Removing the Punch Bowl:
Moderating Vulnerabilities from Global Economic Booms*

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Abstract: Are there any policy choices which could moderate economic booms and their negative consequences? In order to answer this question, it is necessary to control for selection bias—the fact that countries which select certain policies tend to be different than countries which do not. We use propensity-score matching to address this concern and estimate the effect of six policies (increasing interest rates, tightening fiscal policy, allowing exchange rate appreciation, accumulating reserves, increasing controls on capital inflows and strengthening macroprudential regulations) during the boom period of 2002-2007. We find that many of these policies have large and meaningful effects on the occurrence of bank credit booms, equity booms, banking crises, and non-performing loans—but each policy which moderates certain aspects of booms simultaneously aggravates other risks. Some policies do not have consistently significant effects, which may result from either shortcomings of the econometric approach or limits to the policy's effectiveness in tempering booms and their consequences.

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I. Introduction

One of the more famous quotes in monetary economics is William McChesney Martin's observation that the role of a central bank was to order "…the punch bowl removed just as the party was really heating up."[1] The point, of course, is to avoid the hangover that follows a raucous evening. The Global Financial Crisis, coming after the boom during the first years of this century, has been the worst economic hangover since the 1930s. In light of this, attention has recently focused on ways to "remove the punch bowl" during a boom through the use of various policies that could temper the upswing and avoid the downturn that often follows.

In this paper we examine a range of policies that have been suggested as means to moderate booms, including increasing interest rates, tightening fiscal policy, allowing exchange rate appreciation, accumulating reserves, increasing controls on capital inflows and strengthening macroprudential regulations. We evaluate how frequently each of these policies was used during the boom period from 2002 to 2007 in a set of 50 advanced and emerging market countries. We also examine why different countries selected different policy responses and estimate whether these policies were successful in tempering equity and bank credit booms and avoiding subsequent banking crises and increases in non-performing loans.

We find some evidence that certain policies can temper some aspects of booms—but each policy also simultaneously aggravates other challenges measuring in the analysis. For example, sharp increases in interest rates can reduce the occurrence of bank credit booms, equity booms, and banking crises, but simultaneously generate an increase in non-performing loans. Macroprudential regulations (measured using a very broad index) appear to reduce the occurrence of bank credit booms and non-performing loans after about a year, but may simultaneously increase the risk of an equity boom. Exchange rate appreciation may reduce the risk of banking crises and NPLs, but tends to generate booms in both bank credit and equities. All of these policies may also have additional costs and benefits that are not captured in the analysis and which would need to be considered before any implementation of these policies. Many policies are estimated to have effects in the expected direction that are large, but are often not statistically significant across different estimation methodologies. It is unclear if this lack of

[1] Martin was Chairman of the Federal Reserve from 1951 to 1970. This quote comes from his speech to the Investment Bankers' Association of America in October 1955 in which he quotes a journalist commenting on the Federal Reserve's move to raise interest rates.

significance results from the policies not accomplishing their intended goals, or from limits to the estimation technique.

A central contribution of our study is to address the challenge of "selection bias"—of disentangling the effects of policy choices from the effects of just being the type of country that chooses to enact these policies. For example, does raising interest rates temper a boom? Or instead does any apparent relationship between raising interest rates and booms reflect that countries willing to raise interest rates differ in some important ways from those that would not, and these differences are what tempers a boom? We address this challenge through the use of propensity-score matching techniques that enable us to find, for the countries that undertook certain policies, a set of comparison countries that were similar to them but did not undertake those policies.[2] Propensity-score matching enables us to control for selection bias and therefore can provide a better assessment of policy effects than would be obtained through a direct comparison of outcomes without regard for underlying economic characteristics.

Interest in policies to temper booms has been spurred by the Global Financial Crisis. As suggested by Chairman Martin's 1955 quote, however, this topic has been one of longstanding interest for monetary policy; for example, during the mid-2000s, there was a debate on whether interest rates should be used to prick an asset market bubble.[3] But the more recent, post-2008 discussion is distinct from that of earlier eras due to the range of policies now under consideration.[4] Currency appreciation was not a viable option during the Bretton Woods era. Capital controls were pervasive until at least the 1980s, but until the early years of this century the general tendency was towards greater capital account liberalization (Edison, Klein, Ricci, and Sløk, 2004).[5] The newer focus on macroprudential policies has been a direct consequence of the way in which financial disruptions were at the center of Global Financial Crisis (Dell-Ariccia, Igan, Laeven, and Tong, 2012).

This paper is part of a stream of research in the wake of the Global Financial Crisis on the appropriate use of monetary, exchange rate, and regulatory policy for preventing boom-bust cycles. Theoretical research includes work on capital controls that shows how externalities from

[2] This paper complements our earlier work on using propensity-score matching to examine the effects of policies undertaken during crises. See Forbes and Klein (2013).
[3] See, for example, "Alan Greenspan: Monetary Myopia," *The Economist*, January 12, 2006.
[4] Calvo, Leiderman and Reinhart (1996) offered an overview of varied responses to surges of capital inflows by Asian and Latin American countries in the 1990s.
[5] Although there were efforts to manage inflows with these policies in the 1990s, notably by Chile. See De Gregorio, Edwards and Valdés (2000) and Forbes (2007).

capital inflows can be addressed with taxes on these transactions (Bianchi and Mendoza 2010, Korinek 2010 and 2011, Jeanne and Korinek 2010). Empirical research includes analyses of the effects of capital controls and macroprudential policies (Forbes, Fratzscher, and Straub 2013, Klein 2012, Ostry *et al.* 2012, Kuttner and Shim 2013, and Bierne and Friedrich 2014). Related empirical work identifies and predicts credit booms (Mendoza and Terrones 2012, Elekdag and Wu 2011, and Guarín, González, Skandalis and Sánchez 2014). Finally, a number of influential policy-oriented books and papers have called for a re-examination of views on capital controls and macroprudential policies, suggesting that these policies might be part of the regular toolbox employed by governments (Ostry *et al.* 2010, Ostry *et al.* 2011, Habermeier, Kokenyne, and Baba 2011, and Jeanne, Subramanian and Williamson 2012).

The remainder of this paper is as follows. Section II discusses six possible policy responses to global booms; accumulating reserves, exchange rate appreciation, raising policy interest rates, tightening fiscal policy, increasing controls on capital inflows and strengthening macroprudential polices. We show the individual and joint distribution of these policies during the boom years from 2002 to 2007. Section III presents a discrete-choice model which analyzes the determinants of the implementation of these policies. This analysis is interesting in its own right and also serves as the first step for the propensity-score matching which is presented and evaluated in Section IV. Section V uses this methodology to calculate average treatment effects on the treated showing the effect of each of the policies on various measures of booms and crises. Section VI concludes.

I. Policy Responses to Global Booms

The tool that central banks traditionally used to "remove the punch bowl", at least before the Global Financial Crisis, was to raise the short-term policy interest rate. This has the advantage of being a relatively nimble policy that can be more quickly and easily implemented than other options (such as tightening fiscal policy or instituting new regulations). It is also a policy that has broader effects throughout the economy than more narrowly targeted policies (such as capital controls or macroprudential policies).

This breadth of the effect of interest rate adjustments, however, could also be a drawback. A broad instrument may be less appropriate for addressing specific challenges, such as housing price bubbles, than more narrowly targeted policies.[6] Furthermore, there can be unintended consequences of adjusting interest rates, especially for small, open economies. For example, raising interest rates to combat an asset price boom could also increase capital inflows, which could further fuel a boom.

This section examines the policy responses taken by the countries during the global boom from 2002 to 2007 that preceded the recent financial and economic crisis. We focus on six policies; raising the policy interest rate, allowing an exchange rate appreciation, accumulating reserves, tightening fiscal policy, implementing new capital controls, and strengthening macroprudential policies. We are interested in substantial changes in policy that reflect an effort to aggressively combat booms, rather than routine policy changes that would be common in more quiescent periods. Therefore, we generate a set of dummy variables capturing large changes in interest rates, exchange rates, reserves, and fiscal policy by setting a threshold for each of these four policies and identifying years in which a country's change in policy met or exceeded this threshold. Macroprudential policies and capital controls are more discrete measures, and therefore the dummy variables for these policies capture years in which these measures are introduced, or in some cases when their coverage is expanded.[7]

The thresholds that we employ for a policy adjustment to qualify as "large" for interest rates, exchange rates, reserves and fiscal policy are set at the upper 10th percentile of the distribution of annual changes in these variables for the 2002 to 2007 period. In some cases, we also include additional criteria to ensure that the variables capture the intended policy adjustments.

More specifically, the dummy variable measuring large changes in interest rates equals 1 if the policy interest rate rose by 135 basis points or more from its value in the preceding year, provided that the CPI inflation rate is less than 10 percent. This proviso ensures that the change in the policy rate represents a change in the real interest rate and not just a rise in rates reflecting higher inflation. The dummy variable measuring exchange rate appreciation equals 1 when the

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[6] There are also questions on the efficacy of narrowly targeted policies. Countries recently facing housing price booms, such as Britain, Israel, New Zealand and Norway, have used policies such as maximum loan-to-value ratios, tightening underwriting standards and instituting surcharges on risky home loans. How effective these policies have been at tempering the rise in housing prices is open to question. See "Surging house prices test regulators' new weaponry," *Financial Times*, May 6, 2014, p. 2.

[7] As discussed in more detail in the data appendix, we use *de jure* indicators of macroprudential policies and capital controls. These indicators denote the presence of these measures, but not their intensity. For a discussion of the development of these capital control indicators see Fernandez *et al.* (2014).

appreciation of the bilateral dollar exchange rate was 15.9 percent or greater as compared to its value in the previous year. The dummy variable measuring large reserve accumulations equals 1 when the reserve to GDP ratio rose by 4.4 percentage points or more. The dummy variable representing substantial tightening in fiscal policy equals 1 when the general structural budget balance increases by at least 1.36 percent points of GDP relative to the previous year. By focusing on the structural balance, this measure should control for any changes in fiscal policy that occur automatically due to changes in the output gap.

In order to capture any changes in capital controls and macroprudential regulations, we combine several different measures in order to capture the range of policies that could be included and due to limitations with existing data sets. We define an increase in capital controls as occurring when a country either adds any new controls on capital inflows (based on the index compiled in Klein, 2013) or increases regulations on foreign exchange or international exposures in the financial sector (as compiled by Beirne and Friedrich, 2014). We define an increase in macroprudential regulations as any increase in housing related or banking regulations, including for reserve requirements, credit growth limits, loan-to-value ratios, DSTI limits, risk weighting, provisioning, exposure limits and liquidity requirements (based on data compiled in Kuttner and Shim, 2013). Appendix A provides additional information on the definitions and sources for the variables measuring all six of these policy responses.

In order to include as large and varied a sample of countries as possible—which can be important to obtain good matches for propensity-score matching—we use as broad a sample as possible. We begin with all “Advanced Economies” as defined by the International Monetary Fund and all “Emerging Markets” and “Frontier Economies” as defined by Standard & Poor’s BMI indices. Then we exclude countries or years for three reasons: (1) if they do not have data for each of the 6 policy measures for at least one year during the boom period from 2002-2007; (2) they are in a recession (defined as negative GDP growth for the year)[8]; (3) if they are in the euro zone then they cannot qualify as having a major increase in interest rates, major increase in reserves, or substantial exchange rate appreciation (given their limited ability to use those tools under the currency union). This sample and criteria yield a dataset of 50 countries from 2002-2007. The list of countries in the final sample is reported in Appendix B.

[8] This leads to the exclusion of Argentina and Israel in 2002, and Germany, Italy, and Portugal in 2003.

Table 1 presents the number of instances of large changes in each of the six policies (in the right margin of the table), the number of times each policy occurred without any other policy occurring for that country in that year (the diagonal elements), and the number of times each policy occurred in the same year as another policy undertaken by that country (the upper diagonal cells for pairs, and the lower diagonal cells for triplets or quadruplets). The statistics in this table show that from 2002-2007, macroprudential policies were adopted or modified more often than major changes in any of the other variables. The second most popular policy choice was to increase capital flow management measures. The fact that the two most common policy choices are the two policies that were not constructed based on the 10% threshold criteria, however, suggests that this may be an artifact of the condition that we require large changes in the other four categories, and only a change in the macroprudential and capital flow management policies. The number of instances of the other four policies, reserve accumulation, interest rate increases, exchange rate appreciations, and fiscal tightening, are relatively similar by construction (although their incidence over time could fluctuate).

The relatively large values in the diagonal elements in Table 1 indicate that, for the most part, only one type of large policy change was undertaken in any particular year. The exception is that a relatively large number of macroprudential policies were increased in the same year as large reserve accumulations and additional capital controls. There were also more instances of macroprudential policies than of any other policy, however, with twice as many cases (or more) as the four "large" policy changes constructed using thresholds. There were only four cases in which more than two policies were employed in the same year.

Table 2 provides additional information on when policies are repeated or combined with other policies by reporting the number of times one policy was followed by another in the next year. The diagonal elements in this table represent instances in which a large policy change occurred in two consecutive years. The off-diagonal elements represent the number of times the policy listed in the row preceded the policy listed in the column by one year; for example, there was one instance in which a large reserve accumulation preceded, by one year, an exchange rate appreciation (cell (1,2)) and an interest rate appreciation (cell (1,3)), and seven instances in which a macroprudential regulation preceded additional capital controls (cell (6,5)). The most striking point in this table is the relatively low value of its entries; policies tended not to follow one another (with the possible exception of macroprudential policies, which again could simply result from their greater incidence in the overall sample). For the most part, the diagonal elements of this table represent the largest values, indicating a greater tendency for large changes in a particular policy to occur for two years in a row rather than for a large change in one policy to be followed, in the subsequent year, by a large change in another policy.

Figure 1 shows the evolution of these large policy changes over the sample period from 2002 to 2007. One of the biggest changes across time is the decrease in the number of large appreciations. There are nine large appreciations in each of the first two years, 2002 and 2003, but this number drops substantially in the subsequent four years. Another trend is the increased use of macroprudential measures over time. The large number of macroprudential regulations reported in Tables 1 and 2 reflect a dramatic increase from only four or five instances in the first two years of the sample to ten or more in each of the last three years. The figure also shows an increase in the incidence of major fiscal tightening and new capital controls around 2004-2005, with less frequent use of both these policies in 2006. Therefore, although there is less of a consistent trend in the number of countries that increased interest rates, tightened fiscal policy, added capital controls, or increased reserves, there is evidence that countries became more reliant on adding macroprudential regulations and less reliant on allowing large exchange rate appreciation over the boom period from 2002 through 2007.

II. Explaining Policy Choices

Next we consider the determinants of the six policies described in the previous section: reserve accumulation, exchange rate appreciation, interest rate increases, fiscal policy tightening, adding controls on capital inflows, and increasing macroprudential regulations. We continue to use the framework discussed above to focus on major changes in reserves, exchange rates, interest rates and fiscal policy that are above a certain threshold and on any recorded increase in capital controls and macroprudential policies. The results are then used in the following section to generate propensity scores for matching and estimating the effects of the different policies.

To begin, we estimate the likelihood of each of these policies using a logit model for our panel of 50 countries using annual data for the period 2002 to 2007. We begin by considering covariates that have been highlighted in the literature as potential determinants of policy changes and that also are available for a large sample of countries. These covariates can be roughly grouped into four categories (recognizing that some variables could fit in more than one category):

- *Global variables*: global risk and uncertainty (measured by the VXO index), changes in the U.S. policy interest rate, and the logarithm of a world commodity price index;
- *Fairly stable domestic characteristics* (country characteristics that tend to vary more in the cross-section than in the time series): the logarithm of real GDP per capita, the logarithm of an index of institutional quality, a pegged-exchange rate dummy variable, a Euro-area dummy variable, and capital account openness (from Chinn and Ito, 2008);
- *Time-varying domestic variables* (country characteristics that capture changing economic circumstances): the current account relative to GDP, reserves relative to GDP, CPI inflation, the change in private credit, the percentage change in stock market capitalization, the logarithm of a world commodity price index interacted with a dummy variable indicating whether the country is a commodity exporter, and the change in the growth rate of real GDP;
- *Lagged values of the six policy changes*. Six dummy variables capturing a large reserve increase, currency appreciation, interest rate increase, fiscal tightening, new capital control or increase in macroprudential regulations as defined in the previous section.

Details on the definitions and sources for each of these variables is presented in the middle of Appendix A. Each of these covariates is lagged by one year. Using these four categories for the covariates, the logit model we estimate is:

$$Prob(pc_{it} = 1) = F\left(\mathbf{\Phi}_{t-1}^{\text{Global}}\mathbf{B}_{\text{G}} + \mathbf{\Phi}_{i,t-1}^{\text{Time-Varying}}\mathbf{B}_{\text{TV}} + \mathbf{\Phi}_{i,t-1}^{\text{Characteristics}}\mathbf{B}_{\text{C}} + \mathbf{\Phi}_{i,t-1}^{\text{RecentPolicies}}\mathbf{B}_{\text{RP}}\right) \tag{1}$$

where pc_{it} is an episode dummy variable that takes the value of 1 if country *i* adopts a major policy change in year *t* and $\mathbf{\Phi}_{t-1}^{\text{Global}}$, $\mathbf{\Phi}_{i,t-1}^{\text{Vulnerabilities}}$, $\mathbf{\Phi}_{i,t-1}^{\text{Characteristics}}$, and $\mathbf{\Phi}_{i,t-1}^{\text{RecentPolicies}}$ are vectors of variables measuring global variables, time-varying country-specific variables, country characteristics, and recent changes in related policies for country *i*, lagged by one year. All standard errors are robustly estimated.

We first estimate the logit models with the full set of covariates. We then re-estimate the models using only the covariates whose coefficients have a p-value of 0.20 or better, to avoid "overfitting" the model for our subsequent analysis.[9] Table 3 reports the estimates of the logit models for each of the six policies. The model is most successful (in terms of the pseudo-R^2) at explaining large exchange rate appreciations, macroprudential policies, and major increases in the policy interest rate (with pseudo-R^2's of 0.25, 0.27 and 0.27, respectively). The model performs least well in explaining the addition of controls on capital inflows (in which case the pseudo-R^2 is 0.08). All three of covariates measuring global variables (at the top of the table) are significant in predicting large exchange rate appreciations, and the change in the U.S. interest rate is also significant for fiscal tightening and interest rate increases. Among country characteristics, a higher degree of capital account openness is significantly associated with a lower likelihood of an interest rate increase, fiscal tightening, new capital controls and additional macroprudential policies. A country with a pegged exchange rate is less likely to have a fiscal tightening.

Many of the time-varying country variables are significantly associated with large policy changes. For example, countries with (lagged values of) higher reserves as a share of GDP are significantly more likely to accumulate reserves, raise interest rates, tighten fiscal policy, and increase macroprudential regulations, and less likely to have a large currency appreciations. Countries with an increase in real GDP growth are more likely to have a large currency appreciation, tighten fiscal policy, and impose capital controls. Countries with increased capital inflows (as a share of GDP) are more likely to increase interest rates and less likely to allow a major appreciation.

The last few rows of the table show how previous policy actions are associated with subsequent policies—and that countries often continue to adopt the same policies. For example, a large appreciation in one period has a significant positive effect on the likelihood of a large

[9] The covariates of the institutional index, the euro area dummy variable, stock market capitalization and the large reserve change dummy variable do not meet the 0.20 threshold for any of the six models.

appreciation in the subsequent period and an increase in macroprudential measures in pone period has a significant positive effect on the likely in the subsequent period.

Even more important than any individual coefficient estimates, the results in Table 3 suggest that selection bias may be a concern in analyzing the impact of different policy actions during boom periods More specifically, the fact that a number of the country-specific variables (especially those that vary across time) are significant determinants of large policy changes suggests that countries which adopt these policies tend to be different than those which do not. This makes it difficult, without appropriately controlling for this selection bias, to gauge the effect of these large policy changes on outcomes of interest. Propensity score matching offers a method to control for selection bias, and we next turn to a discussion of this technique before presenting our results that employ this methodology.

III. Propensity Score Matching: An Overview and Methodology Tests

a. An Overview of Propensity Score Matching

Countries which undertake a particular policy may differ from countries which do not undertake that policy in ways that affect outcomes over and beyond the effects of the policy itself. This raises the possibility that an analysis comparing the outcomes of countries depending upon whether or not they implement a policy may capture selection bias as well as the impact of that policy. One means of addressing this issue is through the use of propensity score matching. This technique should isolate the effects of policies from the effects of differences across countries which are unrelated to the effect of those policies. This section discusses propensity score matching, which has only recently been used in macroeconomics, monetary economics and international economics, although it has been a staple in other fields, such as labor economics, for several decades.[10]

To illustrate the issue of selection bias, define the adoption of the treatment or policy (such as a major increase in interest rates) by the i^{th} country as $D_i = 1$, and the absence of this

[10] Propensity-score matching is discussed in Dehejia and Wahba (2002), Angrist and Pischke (2008, chapter 3) and Heinrich, Maffioli, and Vazquez (2010). Recent research in macroeconomics that has used this technique includes analyses of monetary policy (Angrist and Kuersteiner (2011), Angrist, Jordá, and Kuersteiner (2013) and Ehrmann and Fratzscher (2006)), the effect of openness on growth (Das and Bergstrom (2012)), financial liberalization (Das and Bergstrom (2012), and Levchenko, Rancière, and Thoenig (2009)), foreign ownership of firms (Chari, Chen, and Dominguez (2011)), the response of economies to crises (Forbes and Klein (2013) and Glick, Guo, and Hutchison (2006)), fiscal policy (Jordà and Taylor (2013)) and the effects of capital controls and macroprudential measures (Forbes, Fratzscher and Straub (2013)).

action as $D_i = 0$. The outcome variable (such as the occurrence of an equity boom) is $Y_{1,i}$ 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 $E[Y_{0,j}|D_j=0]$. The observed difference between the outcomes for these two groups is

$$E[Y_{1,i}|D_i=1] - E[Y_{0,i}|D_i=0] = E[Y_{1,i} - Y_{0,i}|D_i=1] + \{E[Y_{0,i}|D_i=1] - E[Y_{0,i}|D_i=0]\}.^{11} \qquad (2)$$

We are interested in $E[Y_{1,i} - Y_{0,i}|D_i=1]$, which is the effect of the policy relative to the outcome the members of the treated group would have had in the absence of that policy. This is called the average effect of the treatment on the treated or ATT. The observed difference in outcomes between the two groups, however, consists of both the ATT and $\{E[Y_{0,i}|D_i=1] - E[Y_{0,i}|D_i=0]\}$, which represents a selection bias. Selection bias occurs if the treatment is not randomly assigned and if there are differences in outcomes solely because of underlying (i.e., pre-treatment) differences between the treated and control groups.

The effect of sampling bias could be easily minimized if there were a large set of countries that differed along only one or two discrete and observable dimensions with respect to the likelihood of undertaking a policy. In this case, countries could be readily apportioned to a small number of "cells" reflecting all differences along these dimensions. It would be straightforward to calculate the differences between the treated and the untreated in each cell (providing there are enough observations in each cell), and take a weighted average of those differences in order to estimate the effect of different treatments. In practice, however, there are many, multidimensional differences across countries, and it is impossible to simply match treated and control countries with identical macroeconomic characteristics.[12]

It may be possible, however, to match treated countries to control countries based on a set of observable country characteristics, represented by the vector X_i for the i^{th} country. If this matching takes into account the differences in the treated and untreated groups that affect

[11] Angrist and Pischke (2008) show the full derivation of these equations. The observable outcomes are $Y_i = Y_{0,i} + (Y_{1,i} - Y_{0,i})D_i$. The expected values conditional on D_i are $E[Y_i|D_i=1] = E[Y_{1,i}|D_i=1]$, $E[Y_i|D_i=0] = E[Y_{0,i}|D_i=0]$, and $E[(Y_{1,i} - Y_{0,i})D_i|D_i=0] = 0$. Thus, $E[Y_{1,i}|D_i=1] - (E[Y_{0,j}|D_j=0] = E[Y_{1,i} - Y_{0,i}\ |D_i=1]) + E[Y_{0,i}|D_i=1] - E[Y_{0,i}|D_i=0]$.

[12] In this discussion of propensity score matching, we focus on differences across countries and, for example, the vector X_i for the i^{th} country. In our empirical analysis, however, we focus on differences across countries and across time so that the appropriate vector is $X_{i,j}$, representing variables' values for the country i in year j. Thus, rather than pair a particular country with one or more other countries identified through propensity score matching, we will pair a particular country-year observation with one or more other country-year observations.

outcomes, then the sampling bias (or at least any bias that is captured in the vector X_i) disappears, and $E[Y_{0,i}|X_i,D_i=1] - E[Y_{0,i}|X_i,D_i=0] = 0$. This could still leave a multidimensional problem. This problem can be resolved, however, because it is sufficient to match treated and control observations based on a "propensity score," $p(X_i)$, which is the probability that country i receives the treatment (Rosenbaum and Rubin (1983). This single propensity score reduces the number of dimensions over which observations must be matched.[13]

The propensity score is the conditional probability of adopting the treatment (in our case, the policy response), given pre-treatment characteristics, X_i. Continuing to define the adoption of the treatment as $D_i = 1$ (and not adopting the treatment as $D_i = 0$), the propensity score is

$$p(X_i) = \Pr[D_i=1|X_i] \ . \tag{3}$$

In the context of our model, propensity scores are the likelihoods that a country undertakes a major increase in the policy interest rate, currency appreciation, increase in reserves, or fiscal tightening, or introduces or expands any controls on capital inflows or macroprudential regulations. The propensity scores can be generated using logit regressions.

After the propensity scores have been calculated, there are several algorithms that can be used to match each treated observation with one or more untreated observations (i.e. controls) with similar propensity scores. We focus on five matching algorithms: nearest-neighbor without replacement, five-nearest neighbors with replacement, radius with caliper, kernel, and local-linear matching. Each of these has advantages and disadvantages and some techniques may not satisfy key requirements based on the specific data set and problem analyzed.[14] Using different

[13] Rubin and Thomas (1992) show that it is possible to estimate these propensity scores based on the vector of observable characteristics.

[14] All these methods use the propensity score of each treated observation and propensity scores of untreated (control) observations. Nearest-neighbor selects the single control observation with the closest propensity score, and "without replacement" refers to the fact that any control observation can be matched to only one treated observation. Five-nearest neighbors uses more observations from the control group and allows replacement. The radius method includes all nearest neighbors within a maximum radius (referred to as the caliper). The kernel and local-linear matching algorithms each calculate a weighted average of all observations in the control group and assign higher weights (which differ between these two methods) to control observations closer to the treated observation. 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. But this method 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 since it does not place greater weight on closer matches. Local-linear matching has several advantages over kernel

matching methodologies is important to be able to assess which technique performs best as well as to assess the robustness of the results/ This is especially important because even if several techniques all satisfy the necessary tests, the significance of key results can still depend on the matching method used and corresponding construction of the control group.

There are several criteria and tests that can be used to evaluate the performance of matching algorithms. To begin, it is useful to compare the differences in the mean propensity scores between the treated observations and both the untreated observations and the control group observations. By construction, the difference in means should be larger between the treated and untreated groups than for the treated and control groups, but the reduction in means after constructing the control group is a useful exercise to gauge the relative performance of various matching methods and extent of selection bias before the matching. Another check is whether the propensity scores of treated observations are between that of the minimum and maximum propensity scores for the untreated observations (which is called "on support"). If the propensity score of a treated observation is not in the range of any of the untreated observations, it is called "off support". We will report the number of off-support observations for each of the six policies and drop any such treated observations because the untreated observations are not useful comparisons for treated off-support observations. This procedure is also known as imposing a "common support condition" and can help reduce the effect of bad matches.

Another criterion to assess if a matching methodology is valid is to verify that the matching removed any significant differences in observable variables between the treated and control groups. This test of "balancing" or the "independence assumption" requires that:

$$\mathrm{D} \perp X \mid p(X). \qquad (4)$$

This is implemented by calculating the *t*–statistic of the difference in means for each variable between the treated and control group (as identified by one of the matching methods). A successful matching would remove any differences in observables between the treated and the control group.

matching, such as a faster rate of convergence near boundary points and greater robustness to different data design densities.

To conclude this section on the propensity-score matching methodology, it is useful to mention how this approach compares to the more familiar regression analysis.[15] Both matching and regression methodologies estimate the partial correlation of the treatment with the outcome variable conditional on the values of covariates. One difference between these two methods is the weighting of the covariate-specific differences between the treated and the untreated (i.e. control) groups. Some type of weighting is needed 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 that are most similar to the treated but were untreated. In contrast, in regression analysis, the greatest weights for the comparison group are placed on cells where the conditional variance of treatment status is larger; roughly speaking, those cells with equal likelihood of its elements being treated or untreated. These different weighting strategies can lead to large differences between regression and propensity-score matching results.

Propensity score matching also differs from regression analysis in its greater emphasis on modeling the policy change (i.e., fiscal tightening) rather than the specific functional form that links the policy change to the outcome (i.e., how fiscal tightening relates to asset booms). This reduces challenges related to simultaneity and choosing an appropriate lag length. It also easily incorporates the use of a large set of variables to estimate the propensity score. This is useful when there is only vague theoretical guidance on the set of variables to be included in the model.

Propensity score matching, however, also has several disadvantages relative to regression analysis. The requirement for a sufficient number of "similar" observations for matching can be difficult to meet when using an annual, cross-country panel, as is typical in macro and international economics.[16] Satisfying the balancing assumption, as discussed above, is also difficult in some macroeconomic studies, showing the challenges in creating a set of untreated observations that are not significantly different than the treated observations. Even when all of these criteria are satisfied, different matching methodologies can yield different results, so it is important to check for the robustness across the various matching methods to ensure that results are not just an artifact of the matching methodology. Finally, matching methodologies cannot

[15] Angrist and Pischke (2008, Chapter 3) and Imbens (2014) present excellent discussions of the similarities and differences between regression analysis and propensity-score matching, including their relative advantages.
[16] This is one reason why propensity-score matching has been more widely adopted in labor economics—where papers often have larger samples of individuals that provide more degrees of freedom for effective matching.

effectively estimate effects over long periods of time; even if the technique creates an accurate control group at the point that the treatment (policy change) this does not control for subsequent events that may make the treatment and control groups less similar over time.

b. Tests of Matching Methodologies

We use the estimates from the logit models in Table 3 to calculate propensity scores for each of the six policies (reserve accumulation, currency appreciation, interest rate increases, fiscal tightening, capital controls and macroprudential regulation) for each country in our sample for each year from 2002 to 2007. We then use these estimated propensity scores to match treated observations with control observations using the five matching algorithms discussed above.[17] Several statistics assessing the performance of the matching algorithms (and discussed above) are reported in Table 4. For each policy option, a first row reports the number of treated and untreated observations and the number that are "off support". The results in this table show that there are no differences across matching methods in the number of observations that are off support. These statistics show that the logit models do a good job in generating matches that are on-support for most of the major policy responses.

The next row in Table 4 reports the number of covariates failing the independence assumption after matching (and the number failing the independence assumption before matching in parentheses, and the total number of variables in the logit estimate after the colon). This is a critically important test to assess if a matching algorithm is valid. These rows show that the matching successfully removes any significant differences between the treated and untreated groups (as measured by the covariates in Table 3) at the 95% confidence level in every single instance. But before matching, there are statistically significant differences between the treated group and the untreated group; for 2 of 3 covariates in the case of capital controls, 3 of 5 for reserve accumulation, 5 of 9 for exchange rate appreciation, 4 of 10 for an interest rate increase, 6 of 7 for fiscal tightening, and 4 of 7 for macroprudential policies. As discussed above, these differences between treated and untreated means supports concerns that selection bias may affect a comparison of outcomes for the treated and untreated.

[17] 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 are not available for the all of the covariates needed to estimate propensity scores for all observations.

The effectiveness of these tests of the independence assumption is demonstrated for one case, fiscal tightening, in Table 5. This table presents the mean values for the treated group (μ_T), the control group for the entire unmatched sample ($\mu_{C,UM}$), and the control groups for the matched samples created using each of the five matching algorithms ($\mu_{C,M}$) for each of the covariates used to estimate the propensity scores for fiscal tightening. The table also reports the *t*-statistics for tests of the hypothesis that the mean of each variable in the treatment group is equal to the mean in the entire control group (H0: $\mu_T = \mu_{C,UM}$) and the matched control groups (H0: $\mu_T = \mu_{C,M}$). The six covariates for which there is a significant difference in means between the on-support treated observations and the entire set of untreated observations are highlighted. There are significant differences in the treated sample and unmatched control group for six of the seven covariates; the changes in U.S. interest rates, capital account openness, exchange rate peg, the change in real GDP growth, reserves-to-GDP, and the prior large increase in interest rates. As discussed above, this sample selection would bias estimates if any of these variables are correlated with the outcomes being analyzed. The right side of the table, however, shows that the sample selection bias is removed after matching for all cases.

To summarize, this analysis indicates that the matching algorithms perform well. They generate no to few observations that are off support, satisfy the independence assumption in all 30 cases, and generate mean propensity scores that are closer to the treated groups with less mean absolute bias. The results also provide minimal guidance, however, on which matching algorithm should be preferred as the base case for the analysis. All yield similar numbers of countries that are off support. Local-linear matching is somewhat more effective at yielding a mean propensity score closer to that for the control group, but also has a large absolute bias. Therefore, in the discussion of results, we will summarize results using all methods,

IV. Results and Discussion

Now that we have ascertained that the requirements to use propensity-score matching are satisfied, it is possible to use this methodology to compare outcome variables for when countries used these policies (the treated observations) with their matched control groups. More specifically, we test for any impact of the six policies (reserve accumulation, exchange rate appreciation, interest rate increases, fiscal tightening, capital controls and macroprudential regulations) on four outcome variables that capture various aspects of booms and their negative consequences. The outcome variables are: whether the country had a bank credit boom, an equity market boom, a systemic banking crisis, or an increase in non-performing loans (NPLs). The boom and crisis variables are measured as 0-1 dummy variables, with a value equal to 1 if the country had the undesirable outcome (a boom or crisis) while the NPL variable simply captures the change in NPLs.[18] These outcome variables are defined in more detail at the end of Appendix A.

To test for any significant effect of the six policies on these four outcome variables, we calculate the *average treatment effect on the treated (*ATT) for each policy on each outcome variable. The ATT is calculated by comparing the average value of the outcome variable for treated observations with the average value for the respective matched control observations. The timing of any impact of the policy response on these outcome variables could vary based on the exact policy or outcome. For example, an increase in interest rates could quickly affect equity markets, while macroprudential regulations which gradually increased capital requirements might only affect resilience of the banking system after a longer lag. Therefore, we assess the impact of each of the policies on outcome variables in the current year as well as in the following two years. We do not test for any longer-term effects as the propensity-score matching is less accurate over longer periods of time as it does not control for other intervening variables that could affect the outcome variable after the policy change.

The most straightforward way to characterize the effects of the policies over the different time periods is graphs of the ATT's. Even using this graphical summary, however, yields a large number of results: six policy responses by four outcome variables by five matching techniques by three time periods for a total of 360 results. Therefore, we will only report a sample of the results—using a range of different matching methodologies and focusing on estimates that are significant and fairly consistent across techniques.

Figure 2 shows estimates of the effect of each of the six policies on the probability of having a bank credit boom, using radius matching. Each bar shows the magnitude of the

[18] The bank credit boom variable is from Dell-Ariccia et al (2012) and a boom is defined as when a country's credit to GDP ratio is greater than 10 percent compared to a backward-looking measure. The equity boom variable is calculated so that a boom occurs when stock market capitalization (as a share of GDP) has grown by 40 percent or more over the past year (with the 40% threshold defined as occurring in 10% of the sample). The banking crisis dummy is from Laeven and Valencia (2012) and a systemic banking crisis occurs if there are significant signs of financial distress in the banking system and significant banking policy interventions. Non-performing loans are the ratio of defaulting loans to total gross loans, based on data from the World Bank's Global Financial Development Database.

estimated ATT since the change in the policy occurred (the treatment). Dark black shading in a bar indicates that the ATT for that year is significant at the 5% level and medium-blue shading indicates significance at the 10% level. The black line is the fitted line for the average treatment effect. The graphs indicate that substantial increases in interest rates, increased capital controls, and increased macroprudential regulations all reduce bank credit booms within a year. The estimated effect is only significant at the 10% level for capital controls, but is largest for interest rates (with the coefficient indicating that countries which sharply increased their interest rates experienced a 20% reduction in the occurrence of bank credit booms in the following year). A large accumulation of reserves, substantial fiscal tightening, and major currency appreciation all appear to increase the occurrence of credit booms over time, with varying degrees of significance, timing, and magnitude.

Figure 3 shows another example of the results and reports estimates of the effect of each of the six policies on equity booms. The magnitude of the estimates is smaller than for bank credit booms, potentially indicating that the six policies on which we focus are less effective at moderating equity booms than bank credit booms. Sharp increases in interest rates and capital controls continue to moderate the occurrence of equity booms, and this effect is now significant for interest rates (but not capital controls) but smaller in magnitude. Increasing macroprudential measures appear to immediately increase the probability of an equity boom—possibly indicating a positive confidence effect—but this moderates after a year.

Figures 4 and 5 show the effects of the six policies on negative consequences that can result from booms—banking crises and increases in non-performing loans (NPLs)—using local-linear and five-nearest neighbor matching, respectively. Figure 4 shows that sharp currency appreciations and major increase in interest rates can both significantly reduce the occurrence of banking crises (as could tighter fiscal policy—although the effect is not significant). Figure 5 shows that most policies tend to reduce non-performing loans within two years—although the effects are generally insignificant. The one exception is that sharp increases in interest rates tend to increase NPLs (although this is also insignificant).

In some cases, the sign or significance of the estimated effects of these different policies on the four outcome variables fluctuates based on the matching method utilized. Therefore, Table 6 summarizes the key results using all five of the matching methods. The table is color coded to facilitate readability. Green indicates that the estimated ATT improves the measured outcome variable (decreases the probability of having a bank credit boom, equity boom, banking crisis, or increase in NPLs), while red indicates a deterioration in the measured outcome. The results are only colored if the ATT is estimated to have a meaningful effect—defined as causing an increase or decrease in the occurrence of a boom or crisis of at least 5 percent. For the estimated effects on NPLs, a meaningful impact is defined as an increase or decrease of NPLs of at least 1 percent. Cells are left blank if the estimated coefficients are below these thresholds. If the estimated effect changes sign in different years and is above/below these thresholds, then any effect which is significant, occurs in year 1, or is substantially larger than other effects is recorded in the table. For example, as shown on the bottom-right graph of Figure 2, macroprudential regulations are estimated to initially lead to a small increase in NPLs and then a larger decrease in year two—and the last effect is recorded in the table. Also, stars in each cell denote significance—with * indicating significance at the 10% level and ** at the 5% level.

The table shows a number of noteworthy results. First, only some of the cells indicate significant effects. It is uncertain if this reflects that when these policies have been used, they have generally not been able to significantly moderate the occurrence of booms, bank crises and NPLs. Or, some of the insignificant results may reflect the limits of propensity-score matching with annual data and a fairly limited set of countries. Other macroeconomic papers which have used propensity score matching to examine similar questions and obtained more significant estimates generally used higher frequency data.[19] Higher frequency data not only provides more observations for matching and calculating control groups, but potentially even more important, allows a more precise estimate of the average treatment effects immediately after the policy change and before other factors could occur that dilute the effectiveness of the matching.

Even though many of the estimates in Table 6 are not significant, there are a number of noteworthy patterns in the signs and magnitudes of the estimates which provide some evidence of which policies are, and are not, most effective at moderating certain aspects of booms. For example, the top half of the table indicates that sharp increases in interest rates, and capital controls decrease the occurrence of bank credit booms and equity booms Macroprudential regulations also appear to decrease the occurrence of bank credit booms (although the effect can be lagged by a year), but do not appear to stem equity booms and may instead aggravate them. (It

[19] For example, Forbes, Fratzscher and Straub (2013) use monthly data and have much more precise and many more significant results. Forbes and Klein (2013) use quarterly data and also find a number of significant results for certain variables, but not for others.

is worth noting that this measure of macroprudential regulations does not differentiate between different types of policies, which could have varied effects.) Exchange rate appreciation appears to increase the probability of having both credit and equity booms, and fiscal tightening may increase the probability of having bank credit booms.

The bottom half of Table 5 also shows several noteworthy patterns on how the different policies could affect negative consequences of booms—banking crises and non-performing loans. Allowing a substantial exchange rate appreciation, sharply increasing interest rates, and fiscal tightening all appear to reduce the occurrence of bank crises, with the first two policies often significant. Fiscal tightening, reserve accumulation, exchange rate appreciation, and macroprudential regulation all appear to reduce NPLs. Increasing interest rates, however, appears to consistently increase NPLs.

Tying all of these results together suggests that no single policy can effectively address the various risks related to booms and their aftermath. The one policy that is estimated to most effectively reduce the occurrence of bank credit booms, equity booms, and banking crises is to sharply increase interest rates. But a sharp increase in interest rates also appears to increase NPLs and would have many additional effects not incorporated in this analysis, some of which may be incompatible with other objectives. Exchange rate appreciation is estimated to reduce the risk of banking crises and NPLs, but aggravates the risk of booms in both bank credit and equities. Macroprudential regulations, when measured broadly in our analysis, are estimated to reduce the occurrence of bank credit booms and reduce non-performing loans (with both benefits occurring after about a year), but may simultaneously increase the risk of an equity boom. These results based on our broad measure of macroprudential regulations must be interpreted cautiously as different regulations have been used in different countries with different levels of implementation—all of which would be expected to determine their effectiveness—and this degree of differentiation is not incorporated in our results.

Finally, the preliminary results in this analysis should be interpreted subject to a number of important caveats. First, many of the policies aimed at moderating booms may have effects after the two-year horizon studied in this paper. The propensity-score matching methodology, however, does not allow an accurate assessment of these longer term effects. Second, although many of the estimated policy effects are large in magnitude across different matching methodologies but not statistically significant. It is unclear if the lack of significance reflects policies that are ineffective at moderating booms or limits to the estimation technique. The limited degrees of freedom and use of annual time data make it difficult to estimate some effects with any precision. Third, the measures used to identify the various policies are broadly defined and do not capture important distinctions in the applications of these policies which may influence their effectiveness. For example, the measure of macroprudential regulations does not differentiate between different types of regulations, or include many of the newer policies which have received more attention over the last few years. Similarly, the measure of fiscal policy does not differentiate between packages focused on reducing expenditures versus increasing taxes. Finally, many of the policies examined in this paper could have additional effects—both positive and negative—which are not incorporated in the analysis. Policies may also have different degrees of effectiveness based on the country's characteristics. Any serious consideration of any of these policies to moderate booms would need to consider the full set of costs and benefits, as well as country-specific characteristics which could influence these tradeoffs and determine the effectiveness of the specific policy, and not just the limited variables studied in this paper.

V. Next Steps

This paper is still a work in progress. Some of the next steps that we hope to accomplish are:

- Compare the results reported above to estimates obtained using OLS
- Test for the effect of the 6 policy variables on additional outcomes—such as housing prices, leverage, etc; suggestions appreciated on other outcome variables available for a broad sample of countries
- Use different thresholds for policy variables to see if results differ if stricter or looser thresholds
- Use finer gradations for capital controls and macroprudential measures to see if specific types of instruments more/less effective
- Sensitivity tests using different specification in the first stage

VI. Conclusions

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Table 1: Contemporaneous Occurrences of Policies

	Reserves	**ExchRate**	**Int. Rate**	**Fiscal**	**CFM**	**MacroPru**	***Total***
Reserves	8	1	0	2	1	9	*23*
ExchRate		15	0	1	4	1	*23*
Int. Rate			11	2	4	2	*20*
Fiscal	1			14	2	3	*27*
CFM					14	9	*37*
MacroPru		1		2		26	*54*

Diagonal elements represent the number of occurrences of a large policy change with no other large changes in any other of the five policies in that year for that country.
Reserves: Increase in foreign exchange reserves
ExchRate: Exchange Rate Appreciation
Int. Rate: Rise in Policy Interest Rate
Fiscal: Fiscal Tightening
CFM: Capital Flow Management Measures (capital controls)
MacroPru: Macroprudential Measures
Sample: # Countries, 2002 – 2007,

Table 2: Lags and Leads of Policies

Diagonals: Number of times policies occur in consecutive years		**<u>Reading Down Columns:</u> Number of times policy at column head follows by 1 year policy in row**					
		Reserves	**ExchRate**	**Int. Rate**	**Fiscal**	**CFM**	**MacroPru**
<u>Reading Across Rows:</u> No. of times policy in row leads policy by 1 year policy in in column	**Reserves**	6	1	1	2	2	8
	ExchRate	0	4	2	2	3	4
	Int. Rate	1	1	3	4	3	2
	Fiscal	2	1	2	5	3	5
	CFM	0	2	6	2	5	8
	MacroPru	8	3	1	4	7	4

Reserves: Increase in foreign exchange reserves
ExchRate: Exchange Rate Appreciation
Int. Rate: Rise in Policy Interest Rate
Fiscal: Fiscal Tightening
CFM: Capital Flow Management Measures (capital controls)
MacroPru: Macroprudential Measures
Sample: # Countries, 2002 – 2007,

Table 3: Determinants of Policy Change

	Reserves	Apprec.	Interest Rate	Fiscal Policy	Controls	MacroPru	
VXO		0.22*** (0.07)			-0.04 (0.03)	-0.07** (0.03)	**Lagged Global**
ln(Commodity)		8.17** (3.34)					
Δ(US Interest Rate)		-0.009*** (0.003)	0.006** (0.002)	0.003** (0.001)			
ln(RealGDP/Cap.)	-0.60*** (0.17)		0.82** (0.39)				**(Lagged) Country Charac-teristics**
Ln(Commodity) × Exporter Dummy	0.81 (0.56)						
Cap.Acc't Openness			-0.81** (0.35)	-0.41** (0.17)	-0.41*** (0.12)	-0.61*** (0.13)	
Exchange Rate Peg				-1.26** (0.62)			
Δ(Real GDP Growth Rate)		0.25** (0.12)		0.26** (0.10)	0.16** (0.07)		**Lagged Time-Varying Country Specific**
CA / GDP			-12.84** (5.70)			-11.23*** (3.65)	
Reserves / GDP	5.63*** (1.11)	-2.39* (1.26)	4.23** (1.73)	2.71*** (1.02)		3.07** (1.20)	
Δ(Inflows / GDP)		-4.63** (1.45)	5.34*** (1.90)				
CPI Inflation		0.14** (0.06)				0.01*** (0.04)	
Δ(Priv. Credit)	0.13*** (0.04)						
Large Appreciation Dummy		1.36** (0.61)					
Large Interest Rate Rise Dummy			-1.31 (0.91)	1.38* (0.78)		-1.68 (1.20)	
Large Fiscal Tightening Dummy		1.88 (1.19)	-1.19 (0.73)				
Capital Control Dummy			1.55** (0.61)	-1.53 (0.92)			**Lagged Large Policy Changes**
MacroPrudential Dummy	0.92* (0.51)		-3.04*** (1.16)			1.33*** (0.41)	
Constant	0.82 (1.41)	-44.60*** (16.56)	-10.46*** (3.36)	-2.35*** (0.41)	-0.82 (0.53)	-0.47 (0.61)	
No. of Obs.	295	287	287	295	295	295	
Pseudo R^2	0.20	0.25	0.27	0.21	0.08	0.27	

Logit regressions for Large Reserve Accumulation; Large Exchange Rate Appreciation; Large Interest Rate Increases; Substantial Fiscal Tightening; Imposition or Expansion of Capital Controls; Imposition or Expansion of Macroprudential Policies
*, **, *** = significant at 90 percent level, 95 percent level, 99 percent level, respectively.

Table 4: Summary Statistics for Different Matching Algorithms

Policy Analyzed	Treatment Group	Unmatched Untreated	Control Group Based on Matching Algorithm				
			Nearest Neighbor	5 Nearest Neighbors	Radius	Kernel	Local Linear
Reserve Accumulation							
# Obs. (Off Support)	23	272	(1)	(1)	(1)	(1)	(1)
# Failing Indep. (Untreated):All			0 (3):5	0 (3):5	0 (3):5	0 (3):5	0 (3):5
Mean Propensity Score	697.7	403.1	548.2	565.6	542.0	542.9	571.5
Mean Absolute Bias		43.0	13.0	24.8	23.1	23.4	13.1
Exchange Rate Appreciation							
Obs. (Off Support)	23	264	(5)	(5)	(5)	(5)	(5)
# Failing Indep. (Untreated):All			0 (5):9	0 (5):9	0 (5):9	0 (5):9	0 (5):9
Mean Propensity Score[1]	520.8	414.9	330.0	368.6	388.4	387.0	381.2
Mean Absolute Bias		45.5	22.0	10.5	9.2	9.2	15.4
Interest Rate Rise							
Obs. (Off Support)	20	267	(3)	(3)	(3)	(3)	(3)
# Failing Indep. (Untreated):All			0 (4):10	0 (4):10	0 (4):10	0 (4):10	0 (4):10
Mean Propensity Score[1]	412.6	419.0	488.8	381.6	422.9	434.6	391.0
Mean Absolute Bias		37.3	8.0	8.3	5.4	6.4	8.4
Fiscal Tightening							
Obs. (Off Support)	27	268	(2)	(2)	(2)	(2)	(2)
# Failing Indep. (Untreated):All			0 (6): 7	0 (6): 7	0 (6): 7	0 (6): 7	0 (6): 7
Mean Propensity Score[1]	536.8	415.6	505.0	508.6	490.7	492.9	509.4
Mean Absolute Bias		44.6	18.0	10.5	8.5	11.0	21.2
Capital Controls							
Obs. (Off Support)	37	258	(1)	(1)	(1)	(1)	(1)
# Failing Indep. (Untreated):All			0 (2): 3	0 (2): 3	0 (2): 3	0 (2): 3	0 (2): 3
Mean Propensity Score	459.2	421.8	469.8	493.8	516.0	514.0	514.8
Mean Absolute Bias		42.6	1.6	3.9	5.6	5.0	5.9
Macroprudential Regulations							
Obs. (Off Support)	54	241	(6)	(6)	(6)	(6)	(6)
# Failing Indep. (Untreated):All			0 (4): 7	0 (4): 7	0 (4): 7	0 (4): 7	0 (4): 7
Mean Propensity Score[1]	650.1	371.8	500.0	509.1	487.4	479.9	495.7
Mean Absolute Bias		47.3	9.0	5.7	10.0	10.2	11.8

Obs. (Off Support): Treated observations on support (first columns), Total number of untreated observations (second column), number of treated observations off support for respective matching techniques (in parentheses).
 # Failing Indep. (Untreated):All: Number of variables failing independence assumption (i.e. mean is significantly different between treated and control group). First number is for matched controls, number in parentheses is for untreated controls, number after colon is number of variables in logit estimation.
[1]For Treated Group, Propensity score is for on-support observations.

Table 5
Fiscal Tightening: Means for Treatment & Controls

	Treated, All & On-Support		**Untreated**			**Nearest Neighbor**		**5 Nearest Neighbors**		**Radius**		**Kernel**		**Local Linear**	
	$\mu_{T,\,All}$	$\mu_{T,\,ON}$	$\mu_{C,UM}$	**t-stat**		$\mu_{C,M}$	**t-stat**	$\mu_{C,M}$	**t-stat**	$\mu_{C,M}$	**t-stat**	$\mu_{C,M}$	**t-stat**	$\mu_{C,M}$	**t-stat**
Δ(US Int. Rate)	64.8	67.0	-25.7	2.05		42.0	0.63	74.4	0.23	55.7	0.45	63.0	0.11	49.0	0.45
Cap.Acc't Open	0.76	0.77	1.42	2.42		0.37	0.95	0.45	0.78	0.59	0.28	0.66	0.27	0.24	1.25
Exch. Rate Peg	0.15	0.16	0.40	2.55		0.32	1.32	0.19	0.29	0.21	0.34	0.20	0.37	0.36	1.62
Δ(RGDP Growth)	1.69	1.17	-0.06	3.53		1.44	0.34	0.97	0.27	0.78	0.33	0.69	0.75	0.86	0.53
Reserves / GDP	0.26	0.23	0.15	3.32		0.21	0.28	0.19	0.70	0.18	0.46	0.21	0.38	0.23	0.03
Int. Rate Dummy	0.15	0.16	0.04	2.43		0.20	0.36	0.14	0.15	0.19	0.17	0.22	0.52	0.20	0.36
CFM Dummy	0.07	0.08	0.10	0.39		0.04	0.59	0.06	0.33	0.06	0.32	0.05	0.39	0.00	1.44

Note: Shading shows significantly different from Treated at 95% confidence level or better.

Table 6: Summary Results of Average-Treatment Effects

	Matching Method				
	Nearest Neighbor	**5 Nearest Neighbors**	**Radius**	**Kernel**	**Local-linear**
Bank Credit Boom Dummy					
Reserve accumulation			*	*	
ER appreciation					
Interest rate increases	*				
Fiscal tightening	**	*	**	**	**
Capital controls			*	*	*
Macroprudential regulations					
Equity Boom Dummy					
Reserve accumulation					
ER appreciation					
Interest rate increases			**	**	
Fiscal tightening					
Capital controls	*		*		**
Macroprudential regulations	**	**	**	**	**
Banking Crisis Dummy					
Reserve accumulation					
ER appreciation	**	**	**	**	**
Interest rate increases			**	**	**
Fiscal tightening					
Capital controls					
Macroprudential regulations					
Increased Non-Performing Loans					
Reserve accumulation	*				
ER appreciation					
Interest rate increases					
Fiscal tightening			*		
Capital controls					
Macroprudential regulations					

Notes: * indicates significant at the 10% level and ** at the 5% level. Green indicates that the estimated ATT improves the measured outcome variable (decreases the probability of having a bank credit boom, equity boom, banking crisis, or increase in NPLs), while red indicates a deterioration in the measured outcome variable. The direction of the effect is only colored if it is estimated to be greater than 5% or less than -5%, except for NPLs which uses a 1% and -1% threshold. Blank cells indicate that the estimated effect is below the threshold. If the estimated effect changes sign and is above/below the threshold in different years, then the square records whichever effect is significant or occurs in year 1.

Figure 1

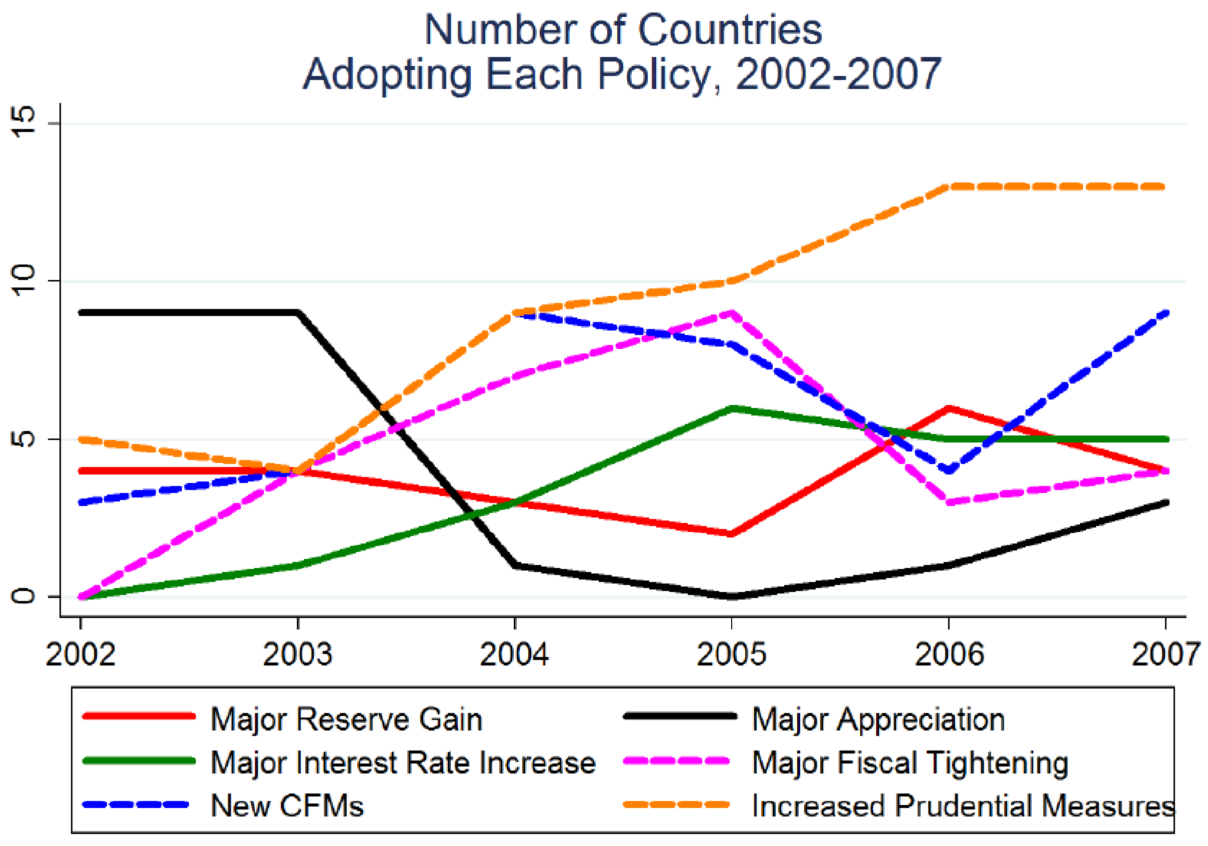
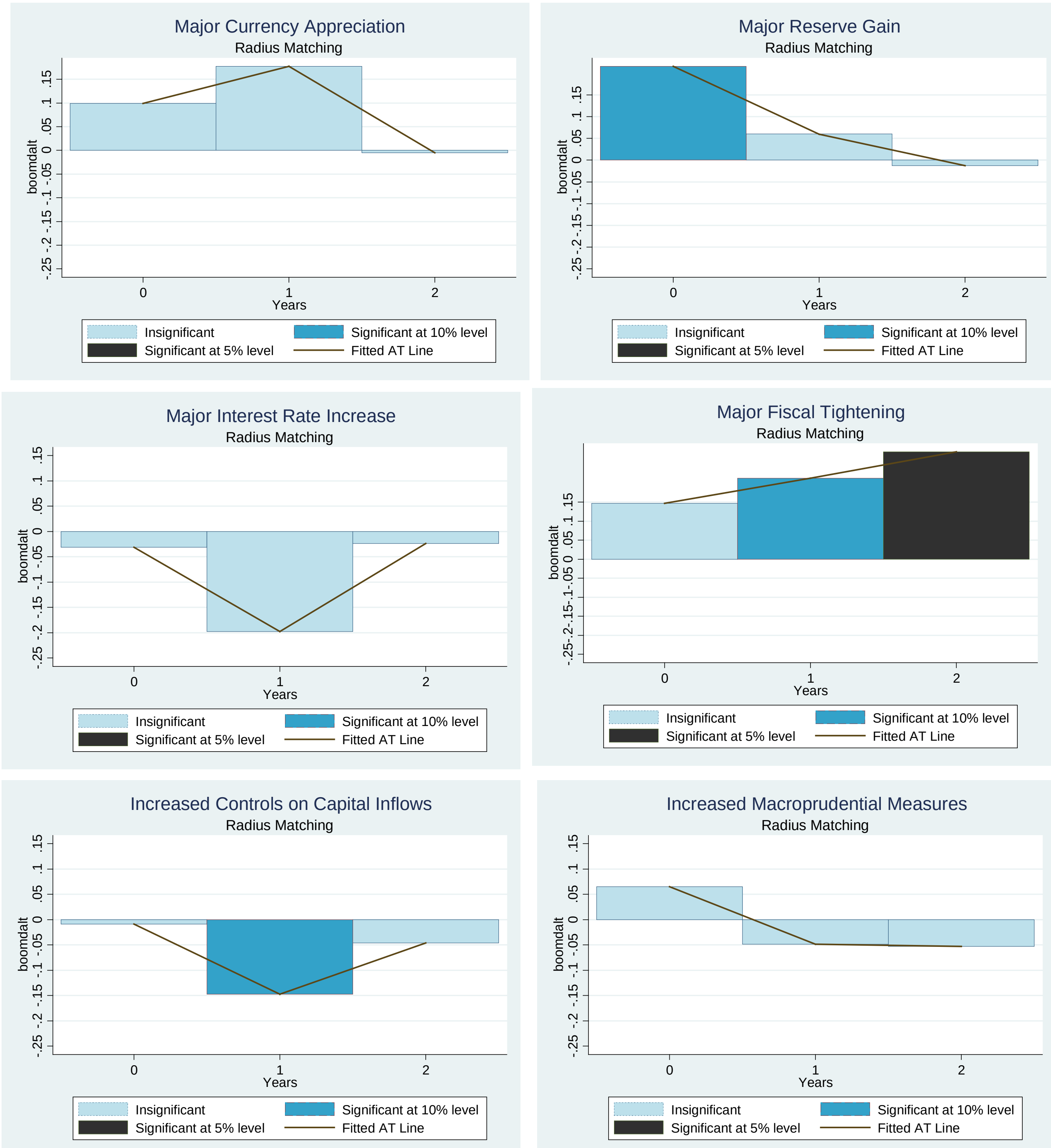
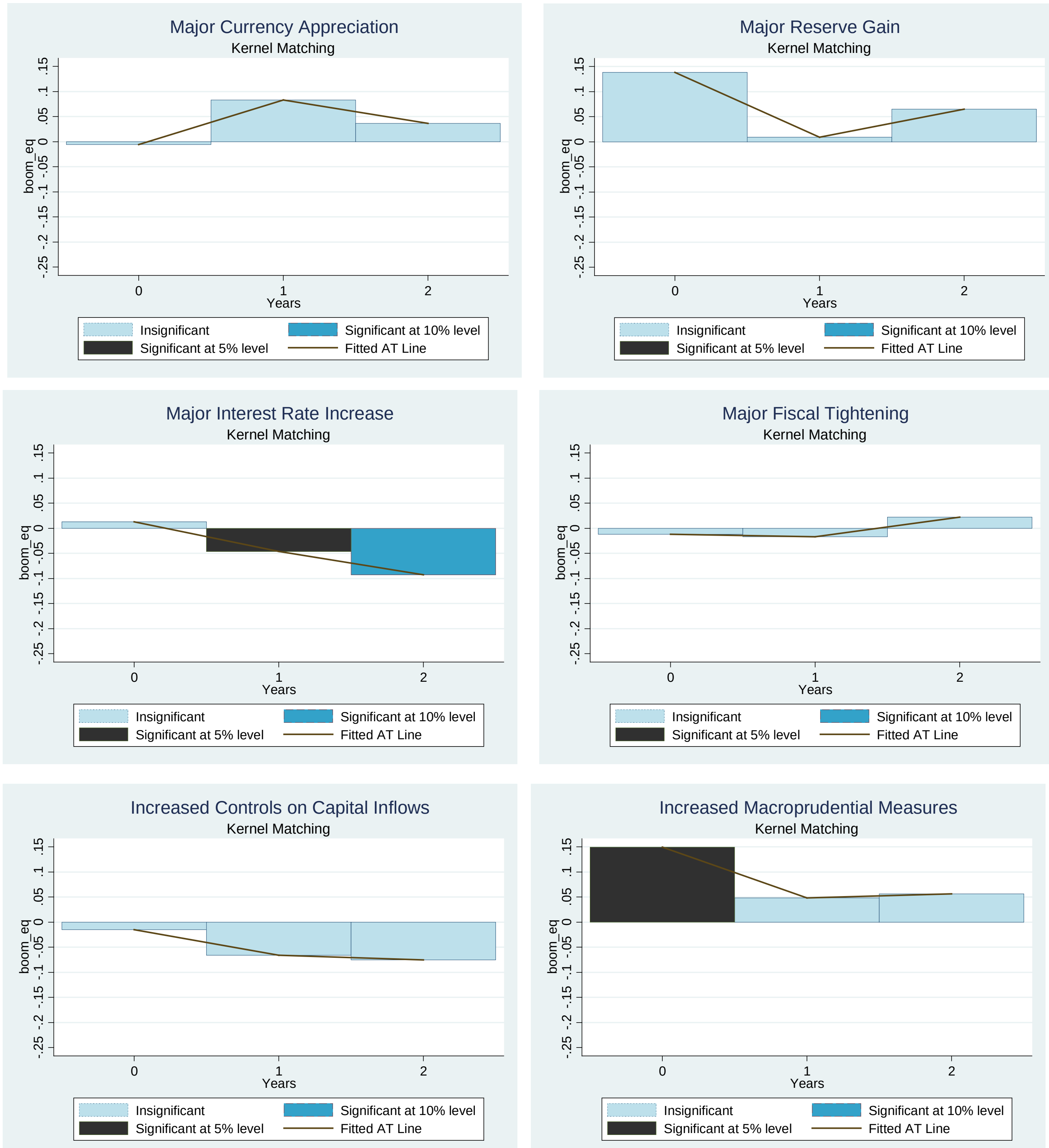

Number of Countries
Adopting Each Policy, 2002-2007
0
5
10
15
2002
2003
2004
2005
2006
2007
Major Reserve Gain
Major Appreciation
Major Interest Rate Increase
Major Fiscal Tightening
New CFMs
Increased Prudential Measures

Figure 2: Average Treatment Effects for Bank Credit Booms



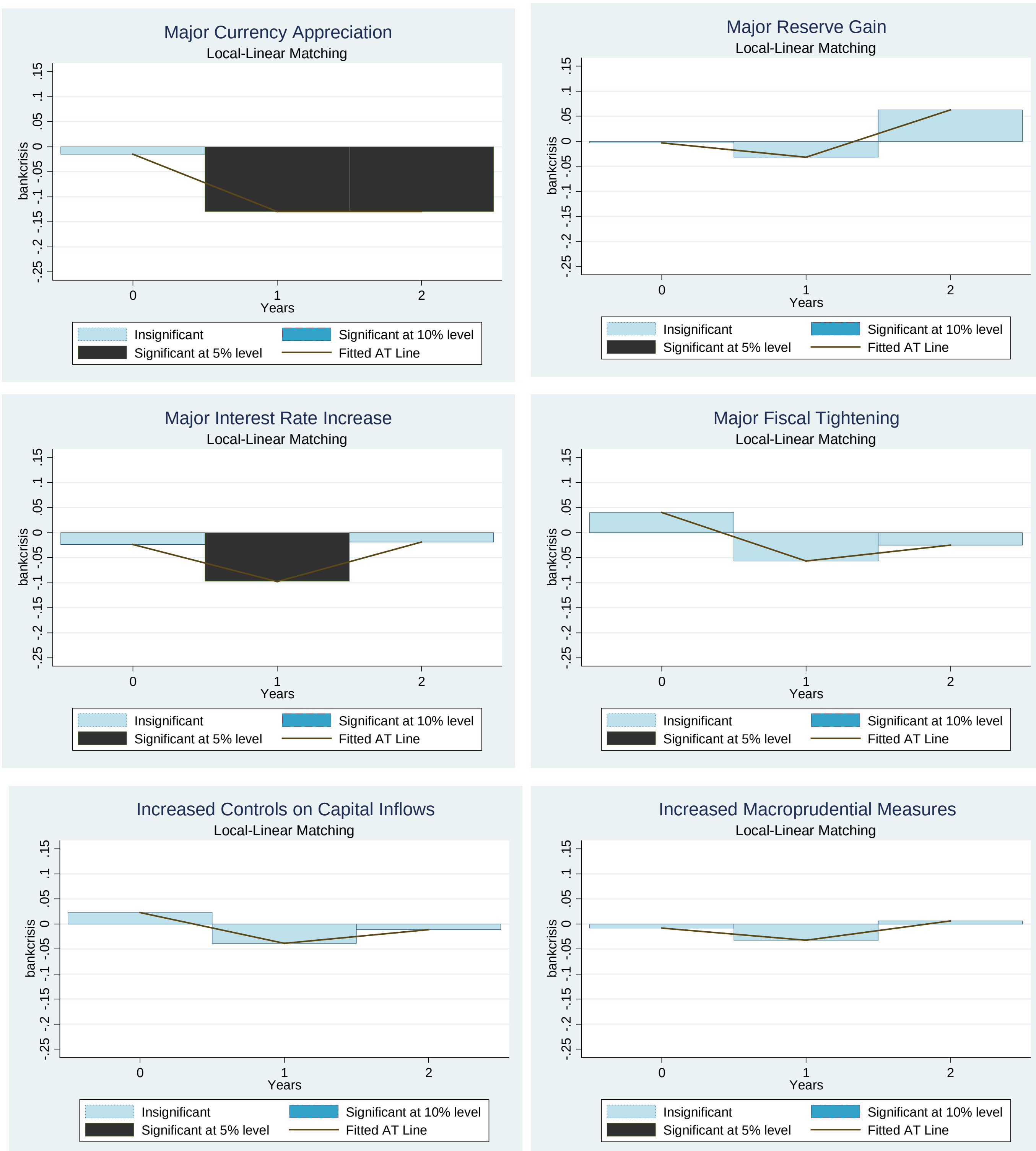
Notes: Reports ATT for each of the six policies on whether the country had a boom in bank credit from the time of the policy change (year 0) and the subsequent 2 years. Estimates obtained using radius matching.

Figure 3: Average Treatment Effects for Equity Booms



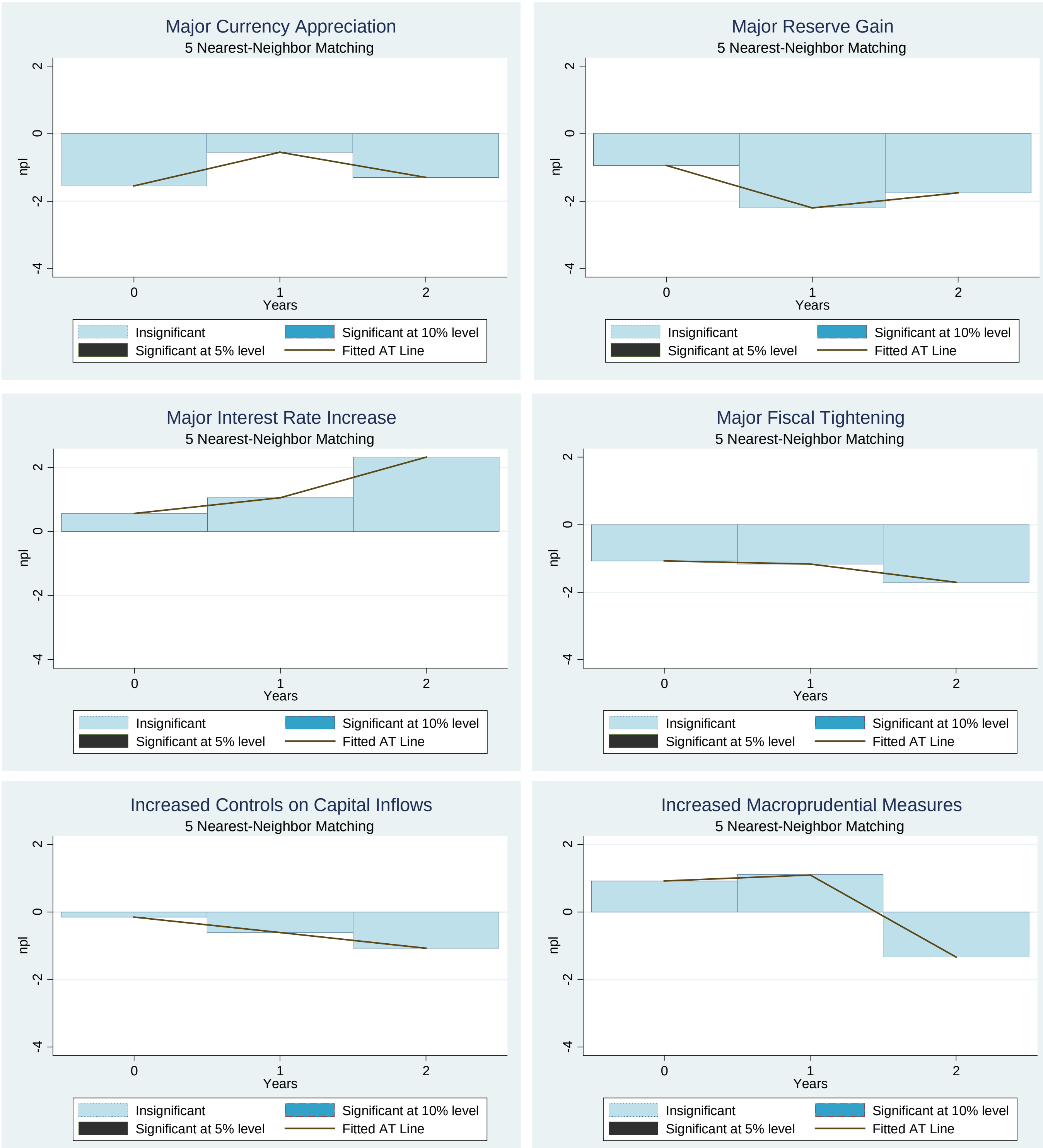
Notes: Reports ATT for each of the six policies on whether the country had an equity boom from the time of the policy change (year 0) and the subsequent 2 years. Estimate obtained using kernel matching.

Figure 4: Average Treatment Effects for Banking Crises



Notes: Reports ATT for each of the six policies on whether the country had a banking crisis from the time of the policy change (year 0) and the subsequent 2 years. Estimate obtained using local-linear matching.

Figure 5: Average Treatment Effects for Non-Performing Loans



Notes: Reports ATT for each of the six policies on whether the country had change in non-performing loans from the time of the policy change (year 0) and the subsequent 2 years. Estimate obtained using five-nearest neighbor.

Appendix A: Variable Definitions

Variable Name	Definition	Data Source
POLICY VARIABLES		
Reserves / GDP	International Reserves as a share of GDP. Reserves includes gold, IMF loans, SDRs, some SWFs, and drawn swap lines.	Reserves data from IMF, IFS CD-ROM, June 2013 (code ..1..DZF), 1990-2012 (Q), data for Vietnam from Haver
Exchange rate	Nominal effective exchange rate, end-of-period	IMF, IFS, accessed 07/09/13
Policy interest rate	Interest rate related to monetary policy for each country; is the policy interest rate if available; if not available is the short-term interest rate; written in basis points	Global Insight, accessed 10/1/13
Fiscal balance / GDP	General government structural budget balance as % of potential GDP (uses cyclically-adjusted deficit for Colombia)	IMF, WEO database provided by Daniel Leigh on 01/22/14
Capital controls	A dummy equal to 1 if a country has an increase in any of the 3 CFM measures over the last year: (1) an increase in the average level of controls on capital inflows from Klein (2012); (2) an increase in financial sector-specific capital controls; (3) an increase in FX-related prudential regulations. The last two components use the narrow measure in Beirned and Freidriech (2012). A country must have data on capital controls in Klein (2012) (in MK data) to be included in this measure.	Calculated using data from Klein (2012) and Beirne and Freidriech (2013). The Beirne and Freidriech (2013) data is an update of Ostry et al. (2012)
Macroprudential regulations	Any net increase in the 8 different components of housing-related and banking regulations as reported by Kuttner & Shin, with a net increase defined as when the sum of any changes in these regulations >0. The components captured are: Reserve Requirements, Credit Growth limits, Loan-to-value limits, DSTI limits, Risk Weighting, Provisioning, Exposure limits, and Liquidity requirements.	From Kuttner and Shim (2013), which is based on Shim et al (2013)
VARIABLES EXPLAINING POLICY CHOICES		
VXO	S&P 100 OEX Volatility Index, end-of-period.	Global Financial Data, accessed 07/11/13
Commodity price index	Economist All-commodity dollar index ; end-of period, expressed in logs.	Global Financial Data, accessed 07/11/13
Commodity price interaction with exporter dummy	Interaction of commodity price index Dummy equal to 1 if a country is a commodity exporter; commodity exporters defined as ((food exports + fuel exports)/merchandise exports) >30%	Calculated based on export data from World Bank's, WDI, accessed 10/8/13
Real GDP per capita	Real GDP per capita, expressed as logs	IMF WEO database, spring 2013

Institutions index	Log of an Institutional variable index calculated as the average of 6 ICRG institutional variables, with each weighted by the maximum value of the variable. The six components are: legal strength, law & order, investment profile, government stability, corruption and bureaucracy quality. Lower values indicating weaker institutions	Calculated using ICRG data compiled by the World Bank; Entire list of variables available (including definitions): http://www.prsgroup.com/VariableHelp.aspx ICRG Methodology: http://www.prsgroup.com/ICRG_Methodology.aspx
Capital account openness	Measure of capital account openness (kaopen), constructed as the principal components from IMF AREARs data, with higher value indicating greater openness	Chinn and Ito (2013), with data updated through end-2011 from their website
Exchange rate peg	Dummy variable equal to 1 if country has a +/- 2% peg.	Klein and Shambaugh (2013), updating Shambaugh (2004)
Euro area dummy	Dummy equal to 1 when a country is a member of the euro area (varies by year when join)	
% change in real GDP growth	Based on real GDP growth, annual basis, y-o-y growth, measured in constant prices	IMF, WEO, spring 2013
Current account balance / GDP	Current account balance expressed as share of GDP	IMF, WEO database, accessed 07/17/13
Reserves / GDP	International Reserves as a share of GDP. Reserves includes gold, IMF loans, SDRs, some SWFs, and drawn swap lines.	Reserves data from IMF, IFS CD-ROM, June 2013 (code ..1..DZF), 1990-2012 (Q), data for Vietnam from Haver
Change in capital inflows/GDP	Change in total Capital Inflows (the sum of direct investment in the reported economy, portfolio investment liabilities, and other investment liabilities), expressed as share of gdp	IMF, BOP as of 09/13
CPI inflation	% change in consumer price index relative to the previous year	IMF, IFS, accessed 07/09/13
Private credit growth	Percent change in private credit by deposit money banks and other financial institutions to GDP (in %) (uses bank credit if private credit NA, such as for Chile)	Thorsten Beck , Asli Demirguc-Kunt , Ross Eric Levine , Martin Cihak and Erik H.B. Feyen (2013). *Financial Development and Structure Dataset (updated April 2013). Available at: http://econ.worldbank.org/WBSITE/EXTERNAL/EXTDEC/EXTRESEARCH/0,,contentMDK:20696167~pagePK:64214825~piPK:64214943~theSitePK:469382,00.html*
Stock market capitalization / GDP	Stock market capitalization to GDP (in %)	Beck, Demirguc-Kunt , Levine , Cihak and Feyen (2013).
OUTCOME VARIABLES		
Bank Credit Boom Dummy	Dummy equal to 1 for bank credit boom. Booms occur when countries credit-to-GDP ratio is greater	*Policies for Macrofinancial Stability: How to Deal with*

	than 10 percent (using contemporaneous information) compared to backward-looking measure (over previous 10 years), with a country-specific, cubic trend. A boom is identified as deviation from trend by more than 1.5 times its standard deviation or annual growth rate of Credit/GDP exceeds 20 percent. Data based on 170 countries from 1960 – 2010.	*Credit Booms*, Giovanni Dell-Ariccia, Deniz Igan, Luc Laeven, Hui Tong, *IMF Staff Discussion Note* SDN/12/06, June 7, 2012. From Deniz Igan e-mail 4/7/2014 and attached bcps_boomlist.dta.
Equity Boom Dummy	Dummy equal to 1 if stock market capitalization to GDP has grown by 40% or more over the past year. 40% threshold selected as equal to about 10% of sample. Stock market capitalization based on total value of all listed shares in a stock market	Calculated using stock market capitalization to GDP data from World Bank's *Global Financial Development Database*, line GFDD.DM.01.
Banking Crisis Dummy	Dummy equal to 1 if country had a banking crisis. Annual data for 203 countries. A banking crisis is defined as systemic if two conditions are met: a. Significant signs of financial distress in the banking system (as indicated by significant bank runs, losses in the banking system, and/or bank liquidations), b. Significant banking policy intervention measures in response to significant losses in the banking system. The first year that both criteria are met is considered as the year when the crisis start becoming systemic. The end of a crisis is defined the year before both real GDP growth and real credit growth are positive for at least two consecutive years.	World Bank's *Global Financial Development Database*, line GFDD.OI.19. From Luc Laeven and Fabián Valencia, 2012. "Systemic Banking Crises Database: An Update", IMF Working Paper WP/12/163
Non-performing loans (NPLs)	Bank Nonperforming Loans to Gross Loans (%).Annual data for 203 countries. Ratio of defaulting loans (payments of interest and principal past due by 90 days or more) to total gross loans (total value of loan portfolio). The loan amount recorded as nonperforming includes the gross value of the loan as recorded on the balance sheet, not just the amount that is overdue.	World Bank's *Global Financial Development Database*, line GFDD.SI.02. Reported by IMF staff. Note that due to differences in national accounting, taxation, and supervisory regimes, these data are not strictly comparable across countries.

Appendix B:
Countries in Final Sample

Argentina	Korea, Republic of
Australia	Latvia
Austria	Lithuania
Belgium	Malaysia
Brazil	Mexico
Bulgaria	Netherlands
Canada	New Zealand
Chile	Norway
China	Peru
Croatia	Philippines
Czech Republic	Poland
Denmark	Portugal
Finland	Romania
France	Russian Federation
Germany	Singapore
Greece	Slovenia
Hong Kong	South Africa
Hungary	Spain
Iceland	Sweden
India	Switzerland
Indonesia	Thailand
Ireland	Turkey
Israel	Ukraine
Italy	United Kingdom
Japan	United States