

Editor's Summary

With this issue *Economía* enters its fifth year. We have published papers on most topics that are relevant to the formulation of economic policy in Latin America. The papers that follow—which were first presented at the panel meeting held in April 2004 and hosted by Harvard University—continue that tradition.

Latin America is clearly the world leader in currency and financial crashes, and these crises entail large costs, sometimes as large as a quarter of GDP. What is much less clear is who bears these costs. Are they equally spread across society, or does one group bear a disproportionate burden? What is the impact on the poor and on the distribution of income? Marina Halac and Sergio Schmukler try to answer these difficult questions in the lead article of this issue. Most research so far shows that crises have distributional effects because the poor and unskilled are the first to lose their jobs, the real value of their meager peso savings plummets with high inflation and devaluation, and social spending is typically cut back in bad times. Halac and Schmukler focus on a different channel: the financial channel. They study financial crises that involve bailouts and ask who pays for these bailouts and how this affects income distribution. Their conclusions are disheartening.

Bailouts have two kinds of distributional effects. Resources are transferred from taxpayers to participants in the banking sector, who get the bailouts. Recipients include bank owners and owners of firms with debts to the banking system—not exactly the poor. Large and small depositors also receive transfers. They are not necessarily rich, but they are not poor either. In Latin America, only households in the upper ranges of the income distribution are likely to have bank accounts. Taxpayers, on the other hand, come from all levels of income. Tax systems in Latin America are hardly progressive, given widespread avoidance of income taxes and heavy reliance on consumption levies that are paid by everyone, including the very poor. In Mexico, for instance, the value added tax (VAT) paid as

a percentage of income is roughly constant across all income deciles. The poor also suffer disproportionately when bailouts are financed not by raising taxes, but by cutting spending, since they tend to rely more heavily on transfers and social spending. The conclusion is unavoidable: transfers from nonparticipants to participants in the banking system are transfers “from the relatively poor to the relatively rich.”

That is not the end of the story, however. Halac and Schmukler show that large transfers also take place within the financial sector. In the Argentine, Ecuadorian, and Uruguayan crises, large and foreign depositors (or investors with access to foreign-based accounts) obtained compensation or even capital gains, while small depositors suffered capital losses. After the crashes in Chile, Ecuador, and Mexico, large borrowers with close ties to banks also benefited most from the crises and their resolution. The conclusion is again inescapable: “financial redistributions during crises benefit the rich and hurt the poor.”

If currency and bank crashes are one perennial concern in Latin America, slow growth is another, at least since the early 1980s. It is now widely accepted that the region’s growth performance in the 1990s was disappointing, especially in view of the many structural reforms put in place early that decade. In 1991–2003 Latin America grew not only much less than the countries of East Asia, but also less than the members of the Organization for Economic Cooperation and Development (OECD). So much for convergence, at least among these three groups. Moreover, Latin America also grew less in 1991–2003 than it did in the 1960s and 1970s, the peak years of import substitution.

What accounts for this disappointing performance? What factors explain the difference with East Asia? Are neoclassical growth models up to the task? José de Gregorio and Jong-Wha Lee revisit these thorny questions, employing a variety of econometric strategies. They begin by estimating an extended Solow model with pooled cross-country data. To deal with endogeneity issues they use three-stage least squares, with lagged values of the independent variables and also several other variables as instruments.

Their basic results accord with the vast empirical growth literature that relies on cross-country regressions: investment rates, fertility, and measures of human capital have significant effects on growth, as do institutional and policy variables such as government consumption, indices of rule of law and respect for property rights, and measures of trade openness.

This estimation procedure and its interpretation are subject to unsettled econometric issues, however, as discussed Daniel Lederman emphasizes in his remarks.

De Gregorio and Lee put their results to work trying to explain Latin America's poor growth performance relative to Asia. They use their point estimates to divide the fitted growth rates for each country into the contribution of each of the explanatory variables. The outcome of this exercise, they argue, suggests that bad policies played a greater role than bad luck in accounting for the gap. Over the whole 1970–2000 period, standard factors such as investment, population growth, and the quality of human resources explain almost half of the difference in per capita GDP growth between the two regions. Policy and institutional factors (rule of law, government consumption, macroeconomic stability, and the degree of openness) explain the other half. Unfavorable terms-of-trade shocks do not matter much—except perhaps in the 1980s, when Latin America suffered a growth collapse that is not well accounted for by the regression results.

Both Latin America and East Asia have suffered currency and financial crisis. Until the 1990s the East Asian countries seemed less vulnerable to this disease, but in that decade they caught up with Latin America in this regard. One lasting difference, however, is that while the Asian economies had farther to fall (since they grew more quickly to begin with), they recovered more sharply in the aftermath of crashes. V-shaped recoveries took place in most of Asia after the 1997–98 crisis, but this recovery pattern is relatively rare in Latin America, with the exception of Chile after 1982 and Mexico after 1994. Moreover, such crises have not been recurrent in Asia, whereas Latin America has often climbed out of one hole only to fall into another.

In the second half of the paper, de Gregorio and Lee examine eighty-one episodes to identify factors that help reduce the output cost of a crisis. Predictably, a good international environment reduces costs. Domestic factors under the control of the policymaker also matter. Sound banks are crucial, since the output costs of a twin crisis (that is, a combined balance-of-payments and banking crash) are about twice the cost of a balance-of-payments crisis alone. It also helps to have ample international liquidity, measured by international reserves as a ratio of M2, before the crisis. De Gregorio and Lee further find that real exchange rate depreciation and expansionary monetary policy help in the recovery, while post-crisis fiscal policy has no effect. Fortunately, these lessons seem to be very

much in the minds of Latin American policymakers in the region. They should help prevent the recurrence of crises—or at least minimize adverse effects, should crises hit again.

While economists argue about the causes of low growth, businesspeople are plagued by no such doubts. Again and again, surveys of business opinions in Latin America underscore one alleged obstacle to investment and growth: scarce and expensive credit. Descriptive statistics seem to confirm this concern, in that many countries in Latin America have experienced a credit crunch over the last decade. Most cases involved reductions of 1–3 percentage points in the ratio of bank credit to GDP. But double-digit declines have occurred as well: Bolivia with a drop of almost 16 percentage points, Mexico with 20 percentage points, and Panama again with 16 percentage points over the last couple of years.

These figures raise as many questions as they answer. Was the decline in bank credit the result of a shift in demand or in supply? In either case, what structural or policy factors caused the shift? Several papers have tackled these hard questions without arriving at a definitive answer. Country experiences are quite heterogeneous, with some cases pointing to the dominance of demand factors (such as low economic activity and firms with little creditworthiness) and others to supply factors (including limited credit from abroad and greater conservativeness in bank lending behavior). One element, however, that is common to almost all Latin American countries is the adoption in the early 1990s of the Basel Accord capital requirements.

So-called Basel I defined risk-based minimum capital requirements, which became a major component of banking regulation throughout the world. It established weights for various categories of assets, and banks were required to hold more capital for categories of assets deemed to be more risky. Basel I also defined the various forms of capital that could be used to meet these requirements. What were the consequences? When asked to increase the capital-asset ratio, bankers may choose to provide more capital and hence raise the numerator; alternatively, they may choose to cut back on loans and reduce the denominator, causing a credit crunch. It is not surprising, therefore, that many studies have tried to ascertain the impact of Basel I on bank lending.

Until now, however, little or no work has used Latin American data. That is the main contribution of the paper by Adolfo Barajas, Ralph Chami, and Thomas Cosimano. They use a new data set on Basel I adoption

dates—which they put together—in combination with a bank data set spanning 2,893 banks drawn from over 150 countries. The aim is to test the hypothesis, both for the world as a whole and for Latin America and the Caribbean, that Basel I caused banks to reduce lending. Barajas, Chami, and Cosimano show that adoption of the Basel Accord was followed by an average increase in capital and lending activities in both Latin America and the Caribbean and throughout the world. They find little indication of a causal link with credit crunches: neither the loan-asset ratio nor the average growth rate of loans declined after Basel I adoption. In the authors' own words, “we do not find evidence that the loan supply curve shifted, on average, after Basel.”

Barajas, Chami, and Cosimano do find, however, that loan growth became more sensitive to certain risk factors; this suggests that banks became more risk averse as a reaction to increased regulatory scrutiny. In Latin America, this effect shows up primarily through greater sensitivity of loan growth to past shortfalls in equity.

The last two papers in this issue address different aspects of a single issue—namely, the impact of minimum wages on employment and unemployment—and reach substantially different conclusions. The paper by Kevin Cowan, Alejandro Micco, and Carmen Pagés focuses on Chile, a country whose otherwise successful performance has been marred by high unemployment since the Asian crisis. Chilean unemployment averaged 6.9 percent in 1993–97. By 1999, it had reached 8.3 percent, and it remained stubbornly high thereafter: it averaged 9.3 percent in 2001–02 and has fallen only slightly since.

This mediocre unemployment performance triggered a heated debate in Chile. Some analysts focus on the effects of a large goods demand shock coming mostly from abroad, which caused a sharp fall in labor demand. With sticky wages, the fall in employment was not surprising. Under this interpretation, the problem is temporary: it should last only as long as the economy is sluggish, with employment picking up as soon as activity recovers. The other camp claims that more permanent factors are at work: labor demand has undergone some kind of structural shift, caused by both technological shocks and the increasing inflexibility of the Chilean labor market. Under this interpretation, the problem is permanent, in the sense that relatively high unemployment will not go away on its own, but rather requires a drastic deregulation of the labor market that reduces hiring and firing costs to make it more attractive for firms to hire labor.

Cowan, Micco, and Pagés land in the middle of this controversy and come down unambiguously on the side of the first interpretation. They find no evidence of a structural break in labor demand, and they argue that the employment slowdown after 1997 resulted from an aggregate demand shock combined with wage rigidity. Yet they add a twist: wages were especially rigid and the employment response especially large and persistent because of two features of the Chilean labor market—namely, a statutory minimum wage that was raised excessively and became binding after the Asian crisis, and widespread indexation of labor contracts to the consumer price index, especially in the manufacturing sector.

Cowan, Micco, and Pagés marshal microeconomic evidence from the Chilean employment survey to show that almost 6 percent of workers were affected by the rise in the minimum wage that took place between 1997 and 2000. They also show that the behavior of aggregate wages is well described by inflation-adjusted two-year contracts, which suggests that indexed long-term contracts extend far beyond the small set of workers who bargain collectively. In short, “high wage rigidity . . . explains why Chile experienced a relatively larger fall in employment than other developing countries that faced substantial output reductions in the 1990s.”

If an excessively high minimum wage appears to be the villain in the Chilean story, the plot in Brazil—as told by Sara Lemos in the last paper in this issue—is very different. Minimum wages play a central role in the Brazilian labor market. Increases, which are large and frequent, guide the government’s centralized wage-setting policy and also serve as a guidepost for wage- and price-setting throughout the economy. Brazil would thus seem to be a good test case for observing and measuring the effects of minimum wages on employment.

Lemos estimates the effects of the minimum wage on wages and employment using panel techniques and monthly Brazilian household data from 1982 to 2000. Her main conclusion is that an increase in the minimum wage strongly compresses the wage distribution, with small negative effects on employment. In the short run, a 10 percent increase in the minimum wage appears to reduce total hours by no more than 0.16 percent. This is a small employment response, much smaller than the –1.0 percent effect often found in the international literature. The other finding Lemos emphasizes is that the small employment response occurs mostly through hours worked and not through the number of jobs. Therefore, she argues,

changes in the minimum wage are likely to have negligible effects on measured unemployment.

Lemos carries out a number of robustness checks, and her results do not go away. The translation of her econometric results into policy advice is likely to prove controversial, however. One important qualification, which she herself emphasizes, is that Brazil experienced relatively high inflation in the period studied, so that changes in prices partially offset changes in nominal wages. This may have cushioned the impact of minimum wage policy on employment. The results could be different in Brazil today, where inflation is much lower, or in other countries with less inflationary environments.

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