Capital Controls and Prudential Measures: What Are They Good For?

Kristin Forbes, MIT-Sloan School of Management and NBER

Marcel Fratzscher, DIW, Humboldt University and CEPR

Roland Straub, European Central Bank

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Abstract: Assessing the effectiveness of capital controls and prudential measures is complicated by selection bias and endogeneity; countries which change their capital flow management (CFM) policies often share certain characteristics and are responding to changes in variables (such as capital flows and exchange rates) that are intended to be influenced by these policies. This paper attempts to adjust for these challenges by estimating propensity scores and using different algorithms to match "control" groups with "treatment" countries that adjust their CFMs. We also create a new database with detailed information on weekly changes in controls on capital inflows and capital outflows and prudential measures for 60 countries from 2009-2011. The results indicate that certain types of CFMs can significantly reduce financial fragilities (such as bank leverage, credit growth, and exposure to short-term debt). Most CFMs do not significantly affect other key targets, however, such as exchange rates, capital flows, interest rate differentials, inflation, equity indices, and different measures of volatility. The main exception is that removing controls on capital outflows can reduce real exchange rate appreciation. Therefore, certain CFMs can be effective in accomplishing specific goals—especially for reducing financial vulnerabilities—but many popular measures appear to be less effective in accomplishing their stated aims.

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*Author contact information: Kristin Forbes at kjforbes@mit.edu; Marcel Fratzscher at MFratzscher@diw.de; and Roland Straub at roland.straub@ecb.int. Thanks to Bogdan Bogdanovic and Daniel Happ for excellent research assistance. Further thanks to Menzie Chinn and Hiro Ito for providing updated measures of their index measuring capital account openness. The views expressed in this paper are those of the authors and do not necessarily reflect those of the ECB or of the Eurosystem.

I. Introduction

Over the last few years, economists and policymakers have become more supportive of "capital-flow management" measures (CFMs) to address the negative effects of large and volatile capital flows. This support has been bolstered by a series of IMF papers developing scenarios in which different CFMs should be "part of a policy toolkit", as well as a series of theoretical papers modeling how CFMs can increase social welfare.[1] A number of countries have followed these recommendations and about 40 countries have adjusted their CFMs at least once from just 2009 through 2011. The stated purpose of these recent changes in CFMs includes: limiting exchange rate appreciation, reducing portfolio inflows, providing greater monetary policy independence, reducing inflation, reducing volatility in a number of variables, and/or reducing other financial fragilities (such as to bank leverage, credit growth, asset bubbles, foreign currency exposure, or short-term liabilities). If CFMs can help accomplish these goals, they could play an important role in helping stabilize economies, especially as capital flows will continue to be volatile and affected by the active use of monetary policy in major developed economies.

Empirical evidence on whether CFMs can accomplish these goals, however, is extremely limited and generally non-conclusive.[2] Two major challenges in past studies measuring the effectiveness of capital controls and prudential measures are selection bias and endogeneity. Countries which adjust their CFMs tend to share certain characteristics and be subject to different challenges than other countries. Moreover, governments tend to adjust their capital flow policies in response to changes in variables such as capital inflows, exchange rates, asset prices, credit growth and other measures of financial stress—which are the variables that CFMs are intended to affect. In other words, it is impossible to measure the effects of capital controls and prudential measures on these key variables without knowing the counterfactual, i.e. what would have happened to these variables if the country had not introduced such measures. Although previous studies are usually aware of these challenges, there are no obvious instruments to appropriately identify estimates.[3]

[1] Key IMF papers building this support include: IMF (2011a, 2011b, 2012) and Ostry et al. (2010, 2011). Key theoretical papers include: Korinek (2010, 2011), Jeanne and Korinek (2010), Costinot, Lorenzoni, and Werning (2011), and Jeanne (2012).

[2] An extensive literature testing for the effect of capital controls has generally concluded that they can shift the composition of capital flows, but do not significantly affect the volume of capital inflows, the exchange rate, or the independence of monetary policy. See Forbes (2007), Cline (2010), and Magud, Reinhart, and Rogoff (2011) for recent surveys. More recent evidence suggests that controls and prudential measures may improve a country's liability structure and increase its resilience to crises, such as Chamon et al. (2012) and Ostry et al. (2010).

[3] One noteworthy exception is Jinjarak, Noy and Zheng (2013) which use a synthetic control method to assess the impact of recent changes in Brazil's tax on capital inflows. They construct counterfactual paths for Brazil's exchange rate and capital flows using exchange rates and capital flows of countries showing similar trends over the previous 3 months.

This paper attempts to address these challenges by using a propensity-score matching methodology. This framework matches "treatment" countries, which changed their CFMs, with different control groups. The control groups can then be used to estimate the counterfactual outcomes for key variables if CFMs had not been introduced.[4] By constructing this counterfactual, it is possible to calculate the difference in mean outcomes between this "control" group and the "treatment" countries which changed their CFMs. An important advantage of this propensity-score matching methodology is that it can solve the problem of non-random sample selection while avoiding strong assumptions about functional form.

In order to perform this analysis, we begin by constructing a new database with detailed information on changes in controls on capital inflows, controls on capital outflows, and prudential measures at a weekly frequency from 2009 through 2011. We construct this database using a large and diverse sample of 60 countries in order to be able to construct accurate control groups with which to assess the impact of various CFMs. We also divide our database into: capital controls affecting equities, bonds or FDI; prudential measures targeting banks or foreign exchange; and any type of CFM that was "major" in the sense it received substantial attention by investors. Then we estimate the probability that a country will change its capital controls or prudential measures each week based on a series of observable variables measuring country-specific characteristics and changes in the global environment.

The resulting probability (propensity score) for each member of the treatment group is then matched with a control group of observations, i.e. country-week observations with similar propensity scores. We use four different algorithms to perform this matching: nearest-neighbor without replacement, five-nearest neighbors, radius with caliper, and local-linear. A series of tests indicates that the matching algorithms perform well. Most treatment observations are "on-support" and the matching techniques remove significant differences between the treated countries and unmatched control groups.

Next, we use these matched samples to estimate the average-treatment effect on the treated (or ATT) of each of the changes in capital controls or prudential measures on a series of outcome variables. We focus on CFMs directed at reducing pressures related to capital inflows (the primary concern over 2009-2011): increased controls on capital inflows, decreased controls on capital outflows, and increased prudential regulations. We estimate the cumulative effects of these CFMs for each week over a 6-month window on outcome variables that are frequently cited as goals for adjusting capital controls and prudential measures:

[4] See Heinrich, Maffioli and Vazquez (2010) for an excellent review of propensity-score matching methods. See Glick, Guo, and Hutchison (2006) for an application of this strategy to estimate the effect of capital account liberalization on currency crises.

the exchange rate, portfolio flows, other macroeconomic variables (interest rate differentials, equity markets, and inflation), financial market volatility (for the exchange rate, portfolio flows, and interest rates) and other financial vulnerabilities (bank leverage, private credit growth, and exposure to short-term debt and portfolio liabilities).

The results indicate that CFMs can have significant effects on some of the variables which they are intended to influence. The strongest effects appear to be in reducing financial vulnerabilities. Increased prudential measures significantly reduce bank leverage and increased controls on capital inflows reduce the growth in private credit. Increased banking regulations and decreased controls on capital outflows both generate significant reductions in country exposure to short-term debt.

In contrast to these robust results showing that CFMs can reduce certain measures of country vulnerability, there is more limited evidence that they can affect two other primary goals: exchange rates and net capital flows. Removing controls on capital outflows is the one policy which generates a significant and robust depreciation of the real exchange rate—especially when focusing only on "major" changes in capital controls—although the maximum estimated effect is a depreciation of less than 2.5% relative to the counterfactual. Changes in capital controls and prudential measures do not significantly affect aggregate portfolio flows and bond flows, although there is some evidence that equity flows can be affected by changes in capital controls targeting either equity or bond flows.

The results also find little evidence that changes in CFMs affect other macroeconomic variables and financial market volatility. Increased inflow controls, decreased outflow controls, and increased prudential measures all have no significant effect on interest-rate differentials versus the United States, on equity indices, or on inflation (although changes in prudential measures can significantly reduce expectations of future inflation). Moreover, none of these different policies leads to a significant and robust reduction in the volatility of exchange rates, portfolio flows, or interest rate differentials.

This series of results has important implications. Much of the recent policy debate on the use of CFMs to reduce exchange rate appreciation has focused on the use of controls on capital inflows. The results in this paper, however, indicate that removing controls on capital outflows would be a more effective tool for limiting exchange rate appreciation. Also, the debate on the use of capital controls (and to a lesser extent prudential measures) has historically focused on affecting the exchange rate, capital flows, and other macroeconomic variables. Only recently has the debate shifted toward using these tools primarily to reduce other forms of financial vulnerability (i.e., Ostry et al., 2012). The evidence suggests that this new

focus is likely to more productive. CFMs have little consistently significant effect on exchange rates, capital flows, and other macroeconomic variables (except for some effects of removing controls on capital outflows on the exchange rate), but more consistently significant effects on other forms of vulnerability—such as reducing bank leverage, the growth in private credit, and exposure to short-term debt. Therefore, capital controls and especially prudential measures appear to be "good for" addressing concerns related to certain measures of financial fragility, but do not appear to be as effective in influencing other targets.

II. Capital Flow Management Events and Dataset

In order to include the largest set of capital-flow management (CFM) events as possible, as well as to have the option of drawing from a broad a set of countries for the control sample, we begin with a larger and more diverse set of countries than is typically used in analyses of capital controls. More specifically, we begin with all "Advanced Economies" (as defined by the International Monetary Fund as of October 2012) and all "Emerging Markets" and "Frontier Economies (as defined by Standard & Poor's BMI indices). We then exclude current members of the euro area, the three largest advanced economies (the United States, United Kingdom, and Japan) and countries that do not have data on equity or bond flows. This yields a sample of 60 countries for our analysis, with additional information on sample selection and the final list of countries listed in Appendix A.

Next, we use several sources to build a database of weekly changes in CFMs during 2009, 2010 and 2011 for this sample of 60 countries. Our primary source is the *Annual Report on Exchange Arrangements and Exchange Restrictions* by the International Monetary Fund. Then we supplement this source with information from financial analyst reports, primary news sources, and academic papers on capital controls and prudential measures. We include any changes in capital controls or prudential measures related to foreign exchange or international transactions, or that differentiate between residents and non-residents in any way. We classify the selected measures into three major groups – controls on capital inflows, controls on capital outflows, and prudential measures. These classifications require some discretionary judgment, and additional information on the full construction of this dataset is included in Appendix A.

More specifically, we define capital controls as measures that limit the rights of either residents or non-residents to enter into international capital transactions, or to affect the transfers and payments associated with these transactions. Typical measures could include taxes on cross-border flows from residents/non-residents, unremunerated reserve requirements (URR) on such flows, or special licensing requirements

and even outright limits or bans. Capital controls may apply to all flows, or may differentiate by the type or duration of the flow (i.e., debt, equity, or direct investment; short-term vs. longer-term). Prudential measures are defined as regulations focused on strengthening the ability of the domestic financial sector to cope with risk related to foreign exchange or international exposure. These measures do not directly target capital flows, but instead the balance-sheet risk which could result from these flows. Therefore, they often focus on the currency of the transaction or exposure, instead of the residency of the parties to the transaction. These techniques are typically implemented at the level of individual institutions, most often financial institutions, even when they serve macro-prudential aims. They commonly consist of limits on banks' open FX position, limits on banks' investments in FX assets, and differential reserve requirements on liabilities in local currency and FX.

After constructing this database of changes in controls on capital inflows, controls on capital outflows, and prudential measures, we then further differentiate these CFMs by several criteria. We label each change in the CFM as an increase or decrease—with an increase meaning a new or stricter regulation and a decrease implying the removal or reduction in a regulation. We also classify each measure as affecting: equities, bonds/fixed income, foreign direct investment (FDI), loans, banks, and/or foreign exchange. In some cases, a specific CFM may simultaneously affect more than one of these categories.[5] CFMs affecting equities, bonds, and FDI are more often capital controls, and CFMs affecting banks, foreign exchange, and loans are more often prudential measures—but this division does not always hold. Finally, we also construct a variable classifying a CFM as "major" or not, based on whether it received attention by financial analysts and investors. (This variable is discussed in more detail in Section IV. B.)

This procedure yields a database of 220 CFM events in which there is a change in controls on capital inflows or outflows or prudential measures. Out of the 60 countries in the sample, 39 countries have at least one event. Table 1 lists the number of inflow controls, outflow controls and prudential measures that were increased/added or decreased/removed by each country. It shows that a range of emerging markets and developing countries from around the world implemented changes in controls or prudential measures over this period, with only a few changes in advanced economies (such as the Czech Republic, Israel, South Korea, and Taiwan). The sample has more changes in prudential measures (121 events) than capital controls (99 events)—with the changes in capital control evenly balanced between changes in controls on inflows (50 events) and outflows (49 events). The CFMs are also more heavily weighted towards policies

[5] For example, a CFM affecting banks' reserve requirements in foreign currency would be classified as a CFM affecting banks and foreign exchange. Similarly, a CFM limiting companies' ability to convert foreign exchange for the purposes of FDI would be classified as a CFM affecting FDI and foreign exchange.

affecting bonds (67) and equities (47) than FDI (18), and more often focus on foreign exchange (130) and banks (107) than loans (46). Our review of analyst comments indicates that 44 of the measures were viewed as "major" by investors.

It is also possible to divide the CFMs into two groups—those responding to pressures related to large and volatile capital inflows and those related to outflows. More specifically, countries concerned about strong net capital inflows, currency appreciation, rapid credit growth, and related vulnerabilities could choose between increasing controls on capital inflows, reducing controls on capital outflows, and/or increasing prudential regulations. Countries concerned about sudden stops, currency depreciation, a contraction in credit, and/or related vulnerabilities could choose between decreasing controls on capital inflows, increasing controls on capital outflows, and/or decreasing prudential regulations. Figure 1 shows the incidence of changes in these groups of CFMs by quarter from 2009 through 2011. The figure and right-hand column of Table 1 shows that the sample is weighted more heavily towards CFMs aimed at moderating capital inflows (with 135 events compared to 85 with the opposite goal). This is not surprising as most of the time period from 2009-2011 was a period of declining risk aversion, expansionary monetary policy in developed countries, and increased global capital flows. In the analysis which follows, we focus on the impact of this larger group of CFMs aimed at moderating capital inflows and which have received the greatest attention by policymakers.[6]

This data on CFM events is then merged with information from several different sources to create the final database for analysis. Weekly market information on global risk (VIX), interest rates (on 3-month treasuries), and equity market indices is taken from Haver and Datastream. Weekly capital flow information on equity and bond flows and asset positions is taken from the Emerging Portfolio Fund Research database. Monthly macroeconomic information on real exchange rates, the money supply (M1), private credit, foreign exchange reserves, GDP, price inflation (CPI), and GDP per capita is taken from the IMF and supplemented with data from the ECB as needed. Monthly measures of institutional quality are taken from the ICRG database. Information on a country's exchange rate regime—both *de jure* and *de facto*—is taken from Ghosh et al. (2011) and information on past capital account openness is from Chinn and Ito (2008, updated as of 04/24/2013). Other information on country financial exposure and liabilities is from the World Bank's Global Financial Development Database. Additional information on this data, including sources and detailed definitions, is available in Appendix B.

[6] We have also extended the analysis to other measures related to concerns about capital outflows, currency depreciations, and related pressures (such as lifting controls on capital inflows and prudential measures and adding controls on capital outflows), but the more limited set of observations and global economic trends during the sample limit the analysis.

III. Matching Methodology and Estimating Propensity Scores

In order to estimate the impact of the CFMs identified in the database discussed above, we use a matching methodology to estimate the counterfactual of what would have happened in different countries if they had not used CFMs. This propensity-score matching methodology is typical in medical studies and labor economics, but is only recently starting to be more widely used in macroeconomic research.[7] In the first step, we use a vector of observable variables to estimate the probability of countries implementing a CFM at a particular point in time. The resulting probability (propensity score) for each member of the treatment group is then matched with a control group of observations, i.e. country-week observations that have similar propensity scores as the CFM event of the treatment group. In the next section, we estimate any different effects of CFMs on the outcome variables of interest for the treated group relative to the matched control group.

A. Calculating Propensity Scores

To begin, we define a "treated" observation as any week in which a country in the sample changes a CFM and a "control" observation as any week when a country does not change a CFM. We also create an "exclusion window" for 3 months before and 3 months after a change in the CFM of interest.[8] During this exclusion window a country cannot be used as a control observation—even if it makes no changes to the relevant CFM during those weeks. This exclusion window prevents labeling countries which have recently implemented or are about to implement a change in a CFM as a control observation. It also prevents matching treated observations for one country with control observations for the same country at slightly different points in time. Moreover, given the many factors which determine exactly when a change in a CFM occurs, we do not expect to be able to predict the exact week in which a change is made.

Next, we need to match each treated observation with a control group based on a set of observable country characteristics (X). Finding two countries that share identical macroeconomic characteristics for all variables in the vector X, however, is impossible. Rosenbaum and Rubin (1985) show that instead it is sufficient to match treated and control observations based on a "propensity score" $P(X)$ which is the probability that a country receives the treatment. The use of a single propensity score thereby reduces the number of dimensions by which to match observations. Rubin and Thomas (1992) further show that it is possible to estimate these propensity scores based on the vector of observable characteristics.

[7] Two prominent examples are: Glick, Guo, and Hutchison (2006) on the link between openness and currency crises and Ehrmann and Fratzscher (2006) for the effect of monetary policy shocks on firms.

[8] We focus on a 3-month exclusion window, as Forbes et al. (2012) find that changes in Brazil's capital controls from 2009 to 2011 can affect capital flows for more than a month, but no longer than 3 months.

In order to select the variables to include in the vector *X* to calculate the propensity scores predicting the probability of a country changing its CFMs, we draw from the literature on the determinants of capital flows and capital controls.[9] Since our database on capital controls and prudential measures is compiled at a weekly frequency, we focus on covariates available at this frequency whenever possible. First, to control for changes in a country's exchange rate or capital flows—which are primary motivations cited for CFMs—we control for percent changes in the country's real effective exchange rate and 6-month portfolio flows. Second, to control for increased risk of inflation and excessive credit creation—other reasons frequently cited as motivations for CFMs—we control for the percent change in domestic credit relative to GDP and consensus CPI inflation forecasts (over the next year).

Third, to control for changes in global sentiment or relative rates of return (including the effects of monetary policy in developed economies) that could affect global capital flows, we control for global risk (measured by the VIX) and changes in the interest-rate differential between each country and the United States (measured for 3-month Treasuries).[10] Fourth, to control for different intervention strategies, exchange rate regimes, and past use of CFMs which could affect the current use of CFMs, we include the percent change in foreign exchange reserves to GDP, a dummy equal to one if the country has a floating exchange rate, and a measure of the country's pre-existing capital account openness. Finally, to control for any effect of a country's income level, the size of its financial sector, and institutional capacity, we also control for GDP per capita, stock market capitalization (to GDP), and the country's "legal compliance".[11] For each variable measured in changes, we calculate the change (or percent change) in the variable relative to one year ago in order to minimize any seasonal effects. We also lag all variables so that any change in the CFM occurs after the variable is measured.[12] All variables are defined in detail in Appendix B.[13]

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[9] See Aizenman and Pasricha (2013) for empirical evidence on what determines a country's use of controls on capital outflows and Fratzscher (2012) for evidence related to a country's use of controls on capital inflows. See Lim et al. (2011) for the determinants of a country's use of macroprudential measures.

[10] Forbes and Warnock (2012) and Fratzscher (2012) provide empirical evidence on the importance of global risk in determining capital flows. Fratzscher, Lo Duca, and Straub (2012) provide evidence of the role of monetary policy in advanced economies.

[11] Lim et al. (2011) discuss the importance of the size of existing financial markets. Habermeier et al. (2011) discuss how institutional features of countries, such as administrative capacity and legal compliance, could also have an effect on the design and enforcement of capital management techniques.

[12] The six-month exclusion window around a change in a CFM (3 months before and after the treatment date) as well as the one-period lag of all explanatory variables should reduce the likelihood that the explanatory variables are influenced by the introduction or anticipation of the CFMs.

[13] We have also controlled for other variables—such as changes in equity indices, changes in expected CPI inflation, and changes in expected GDP growth. None of these variables is significant in any of the specifications and including them does not alter any of the main results. We have also used other measures for key variables—such as

Next, we use this set of covariates in a logit regression explaining changes in CFMs aimed at moderating capital inflows and related pressures each week from 2009 through 2011 for our sample of 60 countries. We estimate the effects for three types of CFMs: increased controls on capital inflows, reduced controls on capital outflows, and increased prudential regulations.[14] The estimates are reported in Table 2 and show that many of the variables expected to affect the probability that a country adopts some type of CFM to reduce capital inflows are significant and have the expected sign. Different variables, however, are important for predicting the use of different types of CFMs.

Focusing on variables that are significant at the 5% level, countries are significantly more likely to increase controls on capital inflows if they have recently experienced an appreciation of the real exchange rate and have a smaller financial market relative to GDP. Countries are significantly more likely to increase prudential measures if they have had higher growth in private credit and higher expected inflation, a floating exchange rate, more open capital account, stronger legal compliance, and if global risk (measured by the VIX) is lower. Fewer changes in recent financial variables have significant effects on a country's probability of removing controls on capital outflows, but countries are more likely to decrease these controls if they have a larger financial market, stronger legal compliance, and less open capital account (which may mean more controls exist that could be removed). It is worth highlighting the estimated relationships between the different types of CFMs and legal compliance. Countries with stronger legal compliance are more likely to use prudential measures and remove controls on capital outflows—with both of these effects nonlinear and decreasing at higher levels of compliance. In contrast, countries with stronger legal compliance may be less, instead of more, likely to add controls on inflows (although this effect is not significant).

We have also estimated each of these regressions predicting the use of different types of CFMs using a stepwise regression in which we use the estimates reported in Table 2, and then estimate the probability of adjusting each type of CFM with a more limited set of control variables that are only significant at the 20% level or less in the first stage. This reduces standard errors for the propensity scores and can cause several variables that are not significant in the full specification to become significant with the more limited set of control variables. More specifically, in regressions predicting increases in controls on

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the nominal exchange rate instead of real exchange rate, or overnight interest rates instead of 3-month rates. These changes also do not affect the key results and we focus on measures that maximize the sample size.

[14] All regressions include robust standard errors. We have also repeated these estimates using a cloglog specification to adjust for the fact that the distribution of the LHS variable is not normal. This has no significant effect on the results.

capital inflows, the coefficients on portfolio flows and CPI expectations become significant at the 5% level (instead of the 10% level) and in regressions predicting decreases in controls on capital outflows, the coefficient on GDP per capita becomes significant at the 5% level. These changes, however, do not significantly affect any of the key results on the matching methodologies and slightly reduce the accuracy of the matching discussed below. Therefore, for our base case, we utilize the larger set of explanatory variables that is consistent across equations in order to predict changes in the use of the different CFMs.

B. Matching the Treatment and Control Groups

We use the estimates in Table 2 to calculate propensity scores for each of the 60 countries in the sample for each week from 2009 through 2011. Then we use these propensity scores to create a control group for each treated observation (each change in a CFM) based on four matching algorithms: nearest-neighbor without replacement, five-nearest neighbors with replacement, radius with caliper, and local-linear matching.[15] In nearest-neighbor matching, an observation from the control group is chosen as a match for a treated observation based on which observation has the closest propensity score. This method "without replacement" requires that untreated observations are used only once, while this method "with replacement" allows untreated observation to be used more than once as a match. This method can be used with more than one "nearest-neighbor" as a control group—and we also estimate the model using five nearest neighbors. The radius method uses the same basic approach, except includes all "nearest neighbors" which fall within a certain radius based on the propensity scores (and we set the caliper at 0.005). The local-linear matching algorithm calculates a weighted average of all observations in the control group using a nonparametric estimator which uses a generalized weighting function to assign a higher weight to control observations closer to the treated observation.[16] The nearest-neighbor algorithm is basically an extreme form of local-linear matching, with all the weight given to the closest propensity score(s).

Each of these matching methodologies has advantages and disadvantages. Nearest-neighbor is straightforward, easy to implement, and minimizes "bad" matches with control observations that have little in common with the treated observation. It is also straightforward to check which country is "matched" as the nearest neighbor in a control group. Nearest neighbor matching, however, ignores useful information from other countries in the control group. Local-linear and radius matching use more

[15] We have also performed the analysis using a kernel matching methodology. Kernel matching is similar to local linear matching, but Fan (1992a, b) shows that local-linear matching has several important advantages, such as a faster rate of convergence near boundary points and greater robustness to different data design densities. In our estimates, kernel matching also does not reduce differences between the control and treatment groups as effectively as the other methodologies. For all of these reasons, we focus on the other four strategies.

[16] See Heckman, Ichimura, and Todd (1997, 1998) for a detailed description of the local-linear matching method.

information and therefore tend to have lower variances—but at the risk of including bad matches. In the following analysis, we use each of the four different matching approaches to perform all the tests, but will focus on results obtained with the local-linear algorithm (based on results explained below).

Results from using these four matching algorithms indicate that the specified model performs well in terms of being able to fit a control group for almost all of the CFM events. More specifically, all 21 treatment observations when countries increase their inflow controls and all 59 when they increase their prudential measures are "on-support" for three of the four matching algorithms.[17] The algorithm which is less accurate in terms of yielding more "off-support" observations is the radius methodology.[18] The different matching algorithms also perform well for 28 of the 29 treatment events involving a decrease in outflow controls, with South Africa's removal of controls in week 7 of 2010 the only event that is off-support for each of the matching algorithms.[19] To be conservative, in the analysis that follows we impose a common-support condition which drops all observations with a propensity score higher than the maximum or lower than the minimum propensity score of the controls in order to reduce the effect of any "bad" matches. The literature suggests that this condition can be important, especially for radius and local-linear matching, although given the accuracy of the matching for this sample, it appears to have little effect on the key estimates reported below.

In order to get an intuitive understanding of how countries are matched between the treatment and control groups, it is useful to consider the "nearest neighbor" for several major treatment events. For example, in two highly publicized examples, Brazil increased its tax on capital inflows on October 29, 2009 and Oct 4, 2010. The first treatment is matched with Indonesia in 2010 (week 5) and the second with Mexico in 2010 (week 31). In another well-known example, Peru increased its prudential measures three times in the first three months of 2010. These treatments are matched with Mexico, the Philippines, and Malaysia (all during 2010 or the first half of 2011). In a prominent example of reduced controls on capital outflows, Malaysia relaxed these controls in April of 2009 and this treatment is matched with India in 2009 (week 21). These examples suggest that the countries identified as "nearest neighbors" generally make intuitive

[17] We apply these matching algorithms with the Stata module PSMATCH2, developed by Leuven and Sianesi (2003). A treatment observation is "on-support" if it has a propensity score that is equal to or below the maximum propensity score of the controls and equal to or above the minimum propensity score of the controls. The number of treatment observations is lower than reported in Table 1 because data is not available to estimate propensity scores for all observations.

[18] The observations that are not on-support using the radius methodology are: Vietnam's increase in inflow controls in weeks 33 and 34 of 2011 and Ukraine's increase in prudential measures in week 16 of 2009.

[19] Using the radius methodology, South Africa's removal of controls on capital outflows in week 45 of 2010 and week 4 of 2011 is also off-support.

sense in terms of the control observation sharing similar country characteristics as the treated country and occurring around the same time as the treatment event.

Although the matches obtained using the nearest neighbor algorithm seem intuitive, it is necessary to test more formally whether the different matching algorithms are able to remove any significant differences between the treatment and control groups that exist in the unmatched samples. Table 3 reports results of these tests for increases in capital inflows. It shows the mean values for the treated group (μ_T) and control group (μ_C) for the unmatched samples for each of the variables in the vector *X* used to estimate propensity scores. The table also reports t-statistics for tests of the hypothesis that the mean of each variable in the treatment group is equal to the mean in the control group (H0: $\mu_T = \mu_C$). There are significant differences between the treatment group and the unmatched control group for six variables. Countries were more likely to increase their controls on capital inflows if they had significantly greater real exchange rate appreciation, higher expected inflation, lower levels of capital account openness, lower income per capita, less developed financial markets, and lower levels of legal compliance (including the level and squared terms). These significant differences across the treatment and unmatched control groups highlight that selection bias is important; countries which chose to increase their controls on capital inflows had significantly different characteristics than countries which did not adjust their controls.

The right side of Table 3, however, indicates that the matching algorithms are able to remove (or at least significantly reduce) this selection bias. The columns show mean values for each of the variables in *X* in the matched control groups using all four of the matching algorithms. It also reports the same t-statistics of tests for significant differences between the treatment and matched control groups for each of the variables in *X*. In each of these tests, there are no longer significant differences between the treatment and control groups. Each of the four matching algorithms has successfully removed the significant differences across groups as measured by the variables in the vector *X* that existed before matching.

Results of this analysis are the same for each of the other CFM changes studied in this paper—removing controls on capital outflows and increasing prudential measures —as well as increasing controls on capital inflows. In each case, there are significant differences in the means of several variables between the treatment and unmatched control groups, but after using each of the matching algorithms, there are no longer any significant differences between the variables in the treatment and matched control groups. For example, in the model predicting increases in prudential measures, there are significant differences between the treatment and unmatched control group for eight variables: private credit growth, expected inflation, the VIX, the floating exchange rate dummy, GDP per capita, financial market size, legal

compliance and legal compliance squared. After using each of the four matching methodologies, there are no longer any significant differences between the treated and matched control groups.

As a final test of the performance of the different matching algorithms, Table 4 reports several statistics for each of the three different changes in CFMs. It lists the mean propensity scores for the treated group, unmatched control group, and matched control group using each of the four algorithms. This comparison shows that the local-linear matching algorithm generates a control group whose mean is closest to that of the treatment group (or 2nd closest for increases in controls on inflows). The table also reports the mean absolute bias of the treatment group relative to the unmatched control group and control group using each of the matching algorithms (with standard deviations in parentheses). In each case, the matching reduces the mean absolute bias by a substantial amount. The table also reports the number of treatment observations that are successfully matched and "on-support" using each algorithm, highlighting the point discussed above that radius matching generates more "off-support" observations for each of the CFMs. Therefore, in order to simplify the discussion that follows and minimize the number of results reported for each test, we will focus on results obtained using the local-linear matching algorithm (although we will discuss any differences with other techniques). Local-linear matching combines the attributes of maximizing the number of countries on-support while also generating mean propensity scores for the matched control group closest to the treatment group.

IV. Impact of Capital Controls and Prudential Measures

In order to test for the impact of different types of CFMs, we compare outcome variables for the treated observations with their matched control groups. Since the matching algorithms developed in the last section appear to have removed significant differences between the treatment and control samples, it is now possible to use the control group to construct a counterfactual of what would have happened to each of the outcome variables if there had not been changes in the CFMs. We focus on outcome variables that are frequently cited as goals for adjusting capital controls and prudential measures: the exchange rate, portfolio flows, other macroeconomic variables (interest rate differentials, equity markets, and inflation), financial market volatility (in the exchange rate, portfolio flows, and interest rates) and other financial vulnerabilities (bank leverage, private credit growth, and exposure to short-term debt and portfolio liabilities).[20]

[20] In most cases, we estimate how changes in CFMs affect the growth rates of these outcome variables. In several cases (such as for the effect on interest-rate differentials), we estimate the effect on the change in the outcome variable. The text and figures indicate how each outcome variable is measured.

To test for any significant effect of CFMs on these variables, we calculate the *average-treatment effect on the treated* (ATT) for each CFM on each outcome variable. The ATT is calculated by comparing the average value of the outcome variable for treated observations with the average value for the respective matched control observations. For our base case using local-linear matching, the average is calculated using higher weights for control observations closer to the treated observation, based on the assigned weights resulting from the nonparametric estimation. For nearest-neighbor matching and radius matching, the average for the control group is calculated using equal weights for all members in the group. Because the propensity scores are estimated in each case, it is necessary to bootstrap the standard errors for the ATT in order to evaluate if there is a significant difference between the two groups.[21]

We test for effects on outcome variables at any week over the six-month window after the treatment in order to capture any immediate as well as lagged effects of CFMs. We do not focus on longer-term effects as the matching algorithms (which incorporate changes in the global environment) are less accurate over longer time periods. In order to estimate effects over this 6-month window, we calculate a cumulative ATT for each of the 26 weeks after the initial treatment. For example, to estimate the ATT on the nominal effective exchange rate, we calculate the average difference between the growth rates of the exchange rate for the treated and control groups, starting from the week of the change in the CFM. For the first treatment period, this would be the difference between the percent change from period 0 (the treatment date) to period 1 (1 week later). For the second post-treatment period, this would be the percent change from period 0 to period 2 (2 weeks). For the twentieth period, this would be the percent change from period 0 to period 20 (20 weeks). One benefit of this approach is that it allows us to capture any effects of changes in CFMs over different time periods rather than choosing, *a priori*, the time period on which to focus. One disadvantage of this approach is that it does not incorporate any adjustment for post-treatment covariates.

A. Results: Base Case

The most straightforward way to characterize the effects of CFMs over the different weekly windows over 6 months is to examine graphs of the ATT for each type of CFM and outcome variable. Figure 2 presents the first series of these graphs. It shows the ATT for the nominal exchange rate, real exchange rate, and net portfolio inflows for the three CFMs aimed at reducing inflow pressures (increases in capital inflow controls, decreases in capital outflow controls and increases in prudential measures). Each bar shows the magnitude of the estimated ATT for the accumulated time in weeks since the treatment (change

[21] See Lechner (2002) for the appropriate methodology. We use 100 repetitions for the bootstrap.

in CFM) occurred. The dark black shading in a bar indicates that the ATT for that week is significant at the 5% level, and the medium-blue shading in a bar indicates that the ATT is significant at the 10% level. The black line is the fitted line for the average treatment effect.

The top two graphs in Figure 2 show that increasing controls on capital inflows causes a depreciation of the real and nominal exchange rates relative to that for the control group. This effect is initially very small (causing less than a 0.5% depreciation over the first 4 weeks), but increases gradually to peak at a depreciation of about 1.2% (nominal) and 1.8% (real) after 23 weeks, before diminishing near the end of the 6 months. Even this maximum effect is small relative to normal volatility in exchange rates. Moreover, this estimated ATT on the nominal and real exchange rate is generally insignificant, except for in weeks 18 to 24 during which the effect on the real exchange rate is usually significant at the 10% level (and only at the 5% level in weeks 22 and 23). When other matching methods are used, however, even these limited significant effects often become insignificant.[22] These results suggest that increasing controls on capital inflows generally does not have significant or economically important effects on a country's exchange rate.

Removing controls on capital outflows (shown in the second row of Figure 2) is estimated to cause a larger deprecation of the nominal and real exchange rates—peaking at a depreciation of almost 2.5% for the real exchange rate after 6 months. This effect continues to be insignificant for the nominal exchange rate, but the effect on the real exchange rate is more often significant and more immediate—especially in the first 6 weeks after controls are reduced. This effect of removing controls on capital outflows on the real exchange rate is also significant for the first 3-6 weeks when other matching algorithms are used. There are also several other weeks at different time horizons when the effect on the real exchange rate is significant at the 5% or 10% level—with the exact weeks depending on the matching algorithm. In sharp contrast, increased prudential measures (shown on the last row in Figure 2) cause an immediate appreciation of the nominal and real exchange rates, followed by a depreciation after about 6 weeks. These effects of prudential regulations on the exchange rate, however, are insignificant. Therefore, out of the three CFMs studied in this paper, removing controls on capital outflows appears to be the most effective tool for reducing any appreciation of the real exchange rate.

[22] For example, when the nearest-neighbor with no replacement and five-nearest neighbor matching algorithms are used, there is no week in which the ATT is significant at the 5% level for the real exchange rate from increased controls on capital inflows. When the radius matching algorithm is used, the ATT is only significant at the 5% level for weeks 22 and 23.

The graphs in the right column of Figure 2 also show the effects of different CFMs on changes in net portfolio inflows.[23] Increased controls on capital inflows cause net portfolio inflows to decline over time, with the effect gradually increasing to peak at a 3% decline in net inflows (relative to the counterfactual) after 5 months. This effect is significant at the 5% level from weeks 13 to 21 based on the local-linear matching algorithm shown, but is not significant during any week when the other matching algorithms are used. Decreased controls on capital outflows and increased prudential measures also appear to generate declines in net capital inflows after about 10 weeks, but these effects are smaller in magnitude and insignificant in every week when any of the four matching algorithms is used. There is some evidence that removing controls on capital outflows or increasing prudential measures might initially increase net capital inflows—possibly reflecting more positive investor sentiment in response to these policy changes—although the estimated effect is small and insignificant. Therefore, of the three CFMs, increased controls on capital inflows appears to be most likely to reduce net capital inflows, but even this effect is not robust to different matching algorithms.

Next, Figure 3 graphs the ATT for several macroeconomic variables that are also mentioned as targets for CFMs, albeit usually of secondary importance relative to exchange rates and portfolio flows. The graphs show effects on the change in a country's interest rate differential versus the United States (using rates on 3-month Treasury bills) and inflation (measured by the CPI), as well as percent change in its equity index. The graphs show no consistently significant effects of any of the changes in CFMs on any of these outcome variables over the 6-month window. Changes in CFMs do not appear to increase interest rate differentials versus the United States. The only result that is significant at the 5% level for even one week is that lifting controls on capital outflows may reduce a country's interest rate differential—but this effect is not significant for any weeks for any of the other three matching algorithms. Increased inflow controls generate lower equity returns, while decreased outflow controls and increased prudential measures generate higher equity returns. Increased inflow controls and prudential measures lower inflation moderately over time, while decreased outflow controls increase inflation. All of these effects, however, are insignificant at the 5% level and usually at the 10% level. Therefore, there is little evidence that any of the CFMs can significantly affect interest rate differentials, equity returns, or inflation.

Figure 4 shifts to evaluating the effect of CFMs on the volatility of key variables. It shows the ATT for the change in volatility in the nominal exchange rate, net portfolio inflows, and interest rate differentials (all defined as above). Volatility is measured as the standard deviation over the previous 26 weeks. The

[23] Net portfolio inflows are calculated as cumulative flows over the last 13 weeks and expressed as a percent of lagged total portfolio assets. Results are similar if portfolio inflows are not expressed as a percent of portfolio assets.

graphs show that increased controls on inflows are more likely to reduce volatility, and decreased controls on outflows are more likely to increase volatility—although most of these effects appear to fade or even reverse outright by the end of the 6-month window. Prudential measures may decrease exchange rate volatility, but increase capital flow volatility and have mixed effects on the volatility of interest rate differentials. Once again, however, most of these estimated effects are insignificant or short-lived, and none of the significant estimates is robust to other matching algorithms.[24] The one result which is significant at the 5% level for more than 2 weeks is that increased prudential measures are estimated to increase the volatility in net portfolio flows after 3 months when local-linear matching is used. Other matching algorithms, however, yield no significant effects in any week. Therefore, there is little evidence that adjusting capital controls or increasing prudential measures can significantly reduce the volatility of key financial variables.

Finally, a goal of capital controls, and especially prudential measures, which has recently received more attention is to reduce different forms of financial vulnerability. Measuring financial fragilities at a high frequency for our diverse sample of countries, however, is not straightforward, so we focus on four available measures that capture different forms of potential vulnerability: bank leverage (measured as bank credit to deposits), the growth in private credit (relative to GDP), short-term external debt (relative to GDP) and portfolio liabilities (as a share of total liabilities).[25] Figure 5 shows that changes in CFMs can have significant and sustained effects on these measures of country vulnerability—especially when compared to the generally insignificant effects on other outcome variables. More specifically, increases in prudential measures reduce bank leverage and this effect is significant for every week from 2 months through the end of the 6-month window. Increases in capital controls reduce the growth in private credit and this effect is significant for every week from 3 months through the end of 5 months (and then remains significant at the 10% level through the end of the window). Decreases in controls on capital outflows reduce the share of short-term debt and this effect is significant for about five months. Moreover, each of these significant results is consistent with estimates obtained using other matching methods (although the exact weeks that effects are significant can fluctuate slightly across algorithms).

The estimates of the impact of CFMs on other measures of financial vulnerability also generally have the expected sign and can be significant for shorter windows, although each of the other effects is not

[24] For example, increased controls on capital inflows do not have a significant effect on the nominal exchange rate or interest rate differentials for any week when any of the other three matching algorithms is used. The same lack of significant effects applies to the estimated effects of increased prudential measures on the nominal exchange rate.
[25] All measured as percent changes.

consistently significant across matching algorithms.[26] Estimates of the effect of each of the changes in CFMs on a country's exposure to portfolio liabilities (not reported) are also negative. This negative effect is significant from 3 months through the end of the 6-month window for changes in prudential measures, but insignificant for the other CFMs.[27] Only one graph in Figure 5 suggests how a CFM may not reduce a financial vulnerability as expected. Adding controls on capital inflows increases a country's exposure to short-term debt. This result is in sharp contrast to the decrease in exposure that occurs after a country reduces outflow controls or increases prudential measures and the result is robust to different matching methods. Although we do not want to place too much emphasis on this result because it is insignificant, it may indicate that after a country increases controls on capital inflows, investors prefer to switch to shorter-term lending to the country due to increased policy uncertainty.

B. Results: More Narrowly Defined CFMs

The last section provided evidence that although capital controls and prudential measures can significantly reduce certain financial vulnerabilities, they do not appear to accomplish most other key goals. The measures of capital controls and prudential measures used in the analysis so far, however, are defined broadly to encompass very different types of policies that may have different goals. For example, the capital control treatments include changes in restrictions on equity flows, bond flows and FDI. The prudential measure treatments include changes in rules affecting banks and foreign exchange. Different types of controls and prudential measures may be more effective at targeting certain variables. Changes in CFMs that receive more widespread attention—especially by financial analysts—may also have greater effects on key outcome variables if CFMs work largely through a signaling effect (as suggested in Forbes et al., 2012). Therefore, this section defines the CFM treatment variables using narrower definitions in order to analyze if specific types of CFMs had different effects than found for the larger group. More specifically, we test for any effects of changes in controls directed at equity and bond flows on different types of portfolio flows, of changes in "major" controls (that received more attention by financial analysts) on portfolio flows and exchange rates, and changes in prudential regulations directed at banks or foreign exchange on various measures of inflation and financial vulnerability.

To begin, we more narrowly define the changes in capital controls in our database as changes targeting bond flows, equity flows, and/or FDI (with some events targeting more than one type of flow). Of the 63

[26] For example, the negative effect of increased inflow controls on bank leverage is not significant for any week when nearest neighbor without replacement or nearest neighbor matching is used.
[27] The effect from increases in prudential measures is significant when matching is done using the local-linear or radius algorithms, but not when using the nearest-neighbor algorithms.

events that involve increased inflow controls or decreased outflow controls, the events are more heavily weighted towards changes targeting bond flows (45 events) and equity flows (33 events) compared to FDI (only 13 events). Figure 6 then uses these more narrowly defined CFM treatments to estimate the ATT of capital controls directly targeting bond flows (the left column) or equity flows (the right column). There are not enough observations to estimate the effect of controls targeting FDI. The figure shows that in each case, the direction of the effect is as expected; tightening controls on bond or equity inflows and reducing controls on bond or equity outflows reduces both bond and equity net inflows.[28] The effect of changes in bond and equity controls on net bond inflows, however, is not significant.

In contrast, changes in equity controls (and especially increased restrictions on equity inflows) generate larger and significant negative effects on net equity inflows. The graphs in the bottom row also show a noteworthy result. Increased controls targeting bond inflows cause a significant and large decline in equity flows—starting at about 10 weeks and continuing through the entire 6-month window. (This result is robust across matching algorithms.) This result is consistent with Forbes et al. (2012), which finds that changes in Brazil's taxes on bond inflows generated a significant reduction in equity allocations to Brazil. This result is also consistent with the argument that capital controls can affect capital flows through some type of signal rather than through a direct cost of the controls (as modeled in Bartolini and Drazen, 1997).

Next, we more narrowly define the capital controls and prudential measures in our database as CFMs that were "major" in the sense that they received more discussion or attention by investors, financial analysts, or international financial institutions. To create this definition, we review a broad sample of analyst reports written from 2009-2012 on CFMs as well as papers written by the IMF and think tanks that survey recent changes in CFMs over this time period.[29] Any CFM event that is mentioned in at least one of these sources is then defined as a "major" event. Only 39 events in the full database of 135 CFMs aimed at reducing inflow pressures are identified as "major" events, confirming that many of the changes in CFMs may have been fairly minor changes or occurred in smaller countries that did not receive substantial attention by investors.

[28] Two graphs which are not shown report the impact of reduced controls on bond outflows on net equity flows and of reduced controls on equity outflows on net bond flows. The graphs are virtually identical to those in Figure 6 showing the impact of reduced controls on equity outflows on net equity flows and of reduced controls on bond outflows on net equity flows.

[29] We use analyst reports written by Goldman Sachs, HSBC, JPMorgan and Morgan Stanley. We also review regular reports on capital flows written by the Institute of International Finance, Peterson Institute for International Economics, and the series of papers related to CFMs written by the IMF.

Figure 7 then shows the ATTs of these "major" CFM events on the nominal and real exchange rate and net portfolio inflows. The estimated effects of these "major" CFM treatments are similar to those for the larger sample of CFM treatments (shown in Figure 2), although there are two noteworthy differences. First, major increases in inflow controls do not appear to have stronger effects in terms of reducing exchange rate appreciation and net portfolio inflows. In fact, exchange rates no longer show evidence of depreciating (relative to the counterfactual) after "major" new controls are announced. There is still some evidence that "major" inflow controls could reduce net portfolio inflows, but this effect is only significant at the 5% level for 3 weeks out of the 6-month window and is also not robust across different matching algorithms. Second, and in contrast, "major" decreases in controls on outflows appear to have a more significant effect on exchange rates than for the full sample of controls. Reduced outflow controls now yield a significant depreciation of the nominal and real exchange rate after about 3 months for at least a month—although the effect is still small in magnitude.[30] Therefore, focusing only on more prominent examples of changes in capital controls and prudential measures generally does not change the result that these CFMs do not appear to have large and significant effects on exchange rates and portfolio inflows, with the exception that prominent examples of removing controls on capital outflows can lead to a moderate depreciation of the nominal and real exchange rates over about 3-4 months.

As a final extension of the base analysis, we classify the changes in prudential measures in our database as measures targeting banks and those targeting foreign exchange exposures. This classification shows that of the 72 increases in prudential measures in the sample, 57 were restrictions aimed directly at banks and 63 aimed directly at foreign exchange exposures (obviously with a substantial number involving restrictions on both, such as limits on the foreign exchange exposure for banks). Then we test if these narrower types of prudential measures have different effects on key outcome variables than found for the broader sample of treatments. These results are reported in Figure 8, with the top row reporting results for the full set of prudential measures, the middle row for measures targeting banks, and the bottom row for measures targeting foreign exchange.

Estimated ATTs using these narrower definitions of prudential measures generally yield the same results as for the broader sample, with only a few noteworthy changes. First, as shown in the left of Figure 8, changes in bank regulations may cause a greater decline in inflation than changes in FX regulations, although the estimated effects continue to be insignificant (as found in the full sample). As an extension, the middle column of Figure 8 reports the ATT of each type of prudential measure on consensus inflation

[30] These results are robust when matching is performed using the radius algorithm, but are insignificant when matching is performed using the nearest neighbor algorithms.

expectations (for CPI inflation over the next 52 weeks). These results show that changes in each group of prudential measures leads to a significant decrease in expected inflation starting about 4 months after the treatment and lasting through the end of the sample. This suggests that increased prudential regulations—whether targeting banks or foreign exchange—may reduce inflation expectations but any effect on actual inflation occurs with a substantial lag. The other noteworthy result is the estimated ATTs of different types of prudential regulations on country exposure to short-term debt. Regulations targeting banks generate a significant and large reduction in country exposure to short-term debt—in contrast to weaker effects for regulations focusing on foreign exchange and the broader sample of regulations.[31] This suggests that prudential regulations focusing on banks may be more successful at reducing this form of financial fragility.

C. Results: Tying it All Together

To summarize, this section reports estimates of the effects of capital controls and prudential regulations on a series of outcome variables over each week in 6-month windows. The results indicate that CFMs can have significant effects on some of the variables which they are intended to influence. The strongest effects of CFMs appear to be in terms of reducing certain financial vulnerabilities. Increased prudential measures significantly reduce bank leverage and increased controls on capital inflows reduce the growth in private credit. Increased banking regulations and decreased controls on capital outflows both generate significant reductions in country exposure to short-term debt.

In contrast to these robust results showing that CFMs can reduce financial fragilities, there is more limited evidence that they can affect two other primary goals: exchange rate appreciation and net capital inflows. Removing controls on capital outflows is the one policy which generates a significant and robust depreciation in the real exchange rate—especially when focusing only on "major" changes in capital controls—although the maximum estimated effect is a depreciation of less than 2.5% relative to the counterfactual. Certain specifications suggest that adding controls on capital inflows may also reduce net portfolio inflows, but this result is not robust to different matching algorithms and the magnitude of the effect is small. A closer look at different types of portfolio flows suggests that bond flows are not affected by capital controls (and even by controls narrowly targeting bond flows), but equity flows are more likely to be affected by changes in capital controls on either equities or bonds.

[31] This result is robust to matching based on the five-nearest neighbor algorithm, but is insignificant when based on the radius or nearest-neighbor with no replacement algorithms.

Finally, the results find little evidence that changes in CFMs affect other macroeconomic variables and financial market volatilities. Increased inflow controls, decreased outflow controls, and increased prudential measures all have no significant effect on interest-rate differentials versus the United States, on equity indices, or on inflation (although changes in prudential measures can reduce expectations of future inflation). Moreover, none of these CFMs leads to a significant and robust reduction in the volatility of exchange rates, portfolio flows, or interest rate differentials.

V. Conclusions

An extensive literature has attempted to assess the impact of capital controls and prudential measures. Two serious challenges for this literature are selection bias and endogeneity; countries which change their capital controls and prudential measures are different than countries which do not change their controls. Countries adjust these policies in response to changes in key macroeconomic variables, which are often the targets of the controls and prudential measures. This paper takes these problems with selection bias and endogeneity seriously. It uses several different matching methodologies in order to create control groups to establish the counterfactual behavior of key outcome variables in the absence of changes in capital-flow management policies. This is the first attempt (to the best of our knowledge) to use this methodology to analyze the impact of capital controls and prudential measures.

In order to perform this analysis, the paper begins by constructing a new database which includes detailed changes in capital controls and prudential measures on a large sample of developed, emerging and developing economies from 2009 through 2011. The analysis then estimates propensity scores predicting the probability that each country adopts a specific capital control or prudential measure based on a set or country-specific and global variables. It uses these propensity scores to match each CFM treatment with a control group to measure the counterfactual effect on key outcome variables. The analysis focuses on the impact of CFMs aimed at reducing pressures from capital inflows—increased controls on capital inflows, decreased controls on capital outflows, and increased prudential regulations.

The results indicate that certain types of CFMs can accomplish specific goals—especially in terms of reducing financial vulnerabilities—but most CFMs are less effective in accomplishing their other stated goals. More specifically, CFMs can significantly reduce financial fragilities such as bank leverage, the growth in private credit, and country exposure to short-term debt. CFMs do not appear to have a significant effect on most other macroeconomic variables and financial market volatilities, including on interest-rate differentials, equity indices, inflation, or on the volatility of exchange rates, portfolio flows,

or interest rate differentials. CFMs have mixed effects on two of their primary goals: reducing exchange rate appreciation and net capital inflows. One type of CFM—removing controls on capital outflows—can yield a significant but small effect on exchange rates (with a maximum depreciation of less than 2.5% relative to the counterfactual). Changes in capital controls can significantly reduce net equity inflows, but do not significantly affect bond inflows or total portfolio inflows (which tend to be dominated by bond instead of equity flows).

These results have two important implications. First, much of the recent policy debate on the use of CFMs to reduce exchange rate appreciation has focused on the use of controls on capital inflows. There is little evidence that controls on capital inflows can accomplish this goal. Instead the evidence suggests that removing controls on capital outflows (if any exist for a given country) would be more effective tool for limiting exchange rate appreciation. This supports recent evidence in Pasricha (2012) and Aizenman and Pasricha (2012) which shows that the liberalizations in capital outflows that occurred in the 2000s were largely a response to surging capital inflows.

Finally, the debate on the use of capital controls (and to a lesser extent prudential measures) has historically focused on affecting the exchange rate, capital flows, and other macroeconomic variables. Only recently has the debate shifted to using these policies to reduce other forms of financial vulnerabilities (i.e., Ostry et al., 2012). The evidence suggests that this new focus is likely to be more productive and effective. Capital controls and prudential measures have no consistently significant effect on exchange rates (except from removing controls on outflows), capital flows, and other macroeconomic variables, while controls and prudential measures appear to be more effective at reducing other forms of financial fragility—such as reducing bank leverage, the growth in private credit, and exposure to short-term debt. Policymakers evaluating whether to use different forms of capital controls and prudential measures should therefore be realistic about what these measures are (and are not) good for.

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Appendix A
Information on Capital Flow Management Data Set

The primary source of information for this dataset is the *Annual Report on Exchange Arrangements and Exchange Restrictions* by the International Monetary Fund. We focus on the end of the section on each country which reports any changes in capital flow management policies which occurred over the past year. We also supplement this source with information from reports from Goldman Sachs, HSBC, Institute of International Finance, JP Morgan Chase, and Morgan Stanley that include information on capital flow policies or country-specific information. We also incorporate information from two papers on capital controls: Magud, Reinhart, and Rogoff (2011) and Reinhart, Kirkegaard, and Sbrancia (2011). Finally, we confirm and add several additional CFM measures using primary news sources.

Examples of the types of CFMs that are included in the database and how they are classified as "capital controls" or "prudential measures" is listed in the table below.

Types of Capital Flow Management Techniques

Capital controls	Prudential Measures
• Quantitative limits on foreign ownership of domestic companies' assets • Quantitative limits on borrowing from abroad • Limits on ability to borrow from offshore entities • Restrictions on purchase of foreign assets including foreign deposits • Special licensing on FDI and other financial transactions • Minimum stay requirements for new capital inflows • Taxes on capital inflows • Reserve requirements on inflows of capital (e.g. unremunerated reserve requirements)	• Reporting requirements and limitations on maturity structure of liabilities and assets • Restrictions on off-balance-sheet activities and derivatives contracts • Limits on asset acquisition • Limits on banks' FX positions • Limits on banks' lending in FX • Asset classification and provisioning rules • Tax on foreign exchange transactions • Capital requirements on assets • Asset-backed reserve requirements • Differential reserve requirements on liabilities in local and FX currencies

Measures which are NOT included as CFMs in the database are:

- Limits on capital flows when targeted at specific countries as part of sanctions for political reasons (such as restrictions on transactions with Libya or Iran.
- Transactions by the central bank or government in foreign exchange markets aimed at affecting the exchange rate.
- Changes in prudential regulations that are not related to foreign exchange and do not differentially affect foreigners—such as an increase in reserve ratios that affects all deposits and the entire banking system.
- Automatic changes in limits on foreign investment that result from pre-specified indexing to inflation (as occurs in Australia).
- Regulations resulting from specific trade disputes or issues related to one specific industry (including specific restrictions on the oil and gas industry).
- Changes in rules related to foreign purchases of land.
- Minor changes affecting nonresidents living or travelling abroad or residents travelling abroad (such as limits on gifts to family members in different countries, payments for education or medical expenses abroad, or access to foreign currency for travel).

In each case, we use the week of the announcement date of the CFM if it is available, and if not, we use the week of the implementation date as listed in the AREARs. In a few cases, multiple CFMs are put into place in the same week. In most of these cases, these multiple measures were aimed at a similar goal and are coded as a single CFM event. For example, in the week of July 1, 2009 Peru enacted different measures to stem appreciation pressures—a control on capital inflows (a ban on foreign purchases of central bank bills and increased fees on central bank liquidity draining instruments) as well increased prudential regulations (increased reserve requirements on certain foreign liabilities). In these cases, the week is coded as implementing a new CFM which is both a control on capital inflows and a prudential measure, affecting both bonds and banks. In a few cases, countries made multiple changes, which may have partially counteracted each other. In these cases, we include the most important control in the database based on the government's intentions according to statements made on the announcement date.[32]

We compile this data on CFMs' using a broad sample of countries. We begin with all "Advanced Economies" (as defined by the International Monetary Fund as of October 2012) and all "Emerging Markets" and "Frontier Economies (as defined by Standard & Poor's BMI indices). We then exclude current members of the euro area, the three largest advanced economies (the United States, United Kingdom, and Japan) and countries that do not have data on equity or bond flows in the EPFR dataset, and very small countries (with GDP less than $15 billion at the end of 2011).[33] This yields a sample of 60 countries which are listed below.

Countries in the Sample

Argentina	Egypt	Lithuania	Russia
Australia	Ghana	Malaysia	Singapore
Bahrain	Hong Kong	Mexico	South Africa
Botswana	Hungary	Morocco	Sri Lanka
Brazil	India	New Zealand	Sweden
Bulgaria	Indonesia	Nigeria	Switzerland
Canada	Israel	Norway	Taiwan
Chile	Jamaica	Oman	Thailand
China	Jordan	Pakistan	Trinidad & Tobago
Colombia	Kazakhstan	Panama	Tunisia
Cote d'Ivoire	Kenya	Peru	Turkey
Croatia	Korea	Philippines	Ukraine
Czech Republic	Kuwait	Poland	United Arab Emirates
Denmark	Latvia	Qatar	Vietnam
Ecuador	Lebanon	Romania	Zambia

[32] For example, in October 2010, Thailand reinstated a 15% withholding tax on foreigners' interest and capital gains on new Thai government bonds issued by the government or GSEs. On the same date, the ceiling on foreign currency deposits with local banks was raised. Since the former announcement received substantial attention, but not the later, the first is coded in the data as a new control on capital inflows, but the change in the prudential regulation is not included in the dataset.

[33] Countries that are excluded because they do not have data on either equity or bond flows in the EPFR dataset are: Bangladesh, Iceland, Iran, Mauritania, and Moldova. Countries that are excluded because they have GDP less than $15 billion at the end of 2011 are: Iceland, Mauritania, Mauritius, Moldova, and Namibia.

Appendix B: Data Definitions and Sources

Variable	Source, Original frequency & Other Notes
Bank Leverage	Bank credit to bank deposits, from the World Banks, Global Financial Development Database, annual (through 2010)
Capital account openness	Measure of openness constructed from the IMF AREARs data with a higher value indicating greater openness, Chinn and Ito (2008), updated as of 04/24/2013 and available at: http://web.pdx.edu/~ito/Chinn-Ito_website.htm
CPI Inflation	IMF IFS, monthly
CPI inflation forecasts, 52 week forward	Consensus expectations, IPA calculations, monthly
Equity index, based on broad market measure	Datastream, weekly (end of week)
Floating exchange rate regime dummy	0-1 dummy that classifies exchange rates based on de jure and de facto measures as peg, intermediate or floating , Source: Ghosh et al. (2011),
Foreign Exchange Reserves as a share of GDP (from previous year)	IMF, IPA calculations, monthly; data for Taiwan from http://www.cbc.gov.tw/ct.asp?xItem=29908&ctNode=859&mp=2
GDP per capita	GDP per capita from the IMF's WEO database; written 3 ways: expressed in nominal USD terms
Global Risk—VIX	Haver, weekly
Interest rate differential between domestic interest rate and U.S. interest rate on 3 month T-bills	All data from Datastream & IPA calculations, weekly, data for Chile is only available using the overnight interest rates
Legal Compliance index	ICRG databases, monthly, Max=12 high legal compliance, Min=0 No legal compliance. Legal compliance index = Sum of Law and Order Index, Bureaucracy Quality Index, and Legislative Strength Index; List of variables available at: http://www.prsgroup.com/VariableHelp.aspx ICRG Methodology: http://www.prsgroup.com/ICRG_Methodology.aspx
Nominal effective exchange rate, (broad)	Haver, JPMorgan, weekly average; higher values mean appreciation of domestic currency
Portfolio bond flows in USD (usually expressed as a % of bond assets at the start of period)	EPFR, weekly. Accumulated over past 13 weeks
Portfolio equity flows in USD (usually expressed as a % of equity assets at the start of period)	EPFR, weekly. Accumulated over past 13 weeks
Portfolio flows in USD, sum of equity and bond flows from above (usually expressed as a % of portfolio assets at the start of period)	EPFR, weekly. Accumulated over past 13 weeks
Portfolio investment as a share of gross capital inflows	Calculated as portfolio investment liabilities in all sectors and materials as a share of total liabilities (gross inflows); data reported in Haver and based on the IMF BOP, quarterly

Private Credit, in local currency or as share of GDP (from previous year)	IMF, IPA calculations, monthly
Real effective exchange rate	BIS real exchange rate index average for 44 countries, and if not available, then from the IMF. The real exchange rate index broad. Data series is constructed for Vietnam. Available monthly; higher values mean appreciation of domestic currency
Short-term external debt to GDP	Short term external debt as share of GDP in USD (all converted into millions), annual basis
Stock market capitalization to GDP	World Banks, Global Financial Development Database, annual

Table 1

Capital Flow Measures

	Controls on Inflows		Controls on Outflows		Prudential Measures		Related to Pressures From Capital:		
	–	+	–	+	–	+	Outflows	Inflows	Total
Argentina	3	2	3	1	2	1	6	6	12
Brazil	1	7	0	0	0	2	1	9	10
Bulgaria	1	0	1	0	0	0	1	1	2
Chile	0	0	1	0	0	0	0	1	1
China	4	0	2	0	2	0	6	2	8
Colombia	1	0	1	1	1	2	3	3	6
Côte d'Ivoire	0	0	0	1	0	0	1	0	1
Croatia	1	0	3	1	3	2	5	5	10
Czech Republic	0	0	1	0	0	1	0	2	2
Ecuador	0	0	0	1	0	0	1	0	1
Ghana	0	0	0	0	0	1	0	1	1
Hungary	0	0	0	0	1	2	1	2	3
India	4	0	0	0	1	2	5	2	7
Indonesia	0	2	0	0	0	4	0	6	6
Israel	0	0	0	0	0	2	0	2	2
Jamaica	0	0	0	0	1	2	1	2	3
Kazakhstan	1	0	2	1	1	1	3	3	6
Kenya	0	0	0	0	0	1	0	1	1
Korea (South)	0	2	1	0	0	6	0	9	9
Latvia	0	0	0	0	0	2	0	2	2
Lebanon	0	0	1	0	0	2	0	3	3
Malaysia	2	0	3	0	3	0	5	3	8
Mexico	1	0	0	0	0	0	1	0	1
Morocco	0	0	2	0	3	0	3	2	5
Nigeria	1	0	0	0	0	0	1	0	1
Oman	0	0	1	0	0	0	0	1	1
Pakistan	0	1	0	0	1	1	1	2	3
Peru	0	1	0	0	2	14	2	15	17
Philippines	1	0	3	0	3	0	4	3	7
Romania	0	0	0	0	3	1	3	1	4
Russia	0	0	0	0	0	9	0	9	9
South Africa	4	0	8	0	1	0	5	8	13
Sri Lanka	2	0	2	0	1	0	3	2	5
Taiwan	0	0	0	0	0	2	0	2	2
Thailand	0	1	2	0	1	0	1	3	4
Tunisia	0	0	1	0	1	0	1	1	2
Turkey	0	0	2	0	5	4	5	6	11
Ukraine	2	1	1	0	11	5	13	7	20
Vietnam	0	4	0	1	2	3	3	7	10
Total	***29***	***21***	***42***	***7***	***49***	***72***	***85***	***135***	***220***

Notes: The "**–**" denotes the removal or easing of a control or prudential measure and the "+" denotes the addition or tightening of measure. Countries included in the sample which do not have a CFM event are: Australia, Bahrain, Botswana, Canada, Denmark, Egypt, Hong Kong, Jordan, Kuwait, Lithuania, New Zealand, Norway, Panama, Poland, Qatar, Singapore, Sweden, Switzerland, Trinidad & Tobago, United Arab Emirates, and Zambia.

Table 2

Logit Regression Results to Calculate Propensity Scores:

Adoption of CFMs to Reduce Capital Inflow Pressures

	Controls on Capital Inflows (Increases)	Controls on Capital Outflows (Decreases)	Prudential Measures (Increases)
Real exchange rate	11.859***	5.072*	1.611
(% change)	(3.012)	(2.615)	(1.860)
Portfolio flows over last 6 months	0.002*	0.004	0.001
(% change)	(0.001)	(0.003)	(0.001)
Consensus CPI,	0.199*	-0.143	0.334***
52-week expectations	(0.114)	(0.103)	(0.066)
Private credit growth to GDP	0.717	0.667	4.568***
(% change)	(2.866)	(2.637)	(1.746)
VIX	0.020	0.041*	-0.062***
	(0.036)	(0.024)	(0.018)
Interest rate vs. US	-0.043	-0.036	0.034
(change in overnight rate)	(0.124)	(0.051)	(0.048)
FX Reserves/GDP	-0.409	-1.611*	-0.729
(% change)	(0.887)	(0.964)	(0.701)
Floating ER dummy	-0.415	0.570	1.594***
	(0.552)	(0.552)	(0.361)
Capital account openness	-0.115	-0.891***	0.569***
	(0.358)	(0.217)	(0.145)
Log GDP per capita	0.213	0.651*	0.052
	(0.403)	(0.345)	(0.219)
Stock market capitalization	-0.012**	0.007***	-0.000
(% of GDP)	(0.006)	(0.002)	(0.001)
Legal compliance	-19.329	100.613**	80.256***
	(21.450)	(43.253)	(24.772)
Legal compliance squared	3.581	-24.562**	-18.981***
	(5.064)	(10.355)	(5.806)
Constant	16.202	-114.093**	-90.838***
	(24.220)	(45.710)	(26.746)
Observations	***4,953***	***4,708***	***4,394***
Pseudo R^2	***0.186***	***0.199***	***0.154***

Notes: Results of a logit regression predicting the probability of a change in the CFM each week. CFM events are listed in Table 1, and an "exclusion window" is created for the 3 months before and after any event. Explanatory variables are defined in Table B. Robust standard errors. Changes are calculated over 52-weeks in order to adjust for any seasonal effects. * indicates significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 3

Increased Controls on Capital Inflows:

Means for Treatment and Control Groups using Different Matching Algorithms

	Mean: Treatment Group (μ_T)	Mean: Unmatched Control (μ_C)	t-Statistics (H0: $\mu_T = \mu_C$)	Nearest Neighbor (no replacement)		5 Nearest Neighbors		Radius with Caliper		Local-linear	
				Mean: Matched Control	t-stat	Mean: Matched Control	t-stat	Mean: Matched Control	t-stat	Mean Matched Control	T-stat
Real exchange rate	0.090	0.008	4.21***	0.076	0.50	0.090	0.00	0.068	0.21	0.083	0.23
Portfolio flows	0.401	-2.541	0.21	-2.200	0.73	-0.743	0.60	-0.283	0.07	-3.706	1.29
Consensus CPI,	7.156	4.158	4.78***	7.575	-0.32	6.498	0.57	6.974	-0.37	6.993	0.13
Credit growth	0.044	0.026	0.99	0.032	0.37	0.026	0.62	0.046	-0.07	0.022	0.70
VIX	25.752	26.482	-0.39	28.036	-0.87	27.422	-0.65	26.363	-0.74	26.935	-0.47
Interest rate vs. US	-0.523	-0.149	-0.56	-0.735	0.09	-0.540	0.01	-0.280	-0.29	-1.300	0.33
FX Reserves/GDP	0.080	0.084	-0.06	0.032	0.66	0.072	0.11	0.057	0.22	0.057	0.32
Floating ER dummy	0.667	0.744	-0.81	0.762	-0.67	0.733	-0.46	0.659	0.51	0.762	-0.67
Capital account openness	0.073	1.016	-2.97***	0.242	-0.48	0.241	-0.48	0.266	-0.46	0.318	-0.71
Log GDP per capita	8.443	9.295	-3.26***	8.492	-0.17	8.508	-0.22	8.526	0.19	8.529	-0.29
Stock market cap.	43.231	84.666	-1.98**	47.662	-0.41	52.142	-0.78	48.646	-0.19	48.260	-0.47
Legal compliance	2.046	2.229	-3.82***	2.016	0.57	2.000	0.84	2.081	-0.91	2.012	0.63
Legal comp. squared	4.216	5.018	-3.76***	4.091	0.55	4.032	0.81	4.369	-0.93	4.078	0.61
Mean Propensity Score	***420.6***	***498.7***		***518.1***		***449.5***		***523.6***		***467.6***	
Observations	***21***	***4932***		***21***		***21***		***19***		***21***	

Notes: Reports difference in means between treatment and control groups, with control group created based on regression results reported in Table 2 and matching performed using algorithms listed at top of table. See Table 2 and Appendix B for detailed variable definitions. * indicates significant at the 10% level, ** at the 5% level and *** at the 1% level.

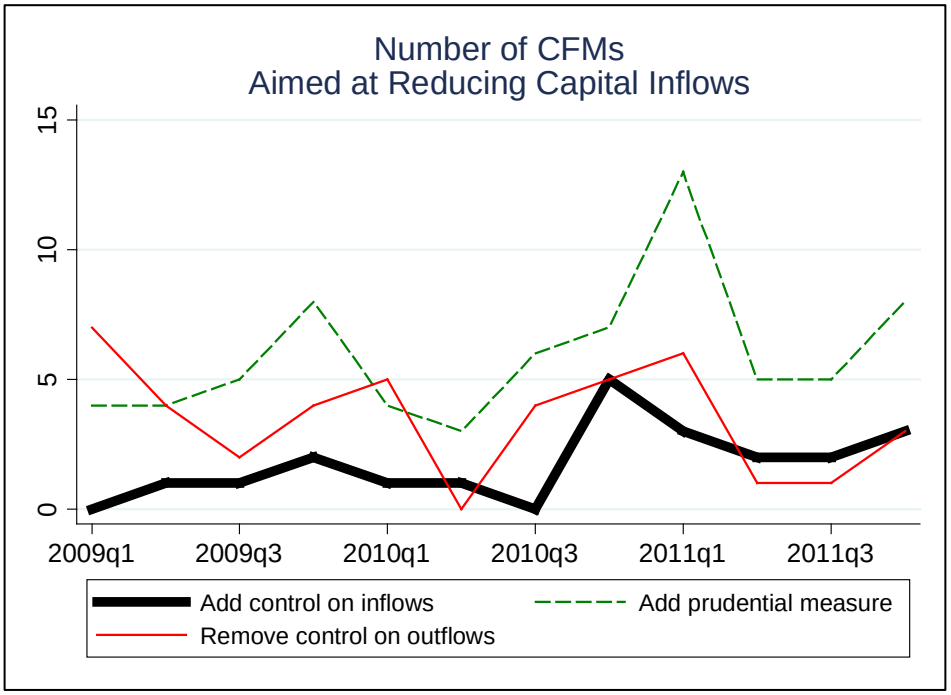
Table 4

Summary of Results for Different Matching Algorithms

			Matched Control Group Based on Matching Algorithm:			
	Treatment Group	**Unmatched Control Group**	**Nearest Neighbor (no replacement)**	**5 Nearest Neighbors**	**Radius with Caliper**	**Local-linear**
Increased controls on capital inflows						
Mean Propensity Score	420.6	498.7	518.1	449.5	523.6	467.6
Mean Absolute Bias		49.32	13.17	12.11	12.46	12.99
(Standard Deviation)		(39.19)	(6.92)	(8.70)	(9.21)	(6.36)
Observations	21	4932	21	21	19	21
Increased prudential measures						
Mean Propensity Score	548.1	476.7	559.3	559.3	576.7	564.8
Mean Absolute Bias		33.99	7.73	6.58	2.99	6.91
(Standard Deviation)		(22.70)	(7.46)	(4.87)	(2.81)	(6.46)
Observations	59	4335	59	59	58	59
Decreased controls on capital outflows						
Mean Propensity Score	439.0	496.8	532.5	499.0	497.4	487.9
Mean Absolute Bias		39.59	17.26	7.00	6.19	17.26
(Standard Deviation)		(35.25)	(12.28)	(4.96)	(4.42)	(12.28)
Observations	29	4679	28	28	26	28

Notes: Reports difference in means between treatment and control groups, with control group created based on regression results reported in Table 2 and matching performed using local-linear and five-nearest neighbors algorithms. * indicates significant at the 10% level, ** at the 5% level and *** at the 1% level.

Figure 1
Incidence of Different Types of CFMs: 2009 – 2011



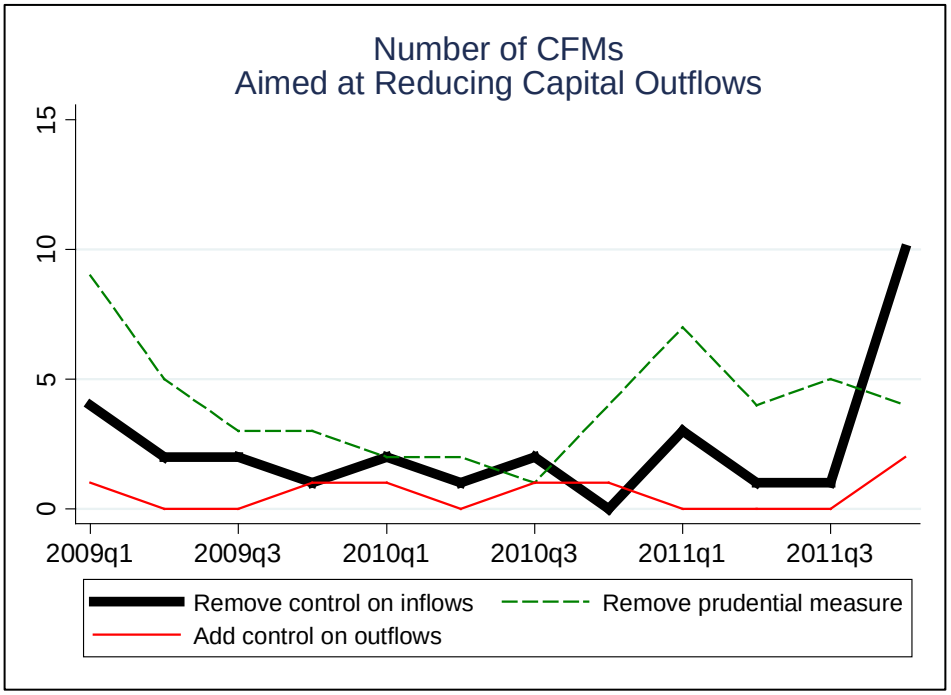
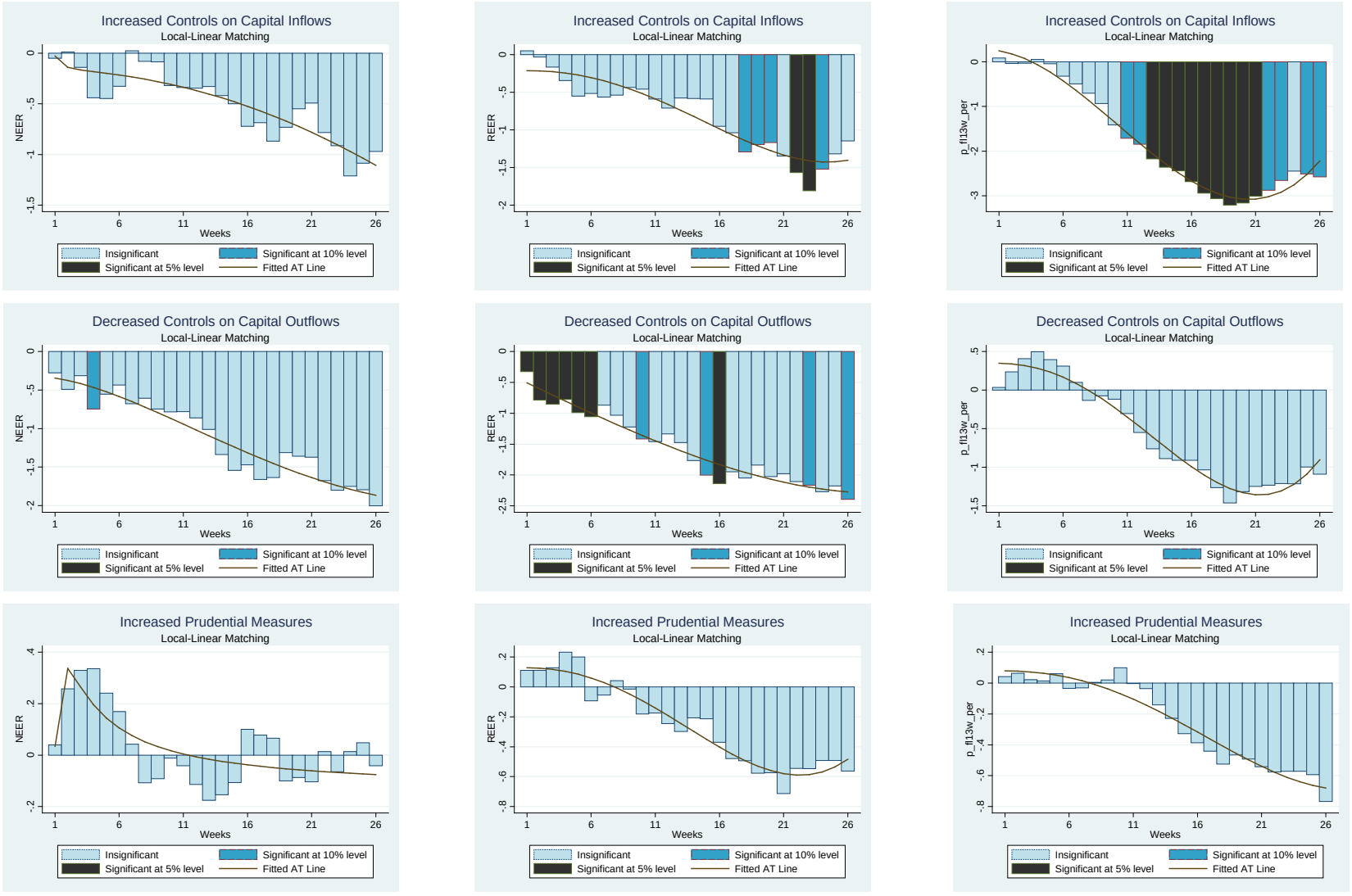


Figure 2

Average Treatment Effects of CFMs: Exchange Rates and Portfolio Flows

% Change in Nominal Exchange Rate | **% Change in Real Exchange Rate** | **Change in Net Portfolio Inflows[1]**

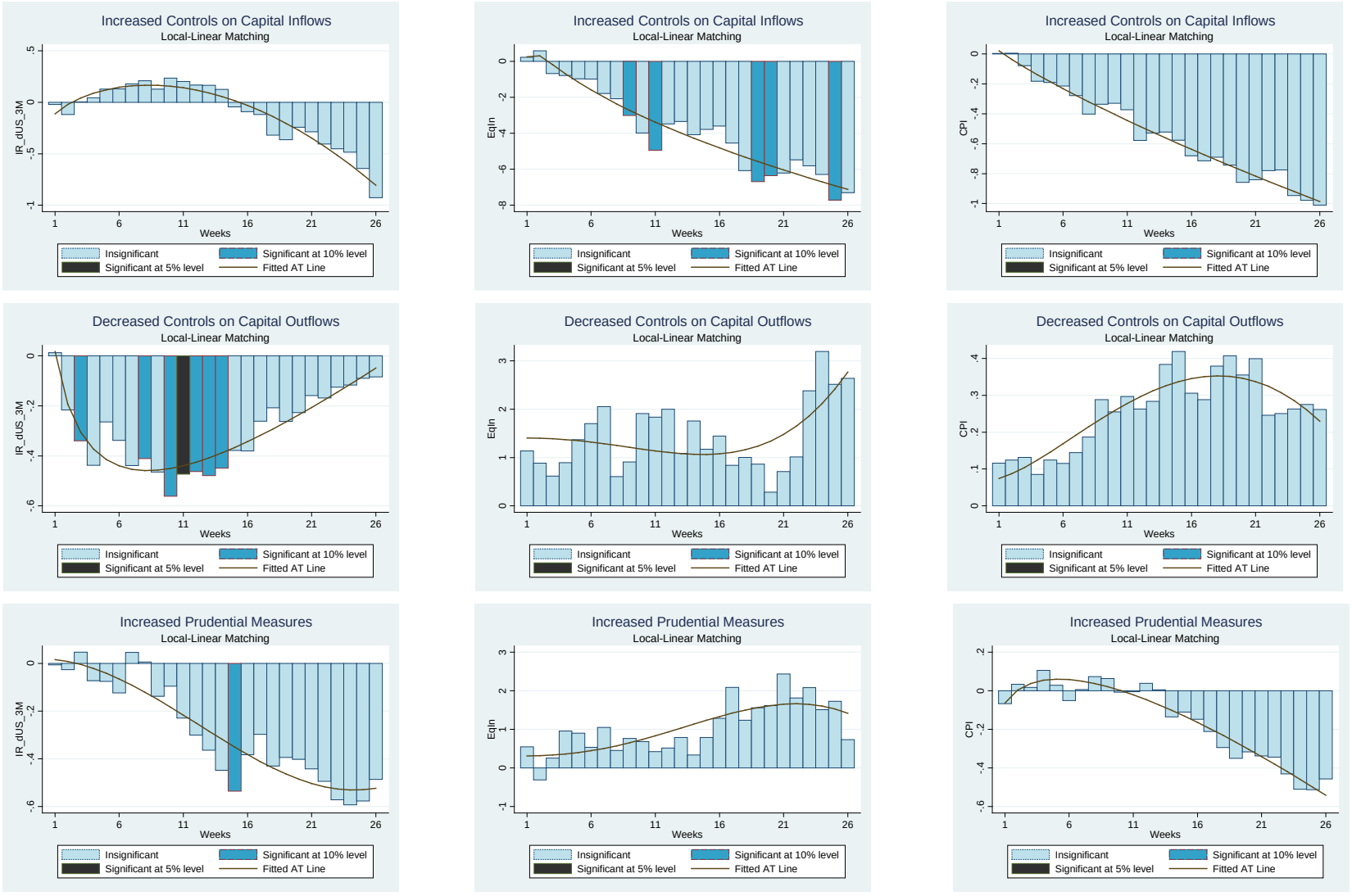


Notes: (1) Net portfolio inflows are cumulative flows over the last 13 weeks and measured as a percent of total portfolio assets lagged one period before the CFM event.

Figure 3

Average Treatment Effects of CFMs: Interest Rates, Equity Indices and Inflation

Change in Interest Rate Differential[1] | **% Change in Equity Index** | **Change in Inflation**

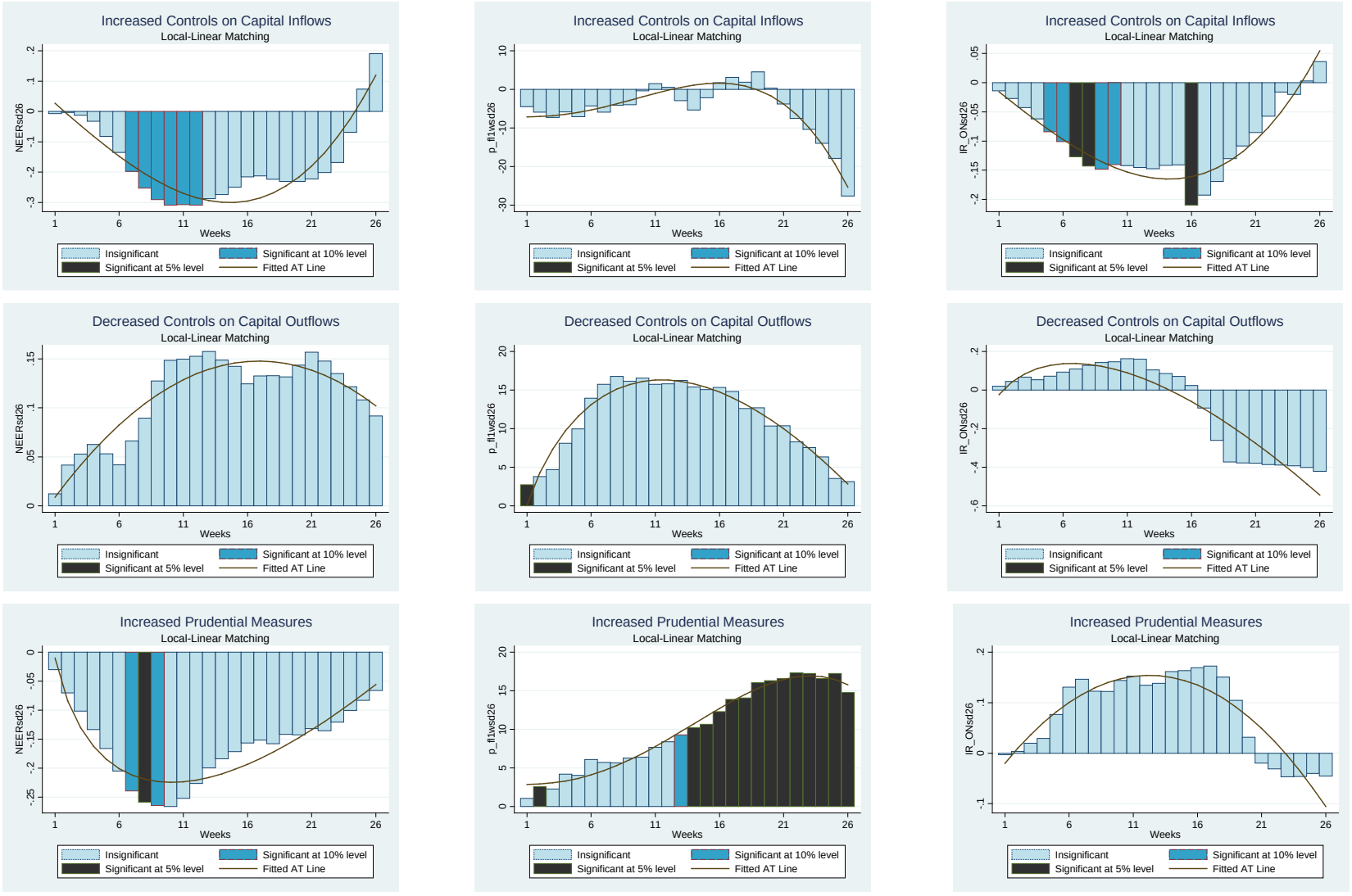


Notes: (1) Interest rate differential versus the United States for 3-month Treasury bills. Inflation measured by the CPI.

Figure 4

Average Treatment Effects of CFMs on Volatilities

Volatility in Nominal Exchange Rate | **Volatility in Net Portfolio Inflows** | **Volatility in Interest Rate Differentials**

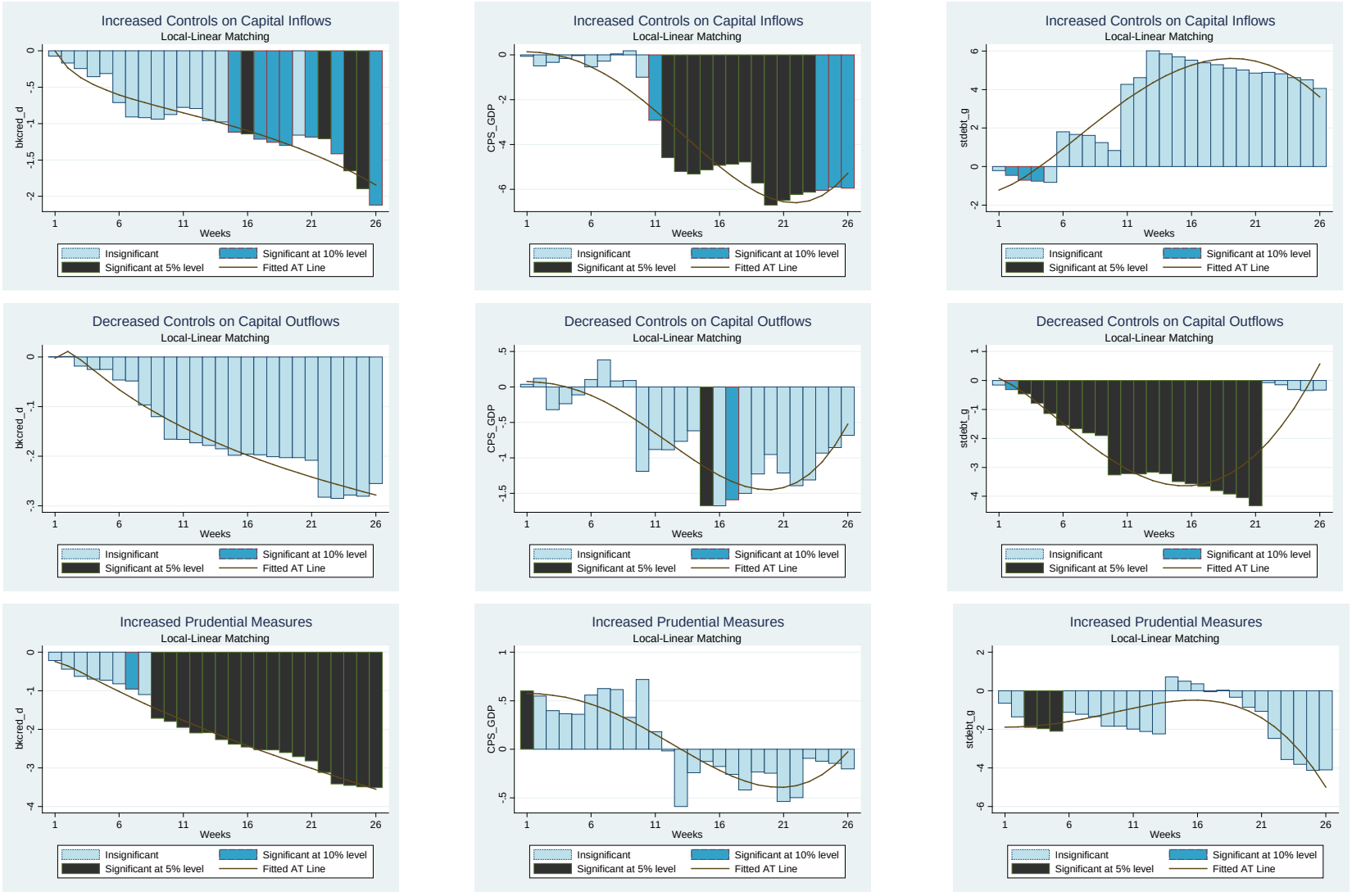


Notes: Volatility defined as standard deviation over previous 26 weeks.

Figure 5

Average Treatment Effects of CFMs: Financial Vulnerabilities

% Change in Bank Leverage[1] | **% Change in Private Credit[2]** | **% Change in Short-term Debt[2]**

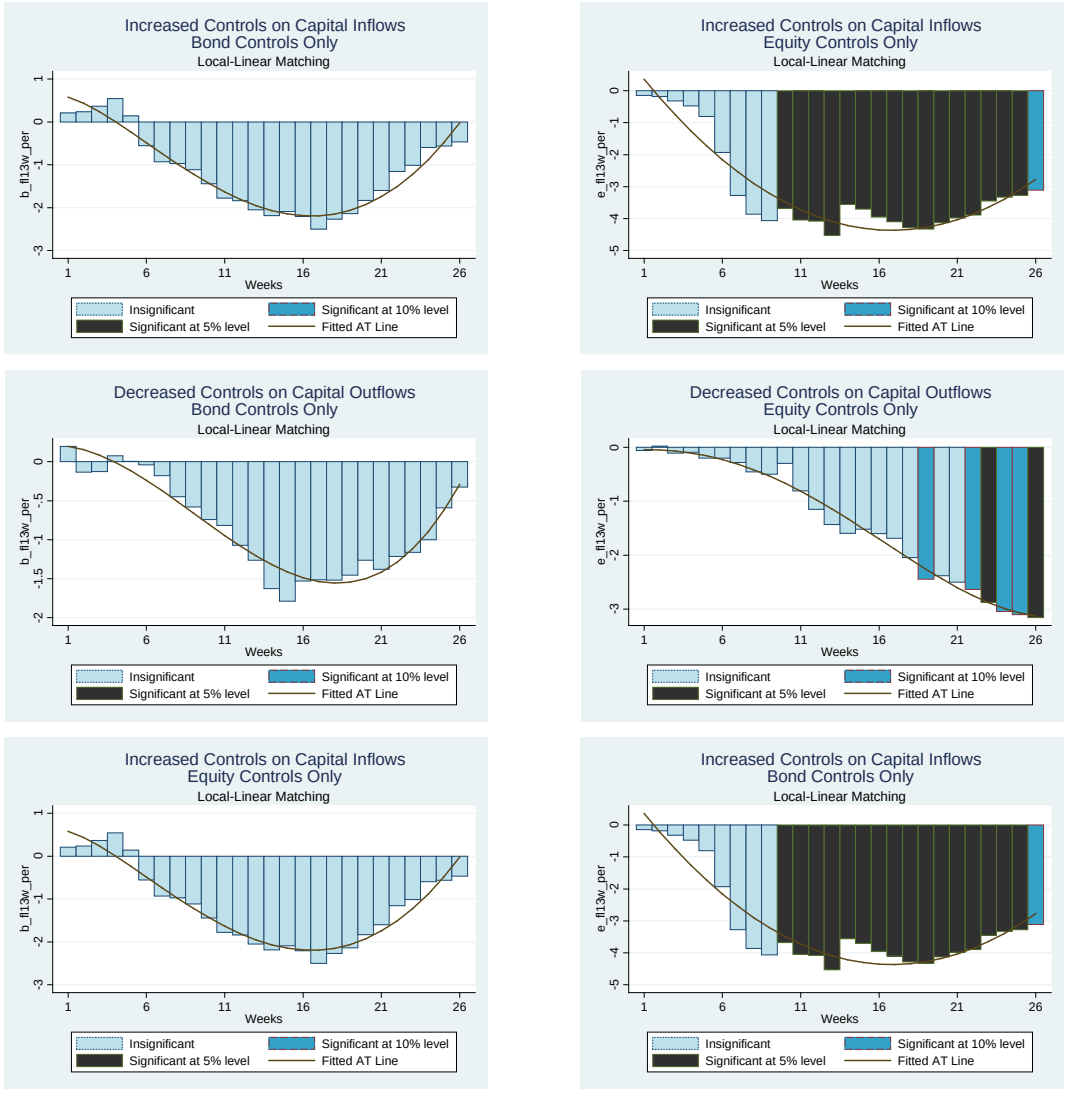


Notes: (1) Bank leverage measured by ratio of bank credit to bank deposits. (2) Measured as a share of GDP.

Figure 6

Average Treatment Effects of CFMs: Controls Targeting Bond or Equity Flows

Change in Net Bond Inflows | **Change in Net Equity Inflows**

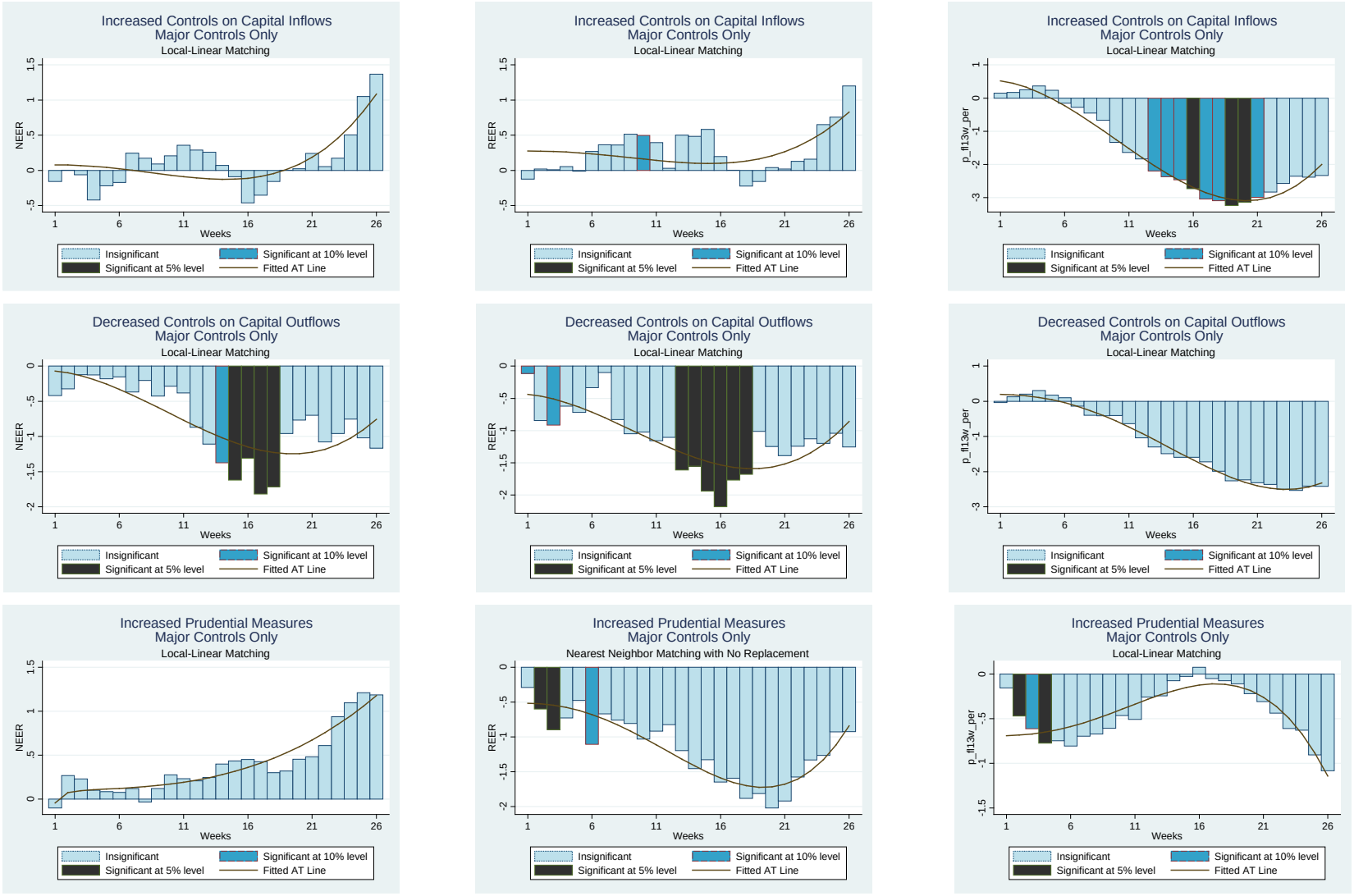


Notes: Net bond and equity inflows are cumulative flows over the last 13 weeks and measured as a percent of total bond or equity assets, respectively, lagged one period before the CFM event.

Figure 7

Average Treatment Effects of "Major" CFMs: Exchange Rates and Portfolio Flows

% Change in Nominal Exchange Rate | **% Change in Real Exchange Rate** | **Change in Net Portfolio Inflows[1]**

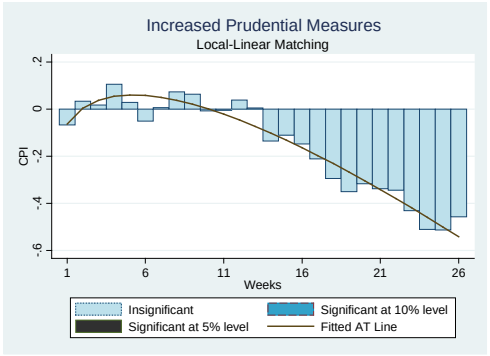


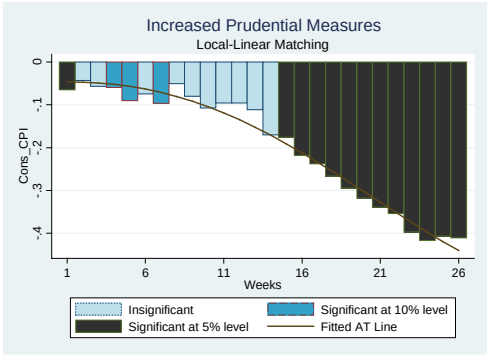
Notes: (1) Net portfolio inflows are cumulative flows over the last 13 weeks and measured as a percent of total portfolio assets lagged one period before the CFM event

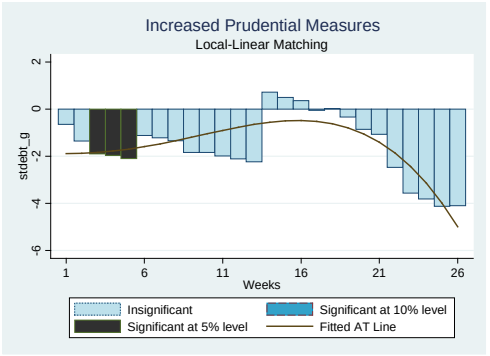
Figure 8

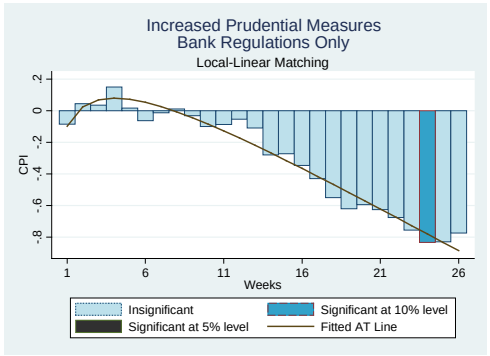
Average Treatment Effects of Prudential Measures Targeting Banks or Foreign Exchange

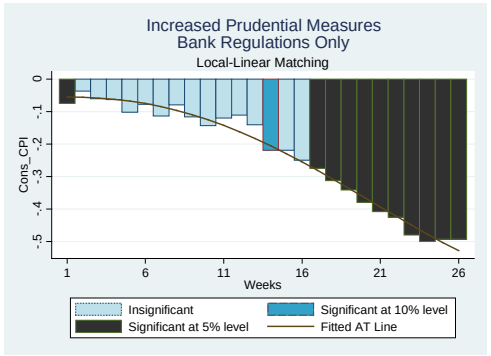
Change in CPI | **Change in Inflation Expectations[1]** | **% Change in Short-term Debt[2]**

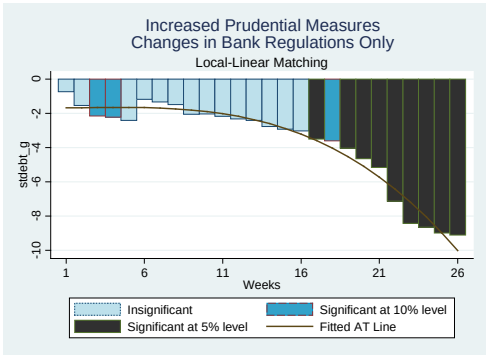


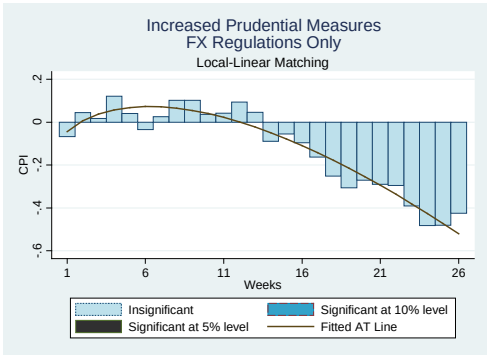


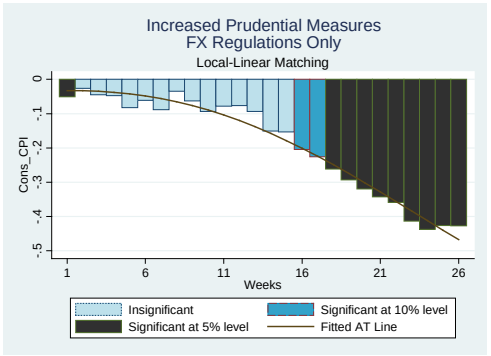


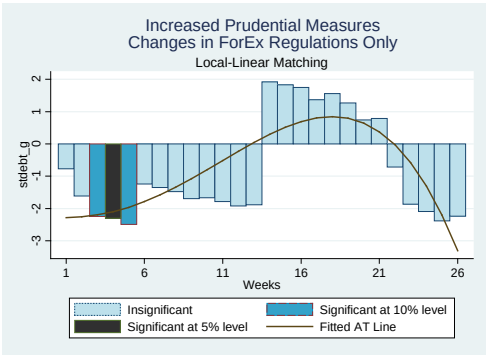












Notes: (1) Measured as consensus expectations for 52-week CPI inflation. (2) Measured as a percent of GDP.