

REVIEW

Capital Controls: Theory, Evidence and Policy Advice

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Edwards, Sebastian, ed., (2000), *Capital Flows and The Emerging Economies*. Chicago, IL: University of Chicago Press.

Haggard, Stephan (2000), *The Political Economy of the Asian Financial Crisis*. Washington DC: Institute for International Economics.

Schulze, Günter G. (2000), *The Political Economy of Capital Controls*. Cambridge, UK: Cambridge University Press.

I

The turmoil in the financial markets during 1997 and 1998, which began with Thailand's devaluation of the baht in July 1997, triggered a vigorous debate on the merits of capital account liberalization. This particular financial crisis – and interestingly not the crisis experienced in Latin America in the early 1990s – led certain prominent international economic policy makers and analysts to

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question the slow yet pronounced trend towards fewer restrictions on international capital flows. Even though few endorsed Malaysia's sudden imposition of capital controls, many observers did recommend that developing nations adopt capital controls designed to reduce inflows of portfolio investments and international bank lending without deterring long-term foreign direct investors.

Now that the dust has settled a little and that several sober analyses of both capital controls and the events of the last few years have emerged, perhaps it is time to evaluate whether this fundamental shift in policy advice stands up to careful scrutiny. A second objective of this review is to assess some of the recent analyses of the prevalence, nature and effects of capital controls and their liberalization. Noting that the consequences of removing capital controls depends critically on a raft of other government policies,¹ many economic analyses of the East Asian crisis have concluded that capital account reforms should have been preceded by measures to strengthen the regulatory oversight of national financial systems. Even if this conclusion is correct, detailed political analyses of capital account reforms in East Asia reveal that the interest groups that were in favour of removing capital controls were adamantly opposed to enhanced domestic financial supervision. Therefore, the sequence of reforms that many feel is optimal from an economic point of view was, for most countries, politically infeasible. The critical issue, then, becomes how to alter the incentives in national political systems so that the economically ideal sequence of reform is not captured by previously influential interest groups. Having set the stage, let us now turn to Schulze's exposition on *The Political Economy of Capital Controls*.

II

Günther Schulze, an assistant professor in the Department of Economics at the University of Konstanz, divides his book into three parts. The first part advances the argument that politicians employ capital controls to extract taxes from the owners of capital, and to alter the capital stock of a nation, thereby affecting the returns to labour and capital and acting as a form of redistributive policy. Schulze critiques explanations for the prevalence of capital controls that are based on the assumption that policy makers are benevolent or, in the terminology of economists, maximize societal welfare. The second part of the book recognizes that the owners of capital, at home and abroad, have an incentive to evade capital controls when the rates of return on capital in a

¹Including the supervision of the financial sector, exchange rate policies, and mandated corporate governance practices.

nation differ from those in the rest of the world. In a novel analysis, Schulze demonstrates that the extent of mis-invoicing² of international trade flows, an important method for evading capital controls, depends not only on the existence of capital controls but also on the height of import tariffs and on the magnitude of tax rates on corporate profits. The third part of Schulze's book attempts to estimate the effects of Norway's capital controls, initially for the 1980s and then for 1954–89. (Norwegian capital controls were eventually abolished on 1 July 1990.) Schulze presents econometric results that suggest that Norway's capital controls created a short-term interest differential between domestic and the Eurokroner interest rates, and that Norwegian savings rates were more highly correlated with Norwegian domestic investment rates when capital controls bit.³

Before discussing Schulze's arguments in detail, it is worth noting the lack of integration of the three parts of his book. One might have hoped that the theoretical model developed to examine the optimal political choice of a disincentive to invest overseas (in part one) could have been extended to allow for mis-invoicing of imports and exports (in part two). That way we could have learned how the factors which determine the extent of mis-invoicing also influence the politically inspired choice of the capital account restrictions in the first place. Furthermore, one might have thought that the empirical section of the book (part three) would have afforded the author with an excellent opportunity to evaluate the theoretical propositions made earlier in the book. Overall, the reader is left with the impression that the three parts of this book

²Even though nations can impose controls on capital flowing in or out of a country, the fact that firms can trade goods internationally opens up ways to circumvent capital controls. A firm can transfer capital abroad by declaring to customs that the value of goods shipped to an overseas customer are less than the actual value of the transaction. The firm must repatriate funds equivalent to the declared value of the transaction, but can keep overseas the difference between the actual and declared value. Likewise, an importing firm can circumvent restrictions on capital inflows but declaring to customs that the value of the goods bought from abroad exceeds the actual amount paid to the foreign supplier. Both of these practices are called 'mis-invoicing'.

³Feldstein and Horioka (1980) showed that, in a nation which takes the world interest rate as given and where there are no impediments to the international movement of capital, an increase in the nation's savings will have no effect on the nation's level of investment. This is because the latter is determined by the world interest rate and can, if necessarily, be financed entirely abroad. An implication of this finding is that the correlation between the national savings rate and national investment rate is zero. Many analysts have used the actual correlation between a nation's savings and investment rates as an indicator of the extent of capital mobility, with a zero correlation taken as the benchmark indicating perfect capital mobility. On this perspective, greater impediments to the international movement of capital – such as a higher tax on overseas investment income – would raise this correlation because more of a nation's investment plans must be funded from domestic savings.

could have been published separately, without detracting from the arguments advanced in each.

III

The first part of Schulze's book is the most provocative. First, Schulze limits the scope of his study to international transactions whose instigators are interested in the direct movement of physical capital. Thus, a capital inflow from abroad will expand the recipient nation's stock of physical capital. Despite the outpouring of research on capital flows during and after the East Asian crisis, all Schulze has to say about 'excess' volatility in foreign exchange markets as a rationale for capital controls is the following:

Capital controls have been suggested as a short-term device to limit self-fulfilling speculative attacks ... The argument runs roughly as follows. Due to different speeds of adjustment in the financial and in the real sector of an economy, excess exchange-rate volatility may cause disruptions in real economic activity and, therefore, limiting short-term capital mobility may be beneficial. Capital controls in the form of a tax on foreign exchange transactions will 'throw sand into the wheels' and stabilize, but not disrupt, exchange markets' activity. Though there may be something to this argument we will not consider it here because it is not targeted at restricting international capital flows as such, but only short-term speculation (p. 22–3).

Irrespective of what you think of this particular case for capital controls, it is worth noting that this statement could have been written in 1980. In an era when foreign direct investment accounts for a much smaller fraction of global capital flows than bank lending and portfolio financial flows, Schulze's narrow focus is untenable. In addition, I found no reference to the East Asian crisis, or for that matter any other financial crisis where capital flows were thought to play a role – even in the introduction and the 'finale' (the title of his concluding chapter) where a discussion of context and caveats would appear to be in order. In sum, I was left with a sense that Schulze's book lacked freshness or much relevance for the issues of the present day.

The objective of part one of Schulze's book is to account for the widespread use of capital controls around the world. Here, Schulze critiques explanations that are motivated by market imperfections and the presumption that policy makers choose government interventions – such as capital controls – so as to maximize societal welfare. He also correctly notes that capital controls are often not the most efficient remedy to misallocations of resources in markets with imperfections, such as minimum wages, tariffs and externalities.

In the aftermath of the East Asian crisis, one such argument has been given considerable prominence by Jagdish Bhagwati and Richard Cooper, and builds on earlier theoretical research by Brecher and Diaz Alejandro (1977); see Bhagwati (1998a, b) and Cooper (1999). In an economy that takes world prices as given, the presence of government-imposed barriers to imports tends to raise the profits of firms in protected import-competing sectors. These (artificially inflated) profits encourage other domestic firms and foreign investors to enter the import-competing sector, so distorting the allocation of resources. This distortion is in addition to the traditional misallocation of resources created by the trade barriers.⁴ A prohibition on foreign direct investment in import-competing sectors would, therefore, reduce part of this additional form of resource misallocation. However, if the objective is to prevent all of this additional source of resource misallocation, then a ban on both domestic and foreign entry is required (and, of course, the best remedy is to eliminate the trade barriers that induce entry in the first place). Given the widespread use of tariffs and non-tariff barriers by developing countries, the existence of considerable tariff peaks in industrial nations, and the sizeable flows of foreign direct investment in the 1990s, this argument may have some plausibility. Indeed, in his evaluation of numerous foreign direct investment projects in developing nations, Moran (1998) argued that one third of such projects would have been unprofitable at world (and not trade-barrier inflated) prices.

Schulze goes too far, however, when he asserts that a benevolent government would not simultaneously impose a tariff and a ban on foreign direct investments in the protected sector. His argument is valid when the imposition of these two policy instruments represents the only departure from a competitive economy. However, in countries where the collection of income and consumption taxes is either extremely expensive or infeasible, taxes collected at the border (such as tariffs) are typically a major source of government revenue. Indeed, when expenditure on imports constitutes a large share of national income, then the next least distortive way for a government to raise revenues is often through levying tariffs. A government that is concerned about maximizing social welfare may, therefore, find itself levying tariffs and, to stem the Brecher–Diaz Alejandro effect, banning foreign investments in import-competing sectors. My counter-argument is not idle speculation either – many developing countries that rely heavily on tariffs and other trade taxes to fund government spending also have restrictive regimes towards foreign direct

⁴In textbook treatments of the effects of import restrictions, the implications of firms entry are typically not considered. Instead, these restrictions are shown to raise the price paid by domestic purchasers above the world price. A higher domestic price in turn reduces the welfare of purchasers and provides an incentive for the import-competing firms to expand. This expansion draws resources away from other productive uses and, therefore, distorts the allocation of resources within the economy.

investments.⁵ In sum, the Brecher and Diaz Alejandro rationale for capital controls – recently resurrected by Bhagwati and Cooper – cannot be dismissed as easily as Schulze suggests.

Schulze wants to shift the discussion away from welfare-maximizing rationales for capital controls towards explanations that place self-interested politicians at the centre of the analysis. He argues, correctly, that it is odd to assume that consumers and producers act in a self-interested manner and, at the same time, to assume that politicians choose policies in a benevolent fashion that maximize the economist's traditional notion of social welfare. Again, he spoils his case by going too far: even if the assumption of self-interested politicians yields theoretical predictions that better account for the observed pattern of capital controls, it does not follow that the social-welfare maximizing perspective on policy choice offers no useful insights. The difference between the market outcomes predicted by each perspective reveals how much the political institutions (assumed in a politically self-interested analysis of policy making) distort market outcomes from their efficient level. In principle, different political institutions, which supply different incentives to government decision makers, could be ranked according to their distortive effects. Although Schulze does not consider how differences in national political systems yield different restrictions on capital controls, potentially revealing considerable cross-country variation that could be evaluated empirically, a richer analysis might pursue this line of reasoning.

Schulze focuses on two tools by which politicians seek to retain power – redistributive taxation and measures that directly alter the factor payments received by citizens. Taxes on investment income earned abroad have a bearing on both tools, as they can raise revenues for direct redistribution and increase the relative attractiveness of investing at home, which raises the nation's capital stock and, in turn, alters wages and the rate of return on capital. (Under quite general conditions a reduction in the capital used by workers, *ceteris paribus*, reduces the marginal productivity of workers and, therefore, the demand for labour and the real wage.) Taking the distribution of the capital stock across a nation's population as given,⁶ a politician chooses that tax on overseas investment income (or more generally that set of capital controls) which creates sufficient support from the populace to ensure that the politician remains in office. When the median voter's capital stock is less than the mean capital stock per person, which can occur if a minority of the population owns a majority of the nation's capital stock, his model predicts that the politically

⁵See IMF (1999) for evidence on the former; and any of UNCTAD's excellent annual *World Investment Reports* for evidence of the latter.

⁶Technically, the relevant distribution is the distribution across the population of the ratio of capital owned to labour units supplied.

optimal tax rate on overseas investment income is positive. This is because the tax falls disproportionately on the minority that owns most of the nation's capital stock and the benefits are conferred, through transfer payments funded by these tax revenues and through the effect of bolstering the national capital-to-labour ratio, on the majority which owns little capital and whose income derives principally from labour income. In this manner, Schulze constructs his explanation for the prevalence of capital controls. He goes on to explore the effects of introducing complications such as minimum wages (which can generate unemployment in his model) and when an economy is large enough that collectively its citizens' supply and demand decisions affect world prices. These complications do not undermine his central results, but do provide useful caveats.

One is entitled to ask how well Schulze's explanation for capital controls accounts for the observed cross-country and inter-temporal variation in these policy instruments. As Schulze makes clear in his first table (p. 2), at present, industrial countries tend to impose far less extensive capital controls than developing nations. Alternatively put, capital controls are more prevalent among poorer nations – yet these are the very nations where the implicit assumption in Schulze's model of a well-functioning system for taxing factor incomes and making transfer payments is questionable. Furthermore, as numerous empirical studies of the effects of immigration and foreign direct investment have shown, the effects of changes in total capital stock or total labour force on wage rates and the rate of return on physical capital are very small. In sum, one of the two effects of a tax on overseas investment income that Schulze identifies is weak, and preconditions for the other effect are unlikely to be met in the very nations which impose the most restrictions on capital flows. This calls into question whether Schulze's model can account for the cross-country variation in observed capital controls.

Despite presenting evidence that the number of industrial countries imposing capital controls has fallen over time, Schulze does not examine which factors could account for a self-interested politician adopting less restrictive policies. This is a pity, because even if readers are not persuaded of the relevance of Schulze's arguments for developing countries, they might have been persuaded by a convincing analysis of why capital controls have fallen in industrial countries. Overall, after going to considerable length to criticize other approaches for failing to account for the observed prevalence of capital controls, it is pretty disappointing to discover that, upon close examination, Schulze's analysis hardly passes the test he has set for others. What compounded my dissatisfaction is the study's focus on physical capital flows, despite the attention given to portfolio flows and bank lending in recent financial crises.

IV

My critique of Schulze's contribution should not be taken as a repudiation of other approaches that give pride of place to self-interested political actors. In my view, Schulze's approach needs to be amended in two ways. First, the close linkages between business people, politicians, and government officials in many nations suggests that non-state actors are likely to influence the policy choices that state actors make – including not just whether a nation reforms its capital account, but how it does so and what other government policies (if any) are reformed at the same time. A broader conception of the political technology of a nation – how a nation translates the inputs of influence and expertise into policy measures – is needed. Second, the rich factual record of capital account restrictions and their liberalization since World War II should be drawn upon when devising research questions – avoiding the temptation to motivate a research programme with a few 'stylized facts' that cannot do justice to national differences in the level of economic development and political technologies. For example, one might ask the following questions.

- In nations that have removed some capital controls, which factors, if any, eroded the political opposition to capital account liberalization?
- Did borrowers see reform as a means to stimulate competition among domestic financial firms, perhaps increasing borrowers' access to funds and reducing the interest rates charged by lenders?
- Or did reform only occur when the domestic financial sector found supplying capital to the home market too constraining, and looked abroad for further opportunities?

A first rate attempt to move the debate over financial reform in the right direction is Stephan Haggard's book on *The Political Economy of the Asian Financial Crisis*. Haggard's book should be essential reading for anyone interested in understanding the economic factors, and their political underpinnings, that unleashed the East Asian crisis, as well as the determinants of the economic and political reform paths since the onset of the crisis. Although Haggard's discussion of the political changes wrought by the crisis is of considerable interest, especially for those concerned with the future political stability of the region, here I will focus on his analysis of how capital account reform can go awry. Haggard, a professor at the University of California, San Diego Graduate School of International Relations and Pacific Studies, asks 'to what extent did politics affect the design and implementation of liberalizing initiatives?' (p. 34).

Haggard emphasizes that businesses took a keen interest not only in the timing and the extent of capital account reform, but in how related government

policies – including the regulatory oversight of financial firms – were implemented during and after the capital account reform. Consequently, he characterizes the process of reform in East Asia as one of ‘captured liberalization’.

Haggard examined the political and economic factors that shaped capital account liberalization in Indonesia, Malaysia, the Republic of Korea, and Thailand. His discussion of the Thai experience is particularly instructive. Thailand’s capital account reforms ended with the creation of the Bangkok International Banking Facility in 1993. Banks which were members of this Facility could take advantage of numerous tax concessions but, more importantly, were allowed to borrow offshore and to re-lend to borrowers in Thailand and abroad. One of the Thai government’s reasons for introducing this Facility was to promote Bangkok as a regional financial hub. Another reason was to encourage the financing of trade that would pass through the poorer eastern and north-eastern sectors of Thailand, where many officials had their political bases. Haggard notes that the Thai financial sector supported the creation of the Facility as a means to expand operations abroad, and not to re-lend to domestic borrowers. (Such re-lending became increasingly attractive as a commercial proposition later in the 1990s as interest rates in Thailand rose above world rates and a fixed exchange rate gave the impression of no currency risk.) As re-lending to domestic borrowers expanded, the importance of the Bank of Thailand’s regulatory oversight of banks in the Facility grew. However, such oversight was increasingly hamstrung by strong financial sector influence over politicians in weak political parties. This influence saw the dismissal of two governors of the Bank of Thailand in the early 1980s, and quite non-intrusive bank regulation endured until the reforms instituted after the crisis.

Haggard’s discussion does not imply that the Thai financial crisis was inevitable, as it was possible – although somewhat unlikely – that Thailand’s interest rates could have remained low enough to make re-lending to domestic borrowers unprofitable. His discussion does, however, imply that the ‘capture’ of this capital account reform effort increased the probability of a crisis occurring. Haggard’s analysis also suggests that a ‘sanitized’ (or to rephrase his term ‘uncaptured’) reform was somewhat unlikely too. This finding is especially important in the light of the prevalent view⁷ among economic analysts that many East Asian nations should have strengthened their financial regulation and supervision before liberalizing inflows of foreign portfolio investments

⁷Mussa (2000) is a recent example of this view. Mussa argues:

High openness to international capital flows, especially short-term credit flows, can be dangerous for countries with weak or inconsistent macroeconomic policies or inadequately capitalized and regulated financial systems. For such countries, public policy has important challenges to meet in preparing for a world economy that is driven towards higher degrees of capital market integration.

and bank loans. Even if this economic diagnosis is correct, Haggard's analysis implies the required policy changes were politically infeasible. The question, then, becomes not only what sequence of policy reforms to adopt, but how to transform national political technologies so that the optimal sequence of reform is not captured by narrow economic and political interests.

V

The collection of papers in Sebastian Edwards' edited volume titled *Capital Flows and the Emerging Economies* was initially presented at a National Bureau of Economic Research conference in February 1998, and many have clearly been revised to take into account subsequent developments in both the financial markets and academic research. The papers are divided into three groups:

- (i) theoretical models of capital flows to developing nations,
- (ii) cross-country empirical analyses of the determinants of the magnitude, and geographical location, of capital flows to developing countries, the spreads on these countries' debts, and the equity returns in emerging markets, and
- (iii) analyses of the determinants of capital flows to Latin America, Asia, and Eastern Europe.

In the light of the considerable attention given in recent years to the contagion of financial turmoil and to Chile's current regime of capital controls, two (of the nine) chapters in this volume are especially germane: the chapter by Guillermo A. Calvo and Enrique G. Mendoza on 'Contagion, Globalization, and the Volatility of Capital Flows,' and the editor's chapter on 'Capital Flows, Real Exchange Rates, and Capital Controls: Some Latin American Experiences.' This is not to cast doubt on the quality of the other seven chapters. For instance, Takatoshi Ito's chapter on 'Capital Flows in Asia' is probably the best concise account of the factors underlying capital flows to, and within, East Asia up until the initial phase of the crisis.

Calvo and Mendoza, professors of economics at the University of Maryland at College Park and Duke University respectively, motivate their study by noting that turmoil in Mexico's financial markets was followed by sharp changes in asset prices in other Latin American markets during the so-called Tequila crisis in 1994–95, and that a similar pattern was observed in the East Asian crisis, with financial turmoil spreading from Thailand to the much of East Asia, and then on to Russia and Brazil. And in the most recent crisis some of the nations who subsequently suffered financial malaise did not have

strong trade linkages with nations who were first hit by the crisis.⁸ Calvo and Mendoza seek to shed light on this phenomenon by examining whether there are mechanisms that lead investors to become increasingly susceptible to contagion as more nations integrate their financial markets with the global capital market.

Calvo and Mendoza develop a theoretical model with three critical assumptions:

- (i) Returns in national financial markets are stochastic.
- (ii) Investors can resolve the uncertainty about the returns in any given financial market by incurring a positive fixed cost.
- (iii) The remuneration of portfolio managers (who do the investing in this model) depend in part on how the returns on their portfolio compare to the returns on a market portfolio.

They build into their model an incentive for investors to diversify their portfolios across national capital markets, by assuming that investors care about the variance as well as the expected mean return of their portfolio. However, as an investor's portfolio is spread more thinly across a greater number of national markets the incentive to incur the fixed cost of resolving uncertainty about the returns in any one market declines. This is because increased opportunities for diversification lead the investor to reduce the proportion of his or her portfolio invested in each market, and so the benefit to an investor of knowing the actual return in any one market is given a smaller and smaller weight in the portfolio's overall performance. One implication is that each investor may decide not to verify a rumour of an adverse economic event – such as a bank failure – which might considerably reduce the returns from investing in a market. Furthermore, since each investor's compensation is tied to the relative performance of his or her portfolio; no single investor will retain his or her holdings in a market where large numbers of other investors respond to a rumour by liquidating their holdings. Unsubstantiated rumours can trigger the simultaneous exit of investors from a nation's capital market. Another implication of Calvo and Mendoza's model is that, even though increasing financial market integration may expand the scope for portfolio diversification, there is a price to be paid in terms of a greater potential for financial instability.

Calvo and Mendoza then fit the parameters of their model to the mean and variance of, and the covariances between, 144 countries' financial data. The resulting parameters imply that investors will not verify a rumour in an

⁸This suggests that sudden changes in trade flows were not the only mechanism by which macro-economic shocks in one nation were transmitted to other nations.

emerging market if the fixed costs of verification exceed one sixth of the mean return of their portfolios (prior to the rumour). Furthermore, in simulations with Mexican data, they found that such rumours could trigger a sizeable \$15 billion outflow of capital.

If contagion is taken to be the sequential collapse of financial markets located in different nations, then Calvo and Mendoza's theory cannot be thought of as an explanation of contagion, as exit from only one market is considered. Instead, it is probably more accurate to think of their model as an explanation for herd behaviour, where each investor simultaneously decides to liquidate his or her holdings in a given financial market. Their principal insight is, in my opinion, that – in the absence of any complementary government measures – such herd behaviour is more likely as financial market integration continues.

In this model, much turns on the cost of verifying rumours. If this model is to be taken seriously, then policy makers in emerging markets, who want to integrate their national capital markets with the world financial system, should seriously consider subsidizing, or otherwise reducing, the costs of verification. Presumably the timely disclosure of information would go some way towards reducing these costs, and Calvo and Mendoza's model could be thought of as reinforcing the often heard, but alas vague, calls for public sector 'transparency'. But such verification need not be provided solely by the public sector. Their model can also be interpreted as providing an upper bound on investors' willingness to pay for financial research to ascertain the veracity of rumours. That willingness to pay provides private research firms with an incentive to acquire reputations for producing accurate analyses of emerging markets. To the extent that competition between research firms reduces the cost of verifying rumours, then Calvo and Mendoza's logic implies that the potential for financial instability will diminish.

VI

That capital account reforms have been followed by currency and banking crises in so many countries has – especially in the light of the East Asian crisis – shifted policy discussions away from the merits of such reforms towards consideration of whether certain capital account reforms are better able than others to secure the benefits of financial market integration (including access to foreign capital and enhanced possibilities for diversification and smoothing consumption) without endangering financial stability. Since the East Asian crisis, the starting point for such a discussion is often to differentiate among foreign direct investment (FDI), portfolio investments, and lending by foreign banks to domestic banks. FDI is thought to be motivated by longer-term considerations, to facilitate the transfer of technology and managerial best

practices, and is not considered prone to sudden reversals. Portfolio investments and international bank lending, by contrast, tend to be lumped together and called 'hot money', reflecting the fear that such flows are prone to sudden and sizeable reversals, which in turn cause asset price and exchange rate collapses. Putting to one side the fact that each of these claims can – and has – been challenged, the discussion then turns to whether certain capital controls can help developing countries to reduce their 'dependence' on hot money. As the World Bank's former chief economist, Joseph Stiglitz, put it to the *New York Times*, 'You want to look for policies that discourage hot money but facilitate the flow of long term loans, and there is evidence that the Chilean approach or some version of it, does this' (1 February 1998).

I would contend that this statement accurately characterizes the prevailing conventional wisdom on this particular aspect of capital account reform.⁹ Furthermore, as this quotation makes clear, Chile's policies towards capital inflows (which are described below) have been commended to developing countries. The significance of Sebastian Edwards' chapter is that it contains a careful evaluation of Chile's policies towards capital controls and, therefore, of the policy advice being dispensed by, among others, the leading multilateral institutions.

Edwards begins by describing the two central elements of Chile's current controls on capital inflows, which were implemented in 1991. First, there are minimum stays on foreign direct investments but no restrictions on the repatriation of profits. Second, there are unremunerated reserve requirements for all other inflows of capital (including trade credits). These requirements force foreign investors to set aside a mandated percentage of their investment to be held in an account at the central bank for one year. These accounts pay no interest, hence the forgone interest acts a penalty on the investment. This penalty can be thought of as an entry fee, which is paid irrespective of the length of the investment and consequently penalizes longer-term investments less as the entry fee is spread out over a greater time horizon. Until the East Asian crisis, this unremunerated reserve requirement stood at 30%. However, during the crisis the requirement was reduced to 0%, so as to attract funds to Chile. Even though this requirement currently stands at 0%, nothing prevents the Chilean authorities from increasing the requirement in the future.

Although the Chilean scheme is currently advocated as a means to shift the composition of inflows from 'hot money' to FDI, the Chilean government implemented the scheme after concerns that capital inflows were having

⁹Despite Stiglitz's rather colourful remarks after resigning his post at the World Bank, statements in a similar vein to the one quoted here have been made by senior officials of the International Monetary Fund during and after the East Asian crisis, including its former managing director, Michel Camdessus.

adverse effects on inflation and the real exchange rate. It was hoped that these capital controls would enable (if need be) Chilean domestic interest rates to rise above world interest rates, enhancing Chile's room for monetary manoeuvre. Edwards correctly notes that, although an evaluation of Chile's capital controls on the composition of capital inflows, real exchange rates and interest rate differentials would be of interest, such an evaluation can be only part of an overall assessment of the effects of Chile's unremunerated reserve requirements. Other consequences of these requirements need to be taken into account. For example, Edwards presents evidence that these requirements have raised considerably the cost of capital to small- and medium-sized enterprises, which is likely to have had adverse consequences for capital accumulation and growth.

On the basis of Edwards' empirical analysis, the best case for Chilean-type controls rests on their effects on the composition of capital inflows. Before the current scheme was introduced, the percentage of long-term flows in total capital inflows was less than 10% (for 1988–90, the three years before the current scheme's introduction). In contrast, during 1995 and 1996, this percentage had risen to over 90%. Furthermore, this shift in composition did not come at the expense of an overall reduction in capital inflows. And comparing across countries, this percentage is much higher in Chile than in other developing economies.

Edwards conducts a barrage of econometric tests and finds that the effects of unremunerated reserve requirements on interest rate differentials and on the real exchange rate are much weaker and confined (at most) to the short term. This suggests that these controls did little to enhance Chilean monetary autonomy or to slow down the competitiveness-reducing effects of large capital inflows on the real exchange rate. Before one concludes that Chile's unremunerated reserve requirements have helped stave off financial crises by reducing her exposure to hot money, Edwards points out that similar requirements were in place during 1978–82, and that they did not prevent an enormous financial crisis in 1981–82, where the Chilean peso was devalued by almost 90%. The difference between the early 1980s and the late 1990s was that extensive banking reforms had been introduced in 1986, which improved the stability and competitiveness of the banking system. Edwards' analysis must surely cast doubt on whether Chile's regime for regulating capital inflows should be held up as a model for other developing nations. Instead, the relatively more difficult task of credible banking reform seems to be the recommendation which follows best from Chile's experience.

Given these findings, it is disturbing how quickly Chilean-type capital controls became the policy recommendation *du jour*, especially as the vast bulk of serious empirical work on the effects of these controls started during (or after) the crisis. This raises concerns about the level of scrutiny given during crises to new policy proposals at the leading international financial institutions

(IFIs). These concerns are not confined to the IFIs – several leading economic commentators also wrote, on the basis of very little evidence, in support of Chilean-style capital controls. It appears that a plausible idea and the need to be seen to have a ‘solution’ to pressing problems is enough for a proposal to gain wide currency during a crisis. This form of contagion is worthy of analysis too.

VII

What have we learnt about the case for further capital account liberalization? It is important to remember that access to world capital markets enables a nation to finance investments in all manner of socially as well as economically beneficial activities, without being constrained by the levels of domestic savings that a nation can muster. Eroding this constraint is the fundamental – and still unchallenged – benefit of capital account reform. The key question, of course, is to how make the best use of these funds, and that is where the nature of a nation’s political technology – how a nation translates the inputs of influence and expertise into policy outcomes – becomes critical. Since successful capital account reforms require that a bevy of complementary government policies are in place, this forces us to recognize that the real issue is whether a nation’s political technology can adopt – and, more importantly, sustain – a package of reforms.

Shifting the focus towards the political prerequisites for reform is the next step in the development of both academic research and policy advice. Given the strong incentives that distressed or aggressive financial institutions have in supporting politicians and officials who tolerate little regulatory oversight, strong countervailing pressures on national leaders must be developed. These pressures may come from within nations or from elsewhere. If, as we are constantly being reminded by certain IFIs and non-governmental organizations, financial crises disproportionately hurt the poor and workers, then one would have thought that independent trade unions could begin to use their clout to support rigorous financial oversight. Furthermore, financial crises can discredit a whole cadre of political and business leaders, which may enable new ideas and constitutional arrangements to come to the fore. These factors could be reinforced by making the consequences of lax regulatory oversight more costly to officials, and by reducing the dependence of political parties on monetary contributions from the financial sector.

In the aftermath of the East Asian crisis, there was a profound shift in the advice given to developing countries about how to manage capital inflows. Certain activist policies could, it was claimed, reduce the amount of hot money entering a country and the risk of sudden outflows that can trigger a financial

crisis. In the light of subsequent research, this shift in policy advice has been found to have fairly weak empirical support. Worse still, following this advice invariably diverts attention away from the politically difficult task of reforming the oversight of national financial systems. This is particularly disturbing, as so much recent research points to such reform as the key to financial stability in developing countries.

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