When Do Regulators Lean Against the Wind?: The Political Economy of Implementing Macro-prudential Regulatory Tools: Preliminary results

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Early working draft containing **preliminary** results. Comments welcome.[1]

Abstract

In the aftermath of the global financial crisis, macro-prudential regulatory (MPR) tools, which aim to limit the build-up of systemic risk and the macroeconomic costs of financial instability, have gained widespread attention. An important element of MPR tools involves implementing counter-cyclical regulatory measures to dampen credit cycles. Yet the political dynamics of MPR tools are potentially complicated in that their implementation involves moving against market and public sentiment during boom periods as well as affecting who can obtain access to financing. The use of MPR tools can be highly and conspicuously distributional, thus potentially constraining their use and effectiveness. In many cases, the allocation of MPR responsibilities to hitherto independent central banks creates additional concerns about the nature of their accountability relationship with the rest of the political process and the public at large. To shed light on these critical issues, we provide the first cross-national statistical political economy analysis of MPR implementation. Our analysis assesses the relative importance of political credit cycles, institutional designs, and societal demands for credit tightening and easing. Contrary to our expectations, we find little evidence for political MPR cycles and weak evidence that central bank independence plays an important role in shaping MPR decisions. This finding has important implications for debates about possible tradeoffs between MPR democratic accountability and effectiveness.

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1 Introduction

In the wake of the global financial crisis, politicians, regulators, and central bankers have turned to a new macro-prudential regulatory (MPR) philosophy aimed at limiting the build-up of systemic risk and the macroeconomic costs of financial instability. As opposed to the pre-crisis micro-prudential focus on protecting the integrity of individual financial institutions, markets, and instruments, an important element of this philosophy prioritizes the creation of new counter-cyclical regulatory tools. Much faith is now being placed in the efficacy of these tools in preventing and mitigating the costs of the next financial crisis. Some of this faith is based on the perceived success of macro-prudential regulation in places such as East Asia and Canada.

However, we presently lack a coherent understanding of the context-specific political constraints that may shape what macro-prudential tools are actually used and how. These constraints may dramatically limit what tools are feasible in a given context and could lead to unintended negative consequences. For example, as part of the shift to a focus on macro-prudential and counter-cyclical regulation, central banks in a number of places, such as the United States, United Kingdom, and the Eurozone, have been given greater regulatory authority. These central banks have previously been seen as very successful in fighting inflation. However, it is uncertain if this success will transfer over to the newly created macro-prudential regulations. Macro-prudential regulation could be a much more politicised issue than monetary policy in these contexts. Regulators may be biased either towards non-intervention, because they would be subject to political pressure against tightening during a boom, or towards intervention, because they would face less criticism for puncturing a non-bubble than for failing to spot a real one. Perceived regulatory failures could end up eroding the reputations of central banks, thus damaging their ability to curtail inflation.

The existing literature says little about the salient political features that shape how regulators respond to these pressures. We thus need a better understanding of how the political economy context shapes macro-prudential regulatory systems. In this paper we make the first attempt at doing this by employing random forest classification to examine what political, institutional, and economic factors affect policy-makers' decisions to tighten monetary policy.

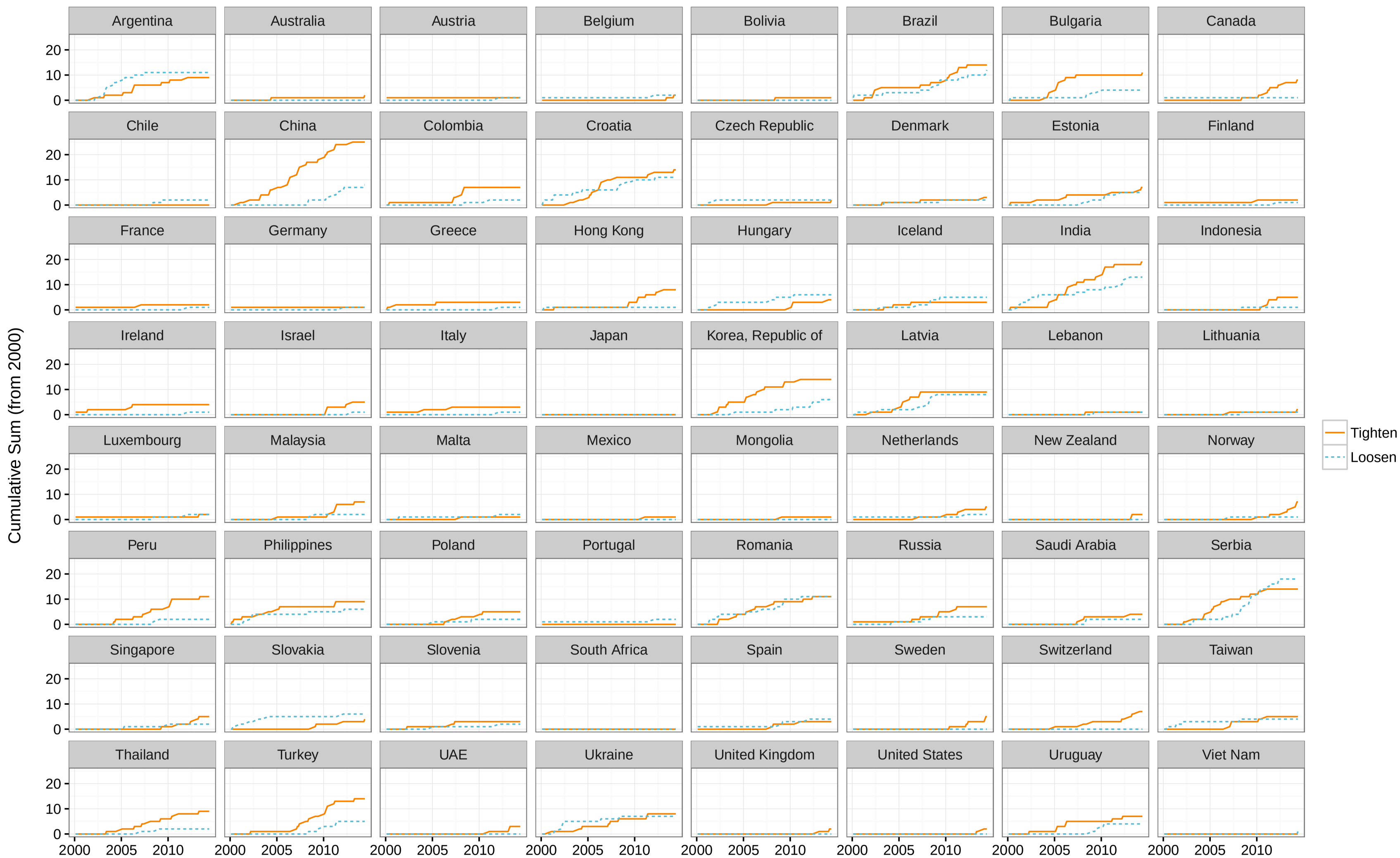
At this preliminary stage of our research we begin directly with the discussion of our empirical model including included variables and our theoretical reasoning for their inclusion, as well as estimation strategy and preliminary results. It is important to emphasise that we regard these these results as preliminary, not least of of all because considerable work needs to be done to find missing data for important cases that we currently can not include.

2 Dependent variables

To better understand how economic and political factors may influence macro-prudential regulatory decisions, we create two dependent variables derived from a new data set of MPR policies compiled by Reinhardt and Sowerbutts (2015). Aggregating a number of sources, mostly from IMF staff economists, and supplemented with additional hand-coded incidents, they generated quarterly indicators of MPR tightening and loosening for 64 countries between 2000 and 2014. These indicators are binary measures of tightening and loosening decisions for a range of individual MPR instruments including lending standards, reserve requirements, capital regulation, risk weights, underwriting standards, profit distribution, and loan to value ratios.

The use of some of these policies is rarely observed in the sample. So, we aggregated the data into a binary variable that we use as our dependent variable. The variable captures if a country took an action that Reinhardt and Sowerbutts (2015) classified as **MPR tightening** in a given quarter. This variable equals one for each country-quarter that any macro-prudential regulation was tightened and zero otherwise. Figure 1 shows the cumulative sum (from the year 2000) of these policies' use in each country-quarter in our sample. See also Figure A-1 in the Online Appendix for an aggregated view of the policy trends across all countries. We also created a similar variable of **MPR loosening**. However, there were many fewer observations in our full sample when considering missing-ness of the explanatory variables so we do not discuss results from models with this variable in the main text. Please see the Online Appendix for details.

Figure 1: Cumulative Decisions to Loosen and Tighten Macro-prudential Regulatory Policy (from 2000)



3 Potential explanatory variables

There are a number of economic and political factors that may affect decisions to tighten and loosen macro-prudential regulation. Note that we discuss all of the variables we examined even those we ultimately did not include in the estimation models shown below. Typically variables were excluded due to high missing-ness. Please see Figure A-2 in the Online Appendix for an overview of the missing values.

3.1 Economic conditions

Governments may feel a need to tighten macro-prudential regulation when asset prices are rising rapidly. **Residential property prices** are a key set of asset prices that macro-prudential regulation may be used to respond to. Measuring national-level residential property prices such that they can be compared across countries is notoriously difficult (see Scatigna, Szemere, and Tsatsaronis 2014). We use the 57 national series that were selected by the Bank of International Settlements (BIS) with the aim of being comparable across countries (Bank of International Settlements 2016). These indices are at quarterly intervals and in terms of real year-on-year percentage changes. We focus on the change in property prices, as differences in the price level can be caused by complex sets of idiosyncratic long-term factors that do not indicate systemic difficulties.

Similarly, governments may be more likely to tighten when there are credit bubbles so as to head off unsustainable lending that would lead to a full blown crisis. To test for this, we gathered data from the BIS on quarterly **credit provided to the non-financial sector** as a percentage of GDP. We used this data to create a variable of year-on-year credit provision changes. As with housing prices, we focus on changes to credit provision.

As macro-prudential regulation is broadly an attempt to strengthen financial markets, it is important to include the financial market stress policy-makers perceived in real-time. To do this we use the **FinStress** measure from Gandrud and Hallerberg (2015). They created a real-time indicator of financial market stress for over 180 countries between 2003 and 2011 using a text analysis of *Economist Intelligence Unit* monthly country reports. The variable ranges from zero (low stress) to one (high stress). We converted this monthly variable to country-quarter averages. We do not include FinStress in the models below because it shrinks the time period of our sample and does not substantially change the model results. In models that do include FinStress, countries are more likely to tighten MPR when FinStress is lower. This is consistent with an approach to macro-prudential regulation that aims to prevent trouble in the future.

We examined a number of other economic indicators from the World Bank's Development Indicators (WDI, World Bank 2016).[2] These included **GDP growth**. Policy-makers may use MPR tools when growth is rising rapidly. Additionally, from the WDI, we include **inflation rate** as a control. In models shown in the Online Appendix we include **domestic credit growth**–as in the main text we opt for the BIS measure of credit change as this was available on a quarterly basis. All World Bank Development Indicators are recorded at the annual level.[3]

3.2 Monetary policy environment

Policy-makers may turn to macro-prudential tools when they lack the monetary policy tools needed to constrain bubbles. To test this we included annual average standardised **central bank policy interest rates**. This data is from the IMF's International Financial Statistics (International Monetary Fund 2016). Perhaps countries with already high policy rates–and thus little room to maneuver are more likely to turn to MPR tools. We also used this variable to create a measure of central bank policy interest rate year-on-year percentage change. This variable allows us to examine if the rate of monetary policy change, not just the level, may be important. It could be that macro-prudential and monetary policies are treated as complementary–countries could tighten monetary policy and macro-prudential regulation simultaneously to avoid or quell bubbles. Conversely, these policies may be treated as substitutes. This is an empirical question that we examine below.

The interest rate environment in the United States may also play an important role in macro-prudential risks that policy-makers could be responding to. If interest rates in the United States are low, then investors may search for yield elsewhere causing bubbles. So, we included the quarterly average of the effective US **federal funds rate**. This data is from the Federal Reserve Bank of St. Louis' FRED data base.[4]

We also examined whether or not a country's **exchange rate regime** impacted their propensity to use macro-prudential tools. Perhaps having a more fixed exchange rate regime would prevent policy-makers from using monetary policy to tame credit cycles. To examine this, we used the Ilzetzki, Reinhart, and Rogoff (2010) coarse exchange rate regime measure. Their measure has six categories, with higher values indicating more flexible exchange rate regimes. It is available through 2010. However, we did not find any meaningful results with this measure and do not include it with the estimates below.

[2]The indicator IDs are NY.GDP.MKTP.KD.ZG, FS.AST.DOMS.GD.ZS, and FP.CPI.TOTL.ZG, respectively. Note that we created the domestic credit growth variable by finding the year-on-year percentage change in domestic credit as a percentage of GDP.

[3]We also examined models with one year lags of these variables. In general these lags were not statistically significant.

[4]The FRED indicator ID is FEDFUNDS. Accessed April 2016.

Elected politicians may have incentives to not tighten macro-prudential regulation as this may slow economic growth in the short-term, hurting voters, even if it promotes stability in the longer-term. Countries with more **central bank independence** (CBI) could suffer less from such a time-inconsistency problem. Independent central banks were created under the rationale that they would not suffer from a similar electorally induced time-inconsistency problem in monetary policy-making the way that elected politicians do (McNamara 2002). So, we would expect that if such a time-inconsistency problem exists for politicians regarding MPR tools that countries with independent central banks would be more likely to tighten MPR. We use a standard measure of CBI first devised by Cukierman, Web, and Neyapti (1992) and recently updated through 2010 for about 80 countries by Bodea and Hicks (2015). It ranges from 0.12 to 0.95 in the sample. Higher values indicate more central bank independence. All countries in the Eurozone have the same CBI score. We assume that countries' CBI scores were constant from 2010 through the end of our sample in order to allow us to include important tightening events from later in the Global Financial Crisis.

Central bank independence should have a more important impact on tightening if the central bank plays a larger part in macro-prudential decision-making. To examine this, we included the central bank's and ministry of finance's (MoF) de facto involvement in macro-prudential decision-making. We would expect that more ministry of finance involvement would have the opposite effect of independent central bank involvement–i.e. arrangements with more MoF involvement are less likely to tighten and more likely to loosen. Ministries of finance are more attuned to politicians' political incentives and so voters' demands for easy credit. To test this, we used data on macro-prudential governance frameworks from Lim et al. (2013). Their **MaPP** (measuring central bank involvement) and **MoF** indices range from a low of zero where there is no involvement to four where these actors are primarily or solely responsible for MPR. Surprisingly, these measures were never statistically meaningful in our various estimation models either by themselves or in interaction with central bank independence. Lim et al. (2013) in fact found only very weak evidence that these arrangements affect MPR response times. Given that including these variables greatly reduces our sample, results from models with them are not shown below.

3.3 Removal pressures and economic ideology

It may be that politicians who are more accountable to voters with short-time horizons and who benefit from easy credit would be less likely to tighten macro-prudential regulation. Conversely, politicians in highly democratic countries may be more likely to make preventative MPR tightening moves as they could suffer at the ballot box if they are viewed as having incompetently handled bubble conditions. To examine these possibilities, we used **Unified Democracy Scores** (UDS) from Pemstein, Meserve, and Melton (2010) (updated through 2012). UDS scores are found using a Bayesian latent variable model of eleven commonly used measures of democracy. We employ the posterior mean estimates from their estimation model. The variable ranges from about -2.1 to 2.2 where larger scores indicate a higher level of democracy.

Building on the long established political business cycle literature (e.g. Nordhaus 1975; Drazen 2001) we examine whether electoral accountability not only affects MPR decision-making, but actually creates macro-prudential regulatory electoral cycles. Elected politicians may be more likely to loosen and less likely to tighten macro-prudential regulation if they are close to an **election**. Tightening would slow credit provision to the economy, which voters would dislike. To examine this possibility, we gathered executive election dates from Hyde and Marinov (2012).[5] These dates are for elections in democracies and autocracies. Not only would politicians loosen or avoid tightening in the immediate election quarter, but also in the quarters leading up to the election. As such, we created a binary executive election variable that was one in the election quarter and the three previous quarters. It was zero otherwise. Continuing with the logic of the policy cycle, we would expect that politicians tighten macro-prudential regulation after elections. They potentially do not have to face voters again for awhile and so can use the post-election period to stabilise the economy (which was possibly destabilised by their pre-election loosening). So, we also include a post-election period variable that was one for the four quarters after an election and zero otherwise.

Inequality may influence the implementation of macro-prudential regulation. Rajan (2012) and Calomiris and Haber (2014) suggest inequality is a root cause of credit booms in democracies, especially in societies with limited capacity or political will to implement redistributive policies. Faced with such conditions, democratic governments may aim to boost the consumption of lower-income households by manufacturing credit booms through less stringent financial regulation. Indeed, Piketty and Saez (2013) show that large increases in private debt before the Great Depression and Great Recession were associated with widening income inequality.

Politicians may prefer not to intervene and instead permit credit bubbles to inflate in order to sustain their popularity. To assess the influence of inequality, we draw on two standard measures of the Gini index, devised by Solt (2008) and later updated in Solt (2014). The measures–the market-income Gini index and the net-income Gini index, range from zero to 100, with higher values indicating greater income inequality. The measures respectively capture the income distribution before and after public redistributive policies are taken into account.

We use these measures to also assess the extent to which of public **redistribution** shapes MPR decisions. To do this we created an indicator of redistribution relative to market inequality (the difference between

[5] We used Version 4 of the data set.

the market-income and net-income Gini indices divided by market-income and multiplied by 100); that is, the percentage by which market-income inequality is reduced by redistribution. If regulators in societies with limited redistributive policies are susceptible to political pressure or incentives to accommodate easier credit conditions, then one would expect such countries to exhibit weaker tendencies toward macro-prudential tightening. Conversely, one might expect such countries to display stronger tendencies toward tightening to the extent to macro-prudential regulators show resolve in leaning against the wind of inequality-induced credit cycles.

Politicians' **economic ideology** might play a role in macro-prudential decisions. To test this we include the government executive's economic policy orientation from the Database of Political Institutions (DPI, Beck et al. 2001 updated through 2012). It is one for right-leaning governments, two for centre-leaning, and three for left-leaning. We never found any support for this variable, so results from models using it are not shown below.

4 Estimation model: random forests

Logistic regression is an obvious estimation method for examining our binary dependent variables. However, our data does not fit nicely into this modeling technique. Many of the explanatory variables are strongly correlated with one another, presenting issues of multicolinearity (see the Online Appendix for the correlation matrix of the key independent variables). We observe relatively few events compared to non-events.[6] This presents well known problems for standard logistic regression (King and Zeng 2001). We also have many predictors relative to the number of observed macro-prudential regulatory decisions. All of these issues point to the usefulness of an alternative modeling strategy: random forest classification (Breiman 1996; Breiman 2001).[7]

Random forest classification is a non-parametric method that allows us to include many correlated variables in the same estimation model (Jones and Linder 2015). Though previously rarely used in political science and political economy, random forests are increasingly relied upon (e.g. Gandrud and Hallerberg 2015; Hill and Jones 2014; Jones and Linder 2015; Muchlinski et al. 2016; Shellman, Levey, and Young 2013; Spirling 2012). Random forests builds on a method know as Classification and Regression Trees (CART). A CART algorithm starts with the complete data set (root node) and recursively partitions (branches)

[6]In the full sample there were 3,840 country-quarters, 355 observations of country-quarters with any MPR tightening and 205 instances of loosening. There are many fewer still when we stepwise delete observations with missing predictor values. A common way of addressing this type of situation is to use rare events logistic regression (King and Zeng 2001). Muchlinski et al. (2016) show that random forests outperform these types of models in prediction. See also the Online Appendix for the event counts in the modeling sample.

[7]We used the `randomForestsSRC` package for R (Ishwaran and Kogalur 2016) to estimate the models.

the observations into increasingly homogeneous groups on the predictor space based on their values of the predictor variables (see Muchlinski et al. 2016, 92). This creates a single classification tree. However, CART has the problem of building overfitted trees. Random forests help overcome this problem by finding multiple trees for bootstrapped samples of the data and then averaging over the trees in this "forest".

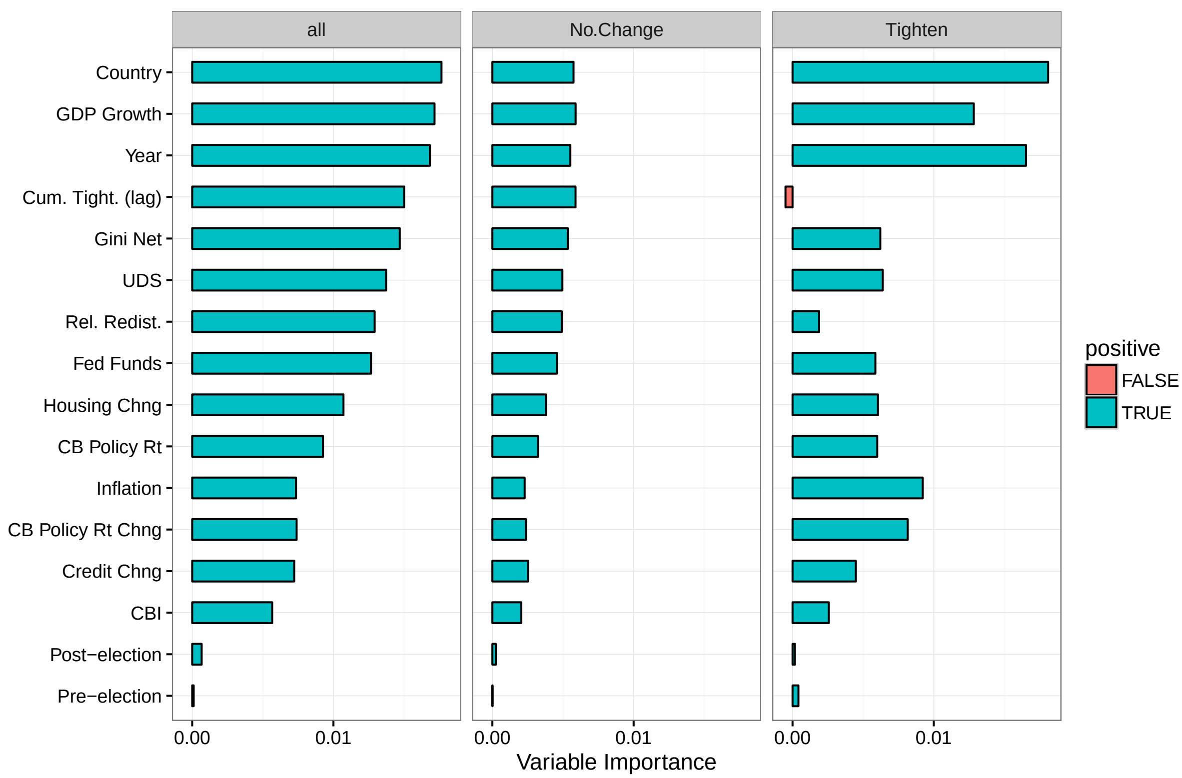
The method allows us to explore our data set of relatively rare events and find potential non-linearities and interactions among our correlated variables. This would be difficult in a logistic regression context. Given the large number of predictors relative to events and the predictors' high correlations with each other, we would not be able to use standard logistic regression to easily assess the predictors' relative importance in a single model. Instead we would likely need to stepwise include the variables in a series of models. Standard practice is to the use significance tests to examine if each variable's estimated effect is statistically significantly different from zero, controlling for only a subset of other predictors in the stepwise model. This approach would create highly model-dependent results. Random forests help us to avoid this problem because we can include many variables, even highly correlated variables, in the same model and estimate their importance for predicting MPR choices. The method also has high robustness to noise and outliers (Muchlinski et al. 2016, 93). As we will see, there are important outliers in the data that would produce highly unrealistic results under standard logistic regression assumptions.

Please see the Online Appendix for details about the cases and events included in the random forest models after stepwise deletion. We tested the possibility for pairwise interactions in our model, but did not find strong evidence for these. Please see the Online Appendix for details. Note that in addition to our random forest models, for comparison we ran the analyses using logistic regression models. The results are also presented in the Online Appendix.

4.1 MPR Tightening: preliminary results

We first discuss the random forest with macro-prudential regulatory policy tightening as the response variable. To assess each predictor variable's relative performance for classifying country-quarters as experiencing MPR tightening, we started by examining the variables' permutation importance in the random forest model. Permutation importance (Breiman 2001) is found by noting the prediction error on the out-of-bag (OOB) data–the data excluded from a single tree's bootstrap sample. For a given variable, OOB cases are then randomly permuted in the variable and the prediction error is recorded. The variable importance for the given variable is found by averaging the difference between the permuted and unperturbed error rates. Variable importances for the MPR tightening model are show in Figure 2.

Figure 2: Variable Permutation Importance for Classifying Macro-prudential Policy Tightening



Bars coloured by whether or not they have positive variable importance.

The country and year "fixed effects" have high variable importance, especially for decisions to tighten MPR. This suggests that there are other important unobserved factors that vary by country and time contributing to MPR tightening decisions. As we will see below when looking at partial dependences (see Figure 3), the year variable is clearly capturing features of the Global Financial Crisis not picked up by the other variables. GDP growth, the central bank policy rate, and inflation are also relatively very important. Conversely, elections, inequality, redistribution, and central bank independence are found to be relatively unimportant. Note that we also examined another measure of variable importance–minimum depth (Ishwaran et al. 2010). Results are shown in the Online Appendix. The minimum depth results are broadly similar to the permutation importance.

To get a sense of the estimated form and magnitude of the variables' effects, we found their partial dependences with MPR tightening. These estimates are shown in Figure 3.[8] Partial dependence is calculated by finding the average prediction from the random forest for each value of $X = x$ variable of interest over all other covariates in X using:[9]

$$f(x) = \frac{1}{n}\sum_{i=1}^{n} f(x,\ x_{i,o}). \tag{1}$$

f is the predicted MPR tightening decision. x is the variable for which we want to find the partial dependence and $x_{i,o}$ are the other variables (see Friedman 2000; Ehrlinger 2015b, 16). Given the binary dependent data the summand is the log of the fraction of total votes for the classification–the predicted logit–of y defined by:

$$f(x) = \mathrm{log}p_k(x) - \frac{1}{K}\sum_{j=1}^{K} \mathrm{log}p_j(x). \tag{2}$$

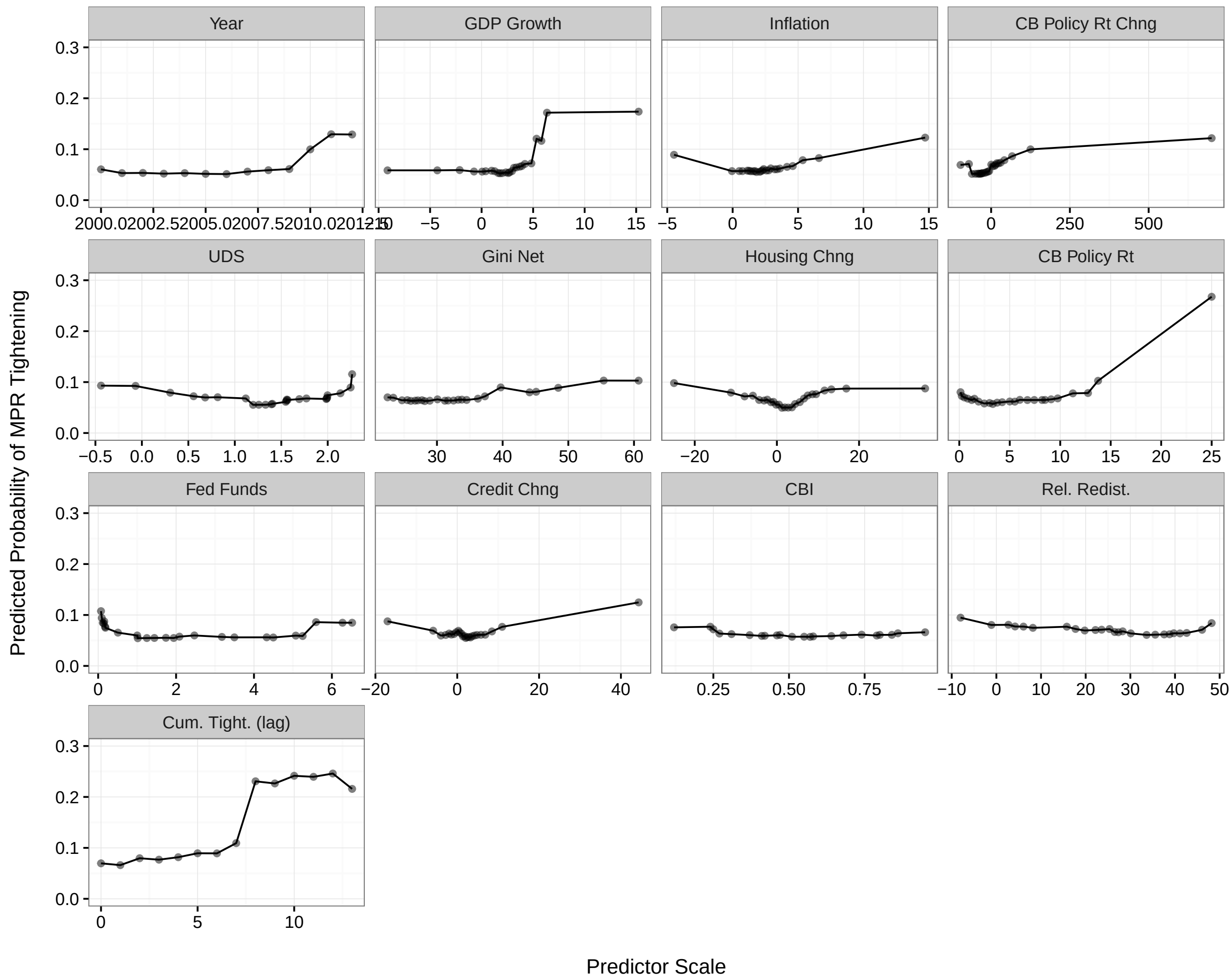
K is the number of classes in y. k is the predicted class. p_j is the proportion of votes for class j (Muchlinski et al. 2016, 99). We can think of partial dependence as the average predicted probability of MPR tightening for a value of one explanatory variable averaged within the joint values of the other predictors (Jones and Linder 2015, 8) or possibly in more familiar terms: the marginal effect of a variable on the probability of tightening (Muchlinski et al. 2016, 98).

Before examining the partial dependences in Figure 3, it is important to consider how each variable's degree of time variance shapes the magnitude of its estimated effect. All else equal, we would expect time-variant variables–such as GDP growth and credit provision–to have a larger impact on the predicted probability of

[8]We used the `ggRandomForests` package (Ehrlinger 2015a) for R to create partial dependence plots. We also use it to find minimum depths shown in the Online Appendix.

[9]Largely for computational reasons, for variables with many values predictions are made for a subset of the values.

Figure 3: Partial Dependence Plot for Macro-prudential Regulatory Policy Tightening



Variables with very low variable permutation importance are not shown. The "fixed effect" country variable is also not shown. Note that predictions are for policy change to be made per quarter.

tightening in a given quarter than largely time-invariant factors–such as CBI and democracy. Higher values of variables that change considerably over time will affect the quarterly probability more as the average effect of a high value is created from an average of relatively short high value periods that could closely correspond with events prompting a policy change. Time-invariant variables' affects are found by averaging over close to or exactly the entire observation period, so the partial dependence would tend to be lower, all else equal.

Countries with higher central bank policy rates have a higher probability of tightening. There does not appear to be much difference in the low probability of tightening if the policy rate is below about 10 percent. Above this point the predicted probability of tightening increases to between 10 to about 30 percent. This result appears to be largely driven by Brazil. Brazil tightened in nine quarters between 2002 and 2010 and was only one of four countries to have such a high policy rate at any point in the estimation sample. One of the other countries–Colombia in 2000–also tightened during a high policy rate period.[10] As such it appears that countries with less monetary policy room for maneuver, are more likely to resort to macro-prudential tightening. Interestingly, countries with increasing policy rates are also more likely to tighten. This suggests that macro-prudential tightening may be used as a complement to monetary policy tightening.

GDP growth appears to have a relationship with MPR tightening in a manner that we would expect from a model of policy-makers counter-cyclically tightening in an attempt to calm bubbles. There is a low probability of MPR tightening at growth levels up to about 5 percent of GDP. From this point, the probability of tightening rises, reaches almost 20 percent per quarter.

Housing price changes appear to have a U-shaped relationship with MPR tightening. When housing prices are stable–around a zero percent change–there is approximately a 5 percent probability of tightening. Large year-on-year quarterly housing price increases almost double the probability of tightening to a little under 10 percent per quarter. This is what we would expect from policy-makers using MPR tightening to quell property price bubbles. Interestingly, large housing price declines are also associated with tightening. The countries in the model where housing prices declined more than 5 percent and had tightening include Brazil, Canada, Singapore, and Spain. Brazil tightened reserve requirements. Despite falling housing prices, the others tightened lending standards following the start of the Global Financial Crisis. Given the wider crisis context in which these countries tightened, it may be that their policy moves were intended to prevent contagion from the external crisis which was already hitting economic growth and housing prices. We see a possibly similar Global Financial Crisis effect by looking at the partial dependence for the year variable. The predicted probability of tightening increases noticeably from 2009.[11] Changes in credit provision to non-financial institutions has a broadly similar, though shallower partial dependence.

Having already instituted an MPR measure greatly increases the probability of doing it again. We can see this by looking at the partial dependence for the cumulative sum of previous policy tightening measures. To a certain extent this finding likely reflects unobserved factors that incline a country to tighten. At the same time, it could also indicate that once MPR tools are put in a country's toolbox, they are more likely to be relied on again in the future.

We never found any reasonable evidence that elections play an important role in predicting MPR tightening (see the Online Appendix for further details). This suggests against the idea of a macro-prudential electoral

[10]Due to missing data, the Colombia event was not included in the estimation sample. The other two countries were Indonesia and South Africa.

[11]Note the flattening off of the probability of tightening in 2012. This may be an artifact of missing data. Only a few countries had data on all of the predictor variables in 2012. See Table A-1 in the Online Appendix.

cycle. Additionally, using various importance measures and by looking at its partial dependence we did not find much evidence that central bank independence is an important predictor. These two findings complement each other. If there is not a macro-prudential electoral cycle, then countries with and without independent central banks would not have meaningful differences in tightening choices because the anticipated effect of CBI would be to mitigate the non-existent MPR electoral cycles.

While we may not have found evidence that electoral cycles influence MPR tightening decisions, democratic accountability in general does seem to have some relationship with MPR tightening. Less democratic countries, measured by having lower Unified Democracy scores are more likely to tighten than those with higher scores, suggesting that politicians in less democratic countries are under less pressure to maintain credit levels to please voters. There are notable exceptions, however. The very democratic countries of Canada, Finland, Norway, Sweden, and Switzerland all tightened in the sample. To the extent that level of democracy affects tightening decisions, it seems to be possible to overcome it with other, potentially unobserved factors.

There is some evidence that inequality and redistribution change the probability of tightening, though not in the way we initially expected. Based on the previous literature, we anticipated that countries with higher inequality, especially even after redistribution, would be less likely to tighten in order to not alienate less advantaged supporters by reducing their access to credit. However, we found that countries with higher inequality (using both the market and post-redistribution measures, though only the latter is included in the model shown) are more likely to tighten. Similarly, countries with less redistribution are slightly more likely to tighten. Countries such as Brazil, Colombia, Peru, and Thailand all had Gini scores above 45 (on both measures) and tightened over multiple quarters. Almost all of these countries tightened by increasing reserve requirements. One possible explanation for these results that contradict our expectations, is that political pressures to not tighten in unequal societies make a country more likely to get into situations where they need to tighten. These countries also seem to tighten in such a way–increasing reserve requirements–that does not immediately impact borrowers. Though presumably such policies would tighten credit availability over the medium-term.

5 Conclusions

In this paper we have found a strong tendency for countries to make macro-prudential regulation in largely expected ways in response to economic conditions–particularly rapid GDP growth, housing prices and credit provision changes. Policy-makers also appear to tighten MPR more when monetary policy space is constrained by already high policy interest rates. Additionally, MPR seems to sometimes be used to complement interest rate increases.

The main novel empirical finding of our paper is that we did not find evidence for a macro-prudential regulation election cycle. This is surprising given our strong priors that policy-makers would use MPR tools to improve economic performance before elections. Similarly surprising and, complementary, is our finding that formal central bank independence and the central bank's position in the MPR framework does not seem to affect MPR decisions. This complements the electoral cycle finding. If there is no MPR electoral cycle, then there should be little difference in MPR between countries with more and less independent central banks.

These findings have potentially important implications for the design of macro-prudential regulatory institutions. Current thinking posits a tradeoff between MPR democratic accountability and effectiveness. Policy-making by more electorally independent actors has been purported to be more effective, while also producing policy that is less in keeping with (assumed short-sighted) voters' wishes. However, the initial evidence we found in this paper suggests that such a tradeoff may not exist as formal central bank independence may not be necessary for taking MPR tightening decisions.

It is nonetheless very important to note that these findings are based on a relatively gross conceptualisation of MPR as tightening and loosening and fairly limited data. More work is needed to understand if there are differences in terms of particular MPR tools when more data becomes available.

Online Appendix 1 Additional descriptive statistics

Figure A-1: Cumulative Sum of MPR Tightening and Loosening (whole sample)

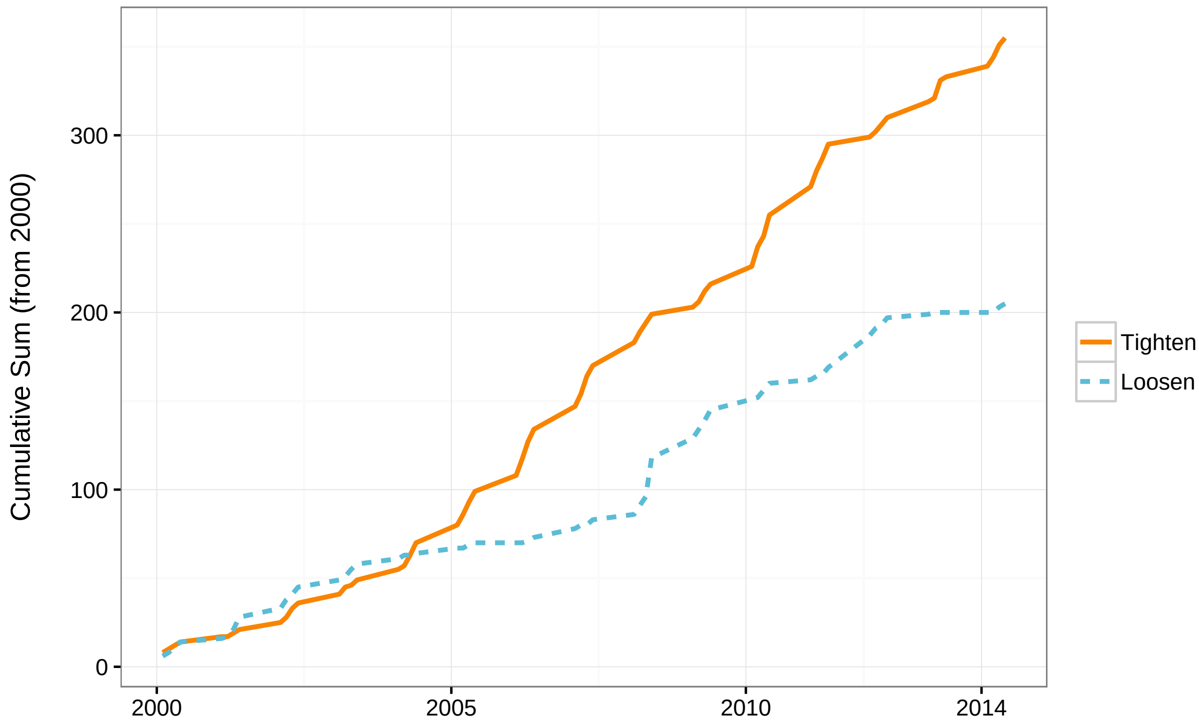


Figure A-2: Map of Missing Values for Key Explanatory Variables

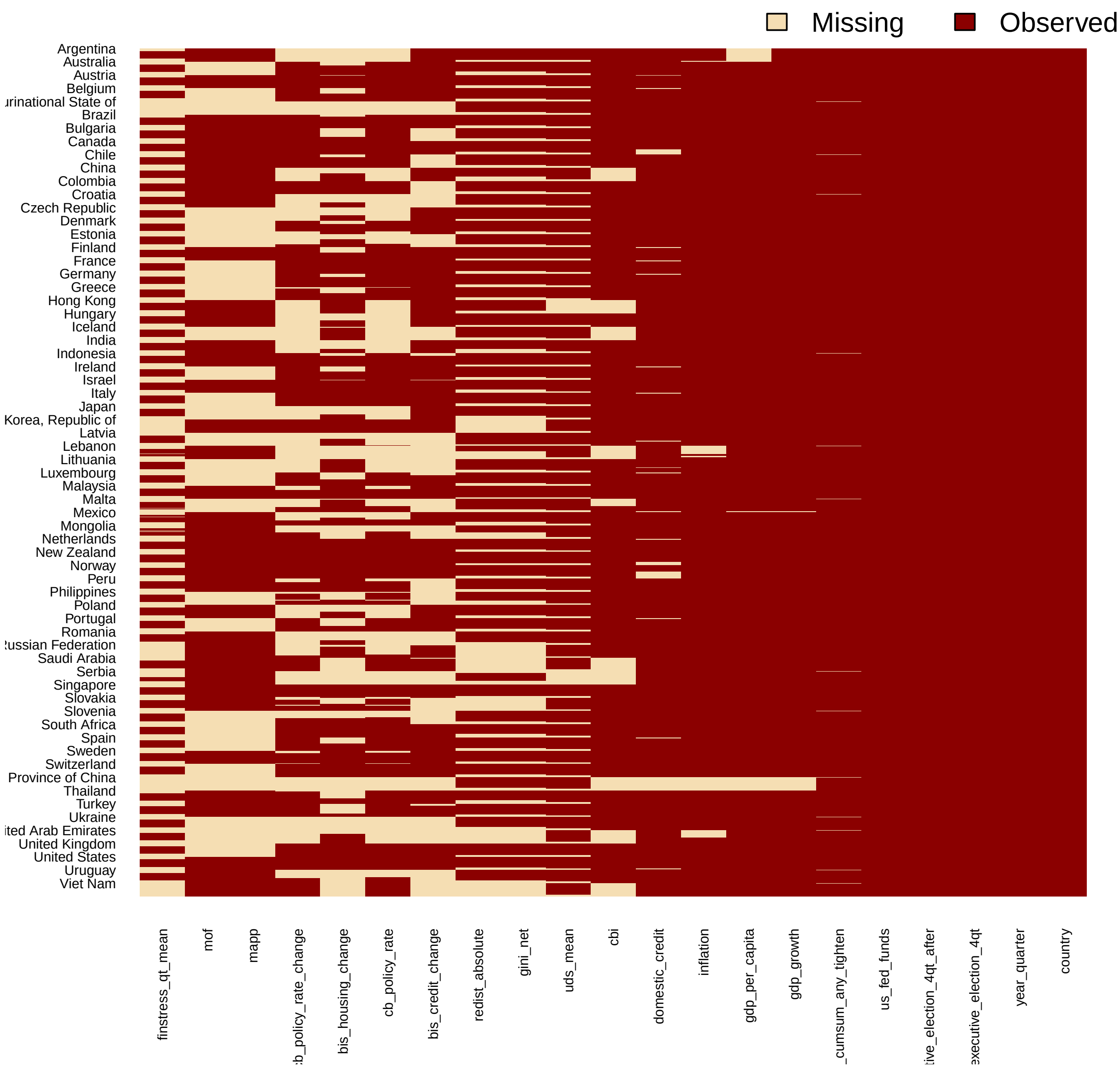


Figure A-3: Correlations between key explanatory variables

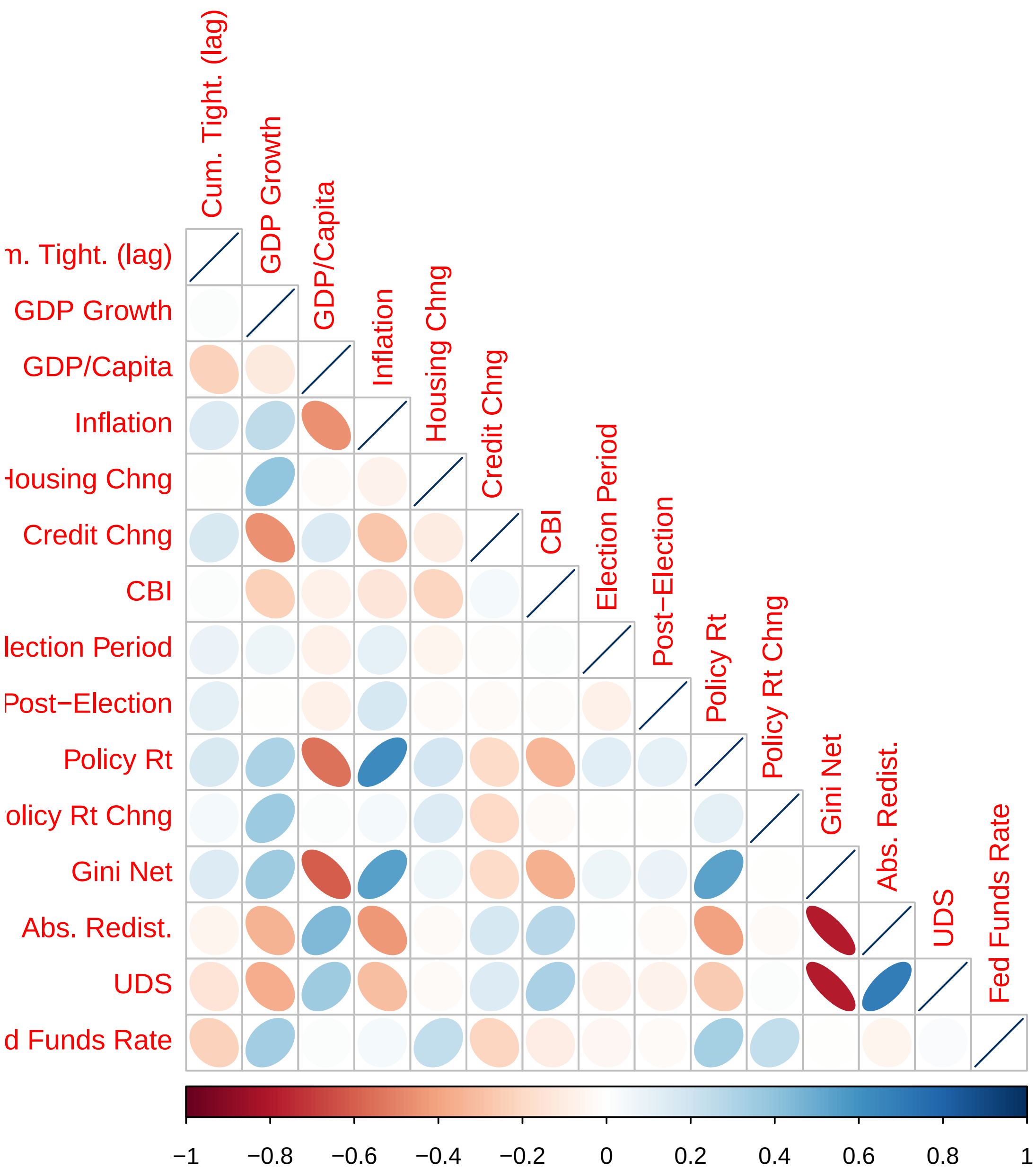


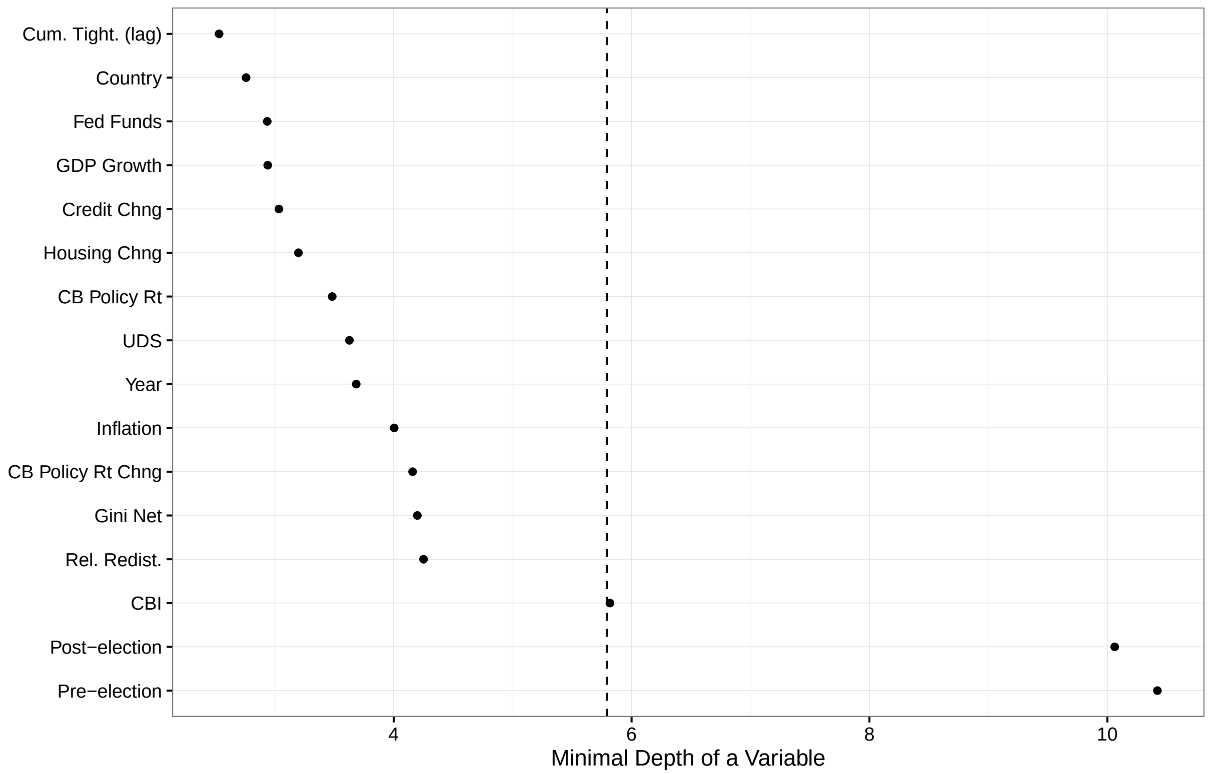
Table A-1: Country Quarter-Year Sample Included in the Random Forests After Deleting Cases with Missing Values

Country	First Year	Last Year
Australia	2004	2010
Austria	2001	2011
Belgium	2006	2011
Brazil	2002	2011
Canada	2000	2011
Denmark	2003	2011
Finland	2006	2012
France	2000	2011
Germany	2004	2011
Greece	2007	2011
Indonesia	2003	2012
Ireland	2006	2011
Israel	2000	2010
Italy	2000	2011
Luxembourg	2008	2011
Malaysia	2005	2012
Mexico	2009	2010
Netherlands	2000	2011
New Zealand	2000	2012
Norway	2000	2011
Portugal	2009	2011
Singapore	2000	2012
South Africa	2000	2010
Spain	2006	2011
Sweden	2003	2011
Switzerland	2000	2011
Thailand	2009	2010
Turkey	2011	2011
United Kingdom	2000	2012
United States	2000	2011

Table A-2: Number of Events and Total Observations for the Random Forests Estimation Sample

Tighten	Loosen	Total
60	24	1025

Figure A-4: Minimal Depth For Trees Classifying Macro-prudential Policy Tightening

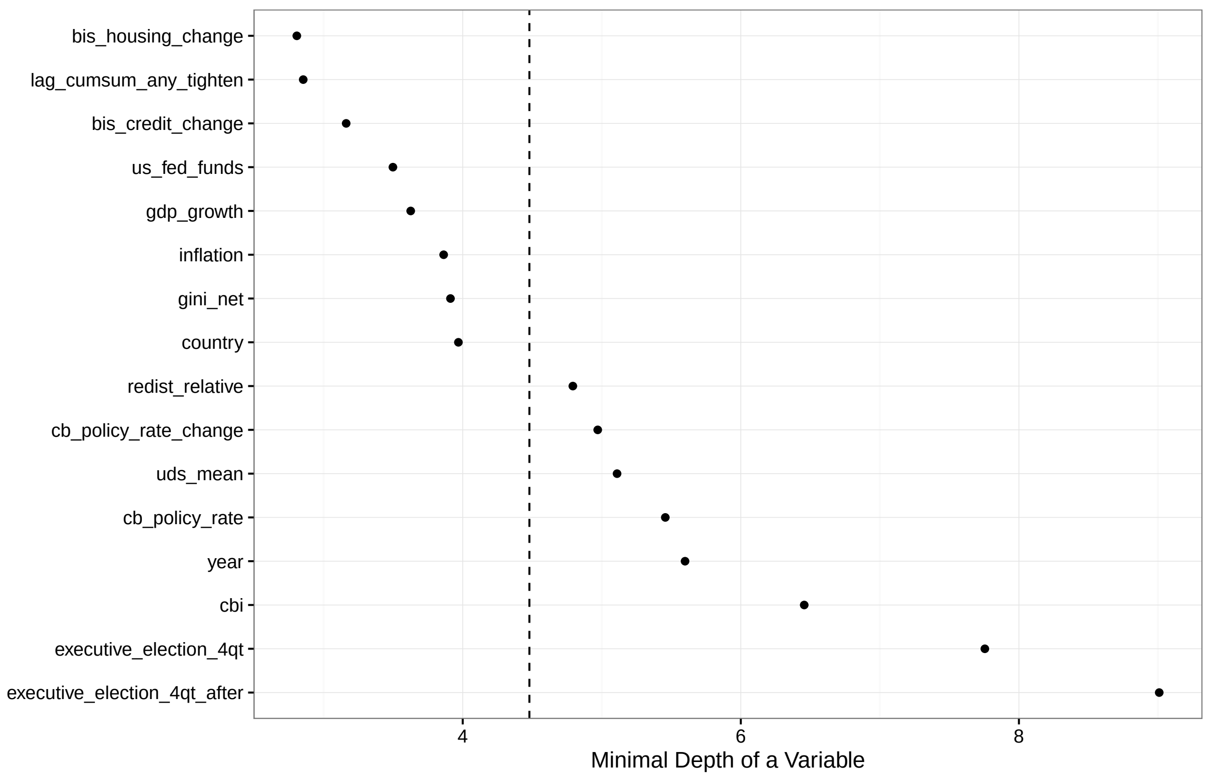


The dashed vertical line indicates mean minimum depth across the variables. Minimum depths below the mean depth are considered to be important in forest prediction.

Online Appendix 2 Minimal depth importance measure

Figures A-4 and A-5 show the minimal depths for each variable included in our two random forest models as an alternative to permutation importance. The assumption behind these plots is that variables have a higher impact on predicting MPR tightening if they more frequently split nodes closer to the "trunk" of the tree, i.e. the root node (Ehrlinger 2015b, 11). Lower minimum depths indicate that a variable is more important for predicting the MPR choice. Using the threshold rule developed by Ishwaran et al. (2010), minimum depth values below the mean minimum depth across the variables indicate variables that are important for predicting MPR choices. This threshold is represented in figures A-4 and A-5 by the dashed vertical line.

Figure A-5: Minimal Depth For Trees Classifying Macro-prudential Policy Loosening



The dashed vertical line indicates mean minimum depth across the variables. Minimum depths below the mean depth are considered to be important in forest prediction.

Online Appendix 3 Logistic regressions: Garbage cans

We also ran our models with classical logistic regression and logistic regression with the minimally informative priors suggested by Gelman et al. (2008).[12] The results from these analyses are shown in tables A-3 and A-4. We can see that the classical logistic regressions with garbage can model specifications–i.e. including all of the variables–produces highly unlikely coefficient estimates that are very unstable, depending highly on the model specification (not shown). Remember that in logistic regression a coefficient change of five moves a probability from 0.01 to 0.5 and likewise from 0.5 to 0.99. The central bank independence coefficient for the model predicting macro-prudential tightening is improbably large, especially given the random forest findings. The estimated intercepts are also nonsensically large. Even when using minimally informative priors to tame the coefficient estimates, we still end up with non-sensical intercepts. Many of the coefficients (e.g. GDP growth and year) are in highly unlikely directions given our theoretical priors and the random forest findings.

All of these issues are symptomatic of estimating logistic regressions on rare events with models that include many highly correlated predictors. Thus our need to look for alternative modelling strategies, such as random forests, for examining the relative importance of relatively many economic and political factors.

Online Appendix 4 Logistic stepwise variable inclusion

Following Achen (2005) we also ran the logistic regressions with minimally informative priors for MPR tightening by stepwise including the variables. See tables A-5 and A-6. Results tended to be similar in general direction to the random forest findings. For example, GDP growth was consistently statistically significant and positive, while the election timing variables were not statistically significant. The lagged cumulative sum of a country's tightening measures was conversely found to be negative whereas in the random forests having tightened more was much more likely to predict further tightening. This may be related to the non-linearity of the possible effect. Similarly BIS housing price change was not found to be significant possibly due to a nonlinear relationship with MPR tightening. In a models where the effect of housing price change is modeled with a second order polynomial (not shown) the effect was significant at the 10 percent level.

Interestingly, in models that did not include the variables from the Bank of International Settlements (Table A-5) central bank independence was consistently positive and statistically significant, i.e. having more central bank independence was more likely to lead to tightening. However, when we include the housing price change and credit change variables from the BIS (Table A-6), central bank independence–and other variables like

[12] We ran this analysis using the `bayesglm` function from the R `arm` package (Gelman and Su 2015).

Table A-3: Logistic Regression Estimates of Macro-prudential Tightening and Loosening

	Tightening MPR	Loosening MPR
(Intercept)	719.11^{**}	-293.66
	(236.74)	(5601.11)
lag_cumsum_any_tighten	0.35^{*}	-0.10
	(0.17)	(0.27)
gdp_growth	-0.26^{**}	0.19
	(0.10)	(0.13)
bis_housing_change	0.02	-0.07
	(0.03)	(0.04)
bis_credit_change	-0.10^{*}	0.08
	(0.04)	(0.06)
inflation	-0.14	0.06
	(0.12)	(0.16)
us_fed_funds	0.12	0.20
	(0.12)	(0.20)
gini_net	0.14	0.15
	(0.13)	(0.22)
redist_relative	0.05	0.11
	(0.06)	(0.09)
executive_election_4qt1	0.00	-0.52
	(0.63)	(1.05)
executive_election_4qt_after	0.08	-0.59
	(0.68)	(1.03)
cb_policy_rate	-0.03	0.02
	(0.12)	(0.20)
cb_policy_rate_change	-0.00	0.01
	(0.00)	(0.01)
cbi	-11.04^{*}	-4.17
	(5.46)	(10.35)
uds_mean	-0.86	-0.61
	(0.96)	(2.45)
AIC	397.55	241.24
BIC	619.51	463.20
Log Likelihood	-153.78	-75.62
Deviance	307.55	151.24
Num. obs.	1025	1025

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Country and year fixed effect estimates not shown.

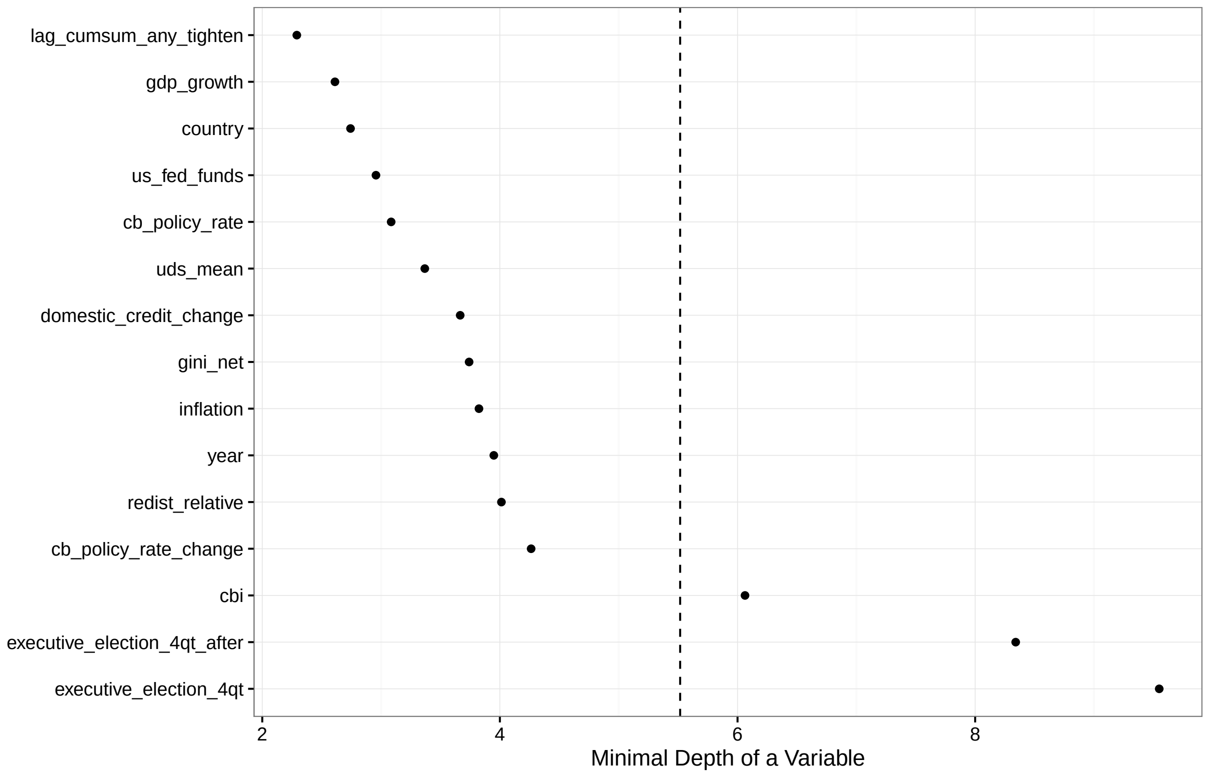
Table A-4: Logistic Regression (with minimally informative priors) Estimates of Macro-prudential Tightening and Loosening

	Tightening MPR	Loosening MPR
(Intercept)	616.05^{**}	-176.10
	(197.36)	(235.91)
lag_cumsum_any_tighten	0.13	-0.27
	(0.11)	(0.16)
gdp_growth	-0.22^{**}	0.16
	(0.08)	(0.10)
bis_housing_change	-0.00	-0.05
	(0.03)	(0.03)
bis_credit_change	-0.08^{*}	0.05
	(0.03)	(0.05)
inflation	-0.13	-0.02
	(0.10)	(0.13)
us_fed_funds	0.14	0.10
	(0.11)	(0.16)
gini_net	0.02	0.03
	(0.06)	(0.08)
redist_relative	0.02	0.02
	(0.03)	(0.04)
executive_election_4qt1	-0.14	-0.20
	(0.56)	(0.83)
executive_election_4qt_after	0.05	0.03
	(0.61)	(0.83)
cb_policy_rate	-0.10	0.02
	(0.09)	(0.12)
cb_policy_rate_change	-0.00	0.00
	(0.00)	(0.01)
cbi	-1.48	0.82
	(1.59)	(1.91)
uds_mean	-0.22	0.62
	(0.61)	(0.83)
AIC	479.98	318.06
BIC	869.65	707.73
Log Likelihood	-160.99	-80.03
Deviance	321.98	160.06
Num. obs.	1025	1025

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Country and year fixed effect estimates not shown.

Figure A-6: Minimal Depth For Trees Classifying Macro-prudential Policy Tightening (no BIS variables and using WDI domestic credit change)



The dashed vertical line indicates mean minimum depth across the variables. Minimum depths below the mean depth are considered to be important in forest prediction.

GDP Growth–is no longer statistically significant. Do to listwise deletion of missing values, including the BIS variables shrinks our sample considerably from over 1,000 country-quarters between approximately 500 to 700. To examine if the missing data may be driving the statistically insignificant result, we reran the models without the BIS variables, but subsetting the sample to include only observations where there was no missing data for them. This also produces a statistically insignificant CBI estimate (see model B7 in Table A-6). Note that we also found using the annual WDI domestic credit change variable (model A7 Table A-5) that CBI is significant with this measure of credit provision change. However, these results are not consistent with a random forests model excluding the BIS variables and including domestic credit provision change from the WDI (Figure A-6). More work is needed to robustly pin down the effect of CBI.

Table A-5: Stepwise Variable Included Logistic Regression (with minimally informative priors) Estimates of Macro-prudential Tightening

	A1	A2	A3	A4	A5	A6	A7
(Intercept)	-3.05^{**}	-3.44^{***}	-5.14^{***}	-5.16^{***}	-5.08^{***}	-4.10^{***}	-5.25^{***}
	(0.95)	(0.52)	(0.82)	(0.82)	(0.83)	(1.12)	(0.85)
lag_cumsum_any_tighten	-0.15^{***}	-0.19^{***}	-0.13^{***}	-0.13^{***}	-0.13^{***}	-0.09^{*}	-0.14^{***}
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
gdp_growth	0.17^{***}	0.06	0.13^{***}	0.13^{***}	0.13^{***}	0.21^{***}	0.13^{***}
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
inflation	0.08^{*}	0.04	0.03	0.03	0.03	0.02	0.03^{*}
	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
us_fed_funds	0.27						
	(0.16)						
finstress_qt_mean	-1.65						
	(0.92)						
cb_policy_rate		-0.05					
		(0.04)					
cb_policy_rate_change		0.00					
		(0.00)					
uds_mean			-0.15	-0.14	-0.16	-0.07	-0.61
			(0.33)	(0.33)	(0.34)	(0.36)	(0.36)
cbi			2.54^{**}	2.53^{**}	2.52^{**}		2.57^{**}
			(0.91)	(0.91)	(0.92)		(0.95)
executive_election_4qt1				0.17	0.06		
				(0.22)	(0.23)		
executive_election_4qt_after1					-0.46		
					(0.26)		
gini_net						0.01	
						(0.02)	
domestic_credit_change							0.00^{***}
							(0.00)
AIC	1138.66	1206.54	1500.61	1502.04	1500.36	1320.14	1391.59
BIC	1600.36	1683.66	1993.77	2001.14	2005.40	1805.08	1886.05
Log Likelihood	-486.33	-520.27	-667.30	-667.02	-665.18	-577.07	-611.79
Deviance	972.66	1040.54	1334.61	1334.04	1330.36	1154.14	1223.59
Num. obs.	1925	2318	2812	2812	2812	2547	2661

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Country and year fixed effect estimates not shown.

Table A-6: Stepwise Variable Included Logistic Regression (with minimally informative priors) Estimates of Macro-prudential Tightening–No BIS variables included

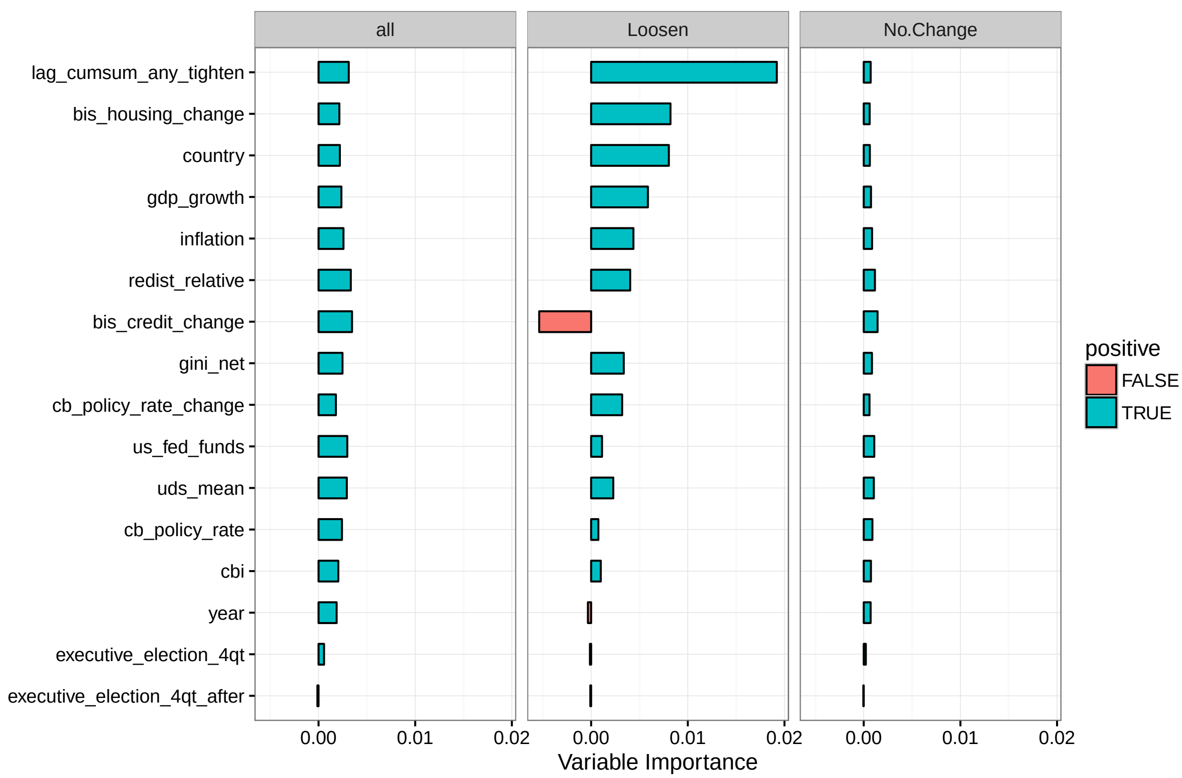
	B1	B2	B3	B4	B5	B6	B7
(Intercept)	-2.24	-4.47^{***}	-5.12^{***}	-5.11^{***}	-5.09^{***}	-4.44^{*}	-5.15^{***}
	(1.35)	(0.72)	(1.28)	(1.28)	(1.28)	(2.17)	(1.26)
lag_cumsum_any_tighten	-0.28^{**}	-0.25^{***}	-0.19^{**}	-0.19^{**}	-0.19^{**}	-0.26^{*}	-0.16^{**}
	(0.10)	(0.06)	(0.06)	(0.06)	(0.06)	(0.10)	(0.06)
gdp_growth	0.04	0.16^{*}	0.06	0.06	0.06	0.12	0.02
	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.09)	(0.06)
inflation	0.22^{*}	0.09	0.14	0.14	0.15^{*}	0.21^{*}	0.10
	(0.09)	(0.10)	(0.07)	(0.07)	(0.08)	(0.10)	(0.07)
us_fed_funds	-0.09						
	(0.24)						
finstress_qt_mean	-3.74^{*}						
	(1.70)						
bis_housing_change	0.01	0.02	0.01	0.01	0.01	0.02	
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
bis_credit_change	0.04	0.08^{**}	0.09^{***}	0.09^{***}	0.09^{***}	0.10^{***}	
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	
cb_policy_rate		-0.03					
		(0.07)					
cb_policy_rate_change		0.00					
		(0.00)					
uds_mean			-0.23	-0.23	-0.25	-0.10	-0.15
			(0.45)	(0.45)	(0.45)	(0.59)	(0.44)
cbi			1.39	1.38	1.37		1.70
			(1.43)	(1.43)	(1.43)		(1.42)
executive_election_4qt1				0.10	-0.02		
				(0.39)	(0.41)		
executive_election_4qt_after1					-0.48		
					(0.48)		
gini_net						-0.00	
						(0.05)	
AIC	493.02	767.49	677.31	679.33	679.98	524.65	687.60
BIC	904.65	1215.23	1120.98	1128.22	1134.09	953.32	1120.83
Log Likelihood	-161.51	-298.75	-253.66	-253.66	-252.99	-177.33	-260.80
Deviance	323.02	597.49	507.31	507.33	505.98	354.65	521.60
Num. obs.	937	1433	1366	1366	1366	1145	1366

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Country and year fixed effect estimates not shown.

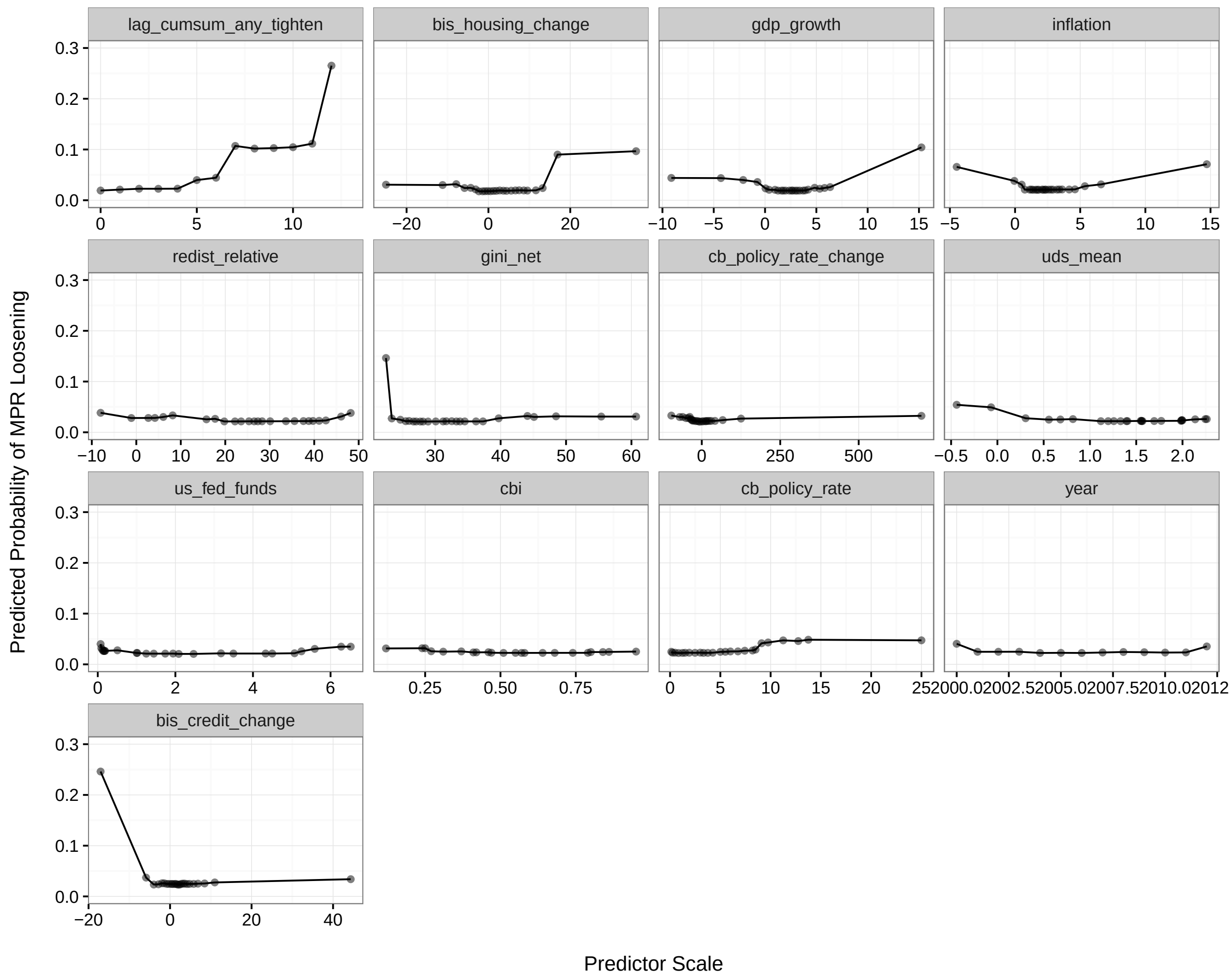
Model B7 includes only observations for which there is no missing data for the BIS housing price and credit provision change variables.

Figure A-7: Variable Permutation Importance for Classifying Macro-prudential Policy Loosening



Bars coloured by whether or not they have positive variable importance.

Figure A-8: Partial Dependence Plot for Macro-prudential Regulatory Policy Loosening



Variables with very low variable permutation importance are not shown. The "fixed effects" country variable is not shown. Note that predictions are for policy change to be made per quarter.

Online Appendix 5 MPR loosening–preliminary results

It is important to note a few data caveats about macro-prudential loosening before discussing our results, which are shown in figures A-7 and A-8. Chiefly, many instances of MPR loosening occurred in the most recent period of our sample as countries began to wind down their responses to the Global Financial Crisis. However, we lack data on many of our covariates after 2011, so our effective sample of MPR loosening decisions is very limited.

As a sanity check, we can see that countries that have tightened macro-prudential regulation more in the past are more likely to loosen. They have more opportunities to loosen. Quickly contracting credit also, as we expected, increases the probability of loosening. Interestingly, Brazil loosened MPR when it had contracting credit and rapidly increasing housing prices. This case is driving the (unexpected) finding that increasing house prices increase the probability of loosening. However, Brazil loosened reserve requirements–a policy move responding to tightening credit conditions–rather than lending standards, for example, which would have been more directly targeted at housing prices. The high probability of loosening when there is already high growth is being driven exclusively by Singapore, which in 2010 had GDP growth over 15 percent and also loosened lending standards. It is unclear what generalisable conclusions to draw from this one data point. Overall, given the sparsity of data for MPR loosening, we should be cautious of generalising conclusions from these findings.

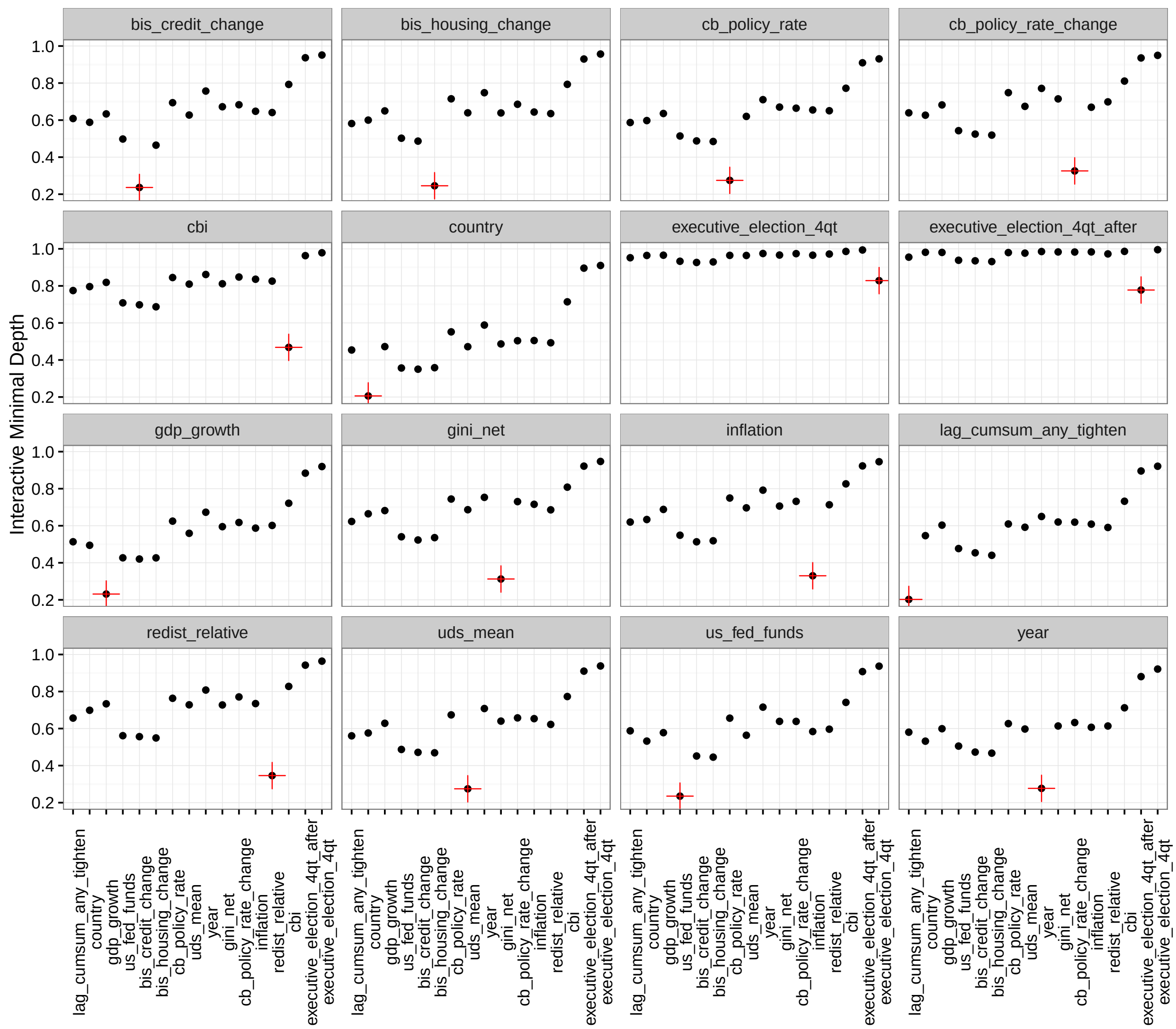
Online Appendix 6 Exploring possible interactions

One advantage of random forests is that we can explore many possible interactions between our predictors (Jones and Linder 2015, 18–21). To test for possible interactions in our random forest model we used a method that builds on the minimal depth approach (Ishwaran et al. 2010). We constructed a $p \times p$ matrix, where p is the number of predictors. The diagonal is the (normalised) minimal depth relative to the root node. Each off diagonal term is the minimal depth term relative to the diagonal in that row. Small off diagonal term values indicate that the term splits close to the diagonal term (Ehrlinger 2015b, 17–19), i.e. that there is an interaction between the two variables.

We visualise these matrices in figures A-9 and A-10.[13] The red crosses indicate the diagonal values and the other points show the interaction scores. Based on these plots, there is little evidence for pairwise interactions in our models.

[13] We used the `ggRandomForests` package for R to create these plots.

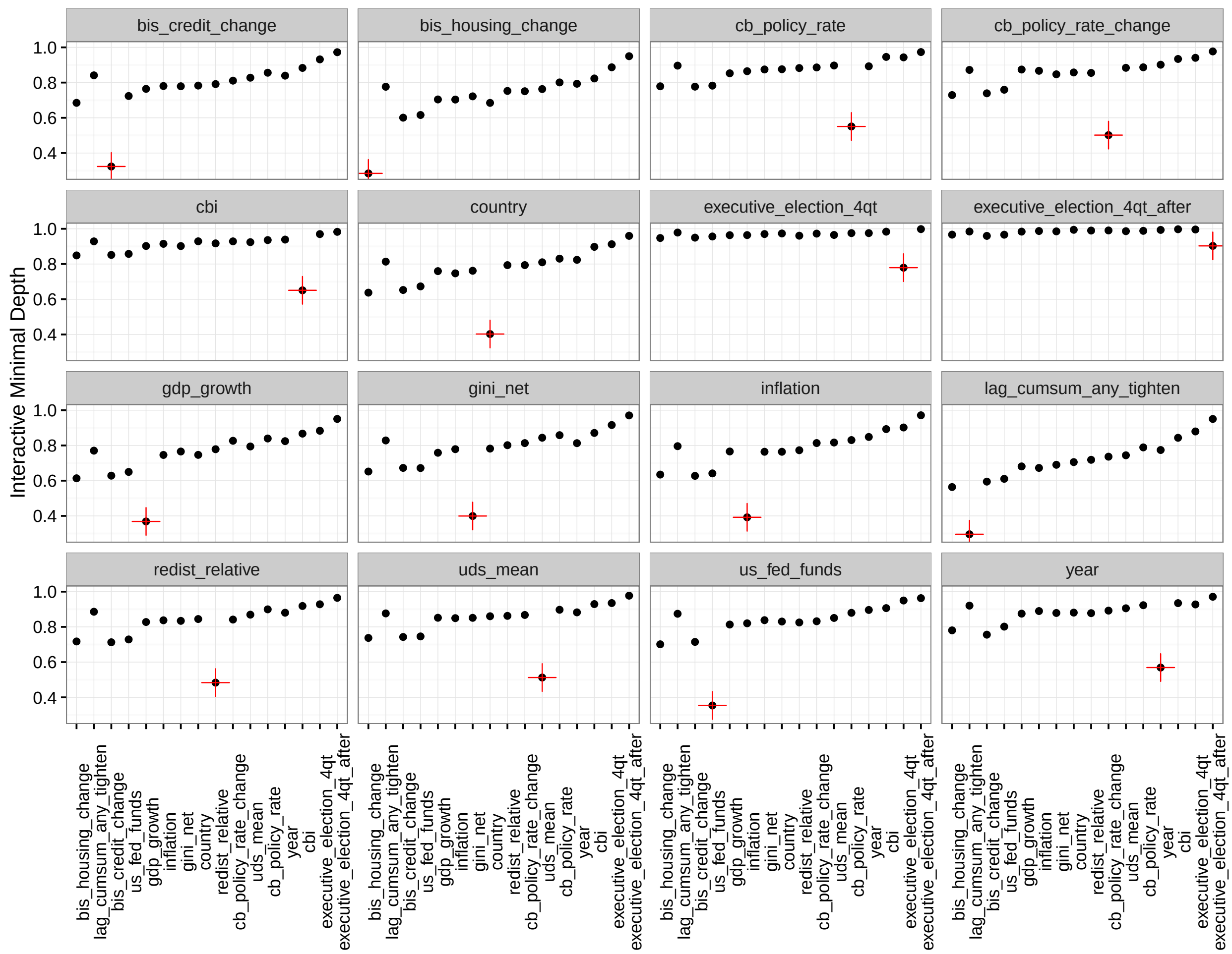
Figure A-9: Minimal Depth Variable Interactions for MPR Tightening



Red crosses indicate diagonal minimal depth values.

All other points are the interaction scores.

Figure A-10: Minimal Depth Variable Interactions for MPR Loosening



Red crosses indicate diagonal minimal depth values.

All other points are the interaction scores.

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