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## Capital Controls in Brazil – Stemming a Tide with a Signal?

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**Abstract:** Controls on capital inflows have been experiencing a period akin to a renaissance since the beginning of the global financial crisis in 2008, with several prominent countries choosing to impose controls; e.g., Thailand, Korea, Peru, Indonesia, and Brazil. We focus on the case of Brazil, a country that instituted five changes in its capital account regime in 2008-2011, and ask what the impacts of these policy changes were. Using the Abadie et al. (2010) synthetic control methodology, we construct counterfactuals (i.e., Brazil with no capital account policy change) for each policy change event. 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 are relaxed as was done in the immediate aftermath of the Lehman bankruptcy in 2008 and in January 2011 by the newly inaugurated government of Dilma Rousseff. We hypothesize that price-based capital controls' only perceptible effect are to be found in the content of the signal they broadcast regarding the government's larger intentions and sensibilities. Brazil's left-of-center government was widely perceived as ambivalent to markets. An imposition of controls was not perceived as 'news' and thus had no impact. A willingness to remove controls was perceived, however, as a noteworthy indication that the government was not as hostile to the international financial markets as many expected it to be.

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“What was just a trickle of controls before the current crisis is now a flood.”  
(Grabel and Chang, Financial Times, 25/10/2010)

## 1. Introduction

Capital controls are a varied lot. While there are many types of controls, one of the most basic distinctions is between controls on outflows and inflows. The economics literature consistently finds controls on outflows as inefficient and harmful (at least in theory – there is not that much distinct empirical research on capital outflows and a careful attempt to connect them to fiscal requirements<sup>1</sup>). Controls on capital inflows, however, have been experiencing a period akin to a renaissance since the beginning of the global financial crisis in 2008. This renaissance has manifested itself both in empirical and theoretical research on the issue, and most importantly in the decision by many countries to impose controls; Thailand, Korea, Peru, Indonesia, and Brazil are some of the prominent examples.

The International Monetary Fund (IMF) has advocated the removal of all controls on outflows and inflows throughout the 1990s.<sup>2</sup> The Asian Crisis of 1997-8, however, initiated a 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 (euphemistically naming them ‘capital flow management policies’, and with several caveats and pre-conditions<sup>3</sup>).

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 all other policy

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<sup>1</sup> Binici et al. (2010) provide a recent empirical attempt to differentiate between the impact of controls on outflows and inflows.

<sup>2</sup> The IMF’s campaign to liberalize capital flows culminated in an attempt to insert this issue into its charter – see Joyce and Noy (2008) for details and empirical evidence.

<sup>3</sup> Figure 1 in Ostry et al. (2011) provides a parsimonious summary of these caveats and preconditions.

alternatives have been exhausted. Here, we are interested in asking the basic questions that should be asked, and probably are asked, at the IMF. Are capital controls on inflows, at the time of an inflow surge, effective? And if they are, what are their effects? We attempt to answer these questions using the Brazilian experience of 2008-2010 in imposing new (price-based) controls as the Global Financial Crisis (GFC) unfolded. The IMF's position is that controls should only be imposed in these kinds of 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 GFC.

There are five possible impacts of capital controls on inflows: (1) reduce the volume of capital inflows; (2) change the composition of inflows (in accordance with the specific controls imposed); (3) impact the real exchange rate (preventing an appreciation); (4) enable a more independent pursuit of monetary policy (as it relaxes the international trilemma's constraints); and (5) increase/decrease financial stability.<sup>4</sup> We are unable to directly deal 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 (three months). We thus focus on the volume of capital inflows, on the exchange rate, and on domestic monetary policy (the interest rate).

There are two recent survey papers on capital controls (Magud, et al., 2011; and Ostry et al., 2010). Both conclude that the literature on the impacts of capital controls is inconclusive, with some observed effects on the composition of flows, but very little effect

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<sup>4</sup> 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 et al., 2006).

on volumes of flows (and even less agreement on the impact of controls on the exchange rate and policy/interest rates).<sup>5</sup>

As Magud et al. (2011) bluntly point out, however, 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 mostly on the two poster-children of capital controls, Malaysia (outflows) and Chile (inflows).<sup>6</sup>

Another distinction that appears important is the distinction between 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 long-term effects. The IMF, in its support for re-considering 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 here 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.

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<sup>5</sup> We do not provide a significant review of this large literature since these two recent surveys are available. An earlier survey of this literature is Edwards (1999).

<sup>6</sup> Malaysia famously imposed temporary controls on capital outflows in the aftermath of the Asian Crisis of 1997-8, 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.

We focus on a set of controls imposed (and relaxed) by Brazil in the last few years, 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 U.S. and European mutual funds investing internationally, and a new methodology to estimate the counter-factual (no imposition of controls). We use a methodological innovation recently formalized in Abadie, Diamond and Hainmueller (2010, henceforth ADH); a paper that investigated tobacco taxes in California. The methodology 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 (other U.S. states in the ADH case, other countries, in ours). 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 all non-treated country observations (with weights estimated from pre-treatment data). The ADH procedure allows us to construct a no-policy-change counterfactual and thus measure in detail the impact of the controls themselves. It further does not require us to make many structural assumptions that would have been difficult to theoretically justify.

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 aftermath of imposition of controls. Our data does 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 large. In addition to examining the impact on equity



flows, we also look for any impact of the controls on exchange rate. We use the same synthetic control methodology (Abadie et al., 2010) in order to develop an alternative counter-factual 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 time 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 rather briefly describe them at the end of the next section. We do not examine financial stability since our focus is the short-term (three months) rather than the long-term that is at the core of the financial stability argument.

## 2. Capital controls and flows in Brazil – The data details

### *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 percent on fixed-income investments.<sup>7</sup> Investments related to equities remained exempt

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<sup>7</sup> This tax, known as the IOF (*imposto sobre operações financeiras*), has been used during the 1990s as well.

from taxes for a while later.<sup>8</sup> 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 percent tax on fixed-income and equity inflows was reintroduced in October 2009 with further widening its application the next month. The tax was later increased to 6 percent in two stages (in October 2010); but then reduced back down to 2 percent in January 2011.<sup>9</sup> Our dating of these capital account policy changes relies on OECD (2011).

### *The Flows*

The weekly mutual fund flows data we use are from Emerging Portfolio Fund Research (EPFR). 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). Flows that target a broader regional market including this country, e.g. Latin America, are excluded from our calculations and are thus not present in much of the analysis. As robustness check, we also calculate the broad regional flows to Latin America, which are the sum of all flows channeled to this region, and evaluate its response to Brazilian capital control. In a similar manner, we calculate the Total Net Asset 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,

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<sup>8</sup> In May 2008, the tax was extended to cover "simultaneous operations" to prevent circumvention of the inflow tax (circumvention which was apparently widespread).

<sup>9</sup> Tax was also expanded to cover margin calls on derivative positions and foreign borrowing with maturities below one year.

we calculate the weekly stock market return on the major stock market index measured in local currency. Weekly bond market return is calculated similarly. Bond index are from JP Morgan GBI and EMBI and are measured in local currency. Weekly Return on Foreign Exchange rate is calculated as the weekly return of the local currency against USD.

For every episode of capital control, we study 12 weeks (approximately 1 quarter) before and after the control date. We include a country as a possible component of the control group if 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 32 to 37 countries depending on the episode.

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 et al. (2010) use, and only slightly less than the number used in the first paper to use this methodology (Abadie and Gardeazabal, 2003).

### 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 – three variables are potential policy aims, and which 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.<sup>10</sup> The ADH methodology requires the assumption that the event has no effect on the outcome variable before the date of impact  $T_0$  ( $Y_{it}^I = Y_{it}^N \forall t < T_0$ ). The observed outcome is defined by  $Y_{it} = Y_{it}^N + \alpha_{it} D_{it}$  where  $\alpha_{it}$  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  $\alpha_{it}$  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  $Y_{it}^N$  but only  $Y_{it}^I$ .

Although there is no way of accurately predicting the country-specific determinants of  $Y_{it}$ , the structure of the emerging market economies is fairly similar and the external shocks affecting them were fairly similar as well (except for mean zero iid shocks  $\varepsilon_{it}$ ). In this case,  $Y_{1t}$  can be calculated as the weighted average of the  $Y_{it}$  (for  $i = 2, \dots, J$ ) observations from the other countries; i.e.,

$$Y_{1t}^N = \delta + \sum_{j=2}^J \omega_j Y_{jt}^N + \alpha_{1t} D_{1t} + \varepsilon_{1t} \quad (1)$$

For pre-impact observations ( $t < T_0$ ) this equation can be estimated to obtain the weights allocated to the different country observations,  $\omega_j$ .<sup>11</sup> The following estimation equation is used for each variable of interest (capital inflows, exchange rate, interest rate), based only the pre-impact observations, to obtain estimates for  $\delta$  and  $\omega_j$ :

<sup>10</sup> This description is a modified version of Abadie *et al.* (2010). To simplify comparison, we follow their notation where  $I$  denotes intervention (capital account policy changed) and  $N$  denotes non-intervention (policy not changed).

<sup>11</sup> The Abadie *et al.* (2010) specifications include an additional vector of variables that determine the variable of interest but are unaffected by the treatment. We estimate the model with additional variables that may affect capital inflows; more details are available in the previous data section and in the data appendix.

$$Y_{it}^N = \delta + \sum_{j=2}^J \omega_j Y_{jt}^N + \varepsilon_{it} \quad (2)$$

Abadie et al. (2010) show that under acceptable assumptions, one can estimate  $\alpha_{it}$  for  $t \geq T_0$  by calculating

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

where the second term on the right hand side of the equation is calculated using the weights ( $\hat{\omega}_j$ ) estimated in equation (1) and the post-treatment observations for the control group (the other countries' observations).

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.<sup>12</sup>

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 et al. (2010), we can use permutation tests to examine the statistical significance of our results: We separately assume that every other country in our sample implements a similar (and imaginary) capital control in the same year. We then produce counterfactual synthetic control for each "placebo control." These synthetic

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<sup>12</sup> Results for the weights we obtain are available in appendix B.

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:

$$\hat{\alpha}_{it}^P = Y_{it}^I - \hat{Y}_{it}^N = Y_{it}^I - [\hat{\delta} + \sum_{j=2}^J \hat{\omega}_j Y_{jt}^N] \quad \text{for } t \geq T_0 \text{ and } j \neq P \quad (4)$$

Essentially, we investigate whether the  $\hat{\alpha}_{1t}$  we estimated for Brazil are statistically different from the placebo  $\hat{\alpha}_{it}^P$  for  $i > 1$ . This procedure, of course, should only be applied in the cases where our initial estimates of the capital controls' effects yielded any statistically and/or economically significant observed impacts. This is not the case in some of our estimations, and we therefore include only the placebo results for episodes in which we find any apparent impact of the change in the capital control regime.

#### 4. Results

##### *Results for Capital Inflows and exchange rate*

We estimate the weights for equation (2) and then 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 therefore show the counter-factual 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 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 that funds start flowing in again (net) about two 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 that resulted from the controls. While we observe a continuation of the inflow for the counter-factual

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.<sup>13</sup> A similarly very brief deviation from the counter-factual can be also observed for the exchange rate (figure 1B). Within 3-4 weeks, we can longer identify any residual impact of the imposition of controls on the exchange rate.

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 counter-factual Brazil (without the relaxation of controls) would have experienced a continuing capital flight. LatAm funds, also seem to continue declining during this period, though at a slowing rate, which suggests that the removal of the IOF did indeed have the intended effect. In figure 2B we present the placebo test for this episode; the evidence only suggests an impact that is (weakly) statistically observable as non-coincidental (i.e., the gap between the Brazilian flows and the counter-factual is bigger than for the majority of the placebos). The evidence regarding the exchange rate is not as robust (figure 2C), but there still does appear to be a longer-term impact on the exchange rate than the one we observed in the first episode. Even that, however, appear to be a transitory phenomenon.

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 LatAm (if anything, the inflows for Brazil are rising faster than for

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<sup>13</sup> These results for the LatAm funds are not presented in the figures but are available upon request.

other Latin American funds).<sup>14</sup> We find no evidence that the imposition of controls had any impact of the exchange rate (figure 3B).

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 LatAm countries; but with a bigger impact for Brazil). In both acts Three and Four of 2009 and 2010, the post-control inflow boom episode seem to be large and unique (since the actual is significantly larger than the synthetic and unique to Brazil relative to LatAm 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 two 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 non-significant.<sup>15</sup>

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 LatAm in the first three months of 2011. This conclusion is borne out when examining the placebos graphed in figure 5B; again, we surmise that a relaxation of controls did appear to have a very-short-term, but both statistically and economically meaningful impact on capital flows. In the longer-term (three months in our framework) there does not seem to be any significant impact. The same findings, a brief deviation from the counter-factual and a reversion back

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<sup>14</sup> 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).

<sup>15</sup> Results are available upon request from the corresponding author.



to the pre-change equilibrium can also be found in the estimations of the exchange rate (figure 5C).

### *Summary of Empirical Findings – Capital Controls as a Signal*

To summarize, after controlling for the counter-factual (Brazil with no capital account policy change) for each event in which Brazil modified its capital controls during the first three years of the Global Financial Crisis, we find no evidence that any tightening of controls were 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 Brazilian government 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 is 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.

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.<sup>16</sup> Our hypothesis is that price-based, mild capital controls' only perceptible effect 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 apparently 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 of course, suggests that the same policy may have a different impact within a political environment in which the government is perceived differently.

### *Brazil and the BRICS*

The weights we obtained in constructing our synthetic controls (see appendix) have no real economic meaning, but do describe the conditional correlation between flows to Brazil and flows to the other countries in our (comprehensive) sample. Interestingly, the other BRIC countries (Russia, India and China) seem to figure quite prominently as controls (with the occasional addition of Mexico, Australia, Indonesia, Taiwan). This is interesting in and of

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<sup>16</sup> Luiz Inácio Lula da Silva. Replaced on 1/1/2011 by Dilma Rousseff from the same left-of-center political party.

itself, since Russia and Brazil are large commodity exporters, and India and China's sectoral composition of trade is quite different). Investment managers of the funds included in the dataset, apparently, seem to treat the 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/relaxation 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, Russia, India and China. In most cases, we do not observe any statistically visible deviation between the synthetic and the actual flows – so that Brazil's policy changes had no apparent impact. In a few instances, however, there do seem to be notable deviations; in particular we observe that for China in the first episode (an inflow surge), Russia in the second episode (outflow), China again in the third episode (again an inflow surge), and India and China in the fifth episode (outflow). But these deviations fit with our notion that Brazil is attempting to 'lean-against-the-wind' while the other BRICs 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 BRICs were experiencing very similar capital inflow surges or capital flights/retrenchments.

## 5. 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 Brazilian government, as well as other representatives from emerging markets, found the IMF's limited support of capital controls as a prudential policy tool as 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 (and less controversial among economists) monetary and fiscal adjustments (Chowdhury and Keller, 2012). If one uses the broad framework that the IMF suggests, however, on most accounts Brazil in 2008-10 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. 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 policy makers fully understand the inability of these controls to make any substantial impact, but nevertheless resort to adopting them. We leave that possibility for future work.



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Figure 1A: 2008-3-12 Taxing fixed income investment – CAPITAL FLOWS

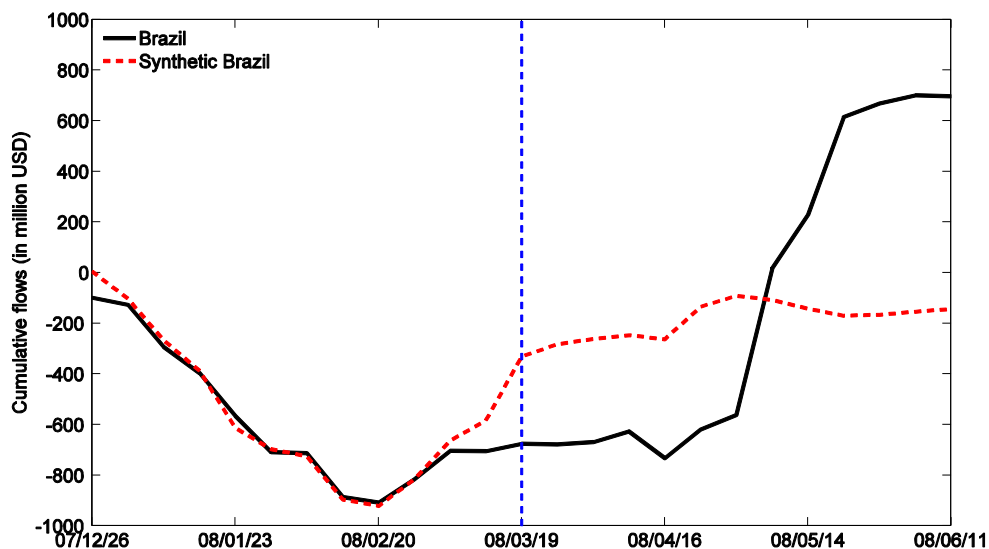


Figure 1B: 2008-3-12 Taxing fixed income investment – EXCHANGE RATE

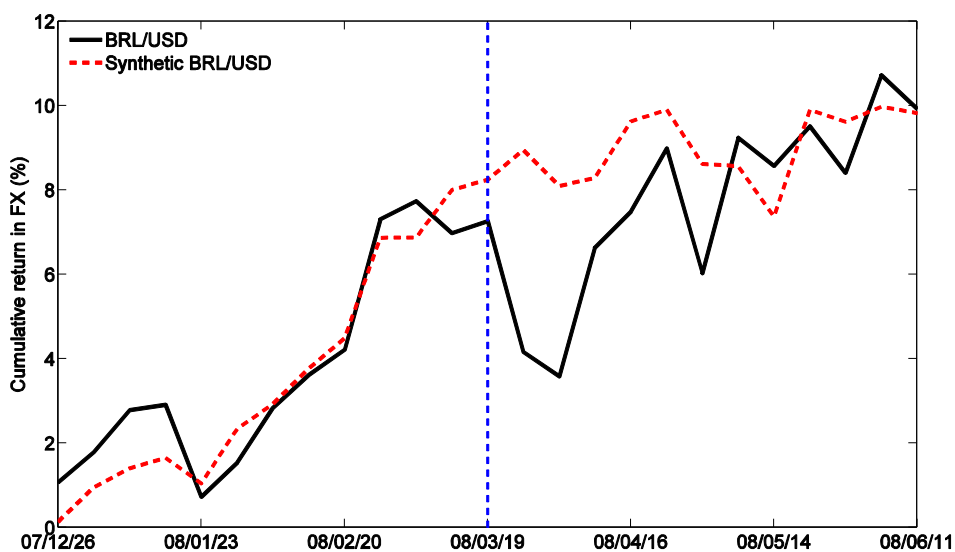




Figure 2A: 2008-10-23 Cutting fixed income tax – CAPITAL FLOWS

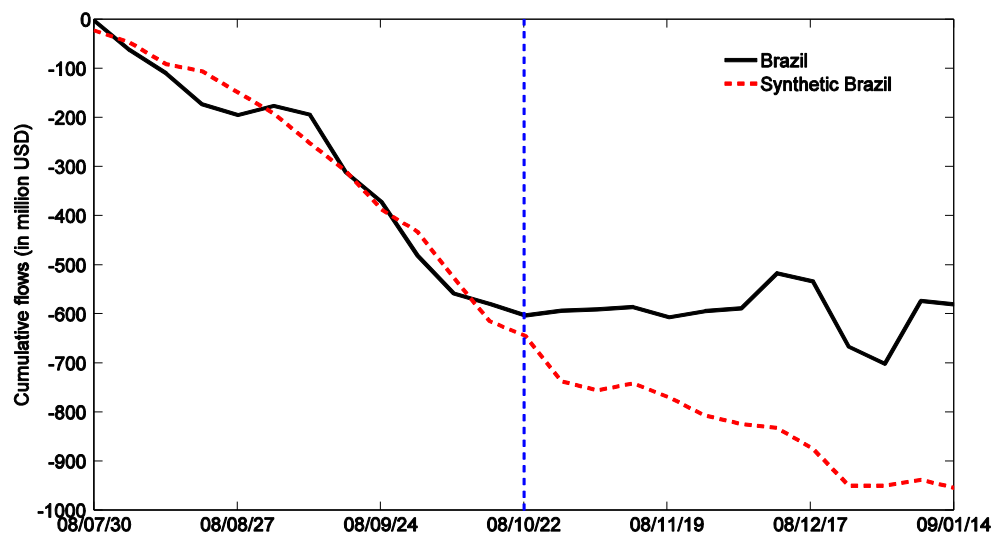


Figure 2B: 2008-10-23 Cutting fixed income tax – Placebos

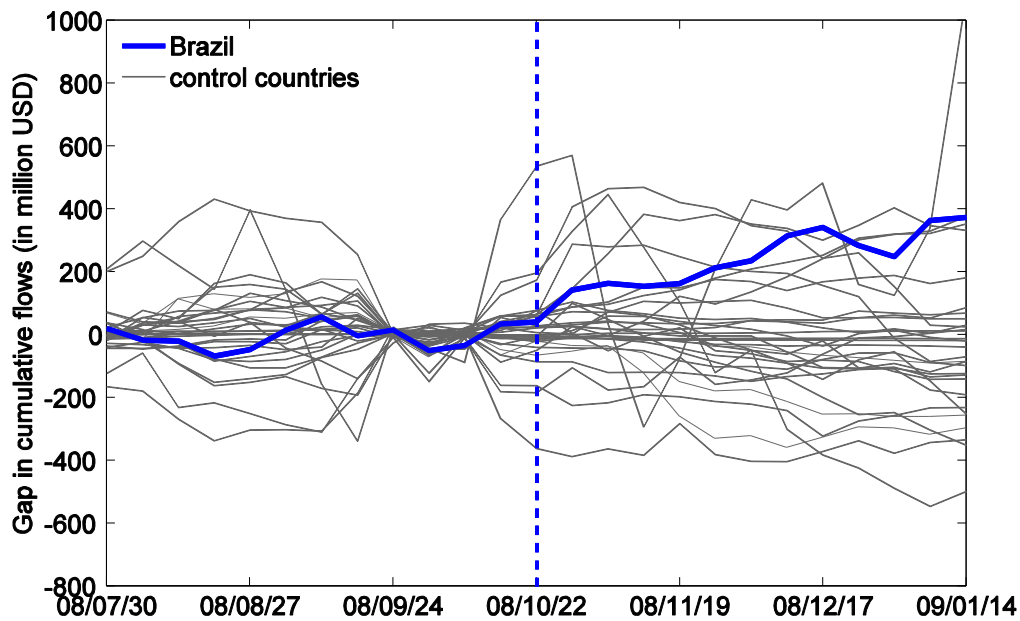


Figure 2c: 2008-10-23 Cutting fixed income tax – EXCHANGE RATE

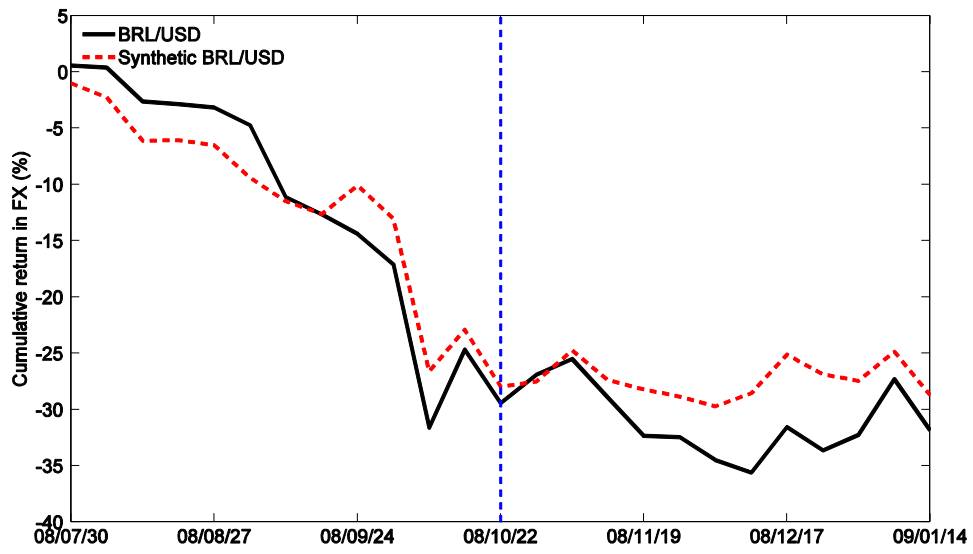


Figure 3A: 2009-10-20 Taxing stock and bond investment at 2% - CAPITAL FLOWS

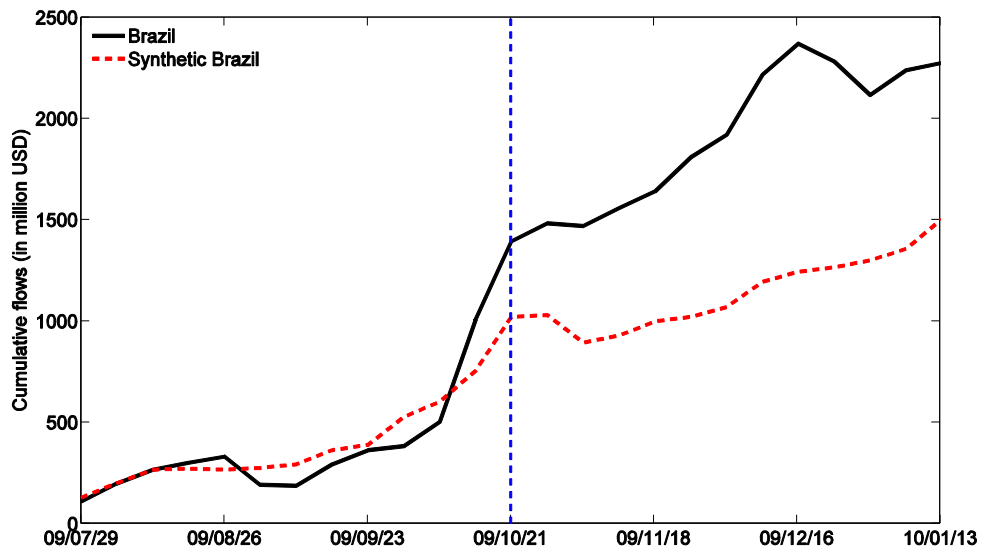
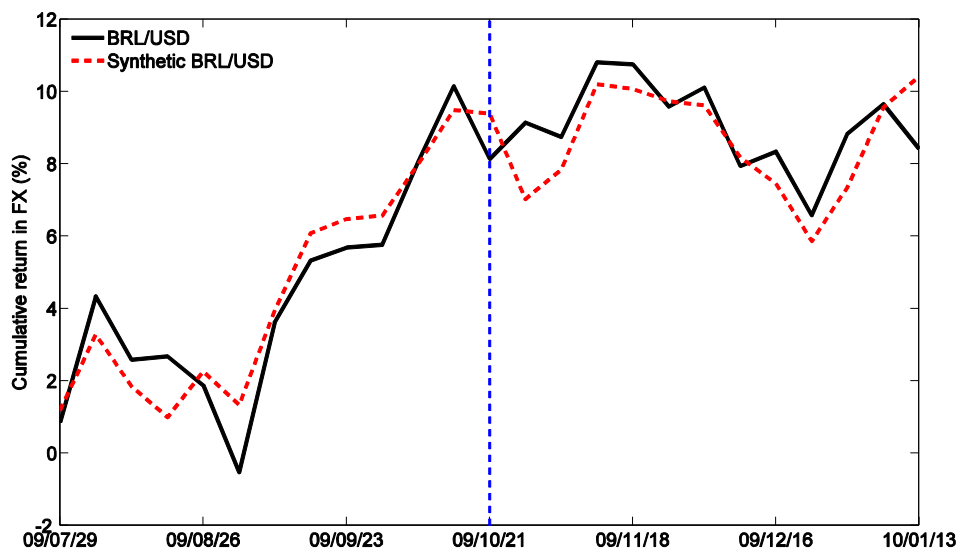
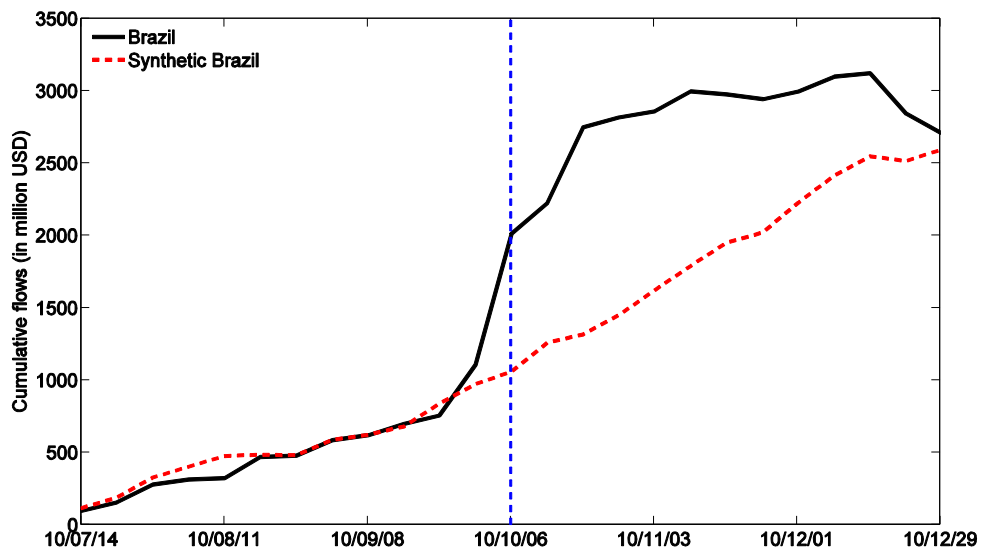


Figure 3B: 2009-10-20 Taxing stock and bond investment at 2% - EXCHANGE RATE



**Figure 4A: 2010-10-4 Increasing taxes 2 to 4%**



**Figure 4B: 2010-10-18 Increasing taxes 4 to 6%  
(and increasing taxes on margins from 0.38 to 6%)**

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

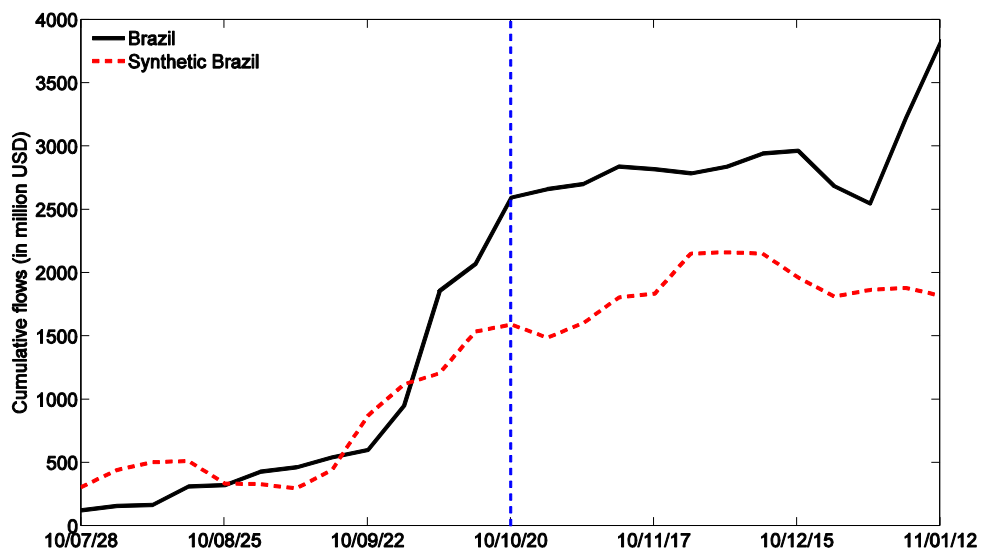


Figure 5A: 2011-1-3 Reducing taxes from 6% to 2% - CAPITAL FLOWS

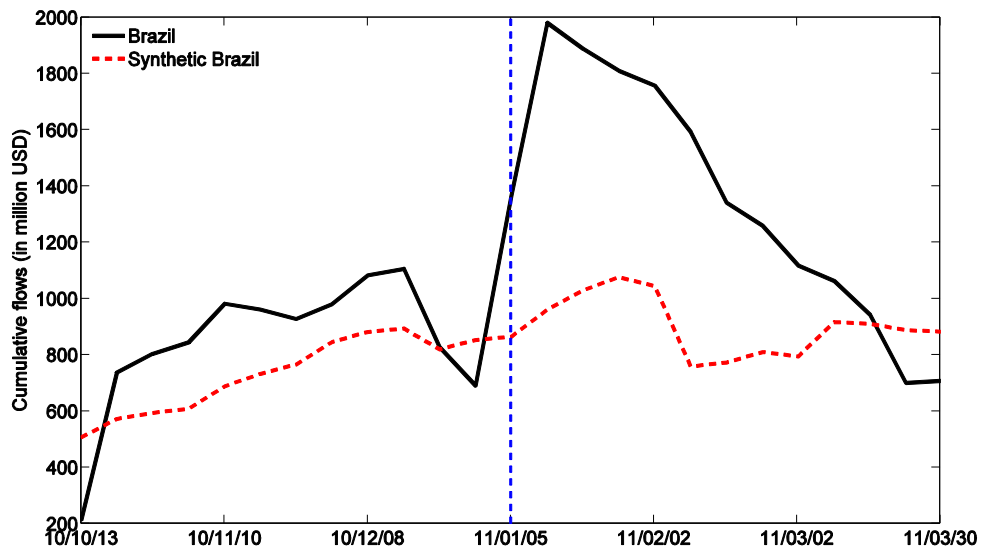


Figure 5B: 2011-1-3 Reducing taxes from 6% to 2% - Placebos

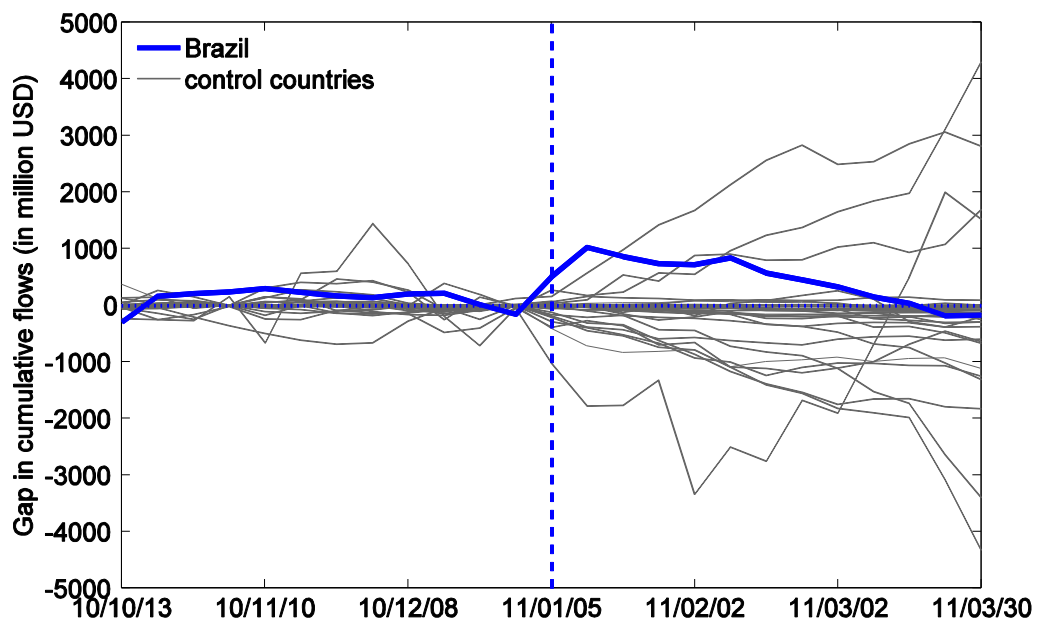
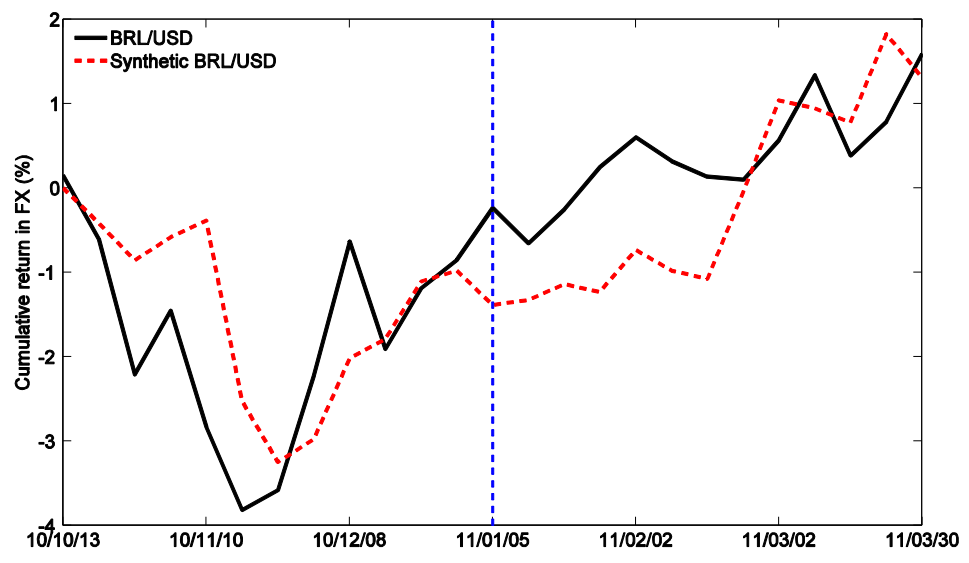


Figure 5c: 2011-1-3 Reducing taxes from 6% to 2% - EXCHANGE RATE



**Figure 1A Estimation results**

	Treated	Synthetic
RMSPE	53.084	
cumulative flow on the week end at 2008-2-13	-712.080	-724.852
cumulative flow on the week end at 2008-3-12	-816.750	-817.039
Mutual Fund return	6.086	1.972
log(TNA)	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

**Figure 2A Estimation results**

	Treated	Synthetic
RMSPE	36.664	
cumulative flow on the week end at 2008-9-24	-370.620	-386.686
cumulative flow on the week end at 2008-10-8	-557.050	-524.667
Mutual Fund return	-54.740	-42.002
log(TNA)	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

**Figure 3A Estimation results**

	Treated	Synthetic
RMSPE	101.242	
cumulative flow on the week end at 2009-8-5	197.340	199.387
cumulative flow on the week end at 2009-8-12	265.390	266.513
Mutual Fund return	26.661	25.029
log(TNA)	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

**Figure 4A Estimation results**

	Treated	Synthetic
RMSPE	70.546	
cumulative flow on the week end at 2010-8-25	480.330	480.233
cumulative flow on the week end at 2010-9-1	587.620	587.774
Mutual Fund return	10.150	10.112
log(TNA)	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

**Figure 4B Estimation results**

	Treated	Synthetic
RMSPE	307.223	
cumulative flow on the week end at 2010-9-25	606.060	874.442
cumulative flow on the week end at 2010-10-13	2,074.290	1,536.694
Mutual Fund return	14.347	11.746
log(TNA)	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

**Figure 5 Estimation results**

	Treated	Synthetic
RMSPE	206.968	
cumulative flow on the week end at 2010-11-3	844.590	607.302
cumulative flow on the week end at 2010-11-29	688.920	851.904
Mutual Fund return	-0.064	3.382
log(TNA)	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



## Appendix on Synthetic Weight for Capital Flows to Brazil

### 2008-3-12: Taxing fixed income investment

Country	Weight
Australia	0.119
Austria	0.000
Belgium	0.000
Canada	0.000
Chile	0.051
China	0.000
Czech Republic	0.000
Egypt	0.000
France	0.000
Hong Kong	0.000
India	0.000
Indonesia	0.000
Israel	0.000
Italy	0.000
Japan	0.000
Malaysia	0.000
Mexico	0.000
Netherlands	0.000
Norway	0.000
Philippines	0.000
Russia	0.000
Singapore	0.000
South Africa	0.000
South Korea	0.000
Spain	0.000
Sweden	0.000
Switzerland	0.000
Taiwan	0.496
Thailand	0.000
Turkey	0.000
United Kingdom	0.326
United States	0.009
Vietnam	0.000

**2008-10-23:** Cutting fixed income tax.

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

**2009-10-20: Taxing stock and bond investment at 2%**

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

**2010-10-4: Increasing taxes 2 to 4%**

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

**2010-10-18:** Increasing taxes 4 to 6% and increasing taxes on margins from 0.38 to 6%

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

**2011-1-3: Reducing taxes from 6 to 2 %**

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

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