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What lessons can Asia draw from capital controls in Brazil during 2008 - 2012?

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What Lessons Can Asia Draw from Capital Controls in Brazil during 2008–2012?

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and Huanhuan Zheng

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Abstract

Driven by waves of foreign capital inflows and outflows, Indonesia, the Republic of Korea, and Thailand—among several other emerging markets—have resorted to capital control policy since 2006. Are capital controls effective? Controls on capital inflows have been experiencing a renaissance since 2008, with several prominent Asian and Latin American countries implementing them. We focus on Brazil, which instituted five changes in its capital account regime over 2008–2011. Using the synthetic control method, we construct counterfactuals (i.e., Brazil with no policy change) for each of these changes. We find no evidence that any tightening of controls was effective in reducing the magnitudes of capital inflows, but we observe some modest and short-lived success in preventing further declines in inflows when the capital controls were relaxed. We hypothesize that price-based capital controls' only perceptible effect is to be found in the content of the signal they broadcast regarding the government's larger intentions and sensibilities. In the case of Brazil, its left-of-center government's willingness to remove controls was perceived as a noteworthy indication that the government was not as hostile to the international financial markets as many expected it to be. Based on these findings, the effectiveness of capital controls should be viewed on a case-by-case basis, together with the political economy considerations, and other policy tools, i.e., foreign exchange intervention.

JEL Classification: F32, G23, E60

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1. INTRODUCTION

Controls on capital inflows have been experiencing a period similar to a renaissance since the beginning of the global financial crisis in 2008. This renaissance has manifested itself most importantly in prominent cases of new controls being put in place, most notably in Iceland, Indonesia, the Republic of Korea, Peru, and Thailand.¹ In conjunction with this reemergence of controls as an actual policy, the theoretical literature has shifted, with several contributions that explain the possible advantages of short-term controls using theoretical models.²

Maybe the most pronounced shift has occurred at the International Monetary Fund (IMF), which advocated the removal of all controls on outflows and inflows throughout the 1990s.³ The Asian Crisis of 1997/1998, however, initiated a very slow process of conversion within the IMF that culminated recently with its decision to explicitly and openly support the imposition of controls on capital inflows.⁴ The basic premise of this new IMF stance on capital controls is that these should be imposed when countries are facing a capital inflow surge and after other policy alternatives have been exhausted.

Here, we are interested in examining whether capital controls on inflows, imposed at the time of an inflow surge, are effective? And if they are, what are their effects? We attempt to answer these questions using the Brazilian experience of 2008–2011 in imposing new (price-based) controls as the global financial crisis unfolded. A prevailing view is that controls should only be imposed in these kinds of crisis circumstances, rather than as business-as-usual policies; and this position justifies our choice to focus on Brazil's policy during the evolution of the global financial crisis.⁵

There are broadly five possible impacts of capital controls on inflows: (i) reduce the volume of capital inflows; (ii) change the composition of inflows (in accordance with the specific controls imposed); (iii) impact the real exchange rate (preventing an appreciation); (iv) enable a more independent pursuit of monetary policy (as it relaxes the international trilemma's constraints); and (v) increase financial stability.⁶ We are unable to deal directly with the second impact (composition of flows) given our data limitations and choose not to examine the impact on financial stability since this is a longer term impact, and our focus here is on the short term (3 months). We thus focus on the volume of capital inflows, on the exchange rate, and on domestic monetary policy (the interest rate).

¹ The most basic distinction is between controls on outflows and inflows. The economics literature consistently finds controls on outflows as inefficient and harmful. Binici, Hutchison, and Schindler (2010) provide a recent empirical attempt to differentiate between the impact of controls on outflows and inflows.

² Jeanne (2012a) provides a selective summary of this new theoretical literature; a more recent examination that looks at the impact of capital account policies on the real exchange rate is available in Jeanne (2012b).

³ The IMF's campaign to liberalize capital flows culminated in an attempt to insert this aim into its charter. See Joyce and Noy (2008) for details and empirical evidence.

⁴ The most recent summary of this new IMF view is available in IMF (2012). Figure 1 in Ostry et al. (2011) provides a summary of the caveats and preconditions that, according to the IMF, should accompany the imposition of controls.

⁵ Klein (2012) employs a related distinction between controls as gates (temporary and specific measures) and controls as walls (aiming to block most or all cross-border capital transactions regularly).

⁶ The evidence on financial stability in general, and in particular about the impact of controls on the likelihood of financial crises is quite mixed (see, for example, Glick, Guo, and Hutchison 2006).

Two recent survey papers on capital controls (Magud, Reinhart, and Rogoff 2011; Ostry et al. 2010) find that the empirical literature on the impacts of capital controls is inconclusive, with some observed effects on the composition of flows, but very little effect on volumes of flows (and even less agreement on the impact of controls on the exchange rate and policy interest rates).⁷

As Magud, Reinhart, and Rogoff (2011) point out, this evaluation of capital controls literature suffers from several apples-to-oranges problems. Most relevant to our work are two problems: First, the literature mostly ignores the heterogeneity of capital controls imposed across countries and over time, and uses cross-country comparisons that utilize control indices that hide these distinctions (the ones developed by Miniane [2004], Chinn and Ito [2006], and Schindler [2009] are frequently used). Second, the case studies literature focuses almost exclusively on the two poster children of capital controls, Malaysia (outflows) and Chile (inflows).⁸

Another difference that appears important is the distinction between the short- and long-term impacts of capital account policies. Long-term impacts, while potentially more important, are generally more difficult to identify precisely, and this has certainly been the case in this literature. Many of the papers that do manage to identify some precise impact of controls, do so only in the short term, and fail to find any longer term effects. The IMF, in its support for reconsidering the use of capital control as a policy, argues that their use should be temporary, and their aim is precisely to have a short-term effect on the volume of capital inflows. We thus focus exclusively on the question of the short term, and ignore long-term effects, which are probably unidentifiable with our methodology, even if they exist at all.

We focus on a set of controls imposed (and relaxed) by Brazil since 2008, in an attempt to control the amount of capital flowing into the country. By focusing on Brazil during the global financial crisis, we directly examine the IMF's support for imposition of controls in the face of capital inflow surges in a country that has financial markets that are largely open to capital flows.

We use micro-level data on capital flows from mutual funds investing internationally, and a new methodology to estimate the counterfactual (no imposition of controls). We use a methodological innovation recently formalized in Abadie, Diamond, and Hainmueller (2010). The methodology (henceforth ADH) is based on simulating conditions after an exogenous event (in their case, a change in the tax rate, in ours, the imposition or change in the rules governing capital inflows). The synthetic counterfactual's construction is based on the relationship to a control group. The ADH algorithm does not presume to impose any ad hoc assumptions about the likely control group, but rather derives this control group as a weighted average of observations from all the non-treated units of observation with weights estimated from pre-treatment data (in our case the non-treated units are countries that have not changed their capital account policies during our sample). The ADH procedure allows us to construct a no-policy-change counterfactual and thus measure in detail the impact of the controls. It does not require us to make many structural assumptions that would have been difficult to justify theoretically.

To be thorough, we need to find a way to examine all five possible impacts. We have weekly data on capital inflows from mutual funds and examine the evolution of these inflows in the

⁷ We do not provide a significant review of this extensive literature since these two recent surveys are available. An earlier survey of this literature is Edwards (1999).

⁸ Malaysia famously imposed temporary controls on capital outflows in the aftermath of the Asian crisis of 1997/1998, and this act generated a heated debate on the topic. Chile imposed a set of taxes on short-term flows in the 1990s that were fairly widely perceived as successful in lengthening the maturity of flows.

aftermath of the imposition of controls. Our data do not allow us to examine the impact of controls on other types of flows, such as foreign direct investment or bank loans, but the flows we examine are relatively representative.⁹ In addition to examining the impact on equity flows, we look for any impact of the controls on exchange rate. We use the same synthetic control methodology (ADH) to develop an alternative counterfactual exchange rate without controls. Again, the ADH methodology allows us to skirt the difficulty of wedding our analysis to any one exchange-rate-determination model, since the literature on the determination of exchange rates is both voluminous and contentious. We implement the same methodology for interest rates, but since interest rate policy changed very little during this period in Brazil, our model is not good enough to capture accurately a synthetic control with a good fit for the ex-ante data. Given that limitation, we do not present our results regarding interest rates but briefly describe them at the end of the next section. We do not examine financial stability since our focus is the short term (3 months) rather than the long term, which is at the core of the financial stability argument.

2. CAPITAL CONTROLS AND FLOWS IN BRAZIL—THE DATA DETAILS

2.1 The Controls

Brazil liberalized its capital flow regimes gradually starting from the early 1990s, culminating in an almost completely open capital account by the mid-2000s, including a flexible exchange rate regime (see Goldfajn and Minella [2005] and Carvalho and Garcia [2008] for details, and Baba and Kokenyne [2011] for an evaluation of this capital account regime in the run-up to just before the global financial crisis). After a fairly brief period of no taxes on foreign capital transactions, taxes were reintroduced in March 2008 at the rate of 1.5% on fixed-income investments.¹⁰ Investments related to equities remained exempt from taxes for a while.¹¹ The tax was reduced to zero in October 2008 at the peak of the global financial crisis, when the exchange rate came under depreciation pressures (as in many other big emerging markets). A 2% tax on fixed-income and equity inflows was reintroduced in October 2009, and its application was widened the next month. The tax was later increased to 6% in two stages (in October 2010), but reduced to 2% in January 2011.¹²

2.2 The Flows

The weekly mutual fund flows data we use are from the Emerging Portfolio Fund Research (EPFR) dataset. We calculate the weekly flows to a specific country as the aggregate flows channeled specifically to this country (from mutual funds whose focus is country-specific). For robustness, we also calculate the broad regional flows to Latin America, which are the sum of all flows channeled to this region including flows that target a broader regional market, e.g., Latin

⁹ A comparison with capital flow data from the IMF's Balance of Payments Statistics dataset is in Appendix C.

¹⁰ This tax on financial operations, known as the IOF (*imposto sobre operações financeiras*), was also used during the 1990s.

¹¹ In May 2008, the tax was extended to cover "simultaneous operations" to prevent circumvention of the inflow tax (which was apparently widespread).

¹² Tax was also expanded to cover margin calls on derivative positions and foreign borrowing with maturities below 1 year. Our dating of these capital account policy changes relies on OECD (2011), along with news articles from Reuters and *Financial Times*.

America, and evaluate its response to Brazilian capital control. We use the weekly EPFR fund data rather than the monthly measures both because we are interested in the weekly dynamics following policy treatment, and as we find that these weekly fund data are better correlated with the International Financial Statistics' Balance of Payments portfolio data than the EPFR monthly aggregates.

In a similar manner, we calculate the total net assets under management (TNA) by summing up the TNA of all funds targeting the designated country; and obtain the mutual fund return by taking the aggregated return of all funds that specialize in the designated country.

Other than the fund-specific characteristics, we also control for the country's stock and bond market performance as well as its foreign exchange rate fluctuations. Specifically, we calculate the weekly stock market return based on the national stock market index measured in local currency. Weekly bond market return is calculated similarly. Bond indices are from JP Morgan GBI and EMBI and are measured in local currency. Weekly return on the foreign exchange rate is calculated as the weekly return of the local currency against the US dollar.

For every episode of capital control, we study 12 weeks (about one quarter) before and after the control announcement date. We include a country as a possible component of the control group if (i) there are no capital controls of any kind imposed for the given sample period, and (ii) there are no missing observations in either variables described above for the given sample period. Generally, only a few small countries drop out of the sample. The final control group sample contains 33–37 countries depending on the episode.¹³ The list of countries used as controls in each specification is in Appendix D.

One of the ADH algorithm's advantages is the ability to use this synthetic control methodology to estimate unbiased coefficients with relatively few pre-event observations. In our case, we use 12 weekly observations pre-treatment for the estimation (see details below)—a similar number to what Abadie, Diamond, and Hainmueller (2010) use, and only slightly less than the number used in the first paper to employ this methodology (Abadie and Gardeazabal 2003). When we examine the data for the control group of countries (those whose synthetic weights are significantly different from zero) and Brazil, we generally find similar trends in the pre-treatment data, suggesting that the shocks Brazil was experiencing were common and there is no evidence to support the argument that the conditions leading to the placing of controls were unique to Brazil. None of our control countries had any change in its capital account policy implemented in our sample periods.¹⁴

3. METHODOLOGY

Y_{it} is the outcome variable that is evaluated based on the controls' impact on the treated country i , (with $i=1$ for Brazil and $i>1$ for all other countries) and time t (for time periods $t=1, \dots, T_0, \dots, T$; where $T_0=13$ is the time of imposition of controls or a change in the control's details) and $T=25$. In this paper, we examine three outcome variables—all three variables are

¹³ In different contexts, Abadie, Diamond, and Hainmueller (2012) and Cavallo et al. (forthcoming) use country-level panels (annual) with a synthetic control methodology—in the first case, to examine the impact of German reunification, in the latter to examine the macroeconomic impact of natural catastrophes.

¹⁴ Some of the countries may have long-standing regulations of various types on the capital account. This is not likely to affect our results. Policy impact tends to be short-term, as they are very likely to be arbitrated away, and in any case these impacts, if they exist, should be consistent throughout the studied episodes. Thus, we already adjust for these consistent impacts with our estimation methodology (that only uses the pre-treatment sample separately for each episode).

potential policy aims, and all may have been affected by the imposition or relaxation of capital controls: aggregate capital flows (as measured in our mutual fund database), the exchange rate, and the interest rate.

Y_{it}^I is the outcome variable in the presence of the controls and Y_{it}^N is the outcome variable had the controls not been imposed.¹⁵ The ADH methodology requires the assumption that the event has no effect on the outcome variable before the date of impact T_0 ($Y_{it}^I = Y_{it}^N \forall t < T_0$). This assumption, in our context, means that the policy change was not anticipated. We present evidence to support this assumption in Appendix B. The observed outcome is defined by $Y_{it} = Y_{it}^N + \alpha_{it} D_{it}$ where α_{it} is the effect of the capital controls change on the variable of interest ($Y_{it}^I - Y_{it}^N$) and D_{it} is the binary indicator denoting the event occurrence ($D_{it}=1$ for $t \geq T_0$ and $i=1$; and $D_{it}=0$ otherwise). The aim is to estimate α_{it} for all $t \geq T_0$ for Brazil ($i=1$). The problem is that for all $t \geq T_0$ it is not possible to observe Y_{1t}^N but only Y_{1t}^I .

Although there is no way of accurately predicting the country-specific determinants of Y_{it} , the structure of the emerging market economies in our sample is fairly similar and the external shocks affecting them were fairly similar as well (except for mean zero iid shocks ε_{it}). In this case, we suppose that Y_{it}^N can be given by the following factor model:

$$Y_{it}^N = \delta_i + \theta_i Z_i + \lambda_i \mu_i + \varepsilon_{it}, \quad (1)$$

where Z_i is a vector of observed covariates and μ_i is a vector of unknown factor loadings.

Furthermore, we let $W = (\omega_2, \dots, \omega_{I+1})'$ be a vector of weights allocated to the different country observations such that $\omega_i \geq 0$ for $i = 2, \dots, I+1$ and $\sum_{i=2}^{I+1} \omega_i = 1$. The synthetic control is a weighted combination of the controls observations such that it replicates a treated unit as if the treatment had not occurred.

Suppose a set of optimal weights $(\hat{\omega}_2, \dots, \hat{\omega}_{I+1})$ can accurately replicate Brazil's pre-treatment observations. Abadie, Diamond, and Hainmueller (2010, Appendix B) show that with a few reasonable assumptions, $Y_{1t}^N = \sum_{i=2}^{I+1} \hat{\omega}_i Y_{it}$. Furthermore, they prove that this equality will hold for all t given the number of pre-treatment periods is large enough.¹⁶ Therefore, we can use

$$\hat{\alpha}_{it} = Y_{1t} - \sum_{j=2}^{I+1} \hat{\omega}_j Y_{jt} \text{ for } t \geq T_0 \text{ as an estimator for } \alpha_{1,t} \text{ provided we choose a set of weights, } W.$$

¹⁵ This description is a modified version of Abadie, Diamond, and Hainmueller (2010). To simplify comparison, we follow their notation where I denotes intervention (capital account policy changed) and N denotes nonintervention (policy not changed).

¹⁶ See proof in Abadie, Diamond, and Hainmueller (2010, Appendix B). Other recent papers that used the ADH methodology, albeit in very different contexts, are Nannicini and Billmeier (2011); Pinotti (2011); Abadie, Diamond, and Hainmueller (2012); Hinrichs (2012); Cavallo et al. (forthcoming); and duPont and Noy (forthcoming).

We define $X_1 = (Z_1', Y_1^{K_1}, \dots, Y_1^{K_M})$ a vector of pre-intervention characteristics of Brazil's capital flow regime, and similarly X_0 for the control countries. The set of weights W is obtained by minimizing the distance between the observations of the treated unit X_1 and the observations for the group of control X_0W during the pre-treatment period. We choose W such that the following equation is minimized:

$$\|X_1 - X_0W\|_V = \sqrt{(X_1 - X_0W)' V (X_1 - X_0W)} \quad (2)$$

where V is a $(k \times k)$ symmetric and positive semi-definite matrix (k is the number of explanatory variables). The choice of V is important as it can greatly impact the mean square prediction error. We use the STATA *synth* routine to obtain V such that the mean squared prediction error is minimized for the period prior to the policy change.¹⁷

The estimates of equation (1) are only used for constructing the counterfactual as accurately as possible. Thus, we are not interested in the actual coefficient estimates of these regressions as they have no economic significance or otherwise interpretable meaning.¹⁸

The usual statistical significance of our reported results, based on regression-based standard errors, is not relevant in this case since the uncertainty regarding the estimate of $\hat{\alpha}_{it}$ does not come from uncertainty about the aggregate data. Uncertainty in comparative case studies with synthetic control is derived from uncertainty regarding the ability of the post-treatment synthetic control to replicate the counterfactual post-treatment in the treated observations.

Following Abadie, Diamond, and Hainmueller (2010), we use permutation tests to examine the statistical significance of our results. We separately assume that every other country in our control sample implements a similar (and imaginary) capital control in the same year. We then produce counterfactual synthetic control for each “placebo control.” These synthetic counterfactuals for the placebos are then used to calculate the impact of the placebo capital controls ($\hat{\alpha}_{it}^P$) in every year following its (non)-occurrence with the following formula:

$$\hat{\alpha}_{it}^P = Y_{it}^I - \hat{Y}_{it}^N = Y_{it}^I - [\hat{\delta} + \sum_{j=2}^{I+1} \hat{\omega}_j Y_{jt}^N] \quad \text{for } t \geq T_0 \text{ and } j \neq P \quad (3)$$

Essentially, we investigate whether the $\hat{\alpha}_{it}$ we estimated for Brazil are statistically different from the placebo $\hat{\alpha}_{it}^P$ for $i > 1$. We present the placebo results only for episodes in which we find any visible impact of the change in the capital control regime.

¹⁷ The STATA program is described at <http://www.mit.edu/~jhainm/synthpage.html>.

¹⁸ Results for the synthetic weights we obtain are in Appendix A. When we examine the data for the control group of countries (those whose weights are different from zero) and Brazil, we generally find similar trends in the pre-events data, suggesting that the shocks Brazil were experiencing were common and there is no evidence to support the argument that the conditions leading to the placing of controls were unique to Brazil. These figures are available upon request.

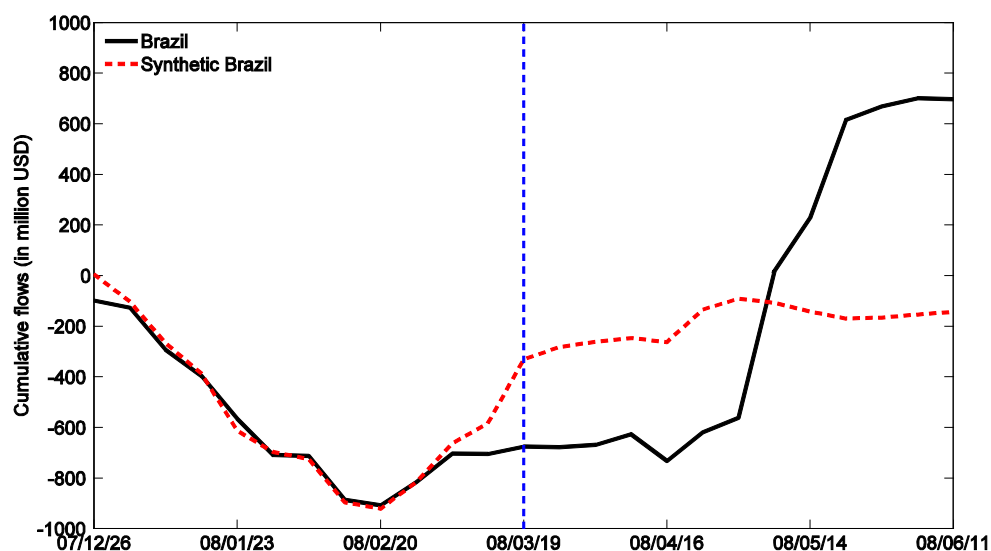
4. RESULTS

4.1 Results for Capital Inflows and Exchange Rate

We graph the actual evolution of capital inflows (as recorded in the EPFR data we use) and the synthetic control that assumes no change in policy. These figures show the counterfactual evolution of capital flows had the changes in capital account policy not occurred. We summarize these results chronologically for each change in Brazil's capital account policies over 2008–2011.

The first act—March 2008 (taxing fixed income only)—is reported in Figure 1. We observe a decline in flows in the run-up to the placing of controls, but funds start flowing in again (net) about 2 weeks before the episode; this budding inflow may be the impetus for the placing of controls (Figure 1a). The placing of controls did not appear to have a large influence. A small and temporary slowdown in the inflow episode resulted from the controls. While we observe a continuation of the inflow for the counterfactual scenario, Brazil experienced a similar dramatic rise, but with about a month's delay. We are not confident that this delay, however, is a result of the imposed controls since it is also present in inflows to other Latin American destinations that did not put any controls in place.¹⁹ A similarly very brief deviation from the counterfactual can be also observed for the exchange rate (Figure 1b). Within 3–4 weeks, we can no longer identify any residual impact of the imposition of controls on the exchange rate.

Figure 1a: Taxing Fixed Income Investment—Capital Flows
12 March 2008

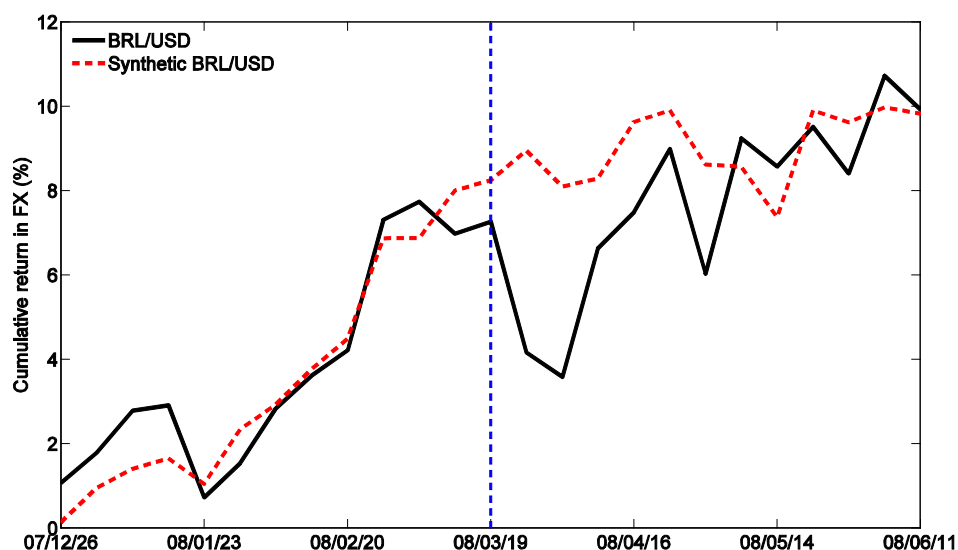


USD = US dollar.

Source: Authors' calculation.

¹⁹ These results for the Latin American funds are not presented in the figures but are available upon request.

Figure 1b: Taxing Fixed Income Investment—Exchange Rate
12 March 2008

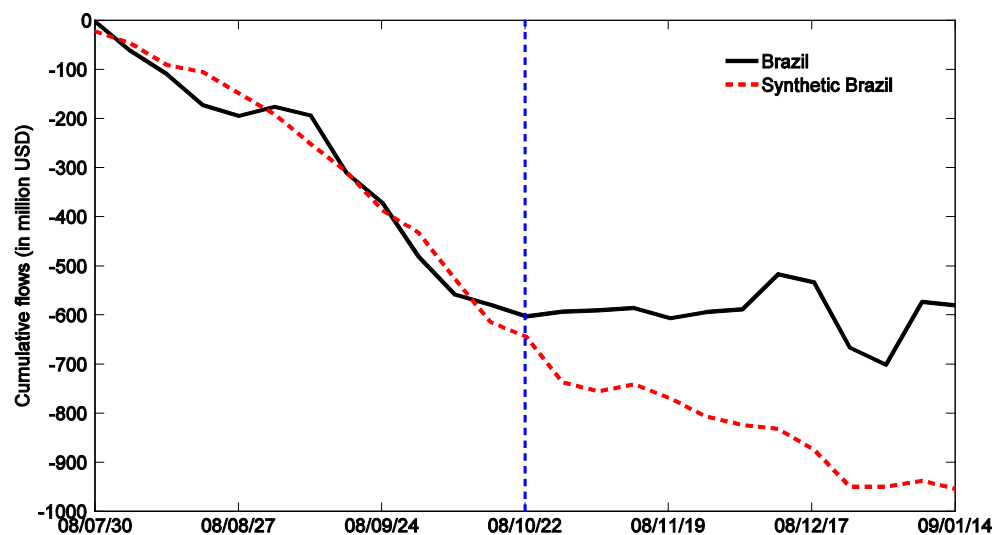


BRL = Brazilian real, FX = foreign exchange, USD = US dollar.

Source: Authors' calculation.

In Figure 2, we report on the second act—October 2008 (removing the fixed-income tax during the Lehman aftermath). Inflows were decreasing rapidly throughout the pre-crisis period starting in July 2008 (Figure 2a). We observe evidence of a slowdown in the capital outflows as a result of this removal of controls in October. The counterfactual Brazil (without the relaxation of controls) would have experienced a continuing capital flight. Latin American funds also seem to continue declining during this period, though at a slowing rate, which suggests that the removal of the tax on financial operations (IOF) had the intended effect. In Figure 2b we present the placebo test for this episode; the evidence only suggests an impact that is statistically observable as non-coincidental (i.e., the gap between the Brazilian flows and the counterfactual is bigger than for the majority of the placebos). The evidence regarding the exchange rate is not as robust (Figure 2c), but there does appear to be a longer term impact on the exchange rate than the one we observed in the first episode. Even that, however, appeared to be a transitory phenomenon.

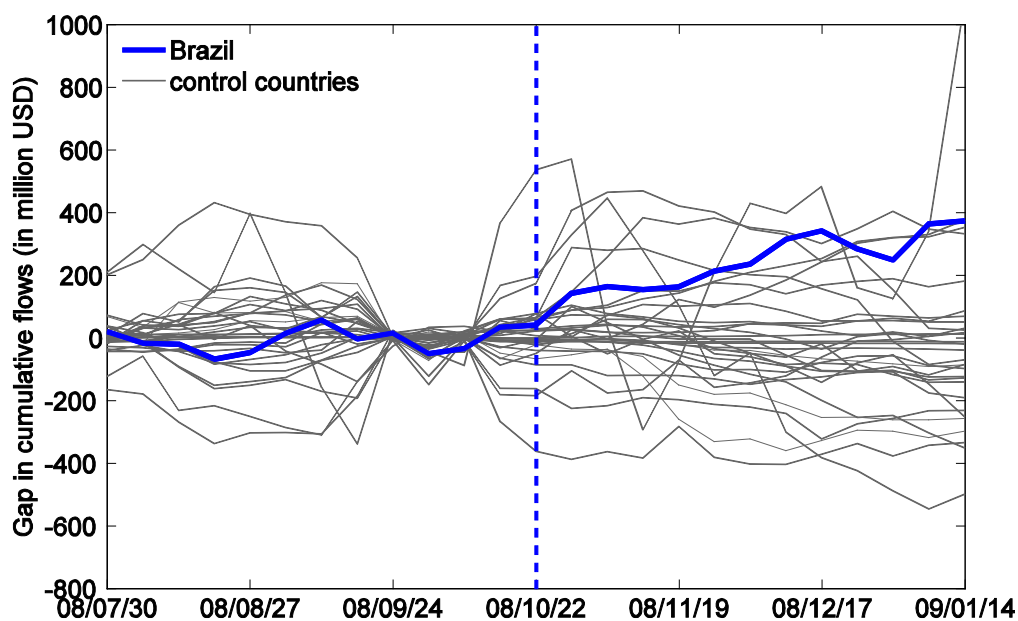
Figure 2a: Cutting Fixed Income Tax—Capital Flows
23 October 2008



USD = US dollar.

Source: Authors' calculation.

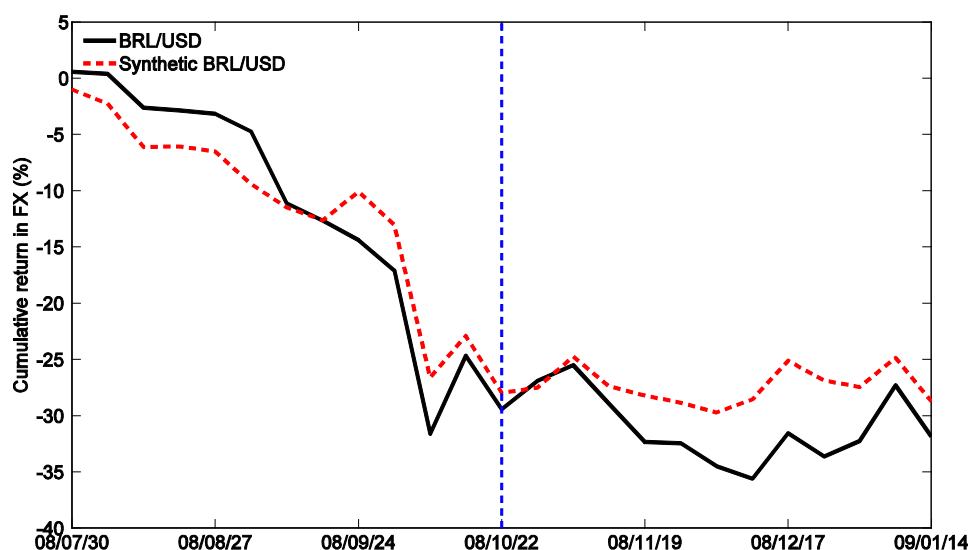
Figure 2b: Cutting Fixed Income Tax—Placebos
23 October 2008



USD = US dollar.

Source: Authors' calculation.

Figure 2c: Cutting Fixed Income Tax—Exchange Rate
23 October 2008



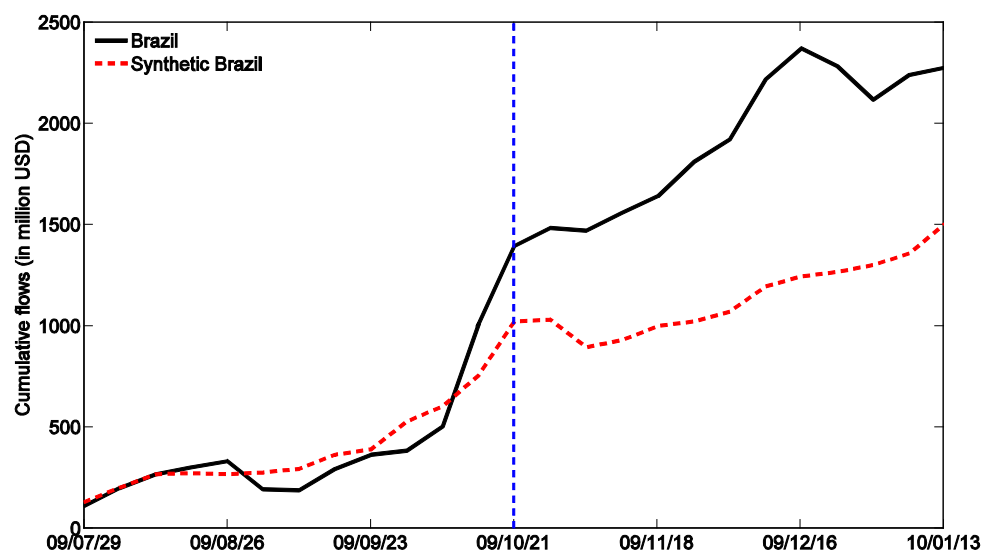
BRL = Brazilian real, FX = foreign exchange, USD = US dollar.

Source: Authors' calculation.

Third act—October 2009 (taxing both equity and bonds at 2%): The policy aim was to reduce inflows, and that did not seem to work (Figure 3a). Brazil continued experiencing inflows as did the rest of Latin America (if anything, the inflows for Brazil are rising faster than for other Latin American funds).²⁰ We find no evidence that the imposition of controls had any impact on the exchange rate (Figure 3b).

²⁰ If anything, inflows increased further after the tightening of controls. Since the controls were imposed as a capital inflow surge was beginning, it is difficult to speculate whether the controls were somehow a signal that encouraged further flows (a possibility that is suggested in the survey data that Forbes et al. [2012] present).

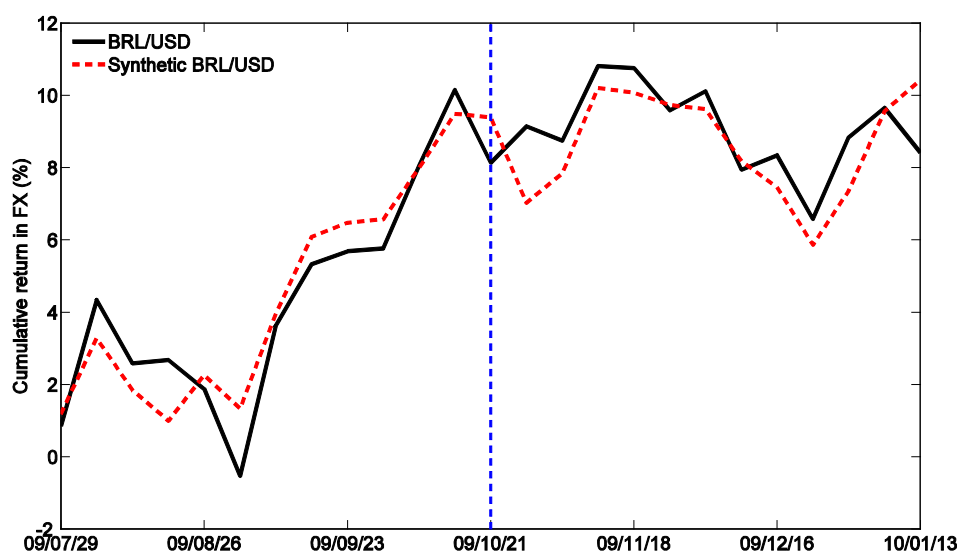
Figure 3a: Taxing Stock and Bond Investment at 2%—Capital Flows
20 October 2009



USD = US dollar.

Source: Authors' calculation.

Figure 3b: Taxing Stock and Bond Investment at 2%—Exchange Rate
20 October 2009



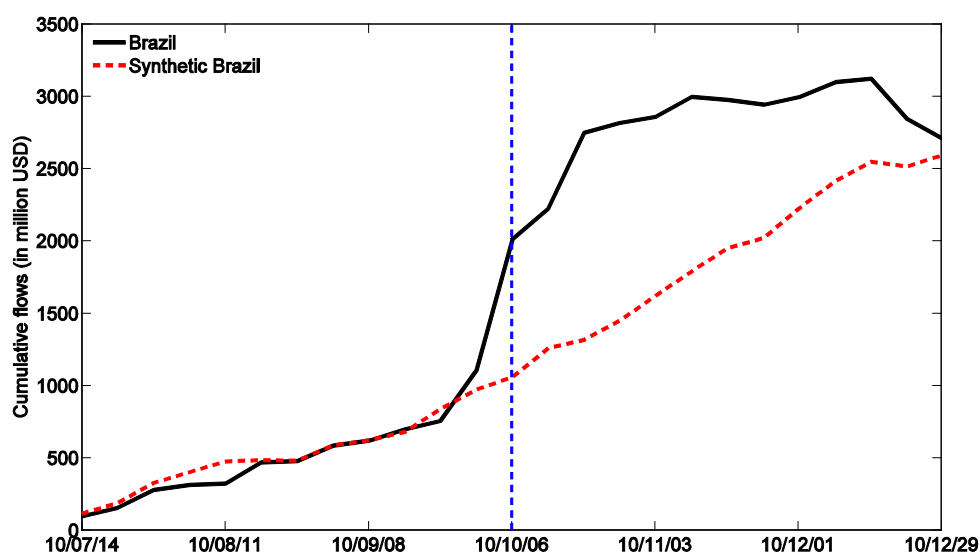
BRL = Brazilian real, FX = foreign exchange, USD = US dollar.

Source: Authors' calculation.

Fourth act—October 2010 (tax going up to 4% on fixed income): In Figure 4a, we again observe an ineffective control as increase in the IOF does not interrupt the continuing inflow episode (as it did for other Latin American countries, but with a bigger impact for Brazil). In both acts three

and four of 2009 and 2010, the post-control inflow boom episodes seem to be large and unique (since the actual is significantly larger than the synthetic and unique to Brazil relative to Latin American funds). The controls did not manage to stem the volume of these inflows, though they may have produced other desirable outcomes (more on that below). In the next change in policy, presented in Figure 4b, the IOF was further increased to 6% only 2 weeks after the previous increase (October 2010). Again, the further tightening of controls appears ineffective in stemming inflows. We do not show the corresponding figures for the exchange rate, but the results are similarly nonsignificant.²¹

Figure 4a: Increasing Taxes from 2% to 4%—Capital Flows
4 October 2010

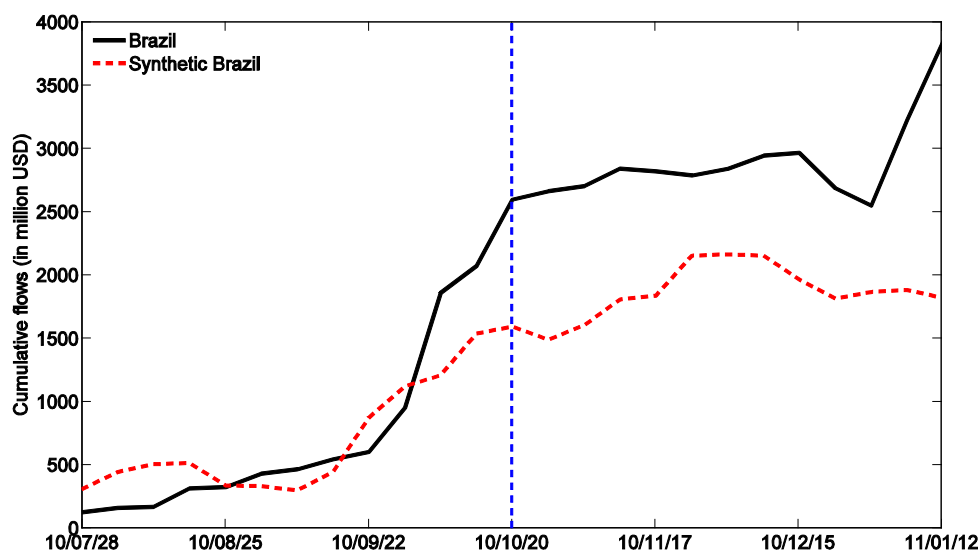


USD = US dollar.

Source: Authors' calculation.

²¹ Results are available upon request from the corresponding author.

Figure 4b: Increasing Taxes from 4.00% to 6.00%—Exchange Rate
(and increasing taxes on margins from 0.38% to 6.00%)
18 October 2010



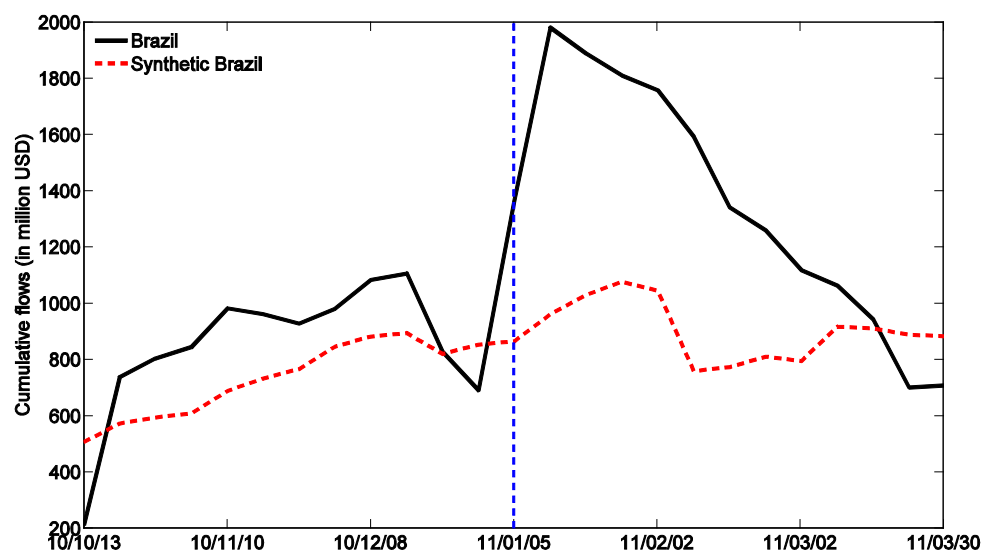
USD = US dollar.

Note: The sample period overlaps with the last synthetic analysis. The large gap several weeks before the capital control may be attributed to previous control.

Source: Authors' calculation.

Fifth act—January 2011 (reducing taxes on equities). In Figure 5a, we observe a short-run surge in equity investment that is unique to the Brazil funds and may be attributable to the reduction in the tax on equities. But this surge reverses quickly; and post-reversal decline is equivalent to a general decline in funds going to Latin America in the first 3 months of 2011. This conclusion is borne out when examining the placebos graphed in Figure 5b; again, we surmise that a relaxation of controls appeared to have a very short-term, but both statistically and economically meaningful, impact on capital flows. In the longer term (3 months in our framework), there does not seem to be any significant impact. The same findings, a brief deviation from the counterfactual and a reversion back to the pre-change equilibrium, can also be found in the estimations of the exchange rate (Figure 5c).

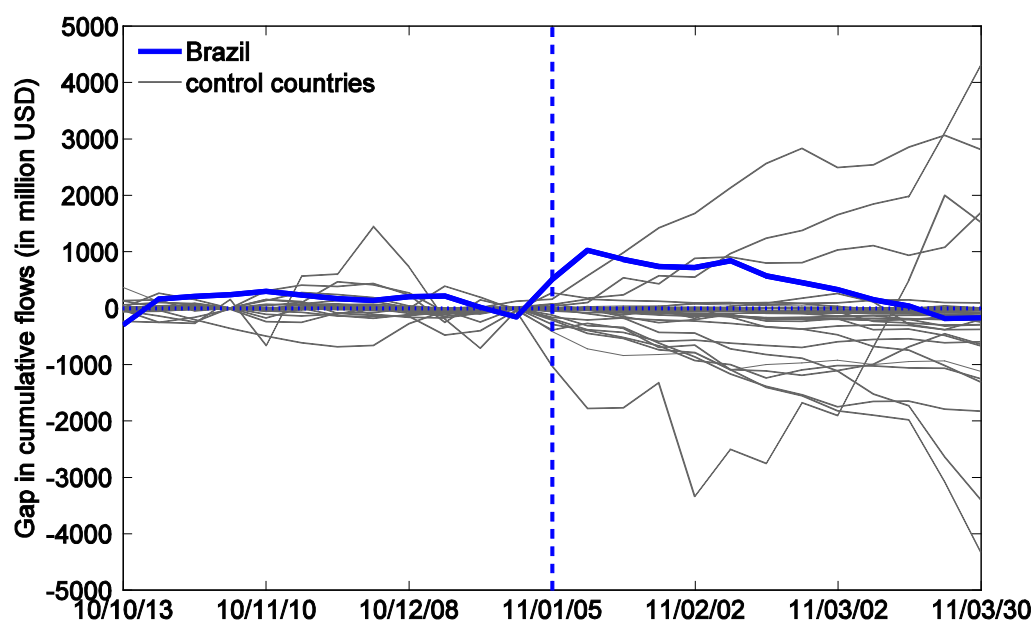
Figure 5a: Reducing Taxes from 6% to 2%—Capital Flows
3 January 2011



USD = US dollar.

Source: Authors' calculation.

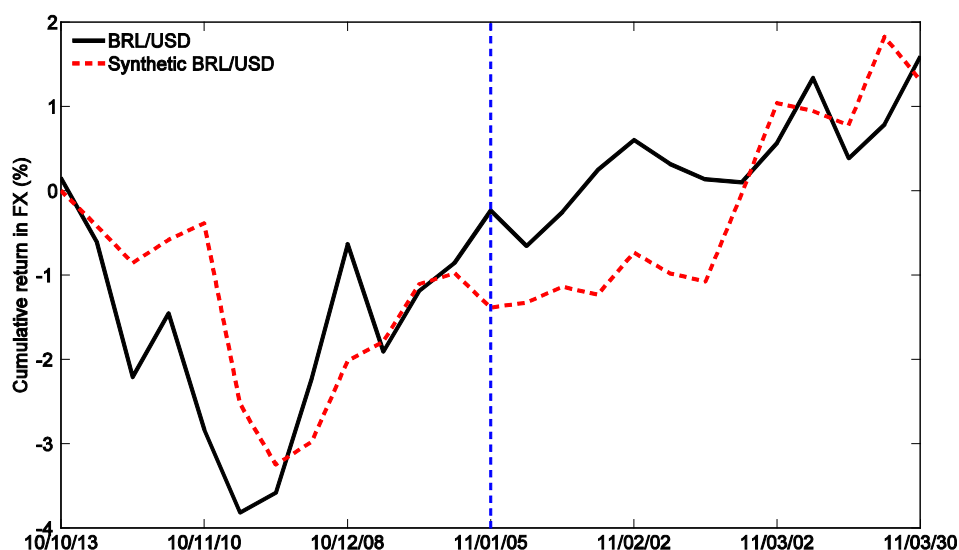
Figure 5b: Reducing Taxes from 6% to 2%—Placebos
3 January 2011



USD = US dollar.

Source: Authors' calculation.

Figure 5c: Reducing Taxes from 6% to 2%—Exchange Rate
3 January 2011



BRL = Brazilian real, FX = foreign exchange, USD = US dollar.

Source: Authors' calculation.

4.2 Summary of Empirical Findings—Capital Controls as a Signal

To summarize, after controlling for the counterfactual (Brazil with no capital account policy change) for each event in which Brazil modified its capital controls during the first 3 years of the global financial crisis, we find no evidence that any tightening of controls was effective in reducing the magnitudes of capital inflows into the country. We do observe some modest success in preventing further declines in inflows when the capital controls are relaxed, as was done in the immediate aftermath of the Lehman bankruptcy in 2008 and the associated massive credit contraction worldwide. A similar modest success can be attributed to the decision by the Government of Brazil to reduce taxes in January 2011.

Both of these decisions to relax controls were instituted during a capital outflow episode, and these successes were more evident in preventing further decreases in capital inflows than in any sustained impact on the exchange rate. These results complement survey responses described in Forbes et al. (2012). In these surveys of investment managers, the overall conclusion Forbes et al. (2012) reach is that investment managers' reactions to fairly limited capital account policy changes in a large open economy like Brazil are very muted and remarkably heterogeneous. Given these heterogeneous sentiments, it may not be a surprise that we find so little impact that can be accounted for by the tightening of the capital account regime.

Another possibility is that the controls did have a portfolio balancing impact even if they had no impact on funds' size, since the households and corporations and other entities that invest in funds did not react to the imposition of controls. It may still be the case that fund managers did divert money from Brazil to other countries within the funds they manage. Given the data limitations, we cannot test this possibility.

Why did we find an asymmetric impact? Why is there a significant impact when controls were removed? The interviews that were conducted by Forbes et al. (2012) suggest that many money managers were more interested in the signal content of the capital account policy change rather than in the direct impact of the changes on their tax liability and therefore on their bottom lines. Brazil, throughout this period, was controlled by the left-of-center Workers' Party headed by Lula.²² Our hypothesis is that price-based, mild capital controls' only perceptible effects are to be found in the content of the signal they broadcast regarding the government's larger intentions and sensibilities. In Lula's case, the government was widely perceived as ambivalent to markets, and especially to the international capital markets. Thus, an imposition of (fairly mild) controls was not perceived as news and thus had no impact. A willingness to remove controls, however, as happened in October 2008 in the middle of the global post-Lehman financial panic and then again in January 2011, just after the transition from Lula to Dilma Rousseff's administration, were both perceived as noteworthy indications that the government was not as hostile to the international financial markets as many expected it to be. The removal of controls was thus effective since the presumption was that the government had some antipathy to financial markets and foreign investors. This suggests that the same policy may have a different impact within a political environment in which the government is perceived differently.

4.3 Brazil and BRIC

The weights we obtained in constructing our synthetic controls (see Appendix D) have no real economic meaning, but do describe the conditional correlation between flows to Brazil and flows to the other countries in our sample. Interestingly, the other BRIC countries (the Russian Federation, India, and the People's Republic of China [PRC]) seem to figure quite prominently as controls (with the occasional addition of Australia; Indonesia; Mexico; and Taipei, China). This is interesting in and of itself, since the Russian Federation and Brazil are large commodity exporters, and India and the PRC's sectoral composition of trade is quite different. Investment managers of the funds included in the dataset seem to treat BRIC as similar substitutes and capital inflows to them appear to be motivated similarly. Capital flows in Brazil are correlated much more closely with the other members of the BRIC club rather than with regional neighbors like Argentina or Chile, and other large agricultural exporters like Colombia or Thailand.

Forbes et al. (2012) focus on the externalities created by the imposition of capital controls, and how the imposition of controls in one country (Brazil) may lead to reallocation of portfolio shares that may have an impact on other countries' capital flows. We therefore estimate the impact of Brazil's five episodes of change in its capital account regime on the other BRIC club members, the Russian Federation, India, and the PRC. In most cases, we do not observe any statistically visible deviation between the synthetic and the actual flows—so Brazil's policy changes had no apparent impact. In a few instances, however, there do seem to be notable deviations, in particular for the PRC in the first episode (an inflow surge), the Russian Federation in the second episode (outflow), the PRC again in the third episode (again an inflow surge), and India and the PRC in the fifth episode (outflow). But these deviations fit with our notion that Brazil is attempting to “lean against the wind” (countercyclical policy) while the other BRIC countries are facing the same head- or tail- winds themselves. Unlike Forbes et al. (2012), we are hesitant to conclude that this is a sign of an externality; it is equally plausible that these changes in controls were implemented when all the BRIC countries were experiencing very similar capital inflow surges or capital flights and/or retrenchments.

²² Luiz Inácio Lula da Silva. Replaced on 1 January 2011 by Dilma Rousseff from the same left-of-center political party.

5. WHAT CAN ASIA LEARN?

The experiences of Brazil with capital controls suggest that the effectiveness of capital controls should be viewed on a scenario-based analysis. In the case of Brazil, a hypothesis for explaining the asymmetric effects of capital controls is possibly a political economy one. Foreign investors may take the elimination or relaxation of the IOF tax as an investor-friendly policy signal (hence the material effect on net inflows of international mutual funds), whereas the imposition of capital controls was not considered a surprise or unanticipated news for foreign investors, given the left-of-center government was in charge. The policy implementation should therefore take into account the perception of the market investors, both on the policy and on the tax liability effect of the actual control measure.

Yet, facing the uncertainty of foreign capital monetary authorities in Asia may be forced to reconsider the effectiveness of capital control measures, in relation to their long-term objectives, i.e., growth, inflation, productivity, and employment. Thailand imposed capital control in December 2006, only for them to be removed almost immediately after the sharp negative reaction of the domestic equity market. Against the uncertainty of outcomes, Brazil has reimposed and subsequently relaxed control controls since 2008, with the majority of measures eventually relaxed by the end of 2012. Across emerging markets, the beginning of 2013, however, sees the resurgence of foreign capital inflows and the continual appreciation of currencies, notably in Asia (the Republic of Korea and Thailand) and Latin America (Peru). Whether capital control would be the right policy measure this time around is debatable, and using the counterfactual-Thailand or the counterfactual-Peru may be helpful. Clearly, any policy consideration has to take into account the implementation of capital control measures in conjunction with other policies, political economy, and market factors. Capital controls may be supplemental to foreign exchange intervention, with respect to the elasticity, composition, and level of capital flows (Blanchard et al. 2013; Garcia and Chamon 2013). When it comes to managing capital flows, the choice is not always easy; as for other interventions, capital control is distortionary (relative to leaving market forces on interest rates and currency appreciation) and tends to lose its effectiveness over time (international investors can find loopholes across financial instruments and/or move to other uncontrolled markets).

6. CAVEATS AND FUTURE RESEARCH

In an IMF April 2011 meeting discussing the IMF's guidelines for supporting the use of capital controls, the Brazilian finance minister Guido Mantega voiced his opposition. He declared: "We oppose any guidelines, frameworks or "codes of conduct" that attempt to constrain, directly or indirectly, policy responses of countries facing surges in volatile capital inflows. Governments must have flexibility and discretion to adopt policies that they consider appropriate" (Reddy 2011). The Government of Brazil, as well as other representatives from emerging markets, found the IMF's limited support of capital controls as a prudential policy tool too limited and constraining, and argued for a broader mandate to use this set of tools.

In what can be perceived as a limited confirmation of this concern, IMF researchers recently concluded, in the case of several Eastern European countries that were experiencing heavy inflows, that the conditions prevailing in these cases did not justify the imposition of controls; and advocated more conventional monetary and fiscal adjustments (Chowdhury and Keller 2012). If one uses the broad framework that the IMF suggests, however, on most accounts Brazil in 2008–2010 appeared to have been a good candidate for the imposition of controls.

It is remarkable, therefore, that we fail to find much impact of these controls given their intended rationale in limiting the volume of capital flowing into a potentially overheated economy, and the vocal support these policies have garnered from many corners of the policy world. These findings suggest that mild price-based controls appear effective only if they are surprising and provide a signal regarding the government's larger policy trajectory. Clearly, using controls as a signal is both costly, inefficient, and can only be used infrequently. A sceptical reader may, of course, suggest that our results may not be robust and our failure to uncover the direct impacts of controls (bar their signalling impact) is a failure of our methodology. While this is a possibility, a spate of other recent work has also failed to find much evidence for a significant impact of mild controls, or is generally sceptical of any claims of the efficacy of these controls—such as those that were implemented in the Brazilian case (e.g., Calvo 2010; Warnock 2011; Edwards 2012; Fratzscher 2012; Straetmans et al. forthcoming; and Chari forthcoming).

An additional possibility is that the Brazilian case is not representative and any conclusions will not be applicable elsewhere. This is a general criticism of any case study, as the applicability of a case is never assured; again, we argue that given the prevalence of similar conclusions in this recent spate of cross-country research projects, we believe the Brazilian case is most likely representative of the larger pattern: mild capital controls are largely ineffective unless they provide a signal regarding the general trajectory of government economic policy (draconian controls are, by definition, effective).

The reasons for instituting these policies, of course, may be political and electoral in nature, rather than being truly guided by a desire to obtain any of the impacts we described. It may be indeed that policymakers fully understand the inability of these controls to have any substantial impact, but nevertheless resort to adopting them. We leave that possibility for future work.

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APPENDIX A: ESTIMATION RESULTS FOR SYNTHETIC ANALYSIS

The following set of tables compares the pre-treatment characteristics of the treated (actual) Brazil with that of the synthetic Brazil. Synthetic Brazil is constructed as the combination of countries chosen from the sample that most closely resembled Brazil in terms of capital flows before Brazil introduced the capital account policy change specified in the title of each table (and the date it was implemented). Appendix C contains a full list of countries and their synthetic weights used to construct the synthetic observation. The reported statistics are the mean values of the actual and synthetic explanatory variables for the pre-treatment periods, which are 12 weeks prior to the week of policy change. Root mean squared prediction error (RMSPE) is calculated as the root mean of the weighted squared distance between the treated and synthetic capital flows for the pre-treatment periods.

Table A1: Estimation Results—Taxing Fixed-Income Investment

12 March 2008		
Variables	Treated	Synthetic
Cumulative Flow on the week ending at 6 February 2008	(712.080)	(724.852)
Cumulative Flow on the week ending at 27 February 2008	(816.750)	(817.039)
Mutual Fund Return	6.086	1.972
Log (Total Net Assets)	9.464	8.863
Weekly Return on Stock Market Index	1.904	(0.473)
Weekly Return on Bond Market Index	1.898	1.812
Weekly Return on Foreign Exchange Rate	5.704	3.991
RMSPE	53.084	

() = negative, RMSPE = root mean squared prediction error.

Source: Authors' calculation.

Table A2: Estimation Results —Cutting Fixed-Income Tax

23 October 2008		
Variables	Treated	Synthetic
Cumulative Flow on the week ending at 24 September 2008	(370.620)	(386.686)
Cumulative Flow on the week ending at 8 October 2008	(557.050)	(524.667)
Mutual Fund Return	(54.740)	(42.002)
Log (Total Net Assets)	9.225	8.689
Weekly Return on Stock Market Index	(36.250)	(35.266)
Weekly Return on Bond Market Index	2.084	0.219
Weekly Return on Foreign Exchange Rate	(21.446)	(9.756)
RMSPE	36.664	

() = negative, RMSPE = root mean squared prediction error.

Source: Authors' calculation.

Table A3: Estimation Results—Taxing Stock and Bond Investment at 2%
20 October 2009

Variables	Treated	Synthetic
Cumulative Flow on the week ending at 5 August 2009	197.340	199.387
Cumulative Flow on the week ending at 12 August 2009	265.390	266.513
Mutual Fund Return	26.661	25.029
Log (Total Net Assets)	9.609	8.558
Weekly Return on Stock Market Index	18.949	23.702
Weekly Return on Bond Market Index	1.358	2.574
Weekly Return on Foreign Exchange Rate	8.262	5.382
RMSPE (Root Mean Squared Prediction Error)	101.242	

() = negative, RMSPE = root mean squared prediction error.

Source: Authors' calculation.

Table A4a: Estimation Results – Increasing Taxes from 2% to 4%

4 October 2010

Variables	Treated	Synthetic
Cumulative Flow on the week ending at 25 August 2010	480.330	480.233
Cumulative Flow on the week ending at 1 September 2010	587.620	587.774
Mutual Fund Return	10.150	10.112
Log (Total Net Assets)	9.921	9.869
Weekly Return on Stock Market Index	7.818	7.779
Weekly Return on Bond Market Index	1.632	1.293
Weekly Return on Foreign Exchange Rate	3.482	3.825
RMSPE	70.546	

RMSPE = root mean squared prediction error.

Source: Authors' calculation.

Table A4b: Estimation Results—Increasing Taxes from 4% to 6%**18 October 2010**

Variables	Treated	Synthetic
Cumulative Flow on the week ending at 25 September 2010	606.060	874.442
Cumulative Flow on the week ending at 13 October 2010	2,074.290	1,536.694
Mutual Fund Return	14.347	11.746
Log (Total Net Assets)	9.964	10.615
Weekly Return on Stock Market Index	9.170	10.649
Weekly Return on Bond Market Index	1.536	0.365
Weekly Return on Foreign Exchange Rate	5.048	1.718
RMSPE	307.223	

RMSPE = root mean squared prediction error.

Source: Authors' calculation.

Table A5: Estimation Results: Reducing Taxes from 6% to 2%**3 January 2011**

Variables	Treated	Synthetic
Cumulative flow on the week ending at 3 November 2010	844.590	607.302
Cumulative flow on the week ending at 29 November 2010	688.920	851.904
Mutual Fund Return	(0.064)	3.382
Log (Total Net Assets)	10.115	9.082
Weekly Return on Stock Market Index	(1.809)	5.026
Weekly Return on Bond Market Index	1.352	1.873
Weekly Return on Foreign Exchange Rate	(0.657)	2.985
RMSPE	206.968	

() = negative, RMSPE = root mean squared prediction error.

Source: Authors' calculation.

APPENDIX B: WERE BRAZILIAN CAPITAL CONTROLS ANTICIPATED?

In this appendix, we examine an empirical association between the announcements of capital control measures and any potential market and public anticipation of these announcements. We aim to verify that the timing of announcements used in the main study is indeed a surprise (regardless of its news content and economic significance) and therefore cannot be forecasted quantitatively by capital market indicators and surveys. While our main findings in sections 2–4 are based on five major announcements from 2008 to 2011, as we were constrained by the weekly fund flows data, this appendix reports several tests over the whole post-global financial crisis sample period, 2008–2012, and provides a description of all announcements of control measures, as briefly outlined in the following.

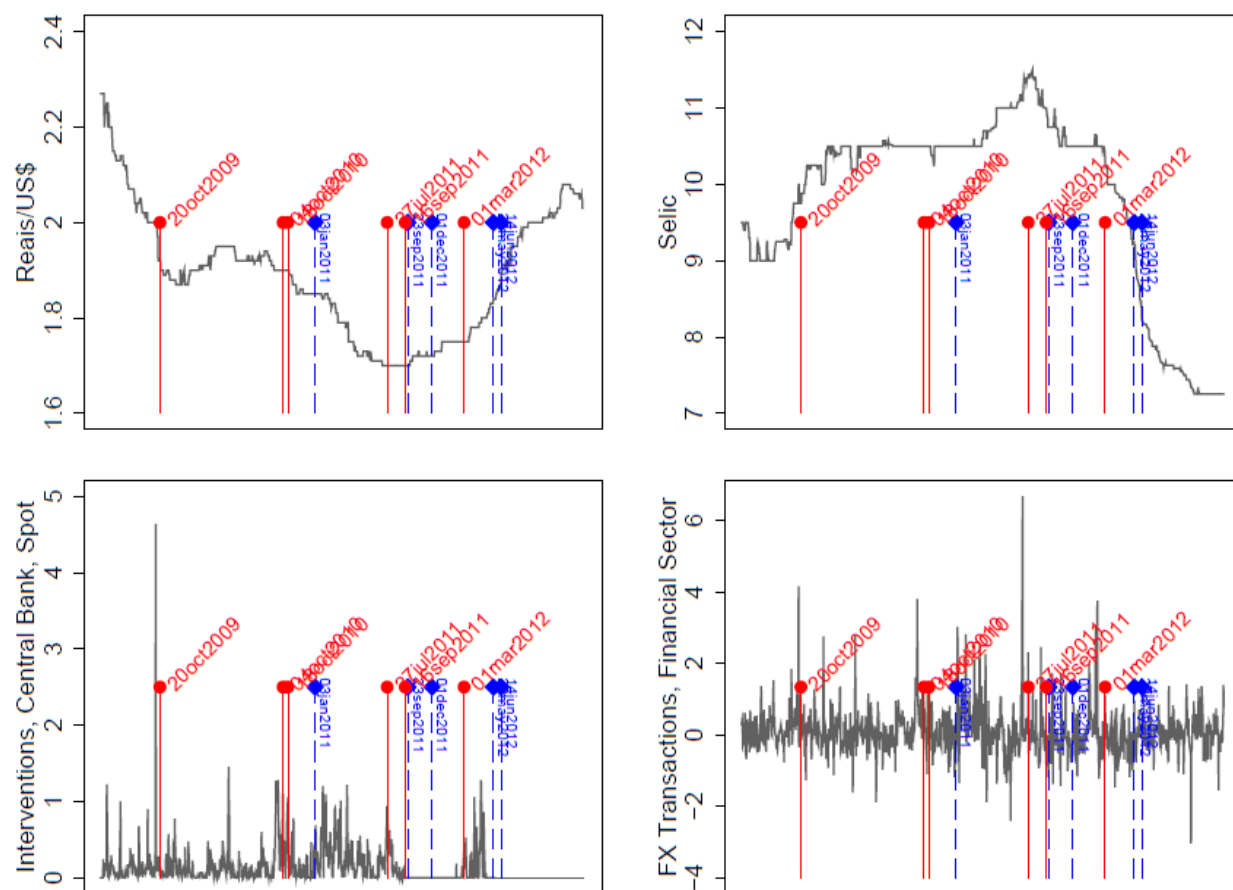
The tax on financial operations (IOF) that was initially imposed on foreign investors' bond investments was announced on 12 March 2008. On 23 October 2008, the government announced the elimination of the financial transaction tax on foreign investors, which was 1.50% on foreign exchange transactions for capital inflows and 0.38% on foreign currency loans. By mid-October 2009, the Brazilian real appreciated markedly and the IOF was re-imposed on 20 October 2009. In September 2010, the Brazilian real appreciation increased markedly again, and on 4 October 2010, the government announced an immediate increase of the IOF on bonds from 2% to 4%. On 18 October 2010, the government announced an additional IOF increase from 4% to 6%. By early December 2010, the market was speculating whether the IOF might be raised further from 6% on bond investments, but it remained 2% on equity investments (since speculation in the stock market did not seem to pose much threat). On 3 January 2011, the government announced a reduction in the IOF on private equity funds, venture capital funds, and depository receipts from 6% to 2%. By mid-2011, as the appreciation of the Brazilian real regained momentum, the government announced a tax on trading of currency derivatives on 27 July 2011, and outlined its detailed plan on 16 September 2011 for a 1% tax on currency derivatives, but on 23 September 2011 it decided to delay the tax until the end of December. As the threat of the euro crisis overshadowed global markets, the government announced on 1 December 2011 that the IOF on equity investments and corporate bond investments was immediately scrapped. By early March 2012, however, the appreciation of the Brazilian real renewed and the government announced on 1 March 2012 that the 6% IOF on foreign loans with maturities up to 2 years now extended to those with maturities up to 3 years. On 23 May 2012, the government decided to drop the IOF on the purchase of derivatives instruments for exporters, and on 14 June 2012, the government announced a reduction in the financial transaction tax on foreign loans to domestic firms.

To ascertain whether the public anticipated the announcements, we look at the daily movements of several capital market indicators, including business surveys of expectation concerning the Brazilian real–US dollar exchange rate and the Special System for Settlement and Custody (Selic) rates, as well as spot foreign exchange interventions by the Banco Central do Brasil (\$ billion) and the net (purchases minus sales) foreign exchange transactions by the financial sector in Brazil (\$ billion). We collected the data from DataStream, which make available these daily series from 4 May 2009; we are thus able to study 11 out of the 13 dates mentioned above. Figure B1 plots the four series, marked with the 11 announcement dates. It appears the announcements were not forecast by these market indicators but there are some possibilities: on one occasion for an increase in the capital control measure announced on 20 October 2009, when it was preceded by a significant intervention in spot foreign exchange markets by the central bank on 8 October 2009; and on several occasions when correlations

appeared between net foreign exchange transactions by the financial sector and decreases of capital control measures.

We formally test the relationship between the announcement dates and the market indicators by using probabilistic regressions. Based on the Dickey-Fuller tests, we find that the Brazilian real–US dollar and the Selic series are non-stationary; these are then differenced. As a first step, we estimate a probit model of announcement dates on the four variables contemporaneously. Columns (1) and (2) of Table B1 report the estimation results for the six increases and the five decreases of the control announcement dates, respectively. We find the central bank's intervention is positively associated with the increase announcements, while the Brazilian real's depreciation is positively associated with the decrease announcements. However, the explanatory power of both probit regressions, as measured by the Pseudo R^2 , is very low. Next, we proceed with a Bivariate probit regression, whereby the increase announcement is a function of the Selic rate, central bank intervention, and net (purchases minus sales) foreign exchange transactions by the financial sector; and the decrease announcement is a function of the Brazilian real–US dollar, Selic rate, and net foreign exchange transactions by the financial sector, and both functions are estimated simultaneously. To account for any lagged effects of these market indicators, we also include their lags up to 3 days preceding the announcement dates. The estimation results in column (3) of the table suggest that any association between the announcement dates and the market indicators is rather weak and not statistically significant. Neither the increase nor the decrease announcements signify any statistical relationship with the capital market indicators, all of which are publicly available. Therefore, we conclude that these policy changes were not anticipated in any rigorous (actionable) way by market participants for the episodes of capital control measures that we are focusing on.

Figure B1: Announcements of Capital Control Measures and Capital Market Indicators



FX = foreign exchange, Selic = Special System for Settlement and Custody, US = United States.

Note: This figure plots the daily series of business surveys of expectation regarding the Brazilian real–US dollar exchange rate and Selic rates, as well as spot foreign exchange interventions by Banco Central do Brasil (\$ billion) and the net (purchases minus sales) foreign exchange (FX) transactions by financial sectors in Brazil (\$ billion). The drop lines in solid identify announcement dates of capital control tightening (increase); the drop lines in dash identify announcement dates of capital control loosening (decrease).

Source: Authors' calculation.

Table B2: Anticipation of the Announcements of Capital Control Measures

Capital Control	Probit				Bivariate Probit			
	(1)		(2)		(3)		(3)	
	Increase		Decrease		Increase		Decrease	
Surveyed Var.	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Brazilian real–US dollar _t	3.90	(3.17)	31.01	(13.18) ^b			27.01	(38.70)
Selic _t	-2.70	(2.19)	-1.94	(1.32)	-6.87	(5.33)	-0.82	(6.70)
Intervention _t	0.53	(0.26) ^b	-0.63	(1.17)	-1.72	(3.86)		
FX Gross Flows _t	0.20	(0.16)	-0.19	(0.30)	-0.20	(0.45)	-0.21	(0.53)
Brazilian real–US dollar _{t-1}					-1.66	(9.19)	-7.57	(4.78)
Brazilian real–US dollar _{t-2}					-0.55	(6.16)	-9.00	(5.69)
Brazilian real–US dollar _{t-3}					1.94	(5.03)	-0.92	(4.77)
Selic _{t-1}					0.86	(0.91)		
Selic _{t-2}					2.02	(3.72)		
Selic _{t-3}					-12.26	(14.48)		
Intervention _{t-1}					-0.08	(0.49)	-0.38	(0.50)
Intervention _{t-2}					-0.34	(0.52)	-0.24	(0.46)
Intervention _{t-3}					-0.26	(0.68)	-0.76	(0.71)
FX Gross Flows _{t-1}							-19.32	(58.91)
FX Gross Flows _{t-2}							70.46	(69.06)
FX Gross Flows _{t-3}							5.84	(47.87)
Constant term	-2.70	(0.17) ^a	-2.58	(0.18) ^a	-2.64	(0.42) ^a	-3.15	(0.59) ^a
No. of days	979		979		391			
Pseudo R ²	0.0825		0.0560		Prob.>chi ² =0.9937			

FX = foreign exchange, Selic = Special System for Settlement and Custody, US = United States., Var. = Variable, coeff. = coefficient, s.e. = standard error, R² = goodness of fit, chi² = Chi-Square distribution.

Note: This table reports an empirical association between the announcements of capital control measures and any potential market and public anticipation. The estimation is based on 11 (out of 13) announcement dates from 2008 to 2012. The daily series, collected from DataStream, are available from 4 May 2009, and include business surveys of expectation regarding the Brazilian real–US dollar exchange rate and Selic rates, as well as spot foreign exchange interventions by the Banco Central do Brasil (\$ billion) and the net (purchases minus sales) foreign exchange transactions by financial sectors in Brazil (\$ billion). Based on the Dickey-Fuller tests, the Brazilian real–US dollar exchange rate and the Selic series are non-stationary; these two series are first differenced. Standard errors are in parentheses.

^a Denotes statistical significance at 1% level.

^b Denotes statistical significance at 5% level.

^c Denotes statistical significance at 10% level.

Source: Authors' calculation.

APPENDIX C: CORRELATION BETWEEN EMERGING PORTFOLIO FUND RESEARCH AND BALANCE OF PAYMENTS STATISTICS DATA

This table reports the correlation coefficients between Emerging Portfolio Fund Research (EPFR) mutual fund flows and three official measures of cross-border capital flows reported by the International Monetary Fund (IMF) Balance of Payments (BOP) statistics—the net portfolio investment (PI), the net equity component of the portfolio investment (EPI), and the foreign direct investment (FDI). The sample period is from the fourth quarter (Q4) of 2007 to Q1 2011.

Country	$\rho_{PI,EPFR}$	$\rho_{EPI,EPFR}$	$\rho_{FDI,EPFR}$	Country	$\rho_{PI,EPFR}$	$\rho_{EPI,EPFR}$	$\rho_{FDI,EPFR}$
Australia	0.174	(0.053)	0.174	Republic of Korea	0.173	(0.167)	(0.181)
Austria	0.076	(0.211)	0.428	Malaysia	0.585^c	0.454	(0.317)
Belgium	0.401	0.150	0.367	Mexico	(0.263)		0.194
Brazil	0.245	0.478	0.140	Netherlands	0.304	0.173	(0.155)
Canada	0.196	0.167	0.210	New Zealand	0.566	0.169	(0.350)
Chile	(0.211)	(0.121)	0.162	Norway	(0.217)	0.192	0.104
PRC	(0.086)	(0.032)	0.249	Philippines	0.543^c	0.509	(0.174)
Colombia	0.104		(0.900)^a	Poland	0.231	0.065	(0.632)
Czech Republic	(0.121)	0.031	0.675^b	Portugal	(0.157)	(0.179)	0.874^c
Denmark	(0.376)	(0.355)	(0.537)	Russian Federation	0.563^c	0.601^c	0.032
Egypt	(0.379)			Singapore	(0.263)		0.139
Finland	(0.743)	(0.564)	(0.479)	South Africa	(0.055)	0.058	0.050
France	0.023	(0.059)	0.044	Spain	(0.018)	0.036	(0.261)
Germany	(0.399)	(0.033)	0.139	Sweden	(0.078)	0.457	(0.260)
Greece	0.169	0.040	(0.120)	Switzerland	(0.272)	0.171	(0.353)
Hong Kong, China	(0.086)	0.086	(0.115)	Taipei, China	(0.005)		0.299
India	0.731^b	0.652	0.006	Thailand	0.022	0.076	0.283
Indonesia	0.163	0.838^a	(0.044)	Turkey	0.081		(0.149)
Israel	0.037	0.080	0.552^c	United Kingdom	(0.284)	(0.287)	0.009
Italy	(0.058)	(0.505)	0.502	United States	0.000	(0.004)	0.338
Japan	0.242	0.460	0.239	Viet Nam	0.228		(0.172)

() = negative, PRC = People's Republic of China.

^a Denotes statistical significance at 1% level.

^b Denotes statistical significance at 5% level.

^c Denotes statistical significance at 10% level.

Sources: International Monetary Fund (IMF) Balance of Payments Statistics (BOP); Central Bank of the Republic of China (Taipei, China); and Central Bank of Egypt.

APPENDIX D: COUNTRY WEIGHTS FOR SYNTHETIC CAPITAL FLOWS TO BRAZIL

For each episode of capital control, we study 12 weeks (about one quarter) before and after the week of its implementation. A country is included as a possible component of the control group if, for the given 25-week sample period, it did not introduce any capital controls and it had no missing observations for the selected control variables: mutual fund return, total net assets (TNA), weekly return on stock market index, weekly return on bond market index, and weekly return on foreign exchange rate.

Table D1: Taxing Fixed-Income Investment
12 March 2008

Country	Weight	Country	Weight	Country	Weight
Australia	0.119	Indonesia	0.000	Singapore	0.000
Austria	0.000	Israel	0.000	South Africa	0.000
Belgium	0.000	Italy	0.000	Spain	0.000
Canada	0.000	Japan	0.000	Sweden	0.000
Chile	0.051	Republic of Korea	0.000	Switzerland	0.000
PRC	0.000	Malaysia	0.000	Taipei, China	0.496
Czech Republic	0.000	Mexico	0.000	Thailand	0.000
Egypt	0.000	Netherlands	0.000	Turkey	0.000
France	0.000	Norway	0.000	United Kingdom	0.326
Hong Kong, China	0.000	Philippines	0.000	United States	0.009
India	0.000	Russian Federation	0.000	Viet Nam	0.000

PRC = People's Republic of China.

Source: Authors' calculation.

Table D2: Cutting Fixed-Income Tax
23 October 2008

Country	Weight	Country	Weight	Country	Weight
Australia	0.000	Indonesia	0.000	Singapore	0.000
Austria	0.000	Israel	0.000	South Africa	0.000
Belgium	0.000	Italy	0.000	Spain	0.000
Canada	0.000	Japan	0.000	Sweden	0.000
Chile	0.000	Republic of Korea	0.000	Switzerland	0.000
PRC	0.000	Malaysia	0.000	Taipei,China	0.294
Czech Republic	0.000	Mexico	0.140	Thailand	0.000
Egypt	0.000	Netherlands	0.000	Turkey	0.000
France	0.000	New Zealand	0.000	United Kingdom	0.000
Germany	0.000	Norway	0.000	United States	0.000
Greece	0.000	Philippines	0.000	Viet Nam	0.000
Hong Kong, China	0.000	Portugal	0.000		
India	0.322	Russian Federation	0.244		

PRC = People's Republic of China.

Source: Authors' calculation.

Table D3: Taxing Stock and Bond Investment at 2%
20 October 2009

Country	Weight	Country	Weight	Country	Weight
Australia	0.097	Greece	0.000	Russian Federation	0.456
Austria	0.000	Hong Kong, China	0.000	Singapore	0.000
Belgium	0.000	India	0.320	South Africa	0.000
Canada	0.000	Indonesia	0.126	Spain	0.000
Chile	0.000	Israel	0.000	Sweden	0.000
PRC	0.000	Italy	0.000	Switzerland	0.000
Colombia	0.000	Japan	0.000	Taipei,China	0.000
Czech Republic	0.000	Malaysia	0.000	Thailand	0.000
Denmark	0.000	Mexico	0.000	Turkey	0.000
Egypt	0.000	Netherlands	0.000	United Kingdom	0.000
France	0.000	New Zealand	0.000	United States	0.000
Germany	0.000	Norway	0.000	Viet Nam	0.000

PRC = People's Republic of China.

Source: Authors' calculation.

Table D4: Increasing Taxes from 2% to 4%**4 October 2010**

Country	Weight	Country	Weight	Country	Weight
Australia	0.000	Germany	0.312	Philippines	0.023
Austria	0.000	Greece	0.000	Poland	0.000
Belgium	0.000	Hong Kong, China	0.120	Singapore	0.000
Canada	0.000	India	0.306	South Africa	0.000
Chile	0.000	Israel	0.000	Spain	0.000
PRC	0.202	Italy	0.000	Sweden	0.000
Colombia	0.000	Japan	0.000	Switzerland	0.000
Czech Republic	0.000	Malaysia	0.000	United Kingdom	0.000
Denmark	0.000	Mexico	0.000	United States	0.000
Egypt	0.000	Netherlands	0.000	Viet Nam	0.000
Finland	0.000	New Zealand	0.000		
France	0.032	Norway	0.000		

PRC = People's Republic of China.

Source: Authors' calculation.

**Table D5: Increasing Taxes from 4.00% to 6.00%
and Increasing Taxes on Margins from 0.38% to 6%****18 October 2010**

Country	Weight	Country	Weight	Country	Weight
Australia	0.000	Germany	0.000	Philippines	0.000
Austria	0.000	Greece	0.000	Poland	0.000
Belgium	0.000	Hong Kong, China	0.000	Singapore	0.000
Canada	0.000	India	0.097	South Africa	0.000
Chile	0.000	Israel	0.000	Spain	0.000
PRC	0.903	Italy	0.000	Sweden	0.000
Colombia	0.000	Japan	0.000	Switzerland	0.000
Czech Republic	0.000	Malaysia	0.000	United Kingdom	0.000
Denmark	0.000	Mexico	0.000	United States	0.000
Egypt	0.000	Netherlands	0.000	Viet Nam	0.000
Finland	0.000	New Zealand	0.000		
France	0.000	Norway	0.000		

PRC = People's Republic of China.

Source: Authors' calculation.

Table D6: Reducing Taxes from 6% to 2% 3 January 2011

Country	Weight	Country	Weight	Country	Weight
Australia	0.952	Germany	0.048	Philippines	0.000
Austria	0.000	Greece	0.000	Poland	0.000
Belgium	0.000	Hong Kong, China	0.000	Portugal	0.000
Canada	0.000	India	0.000	Russian Federation	0.000
Chile	0.000	Israel	0.000	Singapore	0.000
PRC	0.000	Italy	0.000	South Africa	0.000
Colombia	0.000	Japan	0.000	Spain	0.000
Czech Republic	0.000	Malaysia	0.000	Sweden	0.000
Denmark	0.000	Mexico	0.000	Switzerland	0.000
Egypt	0.000	Netherlands	0.000	United Kingdom	0.000
Finland	0.000	New Zealand	0.000	United States	0.000
France	0.000	Norway	0.000	Viet Nam	0.000

PRC = People's Republic of China.

Source: Authors' calculation.