in the period since 1976 for the United States but is less so in the later part of the period, suggesting that monetary shocks have been the most important overall, but that goods-market shocks have gained in importance since 1982. The story is somewhat different for Japan and Germany, with positive correlations for Japan for both subperiods suggesting a predominance of goods-market shocks.

A central question in the evaluation of flexible rates is whether the great variability in nominal and real exchange rates since fixed rates were abandoned in 1973 reflects appropriate economic signals for the allocation of resources or whether a substantial portion of the variation reflects extraneous factors, for example, speculative bubbles. Obstfeld is skeptical of empirical studies that purport to show the existence of bubbles. In particular, he observes that unmeasured changes in certain variables, such as expectations about future investment demand, can give rise to a pattern of exchange rate movements that could look like a divergent bubble to an econometrician. In the absence of more powerful econometric tests, Obstfeld argues in favor of the theoretical presumption against such behavior. Indeed, he argues that “econometric results purporting to detect divergent bubbles may be viewed more plausibly as reflecting . . . misspecification.”

Obstfeld shows that forward exchange rates or their theoretical equivalent, nominal interest rate differentials, are useless for predicting future spot rates. Some have argued that such results, typical of those

obtained by other investigators, suggest that movements in the exchange rate may be spurious and an inappropriate guide to allocation. Obstfeld argues, instead, that the most likely explanation of the results is that the dollar was swept upward by a succession of shocks that the public did not expect. Hence he does not regard the failure of forward premiums to provide information about future exchange rate movements as evidence that the movements themselves are spurious, but rather that they were caused by unanticipated events.

A recurrent theme in Obstfeld's paper is that improved policy coordination among nations could improve the current system's performance. While not all observers will agree with his conclusion that less flexible exchange rates are neither feasible nor desirable, many will agree with his support of current attempts to institutionalize a multilateral approach to policy formation.

THE DIVISION of income between consumption and saving is central to the determination of aggregate demand and output in the short run and to the nation's wealth and capital stock in the long run. It is, therefore, not surprising that few economic relationships have received more theoretical and empirical attention than has the aggregate consumption function. In spite of this attention, major questions crucial to understanding how government policy affects the economy remain unanswered. Existing theories differ significantly in their predictions of how the economic events of the last decade—from the unprecedented peacetime government deficit, to the dramatic variations in both nominal and real interest rates, to the Reagan tax cuts—should affect consumer behavior. In the third paper of this issue, Alan S. Blinder and Angus Deaton reexamine the consumption function with an eye to these crucial questions and in the hope that these "natural economic experiments" will provide empirical evidence to help distinguish among competing theories.

Blinder and Deaton estimate a series of consumption functions of increasing complexity, ranging from one including only labor income and wealth (representing capital income) as explanatory variables to others including, in addition, after-tax interest rates, inflation, the stock of durable goods, and the relative prices of durables, nondurables, and services. The authors use the regressions to address basic questions about the consumption function and to provide a baseline function that

can be used to examine various hypotheses about the way government debt and taxes affect consumption behavior.

According to rational expectations models of consumer behavior, with perfect capital markets consumption should respond only to anticipated changes in income and wealth. Whether anticipated and unanticipated changes have different effects is not just a theoretical issue; it also crucially affects predictions about the timing and speed of consumer responses to policy changes and to other macroeconomic events. When Blinder and Deaton use forecast values from a vector autoregression to represent anticipated values of income and wealth, they find consistently that only unanticipated changes in wealth are important for explaining consumption. However, the significance of anticipated income depends on which other variables are included in the equation. Anticipated income is significant when only income and wealth variables are included but loses significance when interest rates and unanticipated inflation are added as explanatory variables.

The role of the added variables themselves is often inconclusive. According to theory, higher real interest rates have an ambiguous effect on consumption, but the nominal interest rate and inflation should have coefficients of opposite sign and of equal magnitude. When Blinder and Deaton add those variables to their equations, they find a moderately significant negative effect for the after-tax nominal interest rate, but a significant negative—rather than positive—effect for inflation. Whether it is anticipated or unanticipated inflation that matters is not well determined, the statistical significance of each depending on whether relative price variables are included in the regression. When the authors disaggregate consumption, they find a significant negative interest rate effect that can be traced to housing and transportation services—not an implausible finding given that the stock of houses and automobiles is highly sensitive to interest rates. These equations make no attempt to control for special tax effects. Nevertheless, they do not suggest structural instability over the period, which might be expected if the large Reagan tax cuts had altered the historical relation between consumption and after-tax income. In particular, statistical tests reject neither the equality of the coefficients before and after 1981:3, the quarter in which the Reagan tax cuts went into effect, nor, in most cases, the equality of coefficients for the first and second half of the authors' sample period, 1954:1 to 1984:4.

A distinctive feature of the permanent income and life-cycle hypotheses of consumption is that they imply that consumers should react much less to temporary than to permanent income changes. Whether temporary tax changes affect demand is of obvious policy importance, and tax law changes themselves provide a natural arena for testing that question, since they are typically identified as permanent or temporary. Blinder and Deaton proceed by constructing two “special” income series: the first consists of the 1968–70 tax surcharge, the 1975 tax rebate, and the 1975 tax rate reductions, which they view as initially being regarded as temporary and gradually becoming recognized as permanent by 1977:1; the second consists of the temporary tax changes implicit in regarding the phased-in Reagan tax cuts of 1981–84 as a permanent tax cut starting in 1981, offset by temporary increases that declined annually between 1981 and 1984.

The results for both types of “special” income suggest that the effects of temporary changes are near zero in the first quarter; for the pre-1980 episodes they are actually of the wrong sign after two quarters and beyond. The authors note that these results, although imprecise, are not in accord with either the permanent income hypothesis or the hypothesis that consumers spend on the basis of conventional measured income. Consistent with predictions of the permanent income hypothesis, consumers do not appear to have spent a significant part of the income from temporary tax cuts in the pre-1980 episodes. On the other hand, they appear to have ignored the scheduled permanent tax reductions of 1981–84 until they actually occurred, a result that appears inconsistent with the permanent income hypothesis.

Conventional wisdom holds that households regard government debt as part of their wealth and largely ignore the effect of government dissaving (deficits) on the future tax obligations of the private sector. Robert Barro and other economists have argued that households can see through the government “veil” and will adjust their saving to offset changes in government saving. Empirical studies have given ambiguous results, but the extraordinary government deficits in recent years provide some prospect for resolving the issue. According to the Barro equivalence hypothesis, government debt should be omitted from household net worth, and household labor income should be adjusted for the government’s deficit. Similarly, household income and wealth should be adjusted for retained corporate earnings. Blinder and Deaton rerun their

equations, allowing separately for the adjustments to income and wealth implied by the equivalence view and permitting the effects of these adjustments to be freely estimated. The results are mixed, not giving a consistent verdict on the equivalence hypothesis. The estimate of the labor income variable accords with the Barro theory, while the coefficient on government debt does not. Debt appears to count fully in private wealth. The authors stress that any conclusions about consumers' responses to deficits are far from proven because, in their formal statistical tests, they are unable to reject either the extreme equivalence hypothesis or the traditional view.

In principle, the two effects, representing spending out of labor and capital income, should be combined. Accordingly, the authors simulate the net consumption response from a hypothetical temporary tax cut that leaves the government debt permanently higher. According to the equivalence hypothesis, such an event would have no effect on consumption; their estimates, however, imply increases in consumption in all save one period following the tax cut. This consumption pattern resembles the pattern predicted by the traditional view that households make no special allowance for government debt.

IN THE EARLY part of this decade, many less developed countries that had borrowed heavily from Western banks found themselves unable to meet their scheduled interest payments, and the crisis that ensued is not yet over. There was, however, a distinctive pattern to this debt crisis. Most Latin American LDCs faced immense debt problems, while most developing nations in East Asia avoided those problems and prospered. In the first report of this issue, Jeffrey D. Sachs compares the performance of countries in these two regions in order to determine what political and economic characteristics account for the difference.

Sachs considers, but rejects as counterfactual, a number of fashionable explanations of the differential performance of Latin and East Asian LDCs. One is that the external shocks of the period 1975–83 hit Latin America with greater force. It is true that the shocks differently changed the terms of trade of many countries and raised real interest rates for some borrowers more than for others. But Sachs shows that changes in the overall terms of trade actually rose slightly in both regions and were not correlated with debt problems across individual countries. Within Latin America, Brazil, Chile, and Peru suffered serious income losses

from declines in their terms of trade, while the terms of trade improved for Mexico and Venezuela. Yet all five had to reschedule foreign debts. The terms of trade worsened for Korea and Thailand and improved for Indonesia and Malaysia, but none of the four has had to reschedule debts. Real interest rates did rise more in Latin America than in East Asia, largely because Latin debt terms were more closely tied to short-term market interest rates. But as a percentage of GDP, the income loss due to higher interest costs was only slightly higher on average in Latin America than it was in East Asia; and again, it was not correlated with debt problems across countries.

Sachs also shows that the “big government” explanation for the Latin economies’ problems is not supported by a comparison of the two regions. The Reagan administration’s policy position at the International Monetary Fund has been to tie aid to tax reductions; yet taxes in Latin America are not higher than those in East Asia. Government spending is not notably larger in Latin America. And government involvement in the economy—as opposed to unfettered free enterprise—characterizes both regions.

Sachs shows that the crucial difference between the two regions is the debt service ratios they reached in the early 1980s. Using a comprehensive measure of debt service requirements—the sum of interest payments, repayment of short-term debt, and amortization of longer term debt—he shows that annual debt service as a percentage of exports rose to well over 100 percent in all the Latin countries that experienced debt crises and stayed below 100 percent in all Asian countries that did not. Once new lending dropped off in 1982, it became impossible for Latin countries with such high debt service ratios to service their debt fully. Furthermore, in lenders’ assessment of risk, the high debt service ratios themselves led to the drop-off in new lending and thus to the debt crisis.

Sachs shows that the disparity between the two regions’ debt service ratios did not arise because of unusually large past current account deficits in Latin America; deficits relative to gross domestic product in the 1970s were not very different in the two regions. However, capital flight from several Latin American countries—which occurred as the private sector accumulated foreign assets before currencies were devalued—added substantially to gross indebtedness, making debt-GDP ratios somewhat larger than those in East Asia. More important, because

exports are a much lower share of GDP in Latin America than they are in East Asia, these developments made Latin debt-export ratios substantially higher. Finally, because more Latin American debt was short-term and its interest cost was tied to rising U.S. short-term interest rates, the debt service to export ratios in Latin America became dramatically higher than those in East Asia.

Sachs offers a wide-ranging discussion of how the long-run policy posture in Latin America has discouraged the growth of export sectors. Both protectionist trade policies and exchange rate management have been biased against exports and have led to inefficient import-competing industries. Sachs argues that export expansion, accomplished in part through currency devaluation, will be needed to service debts and resume economic growth. But factions that would lose from such policies, such as workers in the import-competing sectors and enterprises that enjoy cheap foreign inputs, oppose them. This political debate is of long standing. And Sachs suggests that the relatively weak political stature of rural interests in Latin America, which would benefit from a lower-valued currency, helps explain the historical bias toward policies that discourage exports more generally. By contrast, rural interests have been politically important in most Asian economies, helping to explain the export orientation of those economies.

PRICE INFLATION has slowed substantially in the past few years, and a moderation in wage increases has been an important factor behind this slowing. In the second report of this issue, Daniel J. B. Mitchell assesses whether developments in the union wage sector point to a decline in the norm rate of wage increase in that sector that might presage an extended period of wage moderation in the years ahead. Mitchell examines two types of evidence: the quantitative behavior of union wages, including the prevalence of wage concessions, defined as freezes or reductions in wage rates, and changes in other dimensions of wage contracts, such as profit sharing, and in other labor market characteristics, for example, the degree of unionization.

Mitchell finds that, since 1981, the growth of union wages has slowed more sharply than that of nonunion wages both absolutely and relative to econometric predictions based on unemployment and price inflation. The average size of scheduled wage adjustments in all union settlements declined in each year from 1981 to 1984 despite the vigorous economic

expansion that started in 1983, and the overpredictions of union wage growth from Mitchell's wage equations remained substantial throughout this period. Furthermore, Mitchell finds that wage concessions, which were made by workers in a limited number of industries in 1981, had spread to many more industries by the first half of this year. Although concessions initially appeared in industries experiencing particularly severe competition as a result of international trade and deregulation, the broadening list of industries experiencing concessions indicates that they have spread to industries without these particular problems. Mitchell interprets these developments as evidence that wage norms have shifted down throughout the union sector.

Mitchell cites additional evidence that the balance of power between unions and management has tipped away from unions on a broad front. Union representation fell from 22 percent of private sector employees in 1980 to 17 percent in 1984. Only one-fourth of this decline can be explained by changes in the industrial mix of employment. The incidence of strikes, which Mitchell regards as an indicator of union militancy, has also been historically low in recent years, and, in a number of prominent conflicts, employers have succeeded in operating with nonunion personnel. In this and other respects, management's attitude on union relations appears to have hardened. Mitchell finds that some of these developments had parallels in the early 1960s when, according to Perry (*BPEA, 1:1980*), wage norms shifted moderately downward.

Mitchell further considers the effects on future wage developments that can be expected from two contractual features of many recent concessionary wage settlements: the substitution of bonus payments for conventional wage increases and the establishment of two-tier wage plans. Because they do not increase the base wage, bonuses raise long-run wage costs less than does a wage increase with the same immediate compensation cost. Two-tier wage plans offer newly hired employees lower wages than existing employees receive. As long as the proportion of workers in the lower tier is expanding, average wage costs will rise more slowly than the pay scale for the upper tier workers alone. Finally, Mitchell shows that cost-of-living adjustment clauses are now somewhat less prevalent than they were in the 1970s and, more important, provide proportionally smaller wage adjustments for a given increase in consumer prices.

From this array of evidence on the recent and current union scene,



Mitchell concludes that wage norms have shifted downward and, further, that contractual features that have evolved recently—the two-tier plans, profit sharing, and smaller COLAs—will modify wage cost increases associated with future changes in basic pay scales. Thus he predicts that average wage increases, in the important union sector at least, will remain moderate even in the face of continued expansion in the economy and reduced unemployment.

ATTEMPTS TO UNDERSTAND the U.S. productivity slowdown of the past fifteen years by analyzing broad economic aggregates have had only limited success. In particular, aggregate studies cannot properly allow for the possible role of variations in technological change because data relevant to measuring technological change are unavailable. In the third report of this issue, Martin Neil Baily and Alok K. Chakrabarti present an analysis at a highly disaggregated level that explores the role that a slowing of innovation may have played in retarding productivity growth.

The authors present evidence based on their own detailed data on innovations in the chemical and textile industries. The data were acquired through a comprehensive search of technical literature from the period 1967–82, and on interviews with managers, engineers, and scientists in the two industries. Innovations, in the authors' terminology, represent the last stage of the process by which the results of research reach commercial application. Research may lead to invention and patents. Some inventions, in turn, may be carried forward through a further stage of research and development. If and when a new product or process emerges from this stage and is introduced commercially, it qualifies as an innovation. And it is at this time that it can be expected to begin to have an effect on productivity.

Baily and Chakrabarti show that capacity utilization has had a substantial effect on productivity in the chemical industry because production is highly capital-intensive and involves major overhead costs. They show that, after adjustment for capacity utilization, productivity growth slowed sharply from 1965–73 to 1973–79 and then recovered somewhat in 1979–83. When they analyze their innovations data, Baily and Chakrabarti find that the number of innovations in the chemical industry correlates quite closely with this pattern of adjusted productivity growth. The authors distinguish four types of innovations—products, processes, equipment, and instruments—and find that the average yearly

number of innovations in each category was slowest in the middle period. When they further subdivide process innovations into those related to productivity, environmental requirements, and energy saving, the productivity-enhancing innovations are clearly weakest in the middle period. Furthermore, the decline after 1967–73 in innovations regarded as important by experts is even more noticeable. The authors also find exceptions that support their general findings. Interviews with employees of 3M, a specialty chemical company whose production methods are not as capital-intensive as those of the bulk chemical producers, revealed that the company experienced no slowdown in innovation, no slowdown in productivity, and no excess capacity.

In the textile industry, where equipment innovations are particularly important to productivity, Baily and Chakrabarti find little significant variation in such innovation over the three periods. The same is true for productivity-enhancing process innovations. These findings again correspond roughly to their estimate of adjusted productivity growth in textiles, which varied much less than productivity growth in chemicals.

The authors report that their interviews with industry specialists generally supported the finding from the innovations data that productivity was closely linked to innovations. At least some specialists also rejected the often heard conjecture that labor quality, work effort, or other labor issues had been important in productivity variations. Baily and Chakrabarti thus appear to have established the significance of the link between innovations and productivity in the two industries for which they have data, evidence that suggests that innovations may have been important in explaining economywide productivity performance over the past fifteen years.