Editors' Summary

THE BROOKINGS PANEL on Economic Activity held its seventy-first conference in Washington, D.C., on March 29 and 30, 2001. This issue of *Brookings Papers on Economic Activity* includes the papers and discussions presented at the conference. The first paper investigates the causes of the Russian financial crisis of 1998 and evaluates the emergency international effort that was undertaken to avert the crisis. The second paper studies the effect of interstate wage differences on the location decisions of immigrants and estimates the resulting gains in macroeconomic efficiency. The first report analyzes whether the decreased frequency of recessions in the past two decades can be attributed to a secular decline in the variability of quarter-to-quarter changes in output. The second report examines the usefulness of the Index of Consumer Sentiment as a tool for forecasting recessions. The issue concludes with a symposium of three papers on the sustainability of the recent large current account deficits in the U.S. balance of payments. Each takes a somewhat different approach to understanding these deficits. The first symposium paper sees U.S. assets as attractive investments that are likely to perpetuate large capital account surpluses. The second focuses on entrenched global demand for the dollar as international money. The third paper views capital flows in terms of a country portfolio model in which investors at home and abroad increase or reduce their holdings of U.S. assets in response to changes in wealth and in risk-adjusted returns.

IN THE SUMMER OF 1998 the flight of capital from Russia in the face of failed attempts by the International Monetary Fund (IMF) and others to stabilize the ruble caused a crisis that reverberated throughout the world's financial markets. The IMF had sought to avoid the inflation and financial market disruption that a sharp devaluation might bring and to encourage

and facilitate fiscal and structural reforms within Russia. When the effort failed, inflation soared, output declined, and the reformist government of Prime Minister Sergei Kirienko fell. The Russian meltdown was followed by sharp declines in stock markets worldwide; it also led to massive losses at Long Term Capital Management, a large U.S.-based hedge fund, whose potential failure U.S. authorities and other knowledgeable observers saw as a threat to the financial system. In the first paper of this issue, Homi Kharas, Brian Pinto, and Sergei Ulatov analyze the Russian crisis and the response of the international community.

The authors first review the economic situation in Russia in the years leading up to the crisis. The policy of stabilizing the exchange rate around a crawling peg had helped bring annual inflation down from 200 percent in 1994 to near 10 percent in 1997. But otherwise performance had been dismal. National output, which had fallen by 40 percent between 1990 and 1995, continued to stagnate or decline over the next two years. And Russia's financial situation worsened after the East Asian crisis started in the summer of 1997. In the first half of that year, foreign investment in Russian equities and government short-term debt had risen sharply, raising the foreign reserves of the Central Bank of Russia (CBR) to $25 billion by July. But by the fall, some of the capital inflows had reversed and interest rates on GKOs (short-term government debt instruments) were rising. Under pressure from the IMF, the government formulated a new Fiscal Action Plan designed to increase tax collection and control expenditure. In subsequent months such commitments to reform would be repeated in the face of growing skepticism about the government's ability to deliver on them. And by the late spring of 1998, in an environment of rising interest rates and falling oil prices, the third and final bout of instability began, coincident with the crisis in Indonesia.

The international financial community reacted forcefully. In June 1998 the IMF released a $670 million tranche of a previously negotiated loan, and in July an IMF-led rescue package totaling $22.6 billion was announced, with funds committed mainly from the IMF and the World Bank but including $1.5 billion from the government of Japan. Only about one quarter of this scheduled total was actually disbursed before the ruble collapsed. The July package included requirements for fiscal and structural reforms and was accompanied by a plan for a market-based debt swap of GKOs, whose interest rates had soared to over 100 percent in July, for dollar-denominated Eurobonds. The Russian government had already used

Eurobonds rather than GKOs to raise financing, having sold two issues in June.

The prospects for success of the support package and the debt swap were closely interrelated. It was crucial that the support package, with its massive financial support conditioned on fiscal and structural reform, restore investor confidence. If it did, capital flight and pressure on the ruble would abate, avoiding faster inflation, reducing ruble interest rates, and buying time for the implementation of reforms. Additionally, the reduction in government borrowing costs, arising both from lower ruble interest rates and from the swap to dollar-denominated debt, would improve the fiscal outlook, helping justify investors' continued confidence. But the effort failed. Capital outflows continued, and Russian banks with foreign exchange exposure came under pressure. On August 17, 1998, the government devalued the ruble, restructured its ruble debt, and imposed a ninety-day moratorium on private foreign principal payments. Three weeks and $3 billion in lost reserves later, Russia floated the ruble, which by the fall of that year was fluctuating around a rate roughly one-third its previously pegged value.

The authors inform their analysis of the Russian crisis with reference to two recent types of model of speculative attacks on a currency. First-generation models emphasize unsustainable policy fundamentals, which lead to speculative attacks once it becomes apparent that the reserves available to support an overvalued currency are inadequate. Second-generation models emphasize the possibility of multiple equilibria and shifting market expectations. In these models, even if the currency is not overvalued, sudden capital outflows precipitated by a shock to market confidence can cause a self-fulfilling crisis that shifts the exchange rate to a new, lower equilibrium. The authors recognize that, in any real-world situation, confidence and the fundamentals are intertwined, but they emphasize a first-generation explanation for the Russian crisis, focusing on a fiscal situation and an exchange rate that they conclude were unsustainable.

The authors see Russia's fiscal dynamics in the years immediately preceding the crisis as driven by the interaction of three main factors relating to both policy and performance. First, the crawling peg exchange rate that was used to slow inflation in the three years preceding the crisis allowed a 40 percent real appreciation of the ruble. The resulting overvaluation together with high real ruble interest rates depressed output and

the competitiveness of domestic enterprises. Second, the government was unable to collect sufficient revenue, both because of the output depression and because of the "nonpayments system"—growing tax arrears and the increasing use of nonmonetary exchange within the business sector—that arose to prop up otherwise failing enterprises. Third, the government took on new, dollar-denominated debt to augment its reserves, a strategy that added to the financial risk of the government and the banking system.

For a time, the relatively low interest rate that was paid on foreign currency–denominated debt, which in 1995 represented 76 percent of total government debt, alongside still rapid domestic inflation, kept the debt-to-GDP ratio from rising despite large fiscal deficits. But this situation, which the authors regard as "one-shot gains from a real appreciation," could not continue without fundamental improvement in the fiscal balance. The authors estimate that by the spring of 1998, with a target fiscal deficit of $25 billion of which $19 billion would be financed with ruble debt, the weighted real marginal interest cost of government borrowing would be 27 percent—a rate that would lead to explosive debt growth.

The authors point to rising market interest rates during the immediate precrisis period as direct evidence of the increasing risk that investors saw in Russian debt. The spread on new issues of Russian Eurobonds over U.S. Treasury securities rose from 375 basis points to 753 basis points between June 1997 and June 1998, and to 940 basis points by the time of the July 24 swap of Eurobonds for GKOs. Domestic interest rates were rising even more sharply. To quantify the markets' changing evaluation of risk on ruble securities, the authors use a model that equates the one-year GKO rate to the sum of four elements: the one-year U.S. Treasury rate (which represents the risk-free rate); the target rate of ruble devaluation; default risk, measured by the yield difference between dollar-denominated Russian Eurobonds and U.S. Treasuries; and devaluation risk, the residual difference between the GKO yield and the other three elements. They show that both default risk and devaluation risk rose in the fall of 1997, after the spread of the Thai crisis to other Asian markets. Both rose sharply again starting in June 1998. On August 14, the last business day before the devaluation, the yield on GKOs reached 145 percent, of which the authors attribute 24 percentage points to default risk and 110 percentage points to devaluation risk.

The authors see the increasingly precarious position of the major Russian banks as contributing to the timing of the crisis. The banks, which had

been favored in the reorganization of the economy earlier in the decade, were vulnerable to a devaluation, both from their own foreign currency exposure and from capital losses they would incur on their holdings of government debt. The authors suggest that some market participants may have expected that the government would find a way to forestall a crisis until the banks' exposure had been reduced. However, when the markets did not respond favorably to the swap of Eurobonds for GKOs, the banks were forced to liquidate both ruble- and dollar-denominated assets, depressing their prices and adding to their perceived riskiness, thus helping precipitate the crisis.

Even with hindsight, it is difficult to judge what chance of success the IMF-led program had ex ante. The authors recognize that uncertainty will always surround attempts to stabilize volatile markets, especially in economies like Russia's, which are highly vulnerable to shifts in international capital flows. However, they observe that the very crisis the IMF and others had sought to avert led to conditions that ultimately fostered recovery. The crisis forced the long-postponed fiscal reforms by suspending the government's access to borrowing, it accomplished a real devaluation that improved the profitability of domestic industry, and it reduced the real value of public debt through inflation. The authors suggest that, in the Russian case, it might have been preferable to deliberately change the exchange rate and reach an agreement with creditors ahead of a crisis, rather than take the risk that the markets would force the same actions at a higher cost, as they eventually did.

AFTER SEVERAL DECADES OF relatively low immigration in the mid-twentieth century, immigration has again become an important source of growth in the U.S. population. Legal immigrants who arrived in the United States between 1990 and 2000 make up about 4 percent of the current population and are responsible for one-third of the increase in the population over that period. In addition, nearly 4 percent of the current population are naturalized citizens. Perhaps the most important effect of immigration is that it enriches the nation's cultural diversity. But it also has significant effects on the economy, affecting the average age, skill endowment, and geographical distribution of the labor force. The consequences for the wages and welfare of the native population are matters of controversy. Increases in the supply of particular skills due to immigration may depress the wages of native workers with the same skills but may also

increase the supply of much-needed labor in particular occupations or regions and will in general raise the returns to capital. In the second paper of this issue, George Borjas investigates the determinants of the location choices and mobility of immigrants and analyzes how these choices affect the returns to native capital and labor.

Labor market efficiency requires that the value of the marginal product of labor be equalized across labor markets in the country's various regions. But even in the United States, with its highly developed capital markets and relatively flexible labor markets, the convergence of incomes across regions is quite slow. Indeed, available evidence suggests that it takes roughly thirty years to eliminate half of initial income differentials across states. Although interstate wage differentials provide an incentive to workers to move, there are substantial impediments to their doing so. These include not just the costs of moving family and possessions, but also the incremental costs of searching for a job, selling a home and buying a new one, changing schools and doctors, and the cost of having to leave extended family members behind. Capital flows across localities help equilibrate the national economy, but because moving existing physical capital is expensive, the adjustment of capital stocks depends largely on new investment and therefore can take many years.

Given the slow and costly adjustment of both native workers and capital, immigrants can play an important role in equilibrating labor markets. Immigrants, in contrast to native workers, have already made the decision to incur the costs of relocation, and if their objective is to maximize their income, the differences in costs and benefits across different regions in the United States will determine where they settle. On the hypothesis that immigrants are drawn to those regions that offer them the highest wages for their skills, they can be credited with speeding the elimination of wage differentials and so reducing the need for the costly migration of natives.

Borjas observes that the hypothesis that economic incentives play an important role in immigrants' location decisions has a number of testable implications. According to this hypothesis, the observed geographic distribution of immigrants should differ from that of natives, even after controlling for differences in age, sex, and education. Immigrants should tend to be more tightly clustered in those states that offer them the best economic opportunities, and if the pattern of wage rates across regions differs by skill class, immigrants with different skills should be found living in different regions. Moreover, because economic opportunities across

regions change over time, and because immigrants that have settled face the same relocation costs as do natives, workers who arrived in earlier immigrant waves should be found living in different states than the newest immigrants. Recent movers, on the other hand, whether native or foreign born, should be sensitive to interstate wage differentials in the same way as new immigrants.

Before undertaking his empirical investigation, Borjas provides a theoretical analysis of how immigration and the location decisions of immigrants affect the returns to domestic capital and labor. He assumes a competitive model with a single final product produced jointly by capital and labor under constant returns to scale. The model has two regions, each with the same production function. Using this model, Borjas first analyzes the short-run impact of immigration, taking as given the capital stock and native labor supply and their distribution across regions. In a competitive market, wages equal workers' marginal product; thus the last immigrant to enter a particular region's labor force receives the entire increase in output that he or she generates.

However, a finite increase in a region's labor supply, given fixed capital, decreases the marginal product of labor and hence the wages of inframarginal workers, including earlier immigrants, and increases the marginal product of and total returns to capital. Because inframarginal immigrants also find their wages lowered by other immigrants, the gain to capital from immigration equals the sum of the losses to native labor and inframarginal immigrants. In models like Borjas's, which take the capital stock as given, the redistribution between capital and labor is substantial, and most of labor's loss is borne by native labor.

How do the specific location decisions of immigrants affect the magnitude and distribution of gains and losses, taking domestic factors as fixed? To illuminate the answer, Borjas assumes an identical production function for each region, with a linear marginal product of labor schedule. Under this assumption, convenient triangular estimates of losses and gains are exact, and Borjas provides formulas giving the effects on aggregate output and its distribution, as a function of the magnitude of immigration, the location of new immigrants, and the parameters of the production function. The results are intuitive: Aggregate output and the incomes of immigrants are maximized if immigrants cluster in the high-wage region. The net gains to native factors—the higher returns to capital minus the lower wages to native labor—constitute the increase in aggregate output not

captured by immigrants. These net gains are largest when immigrants cluster in one region, whether it is the high- or the low-wage region. This reflects the fact that the inframarginal effect of immigrants, depressing the wages of earlier immigrants, is greatest when immigrants cluster. The net gains to domestic factors are smallest when immigrants distribute themselves evenly across the two regions. These results generalize beyond the linear marginal product case but have to be qualified if the elasticities of the marginal product schedules differ across regions.

Even though the short-run gains to the native population are the same when immigrants cluster in the low- rather than the high-wage region, immigrants' location choices are not a matter of indifference to natives. If, in the long run, native labor migrates so as to eliminate regional differences in wages, the long-run level of aggregate output and wages will not depend on where immigrants initially locate. But the costs incurred by natives relocating will obviously be less if immigrants originally locate in the regions where wages are high. If the quantity of immigration is sufficient to eliminate the initial wage gap, the economy will be in equilibrium without movement of domestic factors, and the relocation costs of natives will be saved.

Most existing estimates of the effects of gains from immigration have ignored these regional differences in wages and the way the location choices of immigrants affect the welfare of natives. A complete accounting of the gains from immigration requires quantification of the effects just described, to which Borjas turns next. He takes as a baseline the level of native income that would initially result if immigrants replicated the geographical sorting of the native population. He refers to the difference between this baseline and the gains to natives as the efficiency gain from immigration—the sum of the benefits from immigrants clustering and those from the reduction in natives' relocation costs.

Of course, it is not obvious that immigrants' location decisions are determined solely by wage incentives. They could be strongly influenced by historical settlement patterns or by proximity to their point of entry. To explore these possibilities, Borjas turns to an empirical examination of the link between interstate wage differentials and the location decisions of immigrants and natives, using data from the 1960–90 Public Use Microdata Samples of the decennial census. He classifies as immigrants those individuals born abroad who are either noncitizens or naturalized citizens; all others are classified as natives. He looks at five skill groups

defined by years of educational attainment. To obtain the time series of standardized wage rates for each educational group by state, he estimates separate wage equations (in logarithms) for each of the census years 1960, 1970, and 1980, controlling for age and sex. These skill-standardized wages are normalized so that they can be interpreted as the wage differential, in percent, between the wage of a given state-educational group and the mean wage in the United States in the same period.

The resulting wage differentials vary widely across states for a given educational group. For example, in 1980 a college graduate living in Nevada, a state at the 80th percentile of the earnings distribution, would on average expect to earn 17 percent more than if he or she lived in Wyoming, at the 20th percentile. Borjas also finds that, although some states tend to pay high wages and others low wages, the correlation of wages by education across states is far from perfect. Relative wages across states also vary substantially over time: the correlation between 1960 wages and 1980 wages varies from 0.75 to 0.92, depending on the education group. These variations allow Borjas to identify the response of immigrants in his formal analysis.

Immigrants have tended to cluster in a relatively few states. In 1990, 74 percent of immigrants who had arrived in the United States within the past five years lived in one of six states. This clustering might suggest that social networking and other nonwage considerations are important to their location decisions. However, Borjas finds that immigrants with different skills tend to live in different states and that where they settle has changed over time. Although fewer than 20 percent of immigrants who were high school dropouts lived outside of the six main immigrant-receiving states in 1990, almost 40 percent of immigrants with a college degree did so. These facts leave considerable room for economic factors in explaining location choice.

Borjas proceeds to a more formal examination of the hypothesis that the relative supplies of immigrants and natives in various states depend on interstate wage differentials. He measures relative supply by the fraction of all immigrants in a particular education group arriving in a given five-year period that live in a given state, divided by the fraction of natives from that education group residing in that state. This relative supply index is calculated for 1965, 1975, and 1985, the midpoints of the decades for which Borjas has constructed state wage differentials. Simple tabulation strongly suggests the importance of wages in determining immigrants' location

choices. In 1980, for example, the index expressing the average supply of new immigrants relative to native workers in the five states with the highest wages is 2.8, whereas that in the five states with the lowest wages is 0.2. Borjas also shows that the newest immigrants are overrepresented not only relative to the native population but also relative to earlier immigrants, defined as those who had been in the United States for at least five years before the date of the wage index. This measure of the relative supply index varies from 1.2 for the highest-wage states to 0.5 for the lowest, suggesting that although networking with older immigrants may matter, purely economic considerations are also important.

These cross-sectional results may be contaminated if immigrants are attracted to particular states because of unobserved characteristics that are correlated with wage differentials. The availability of wage and relative supply information for three different time periods makes it possible to control for such effects. Borjas does so in regressions of decade-to-decade changes in the relative supply index on changes in state-education wage differentials with and without fixed effects for skill level, state, and time. Initially, these regressions assume the same response of supply to wage differentials across skills and time. The regressions are run for four relative supply measures: immigrants relative to natives, earlier immigrants relative to natives, new immigrants relative to natives, and new immigrants relative to earlier immigrants.

The results generally support Borjas's hypothesis. Compared either with natives or with earlier immigrants, new immigrants have a relative supply elasticity of 1.3. Earlier immigrants appear no more responsive to wage differentials than do natives. Estimating the equation separately for the 1960–70 and 1970–80 periods makes relatively little difference to the qualitative results, although the results are stronger for the 1970–80 period. Borjas also investigates the relationship separately for men and women and finds that women have a weaker supply elasticity, consistent with the view that the location decisions of female immigrants are more heavily influenced by family considerations. Borjas is aware that wage growth may be correlated with the error in the supply equation, but his attempt to use the previous decades' wage growth as an instrument is not very successful, giving unstable results and large standard errors.

Borjas examines the possibility that the results are dominated by California, where immigrants may cluster because it is close to the Mexican border, or by Mexican immigrants irrespective of their location. He finds

that the results are insensitive to excluding either. Consistent with that finding, separate regressions for immigrants from Europe, Asia, and Latin America give similar results. He also finds that natives who choose to move are more sensitive to wage differentials than earlier immigrants, but less sensitive than newly arrived immigrants.

The finding that new immigrants are significantly more responsive than natives to wage differentials suggests that the rate of wage convergence will be faster among those skill groups and in those periods that experience high levels of immigration. To test this implication, Borjas first estimates wage convergence equations for each education group in each of four decades: 1950–60, 1960–70, 1970–80, and 1980–90. His results are roughly consistent with earlier estimates on wage convergence, with sixteen of the twenty time-education coefficients having the expected negative sign, and with the mean of the convergence coefficients suggesting a half-life of interstate wage differentials of approximately thirty-five years. He then runs a second-stage regression, explaining the estimated convergence coefficients by the ratio of the number of new immigrants relative to the native population for the particular education group. The results show a strong and statistically significant relationship between convergence and immigration. In the 1980s, when immigration increased the supply of labor by nearly 10 percent, the coefficient estimates imply that immigration roughly doubled the rate of convergence.

How large are the benefits derived from immigrants clustering in high-wage regions rather than replicating the geographic distribution of natives? The answer obviously depends on how rapidly natives respond to wage differentials. If natives' migration costs are low and natives respond rapidly to wage differentials, wage gaps will be short lived and eliminated at low cost, even in the absence of immigration. In this case the efficiency gains from immigrants seeking out high-wage regions will be small. If instead native migration costs are high and the response of natives is slow, the benefits of immigration will be much greater. To arrive at a back-of-the-envelope estimate of these benefits and clarify the importance of various parameters to the results, Borjas analyzes a simple, two-region model roughly calibrated to represent the United States. The model parallels the earlier static model, but rather than assuming that native labor and capital are fixed, it is assumed that native migration costs are quadratic in the rate of internal migration. In both regions Borjas assumes the same constant elasticity of output with respect to labor, and the same corre-

sponding constant elasticity of the wage with respect to labor. In subsequent simulations these elasticities are assumed to be 0.7 and 0.3, respectively, corresponding roughly to a Cobb-Douglas characterization of the U.S. economy with equal capital in the two regions. Consistent with the assumption of quadratic costs of internal migration, the log difference in the regional labor supplies changes in constant proportion to the log difference in regional wage rates. Given this model, Borjas is able to derive expressions giving the time path of employment and wages as functions of the initial distribution of native labor and the quantity of immigrants and their choice of location.

Armed with these analytic results, Borjas simulates the model roughly scaled to the U.S. experience, assuming that there are 100 million native workers, that preimmigration GDP is $10 trillion, and that immigrants increase the labor supply by 10 percent. A real rate of interest of 3 percent a year is used to discount the stream of costs and benefits. Borjas simulates the models under a variety of assumptions about the initial distribution of natives and the attendant wage differential, the magnitude of migration, the location choices of immigrants, and the cost of internal migration. If the initial distribution of natives implies a wage gap of 12.2 percent, and immigrants are sufficient in number to close half of that gap when they all locate in the high-wage region, the efficiency gains are modest even with relatively high costs of internal migration. The net gains to native factors are $13.2 billion a year if native workers are completely immobile, and only $7.9 billion if native workers are mobile but migration costs are twice the annual income of the average worker. If migration costs are only half a year's income, the annualized net gains to native factors fall to $4.7 billion. Doubling the size of the initial wage differential roughly doubles these efficiency gains. Although migration costs of two years' income seem substantial, Borjas observes that the estimates using these costs eliminate wage differentials much more rapidly than the roughly thirty-year half-life that other studies have typically found. His model would require migration costs of three times the average worker's income to obtain that slow a speed of adjustment.

Borjas concludes that taking account of how immigrants respond to wage signals adds importantly to our understanding of the impact of immigration on the U.S. economy. The efficiency gains he estimates are in addition to the conventional estimates, which do not account for the benefits of immigration clustering in high-wage regions. Although his estimates of the

efficiency gain may seem trivial in a $10 trillion economy, they roughly double the conventional estimates.

SINCE 1982, THE GROWTH OF THE U.S. economy has been interrupted only once by recession, in 1990–91. That downturn ended a thirty-one-quarter expansion and was followed by the current expansion, now forty quarters old and already the longest on record. Many observers have attributed this sustained growth to some combination of wise policy, especially monetary policy, and good luck—the absence of large negative shocks like those that plagued the economy in the 1970s, and the positive productivity gains from the revolution in information technology. In the first report of this volume, Olivier Blanchard and John Simon hypothesize that something else has been at work: a long-term decline in the underlying volatility of output that, by itself, is sufficient to account for the increased duration of expansions. After quantifying this underlying decline in output volatility and its implications for the length of expansions, Blanchard and Simon examine its proximate causes and relationship to inflation volatility and compare the U.S. experience with that of the other Group of Seven (G-7) large industrial economies.

Blanchard and Simon show that the standard deviation of quarterly real output growth, measured over overlapping twenty-quarter intervals or "windows," declined from about 1.5 percent a quarter in the early 1950s to less than 0.5 percent a quarter in the late 1990s, although this decline was interrupted by an increase in volatility from the late 1960s to the mid-1980s. These simple rolling measures of the time variation in output volatility are suggestive. But they are vulnerable to the complaint that they do not take account of variation over time in the economy's trend growth or in systematic features of output dynamics. To account for these other influences, Blanchard and Simon examine volatility within a more sophisticated framework.

With each twenty-quarter window providing one observation, they estimate a first-order autoregressive (AR) process for the deviation of output from trend. The rolling window allows time variation in the standard deviation of the residuals, which they interpret as exogenous shocks, and in the trend growth rate and the AR coefficient. While acknowledging that the assumption that the output process is first-order does not fully capture the economy's dynamics, the authors report that adding more lags to the AR process does not alter their conclusions; they therefore use the first-order

AR specification in order to greatly simplify the presentation and interpretation of their results. They find that the time pattern of the standard deviation of the regression residual is nearly identical to the time pattern of the volatility of output itself. Although the AR coefficient is slightly lower at the end of the 1990s than in the rest of the sample, the difference is insignificant, and its variation contributes little to the changes in output volatility. Their estimated trend growth rate of output does vary substantially, and statistically significantly, over the period, but it ends the period roughly where it starts. It thus appears that the decline in output volatility comes from a smaller standard deviation of shocks to the output process, rather than from changes in the other two parameters.

For given parameter estimates and a given distribution of shocks, Blanchard and Simon's stochastic process for output predicts a particular distribution of the lengths of expansions. To see how nearly such predictions capture actual experience, they estimate the parameters for two periods, 1947–81 and 1982–2000, thus splitting the sample at the peak of the cycle preceding the last two expansions. The estimates of the expected trend growth rate are virtually identical for the two periods, but the standard error of the shocks in the later period is only half as large as that in the earlier one. This by itself suggests a halving of the volatility of output around trend in the later period, an effect that is slightly offset by an increase in the AR coefficient. To determine the implications of these changes for the expected length of expansions, the authors simulate the model for output using the parameters for each period. For each period they draw 100,000 shocks from a distribution using the standard deviation of shocks from that period. They then use these simulations of output to calculate the median and mean expansion length that the model predicts.

Blanchard and Simon define the beginning of a recession as two consecutive quarters of negative growth following an expansion, and the beginning of an expansion as two consecutive quarters of positive growth following a recession—the traditional approximation to the official dating by the National Bureau of Economic Research (NBER). They find that their model replicates the observed lengths of expansions for the two halves of their sample reasonably well. For example, the simulations give a mean expansion length of seventeen quarters in the earlier period, compared with an actual mean of nineteen quarters, and a mean expansion of fifty-one quarters in the later period, compared with an actual mean of thirty-six. As expected, the decline in the standard deviation of shocks is

the dominant reason for the change in expected expansion length. Repeating their simulations with the volatilities for the two periods switched generates longer expected expansions in the earlier period (ninety-nine quarters rather than seventeen) and shorter expected expansions in the second (fourteen quarters instead of fifty-one). Switching the AR coefficients between periods is less important and indeed works in the other direction: the larger coefficient from the second period shortens the expected expansion length because an output shock persists longer, making it more likely that output will decrease two quarters in a row. Switching trend growth rates makes essentially no difference, because the estimated rates are nearly the same in the two periods.

The authors recognize that if recessions are usually caused by large, infrequent events like the OPEC oil shocks or the Volcker disinflation, the reduction in volatility they estimate may simply reflect an absence of large shocks in the last few years. In that case their characterization of the output process may be misleading. Blanchard and Simon examine this possibility in two ways. First, they reestimate the rolling regressions to include a dummy variable representing quarters of NBER-dated recessions. They find that removing a large fraction of the output variability during recessions in this way does not qualitatively change their results: the standard deviation of output shocks still falls substantially, from roughly 1.2 percent a quarter at the start of the sample to 0.4 percent a quarter at the end. Second, they look at the distribution of the residuals from their rolling regressions to see whether there is direct evidence of two kinds of shocks, one frequent and small, the other infrequent and large. If there were these two kinds of shocks, one would expect the distribution of shocks to exhibit skewness or excess kurtosis, or both. Yet except for a brief period during the 1980 recession, they find little evidence of either. The authors also estimate a model where a large shock occurs with a certain probability, and they find they cannot reject the null hypothesis that the probability is zero, nor can they find evidence that it has declined over time.

The time variation in output volatility, and the parallel movements in the standard deviation of their estimated shocks, could be regarded as a declining trend over the entire period, interrupted by events in the 1970s and early 1980s. Or it could be viewed as a structural change that abruptly shifted volatility down in the early to mid-1980s, reflecting, as some have suggested, improvement in monetary policy or changes in inventory behavior. The authors suggest that the first interpretation is more likely to

be correct. In the 1970s the economy experienced major increases in raw materials prices, most notably oil. These price increases and the ensuing inflation, and the Volcker disinflation that ended it, are natural candidates for explaining the increases in output variability. The period of high inflation in the 1970s and early 1980s did, of course, correspond to the period of high output volatility. But in the 1950s and early 1960s, inflation was low and output volatility was high. Inflation volatility, also measured using a twenty-quarter rolling window, correlates better with output volatility, since both are high at the start of the period. A regression of output volatility on a time trend, mean inflation, and inflation volatility shows all three to be significant.

Blanchard and Simon recognize that correlation is not causation, and that the correlations among inflation, inflation volatility, and output volatility may reflect the influence of another factor such as the supply shocks of the 1970s. To help sort this out, they look at the experience of the other G-7 countries, because if supply shocks are common across countries, they can at least partially control for them by using fixed effects in cross-country panel regressions. They first note that the experience of most of the G-7 countries is much like that of the United States. Although the volatility of output growth varied from 1.5 percent (in Germany) to 1.0 percent (in the United States) in the early 1960s, volatility has declined significantly in almost all the G-7 countries over the past forty years. Today their volatilities are strikingly similar, clustering around 0.5 percent, except in Japan, where the standard deviation of output growth, having fallen to similarly low levels in the late 1980s, is now higher than it was at the start of the sample. Since the rise largely coincides with Japan's ongoing slump, the authors speculate that decreased liquidity among consumers and firms may have led to stronger responses to cash flow variations, and that the zero floor on nominal interest rates may have made monetary policy ineffective in offsetting shocks. They do not pursue these explanations, but they suggest that Japan is a potentially useful case for learning about the factors that influence volatility in economies in prolonged slumps.

Setting Japan aside, the authors estimate a panel regression relating each country's output growth volatility to its mean inflation rate and inflation volatility, including time and country fixed effects. Each variable is time varying, as measured by the same twenty-quarter window used in the U.S. analysis. If the effects of the supply shocks are indeed common

across countries, this specification will give the relationship between output volatility and the inflation variables, after controlling for the supply shocks. The authors recognize that such common shocks are not likely to have identical effects on output and inflation across countries, but they regard this specification as a significant improvement on the regression for the United States alone. They also recognize that the panel regression does not resolve the issue of the direction of causation, because output growth volatility may cause inflation volatility and because other country-specific factors may affect both. The estimates do suggest that the level of inflation is unimportant and that there is a strong and highly significant relationship between the volatility of inflation and the volatility of output growth. The authors show graphically that the panel regression does quite a good job of explaining U.S. output volatility, and that its estimated time path resembles a regular trend decline once the effects of U.S. inflation volatility and the G-7 time dummies are removed.

At least since Arthur Burns's 1960 presidential address to the American Economic Association, it has been suggested that structural changes may be under way in the economy that would make for a trend decline in output volatility. Burns himself mentioned the change in the composition of output (the shift toward services), improvements in capital markets, and automatic stabilizers like the income tax as factors tending to reduce volatility. The authors explore this line of thought by decomposing GDP into consumption, investment, government spending, net exports, and inventory investment, to see how important changes in the volatility of individual components, the correlation of components, and the relative importance of components with different volatilities are to the aggregate story.

The authors compute rolling standard deviations of the rate of growth of each component times its share of GDP, thereby weighting each component's variation by its importance in the aggregate economy. Weighting by shares is equivalent to using changes in each component divided by total output and avoids the difficulty, which arises for inventory investment and net exports, of extreme observations when their level is near zero. Although the weighted volatilities of net exports and inventory investment are quite large—indeed, as large as for any other component in the latter part of the period—the authors do not find a clear trend in either. The volatility of government spending was very high during the Korean War but has been low ever since. Hence most of the trend decrease in aggregate

volatility can be traced to decreases in the volatility of consumption and investment. Consumption volatility shows a large decrease in the 1950s and has been halved since the mid-1960s; investment volatility was much lower in the 1990s (and briefly in the mid-1960s) than it was for most of the period. The importance of these declines leads the authors to further disaggregate consumption into durables, nondurables, and services, and investment into residential and nonresidential. The volatilities of all of these components show roughly similar behavior over time, beginning the sample quite high, declining sharply through the mid- to the late 1960s, rising in the 1970s and most of the 1980s, and declining to historic lows during the second half of the 1990s. There are some differences in timing.

The standard deviation of output depends not only on the standard deviations of its components, but also on their correlations. The authors compute time-varying correlations of changes in each component with final sales, deflated by total output. There is a good deal of time variation in each correlation, but except for the correlation between inventory investment and sales, no clear trends emerge. Until the mid-1980s inventory investment tended to move with sales, amplifying the variance of production relative to sales. Since then inventory investment has become countercyclical, stabilizing output. This decline in correlation is clearly one of the factors behind the decrease in output volatility. However, it is somewhat puzzling because it occurred when improvements in the tracking and forecasting of sales, which should have allowed firms to maintain more stable inventory-to-sales ratios, might have been expected to result in more, not less, procyclical inventory investment.

Changes in the composition of output have been substantial over the last fifty years and might be expected to explain a substantial part of the decline in aggregate output volatility. But the authors find that these changes roughly offset each other. For example, although the share of relatively volatile nonresidential investment increased significantly between 1950 and today, so did the share of low-volatility consumption of services. The volatility of a counterfactual series for output growth using the 1947 shares of the various components is nearly identical with the volatility of actual GDP growth. The authors conclude that composition effects have had little to do with the general pattern of output volatility over the last fifty years.

If Blanchard and Simon's model of expansions and recessions is correct, recent trends augur well for the stability of the U.S. economy. Not

only is the trend in output volatility at a historically low level, but the economy's trend growth rate is apparently near the highs of the 1950s and 1960s. According to their model, the combination of modest shocks and a rapid underlying trend output growth rate should mean that expansions as long as or longer than the present one will be the norm rather than the exception. On the other hand, if large shocks such as those experienced in the 1970s arise from sources outside the model, the estimated decline in volatility may be misleading, and such promising predictions could be disappointed. The authors' results, which indicate that high inflation volatility has been accompanied by high output volatility, also suggest that a change in the benign inflation environment of recent years would raise the risk of a new recession.

DURING THE LONG ECONOMIC EXPANSION that started in the early 1990s, consumer spending has been a major source of rising demand, with the consumer saving rate declining to a historic low in 2000. In recent months, concerns that the economic slowdown would turn into a recession have centered on the possibility that consumer spending might weaken significantly. That risk is heightened by the decline in the stock market, which many analysts believe could reduce consumers' spending, both by reducing their wealth and by dampening their optimism. In this environment both policymakers and private forecasters have looked to evidence about consumer sentiment for signs of where consumer spending is heading. Two popular indicators—the Conference Board's Index of Consumer Confidence and the Index of Consumer Sentiment (ICS) produced by the Survey Research Center of the University of Michigan—provide monthly survey-based evidence bearing on consumer attitudes. In the second report in this issue, Philip Howrey provides an innovative statistical analysis of the usefulness of the ICS for forecasting.

Many previous studies have analyzed the contribution of the two survey measures to greater accuracy in forecasting GDP. These studies generally find that measures of consumer attitudes provide only a small, albeit statistically significant, improvement in forecasts once other economic variables are taken into account. Howrey explores a somewhat different question. Noting that recessions are periods of special interest, with unemployment rising substantially and with output dynamics possibly driven by atypical responses of economic agents, he devises a method for using the ICS, alone or in conjunction with other variables, to estimate the

probability that a recession is about to occur. To obtain these probabilities, he starts with a regression explaining GDP growth. Using the coefficient estimates and an estimated distribution of errors, he is able not only to predict output in successive quarters, but also to compute the probability that a sequence of shocks will be sufficiently negative so that a particular future quarter will mark the beginning of a recession. This procedure requires a statistical rather than a judgmental definition of a recession. Howrey defines the start of a recession as the first of two or more successive quarters of decline in a centered, three-quarter moving average of quarterly GDP growth rates, and the end of a recession as the last of two or more quarters of negative growth, measured the same way. Recessions defined this way correspond closely to recessions as officially dated by the National Bureau of Economic Research.

Howrey first examines the performance of a single-equation autoregressive (AR) model using the ICS and three other indicator variables frequently used by economic forecasters: the yield spread between ten-year and three-month Treasury securities, the composite price index of the New York Stock Exchange, and the Conference Board's Composite Index of Leading Indicators. He subsequently examines a vector autoregressive (VAR) model using these same four indicator variables along with three additional economic variables: the inflation rate, the unemployment rate, and the three-month interest rate on Treasury bills. The estimation covers different intervals in the 1960–2000 period, depending on data availability.

For both the AR and the VAR regressions, Howrey estimates a number of specifications corresponding to all possible combinations of the four indicator variables. Using each resulting estimated equation, he then performs 1,000 stochastic simulations with disturbance terms drawn randomly from a normal distribution with a covariance matrix from the estimated equation. For each quarter, some fraction (sometimes zero) of these 1,000 simulations result in a recession, and that fraction represents that specification's predicted probability of a recession for that quarter. Since Howrey is interested in probabilities spanning time horizons ranging from one to four quarters ahead, he applies this method to calculate four separate probabilities, measuring the likelihood that a recession will begin in the next quarter, in either of the next two quarters, in any of the next three quarters, and in any of the next four quarters.

To measure the accuracy of these predictions, Howrey uses a quadratic probability score (QPS) for each specification and time horizon. Each quarter is assigned a value of 1 if it was actually a recession quarter and 0 if it was not. The difference between this value and the probability of recession predicted by a particular specification measures its error for each quarter. The QPS is the sum of the square of these errors over the entire sample period. A perfect forecasting model would have a QPS of zero, with a recession probability of 1 for each recession quarter and a probability of 0 for all other quarters. Howrey uses the QPS to evaluate the usefulness of the ICS and the other variables in predicting recessions. To do this he compares the QPSs for all four time horizons and across equation specifications that span all possible combinations of the explanatory variables.

Turning first to the results with the AR-estimated equations, when each variable is used alone, the yield spread performs best, producing the lowest QPS. The ICS does not fare particularly well on its own, although it is statistically significant. However, when variables are used together, combinations that include the ICS always produce the lowest QPSs over most horizons, and the ICS is always highly significant. The combination of the ICS, the yield spread, and the stock price index achieves the best score among all AR specifications for three of the four horizons considered. Interestingly, overall statistical measures like R^2 and the standard error of the regression produce quite different rankings among specifications than does the QPS, which measures only the ability to predict recessions.

Results for the VAR equations differ in the details but also indicate a role for the ICS in predicting recessions. Since the VAR without any indicator variables is itself a useful forecaster of recessions, it provides a stronger test of the ICS's contribution than the AR equations do. The yield spread generally has less significance in these equations than in the AR equations, presumably because the other economic variables reflect much of the same information that it conveys. The ICS is significant in all equations. Its QPS performance does not stand out at the one-quarter horizon, but the specification using just the ICS and the stock price index has the best QPS over the two- to four-quarter horizons.

Howrey illustrates the performance of this last specification by showing its predictions of recession probabilities before and during each of the past five recessions. It does especially well in predicting the large, back-to-back

recessions that started in 1980:1 and 1981:4. In the quarter before each of these downturns, the model indicated the probability of a recession starting in the following quarter as 0.58 and 0.34, respectively, and the probability of a recession starting in one of the next four quarters as 0.80 and 0.70. In the quarter before each of the other three recessions, the warnings were more muted: the probability of a recession in the next quarter ranged only from 0.07 to 0.22, and that of a recession starting in one of the next four quarters ranged from 0.21 to 0.49. Notably, the model emitted no clearly false signals of recession over the entire period. Applying the model to the present economy, with data available as of mid-April, Howrey calculates the probability of recession starting in one of the four quarters of 2001 as 0.22, which he interprets as a clear warning signal.

THE CURRENT ACCOUNT OF THE U.S. balance of payments has been in deficit in every year since 1982, with the single exception of 1991, when a small surplus was recorded. Since 1997 the deficit has much more than doubled in size, reaching a record $435 billion (4.4 percent of GDP) in 2000. The corresponding net inflows of capital from abroad, together with the sharp appreciation of U.S. equities and other assets in the 1990s, brought the foreign net investment position in the United States to roughly $2 trillion at the end of 2000. These developments have led many to question whether current account deficits on the scale of recent years are sustainable. The last three papers in this issue, by Richard Cooper, Ronald McKinnon, and Jaume Ventura, constitute a symposium on the current account that addresses this question from different perspectives.

WHILE NOTING THAT SEVERAL other countries also have large current account deficits or surpluses relative to their GDP, Richard Cooper observes that the sheer size of the U.S. deficit has made it a dominating feature of the world economy in recent years. But his analysis leads him to a sanguine assessment of the U.S. position: the deficit does not, in his view, pose a serious financing problem. Rather than ask how the United States can continue to attract sufficient capital to "cover" the current shortfall, he finds it more useful to think of the strength of capital inflows as *causing* the current account deficit. For the past two decades foreigners have, on balance, wanted to buy more U.S. assets than Americans have wanted to invest abroad. This strong foreign demand for U.S. assets has influenced the exchange rate of the dollar, and this, together with other

developments in the U.S. and foreign economies, has produced the observed current account deficits.

This perspective, in which desired capital flows are the main driving force in the balance of payments, will not be appropriate for all places and times, but Cooper sees it as central to understanding the present U.S. situation. He offers a number of reasons why investing in the United States has been attractive to foreigners. First, U.S. interest rates have been higher than rates in most other advanced economies, and the returns on U.S. fixed-income investments have been less risky than those in emerging market economies. Second, equity returns in the United States tend to be higher and more reliable than in other advanced economies, in part because U.S. business capital-output ratios are lower. Finally, markets for both stocks and bonds in the United States are more liquid than those in most other countries, are more transparent, and offer greater protection of investors' rights.

Given the attractiveness of U.S. assets and the size of the U.S. economy, which accounts for over a quarter of world GDP, recent rates of net foreign investment in the United States make economic sense, Cooper argues. Cooper calculates that the approximately $400 billion by which foreign investment flows to the United States exceed U.S. investment flows abroad—the counterpart of the present current account deficit—amounts to roughly 8 percent of global foreign saving. Allowing for continued U.S. investment abroad at current levels, total foreign investment in the United States would have to be a few percentage points more than this 8 percent of foreign gross saving for the U.S. current account deficit to remain in its present range. Cooper views continued flows of this magnitude as sustainable.

He also recognizes, however, that circumstances may change in a way that reduces the desired level of these investment inflows, and he discusses the likely consequences. The dollar would depreciate, improving the competitiveness of U.S. goods and, in time, reducing the current account deficit to match the eventual new level of net foreign investment. However, because this adjustment would occur with a lag, and because expectations in foreign exchange markets are fragile, the equilibration process might not go smoothly—the dollar's depreciation might well overshoot. Cooper believes, however, that any sharp and substantial depreciation would probably lead foreign officials to intervene to brake the dollar's fall. He suggests that European exporters, for example, would strongly protest

any rise in the euro much above the $1.17 level at which it was launched. In the right circumstances, such as the overappreciation of the dollar in the mid-1980s, Cooper regards such interventions as useful. And he recommends that the U.S. authorities increase their holdings of foreign exchange reserves, so that they have the option of taking part in a coordinated intervention should the need arise.

RONALD MCKINNON'S ANALYSIS OF the current account emphasizes the special role of the dollar in today's world economy. He observes that most international financial and commercial business is denominated in dollars, and this makes American banks and other financial institutions largely immune from currency risk, despite the fact that a substantial part of their deposit liabilities is owed to foreigners. This situation effectively gives the United States unusual access to credit from the rest of the world. U.S. financial markets and institutions are strong and relatively well regulated, but McKinnon argues that these features are not the primary reason for the dollar's unique status. Rather, he sees the preferred position of the United States as an international borrower as having arisen serendipitously from international arrangements that evolved after World War II.

When Japan and the major nations of Europe imposed tight controls on foreign exchange transactions after that war, and the Bretton Woods system pegged their currencies and others to the dollar, the dollar became the only currency in which international transactions could freely take place. This position of the dollar as "international money" constituted a natural monopoly, based on economies of scale that provided disincentives to doing business in a different currency. Thus, once the dollar had become established as the currency of choice in financial and commercial markets, the dollar standard was maintained even after 1971, when the Bretton Woods system of exchange parities broke down, and after the other major industrial countries liberalized their capital accounts.

McKinnon cites some important macroeconomic advantages to this special position of the dollar. The United States is free to focus its monetary policy on domestic inflation and output targets, without concern for the effect of exchange rate changes on the value of its liabilities. He reasons that many countries are reluctant to see their currencies appreciate against the dollar, for fear of losing competitiveness on world markets, and are therefore unlikely to resist the accumulation of large dollar reserves. An important consequence is that the United States faces only a soft con-

straint on its international borrowing. And as world demand for liquidity grows in tandem with world output, the United States satisfies that need by issuing liabilities that effectively are never expected to be repaid. This growth in dollar liabilities to the rest of the world has, in recent years, been accompanied by rising deficits in the U.S. current account. But McKinnon notes that a current account deficit is not needed for the United States to provide liquidity. Gross short-term capital inflows, taking the form of currency, U.S. bank deposits, and the like, can be accompanied by gross long-term outflows, such as foreign direct investment, and thus provide international liquidity at times of current account surplus, as they have in the past.

Despite these advantages for the dollar, McKinnon sees two types of problems that could arise if large current account deficits continue. The first is that the soft borrowing constraint can lead to much greater leveraging of U.S. households. This arises because the willingness of foreigners to hold dollars allows U.S. banks and other credit providers to finance themselves cheaply abroad to expand their domestic lending. The second problem is that, other things equal, larger U.S. current account deficits hurt U.S. manufacturing firms and other U.S. producers. Many economists would see the damage to manufacturing as simply the result of free competition and the market at work. But McKinnon argues that the dollar's unique position biases the market, and that the shrinkage in the U.S. industrial base has been unusually large as a consequence. This in turn threatens the growth of protectionist measures, which he believes would be easier to resist politically if the U.S. trade accounts were closer to balance.

Some observers hold that the current account deficits of recent years are unsustainable and will ultimately lead to flight from the dollar, undermining its present international role. McKinnon, however, argues that the de facto dollar standard will endure under any likely scenario. For example, some have argued that a slowdown in U.S. growth relative to growth in the rest of the world would cause a sharp reversal of capital flows out of the United States. But McKinnon points out that faster growth abroad would also increase foreign demand for dollar liquidity. McKinnon also believes that any incipient run on the dollar by private agents would be offset by the accumulation of official reserves designed to prevent a sharp dollar depreciation. In his view, only chronic high inflation in the United States would jeopardize the dollar's role in the international economy.

JAUME VENTURA ALSO EMPHASIZES investment flows in analyzing the current account, treating them in the context of a portfolio model aggregated up to the U.S. macroeconomy as a whole. For this purpose he proposes an unconventional concept, a "U.S. portfolio," defined as the sum of all productive assets located in the United States, plus the U.S. net foreign asset position. In his model all foreign claims, ranging from Treasury securities to equity stakes in U.S. assets, are considered U.S. liabilities that differ only in the degree of risk and liquidity. Thus, for example, he would treat a Honda plant in the Midwest as a U.S.-owned asset, with a corresponding foreign claim in the form of the foreign equity stake, whereas a conventional view would treat the Honda plant as an asset owned by a foreign company.

On either view, U.S. net worth is equal to the value of the U.S. portfolio. But Ventura's treatment makes a difference when considering the composition of the country portfolio and its implications for portfolio adjustments. For his purpose, the composition is defined by the share of the net foreign asset position in the portfolio. A negative balance on the stock of net foreign assets indicates that the U.S. portfolio is leveraged, with the value of its assets exceeding net worth. Foreign direct investment, like the Honda plant, is also a form of U.S. borrowing from abroad that leverages the ownership of U.S. assets, which include the Honda plant.

Under standard portfolio analysis, changes in the relative attractiveness of U.S. assets will prompt portfolio adjustments, either inside the United States or abroad, that change the composition of the portfolio between net U.S. and foreign assets. But under plausible assumptions such as constant relative risk aversion, changes in the level of net worth do not change the desired composition of the portfolio. These considerations lead Ventura to examine changes in wealth and changes in risk-adjusted returns as the factors driving current account developments in the past two decades.

Ventura offers only a cursory explanation for the large current account deficits from 1980 to 1992. He observes that high U.S. interest rates, stemming from tight monetary policy and large fiscal deficits, together with the international debt crisis of the 1980s, made U.S. assets relatively more attractive. Portfolio adjustment toward U.S. assets, as predicted by his model, thus provides a plausible explanation for the increase in capital inflows to the United States that occurred. Ventura calculates that the continuing net inflows steadily reduced the share of net foreign assets in total

U.S. wealth over this period, with the stock of net foreign assets becoming negative for the first time in the late 1980s.

For most of the 1990s, however, Ventura sees the rise in U.S. wealth driving portfolio adjustment, and hence the current account. His view of this period follows from his unconventional treatment of the composition of the U.S. portfolio. The United States' net indebtedness grew sharply until 1992, representing, in his model, increased leverage of the U.S. portfolio. When U.S. wealth rose with the stock market boom of the 1990s, U.S. liabilities to foreigners had to increase to maintain this desired leverage, beyond the increase in wealth. In Ventura's model, this portfolio process explains the large current account deficits of the 1990s. Alongside the stock market–driven rise in wealth, those deficits have not produced any significant change in the balance between net foreign and domestic assets in the U.S. portfolio. The share of net foreign assets in total U.S. wealth first rose after 1992, then declined, and then rose again, ending the decade at about the 1992 share.

Ventura next turns to some questions raised by his linking of the current account to the rise in stock market wealth. One question is why the strong U.S. stock market, if driven by prospects of future profitability as Robert Hall's e-capital model suggests, has not led to a desire to raise the share of U.S. assets in portfolios here and abroad. Two possible answers are, first, that large past gains do not predict comparably large future gains, and second, that e-capital enhances future profitability abroad as much as it does in the United States. The most pertinent question for the future of the current account turns on whether the rise in stock prices will be reversed, as it was in 2000 for shares in most technology companies. If this should happen, it would generate a reduction in aggregate wealth and saving inclusive of capital losses. With the United States a debtor economy, Ventura's model predicts that this would lead to a reduction in the current account deficit. Indeed, he suggests that a quick and steep collapse of stock prices would require a current account surplus to keep portfolio leverage from growing. A protracted and gradual market decline would move the current account to near balance, with a return to moderate deficits as the growth rate of wealth returns to its longer-term trend.