Barry EICHENGREEN

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Capital Flows and Crises

Barry Eichengreen

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I Background

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1 Introduction

The implications of capital mobility for growth and stability is one of the most contentious and least understood issues of our day. The controversy is long standing: one only need recall Nurkse's emphasis on destabilizing capital flows in the 1920s or Keynes's and White's effort to construct an international regime with limits on capital mobility following World War II.[1] But the depth of disagreement in both academic and policy circles was pointed up by the Asian crisis. This event, which followed on the heels of widespread capital account liberalization in the first half of the 1990s, convinced many observers that early opening to international capital markets is a recipe for disaster. But it also led others to the opposite conclusion—that the problem in Asia was the failure to more fully deregulate markets and transactions, including international financial transactions, and to limit government's role. It is hard to think of another issue over which there is more dispute or where the stakes for policy are higher.

The essays here speak to this controversy. They provide historical, theoretical, empirical, and policy perspectives on capital flows. The emphasis is on the connections between capital flows and crises, because this is where much of the controversy resides. But crises are not the entire story. If they were, economists would have no compunction about recommending and policymakers would have no reluctance about pursuing policies to limit the economy's exposure to capital flows. There are compelling arguments against such policies, reflected in the models of theorists and the revealed preference of governments. Foreign finance can be used to augment domestic savings, helping relax resource constraints on capital formation; depending on its form, it can come packaged with expertise and be a conduit for technology transfer. International financial markets are

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also a source of discipline on policymakers. For all these reasons, there are grounds for thinking that capital flows can have positive welfare effects. But the magnitude of these benefits is disputed, and none them is guaranteed, especially if capital movements set the stage for costly crises.

History illustrates these points. The twentieth century, although not a controlled experiment, is a rich repository of information on the effects of capital flows. The century, broadly speaking, can be divided into four parts, corresponding to four distinct capital market regimes. Before 1914, capital movements were free and net flows across borders reached levels never achieved again, not even in the 1990s. World War I was followed by a two-decade-long transition unlike both the period of free capital mobility that preceded it and the period of tight controls that followed. In the 1920s, policymakers aspired to reconstruct international financial markets and transactions along prewar lines but never entirely succeeded. Capital flows were smaller, the postwar boom in lending was brief, and governments played a more prominent role in artificially supporting and attempting to sustain those flows. When the world economy collapsed into the depression of the 1930s, capital markets collapsed along with it, culminating in the most severe international financial crisis of the century. Currencies were forced off the gold standard, countries defaulted on their debts, and governments slapped controls on capital flows. These policies and the problems to which they were a response set the stage for the third quarter of the century, during which countries maintained tight restrictions on international financial transactions. Measures limiting capital account transactions were then progressively relaxed and lifted starting in the 1970s, inaugurating another period of growing capital flows.

Chapter 2 surveys this historical experience in an effort to recover its implications for the policy debate. It argues that it is impossible to understand the pattern of capital flows—their magnitude, direction, and effects—without reference to the broader international system in which they occurred. Lending has tended to surge and its impact has been most favorable when international trade has been expanding, other international factor flows (labor flows in particular) have been unimpeded, macroeconomic policies have been sound and stable, and political ties between lenders and borrowers have created a "nexus of contracts" discouraging opportunistic behavior by the parties to these transactions. The importance of these contextual fac-

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tors is evident in the contrast between the periods immediately before and after World War I. International migration was freer before this watershed; capital and labor flowed in the same channels. Trade was freer: foreign finance flowed more naturally into export-linked activities that generated the foreign exchange revenues needed to service and repay the loans. Neither aspect of the prewar system survived the 1914–1918 war. The political context also changed, as flows between the European powers and the overseas regions of recent European settlement were superceded by loans from the United States to the once and future belligerents of continental Europe, notably Germany. As monetary and fiscal policies in both the borrowing and lending countries became politicized, they increasingly responded to capital movements in destabilizing rather than stabilizing ways.

These observations help shed light on the effects of capital flows in the 1970s and the 1990s, two occasions when surges of international lending again culminated in costly crises. On both occasions, lending was promoted by the expansion of trade, the deregulation of financial markets, and economic reform in the borrowing regions. But this foreign money was not always invested productively, especially when governments and state banks with political agendas were on the receiving end. The flow of finance was then disrupted by sharp policy shifts in the lending countries. How easily the borrowers accommodated the consequences depended in part on the openness of the trading system. (Whereas the "new protectionism" of the 1980s complicated the efforts of Latin American debtors to export their way out of their crisis, for example, in the 1990s the North American Free Trade Agreement, NAFTA, and the willingness of the United States to act as importer of last resort had the opposite effect.) It depended on the flexibility of their economies and (as the counterexample of Indonesia reveals) on the robustness of their political systems.

This historical perspective makes clear that the association of capital flows with crises is not new. But is it growing closer? Chapter 2 reports the findings of a research project, conducted jointly with Michael Bordo and other collaborators, tracing changes over time in the frequency and severity of banking and currency crises and of their bad seed, the twin crisis. The most intriguing comparison from the point of view of current policy concerns is between the two periods of high capital mobility, before 1914 and after 1971. There is

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little sign that crises are growing more severe, whether measured by the associated output losses or the time before recovery commences. By contrast, there is a clear increase in the frequency of crises, currency crises in particular—and because currency crises have occurred more frequently, there has been an increase in the incidence of twin crises (this despite the fact that the frequency of banking crises has remained essentially unchanged). Throughout the twentieth century, twin crises have been especially costly and disruptive to output. It is understandable that observers should have evinced concern about their growing frequency in the century's closing decades. The question, for present purposes, is whether their causes are essentially the same as in prior periods or whether their nature is changing. The last two sections of this volume attempt to establish these facts with reference to both recent experience and historical evidence and to draw out their implications.

Chapters 3 and 4 first review the evidence of the connections between capital mobility and growth. Chapter 3 summarizes what cross-country studies have to say about these connections. As it turns out, they say disappointingly little, positive or negative, about the effects of capital flows on economic growth. In part, this reflects the difficulty of measuring the openness of an economy to capital flows. Observed capital movements reflect not just the stringency of restrictions on foreign borrowing and lending but also the effects of domestic policies that attract or repel foreign investors, and of global economic and financial conditions that determine the attractiveness of the alternative investment opportunities. The standard International Monetary Fund (IMF) indices of the presence or absence of controls on capital account transactions, for their part, provide only a crude summary indicator of the relevant statutory measures.

In addition, the failure of the data to speak more clearly reflects the tendency for investigators to look for these effects in macroeconomic aggregates rather than at the sectoral or firm level. One might say it reflects the economist's habit of generalizing excessively—of attempting to uncover universal rules of economic behavior, in this case about the relationship between capital mobility and growth. If recent crises have taught us one thing, it is that this relationship is likely to be contingent on, inter alia, the strength of the institutions and markets that mediate the influence of the foreign funds, and on the compatibility with an open capital account of a country's other economic policies. Chapter 4 takes this proposition seriously: it asks

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whether the growth effects of capital account liberalization are more favorable in countries with strong legal and contractual institutions (where the "rule of law" prevails) and with relatively deep and developed financial markets. There is considerable support for the first of these contingent relationships but only weak support for the second.[2] In addition, the broader policy context is critical—what turns out to be important, to put it another way, is the sequencing of reforms. Specifically, countries that open the capital account before eliminating severe macroeconomic imbalances are courting disaster. This finding has important policy implications, as I make clear in the final section of this volume.

Before getting there, I first devote five chapters to the crisis problem. Chapters 5 and 6, coauthored with Andrew Rose and Charles Wyplosz, are drawn from an empirical project on the causes of currency crises on which the three of us have been collaborating for some years.[3] There is a large literature in which it is now standard to distinguish currency crises from devaluations, to measure the former using indices of exchange market pressure (constructed from actual exchange rate changes, interest rates changes, and reserve changes), and to compare the behavior of the variables of interest in crisis and noncrisis periods. I like to think that it is from our research project, of which these chapters are the summary statement, that these methodological insights are drawn.

Those in search of simple answers may again be struck by the contingent nature of the findings. As chapter 5 documents, crises are heterogeneous; they vary among themselves and are more difficult to generalize about than currency devaluations. This is not surprising, because it is what theory predicts: there exist several different generations of theoretical models of currency crises based on different assumptions that point to different economic and political covariates.[4] Moreover, as in other contexts where the return to an action depends on how many other economic agents undertake that same action, multiple equilibria can arise. Although many of the propositions suggested by standard models and intuition find support in the data—crises are more likely when growth is weak, unemployment is high, the real exchange rate is overvalued, and macroeconomic policy is lax—the empirical counterparts of those propositions have only limited explanatory power. Their predictive power is even more limited. This is an important caution for those who would otherwise place excessive confidence in so-called leading indicators.

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Chapter 6 applies this approach to the problem of contagion. The definition of contagion developed there has been widely adopted in the literature (so large that some refer, only half in jest, to the subfield of "contagion studies"). We define contagion as present if a crisis elsewhere in the world increases the likelihood of a particular country experiencing a crisis of its own, even after one controls for other observable fundamentals associated with crisis risk. The evidence is consistent with the presence of contagion in foreign exchange markets: a crisis elsewhere increases the probability of a crisis in the subject country by 8 percentage points, even after taking account of other domestic and international economic and political factors.

The limitation of this analysis is the difficulty of distinguishing contagion from common shocks caused by sudden changes in global economic conditions—what Paul Masson (1998) calls "monsoonal effects." We address this issue of identification by placing additional structure on the problem, weighting crises in other countries by the amount of trade in which they engage (to capture potential beggar-thy-neighbor effects), or by the similarity of their macroeconomic structure and policies (to pick up guilt by association). Again, the results are strongly suggestive of contagion, and again, the approach has been widely utilized in the subsequent literature. What is surprising, given how the growth of financial flows has so outstripped the growth of trade, is the tendency for contagion to spread so strongly among trading partners. One is tempted to dismiss this as a figment of the historical data, as trade figured more importantly than finance in the past, and to a greater extent in the past than in the present. In fact, however, subsequent research has shown this pattern to be robust.[5]

Panel data are suited for testing sweeping hypotheses such as the existence of contagion but less useful for providing a feel for the texture and dynamics of crises. For this one must turn to the history. With this in mind, the next three chapters analyze three of the crises of the 1990s—the European Monetary System (EMS) crisis, the Mexican crisis, and the Asian crisis. Chapter 7 compares the Mexican crisis, dubbed by Michel Camdessus (1995) "the first financial crisis of the 21st century," with the Baring-Argentina crisis, the last financial crisis of the nineteenth. Camdessus's remark suggests that there was something distinctive and unprecedented about the Mexican crisis that set it apart from its predecessors. He presumably had in mind the prominence of financial factors and the speed with which

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events unfolded. But the comparison with the Baring crisis a century before suggests that there was little new about the causes of the Mexican crisis or how that episode played itself out. Parallels include the enthusiastic reaction of investors to the combination of low interest rates in the financial center and economic reform in the developing world, and the role of public banks in accentuating the impact of foreign capital on the domestic economy and of political weakness in hamstringing the government's management efforts.

Chapter 8 views the 1992–1993 crisis in the European Monetary System (EMS) from a similar vantage point. It might seem peculiar to describe the 1992–1993 episode as an emerging market crisis. Europe is not an emerging market. Its economy is highly developed and diversified. Its financial markets are deep. Yet despite these differences, the debates provoked by the EMS crisis parallel, to a remarkable extent, those stimulated by its emerging-market successors. There is the debate over the role of fundamentals versus destabilizing shifts in investor sentiment in the outbreak of the crisis. There is the debate over the importance of imbalances in the crisis countries themselves versus shocks from outside (in the European case, the German unification shock; in the Mexican case, the U.S. interest rate shock). There is the debate over the role of capital account liberalization in heightening financial risks, most of Europe's capital controls having been removed in the years leading up to the crisis. And there is the importance of banking-sector problems in limiting resort to interest rate increases to defend the currency. All this suggests that there is little new about the contemporary crisis problem. Only the removal of capital controls was needed to render Europe susceptible. In Latin America and Asia, the combination of capital account liberalization with political democratization, which made it harder for governments to credibly subordinate all other social and economic goals to the overriding imperative of exchange rate stability, sufficed to bring about the same result.

The implication is the extreme difficulty—and the extreme danger—of attempting to operate an exchange rate band, target zone, or crawling peg, and the importance of replacing such regimes with more robust exchange rate arrangements, either harder pegs or freer floats. In Europe, countries responded to these realities by moving to freer floating, as in the cases of Sweden and the United Kingdom, or by accelerating the transition to monetary union, the hardest of all hard pegs.

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It will not surprise the reader that I draw the same lessons from the Asian crisis. Chapter 9 tells the story of that crisis.[6] Financial-sector weaknesses played a larger role than macroeconomic imbalances in setting the stage for the crisis. Among these weaknesses were poorly regulated banking systems, pervasive connected lending, unreliable bankruptcy and insolvency procedures, and weak creditor rights generally. When in the first half of the 1990s capital accounts were liberalized (often in the worst possible way, by permitting banks to borrow offshore while still limiting foreign investment in domestic bond and equity markets, and providing artificial incentives for short-term rather than long-term flows), this volatile mixture combusted. But it is not enough to argue that these policies were ill advised and that future governments should avoid them. It is important to understand that the policies in question were integral to the Asian development model. Banks were the agents of the government's industrial policy; it was in return for acting in that role that they received implicit government guarantees. That they were favored by the particular approach taken to capital account liberalization was hardly coincidental. Not only were they politically well placed, but ensuring their ample funding was critical to the success of the authorities' industry-promoting development plans. Crony capitalism was integral to the pursuit of rapid economic growth in the absence of a transparent contracting environment. In a sense, the Asian crisis was such a flashpoint because it revealed the dark side of that region's exceptionally successful development model, and the International Monetary Fund's advice and conditionality were so controversial because they recognized the need to remake that model along more market-oriented lines if Asian countries were to persist in integrating with world financial markets.

The last two chapters consider these issues of policy advice. Chapter 10 explains that the pressures for financial integration are powerful, bordering on the irresistible. Advances in information and communications technologies render capital controls designed to seal off economies from international financial markets immensely more difficult to operate. Such controls are still workable, but only if a government is able and willing to apply them comprehensively. Doing so is at odds with the desire to develop domestic capital markets and abandon policies of financial repression. Moreover, the spread of democratization in the Third World creates popular resistance to strict regulations that limit citizens' freedom of financial

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action. Although capital controls may have a future, both trends suggest that it will be a limited one.

How then should emerging markets navigate this transition? Chapter 11 suggests some rules for the road on the assumption that capital account liberalization is the ultimate destination—after all, today's Organisation for Economic Cooperation and Development (OECD) economies all have liberalized financial markets and open capital accounts—but that moving in that direction is prudent only once the institutional and policy environment has been strengthened. Until corporate governance and supervisory infrastructures have been sufficiently upgraded to ensure that banks and firms can manage their own risks, policy should be used to limit their external borrowing. As the strengthening of institutions proceeds, foreign direct investment should be liberalized, followed by stock and bond markets. Only then should banks be permitted to borrow offshore. In addition, domestic policies should be better aligned with the capital account regime. This means adapting exchange rate and monetary policies to the openness of the capital account, and specifically abandoning pegged-but-adjustable exchange rates, crawling bands, and target zones in favor of either harder pegs or freer floats.

None of these guidelines are any guarantee against crises, given the volatility of financial markets and the asymmetry of the information environment. Some economic risks are worth taking, and crises are an inevitable concomitant of risk. Crises, like firm failures, can be seen as a manifestation of the Schumpeterian process of creative destruction. The role for economic analysis is to ensure that the creation dominates and that the destruction is not too costly.

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