Capital Controls and Macroprudential 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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COMMENTS APPRECIATED

Abstract: Assessing the effectiveness of capital controls and macroprudential 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 the CFMs are intended to influence. This paper addresses these challenges by using a propensity-score matching methodology. We also create a new database with detailed information on weekly changes in controls on capital inflows, capital outflows and macroprudential measures from 2009 to 2011. The results indicate that certain types of CFMs—especially macroprudential measures—can significantly reduce some measures of financial fragility (such as bank leverage, inflation expectations, bank credit growth, and exposure to portfolio liabilities). Most CFMs do not significantly affect other key targets, however, such as exchange rates, capital flows, interest rate differentials, inflation, equity indices, and different volatilities. The main exception is that removing controls on capital outflows may reduce real exchange rate appreciation. Therefore, certain CFMs can be effective in accomplishing specific goals—especially macroprudential measures aimed at reducing specific financial vulnerabilities—but many other popular measures are not "good for" 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 participants in the NBER's East Asian Seminar on Economics for helpful comments and suggestions. 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, and/or reducing specific measures of financial fragility (such as bank leverage, credit growth, asset bubbles, foreign-currency exposure, or short-term liabilities). But can CFMs accomplish any of these goals? If the answer is affirmative, CFMs could play an important role in helping stabilize economies in the presence of substantial capital flow volatility driven by external factors (see Bluedorn et al., 2013). This paper finds that specific CFMs can significantly influence certain variables (especially direct measures of financial fragility), but many CFMs are not “good for” accomplishing other stated aims.

CFMs primarily refer to two types of measures: (1) moderate, targeted capital controls that can be frequently adjusted in response to changing economic circumstances (also called “gates” by Klein, 2012, in order to differentiate them from more permanent and comprehensive capital controls); and (2) macroprudential measures related to cross-border or foreign currency exposure and lending (not including prudential regulations targeting individual institutions). Empirical evidence on the effectiveness of capital controls suggests that they have limited impact. Most surveys conclude that capital controls do not significantly affect exchange rates, the volume of capital flows, monetary policy independence, and other macroeconomic variables, but they can significantly affect the composition of capital flows and specific measures of financial vulnerability.[2] Even the IMF admits that: “Because capital controls have been used many times in the past, evidence on their effects is more abundant but still surprisingly inconclusive.” (IMF, 2013, pg. 20) Empirical evidence on the effectiveness of macroprudential measures is even more limited, partially due to their limited use before 2009, with the IMF admitting that: “Empirical evidence on the effectiveness of these measures is scant….” (IMF, 2013, pg. 17).

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[1] Key IMF papers 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] For recent surveys of this literature that show limited effects of capital controls, see Forbes (2007), Cline (2010), Magud, Reinhart, and Rogoff (2011), Klein (2012), and IMF (2013). For more recent evidence that controls and macroprudential measures may improve a country’s liability structure and increase its resilience to crises, see Chamon et al. (2013), Ostry et al. (2010), and IMF (2013). For the mixed evidence from just Brazil’s recent use of capital controls, see Chamon and Garcia (2013), Forbes et al. (2012), and Jinjarak et al. (2013).

Two major challenges in assessing the effectiveness of capital controls and macroprudential 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. For example, estimates in this paper show that from 2009 to 2011, countries with stronger institutions were significantly more likely to remove controls on capital outflows and increase prudential measures, but not increase controls on capital inflows. Moreover, governments tend to adjust their CFMs in response to changes in variables such as capital flows, exchange rates, asset prices, credit growth and other measures of financial stress—which are the variables that CFMs are intended to influence. For example, estimates in this paper show that countries are more likely to increase controls on capital inflows and reduce controls on outflows after a real exchange-rate appreciation, and more likely to increase macroprudential measures after an increase in private credit growth and inflation expectations. Although previous studies are usually aware of these challenges resulting from selection bias and endogeneity, it is impossible to control for all differences across countries using a limited set of observable statistics and attempts to find effective instruments to appropriately identify estimates have been unsuccessful.

This paper addresses these challenges by using a propensity-score matching methodology. This methodology is fairly new to the international macroeconomics literature, but has been used for years in medical and labor economics.[3] This methodology estimates a propensity score for each country in each week which captures the probability that the country will change its capital controls or macroprudential measures as a function of a set of observable variables. Then the estimated propensity scores are used to match the "treated" observations (countries which changed their CFMs) with control groups using several different matching algorithms. The constructed control groups can be used to estimate the counterfactual outcomes for key variables if countries had not changed their capital controls or macroprudential measures. This methodology has a number of advantages over standard estimation techniques, including its ability to solve the problem of non-random sample selection while avoiding strong assumptions about functional form and placing greater weight on more "similar" countries.

In order to perform this analysis, we begin by constructing a new database with detailed information on increases and decreases in controls on capital inflows, controls on capital outflows, and macroprudential measures at a weekly frequency from 2009 through 2011. We construct this database using a large sample of 60 countries in order to be able to construct accurate control groups with which to assess the impact of

[3] See Rosenbaum and Rubin (1985) for background on this methodology and Angrist and Pischke (2008) for an excellent discussion and examples from the labor literature.

various CFMs. We also divide our database into: measures aimed at stemming capital inflows/appreciation/credit growth or outflows/depreciation/credit contractions; capital controls affecting equities, bonds, or FDI; macroprudential measures targeting banks or foreign exchange; and CFMs that were "major" in the sense they received substantial attention by investors.

Then we estimate the probability that a country will change each type of capital control or macroprudential measure each week based on a set of observable variables measuring country-specific characteristics and changes in the global environment. We focus on predicting changes in CFMs directed at reducing pressures related to capital inflows (increased controls on inflows, decreased controls on outflows, and increased macroprudential regulations) as these were the primary tools utilized over the sample period from 2009 through 2011. These estimates provide intuitive results on which global variables, macroeconomic policies, and domestic characteristics influence a country's choice to adjust its CFMs. The estimates confirm that there are significant differences in the institutions and macroeconomic characteristics of countries that use these policies—confirming that selection bias is important. The estimates also confirm that many of the variables intended to be influenced by CFMs (such as exchange rates and credit growth) can significantly affect the probability of using each policy—confirming that endogeneity is important.

The results of these logit models are then used to estimate the propensity scores—the probability that each country changes its CFMs at each point in time. The resulting propensity scores are used to match each member of the treatment group with a control group, i.e. country-week observations with similar propensity scores. We use five different algorithms to perform this matching: nearest-neighbor without replacement, five-nearest neighbors, radius with caliper, kernel, and local-linear. A series of tests indicates that most of the matching algorithms perform well, although the radius and kernel methodologies have several disadvantages and/or do not satisfy certain tests. Focusing on our two preferred matching algorithms (local-linear and five-nearest neighbor), all treatment observations are "on-support" and the techniques remove significant differences between the treated countries and unmatched control groups. Therefore, these matching algorithms are able to remove any selection bias that is measured by the variables in the first-stage regression.

Next, we use these matched samples to estimate the average treatment effect on the treated (ATT) of each of the capital controls and macroprudential measures on a series of outcome variables. We estimate the cumulative effects for each week over a six-month window on variables that are frequently cited as goals for adjusting CFMs: the exchange rate, portfolio flows, other macroeconomic variables (interest-rate differentials, equity markets, and inflation), financial market volatilities (for the exchange rate, portfolio flows, and interest rates) and other financial vulnerabilities (private credit growth, bank credit growth, inflation expectations, bank leverage, and exposure to short-term external debt, portfolio liabilities, and foreign-currency liabilities). The results indicate that CFMs can have significant effects on some of the variables which they are intended to influence—although most effects are insignificant, small in magnitude, and not robust across matching methodologies. The strongest and fairly robust effects of CFMs appear to be in reducing certain measures of financial vulnerability—especially by increasing macroprudential regulations. Increased macroprudential measures significantly reduce bank leverage, inflation expectations, bank credit growth, and exposure to portfolio liabilities relative to the counterfactual. Increased controls on capital inflows reduce private credit growth and possibly bank leverage.

In contrast to these robust results showing that CFMs (and especially macroprudential measures) can reduce specific measures of financial 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 can generate a depreciation of the real exchange rate which is significant for over a month—although the maximum estimated effect is a depreciation of less than 2.5% relative to the counterfactual and this result is not significant across all matching methodologies. Changes in capital controls and macroprudential measures do not significantly affect aggregate portfolio flows and bond flows. There is some evidence, however, that increased controls on capital inflows can significantly reduce equity inflows—although this does not translate into a significant decline in net portfolio inflows or depreciation in the nominal or real exchange rate.

The results also show little evidence that changes in CFMs affect other macroeconomic variables and financial market financial market volatilities over the short- and medium-term. Increased inflow controls and decreased outflow controls have no significant effect on equity returns, inflation, interest-rate differentials versus the United States, or the volatility of exchange rates, portfolio flows, or interest-rate differentials. Increased macroprudential regulations also have no consistently significant effect on any of these variables, except that they may reduce interest rate differentials for short periods of time by a small amount and may actually increase—instead of decrease—the volatility of portfolio flows. By improving specific measures of financial fragility (such as bank leverage, inflation expectations, credit growth, and exposure to portfolio liabilities), however, some CFMs (and especially macroprudential measures) may indirectly affect these macroeconomic variables and financial volatilities over longer periods of time than measured in this paper.

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 may be a more effective tool for limiting exchange rate appreciation. Also, the debate on the use of capital controls (and to a lesser extent macroprudential 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 outflow controls on the exchange rate and inflow controls on equity flows), but more consistently significant effects on other forms of vulnerability—such as reducing bank leverage, inflation expectations, bank credit growth, private credit growth, and exposure to portfolio liabilities. Moreover, macroprudential regulations appear to be more effective than capital controls in improving most measures of financial stability, albeit with the important caveat that the distinction between these two categories is often indistinct (Ostry et al., 2011). Therefore, CFMs, and especially macroprudential measures, appear to be "good for" improving specific variables related to financial fragility, but do not appear to be as effective in influencing other targets.

The remainder of this paper is as follows. Section II describes the new dataset on changes in capital controls and macroprudential measures created for this paper. Section III discusses the propensity-score methodology in more detail. Section IV uses this methodology to estimate the probability that each country adjusts its CFMs and then uses these estimates to calculate propensity scores and match each "treatment" with its control group using the five matching algorithms. This section also evaluates the performance of each algorithm. Section V uses the matched groups to estimate if there was any significant effect of the CFMs on key outcome variables. Section VI concludes.

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 group, 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. We supplement this source with information from financial analyst reports, primary news sources, and academic papers on capital controls and macroprudential measures. We include any changes in capital controls or macroprudential 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 macroprudential measures. These classifications require some discretionary judgment, especially as the distinction between capital controls and macroprudential measures is often not clear in practice.

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 that affect the transfers and payments associated with these transactions. Typical measures include taxes on cross-border flows from residents/non-residents, unremunerated reserve requirements (URR) on such flows, special licensing requirements and even outright limits or bans on international transactions. Capital controls may apply to all financial flows, or may differentiate by the type or duration of the flow (i.e., debt, equity, or direct investment; short-term vs. longer-term). Macroprudential measures are defined as regulations focused on strengthening the ability of the domestic financial sector to cope with risks 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 macroprudential 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. Appendix A provides additional information on the construction of this database on changes in CFMs.

After constructing this database of changes in controls on capital inflows, controls on capital outflows, and macroprudential measures, we then further differentiate these CFMs by several criteria. We label each change in a 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 (FX). In some cases, a CFM may simultaneously affect more than one of these categories.[4] CFMs affecting equities, bonds, and FDI are more often capital controls, and CFMs affecting banks, foreign exchange, and loans are more often macroprudential 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 Section V. B.)

The resulting database includes 220 CFM events in which there is a change in controls on capital inflows or outflows or macroprudential 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 macroprudential 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 macroprudential 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 macroprudential measures (121 events) than capital controls (99 events)—with the changes in capital controls evenly balanced between changes in controls on inflows (50 events) and outflows (49 events). The CFM events are also more heavily weighted toward policies 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 CFM events into two groups—those responding to pressures related to large and volatile net capital inflows and those related to net 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 macroprudential 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 macroprudential regulations. Figure 1 and the right-hand columns of Table 1 show the incidence of changes in these groups of CFMs by quarter from 2009 through 2011. The sample is weighted more heavily towards CFMs aimed at moderating capital inflows (with 135 events compared to 85 with the

[4] 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.

opposite goal). This is not surprising as most of the period from 2009 through 2011 was a period of declining risk aversion, expansionary monetary policy in developed countries, and increasing global capital flows. In the analysis which follows, we focus on the impact of this larger group of CFM events aimed at moderating capital inflows and which have received the greatest attention by policymakers.[5]

This dataset on CFM events is then merged with information from several different sources to create the final database for this paper. Weekly market information on global risk (the VIX and TED spread), commodity prices, interest rates (on 3-month treasuries), and equity market indices is taken from Haver, Datastream, and Global Financial Data. Weekly capital flow information on equity and bond flows and asset positions is taken from the Emerging Portfolio Fund Research (EPFR) 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 is the *de facto* arrangement as measured in 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. Detailed information on this data, including sources and definitions, is in Appendix B.

III. Propensity-Score Methodology

In order to estimate the impact of the CFMs identified in the database discussed above, we use a propensity-score matching methodology to construct the counterfactual of what would have happened in different countries if they had not used CFMs. This section discusses the methodology and the next two sections apply the technique.

Countries that adjust their capital controls and/or macroprudential measures are often different from countries which do not make these policy choices. This "selection bias" or "non-random assignment" makes it difficult to assess whether differences in key outcome variables between countries that undertake these policies and those that do not are driven by the policies or simply by underlying differences in the two sets of countries. One approach for adjusting for this selection bias is to include variables in a

[5] We have also extended the analysis to other CFM measures related to concerns about capital outflows, currency depreciations, and related pressures (i.e., lifting controls on capital inflows and macroprudential measures and increasing controls on capital outflows), but the more limited set of observations and global economic trends during the sample limit the analysis.

multivariate regression framework (with or without instrumental variables) that control for differences between countries that do and do not make these policy choices. An alternative approach, which has several important econometric advantages for this paper's analysis, is to use a propensity-score methodology.[6]

To use the propensity-score methodology, define a "treated" observation as $D_i = 1$, which is any week in which country i changes a CFM. Likewise $D_i = 0$ are the "untreated" or "control" weeks, which are any weeks when country i 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.[7] 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. Also define $Y_{1,i}$ as the outcome variable (such as changes in the exchange rate) for the i^{th} member of the treated group and $Y_{0,i}$ for the i^{th} member of the untreated (control) group. Summing over members of each group, we are able to observe $E[Y_{1,i}|D_i=1]$ and $E[Y_{0,i}|D_i=0]$. The variable in which we are interested, however, is the "average treatment effect on the treated" or ATT, which is not observable and is written as:

$$\text{ATT} = E[Y_{1,i} - Y_{0,i}|D_i=1]. \tag{1}$$

The difference in the two observable statistics is a combination of the key variable of interest (the ATT) and sampling bias ($E[Y_{0,i}|D_i=1] - E[Y_{0,i}|D_i=0]$):

$$\begin{aligned} E[Y_{1,i}|D_i=1] - E[Y_{0,i}|D_i=0] &= E[Y_{1,i} - Y_{0,i}|D_i=1] + E[Y_{0,i}|D_i=1] - E[Y_{0,i}|D_i=0] \\ &= \text{ATT} + \text{sampling bias}. \end{aligned} \tag{2}$$

The sampling bias is the difference in outcomes that is attributable to differences in the treated and control group (such as different country characteristics) rather than any effect of the treatment itself.

[6] See Angrist and Pischke (2008, chapter 3) for an excellent summary of this methodology and examples from the labor literature. Also see Heinrich, Maffioli, and Vazquez (2010) for an overview. Four papers that have used this methodology in the international/macroeconomic literature are: Ehrmann and Fratzscher (2006) for the effect of monetary policy shocks on firms, Forbes and Klein (2013) on how different crisis responses affect growth and employment, and Glick, Guo, and Hutchison (2006) and Das and Bergstrom (2012) on the link between openness and currency crises or growth, respectively.

[7] This exclusion window prevents labelling countries which have recently changed or are about to change 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 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. 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.

To take a simple but concrete example, consider how an increase in macroprudential regulations (the treatment, D_i) affects the real exchange rate (Y_i). Using our dataset described in Section II, the mean percent change in the real exchange rate over the next six months after an increase in macroprudential measures is 1.10% for the treated sample and 0.78% for the untreated sample. This could be interpreted as indicating that increasing macroprudential regulations causes an appreciation of the real exchange rate relative to what otherwise would have occurred (ignoring any test for significance). A closer look at the sample of treated and untreated observations, however, indicates that countries that increase their macroprudential regulations over the sample period are also more likely to have a floating exchange rate.[8] More specifically, 80% of the treated group has a floating exchange rate, as compared to just 39% of the control group. The different patterns of exchange rate appreciation between the treatment and control groups may therefore result from different exchange rate regimes in the two groups (selection bias) rather than any effect of changes in macroprudential regulations (the ATT), as specified in equation (2).

Continuing this simple example, an additional calculation shows that selection bias is, in fact, overwhelming any ATT from increasing macroprudential regulations. We limit the sample to only include countries with floating exchange rates in order to remove any selection bias resulting from this variable and repeat the same calculations as above. The percent change in the real exchange rate over the next six months after an increase in macroprudential regulations is now 0.39% for the treated sample and 0.84% for the untreated sample. Countries that increased their macroprudential regulations now appear to have less exchange rate appreciation than countries which did not increase regulations—reversing the earlier pattern. This is obviously not a formal test and ignores many other forms of selection bias and other factors that will be considered in the full analysis below. But it does provide a simple example of how selection bias resulting from differences between the treated and control groups can bias estimates of policy effects.

Any sampling bias between the treated and control groups would be straightforward to adjust for if countries differed along only one or two discrete dimensions. If the set of countries could be readily apportioned to a small number of "cells" reflecting any differences along these dimensions, and there were enough instances of both treated and control cases in each cell, it would be simple to calculate the differences between the treated and the untreated observations in each cell, and take a weighted average of those differences in order to estimate the effect of different policy changes. For example, as shown in the simple example above, if the only difference between countries which did and did not adjust

[8] Measured using the *de facto* floating exchange rate regime dummy from Ghosh et al. (2011).

macroprudential regulations was whether they had a floating exchange rate, it would be straightforward to divide the sample into treatment and control groups based on their exchange rate regime and then calculate the ATT based on differences in outcomes for treated and untreated countries with the same exchange rate regime.

In practice, however, the differences across countries are manifold and multidimensional, and it is impossible to match two countries which share identical macroeconomic characteristics. Propensity-score matching offers a means to address this challenge. This methodology matches countries that undertake the treatments (i.e., policy changes) to a subset of countries that do not, based on a set of observable country characteristics and global variables, represented by the vector X_i for the i^{th} country. This matching methodology controls for differences in the treated and untreated groups that affect outcomes, such that the sampling bias is removed (at least any bias that is captured in the vector X_i). In other words, the key underlying assumption is:

$$E[Y_{0,i}|X_i, D_i{=}1] = E[Y_{0,i}|X_i, D_i{=}0]. \tag{3}$$

This basically requires that conditional on the vector of observable characteristics, the outcome variable is independent of the treatment status, (i.e., $Y_0, Y_1 \perp D_i \mid X$). If this assumption is satisfied, then the ATT can be estimated using two observable terms:

$$ATT = E[Y_{1,i}|D_i{=}1, X_i\,] \; - \; E[Y_{0,i}|D_i{=}0, X_i]\,. \tag{4}$$

This still leaves a multidimensional problem, due to the large set of continuous variables that could be included in X_i. Rosenbaum and Rubin (1985), however, show that it is sufficient to match treated and control observations based on a "propensity score," $P(X_i)$, which is a scalar variable that is the probability that country i receives the treatment (D_i). More specifically, the propensity score, $p(X_i)$ is:

$$p(X_i) = Pr[D_i{=}1|X_i]\,. \tag{5}$$

In our case, the propensity score is the conditional probability of a country adjusting its CFMs given pre-treatment characteristics, X_i, which include country-specific and global variables. This single propensity score reduces the number of dimensions over which observations must be matched. Rubin and Thomas (1992) show that it is possible to estimate these propensity scores based on the vector of observable characteristics. These propensity scores $p(X_i)$ are traditionally estimated using a logit or probit regression.

After the propensity scores have been estimated, there are several algorithms that can be used to match each treated observation with one or more untreated observations (i.e., controls). We focus on five matching algorithms: nearest-neighbor without replacement, five-nearest neighbors with replacement, radius with caliper, kernel, and local linear. Each of the matching algorithms has advantages and disadvantages. There are several test statistics that can be used to assess the accuracy of the matching and whether the algorithm removes any significant differences between the treated and control groups. These tests and statistics are discussed in more detail in Section IV.B. when they are used. If these tests are satisfied, it is then possible to estimate the ATT as:

$$\text{ATT} = \text{E}[Y_{1,i}|D_i\!=\!1\ , p(X_i)]\ -\ \text{E}[Y_{0,i}|D_i\!=\!0, p(X_i)]\ . \tag{6}$$

To conclude, it is useful to mention how this approach compares to the more familiar regression analysis. Multivariate regressions estimate the partial correlation of the treatment with the outcome variable, and can control for the other variables included in the vector X_i. Multivariate regression can also be combined with instrumental variables—although finding "good instruments" that meet the exclusion restrictions is often challenging. One important advantage of propensity-score matching over multivariate regression analysis, however, is that matching does not require assumptions about a linear relationship between the treatments, covariates, and outcomes.

Another important difference between the standard regression approach and the propensity-score methodology is the weighting of the covariate-specific differences between the treated and untreated observations.[9] In both approaches, it is necessary to construct weights for the difference between treated and untreated values across different cells in order to calculate the average effect for the whole sample. In propensity-score methodology, the weights are based on the distribution of covariates among the treated, with the greatest weights put on cells representing the highest likelihood of being treated (i.e., the observations most like the treated observations). In contrast, in regression analysis, the greatest weights are placed on cells where the conditional variance of treatment status is larger (i.e., basically those cells with equal likelihood of its elements being treated or untreated). These two different weighting

[9] Angrist and Pischke (2008, Chapter 3) provide an excellent discussion of the similarities and differences between regression analysis and propensity-score matching approach. Also, although propensity-score matching can reduce asymptotic efficiency relative to a regression framework, Angrist and Hahn (2004) show that there can be efficiency gain in a finite sample, even if there is no asymptotic efficiency gains from the use of propensity-scoring estimators. Given the small size of our sample—this suggests that this potential drawback of the propensity-score methodology is less likely to be an issue.

approaches can significantly affect the estimated average treatment effects if the differences between the treated and the untreated observations vary across cells.

To provide a concrete example, reconsider the example of how selection bias resulting from a country's exchange rate regime (one of the variables in X_i) can affect estimates of how changes in macroprudential regulations affect the real exchange rate. A simple regression of the percent change in the real exchange rate over the next quarter on a dummy variable measuring if the country increases prudential regulations (D_i) yields a positive and significant coefficient of 0.014 with a standard error of 0.005. This suggests a positive correlation between increasing prudential regulations and real exchange rate appreciation when not controlling for selection bias and other variables. Adding an additional dummy variable equal to one if the country has a flexible exchange rate has no significant effect on the estimated coefficient on macroprudential regulations and the exchange rate dummy is not significant. Moreover, if the same regression is repeated for only countries with a floating exchange rate, the coefficient on macroprudential regulations falls to 0.009 and becomes insignificant (with a standard error of 0.006). This suggests that when constructing a counterfactual that places more weight on more similar countries—when similar is measured by having a floating exchange rate regime—the results may change significantly from when the counterfactual weights all countries equally—even if controlling for the exchange rate regime. Put slightly differently, when estimating the effects of increasing macroprudential measures on outcome variables, a propensity-matching approach would put more weight on countries that are more similar (i.e., with a floating exchange rate in this example) than would occur in standard OLS regression. Different weighting schemes when constructing the counterfactual can lead to substantially different conclusions.

IV. Estimating Propensity Scores and Matching Algorithms

This section applies the propensity-score methodology discussed in the last section. It uses a vector of observable variables to estimate a logit model of each country's choice to implement each type of CFM during each week. Then it uses the resulting estimates to calculate propensity scores and match each "treatment" (i.e., each change in capital controls or prudential measures) with a control group. The next section uses these matched groups to estimate if there was any significant effect of the CFMs on the outcome variables of interest for the treated group relative to the matched control group.

A. First-Stage Logit Regressions and Propensity Scores

In order to calculate the propensity scores predicting the probability of a country changing its CFMs as specified in equation (5), we draw from the literature on the determinants of capital flows and capital controls.[10] Since our database on capital controls and macroprudential 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 and capital flows—which are primary motivations cited for adjusting CFMs—we control for percent changes in the country's real effective exchange rate and net portfolio inflows (over the last six months). Second, to control for increased inflation risk and credit growth—other reasons frequently cited as motivations for CFMs—we control for consensus CPI inflation forecasts (over the next year) and the percent change in private credit relative to GDP and

Third, to control for changes in global sentiment and 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 the TED spread), commodity prices (measured by percent changes in the Dow Jones commodity price index), and changes in the interest-rate differential between each country and the United States (on overnight rates).[11] Fourth, to control for different intervention strategies, exchange rate regimes, and past 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 the Chinn-Ito measure of the country's pre-existing capital account openness. Finally, to control for any effect of the size of a country's financial sector, income level, and institutional strength, we also control for stock market capitalization (as a share of GDP), GDP per capita, and the country's "legal compliance".[12] For each variable measured in changes, we calculate the change (or percent change) in the variable relative to the previous year 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.[13] All variables are defined in detail in Appendix B.[14]

[10] 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.
[11] 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.
[12] Lim et al. (2011) discuss the importance of the size of existing financial markets. Habermeier et al. (2011) discuss how institutional features, such as administrative capacity and legal compliance, could also have an effect on the design and enforcement of capital management techniques.
[13] 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. Forbes et al. (2012) find no evidence that markets reacted in advance to recent changes in Brazil's tax on capital inflows.
[14] We have also controlled for other variables—such as changes in equity indices, CPI inflation, and 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 the nominal exchange rate instead of real exchange rate and the spread on 3-month interest rates (instead of overnight). These changes also do not affect the key results, so we focus on measures that maximize the sample size.

Therefore, our base case regression used to explain changes in CFMs aimed at moderating capital inflows and related pressures from 2009 to 2011 can be written as:

$$Prob(CFM_{it} = 1) = F\left(\boldsymbol{\Phi}_{i,t-1}^{\text{Domestic}}\mathbf{B}_{\text{C}} + \boldsymbol{\Phi}_{t-1}^{\text{Global}}\mathbf{B}_{\text{G}}\right), \tag{7}$$

where CFM_{it} is an episode dummy variable that takes the value of 1 if country *i* changes its CFM (increases controls on capital inflows, decreases controls on capital outflows, or increases macroprudential measures) during week *t*; $\boldsymbol{\Phi}_{t-1}^{\text{Global}}$ is a vector of global variables lagged by one week (the VIX, Ted spread, commodity prices and the interest-rate spread); $\boldsymbol{\Phi}_{i,t-1}^{\text{Domestic}}$ is a vector of variables measuring domestic country characteristics. Domestic country characteristics include changes in key macroeconomic variables (the exchange rate, capital flows, inflation expectations, private credit, and reserves) and level variables measuring country characteristics which change less frequently (the exchange rate regime, capital account openness, financial market development, income per capita, and institutional strength). It is worth noting that these controls include variables capturing changes in policies other than CFMs that countries could select during this era of strong capital inflows, such as changes in exchange rates, interest rates, and reserve accumulation (i.e., other aspects of the trilemma). All control variables are lagged. The regression is estimated using a logit model with robust standard errors.[15]

The resulting estimates of equation (7) for our sample of 60 countries are reported in Table 2. Many of the variables expected to affect the probability that a country adopts a CFM to reduce capital inflows are significant and have the expected sign, although the covariates play differing roles for the three types of CFMs. Focusing on variables that are significant at the 5% level, countries are significantly more likely to increase inflow controls and decrease outflow controls if they have had greater real exchange rate appreciation. Countries are significantly more likely to increase macroprudential measures if they have had higher expected inflation, greater private credit growth, a floating exchange rate, and more open capital account. Countries are significantly more likely to remove outflow controls if they have a higher income level, larger financial market, and less open capital account (likely capturing that countries with an open capital account have a more limited ability to remove outflow controls). Countries with stronger legal compliance are significantly more likely to use macroprudential 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 insignificant in the base case).

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[15] We focus on a logit instead of probit model in order to "spread out" the density of scores at very low and high propensity scores.

We have also estimated several different variants of these regressions in order to assess any impact on the key results. First, we use a one-month (instead of three-month) exclusion window before and after a change in a CFM during which an observation cannot be used as a control group. This reduces the explanatory power of the regressions (as expected because countries which recently changed policies are now included as controls), but does not otherwise change the main results. Second, we repeated the base-case 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. Third, we try different combinations of the explanatory variables. For example, we avoid repetition of variables that are highly correlated (such as the TED and VIX). This can increase the significance of variables remaining in the regression, but does not improve the fit of the regression as measured by the Pseudo R^2 and can reduce the ability of the matching methodologies (explained below) to "match" as many observations.

Finally, we use a stepwise regression in order to more formally select variables in the regression. We begin with the estimates reported in Table 2, and then reestimate equation (7) with the 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 causes several variables that were not significant in the full specification to become significant with the more limited set of controls. More specifically, in regressions predicting increased controls on capital inflows, the coefficients on portfolio flows, CPI expectations, financial market size, and institutional strength become significant at the 5% level (with the same signs). In regressions predicting decreased controls on capital outflows, the coefficient on commodity prices becomes significant at the 5% level, and in regressions predicting increased macroprudential measures, the coefficient on the VIX becomes significant. These changes, however, do not significantly affect any of the key results on the matching methodologies and reduces the accuracy of the matching discussed below. Therefore, for our base case, we utilize the larger set of explanatory variables that are 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 five matching algorithms: nearest-neighbor without replacement, five-nearest neighbors with replacement, radius with caliper, kernel, and local-linear matching.[16] 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 observations 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 maximum radius (referred to as the caliper) based on the estimated propensity scores.[17] The kernel and local-linear matching algorithms calculate a weighted average of all observations in the control group using nonparametric estimators which use generalized weighting functions to assign a higher weight to control observations closer to the treated observation.[18] The nearest-neighbor algorithm is basically an extreme form of kernel and local-linear matching, with all the weight given to the closest propensity score.

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, however, ignores useful information from other countries in the control group. Radius, kernel and local-linear matching use more information and therefore tend to have lower variances—but at the risk of including bad matches. Radius matching is less sophisticated than kernel and local-linear matching as it does not place greater weight on better matches within its "radius". Fan (1992a, b) shows that local-linear matching has several important advantages over kernel matching, such as a faster rate of convergence near boundary points and greater robustness to different data design densities. In the following analysis, we begin with each of the five different matching approaches and then use several different tests to evaluate their performance and select the base case for the analysis. Including the different matching methodologies is also useful to test for the robustness of the results, especially as the significance of key results is often highly dependent on the construction of the control group.

Before performing these tests, however, it is useful to consider the "nearest neighbor" for several major treatment events in order to get an intuitive understanding of how countries are matched between the

[16] See Heinrich, Maffioli, and Vázquez (2010) for an excellent discussion of the different matching methodologies and tests to ascertain if the approach is valid.
[17] We set the caliper at 0.005.
[18] The main difference between the two methods is the weighting functions. See Heckman, Ichimura, and Todd (1997, 1998) for a detailed description of the local-linear matching method.

treated and control groups. For example, in a highly publicized example of a change in a CFM, Brazil increased its tax on bond inflows from 4% to 6% on October 19, 2010. This treatment is matched with Mexico in 2010 (week 20). South Africa is an example of a country which was actively reducing controls on capital outflows during this period. Its first major liberalization in the sample was in February 2009, which is matched with Malaysia (2009, week 21) and its last major liberalization in the sample was in December 2011, which was also matched with Malaysia (2011, week 50). A number of diverse countries increased their macroprudential measures, such as Brazil (2010, week 42) which is matched with the Philippines (2010, week 42), Peru (2011, week 1) which is matched with Argentina (2010, week 49), Indonesia (2011, week 4) which is matched with Turkey (2011, week 31), Korea (2010, week 1) which is matched with New Zealand (2010, week 3), and the Czech Republic (2009, week 44) which is matched with Poland (2009, week 31).These examples suggest that the countries identified as "nearest neighbors" generally make intuitive sense in terms of the control observation sharing similar country characteristics as the treated country and often occur around the same time as the treatment event.

Next, we use the additional four matching algorithms (in addition to nearest-neighbor) to create control groups for each of the treated observations. [19] Key statistics from this matching are reported in Table 3, for each of the three treatments. Each section first lists the mean propensity scores for the treated group, unmatched control group, and matched control group using the five algorithms. In most cases, the mean propensity score for the control group is closer to that of the treatment group after matching, indicating that the matched control group is more "similar" to the treatment group than the unmatched control group. According to this comparison, however, there is no matching algorithm that consistently performs best; local-linear matching yields a mean closest to the treated group for inflow controls, nearest neighbor is closest for macroprudential measures, and kernel is closest for outflow controls.

Table 3 also reports the mean absolute bias (and standard deviations) of the treated group relative to the unmatched control group and control groups using each of the matching algorithms. In each case, the matching reduces the mean absolute bias by a substantial amount, with different matching algorithms again performing better or worse based on the treatment. These statistics also capture the bias/efficiency trade-off inherent in selecting a matching technique. Methodologies such as nearest neighbor that only use one observation as the control group tend to have a lower mean absolute bias (as it only uses the most similar observation), but at the cost of ignoring other useful information and therefore having more

[19] We apply these matching algorithms with the Stata module PSMATCH2, developed by Leuven and Sianesi (2003). The number of treated observations is lower than reported in Table 1 because data is not available to estimate propensity scores for all observations.

imprecise estimates (and higher relative standard deviations). Methods that incorporate more observations in the control group (such as local-linear and kernel matching) should be more efficient as they incorporate more information, but at the cost of potentially have greater mean bias due to including poorer matches. There is no standard procedure to select a preferred matching algorithm based on this bias/efficiency tradeoff.

There are, however, two formal tests which can be used to assess if propensity-score matching is a valid approach. The first is the "Common Support Condition" (also known as the "overlap test"). This condition requires that for each set of global and country characteristics in X_i, there is a positive probability that a country-week observation is treated and untreated (i.e., that $0 < p(X_i) < 1$). This assumption is necessary as it requires that countries with the same values in X have a positive probability of being both treated and untreated. Countries are "on-support" if they meet this condition. The last row for each CFM in Table 3 reports the number of countries that are on support using each matching algorithm. All treated observations are on support for the nearest neighbor, five-nearest neighbors, and local-linear matching algorithms for each of the three CFMs. The algorithm which is the least accurate in terms of yielding more "off-support" observations is the radius methodology. The radius method generates 3 countries (out of 21) that are off-support for increased controls on capital inflows, one that is off-support (out of 59) for increased prudential measures, and two (out of 29) for decreased controls on capital outflows.[20] The kernel algorithm has one treated observation that is off support (for increased controls in capital inflows). In the analysis that follows, we only include observations that meet this common-support condition and drop 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 can be important, especially for radius, kernel and local-linear matching.

The other key test to assess if a matching methodology is valid is known as the "balancing" test or "independence assumption". The goal of this test is to verify that the matching was able to remove any significant differences between the treated and control groups that existed in the unmatched samples, i.e., that:

$$D \perp X \mid p(X). \qquad (8)$$

[20] Observations that are off-support using the radius method are: Brazil (2009, week 47), Vietnam (2011, week 34 and 2011, week 33) for controls on inflows; Ukraine (2009, week16) for macroprudential regulations; and South Africa (2010, week 7 and 2011, week 4) for controls on outflows. The observation that is off-support using the kernel methodology is Vietnam (2011, week 33) for controls on inflows.

Table 4 reports results of this test for increased controls on capital inflows. It begins by showing the mean values for the treated group (μ_T) and control group (μ_C) for the unmatched sample 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 treated group is equal to the mean in the control group (H0: $\mu_T = \mu_C$). There are significant differences between the treated and the unmatched control group for seven variables. Countries were significantly more likely to increase their controls on capital inflows if they had greater real exchange rate appreciation, higher expected inflation, less open capital accounts, less developed financial markets, lower income per capita, and lower levels of legal compliance (including the level and squared terms). These significant differences across the treated 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 4, however, indicates that each of the matching algorithms except the kernel methodology is able to remove this selection bias. The columns show mean values for each of the variables in *X* in the matched control groups using all five of the matching algorithms. It also reports the same *t*-statistics of tests for significant differences between the treated and matched control groups for each of the variables in *X*. In each of these tests, there are no longer significant differences between the treated and control groups for the nearest-neighbor, five-nearest neighbor, radius, and local-linear matching algorithms. Each of these four algorithms has successfully removed the significant differences across groups as measured by the variables in the vector *X*. In sharp contrast, after using the kernel algorithm, there are still significant differences between the treated and matched control samples based on five variables in *X*.

Results of this balancing test are similar for increases in macroprudential measures and decreases in controls on capital outflows. In each case, there are significant differences in the means of several variables between the treated and unmatched control groups, but after using each of the matching algorithms except the kernel methodology, there are no longer any significant differences between the variables in the treated and matched control groups. For example, in the model predicting increases in macroprudential measures, there are significant differences between the treated and unmatched control group for nine variables: credit growth, expected inflation, the VIX, commodity prices, the exchange rate dummy, GDP per capita, financial market size, legal compliance, and legal compliance squared. After using all four matching methodologies except the kernel algorithm, there are no longer any significant differences between the treated and matched control groups. After using the kernel algorithm, there are still significant differences between the treated and matched control group according to one variable in *X* at the 5% significance level and five variables at the 10% significance level. Therefore, all matching algorithms successfully meet the key test for balancing except for the kernel technique.

Based on this series of statistics and tests, and in order to simplify the discussion that follows and minimize the number of results reported, we will focus on results obtained using the local-linear matching algorithm as the "base case" and also show key results using five-nearest neighbors. We do not focus on kernel matching as it does not remove all significant differences between the treated and matched control groups. We do not focus on radius matching as it yields the greatest number of observations that are off-support. Of the three remaining methodologies, local-linear has the advantage of producing a mean propensity score for the matched sample that is the closest to that for the treated group for changes in controls on inflows and outflows. Local-linear matching also has the theoretical advantage that it uses all available information. Five-nearest neighbor matching also performs well, especially in terms of yielding low mean absolute bias (as shown in Table 3). For each significant result, however, we will discuss whether the finding is robust to all of the matching algorithms.

V. Impact of Capital Controls and Macroprudential Measures

In order to test for the impact of changes in capital controls and macroprudential measures, we compare outcome variables for the countries which used these policies (the treated observations) with their matched control groups. We use the matching algorithms developed in the last section to construct the counterfactual of what would have happened to each of the outcome variables if countries had not changed their CFMs. We focus on outcome variables that are frequently cited as goals for adjusting capital controls and macroprudential 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 fragilities (bank leverage, private credit growth, bank credit growth, inflation expectations, and exposure to short-term debt, portfolio liabilities and foreign-currency liabilities).[21]

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

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[21] In most cases, we estimate how changes in CFMs affect the growth rate 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.

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 (as well as for kernel 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. In each case, because the propensity scores are estimated, it is necessary to bootstrap the standard errors for the ATT in order to evaluate if there is a significant difference between the treated and control groups.[22]

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 policy change. For example, to estimate the ATT of new controls on capital inflows on the nominal exchange rate, we calculate the average percent change in the exchange rate for the treated and control groups. For the first treatment period, this would be the change from period 0 (the treatment date when the controls were increased) 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 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. Finally, we also winsorize all outcome variables at the 1% level in order to avoid having results driven by extreme outliers.

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 results for key variables targeted by capital controls and macroprudential measures—the exchange rate (nominal and real) and net portfolio inflows. Figure 2a uses local-linear matching and Figure 2b uses five-nearest neighbors. Each bar shows the magnitude of the estimated ATT for the accumulated time in weeks since the change in the CFM occurred (the treatment). The dark black shading indicates that the ATT for that week is significant at the 5% level, and the medium-blue shading

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

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 Figures 2a and 2b show that increased controls on capital inflows have no consistently significant effect on the real or nominal exchange rate relative to that for the control groups. The results based on local-linear matching (in Figure 2a) indicate that increasing controls on inflows may lead to a small deprecation of less than 0.5% over the first two months, increasing gradually to reach a maximum real deprecation of 2% at about 3 months. This effect is only significant at the 5% level for 3 weeks in the 6-month window. Moreover, the results based on five-nearest neighbor matching in Figure 2b indicate that the effect may instead initially be positive, and any longer term effect is insignificant and very small (peaking at only a 1.5% depreciation). These effects—even if they were significant—are very small relative to the normal volatility in exchange rates. Other matching methods also yield no consistent estimates.[23] These results suggest that increased controls on capital inflows do not have a significant or economically important effect on a country's exchange rate.

The second rows in Figures 2a and 2b show the ATT from removing controls on capital outflows. In this case, the effect is estimated to always be negative (a depreciation), and the magnitude is slightly larger. More specifically, removing controls on capital outflows causes a depreciation of the nominal and real exchange rate relative to the counterfactual which grows over time and peaks at about 2% after four months. This effect is more often significant—in the majority of weeks using local-linear matching and at the 10% level for over a month using five-nearest neighbor matching. Results based on radius matching are very similar to those based on local-linear matching (with slightly larger estimated effects).

The bottom rows of Figures 2a and 2b show the impact of increased macroprudential measures. In contrast to the other changes in CFMs, this policy appears to cause an immediate (but small) appreciation of the nominal and real exchange rates. This appreciation does not persist over time (with results mixed based on the matching methodology). Any effects are generally insignificant. Therefore, out of the three CFMs studied in this paper, removing controls on capital outflows appears to be the most likely to reduce any appreciation of the real exchange rate—although any effect appears to be small and have mixed significance.

[23] For example, nearest-neighbor matching indicates that increased inflow controls cause a small appreciation of the nominal and real exchange rates, while radius matching indicates a small depreciation, with estimated effects insignificant in most weeks.

The graphs in the right column of Figures 2a and 2b also show the effects of different CFMs on net portfolio inflows.[24] Increased controls on capital inflows cause net portfolio inflows to decline over time, with the effect gradually increasing to a maximum 3% decline in net inflows (relative to the counterfactual) after 5 months based on local-linear matching. This effect is significant at the 5% level for several weeks, but smaller and always insignificant at the 5% level based on five-nearest neighbor and radius matching (and in most weeks based on the other techniques). Decreased controls on capital outflows and increased macroprudential measures also appear to generate declines in net capital inflows over time, but these effects are even smaller in magnitude and insignificant in every week when any of the five matching algorithms is used.

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 country's interest-rate differential versus the United States (using rates on 3-month Treasury bills), equity index and inflation (measured by the CPI). The graphs use local-linear matching. (Results using five-nearest neighbor are similar and therefore not reported.) The graphs show that increasing controls on capital inflows does not have a significant effect on any of the macroeconomic variables in any week. Reducing controls on capital outflows and increasing prudential measures may reduce a country's interest rate differential relative to the United States, but the effect is small and usually only significant for one to four weeks (including using other matching algorithms). The effect of changes in capital controls and macroprudential measures on equity indices and inflation are insignificant at the 5% level in every week, and usually even at the 10% level. Therefore, there is little evidence that any of the CFMs can significantly affect equity returns or inflation. Reduced controls on outflows and increased prudential measures may have small effects on interest-rate differentials, but only for a short period.

Figure 4 shifts to evaluating the effect of CFMs on the volatility of key variables. Volatility is measured as the standard deviation over the previous 26 weeks and we estimate the volatility in the nominal exchange rate, net portfolio inflows, and interest-rate differentials, all defined as above. (Results on interest-rate volatilities are not reported, but show no consistently significant effects). The graphs show that increased controls on inflows are more likely to reduce volatility, and decreased controls on outflows are more likely to increase volatility. Macroprudential measures may decrease exchange rate volatility, but increase capital flow volatility. Once again, however, most of these estimated effects are insignificant

[24] Net portfolio inflows are calculated as cumulative flows over the last quarter (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.

or short-lived. The exception is that increased macroprudential measures significantly increase the volatility in net portfolio flows after 3 months. This result is also the one result that is consistently significant across matching methodologies. The right-hand column repeats the two analyses that showed any significant effects based on local-linear matching, except now uses five-nearest neighbor matching. It shows the lack of robustness of any effect of increased inflow controls on exchange rate volatility (which also occurs with other matching methods) and the more consistent estimates of the effect of increased macroprudential controls on capital flow volatility (which is also robust to other matching methods). Therefore, there is little evidence that adjusting capital controls or increasing macroprudential measures can significantly reduce the volatility of key financial variables, and increasing macroprudential measures may instead increase the volatility in portfolio flows.

Finally, a goal of capital controls, and especially macroprudential measures, which has recently received more attention is to improve specific measures of financial vulnerability. Measuring financial fragilities at a high frequency for our diverse sample of countries is not straightforward, so we focus on several available measures that capture different forms of potential vulnerability and that are frequently included in early-warning models: the growth in private credit (relative to GDP), the growth in bank credit (relative to GDP), inflation expectations, bank leverage (measured as bank credit to deposits), and exposure to short-term external debt (relative to GDP), portfolio liabilities (as a share of total liabilities), and foreign-currency liabilities (as a share of total liabilities).[25] Different CFMs do not significantly affect all of these variables, but in contrast to the previous results, certain CFMs do have significant and robust effects on many of these key measures of financial fragility. To simplify the discussion of these results, Figure 5 only shows graphs when increased inflow controls, decreased outflow controls, and increased macroprudential measures significantly affect a measure of financial fragility for at least four weeks using local-linear matching. In order to assess the robustness of these significant results, it also repeats the same tests using five-nearest neighbor and radius matching.

Figure 5a shows that increased controls on capital inflows and macroprudential measures can both lead to significant reductions in bank leverage relative to the counterfactual. Data on bank leverage is limited, however, and the results based on five-nearest neighbors are not significant and have very limited observations, so these results should be interpreted with caution.[26] A more robust result is the significant decline in expected inflation that occurs after increases in macroprudential regulations. This result is

[25] All measured as percent changes.
[26] The short time horizon for the ATT based on fiver-nearest neighbor matching reflects the limited data available for the five-nearest neighbors identified in the first stage.

significant from about three months to the end of the sample period for each matching method. The magnitude is also economically significant. An increase in macroprudential regulations is correlated with a decline in expected inflation of about 0.4% over six months.

Figure 5b shows that increased controls on inflows leads to a significant reduction in private credit over a period from about 3 months to near the end of the 6-month window. This effect is significant across matching methods. Increased controls on inflows and increased macroprudential measures may also both generate a significant decline in bank credit, although this effect is not significant for inflow controls across different matching methods. Figure 5c shows that increased macroprudential measures can decrease a countries exposure to portfolio liabilities and controls on outflows may reduce exposure to short-term debt—with the significance of each of these results fluctuating based on the time horizon and matching methodology. When taken as a whole, this series of results indicates that CFMs appear to be most effective in reducing different measures of financial vulnerability. Macroprudential measures appear to be especially potent as they show evidence of being able to significantly reduce bank leverage, inflation expectations, bank credit growth, and exposure to portfolio liabilities.

B. Results: More Narrowly Defined CFMs

The last section provided evidence that although capital controls and macroprudential measures can significantly improve specific measures of financial vulnerability, they do not appear to have significant and robust effects on other key targets. The measures of capital controls and macroprudential 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 events include changes in restrictions on equity flows, bond flows, and FDI. The macroprudential treatments include changes in rules affecting banks and foreign exchange. Different types of capital controls and macroprudential measures may be more effective at targeting certain variables. Changes in CFMs that receive more widespread attention by financial analysts may also have greater effects if CFMs work largely through a signaling effect (as suggested in Forbes et al., 2012). Therefore, this section uses narrower definitions of capital controls and macroprudential measures 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 targeting equity and bond flows, of "major" controls (that received more attention by financial analysts), and of macroprudential regulations directed at banks or foreign exchange.

To begin, we more narrowly classify the capital controls in our database as targeting bond flows, equity flows, and/or FDI (with some events targeting more than one type of flow). Of the 63 events that involve increased inflow controls or decreased outflow controls, more changes target bonds (45 events) and equities (33 events) compared to FDI (only 13 events). Then we estimate the effect of changes in controls targeting bonds or targeting equities on exchange rates (nominal and real), net portfolio inflows, net bond inflows, and net equity inflows. There are no consistently significant effects on real or nominal exchange rates, net portfolio inflows, or net bond inflows from any of these changes in equity or bond controls. There are also no consistently significant effects of lifting controls on bond or equity outflows on any measures of exchange rates or net capital flows. In contrast, however, increasing controls on capital inflows appears to have a significant effect on equity flows—even if the controls target just bonds.

Figure 6 highlights the key results. The first row shows the effect of increased controls on bond inflows, the middle row the effect of increased controls on equity inflows, and the bottom row the effects of increased controls on total portfolio (the sum of equity and bond) flows. Changes in controls in capital inflows—whether targeting bond flows, equity flows, or both, have a greater negative effect on equity than bond flows. 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.[27] Moreover, the effect on equity flows can be large (reaching about a 4% decline), is often significant after two months, and is robust across most matching methods. This suggests that controls on capital inflows are more effective at reducing equity than bond flows—even if the controls target primarily bond flows.

Next, we more narrowly define the capital controls and macroprudential 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. CFMs that received more attention may have a larger effect on outcome variables if CFMs work at least partially through a signaling effect—although it is also possible that larger changes in CFMS generate more attention and therefore have a larger effect. To create this definition of "major" controls, 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.[28] 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

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[27] This result is also consistent with Bartolini and Drazen (1997), which models how changes in capital controls can affect capital flows not directly affected by the policy change when investors have imperfect information so that the change in policy is interpreted as providing a signal about future government policy toward capital mobility.

[28] 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.

fairly minor changes or occurred in smaller countries that did not receive substantial attention by investors.

Most of the estimated ATT's of "major" CFM events are similar to the key results reported above for the larger sample of CFM events. The only noteworthy exceptions are reported in Figure 7. The top row shows the effects of "major" increases in inflow controls and the second row shows effects of "major" decreases in outflow controls. These "major" changes in capital controls have a larger and consistently significant negative effect on net portfolio inflows than found for the larger sample of events. These large and significant estimates have mixed robustness across matching methods. The middle column, however, indicates that the reduction in net portfolio flows may come at a cost. "Major" changes in controls now generate increased volatility in capital inflows—and this effect is significant, robust, and much larger for increased inflow controls. It is also worth noting that the larger and consistently significant reduction in net portfolio inflows from "major" controls on portfolio inflows appears to translate into a greater depreciation of the real exchange rate for reductions in outflow controls, but not for increases in inflow controls—perhaps related to the volatility results. This supports the prior evidence that removing controls on capital outflows is more likely to cause a depreciation of the real exchange rate (relative to the counterfactual) than increased controls on inflows.

As a final extension of the base analysis, we classify the changes in macroprudential measures in our database as measures targeting banks and those targeting foreign exchange exposures. This classification shows that of the 72 increases in macroprudential 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 of banks). Then we test if these narrower types of macroprudential measures have different effects on key outcome variables than found for the broader sample of treatments. The key results reported above generally do not change significantly with these finer measures of macroprudential measures. The only noteworthy differences are that increased regulations targeting banks tend to more often have significant negative effects on private credit growth and exposure to short-term debt than regulations targeting foreign exchange—although robustness varies across matching methodologies.

C. Results: Tying it All Together

To summarize, this section reports estimates of the effects of capital controls and macroprudential regulations on a series of outcome variables by week over 6-month windows. The results indicate that CFMs can have significant effects on some of the variables which they are intended to influence, but in most cases the effects are insignificant, short-lived, small in magnitude, and not robust across different matching methodologies. The strongest and fairly robust effects of CFMs appear to be in reducing certain measures of financial vulnerability. Increased macroprudential measures significantly reduce bank leverage, inflation expectations, bank credit growth, and exposure to portfolio liabilities. Increased controls on capital inflows reduce private credit growth and possibly bank leverage.

In contrast to these robust results showing that CFMs can reduce financial fragilities, there is little consistent evidence that they can affect two other primary goals: exchange rate appreciation and net capital inflows. Removing controls on capital outflows causes a depreciation of the real exchange rate of just over 2% over about 4 months, and this result is only significant for some of the matching methodologies. Increased capital inflow controls and macroprudential measures have even smaller and generally insignificant effects on exchange rates (nominal and real) and capital flows. A closer look at different types of portfolio flows, however, suggests that changes in capital controls have the greatest effects on equity flows and insignificant effects on bond flows (even if the controls primarily target bond flows). For example, increased controls on capital inflows can cause a reduction in equity flows after about two months. This effect is moderate (reaching a 4% decline after 3 months), often significant, and fairly robust across matching methodologies. “Major” changes in capital controls which received more attention from investors are also more likely to affect portfolio inflows, although they can also cause a significant increase in capital flow volatility and translate into little consistent, significant, or economically meaningful impact on the real exchange rate (especially for increased controls on capital inflows).

Finally, the estimates show little evidence that changes in CFMs affect other macroeconomic variables and financial market volatilities over the short- and medium-term. Increased inflow controls and decreased outflow controls have no significant effect on equity returns, inflation, interest-rate differentials versus the United States, or the volatility of exchange rates, portfolio flows, or interest-rate differentials. Increased macroprudential regulations also generally have no consistently significant effect on any of these variables, except that they show some evidence of reducing interest rate differentials for short periods of time by a small amount and may actually increase—instead of decrease—the volatility of portfolio flows. Any indirect effects of CFMs on these macroeconomic and volatility measures due to any effects of the CFMs on specific measures of financial vulnerability, however, would not be captured in this analysis if they occurred after the one-year time window.

VI. Conclusions

An extensive literature has attempted to assess the impact of capital controls and, to a lesser extent, macroprudential measures. Two challenges for this literature are selection bias and endogeneity; countries which change their capital controls and macroprudential measures are different than countries which do not change their CFMs. Countries adjust these policies in response to changes in key macroeconomic variables, which are often the targets of the controls and macroprudential measures. This paper shows that these challenges are not just hypothetical and should be addressed when estimating the effect of CFMs. In order to do so, it uses a propensity-score methodology. This technique uses several matching algorithms to create control groups establishing the counterfactual for key outcome variables in the absence of changes in CFMs. This is the first attempt (to the best of our knowledge) to use this methodology to analyze the impact of capital controls and macroprudential measures.

In order to perform this analysis, the paper begins by constructing a new database which includes detailed information on changes in capital controls and macroprudential measures for a large sample of economies from 2009 through 2011. The analysis then estimates propensity scores predicting the probability that each country adopts a specific capital control or macroprudential measure in each week based on a set or domestic and global variables. It uses these propensity scores to match each policy change (i.e., treatment) with a control group in order to create a counterfactual against which to assess the effect of the policy change on key outcome variables. The analysis focuses on the impact of CFMs aimed at reducing pressures from capital inflows and appreciation—increased controls on capital inflows, decreased controls on capital outflows, and increased macroprudential regulations.

The results indicate that certain 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, macroprudential measures can significantly improve specific measures linked to financial fragility, such as bank leverage, inflation expectations, bank credit growth, and exposure to portfolio liabilities. Increased controls on capital inflows can reduce private credit growth and possibly bank leverage. CFMs do not appear to have a significant effect on most other macroeconomic variables and financial market volatilities over the short and medium-term, however, including on equity indices, inflation, interest-rate differentials, or on the volatility of exchange rates, portfolio flows, or interest rate differentials. CFMs have limited 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 depreciation of the real exchange rate (with a maximum depreciation of less than 2.5% over four months relative to the counterfactual). Increased capital controls can significantly reduce net equity inflows (although not other types of flows), but this effect does not translate into a significant change in the real or nominal exchange rate or reduction in net portfolio inflows.

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 the country) would be a more effective tool for limiting exchange rate appreciation. This supports recent evidence in Pasricha (2012) and Aizenman and Pasricha (2013) which shows that the liberalization in capital outflows that occurred in the 2000s was largely a response to surging capital inflows. This also supports the estimates in Bayoumi and Ohnsorge (2013) that the effects of capital account liberalization in China on net capital flows and exchange rates would primarily occur through changes in capital outflows by Chinese residents rather than capital inflows by foreigners.

Finally, the debate on the use of capital controls (and to a lesser extent macroprudential 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 vulnerability (i.e., Ostry et al., 2012). The evidence suggests that this new focus is likely to be more productive and effective. Capital controls and macroprudential measures have no consistently significant effect on exchange rates (except mixed evidence from removing controls on outflows), capital flows (except equity flows), and other macroeconomic variables over the short- and medium-term. In contrast, controls and macroprudential measures appear to be more effective at reducing other forms of financial fragility—such as reducing bank leverage, inflation expectations, private credit, bank credit and exposure to portfolio liabilities. Over longer time periods, these reductions in specific measures of financial fragility could indirectly improve other macroeconomic measures. Policymakers evaluating whether to use different forms of capital controls and macroprudential 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 for the years 2010−2012. 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 by Goldman Sachs, HSBC, Institute of International Finance, JP Morgan Chase, and Morgan Stanley that include information on capital flow policies or country 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 "macroprudential measures" are listed in the table below.

Types of Capital Flow Management Techniques

Capital controls	Macroprudential 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 • Taxes 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 and/or related to 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 macroprudential regulations that are not related to foreign exchange and do not differentially affect foreigners—such as increases in reserve ratios that affect 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 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 as increased macroprudential 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 macroprudential 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.[29]

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), 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).[30] 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

[29] 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 macroprudential regulation is not included in the dataset.

[30] 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 credit	Private credit by deposit money banks and other financial institutions as a share of GDP; annual; Source: World Bank, Global Financial Development Database
Bank leverage	Bank credit to bank deposits; annual (only available through 2010); Source: World Bank, Global Financial Development Database
Capital account openness	Measure constructed from the International Monetary Fund's AREARs data with a higher value indicating greater openness; annual; Source: Chinn and Ito (2008), updated as of 04/24/2013 and available at: http://web.pdx.edu/~ito/Chinn-Ito_website.htm
Commodity price index	Dow Jones AIG commodity index, closing price; weekly; Source: Global Financial Data
CPI inflation	Consumer price inflation; monthly; Source: International Monetary Fund, International Financial Statistics
CPI inflation forecasts	52 week forward consensus expectations of CPI inflation; monthly; Source: IPA calculations
Equity index	Index based on a broad market measure using end-of-week prices; weekly; Source: Datastream
Floating exchange rate regime dummy	A 0-1 dummy for a floating exchange rate using the *de facto* exchange rate regime (classified as a peg, intermediate, or floating; annual; Source: Ghosh et al. (2011)
Foreign exchange liabilities as a share of total liabilities	Foreign currency denominated liabilities as a share of total liabilities; quarterly; Source: Haver
Foreign exchange reserves as a share of GDP	Monthly; Source: International Fund and IPA calculations, data for Taiwan from http://www.cbc.gov.tw/ct.asp?xItem=29908&ctNode=859&mp=2
GDP per capita	In nominal US$; monthly; Source: International Monetary Fund's WEO database
Interest rate differential between domestic interest rate and U.S. interest rate	Based on overnight interest rates; weekly; Source: Datastream and IPA calculations
Legal compliance index	Index ranging from 0 (no legal compliance) to 12 (high legal compliance); the 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 and the Methodology to create this index is at: http://www.prsgroup.com/ICRG_Methodology.aspx; monthly; Source: ICRG databases
Nominal effective exchange rate	Broad index with higher values indicating an appreciation of the domestic currency; weekly; Source: Haver and JPMorgan
Portfolio bond flows	Net portfolio bond inflows accumulated over past 13 weeks and usually expressed as a % of bond assets at the

	start of period in US$; weekly; Source: EPFR
Portfolio equity flows	Net portfolio equity inflows accumulated over past 13 weeks and usually expressed as a % of bond assets at the start of period in US$; weekly; Source: EPFR
Portfolio flows	Sum of net portfolio bond and portfolio equity inflows; accumulated over past 13 weeks and usually expressed as a % of bond assets at the start of period in US$; weekly; Source: EPFR
Portfolio liabilities as a share of total liabilities	Portfolio investment liabilities in all sectors as a share of total liabilities (gross inflows); quarterly; Source: Haver, based on the International Monetary Fund's BOP
Private credit	Expressed in local currency or as a share of GDP; monthly; Source: International Monetary Fund and IPA calculations
Real effective exchange rate	Broad real exchange rate index average for 44 countries with higher values indicating an appreciation of the domestic currency; monthly; Source: BIS, and if not available, then from the International Monetary Fund; series is constructed for Vietnam.
Short-term external debt to GDP	In millions of US$; annual; World Bank, Global Financial Development Database
Stock market capitalization to GDP	Annual; Source: World Bank, Global Financial Development Database
TED spread	Difference between the 3-month LIBOR and 3-month Treasury Bill yield, closing value; weekly; Source: Global Financial Data
VIX	Measure of market volatility; weekly; Source: Haver

Table 1

Capital Flow Measures

	Controls on Inflows		Controls on Outflows		Macroprudential 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 macroprudential 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)	Macroprudential Measures (Increases)
Real exchange rate	11.222***	6.006**	1.317
(% change)	(3.045)	(2.679)	(1.937)
Portfolio flows over last 6 months	0.001	0.004	0.000
(% change)	(0.001)	(0.004)	(0.001)
Consensus CPI,	0.207*	-0.148	0.337***
52-week expectations	(0.123)	(0.098)	(0.067)
Private credit to GDP	0.652	1.157	4.501**
(% change)	(2.904)	(2.776)	(1.778)
VIX	0.052	-0.032	-0.045
	(0.046)	(0.047)	(0.028)
TED Spread	-2.381	1.077	-0.646
	(1.693)	(1.744)	(0.972)
Commodity prices (% change)	-0.334	-2.536*	0.217
	(1.778)	(1.343)	(0.832)
Interest rate vs. US	-0.037	-0.031	0.042
(change in overnight rate)	(0.143)	(0.069)	(0.055)
FX Reserves/GDP	-0.663	-0.846	-0.817
(% change)	(0.798)	(0.773)	(0.731)
Floating ER dummy	-0.349	0.488	1.615***
	(0.535)	(0.572)	(0.367)
Capital account openness	-0.097	-1.008***	0.579***
	(0.369)	(0.242)	(0.149)
Stock market capitalization	-0.012*	0.006**	-0.000
(% of GDP)	(0.006)	(0.003)	(0.001)
Log GDP per capita	0.224	0.802**	0.052
	(0.398)	(0.354)	(0.225)
Legal compliance	-17.397	105.058**	79.502***
	(21.175)	(42.824)	(24.894)
Legal compliance squared	3.100	-25.638**	-18.826***
	(5.031)	(10.254)	(5.837)
Observations	***4,953***	***4,708***	***4,394***
Pseudo R^2	***0.192***	***0.222***	***0.155***

Notes: Results of a logit regression predicting the probability of a change in the CFM listed at the top in 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 Appendix B. Robust standard errors. Changes are calculated over 52 weeks in order to adjust for any seasonal effects. Constant is included in regression and not reported above.* indicates significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 3

Summary of Results for Different Matching Algorithms

	Treatment Group	Unmatched Control Group	Matched Control Group Based on Matching Algorithm: Nearest Neighbor (no replacement)	5 Nearest Neighbors	Radius with Caliper	Kernel	Local-linear
Increased controls on capital inflows							
Mean propensity score	420.6	498.7	499.3	446.8	491.4	466.8	445.3
Mean absolute bias		47.32	9.93	8.21	10.17	36.89	12.94
(standard deviation)		(36.68)	(7.96)	(6.95)	(9.53)	(27.11)	(10.35)
Observations on support	21	4932	21	21	18	20	21
Increased macroprudential measures							
Mean propensity score	548.1	476.7	548.7	564.5	566.6	521.3	588.3
Mean absolute bias		33.15	8.15	5.42	3.23	18.27	8.01
(standard deviation)		(21.13)	(6.01)	(3.73)	(1.98)	(11.96)	(5.37)
Observations on support	59	4335	59	59	58	59	59
Decreased controls on capital outflows							
Mean propensity score	439.0	496.8	559.2	506.9	487.3	464.3	495.4
Mean absolute bias		38.50	10.29	6.17	6.19	9.19	21.55
(standard deviation)		(32.83)	(6.32)	(3.20)	(4.42)	(6.48)	(20.38)
Observations on support	29	4679	29	29	27	29	29

Notes: Statistics and tests from use of matching algorithms discussed in Section IV.B.

Table 4

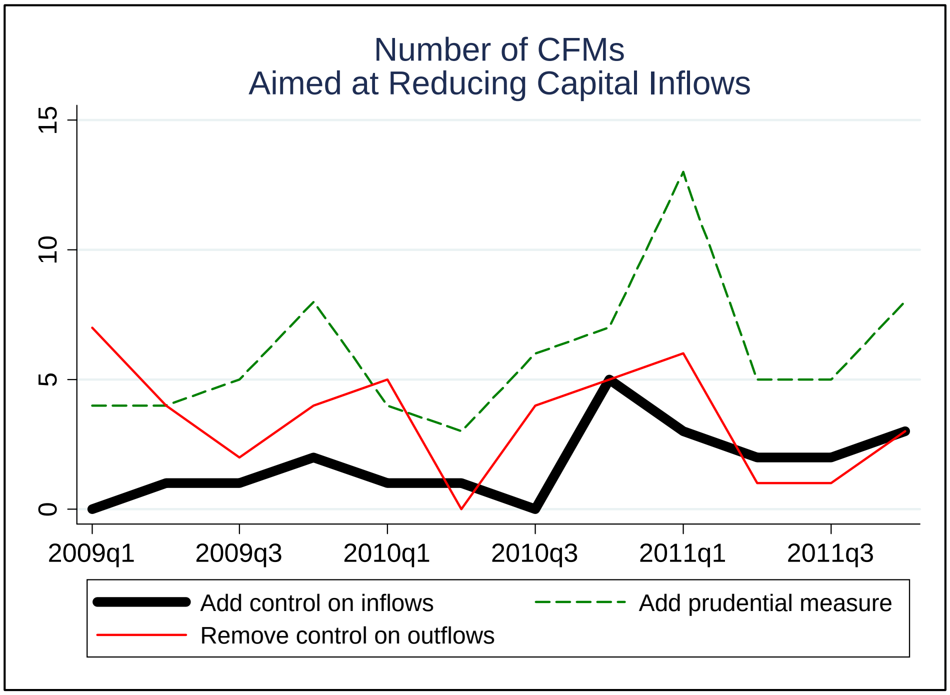
Increased Controls on Capital Inflows:

Means for Treatment and Control Groups using Different Matching Algorithms

	Mean: Treated 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		Kernel		Local-linear	
				Mean: Matched Control	t-stat	Mean: Matched Control	t-stat	Mean: Matched Control	t-stat	Mean Matched Control	t-stat	Mean Matched Control	t-stat
Real ER	0.090	0.008	4.21***	0.093	-0.12	0.100	-0.32	0.068	-0.15	0.039	1.36	0.099	-0.33
Portfolio flows	0.401	-2.541	0.21	3.346	-0.99	-9.611	0.46	-1.073	0.15	-2.163	0.19	1.955	-0.58
Consensus CPI	7.156	4.158	4.78***	6.675	0.43	6.620	0.49	6.492	0.14	4.433	2.30**	6.115	1.03
Credit growth	0.044	0.026	0.99	0.021	0.75	0.027	0.57	0.037	0.34	0.030	0.52	0.012	1.12
VIX	25.752	26.482	-0.39	28.132	-0.93	24.917	0.35	25.592	-0.43	26.357	-0.51	27.791	-0.82
TED	0.268	0.351	-1.39	0.275	-0.17	0.244	0.52	0.292	-0.32	0.331	-0.88	0.271	-0.08
Commodities	0.068	-0.007	1.30	0.038	0.48	0.090	-0.35	0.057	0.02	0.016	0.64	0.058	0.18
Interest rate - US	-0.523	-0.149	-0.56	-0.596	0.03	-1.093	0.25	-0.423	-0.16	-0.396	-0.16	-1.006	0.22
FX Reserves/GDP	0.080	0.084	-0.06	0.108	-0.37	0.089	-0.13	0.085	-0.01	0.095	-0.20	0.134	-0.73
Floating ER	0.667	0.744	-0.81	0.714	-0.33	0.667	0.00	0.681	0.26	0.760	-0.42	0.714	-0.33
CA openness	0.073	1.016	-2.97***	0.159	-0.27	0.126	-0.16	0.259	-0.47	0.874	-2.07**	0.234	-0.51
Stock market cap.	43.231	84.666	-1.98**	47.565	-0.36	47.698	-0.41	52.349	-0.44	78.437	-1.61	48.162	-0.40
GDP per capita	8.443	9.295	-3.26***	8.498	-0.19	8.429	0.05	8.575	-0.04	9.200	-2.04**	8.535	-0.31
Legal compliance	2.046	2.229	-3.82***	2.033	0.26	2.045	0.03	2.094	-0.96	2.180	-2.16**	2.029	0.32
Legal comp. ^{2}	4.216	5.018	-3.76***	4.157	0.27	4.212	0.02	4.425	-0.97	4.807	-2.20**	4.144	0.33
Mean Propensity Score	***420.6***	***498.7***		***499.3***		***446.8***		***491.4***		***466.8***		***445.3***	
Observations	***21***	***4932***		***21***		***21***		***18***		***21***		***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.

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



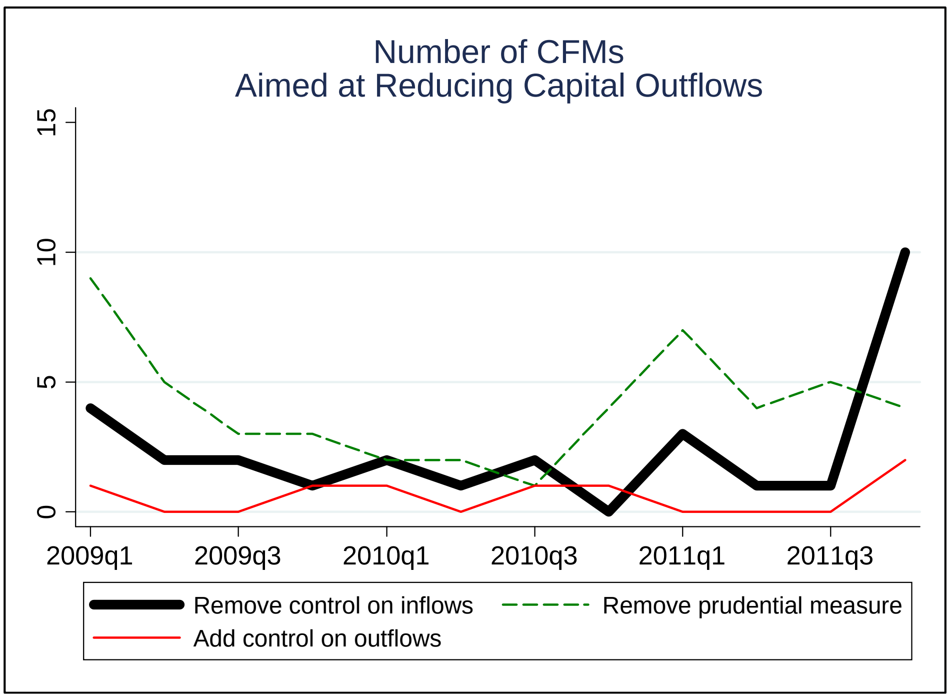
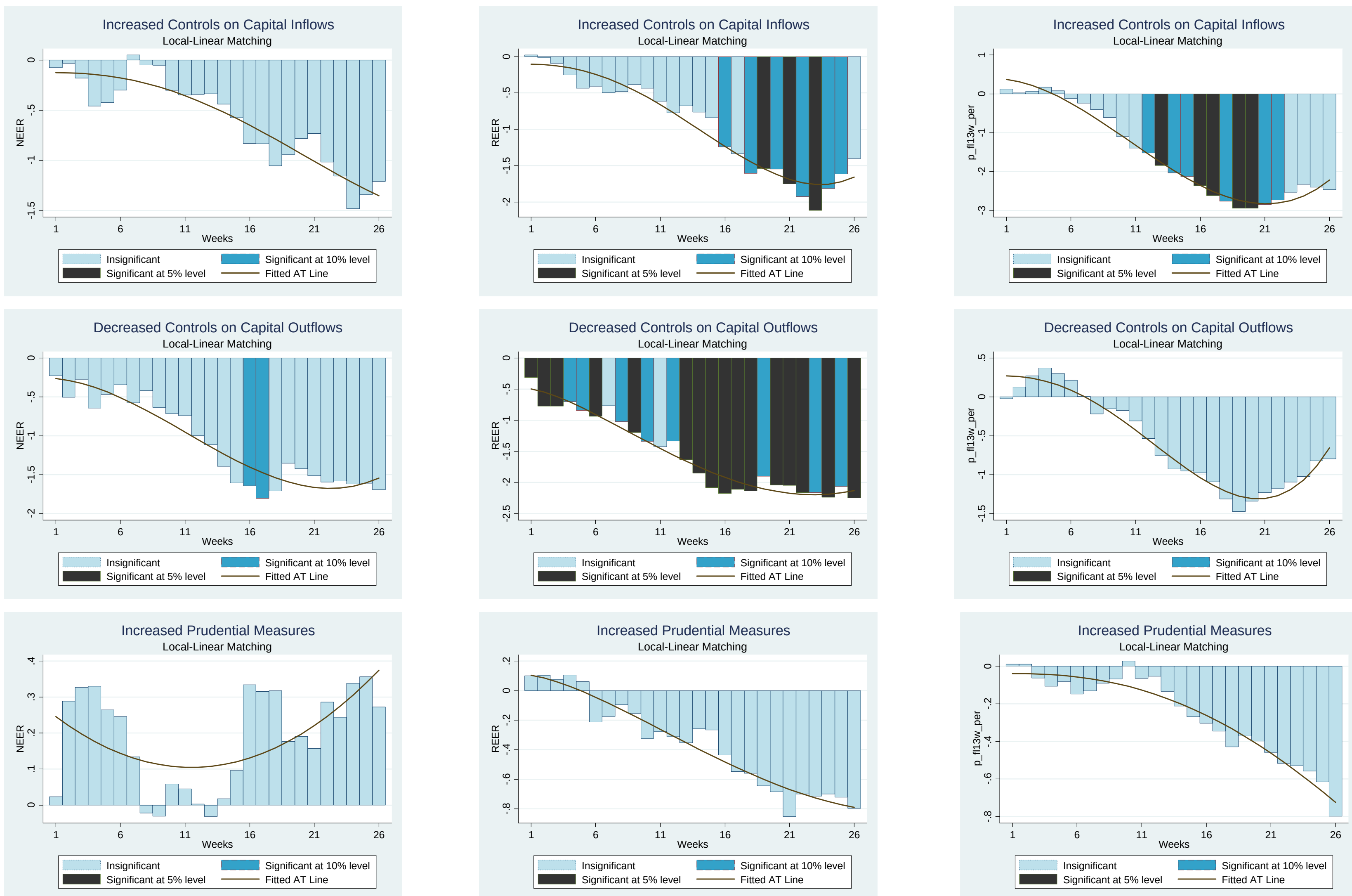


Figure 2a

Average Treatment Effects of CFMs Using Local-Linear Matching: Exchange Rates and Portfolio Flows

% Change in Nominal Exchange Rate[1] | **% Change in Real Exchange Rate[1]** | **Change in Net Portfolio Inflows[2]**

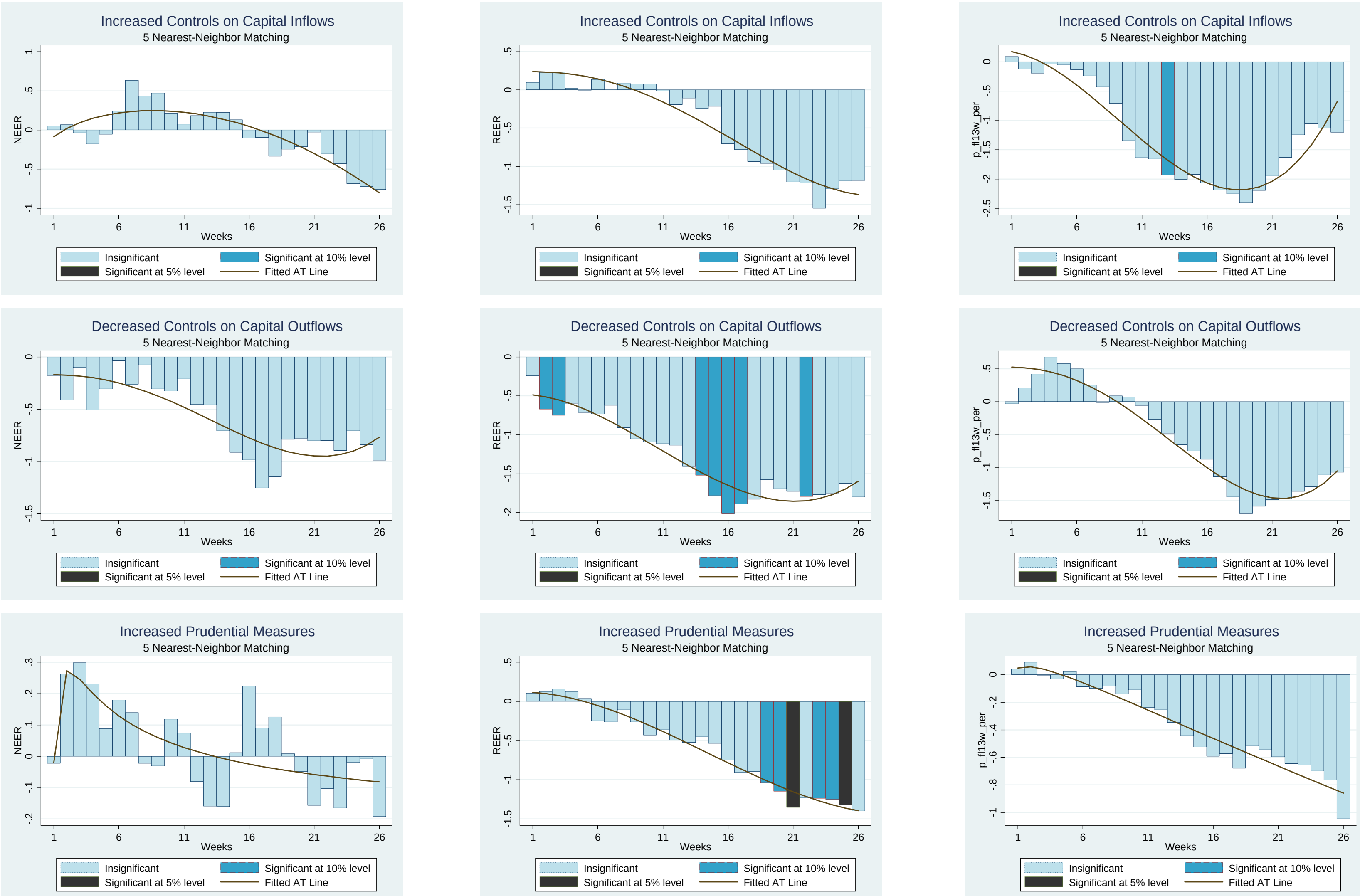


Notes: (1) Based on a broad exchange rate index. (2) Net portfolio inflows are cumulative flows over the last 13 weeks and measured as a percent of total portfolio assets lagged one period before the CFM event.

Figure 2b

Average Treatment Effects of CFMs Using Five-Nearest Neighbors: Exchange Rates and Portfolio Flows

% Change in Nominal Exchange Rate[1] | **% Change in Real Exchange Rate**[1] | **Change in Net Portfolio Inflows**[2]

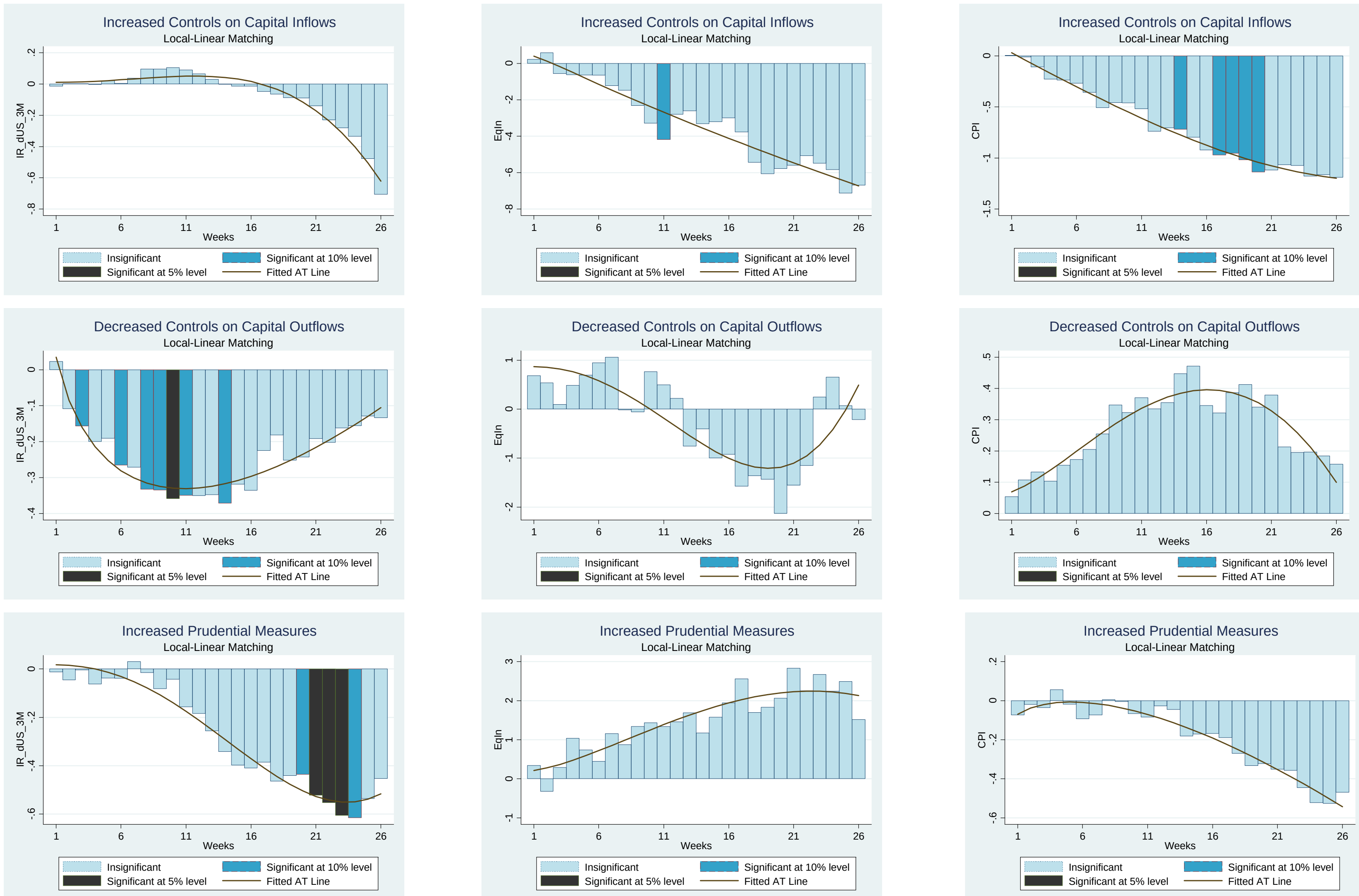


Notes: (1) Based on a broad exchange rate index. (2) Net portfolio inflows are cumulative flows over the last 13 weeks and measured as a percent of total portfolio assets lagged one period before the CFM event.

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[2]**



Notes: (1) Interest rate differential versus the United States for 3-month Treasury bills. (2) Inflation measured by the CPI.

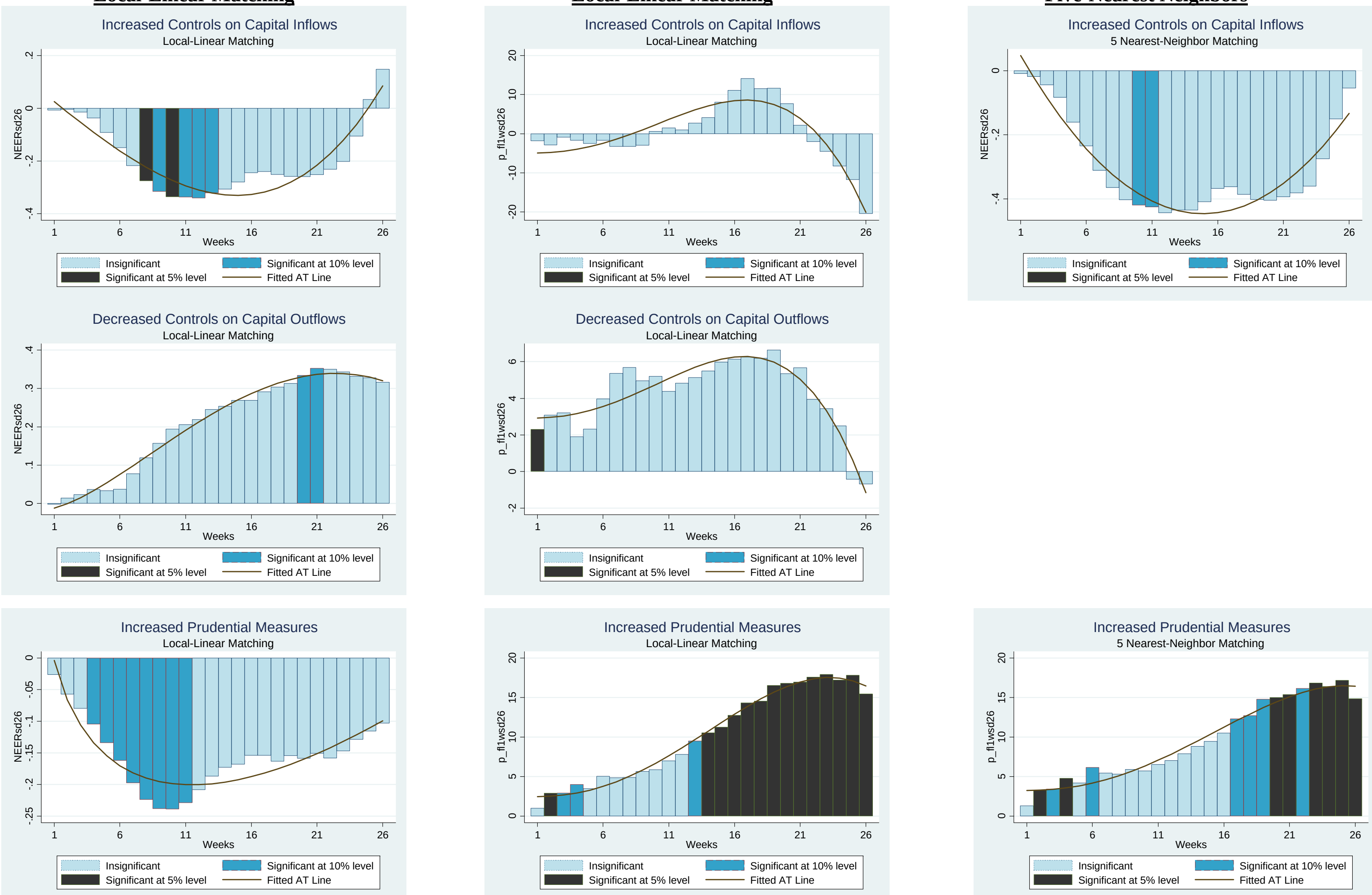
Figure 4

Average Treatment Effects of CFMs on Volatilities

Volatility in Nominal Exchange Rate
Local-Linear Matching

Volatility in Net Portfolio Inflows
Local-Linear Matching

Robustness Check:
Five-Nearest Neighbors



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

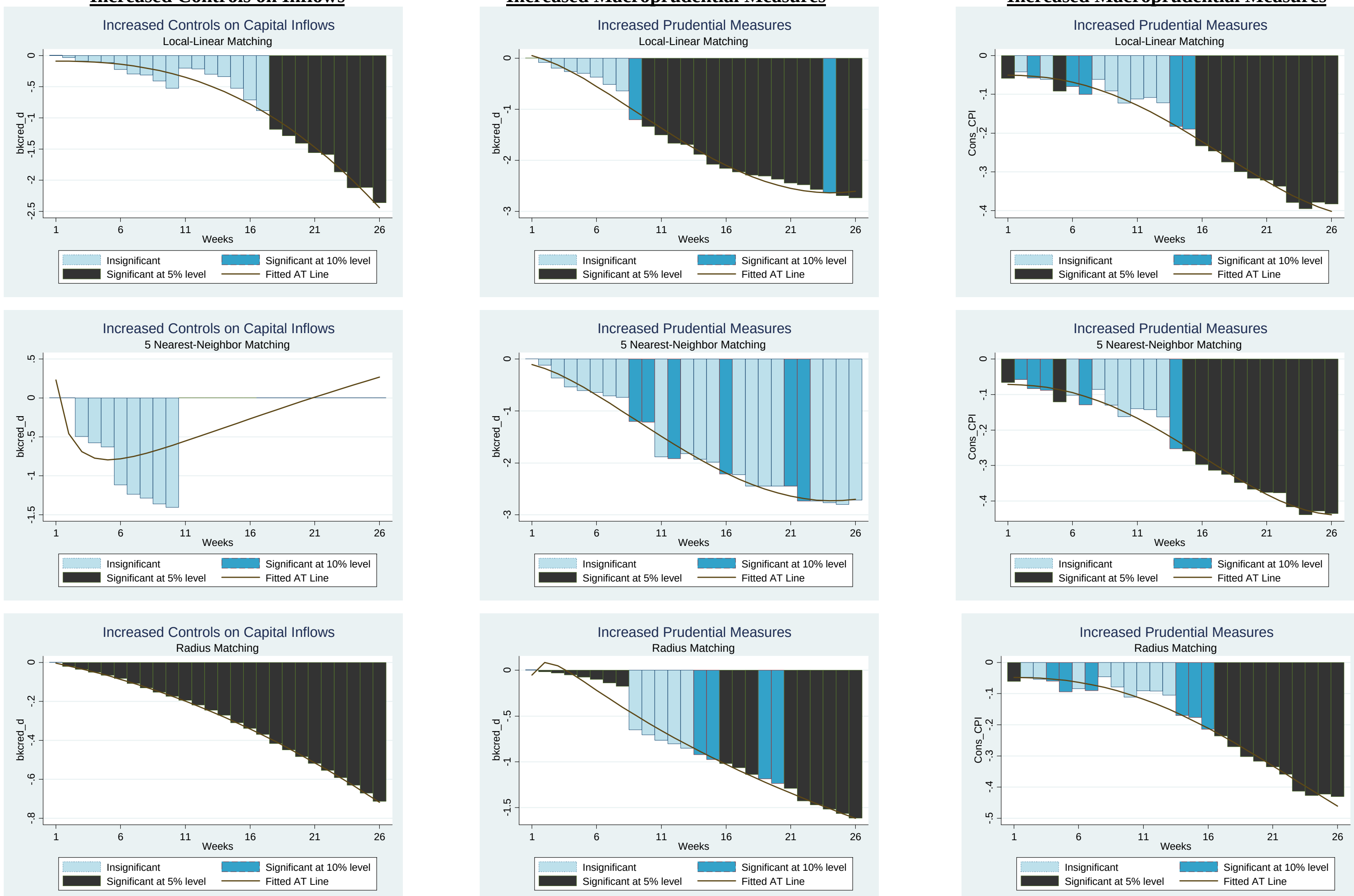
Figure 5a

Average Treatment Effects of CFMs: Financial Vulnerabilities

% Change in Bank Leverage[1]
Increased Controls on Inflows

% Change in Bank Leverage[1]
Increased Macroprudential Measures

Change in Expected Inflation[2]
Increased Macroprudential Measures

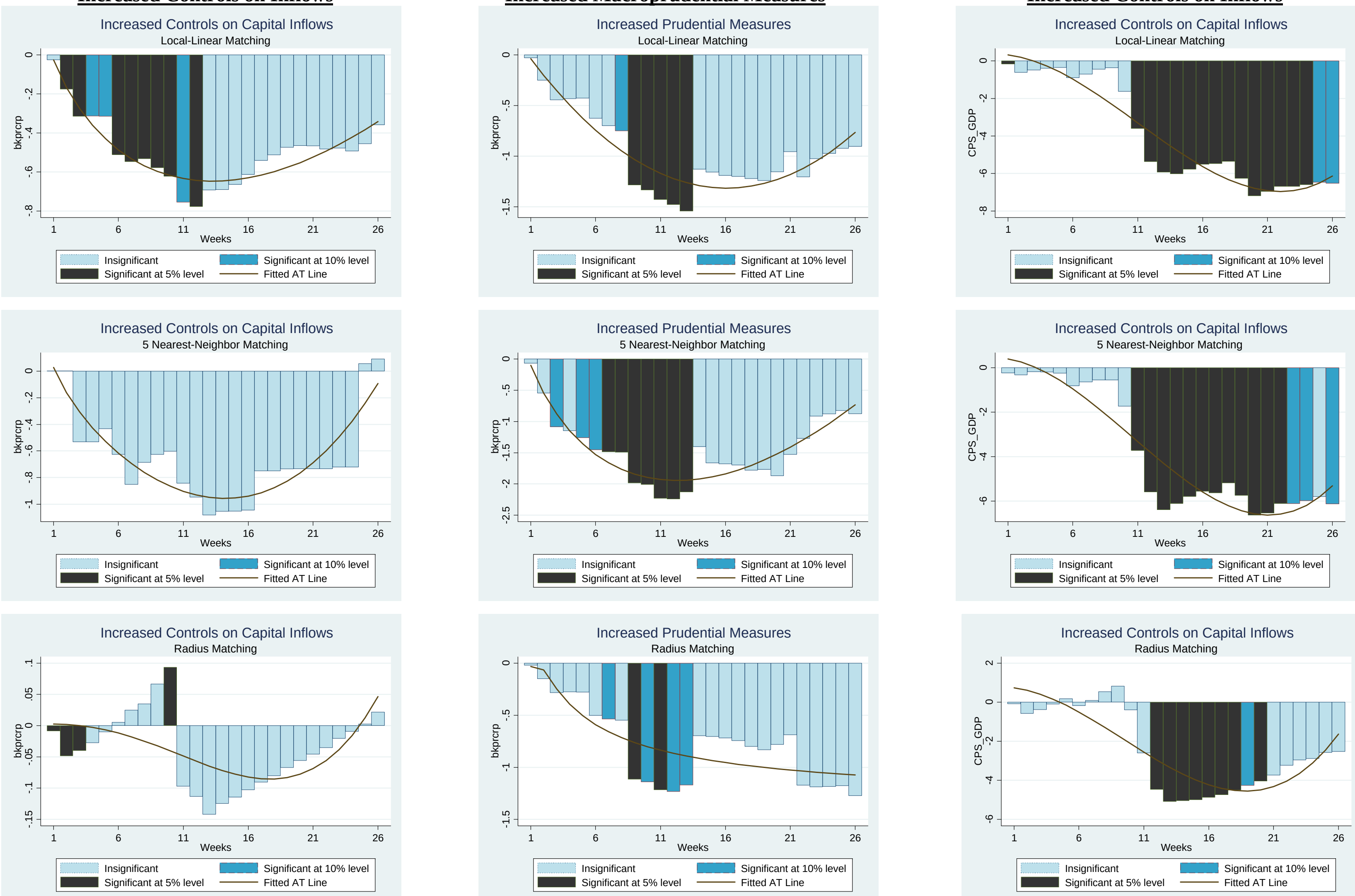


Notes: (1) Bank leverage measured by ratio of bank credit to bank deposits. (2) Consensus inflation forecasts over the next year.

Figure 5b

Average Treatment Effects of CFMs: Financial Vulnerabilities

% Change in Bank Credit[1] Increased Controls on Inflows | **% Change in BankCredit[1] Increased Macroprudential Measures** | **% Change in Private Credit[2] Increased Controls on Inflows**



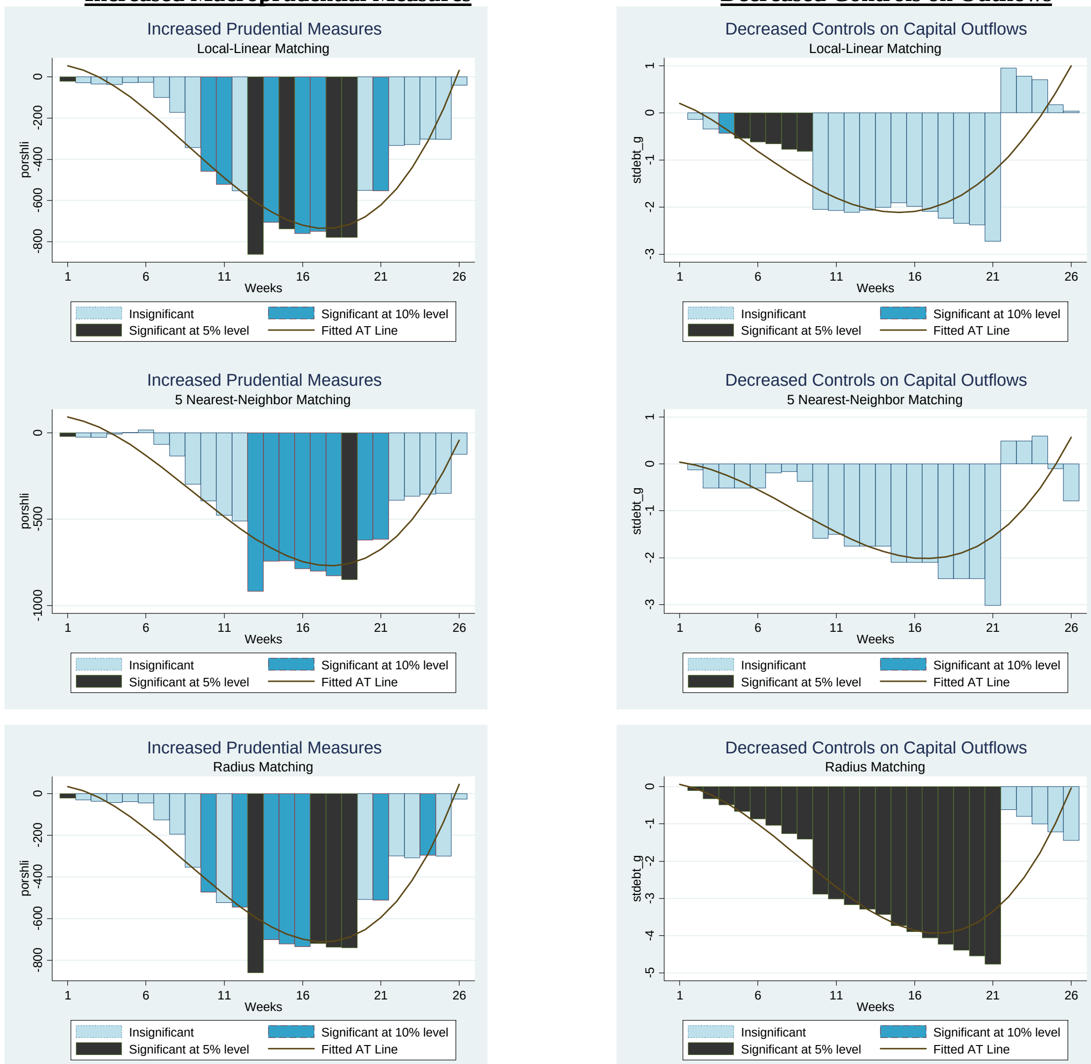
Notes: (1) Bank credit is private credit by deposit money banks and other financial institutions to GDP. (2) Measured as a share of GDP.

Figure 5c

Average Treatment Effects of CFMs: Financial Vulnerabilities

Change in Portfolio Share in Liabilities[1]
Increased Macroprudential Measures

% Change in Short-term Debt[2]
Decreased Controls on Outflows

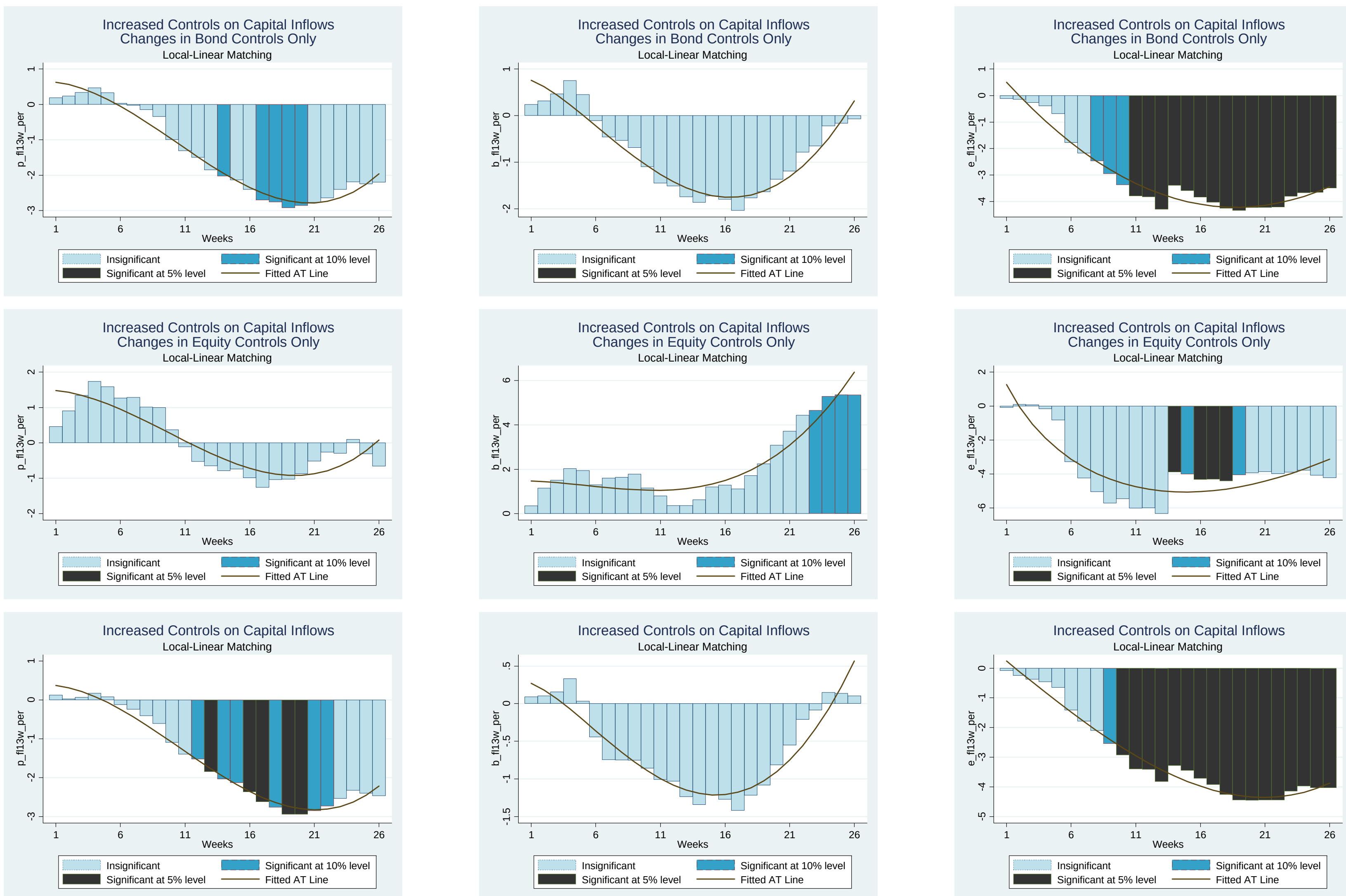


Notes: (1) Portfolio investment liabilities as a share of total liabilities. (2) Measured as a share of GDP.

Figure 6

Average Treatment Effects of CFMs on Portfolio Flows: Increased Controls Targeting Bond or Equity Inflows

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

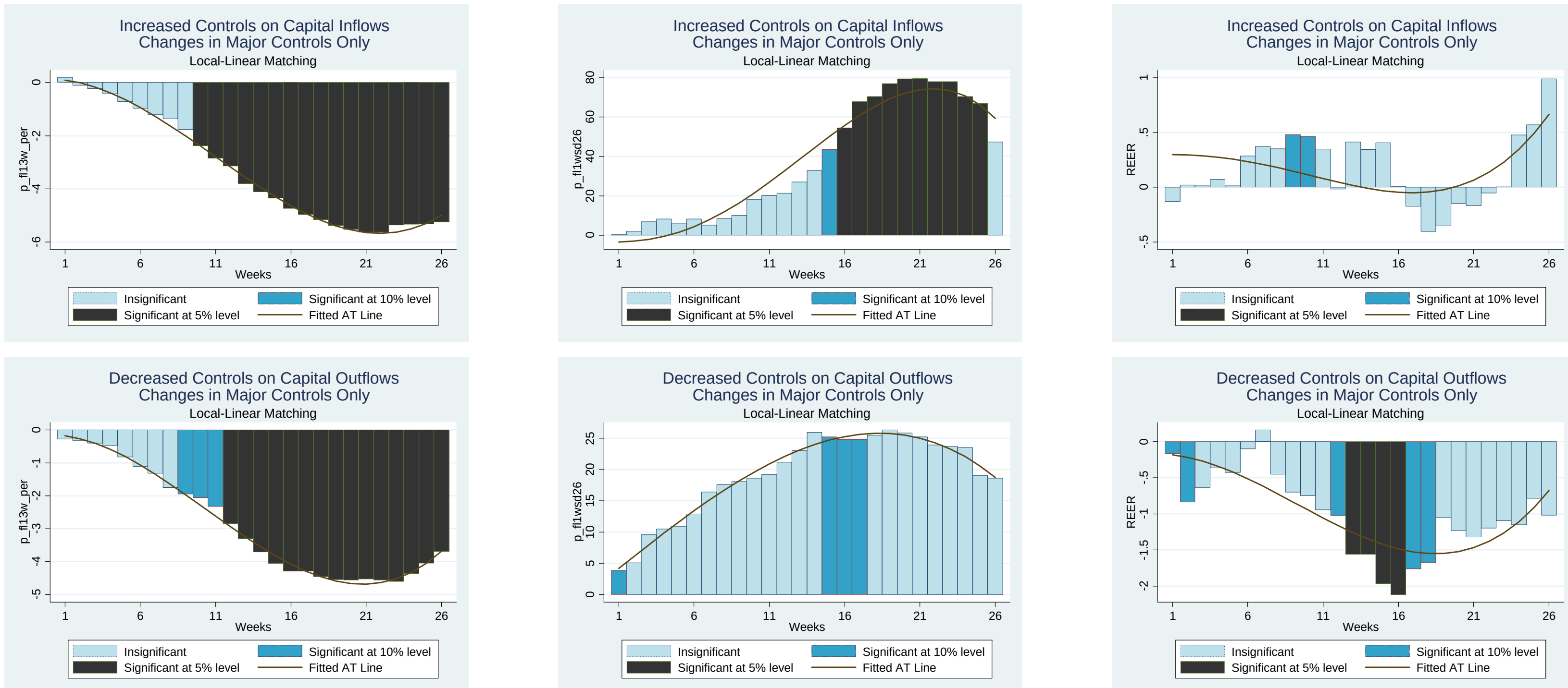


Notes: Net portfolio, bond and equity inflows are cumulative flows over the last 13 weeks and measured as a percent of total portfolio, bond, or equity assets, respectively, lagged one period before the CFM event.

Figure 7

Average Treatment Effects of "Major" CFMs: Portfolio Flows, Volatility and the Exchange Rate

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



Notes: Major CFMs are changes in capital controls and macroprudential measures which received mention by financial analysts and in financial publications. (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