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## CAPITAL CONTROLS AND ELECTORAL CYCLES

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# Capital Controls and Electoral Cycles\*

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## Abstract

This paper studies the relation between the evolution of capital controls and electoral cycles. We exploit a dataset containing detailed information on the level of restrictions on capital flows for 98 countries on an annual base from 1995 to 2015, constructed by Fernandez et al. (2016).

First, we find that restrictions are more likely to increase during an election year. Elections prove to be more closely related to changes in capital controls than any economic variable. Second, these changes are driven predominantly by restrictions on capital outflows and on relatively liquid asset categories. Third, changes tend to occur after elections rather than before. Finally, capital controls increase by more if the new government is more leftist or less liberal than its predecessor, and more electoral uncertainty is related to higher restrictions on capital flows. Overall, these results suggests that theories examining the cyclical properties of capital controls should also consider electoral cycles.

**Keywords:** Capital controls, Election, Electoral cycles

**JEL:** D72, J45, C14

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# Contents

<b>1</b>	<b>Introduction</b>	<b>3</b>
<b>2</b>	<b>Data and Methodology</b>	<b>8</b>
<b>3</b>	<b>Empirical analysis</b>	<b>13</b>
3.1	Main results . . . . .	13
3.2	Additional evidence . . . . .	17
<b>4</b>	<b>Disentangling the average effect</b>	<b>22</b>
4.1	Disaggregated effects . . . . .	22
4.2	Pre- and post-election effects . . . . .	26
4.3	Ideological changes . . . . .	30
4.4	Electoral uncertainty . . . . .	36
<b>5</b>	<b>Conclusion</b>	<b>38</b>
<b>A</b>	<b>Data Sources</b>	<b>43</b>
<b>B</b>	<b>Names of countries included in sample</b>	<b>44</b>
<b>C</b>	<b>Regressions mentioned in main part</b>	<b>45</b>
<b>D</b>	<b>Tables in main part separately for controls on capital outflows and inflows</b>	<b>53</b>
<b>E</b>	<b>Figures</b>	<b>60</b>

# 1 Introduction

The Great Recession and a series of crises hitting countries with free capital mobility has put into question the long-standing paradigm that capital controls lead to inefficiencies, hampering economic growth. A theoretical literature recently emerged, examining the conditions for capital controls to play a growth enhancing role (Benigno et al., 2016; Bianchi, 2011; Farhi and Werning, 2014; Korinek and Sandri, 2016 among many others). This literature advises policymakers to employ counter-cyclical capital control policies, with tighter controls accompanying booms and a release during busts—a view also embraced by the International Monetary Fund (IMF, 2012). This theoretical framework, however, proves helpless in explaining the actual evolution of restrictions on capital flows: booms and busts do not drive changes in the level of controls, which may be a sign that theory “may not be capturing all the relevant economic or *political* factors that determine the cyclical properties of optimal capital controls” (Fernández et al., 2015; emphasis added).

The aim of this paper is to investigate the relation between electoral cycles and the evolution of capital controls. Elections may trigger changes in capital restrictions via several theoretical channels. First, elections are a source of economic instability: changes in government and leaders can result in policy changes.<sup>1</sup> This uncertainty can in turn trigger a surge of capital outflows while drying up capital inflows (Chang, 2010). For instance, Julio and Yook (2012) observe a sharp decrease of FDI flows from US firms to foreign affiliates in the pre-election period of the trading partner. Restricting capital outflows before elections and mitigating capital flow stops by loosening up restrictions on inflows, before moving back to their initial levels once the uncertainty is resolved, might lead to stabilization in such periods of uncertainty. As a second channel, changes in government ideology triggered by elections may influence the amount of capital controls in place.<sup>2</sup> A newly elected left-wing government can restrict capital outflows in order to retain the domestic tax base and increase seigniorage revenue (Alesina et al., 1993; Grilli and Milesi-Ferretti, 1995). For example, in May 1981 François Mitterrand became the first socialist, anti-market politician elected as the French president since the World War II. His program em-

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<sup>1</sup>See Persson and Tabellini, 2000 for an extensive summary of the interplay between politics and economic outcomes.

<sup>2</sup>See Potrafke (2017) for a survey of the links between partisan politics and economic outcomes in OECD countries.

phasized nationalization, higher minimum wages, more paid holidays and a reduction of working hours. Facing massive capital outflows, a series of measures drastically reinforcing capital controls have been adopted shortly after the election (Sachs and Wyplosz, 1986). Conversely, the liberal, business-oriented agenda of Argentinean president Mauricio Macri encompassed lifting restrictions on capital flows, which was implemented soon after his 2015 victory. More generally, left-wing governments tend to impose higher capital controls than right-wing ones (Oatley, 1999; Quinn and Inclan, 1997). Third, in line with Political Business Cycle (PBC) theory (Nordhaus, 1975), the incumbent may manipulate capital controls in the pre-electoral period in order to maximize his reelection probability (Schulze, 2000): higher capital controls depreciate the real exchange rate, improving international competitiveness and thus creating jobs (Moser et al., 2010). This in turn positively affects electoral support for the incumbent (Giordani et al., 2017).

For our empirical analysis, we exploit the dataset assembled by Fernández et al. (2016), which provides detailed information on the *de jure* restrictions on capital flows in 100 countries from 1995 to 2015, based on the IMF’s Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER). An interesting feature of the Fernández et al. (2015) dataset is that it provides a quasi-continuous measure of controls, allowing to track their changes in intensity from one year to the other. Focusing on elections determining the national leader<sup>3</sup>, we study whether electoral years are systematically associated with a particular movement of capital restrictions.<sup>4</sup>

We document a novel and robust connection between elections and *changes* in the intensity of capital controls: they increase significantly during elections years. Moreover the effect is significant in economic terms as it corresponds to about one sixth of a standard deviation. It is robust to controlling for country and time fixed effects, as well as macroeconomic and policy covariates. To account for model and parameter uncertainty, we apply a Bayesian model averaging (BMA) (Magnus et al.,

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<sup>3</sup>More specifically, we consider presidential elections in presidential systems, and general elections in parliamentary ones. When the system does not fit this classification, we proceeded to a case by case study in order to determine who is the *de facto* policy leader and which election (if any) brings him/her in power.

<sup>4</sup>The few previous studies examining the political determinants of capital controls rely on a crude, binary measure of restrictions (Alesina et al., 1993; Grilli and Milesi-Ferretti, 1995) preventing to investigate such a link.

2010). Elections appear to be much more tightly linked with capital controls than any economic variable. This is in line with the observation of Fernández et al. (2015) and Eichengreen and Rose (2014), who do not find any connection between changes in capital controls and the business cycle.<sup>5</sup>

Identification exploits the fact that several elections occur in the same country during our sampling period, and they happen at different points in time in different countries. A crucial underlying assumption is that the probability of having an election is unrelated to the error term conditional on fixed effects and covariates. While this likely holds for planned elections, early elections may not be exogenous to some specific economic or political conditions that simultaneously affect capital controls. When considering only planned elections, we still observe a positive and significant relationship between election years and capital controls, though with a lower magnitude. Further, using the Polity4 index of democracy, we find that the effect persists in democracies, where elections do matter, but vanishes in autocracies. Moreover, the result is not driven by the recently improved perception of capital controls, as we find similar effects for the periods 1995–2004 and the 2005–2015 respectively, or when excluding the euro area.

The rich dataset allows us to distinguish between changes in restrictions on capital outflows and inflows. We find that the positive relation between capital controls and elections is mostly driven by restrictions on capital outflows, which is generally in line with most of the theoretical channels. There is some evidence for higher inflow restrictions in election years, although the effect is smaller and less robust. Looking at disaggregated measures of controls by asset class, the results suggest that governments restrict capital flows by more for asset classes encompassing relatively liquid and thus more easily transferable assets.

Since capital controls are measured at the end of each year, using the precise election date, we compare the effect of elections in the four quarters: if an election takes place early in the year, an increase of capital controls this year is most likely due to the post-election period. Conversely, if the election takes place towards the end of the year, any further restriction was presumably implemented in the pre-election period. We find that additional restrictions are predominantly driven by elections taking place in the first two quarters of the year, suggesting that they are imposed

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<sup>5</sup>“Booms and busts in aggregate activity are associated with virtually no movements in capital controls”, Fernández et al., 2015, p.1.

by newly elected governments. To deepen this point, we use the data of Forbes et al. (2015), which provide information on the evolution on capital controls at the weekly level for 36 countries included in our main sample for the years 2009–2011. We observe a peak of legislative activity related to capital restrictions generated by the election period: there are significantly more changes in the four weeks following the election than those preceding it. But some changes also happen before elections.

Lastly, we investigate whether elections bringing a new leader or a new party/coalition to power have a differential effect on capital controls. We find that even elections seeing the incumbent getting reelected are associated with an increase in capital controls. Potentially, ideological switches of new governments to the right and to the left cancel out each other. To address this issue, we account for movements of governments on the ideological left-to-right scale. Indeed, we find that switches to the left (right) are associated with higher (lower) capital controls during election years, although the effects are not always statistically significant. Potentially, also nationalist, isolationist right-wing parties themselves might impose capital controls. We exploit the Manifesto Project data which provides party-level measures along various ideological dimensions (e.g., nationalization, market economy, etc.). The results support the view that switches to less business-friendly parties are associated with larger increases in capital controls.

Potentially, capital controls increase in cases when incumbents get reelected because of electoral uncertainty: if the outcome of the race is difficult to predict, incumbents might increase capital controls to prevent or limit capital flight. As a proxy for electoral uncertainty, we use the difference in vote shares between the winner and the runner-up. Results suggests that capital controls increase significantly by more the closer the race. If closeness of the election is tightly related to ex ante uncertainty about who gets elected, uncertainty might explain part of the overall effect.

This paper provides four main contributions to the literature. First, our results point out that elections are more closely related to the evolution of capital controls than any macroeconomic indicator. Several contributions suggest that capital controls can be used as a counter-cyclical policy, helping to smooth out capital flows (Benigno et al., 2016; Bianchi, 2011; Farhi and Werning, 2014; Korinek and Sandri, 2016). This paper contributes to the debates on the theoretical motivation for the use of capital controls by providing additional evidence that macroeconomic condi-

tions do not play a role in their evolution, in line with Eichengreen and Rose (2014) and Fernández et al. (2015). It suggests the need for additional theoretical research in order to understand policy-maker decisions from a political economy perspective.

Second, Eichengreen and Rose (2014) have shown that controls are highly persistent, and once defined can remain in place for decades. An important practical implication of our results is that electoral cycles can create a positive trend in the evolution of restrictions. We observe that the positive impact of elections on the change in controls is not compensated by a decrease in the following year. In addition, Eichengreen and Rose (2014) conclude that capital controls only change along variables like quality of regulatory institutions and domestic financial depth, which evolve very slowly over time, and that governments have been unable to adjust capital controls to the business cycle. We document in this paper that policy makers can actually resort to capital controls as a response to relatively frequent events, indicating that the intensity of restrictions is indeed in their toolkit.

Third, this paper complements and updates previous studies examining the political determinants of capital controls (Alfaro, 2004; Alesina et al., 1993; Grilli and Milesi-Ferretti, 1995). They primarily investigate whether leftist governments are associated with higher levels of restrictions, while our paper focuses on the electoral mechanism per se. Also, contrary to these papers, the dataset on which our analysis relies provides a measure of capital control intensity, which allows us to study the intensive margin of capital controls over time. Our study shows the need for future research to collect even more fine-grained data on events changing capital controls.

Fourth, it evidences a new channel through which elections affect economic variables. The impact of elections has been extensively documented on fiscal policy (see for instance Brender and Drazen, 2005; Shi and Svensson, 2006), but also more recently on corporate investment (Durnev, 2010; Julio and Yook, 2012) and on cross-boarder capital flows (Julio and Yook, 2016). To our knowledge, this paper is the first to study the document the relation between elections and capital controls.

The remainder of this paper is structured as follows. In Section 2, we provide a description of the data and introduce the methodology. The main results are provided in Section 3, and Section 4 discusses potential transmission channels. Section 5 offers concluding remarks.

## 2 Data and Methodology

We investigate the relationship between elections and the evolution of capital controls. For this purpose, we exploit the dataset developed by Fernández et al. (2016), extending Schindler’s (2009) data. Based on the IMF’s *Annual Report on Exchange Arrangements and Exchange Restrictions*, it provides detailed information about *de jure* capital controls for 98 countries from 1995 to 2015. A unique feature of this dataset is the level of disaggregation, as it covers ten different asset categories: money market, bonds, equities, collective investments, derivatives, guarantees, financial credits, commercial credits, real estate and direct investments. For each category, Fernández et al. (2016) further disentangle restrictions affecting either inflows or outflows. From this classification, they construct measures of restriction intensity on outflows and on inflows separately by averaging the subcategories. These two measures are then averaged in order to obtain a measure of the overall level of capital controls. The three measures, overall restriction, restriction on outflows, restrictions on inflows, are labelled  $KC$ ,  $KCO$  and  $KCI$  respectively). These indices are bounded between 0 and 1—a score of 0 indicating no restrictions and 1 representing controls on every type of international capital flows—which we rescale to take on values between 0 and 100. The diversity of assets taken into account ensures an important heterogeneity in the level of controls across countries. For instance, even within the euro area the level of controls in 2015 varies from 0 in Belgium, Netherlands and Spain, to 30 in Germany, 43 in Slovenia and 58 in Greece. As evidence of the relevance of their indicator, Fernández et al. (2015) show that it captures very well the movement of effective capital controls in Brazil in the late 2000s, which is often used as a case study of the countercyclical use of capital controls.

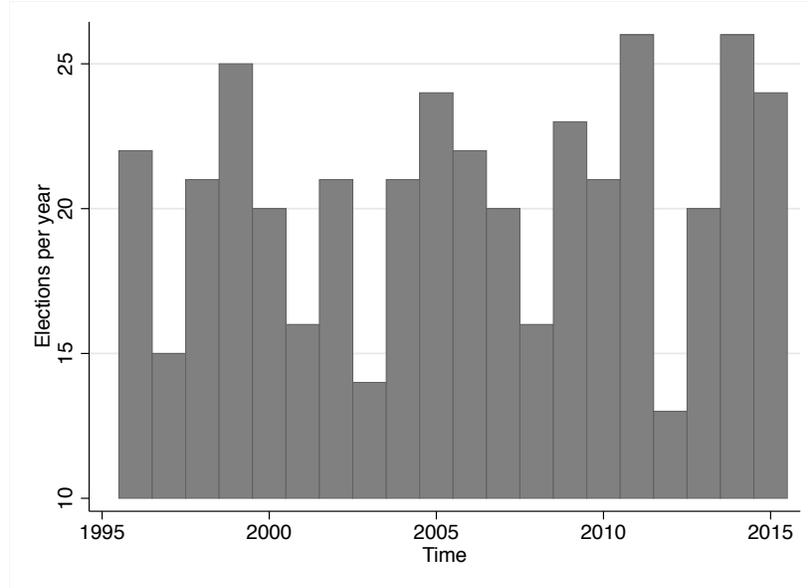
Previous research studying the political determinants of capital controls was relying on a simple binary measure of capital controls (Alesina et al., 1993; Alfaro, 2004; Grilli and Milesi-Ferretti, 1995). In contrast, our more precise measure of capital controls intensity allows us to keep track of the size of changes in the restrictions over time.<sup>6</sup>

Capital controls are typically at the discretion of national governments, which is why we focus on national elections: we consider presidential elections in presi-

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<sup>6</sup>Our sample is restricted to countries included in this dataset. A full list of countries can be found in the Appendix. All but three countries (Bangladesh (18 observations); Iran and Myanmar (19 observations respectively) are observed for the entire sample period.

**Figure 1:** NUMBER OF ELECTIONS ANNUALLY OVER TIME



NOTE: Aggregate of elections taking place by year over the entire sample.

dential systems and general elections in parliamentary systems.<sup>7</sup> From the World Bank’s Database on Political Institutions (DPI 2015) we use the classification of political systems into presidential or parliamentary, and the dates of all parliamentary elections. We verified the system classification and made updates where necessary.<sup>8</sup> Moreover, we collected election dates for the presidential elections. In the sample, we observe a total of 410 elections. Figure 1 displays the distribution of elections per year, which is evenly balanced.

The baseline estimation equation takes the following form:

$$\Delta KC_{c,t} = \alpha + \beta Election_{c,t} + \delta KC_{c,t-1} + \xi X_{c,t} + u_c + \tau_t + \epsilon_{c,t} \quad (1)$$

$\Delta KC_{c,t} = KC_{c,t} - KC_{c,t-1}$  measures the change in capital controls between years  $t$  and  $t-1$  for country  $c$ .  $Election_{c,t}$  is our main explanatory variable: it takes on value 1 for years in which elections took place in country  $c$ , and is zero otherwise.  $KC_{c,t-1}$  is the lagged level of capital controls. Countries with a high level of restrictions are more

<sup>7</sup>If the system of a country does not fit to this classification, we focus on the general elections.

<sup>8</sup>These updates can be found in our final dataset, available upon request.

likely to reduce them, while the opposite might be true in countries with low levels of controls.<sup>9</sup>  $X_{c,t}$  is a set of covariates detailed below. We control for country fixed effects  $u_c$  to measure the within-variation of capital controls per country. Moreover, we control for year fixed effects  $\tau_t$  which take into account common shocks affecting all countries at the same time. Examples are the dot-com bubble, the financial crisis, or joint resolutions of multilateral organizations regarding capital controls to be implemented by many countries at the same time. The error term  $\epsilon_{c,t}$  is clustered at country level as observations are likely correlated at this level.

The coefficient of interest  $\beta$  captures whether changes in capital controls are different in election years than in years without elections. Identification is facilitated by observing several elections per country during our sample period, and variation in elections in different countries over time.  $\beta$  can be interpreted causally if the occurrence of elections (conditional on controls and fixed effects) is unrelated to the error term. The validity of the identification is threatened by two issues. First, the exogeneity assumption can be violated in countries where the incumbent government can trigger early elections (like for instance in Japan). The government could set the election during an economic boom in order to maximize its reelection probability (see for instance Ito, 1990 and Smith, 2004). If income and capital controls were positively correlated, this might lead to higher capital controls during elections without being a causal channel. However, (Fernández et al., 2015) show that there is no such relationship precisely for the sample we are employing. Consequently, this might be less likely an issue.

A second potential threat to identification is reverse causation, i.e., governments try to manipulate the election date in a way that would coincide with changes in capital controls. In our context this is, however, rather unlikely, as capital controls are an instrument at the government’s discretion, potentially in interaction with the central bank. To address this issue, we reviewed all elections included in the sample in order to check whether it was a planned or an early election. More than 75% of elections in our sample were held on time. As we will show in the results section 3, the results hold if we focus exclusively on planned elections and discard early ones.

We select two sets of covariates,  $X_{c,t}$ , which could affect the evolution of capital controls. The first set encompasses variables typically controlled for in studies of

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<sup>9</sup>We rerun everything without the lagged specification. The results are robust to that, and reported in an Online Appendix.

capital controls which are available for most countries and years in our sample. To account for the level of development and the government’s incentive to raise revenue, we control for GDP per capita, GDP growth and government expenditure over GDP (Alfaro, 2004; Eichengreen, 2001; Grilli and Milesi-Ferretti, 1995; Eichengreen and Rose, 2014). Capital controls can be used as a tool to remain in an exchange rate band, e.g., in countries with pegged exchange rates (Leblang, 1997; Milesi-Ferretti, 1998; Bernhard and Leblang, 1999). In addition to the real exchange rate, we also control for six exchange rate indicators, from *de facto* peg to *freely floating* (Ilzetzki et al., 2017). Moreover, we control for inflation (Alesina et al., 1993).

The second set of controls is composed of variables with a larger number of missing values or variables that have not been systematically used in previous studies on the determinants of capital controls. First, the level of central bank independence might matter for capital controls. Governments facing totally independent central banks have less leeway to influence capital restrictions (Alesina et al., 1993; Quinn and Inclan, 1997; Milesi-Ferretti, 1998; Bai and Wei, 2000). To control for (*de jure*) central bank independence, we use the index proposed by Garriga (2016), based on Cukierman et al. (1992). Covering 182 countries between 1970 and 2012, it encompasses most of the countries present in our dataset. To capture the level of democracy, following Eichengreen and Rose (2014), we use the Polity4 index of democracy, ranging on a scale from  $-10$  to  $10$ , higher values indicating a higher democratic level (Quinn, 2000). We employ a measure of perceived corruption from the World Governance Indicators, higher scores indicating lower levels of perceived corruption (Dreher and Siemers, 2009). Finally, our dataset also encompasses a measure of trade openness to account for the difficulty of monitoring capital flows (the sum of imports and exports over GDP) (Grilli and Milesi-Ferretti, 1995).

All the economic variables (GDP per capita, GDP growth, government expenditures, inflation and openness) are introduced in the model with a one year lag, while the political variables (exchange regime, central bank independence, corruption and the polity measure) are current period.<sup>10</sup>

Descriptive statistics are provided in Table1. Data sources are summarized in Appendix A.

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<sup>10</sup>We replicated all the analysis using current periods for all variables. All the results presented below remain very similar.

**Table 1: DESCRIPTIVE STATISTICS**

Variable	Obs	Mean	Std. Dev.	Min	Max
Capital controls (KC)	1,956	37.7	33.9	0	100
Capital controls on outflows (KCO)	1,956	39.6	38.6	0	100
Capital controls on inflows (KCI)	1,956	35.6	32.1	0	100
Changes in capital controls ( $\Delta KC$ )	1,956	-.044	7.713	-78	62
Changes in capital controls on outflows ( $\Delta KCO$ )	1,956	.026	9.567	-100	70
Changes in capital controls on inflows ( $\Delta KCI$ )	1,956	-.129	8.320	-85	65
Election	1,956	.210	.407	0	1
1ST SET OF COVARIATES					
GDPcap	1,824	17074	19078	351	91594
GDP growth	1,824	3.756	3.778	-14.8	33.736
Government expenditure	1,824	15.821	5.029	4.58	42.51
REER	1,824	115.56	99.49	34.53	2462.76
Peg	1,824	.332	.471	0	1
Crawling peg	1,824	.322	.467	0	1
Managed float	1,824	.284	.451	0	1
Free falling	1,824	.020	.139	0	1
Free	1,824	.039	.193	0	1
Dual market	1,824	.004	.062	0	1
Inflation	1,824	8.106	30.053	-4.863	1058.374
2ND SET OF COVARIATES					
Central Bank independence	1,068	.609	.212	.122	.904
Polity	1,068	5.998	5.540	-10	10
Corruption	1,068	.326	1.090	-1.5	2.47
Openness	1,068	.761	.466	.134	4.022

NOTE: Based on the sample of 98 countries from Fernández et al. (2016) for the years 1995–2015. All data sources are detailed in Appendix A.

### 3 Empirical analysis

We begin by presenting the main results based on estimation equation (1), and present various robustness tests.

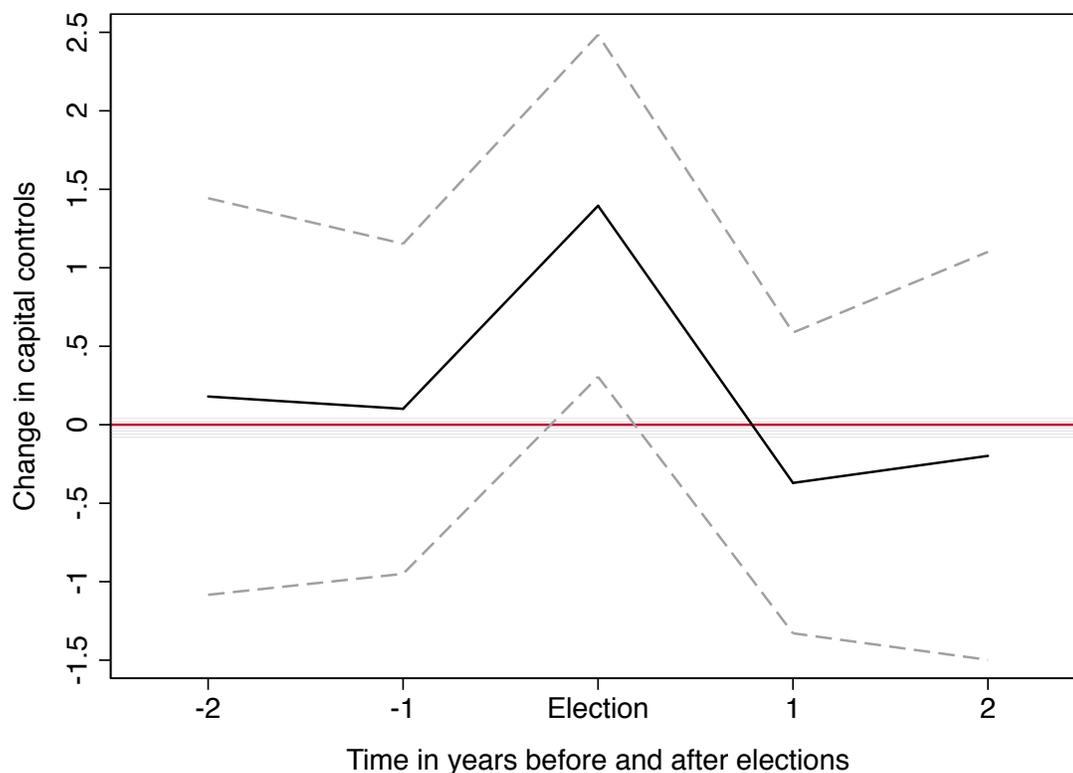
#### 3.1 Main results

The main results are in Table 2. Column (1) shows the result of a raw regression controlling only for the election indicator. The coefficient is positive and significant, suggesting higher capital controls by 1.343 points in election years. This corresponds to about one sixth of a standard deviation. In models (2) and (3) we control for year and country fixed effects one at a time. While the coefficient changes little when year fixed effects are included (coefficient: 1.305), it increases in the specification with country fixed effects (coefficient: 1.561). This indicates that within-country variation of capital controls drives the main effect rather than across-country differences. Both year and country fixed effects are included in column (4), and the effect is similar to the specification with country fixed effects only (coefficient: 1.523). In column (5), which we will refer to as the baseline, we additionally control for the lagged value of capital controls. The coefficient suggests an increase of capital controls by 1.493 points in years when elections take place compared to years without elections. The coefficient of lagged capital controls is significantly negative suggesting that countries with many restrictions tend to lower them, as expected.

If the effect is truly due to elections, we would expect to observe changes in capital controls mostly in election years but not in the adjacent years. We extend the baseline by additionally controlling for two leads and lags of the election year each. The graphical result is in Figure 2 and the corresponding Table C1 in the Appendix. The  $x$ -axis depicts time in years around the election. The solid line represents the coefficient estimates of the change in capital controls, and the dashed lines the 95% confidence intervals. The electoral cycle is very well visible: in the two years preceding and following an election, the effect is close to zero and insignificant. Changes in capital controls peak in the election year and the effect is significant (coefficient: 1.395;  $p$ -value: 0.013).

As outlined in Section 2, we control for two sets of macroeconomic variables previously used in the literature (columns (6) and (7) in Table 2). As control variables

**Figure 2:** ELECTORAL CYCLE OF CAPITAL CONTROLS



NOTE: Based on 98 countries between 1995 and 2015. The solid line shows coefficient estimates for pre-, post-, and election indicators with leads and lags of two years each. Country and year fixed effects are controlled for; standard errors are clustered at country level. The dashed lines represent 95% confidence intervals.

are not available for the entire sample, we rerun the main regression in the subsample of countries and years for which the controls are available (cf. columns (1)–(2) in Table C2 in the Appendix): coefficients are 1.492 and 1.078 respectively, which suggests that sample selection matters for the coefficient size especially in the last specification. The main effect of elections on capital controls after the introductions of these covariates remains positive, 1.529 and 1.138 respectively, and is still significant. In sum, covariates hardly affect the estimated election effect on changes in capital controls<sup>11</sup>.

<sup>11</sup>The equation we estimate suffers from the Nickell bias by construction, as shown by Caselli

Control variables themselves also seem to matter little for explaining changes in capital controls. Only inflation has a significantly positive effect in both specifications; the exchange rate regime “dual market” and corruption are positively significant in one specification each (coefficients of control variables are reported in columns (3)–(4) in Table C2 in the Appendix).

The disconnection between the evolution of capital controls and most economic variables is in line with the results of Eichengreen and Rose (2014) and Fernández et al. (2015). It may, however, depend on the choice of the control variables included in the model. To further check this point, and as the set of controls is relatively large, we implement a Bayesian model averaging in order to take into account model uncertainty (see Moral-Benito, 2015 for a survey, and Magnus et al., 2010 for a formal presentation of the approach). The results, reported in Table C4 in the Appendix, confirm that elections are more tightly connected to capital controls than any other variable considered. For this reason and to keep the sample as large as possible, we resort to reporting regressions without controlling for covariates.<sup>12</sup>

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et al., 1996. The time dimension of our panel being rather moderate ( $T = 20$ ), we alternatively estimate the model using GMM. Results are presented in Table C3 in the Appendix.

<sup>12</sup>Results including controls are nevertheless qualitatively very similar and are available in the Online Appendix.

**Table 2:** CAPITAL CONTROLS INCREASE IN ELECTION YEARS

VARIABLES	(1) Raw	(2) Year FE	(3) Country FE	(4) Country/ Time FE	(5) Baseline	(6) Main controls	(7) Full set controls
Election	1.343*** (0.460)	1.305*** (0.474)	1.561*** (0.478)	1.523*** (0.493)	1.493*** (0.446)	1.519*** (0.443)	1.127** (0.495)
$KC_{c,t-1}$					-0.207*** (0.023)	-0.217*** (0.026)	-0.248*** (0.034)
Observations	1,956	1,956	1,956	1,956	1,956	1,824	1,064
R-squared	0.005	0.017	0.043	0.054	0.171	0.193	0.234
Country FE	NO	NO	YES	YES	YES	YES	YES
Year FE	NO	YES	NO	YES	YES	YES	YES
R <sup>2</sup>	.	0.00424	0.00613	0.00582	0.128	0.139	0.162

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. Based on 98 countries between 1995 and 2015. The dependent variable is the change in capital controls. (1) includes no fixed effects, (2) year fixed effects, (3) country fixed effects, (4)–(7) year and country fixed effects. In (5)–(7) the lagged value of the dependent variable is controlled for. In (6) and (7) covariates are controlled for.

## 3.2 Additional evidence

We implement various checks to ensure the robustness of the link between elections and the evolution of capital controls.

The first set of checks regards the exogeneity and importance of elections. First, as discussed in Section 2, the exogeneity of the elections might be violated if the government can strategically manipulate the election date. As extensively discussed by Smith (2004), incumbent politicians may decide to call an election “when the time is right”. In other words, good economic performance may trigger elections. Since in theory capital controls can be simultaneously used as a counter-cyclical policy, this good performance could trigger both elections and a rise of capital controls. To ensure that the results are not driven by such a mechanism, we estimate equation (1) considering only planned elections (and dropping 100 out of 410 elections which were early). Moreover, as we have argued previously, governments do not seem to employ capital controls as country-cyclical measures.

Results are reported in column (1) of Table 3. The coefficient is smaller than in the baseline (0.892), but still significant, supporting the causal link between elections and capital controls.

Second, if the positive association between elections and capital controls is not spurious or jointly driven by omitted variables, the effect should be stronger in countries in which elections matter, i.e., in democracies. We define a country as a democracy if its Polity4 score is greater than or equal to zero (75 countries in our sample). We indeed find an effect similar to the average in democracies (column (2)). And as expected, the coefficient is less than half as large and insignificant in autocracies (column (3))—even though the interaction turns out to be insignificant (column (4)). Moreover, for robustness we drop eleven countries from our sample which did not hold elections between 1995 and 2015: the results are very similar to the baseline specification (cf. Table C5 in the Appendix).

The second set of checks refers to the persistence of the average effect in different subsamples of countries and time periods. Policies regarding capital controls might depend on the country’s level of development (as discussed for instance in Stiglitz and Ocampo, 2008). Using the IMF country classification, in our data high-income countries have indeed on average lower levels of capital controls (19.7) than middle- (48.8) or low-income countries (63.4) (41, 49 and 8 countries respectively). In Table

**Table 3:** ADDITIONAL EVIDENCE 1

	(1)	(2)	(3)	(4)
	Planned elections	Democracy	Autocracy	Interacted
Election on time	0.892* (0.458)			
Election		1.590*** (0.465)	0.701 (1.315)	1.591*** (0.455)
Election x Autocracy				-0.764 (1.412)
Autocracy				-0.059 (1.617)
$KC_{c,t-1}$	0.790*** (0.026)	-0.210*** (0.022)	-0.381*** (0.091)	-0.239*** (0.025)
Observations	1,856	1,361	474	1,836
Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
R <sup>2</sup>	0.686	0.127	0.195	0.148

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered at country level. Ordinary least squares regression. Based on 98 countries between 1995 and 2015. The dependent variable is the change in capital controls. Year and country fixed effects are always controlled for. In (1) all early elections are dropped. (2) is restricted to democracies (Polity4 index greater or equal to zero; 75 countries), (3) is the subsample of autocracies (Polity4 index smaller than zero; 31 countries). (4) is based on the full sample and an interaction between the election indicator and an indicator taking on value 1 for autocracies.

4 columns (1)–(3) show the main coefficient in the subsamples defined by the three income groups. All three are positive, and the effect is significant in the groups of high- and low-income countries. While the effect is similar to the average in high-income countries (coefficient: 1.495), it is higher in low-income countries (coefficient: 4.039). However, when testing for differences between the country groups in the pooled sample (column (4)), we find no significant differences. This shows that we do not just capture a phenomenon particular to a group of countries with similar development. Instead, we seem to find a more general pattern that persists in more

and less developed countries.

Next, the general attitude towards capital controls has drastically improved during the past decade. The IMF started recommending capital controls as a means of stabilization, in contrast to its earlier advise of market liberalization (Fernández et al., 2015). A graphical inspection of the average amount of capital controls over time confirms that capital controls have been decreasing until 2005, but witnessed a revival since the onset of the financial crisis (cf. Figure 3). We thus split the sample in observations before and since 2005 respectively to make sure that we do not capture a recent positive trend in capital controls that was not yet accounted for by year fixed effects (columns (5)–(6)). The coefficient in the pre-2005 sample is larger than the one in the post-2005 sample (1.729 and 1.002 respectively), and the difference between the two is insignificant as shown by the interaction term in column (7). Even in the period during which capital controls have been decreasing, capital controls tend to increase in election years. The effect is not specific to the post-financial crisis period.<sup>13</sup> This test also presents the merit to rule out a potential alternative explanation for the observed positive effect of elections: if capital controls tend to decrease over time, a positive election effect could simply reflect a pause in the process. But observing a positive effect even in the post-2005 period suggests that this is not the case.

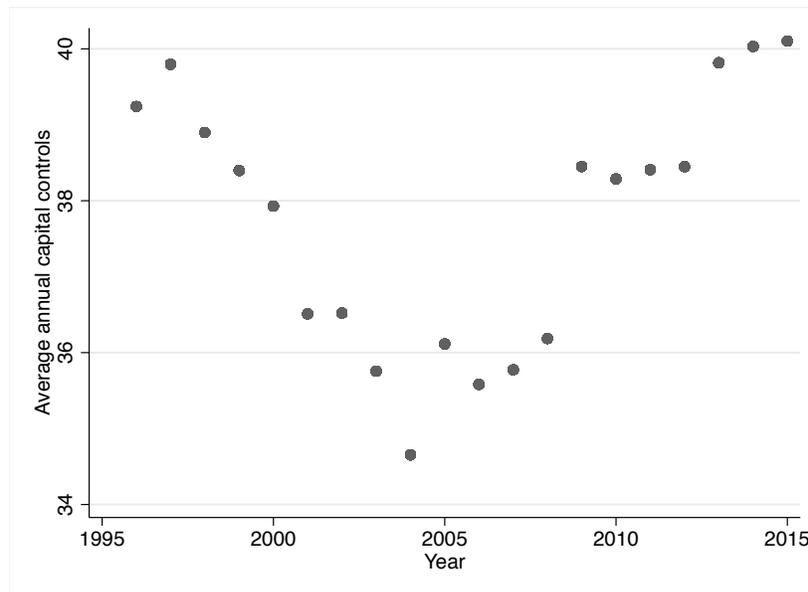
While most governments have considerable discretion over the introduction of capital controls, the group of countries in the euro area might be more restricted with regard to such policy choices. In column (8) we drop all 15 countries in years they are members of the euro area. The coefficient 1.505 is similar to the baseline 1.493 suggesting an effect similar to the average in member states of the euro area. Although euro area states employ significantly fewer capital restrictions than the other countries in our sample (41 vs. 11 on average), they seem to exhibit a similar relationship between capital controls and elections.

Additionally, to make sure that individual countries are not driving the average effect, we estimate the baseline specification using jackknife standard errors (Table C6 in the Appendix). The standard error increases only slightly from 0.446 to 0.450 indicating that country outliers do not affect our estimation.

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<sup>13</sup>The results are robust to running the baseline with controls for country-specific linear time trends.

**Figure 3:** AVERAGE ANNUAL CAPITAL CONTROLS OVER TIME



NOTE: Means of capital controls are calculated by year over the entire sample of 98 countries.

**Table 4: ADDITIONAL EVIDENCE 2**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High	Middle	Low	Interacted	Pre '05	Post '05	Interacted	Without Eurozone
Election	1.495** (0.595)	1.189 (0.729)	4.039*** (0.608)	1.220* (0.705) 1.631 (1.322)	1.729** (0.661)	1.002** (0.453)	1.811** (0.722)	1.505*** (0.495)
Election x low income								
Election x high income				0.355 (0.907)				
Election x 2005							-0.562 (0.838)	
$KC_{c,t-1}$	-0.170*** (0.024)	-0.231*** (0.040)	-0.316*** (0.069)	-0.208*** (0.023)	-0.403*** (0.056)	-0.328*** (0.039)	-0.208*** (0.024)	-0.211*** (0.026)
Observations	820	979	157	1,956	880	1,076	1,956	1,744
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
R <sup>2</sup>					0.200	0.188	0.128	0.129

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Based on observations between 1995 and 2015. Ordinary least squares regression. The dependent variable is the change in capital controls. Year and country fixed effects are always controlled for. (1)-(3) is based on subsamples of high-, middle, and low-income countries (41, 49 and 8 countries respectively). (4) is based on the full sample of 98 countries and the interactions between the election indicator and two indicators taking on value 1 for low- and high-income countries respectively. Middle-income countries are the reference group. (5) is restricted to the pre-2005 sample, (6) is the subsample since 2005. (7) is based on the full sample and an interaction between the election indicator and an indicator taking on value 1 for all years since 2005 is controlled for. (8) is based on the subsample excluding the 15 euro area countries in the years they are members of the euro area.

## 4 Disentangling the average effect

The main result indicates that capital controls increase on average in election years. In this section, we examine the mechanisms by focusing on the channels and the timing of this increase. First, we use disaggregated measures of capital restrictions to check if controls are imposed on inflows and/or outflows, and check for differences according to types of financial assets. Second, we try to understand whether additional restrictions are imposed before or after the election. Finally, we investigate the consequences of new governments getting elected. In particular, we look at ideological switches of governments and electoral uncertainty.

### 4.1 Disaggregated effects

In the previous section, our dependent variable was the average change in capital restriction. In what follows, we use as dependent variables the changes in capital controls on *outflows* and *inflows* respectively which are provided in the Fernández et al. (2016) dataset. While the two are positively correlated, they do not always change together to the same extent ( $\rho = 0.483$ ) In a further step, as our dependent variable is the aggregation of multiple sub-categories, we investigate whether individual components of the aggregate capital control measure drive the results.

Outflow restrictions increase significantly in election years.<sup>14</sup> In the baseline specification (Table 5, column (1)), capital controls on outflows increase by 2.088, which corresponds to roughly a fifth of a standard deviation. Adding control variables changes the election coefficient only little (columns (2)–(3)).

The evidence regarding the election effect on capital inflow restrictions is weaker. Although the effect is always positive, it is only marginally significant in the baseline and when controlling for the first set of covariates (columns (4)–(5)). Moreover, it is less than half in size compared to the effect on outflow restrictions (a Wald test comparing both coefficients is highly significant). In the reduced sample for which all control variables are available (column (6)) the effect gets close to zero (coefficient 0.365) and is insignificant.

Governments seem to be keen on preventing outflows of capital in election years, but are less likely to restrict the inflow of capital. Our baseline finding of an increase

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<sup>14</sup>This result is robust to all specifications detailed in Section 3.1 on the main results. For completeness, the results from all specifications are in Table D1 in the Appendix.

in capital controls by 1.492 is hence primarily driven by more restrictive controls on capital outflows.<sup>15</sup> This is confirmed when controlling for two leads and lags of the election indicator (columns (2)–(3) in Table C1 in the Appendix).

We go further in the level of disaggregation and check which asset classes are particularly affected by the restriction increases. Results with the ten subcategories of capital controls as dependent variables are in Table 6.<sup>16</sup> The coefficients are, in line with the average effect for the aggregate measure, all positive (with the single exception of controls for real estate purchase; column (10)). Four of them are significant such that there is an increase in restrictions regarding: collective investment, equity, guarantees, and the money market. In addition to these, Outflow restrictions increase significantly for bonds, derivatives, and financial credits.

We observe more capital controls in various kinds of categories. This suggests that not only one specific measure is affected, but governments seem to pursue organized strategies and apply a broad range of controls. When looking at the coefficients, it moreover seems that governments increase capital controls on rather liquid assets such as money market instruments or equity, but not on rather illiquid ones like real estate. This shows nicely that governments target capital that can be transferred easily.

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<sup>15</sup>For the remainder of the empirical analysis, we present results with changes in average capital controls as the dependent variable. For robustness, we rerun everything separately for control on outflows and inflows. The corresponding Tables D2–D7 are in the Appendix. Whenever conclusions regarding the two categories diverge, we will mention this explicitly.

<sup>16</sup>Not all of the ten subcategories are available for all years, which is why the sample size varies in the ten specifications. E.g., bonds are available only since 1997 (Fernández et al., 2016).

**Table 5:** CAPITAL CONTROLS ON OUTFLOWS INCREASE IN ELECTION YEARS

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Baseline	Main controls	Full set controls	Baseline	Main controls	Full set controls
Election	2.088*** (0.525)	2.108*** (0.530)	1.914*** (0.644)	0.923* (0.477)	0.964** (0.463)	0.354 (0.555)
$KCO_{c,t-1}$	-0.208*** (0.023)	-0.220*** (0.025)	-0.276*** (0.029)			
$KCI_{c,t-1}$				-0.243*** (0.031)	-0.247*** (0.032)	-0.254*** (0.041)
Observations	1,956	1,824	1,064	1,956	1,824	1,064
Country FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
R <sup>2</sup>	0.119	0.130	0.169	0.151	0.157	0.156

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. Based on 98 countries between 1995 and 2015. The dependent variable is the change in capital controls on outflows in columns (1)–(3) and on inflows in columns (4)–(6). Year and country fixed effects are always controlled for. The lagged value of the dependent variable is controlled for. In (2)–(3) and (5)–(6) covariates are controlled for.

**Table 6:** DISAGGREGATED MEASURES OF CAPITAL CONTROLS

	(1) Collective investments	(2) Money market	(3) Equity	(4) Guarantees	(5) Derivatives
Election	3.610*** (0.818)	2.632*** (0.653)	2.465*** (0.702)	2.143** (0.934)	1.465 (1.006)
Observations	1,918	1,916	1,938	1,928	1,833
R <sup>2</sup>	0.045	0.041	0.048	0.053	0.035
	(6) Financial credits	(7) Direct investment	(8) Bonds	(9) Commercial credits	(10) Real estate
Election	1.247 (0.946)	1.032 (0.863)	0.588 (0.753)	0.142 (0.763)	-0.276 (0.674)
Observations	1,952	1,955	1,692	1,949	1,939
R <sup>2</sup>	0.045	0.046	0.046	0.032	0.036
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. Year and country fixed effects are always controlled for. Based on 98 countries between 1995 and 2015. Disaggregated measures of changes in capital controls in ten subcategories as defined by Fernández et al. (2016) are used as dependent variables.

## 4.2 Pre- and post-election effects

We try to shed light on whether changes take place before or after elections. The challenge is that for each year we have only a single observation for capital controls but elections take place during different months. The official way how capital controls are measured is the following: “[...], the report includes a description of exchange and trade systems as of December 31” (Annual Report on Exchange Arrangements and Exchange Restrictions, IMF, various issues).

Consequently, if an election takes place late in year  $t$ ,  $\Delta KC_{c,t}$  most likely reflects capital controls in the pre-election period, and  $\Delta KC_{c,t+1}$  the ones in the post-election period. Instead, if the election is early in year  $t$ ,  $\Delta KC_{c,t}$  is more likely to measure capital controls during the post-election period. We substitute the election indicator with four indicators referring to the quarter of the year an election took place: *Election Jan–Mar*, *Election Apr–Jun*, *Election Jul–Sept*, and *Election Oct–Dec*. If indicators for the first quarters turn out significant, this would point towards capital controls changing *after* elections. In contrast, if indicators for the later quarters are significant, changes happening *before* elections would be the more likely explanation.

All four coefficients are positive, but only the coefficients for the first two quarters, *Election Jan–Mar* and *Election Apr–Jun*, are significant (Table 7, column (1)). Moreover, these coefficients are considerably larger than those for the last two quarters (2.042 and 2.225 as compared to 1.027 and 0.625). When looking at restrictions on capital outflows (column (2)), the finding is similar in terms of coefficient size. Additionally, the indicator for elections in the last quarter becomes also significant. For capital inflows only the coefficient for the second quarter is significant (column (3)).

This evidence is suggestive for governments increasing capital controls rather *after* an election has taken place. However, we cannot entirely exclude that some governments also adjust capital controls before elections. For a more precise assessment of pre/post election effects, we require the exact date of changes in capital controls instead of annual averages. We use the data from Forbes et al. (2015)) which provides this information for 36 countries included in our sample for the years 2009–2011. The data include measures of “capital-flow management events” which restrict international capital flows. The data also distinguish between inflow and outflow restriction events which makes it comparable to the data used in our main

**Table 7:** TIMING OF CHANGES IN CAPITAL CONTROLS

	(1)	(2)	(3)
	Average	Outflows	Inflows
Election Jan-Mar	2.042* (1.107)	2.852* (1.536)	1.214 (1.014)
Election Apr-Jun	2.225*** (0.734)	3.098*** (1.022)	1.419** (0.586)
Election Jul-Sep	1.027 (1.163)	1.090 (1.404)	0.904 (1.177)
Election Oct-Dec	0.625 (0.602)	1.054* (0.624)	0.238 (0.801)
$KC_{c,t-1}$	-0.207*** (0.024)		
$KCO_{c,t-1}$		-0.208*** (0.023)	
$KCI_{c,t-1}$			-0.242*** (0.031)
Observations	1,956	1,956	1,956
Country FE	YES	YES	YES
Year FE	YES	YES	YES
R <sup>2</sup>	0.128	0.119	0.150

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. Based on 98 countries between 1995 and 2015. The dependent variable is the change in capital controls on outflows.

analysis.<sup>17</sup> Since the sample is restricted compared to the main dataset, we proceed with a graphical analysis.

On average, we observe 71 weeks before and 84 weeks after the election; although for some countries, we observe as many as 150 or as few as 5 weeks. The panel is thus not balanced and observations close to the election will be more precise. We define the election week as 0, and then aggregate the number of events by groups of

<sup>17</sup>Although both datasets by Fernández et al. (2016) and Forbes et al. (2015) are primarily based on information from the Annual Report on Exchange Arrangements and Exchange Restrictions by the IMF, the authors extract information from this source in a different way such that both datasets do not necessarily overlap at all times for the countries and time they cover.

four weeks following and preceding the election week.<sup>18</sup>

In Figure 4 the upper panel shows the number of “capital-flow management events” around the election week which is marked with a vertical line. There is mass to both sides of the election week, and it seems more concentrated close to this week. This supports the view, that changes in capital controls happen close to elections. The event data moreover show that changes in capital controls are not single events. In the subsample of 36 countries covered by Forbes et al. (2015), 5.6 events per country were recorded on average with a range between 1 and 17. Comparing the number of event using symmetric time frames around the election week, we find that more changes take place after the election. However, the difference is only significant with a relatively small window of 4 weeks, and gets insignificant the larger the window.

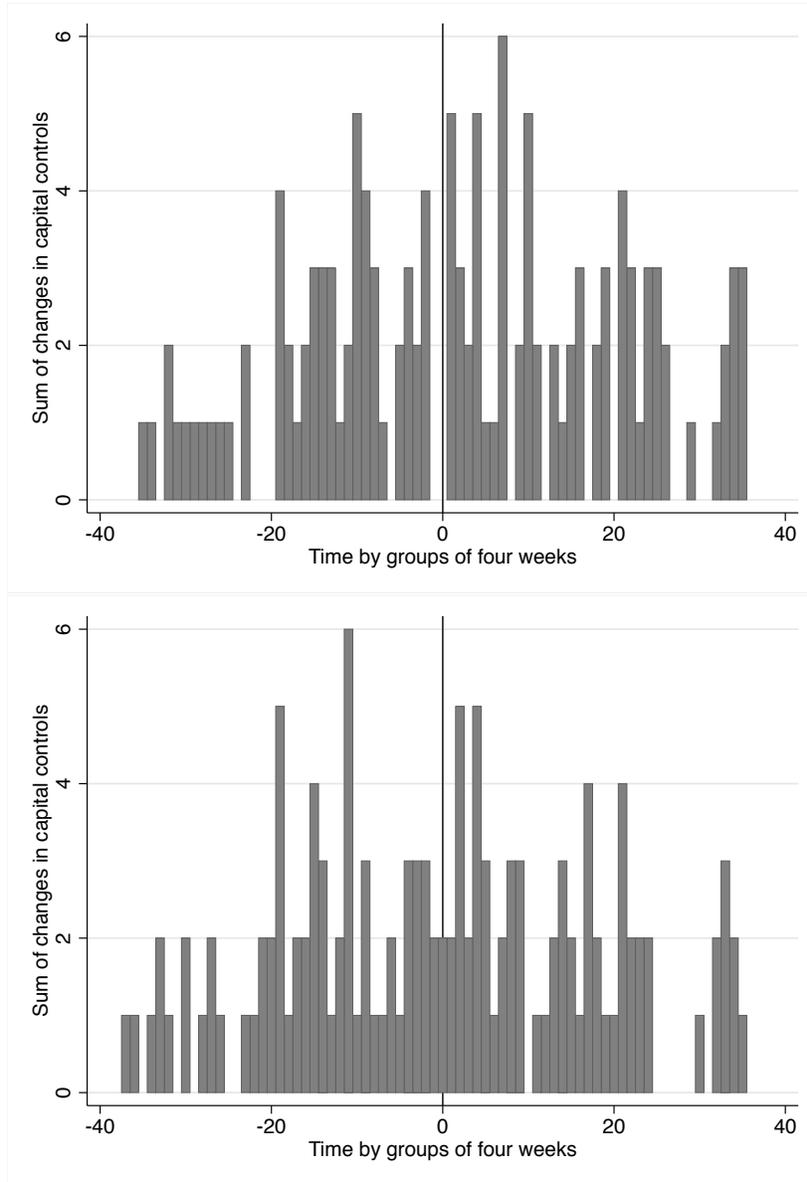
One possibility would be that after elections it typically takes time until governments are formed. For all elections in the sample, we collected the exact date of either the first parliamentary session in parliamentary systems, or the president’s inauguration in presidential ones and repeated the analysis around the week of government formation. We do not find major changes compared to the analysis using the election date (lower panel of Figure 4).

In sum, we find that changes in capital restrictions seem to take place rather close to elections. While they happen more frequently directly after the election, events also take place before the election week.

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<sup>18</sup>The disaggregated graphs showing the number of events per week are in Figure F1 in the Appendix. We also show more aggregated graphs at levels of 12, 24, and 48 weeks in Figures F2–F4. For completeness, we also show the graphs of changes in outflow and inflow controls in the Appendix.

**Figure 4:** By four weeks



NOTE: Based on data from Forbes, Fratzscher and Straub (2015), encompassing observations from 36 countries. The bars show the sum of changes in capital controls aggregated over periods of four weeks. Time is centered around the election week (upper panel) and the week of government formation (lower panel) marked by a vertical line.

### 4.3 Ideological changes

Do capital restrictions change because elections take place per se, or is this related to new governments being installed? We investigate this point by collecting information about political leaders and their parties, documenting for each election whether the old government remained in place. In total, there are 308 leader and 229 party changes. 78% and 88% of them happen during elections respectively.<sup>19</sup> During elections, leaders change in 59% of cases, and parties in 50%. The variables *Change Leader* and *Change Party* take on the value 1 if the political leader or the governing coalition has changed in a given year. In presidential systems, the president is the leader and his party the governing party. In parliamentary systems, the prime minister is the leader, and the governing coalition defines the party.<sup>20</sup> Both variables are highly correlated: a change in party virtually always coincides with a change in leadership (97%). If leaders change, however, this implies a change in governing party in fewer cases (72%). We add these variables and their interaction with the election dummy to the baseline model. We show the results separately for the aggregate, outflows and inflows (Table 8).

Capital controls tend to increase in election years even when there is no change in the leadership or party as indicated by the positive *Election* coefficient and insignificant interaction term (column (1)). Leader changes occur by definition when the incumbent is not running for reelection. Repeating the exercise for the indicator *Incumbent running* instead, leads to the same conclusion (column (2)). Also, jointly controlling for incumbency and leader changes not triggered by incumbents not running yields the same result (column (3)). Party changes during elections do not trigger significant adjustments of capital controls on top of what is observed when governing coalitions remain in power (column (4)). Even when both leaders and parties change simultaneously during an election, capital controls do not change differently (column (5)). If changes in leadership or parties happen outside election years, we find only small and insignificant changes in capital controls throughout all specifications.

The results indicate that changes in capital controls in election years do not de-

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<sup>19</sup>Political leaders can change without triggering a new election when e.g. the prime minister is replaced during an ongoing term, or an interim government takes over after a resignation.

<sup>20</sup>If more than one party forms the governing coalition, changes in one of these parties are also defined as changes in party.

**Table 8:** GOVERNMENT CHANGES CANNOT EXPLAIN ADJUSTMENTS IN CAPITAL CONTROLS DURING ELECTIONS

	(1)	(2)	(3)	(4)	(5)
	Leader	Leader	Leader	Party	Party Leader
Election	0.963* (0.528)	1.599*** (0.490)	1.576*** (0.495)	1.044** (0.471)	1.077** (0.467)
Election x Change Leader	1.311 (1.087)				
Change Leader	-0.390 (0.961)				
Election x Incumbent running		0.559 (1.960)	0.653 (1.972)		
Incumbent running		-0.875 (1.935)	-0.862 (1.945)		
Election x Change Leader			-0.223 (1.461)		
Change Leader			-0.372 (0.962)		
Election x Change Party				0.814 (1.210)	
Change Party				0.179 (1.146)	
Election x Change Leader and Party					0.950 (1.235)
Change Leader and Party					-0.002 (1.169)
$KC_{c,t-1}$	-0.209*** (0.024)	-0.209*** (0.024)	-0.209*** (0.024)	-0.209*** (0.024)	-0.209*** (0.024)
Observations	1,914	1,914	1,914	1,914	1,914
Country FE	YES	YES	YES	YES	YES
R-squared	0.130	0.129	0.128	0.130	0.130

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered at country level. Ordinary least squares regression. Based on 98 countries between 1995 and 2015. The dependent variable is the change in capital controls. *Change Leader* and *Change Party* take on value 1 if the political leader or the governing party has changed in a given year.

pend on whether the same government stays in place or a new one gets elected. Changes in restriction intensity can, however, depend on the government’s ideology. Ideology does not change when the incumbent gets reelected or when the new government shares its predecessor’s ideology. But when ideology changes, the direction of changes in capital controls could depend on whether the new government is more leftist or rightist than the previous one. After elections, leftist governments might have an incentive to install capital controls on outflows to prevent a flight of their tax base—in anticipation of higher taxes introduced by the left government, as documented by Oatley (1999) and Quinn and Inclan (1997), and illustrated by the Mitterrand case in France mentioned in the introduction. Consequently, we would expect higher (lower) capital controls when more leftist (rightist) governments get elected.

Information on political ideology of the governing party (left, center and right) comes from the World Bank’s *Database on Political Institutions* (DPI, 2015). We cross-checked the data on government ideology with multiple online sources and extended it to a five point scale, focusing on economic policy as a defining criterion: left, center-left, center, center-right and right.<sup>21</sup> We define left as 1 and right as 5 to calculate *Right-move* as the difference in ideologies between two years for each country. Positive (negative) numbers indicate ideological movement to the right (left), and 0 no change. Among the 410 elections in our sample 76 (18.5%) see a more leftist government. In 78 cases (19%) the new government is more rightist than the previous one. In the remaining 256 cases (62.4%), government ideology does not change.

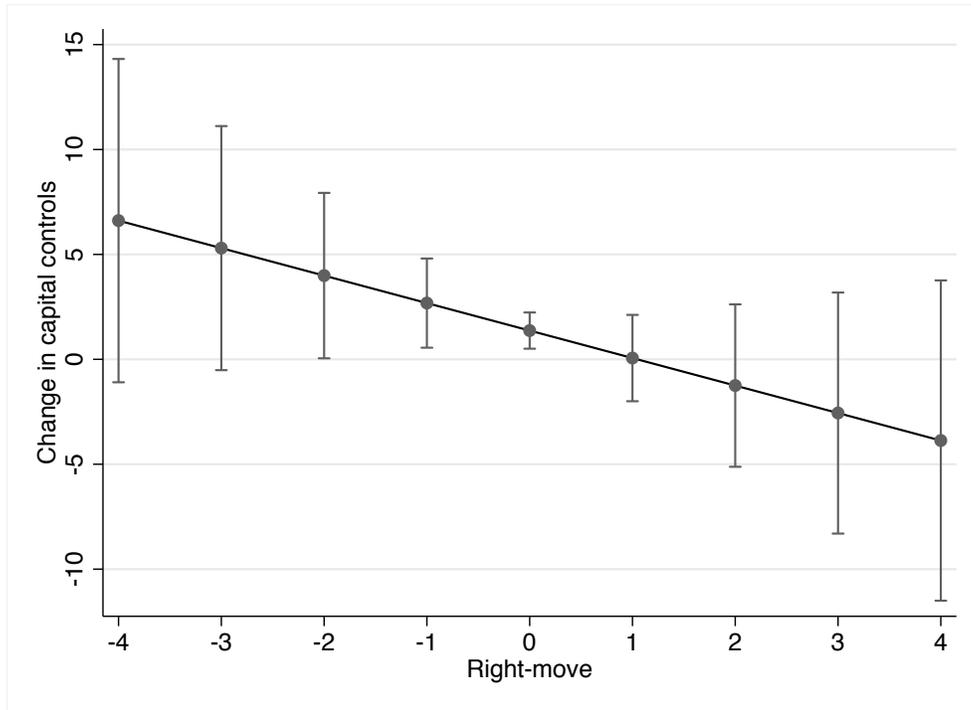
Figure 5 shows the marginal effects of ideological switches on changes in capital controls (regression results are in Table C7). The overall relation between movements to the right and changes in capital flows is negative: the larger the rightwards movement of the government is the more do capital controls decrease, and the opposite for leftwards movements. The marginal effects are significant for left movements by one and two categories (labeled as  $-1$  and  $-2$ ), as well as for no ideological changes.

If leftist governments are rather homogeneous in their call for larger governments and more taxation, their position vis-à-vis market economy may be heterogenous.

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<sup>21</sup>We drop observations from a sixth category that encompasses cases for which a proper classification is meaningless, for instance during a revolution or a civil war. But also for some cases ideology is missing for the entire observation period (e.g. Bahrain, Myanmar, and Saudi Arabia).

**Figure 5:** Capital controls decrease with ideological right-movements of governments



NOTE: Left is defined as 1 and right as 5. *Right-move* is calculated as the difference in ideologies between two years for each country. Positive (negative) numbers indicate ideological movement to the right (left), and 0 no change. The figure shows marginal effects of ideological switches on changes in capital controls.

Similarly, the label “right-wing” encompasses a variety of ideologies ranging from conservative to liberal, masking different attitudes towards globalization and redistribution. The traditional left-right may have lost in relevance, the new cleavage being for or against globalization (Kriesi et al., 2006). This political recomposition would imply a common support for tighter capital restriction from both the far-left and the far-right (Halikiopoulou et al., 2012).<sup>22</sup>

We turn to more fine-grained measures using the Manifesto Project Dataset.<sup>23</sup> It provides information on a government’s stance over various political dimensions us-

<sup>22</sup>Anecdotally, far-right candidate Marine Le Pen, who qualified for the second round of the 2017 French presidential election, declared during the campaign that, if elected, raising capital controls was an option.

<sup>23</sup>Available at <https://manifesto-project.wzb.eu>.

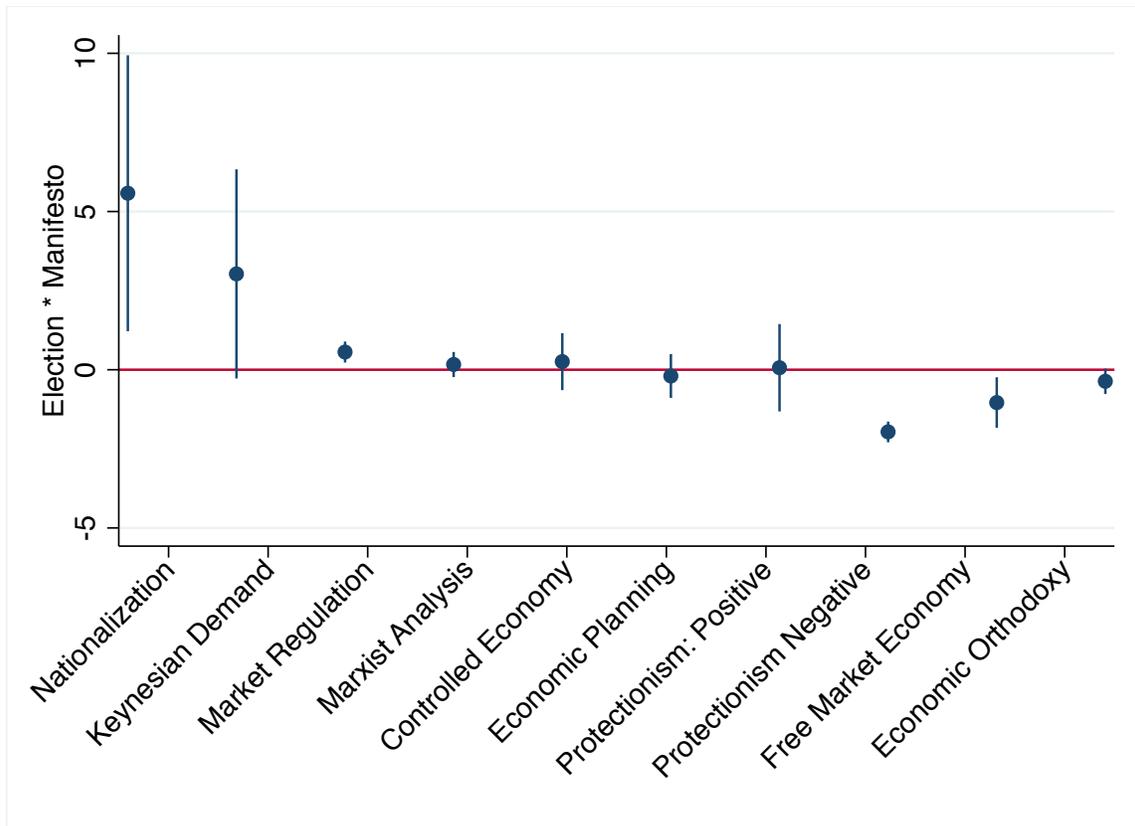
ing a text analysis of their manifestos at each election. It covers 37 countries, most of them members of the OECD. We focus on the variables related to economic policy, and categorize them into variables indicating less liberal policies when increasing, and more liberal ones when increasing. For the less liberal ones we have: *Nationalization*, *Keynesian Demand*, *Market Regulation*, *Marxist Analysis*, *Controlled Economy*, *Economic Planning*, and *Protectionism: Positive*; for the more liberal ones: *Protectionism: Negative*, *Free Market Economy*, and *Economic Orthodoxy*. The variables are coded in such a way that they record the change in the respective variable compared to the previous government in place. Similarly to above, we interact each variable with the *Election* indicator, and do not control for the baseline term of the Manifesto variables for the same reasons. We estimate:

$$\Delta KC_{c,t} = \alpha + \beta Election_{c,t} + \gamma Election_{c,t} \times Manifesto_{c,t} + \xi X_{c,t} + u_c + \tau_t + \epsilon_{c,t}$$

The main election coefficient is positive and highly significant for all specifications (between 2.879 and 2.910). The coefficients of the interaction terms with 90% confidence intervals are presented in Figure 6 (the corresponding regression results are in Table C8 in the Appendix). The seven left coefficients relate to less liberal stances, and the three on the right to more liberal ones. With a single exception (*Economic Planning*), all coefficients of less liberal variables are positive, two of which are significant (*Nationalization* and *Market Regulation*). Elected governments with less liberal views on average increase capital controls on outflows by more than other types of governments. All variables associated to more liberal policies have a negative coefficient, two of them are significant (*Protectionism: Negative* and *Free Market Economy*): governments with more liberal ideologies increase capital controls by less than the other governments.

These results provide additional evidence of a conditional effect of elections on capital controls depending on partisan politics and ideological switches.

Figure 6: Manifesto



NOTE: Standard errors clustered at country level. Ordinary least squares regression. Based on the sample of 37 countries for which Manifesto data are available. The dependent variable is the change in capital controls on outflows. The independent variables are an election indicator and an interaction between the election indicator and a Manifesto variable. Country and year fixed effects are controlled for. The scatter plot shows the point estimates of the interaction coefficient including 90% confidence intervals. The seven (three) specifications on the left (right) relate to less (more) liberal policy views.

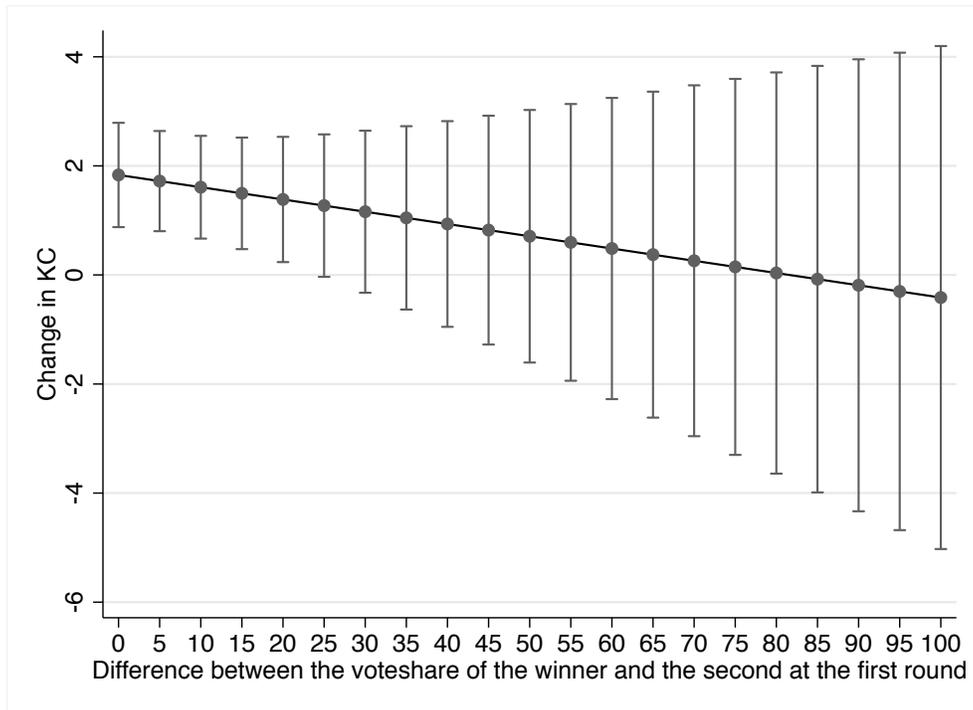
## 4.4 Electoral uncertainty

The above results suggest that ideological changes matter for the evolution of capital controls. Nevertheless, capital controls also increase during elections without ideological switches (recall Figure 5). Moreover, we have seen in Table 8 that even after controlling for leader or party changes, the election dummy remains highly significant. Why would capital controls increase even if the political set up remains identical? Moreover, even if changes in the level of controls are more prevalent in the aftermath of an election, we also observe changes during the pre-election period. Possibly, uncertainty regarding the electoral outcome may drive the increase of restrictions. The threat of a political change can push the incumbent government to raise the level of controls in order to either to prevent or limit capital outflows. The higher the level of electoral uncertainty, the higher the increase of controls.

To investigate this mechanism, we require a measure of electoral uncertainty which allows for cross-country comparisons over time. Because of institutional heterogeneity, this is not a trivial task. For instance, some countries use proportional systems whereas others use majoritarian systems; some countries have single-round elections whereas others have two-round systems. With such constraints in mind, we use as a measure of electoral uncertainty the difference between the vote share of the two leading parties/candidates in the first election round. This *ex post* measure of competition is arguably rather crude, but has the advantage of being available for all countries. An implicit assumption associated with the use of this measure is that *ex ante* and *ex post* uncertainty are correlated. We compute this vote margin using the DPI dataset for parliamentary elections, and we collected additional data for presidential elections. We then run a model controlling for the interaction between the election indicator and the vote margin, restricting the subsample to democracies.

The marginal effect of elections on average capital controls conditional on the vote margin is displayed in Figure 7 with the vote margin on the  $x$ -axis. Looking at the overall level of capital controls, as hypothesized, the level of restrictions increases when the electoral outcome becomes more uncertain: the point estimate for very tight elections (less than a 5 points margin) is about 2, whereas elections do not have any effect once the margin exceeds 25 points.

**Figure 7:** Capital controls increase more if election outcomes are uncertain



NOTE: Expected change in average capital controls conditional on electoral uncertainty. Electoral uncertainty is measured as the difference between the vote share of the two leading parties/candidates in the first election round. 95% confidence interval bands. Restricted to the subsample of democracies.

## 5 Conclusion

A growing theoretical literature suggests that capital controls should be used as a countercyclical policy. However, the empirical literature did not find a link between capital controls and the business cycle nor macroeconomic variables. This paper offers an alternative view on capital controls: the political economy perspective. Since regulation regarding capital flow restriction is mostly in the hands of governments, we investigate whether they use it for other than purely economic reasons.

Our empirical analysis provides some evidence that capital controls do follow an electoral cycle: changes in capital controls peak during election years and are small otherwise. Looking at differential effects, we find that most of our average result is driven by restrictions on capital outflows, which happen more often after elections. While capital controls increase regardless of whether incumbents are reelected or replaced by a new government, capital controls increase more if the new government is more left-wing than its predecessor.

For future research, a lot of work remains to be done in order to identify and distinguish the precise channel of this relationship. On the empirical side, the next steps regarding data collection would be to extend the dataset in terms of countries, years, and the frequency of measuring capital controls. Paired with even richer information about election and government characteristics, such data would allow to investigate more in details the channels through which the electoral mechanism influences the intensity of capital controls. Another important extension would be to use a *de facto* measure of capital controls. Constructing such a measure is however not a trivial task. A possibility would be to use the difference between observed gross capital flows and what would be observed with free capital mobility. However, as explained by Fernández et al. (2016), there is no benchmark for the latter.

On the theoretical side, the existing theories provide partly competing predictions that do not provide sufficient ground for explaining the systematic increase of capital controls during election years that we observe. If many recent theoretical models examine the cyclical properties of capital controls, this paper provides strong evidence that the electoral cycle has to be taken into account. A unified theory encompassing various political motifs is called for.

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# Appendix

## A Data Sources

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Variable	Source(s)	Detailed description
Capital controls	Fernandez et al. (2016)	Annual measure of capital controls in the aggregates, on outflows, inflows and ten subcategories. Measured annually at the end of the year. For 100 countries between 1995 and 2015.
Political system	DPI (2015)	Allocation into presidential and parliamentary system
Dates of parl. elections	DPI (2015)	
Dates of presidential elections	Various internet sources	
GDP per capita	World Bank	
GDP growth	World Bank	
Gov. expenditure over GDP	World Bank	
Inflation	IMF	
Central bank independence	Garriga (2016) based on Cukierman (1992)	CBI Garriga (weighted)
Corruption	World Governance Indicators	Perceived corruption
Democracy	Polity4	Index ranging from $-10$ to $10$ . If the value is greater than or equal to zero, the country is classified as a democracy.
Openness	IMF	Trade openness defined as the sum of imports and exports over GDP
Early/planned elections	Various internet sources	Evidence whether elections took place at a date planned in advance or were held earlier.
Euro area	European Central Bank	Indicator taking on value one if a country was member of the euro area in a given year.
Dates of changes in capital controls	Forbes et al. (2011)	Precise dates of events changing capital controls for a subsample of 36 countries between 2009 and 2011.
Names of leaders and parties	Various internet sources	
Government ideology	DPI; various internet sources	

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## B Names of countries included in sample

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Algeria	Guatemala	Paraguay
Angola	Hungary	Peru
Argentina	Iceland	Philippines
Australia	India	Poland
Austria	Indonesia	Portugal
Bahrain	Iran. Islamic Rep.	Qatar
Bangladesh	Ireland	Russian Federation
Belgium	Israel	Saudi Arabia
Bolivia	Italy	Singapore
Brazil	Jamaica	Slovenia
Brunei Darussalam	Japan	South Africa
Bulgaria	Kazakhstan	Spain
Burkina Faso	Kenya	Sri Lanka
Canada	Korea. Rep.	Swaziland
Chile	Kuwait	Sweden
China	Kyrgyz Republic	Switzerland
Colombia	Latvia	Tanzania
Costa Rica	Lebanon	Thailand
Cote d'Ivoire	Malaysia	Togo
Cyprus	Malta	Tunisia
Czech Republic	Mauritius	Turkey
Denmark	Mexico	Uganda
Dominican Republic	Moldova	Ukraine
Ecuador	Morocco	United Arab Emirates
Egypt. Arab Rep.	Myanmar	United Kingdom
El Salvador	Netherlands	United States
Ethiopia	New Zealand	Uruguay
Finland	Nicaragua	Uzbekistan
France	Nigeria	Venezuela. RB
Georgia	Norway	Vietnam
Germany	Oman	Yemen. Rep.
Ghana	Pakistan	Zambia
Greece	Panama	

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NOTE: Countries with available measures of capital controls for the years 1995–2015 from Fernández et al. (2016).

## C Regressions mentioned in main part

**Table C1:** ELECTORAL CYCLE OF CAPITAL CONTROLS

	(1) KC	(2) KCO	(3) KCI
Election-2	0.180 (0.637)	0.194 (0.810)	0.141 (0.710)
Election-1	0.101 (0.530)	0.223 (0.668)	-0.116 (0.561)
Election	1.395** (0.549)	2.033*** (0.696)	0.710 (0.643)
Election+1	-0.371 (0.483)	-0.518 (0.649)	-0.267 (0.523)
Election+2	-0.198 (0.655)	-0.026 (0.808)	-0.445 (0.701)
KC <sub>c,t-1</sub>	-0.207*** (0.024)		
KCO <sub>c,t-1</sub>		-0.207*** (0.023)	
KCI <sub>c,t-1</sub>			-0.243*** (0.031)
Observations	1,956	1,956	1,956
Country FE	YES	YES	YES
Year FE	YES	YES	YES
R <sup>2</sup>	0.127	0.118	0.149

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. Country and time fixed effects. The dependent variable is the change in capital controls. The lagged value of the dependent variable is controlled for.

**Table C2: ROBUSTNESS: MAIN RESULTS**

VARIABLES	(1)	(2)	(3)	(4)
	Main controls	Full set controls	Main controls	Full set controls
Election	1.492*** (0.438)	1.078** (0.502)	1.519*** (0.443)	1.127** (0.495)
KC <sub>c,t-1</sub>	-0.211*** (0.026)	-0.232*** (0.034)	-0.217*** (0.026)	-0.248*** (0.034)
GDP cap <sub>c,t-1</sub>			0.000 (0.000)	-0.000 (0.000)
GDP growth <sub>c,t-1</sub>			0.095* (0.053)	0.102 (0.066)
Gov expenditure <sub>c,t-1</sub>			0.102 (0.075)	0.161 (0.140)
REER <sub>c,t-1</sub>			-0.004 (0.003)	-0.002 (0.002)
Peg			1.990 (2.222)	2.921 (3.657)
Crawling peg			2.616 (2.169)	5.146 (3.566)
Managed float			2.476 (2.090)	4.115 (3.451)
Free falling			3.045 (2.631)	3.405 (4.411)
Dual market			6.525** (2.807)	7.990 (6.091)
Inflation <sub>c,t-1</sub>			0.018*** (0.003)	0.022*** (0.004)
Central Bank independence				-1.482 (2.072)
Polity				0.014 (0.120)
Corruption				2.182 (1.765)
Openness <sub>c,t-1</sub>				-0.526 (2.892)
Observations	1,824	1,064	1,824	1,064
Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
R <sup>2</sup>	0.135	0.152	0.139	0.162

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is the change in capital controls. The lagged value of the dependent variable is controlled for. (1) and (2) are restricted to the sample for which the main covariates and the full set of covariates are available respectively. In (3) and (4) covariates are controlled for.

**Table C3: ROBUSTNESS: GMM**

VARIABLES	(1) KC	(2) KCO	(3) KCI	(4) KC	(5) KCO	(6) KCI
Election	1.412*** (3.559)	2.030*** (4.292)	0.691 (1.382)	1.453*** (3.021)	1.997*** (4.008)	0.727 (1.203)
Constant				1.392 (0.590)	5.563** (2.049)	0.000 (.)
Observations	1,551	1,551	1,551	1,649	1,649	1,649
Controls	YES	YES	YES	YES	YES	YES
Model	Diff	Diff	Diff	System	System	System
Hansen test (p-value)	0.233	0.190	0.229	0.906	0.816	0.650
AR1 test (p-value)	0.0191	0.00234	0.00600	7.07e-05	0.000899	0.000103
AR2 test (p-value)	0.312	0.0941	0.851	0.796	0.159	0.220

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. The dependent variable is the change in capital controls: aggregate (1), (4); outflows (2), (5); inflows (3), (6). Models (1)–(3) are estimated using Difference-GMM (Arellano and Bond, 1991) models (4)–(6) are estimated using System-GMM (Arellano and Bover, 1995), with two-period lags used as instruments. Country fixed effects and time fixed effects are introduced in all models.

**Table C4:** ROBUSTNESS: BMA

VARIABLES	(1) KA	(2) KAO	(3) KAI	(4) KA	(5) KAO	(6) KAI
Election	1.481 (3.454)	2.120 (3.974)	0.875 (1.901)	0.876 (1.783)	1.554 (2.319)	0.209 (0.403)
KC <sub>c,t-1</sub>	-0.222 (-15.597)			-0.280 (-13.398)		
KCO <sub>c,t-1</sub>		-0.224 (-15.002)				
KCI <sub>c,t-1</sub>			-0.249 (-16.613)			
GDP cap <sub>c,t-1</sub>	-0.000 (-0.022)	-0.000 (-0.148)	0.000 (0.134)	-0.000 (-0.238)	-0.000 (-0.307)	-0.000 (-0.071)
GDP growth <sub>c,t-1</sub>	0.002 (0.110)	0.000 (0.024)	0.004 (0.174)	0.007 (0.217)	0.005 (0.164)	0.005 (0.189)
Gov expenditure <sub>c,t-1</sub>	0.002 (0.102)	0.002 (0.082)	0.002 (0.091)	0.006 (0.139)	0.006 (0.117)	0.002 (0.052)
REER <sub>c,t-1</sub>	-0.000 (-0.204)	-0.000 (-0.094)	-0.001 (-0.350)	-0.000 (-0.188)	-0.000 (-0.119)	-0.001 (-0.222)
Peg	-0.004 (-0.031)	0.007 (0.037)	-0.024 (-0.112)	-0.030 (-0.099)	-0.075 (-0.144)	0.007 (0.024)
Crawling peg	-0.001 (-0.010)	-0.013 (-0.085)	0.012 (0.086)	0.154 (0.290)	0.053 (0.150)	0.382 (0.438)
Managed float	-0.001 (-0.005)	0.006 (0.041)	-0.005 (-0.039)	-0.020 (-0.085)	-0.006 (-0.026)	-0.056 (-0.152)
Free falling	0.013 (0.055)	0.055 (0.125)	-0.010 (-0.039)	-0.096 (-0.144)	0.010 (0.017)	-0.763 (-0.380)
Dual market	0.210 (0.192)	0.020 (0.035)	2.024 (0.560)	0.253 (0.166)	0.349 (0.168)	0.066 (0.068)
Inflation <sub>c,t-1</sub>	0.004 (0.541)	0.004 (0.503)	0.002 (0.341)	0.012 (1.227)	0.015 (1.143)	0.003 (0.480)
Central Bank independence <sub>c,t-1</sub>				-0.339 (-0.232)	-0.636 (-0.269)	-0.093 (-0.116)
Polity				0.003 (0.076)	0.000 (0.011)	0.004 (0.107)
Corruption, WGI				0.159 (0.232)	0.073 (0.134)	0.209 (0.257)
Openness <sub>c,t-1</sub>				-0.025 (-0.061)	-0.180 (-0.178)	0.065 (0.118)
Constant	-2,208.204 (-3.361)	-3,752.320 (-4.515)	-859.822 (-1.236)	-4,433.432 (-7.713)	-5,523.542 (-7.611)	-3,705.596 (-6.330)
Observations	1,668	1,668	1,668	976	976	976
Country FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Model space	2048	2048	2048	32768	32768	32768

NOTE: The dependent variable is the change in capital controls: aggregate (1), (4); outflows (2), (5); inflows (3), (6). Election, country fixed effects and time fixed effects are introduced as focus regressors, all the other variables are introduced as auxiliary. The  $t$  statistics is provided in parentheses. A regressor is considered robustly correlated with the outcome if the  $t$  statistics is greater than one in absolute value (De Luca et al. 2011).

**Table C5: ROBUSTNESS: DROPPING COUNTRIES WITHOUT ELECTIONS**

VARIABLES	(1) KA	(2) KAO	(3) KAI
Election	1.490*** (0.451)	2.086*** (0.529)	0.915* (0.483)
KC <sub>c,t-1</sub>	-0.203*** (0.024)		
KCO <sub>c,t-1</sub>		-0.213*** (0.023)	
KCI <sub>c,t-1</sub>			-0.232*** (0.031)
Observations	1,717	1,717	1,717
R-squared	0.170	0.163	0.182
Country FE	YES	YES	YES
Year FE	YES	YES	YES
r2_a_within	0.124	0.122	0.141

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is respectively the change in capital controls, the change in controls on outflows and the change in controls on inflows. Year and country fixed effects are always controlled for. Eleven countries which never hold elections in the period 1995–2015 are dropped.

**Table C6:** ROBUSTNESS: JACKKNIFE STANDARD ERRORS

	(1)	(2)	(3)
	KC	KCO	KCI
Election	1.493*** (0.450)	2.088*** (0.529)	0.923* (0.481)
$KC_{c,t-1}$	-0.207*** (0.024)		
$KCO_{c,t-1}$		-0.208*** (0.024)	
$KCI_{c,t-1}$			-0.243*** (0.032)
Constant	7.194*** (1.541)	7.318*** (1.714)	8.359*** (1.716)
Observations	1,956	1,956	1,956
Number of countries	98	98	98
Country FE	YES	YES	YES
Year FE	YES	YES	YES
Controls	YES	YES	YES
Adj. R <sup>2</sup>	0.131	0.124	0.152

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Jackknife standard errors clustered at country level. Ordinary least squares regression. The dependent variable is the change in capital controls. Year and country fixed effects are always controlled for.

**Table C7: IDEOLOGICAL MOVEMENTS OF GOVERNMENTS**

	(1) KC	(2) KCO	(3) KCI
Election	1.484*** (0.448)	2.086*** (0.528)	0.906* (0.479)
Right-move	1.258 (0.937)	1.473 (1.249)	1.065 (0.798)
Election x right-move	-1.175 (1.024)	-1.644 (1.331)	-0.700 (0.906)
$KC_{c,t-1}$	-0.207*** (0.024)		
$KCO_{c,t-1}$		-0.207*** (0.023)	
$KCI_{c,t-1}$			-0.243*** (0.031)
Observations	1,956	1,956	1,956
Country FE	YES	YES	YES
Year FE	YES	YES	YES
R <sup>2</sup>	0.128	0.119	0.151

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is respectively the change in capital controls, the change in controls on outflows and the change in controls on inflows. Year and country fixed effects are always controlled for.



## D Tables in main part separately for controls on capital outflows and inflows

**Table D1:** CAPITAL CONTROLS ON OUTFLOWS INCREASE IN ELECTION YEARS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Raw	Year FE	Country FE	Country/ Year FE	Baseline	Main controls	Full set controls
<b>OUTFLOWS</b>							
Election	1.949*** (0.536)	1.874*** (0.557)	2.223*** (0.560)	2.147*** (0.583)	2.088*** (0.525)	2.116*** (0.529)	1.926*** (0.643)
$KCO_{c,t-1}$					-0.208*** (0.023)	-0.219*** (0.025)	-0.276*** (0.029)
R <sup>2</sup>	0.007	0.021	0.037	0.051	0.119	0.129	0.170
<b>INFLOWS</b>							
Election	0.765 (0.505)	0.764 (0.515)	0.931* (0.524)	0.932* (0.534)	0.923* (0.477)	0.976** (0.463)	0.365 (0.556)
$KCI_{c,t-1}$					-0.243*** (0.031)	-0.247*** (0.033)	-0.254*** (0.041)
R <sup>2</sup>	0.001	0.012	0.036	0.048	0.151	0.154	0.157
Obs.	1,956	1,956	1,956	1,956	1,956	1,824	1,064
Country FE	NO	NO	YES	YES	YES	YES	YES
Year FE	NO	YES	NO	YES	YES	YES	YES

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is the change in capital controls on outflows (inflows) in the upper (lower) part of the table. (1) includes no fixed effects, (2) year fixed effects, (3) country fixed effects, (4)–(7) year and country fixed effects. In (5)–(7) the lagged value of the dependent variable is controlled for. In (6) and (7) covariates are controlled for.

**Table D2:** ADDITIONAL EVIDENCE 1: OUTFLOWS

	(1)	(2)	(3)	(4)
	Planned elections	Democracy	Autocracy	Interacted
Election		2.273*** (0.580)	1.349 (1.449)	2.239*** (0.567)
Election x Autocracy				-1.079 (1.556)
Autocracy				1.153 (1.624)
Election on time	1.458*** (0.524)			
$KCO_{c,t-1}$	0.787*** (0.025)	-0.220*** (0.021)	-0.289** (0.129)	-0.234*** (0.025)
Observations	1,856	1,361	474	1,836
Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
R <sup>2</sup>	0.662	0.123	0.146	0.133

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is the change in capital controls on outflows. Year and country fixed effects are always controlled for. In (1) all early elections are dropped. (2) is restricted to democracies (Polity4 index greater or equal to zero), (3) is the subsample of autocracies (polity4 index smaller than zero). (4) is based on the full sample and an interaction between the election indicator and an indicator taking on value 1 for autocracies.

**Table D3:** ADDITIONAL EVIDENCE 1: INFLOWS

	(1)	(2)	(3)	(4)
	Planned elections	Democracy	Autocracy	Interacted
Election		0.936*	0.079	0.980*
		(0.504)	(1.418)	(0.494)
Election x Autocracy				-0.514
				(1.526)
Autocracy				-1.263
				(2.013)
Election on time	0.335			
	(0.508)			
$KCI_{c,t-1}$	0.756***	-0.236***	-0.481***	-0.277***
	(0.033)	(0.032)	(0.088)	(0.036)
Observations	1,856	1,361	474	1,836
R-squared	0.947	0.191	0.357	0.208
Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
R <sup>2</sup>	0.640	0.145	0.257	0.173

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is the change in capital controls on inflows. Year and country fixed effects are always controlled for. In (1) all early elections are dropped. (2) is restricted to democracies (Polity4 index greater or equal to zero), (3) is the subsample of autocracies (polity4 index smaller than zero). (4) is based on the full sample and an interaction between the election indicator and an indicator taking on value 1 for autocracies.

**Table D4: ADDITIONAL EVIDENCE 2: OUTFLOWS**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High	Middle	Low	Interacted	Pre '05	Post '05	Interacted	Without Eurozone
Election	2.320*** (0.797)	1.571* (0.782)	5.494*** (1.246)	1.612** (0.773)	2.155*** (0.803)	1.698*** (0.555)	2.258** (0.911)	2.039*** (0.553)
$KCO_{c,t-1}$	-0.185*** (0.027)	-0.230*** (0.039)	-0.240** (0.082)	-0.208*** (0.023)	-0.427*** (0.066)	-0.377*** (0.050)	-0.208*** (0.023)	-0.210*** (0.024)
Election x low income				2.011 (1.853)				
Election x high income				0.759 (1.068)				
Election x 2005							-0.301 (1.118)	
Observations	820	979	157	1,956	880	1,076	1,956	1,744
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
R <sup>2</sup>	0.089	0.128	0.232	0.104	0.205	0.224	0.118	0.121

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is the change in capital controls on outflows. Year and country fixed effects are always controlled for. (1)-(3) is based on subsamples of high-, middle, and low-income countries. (4) is based on the full sample and the interactions between the election indicator and two indicators taking on value 1 for low- and high-income countries respectively. Middle-income countries are the reference group. (5) is restricted to the pre-2005 sample, (6) is the subsample since 2005. (7) is based on the full sample and an interaction between the election indicator and an indicator taking on value 1 for all years since 2005 is controlled for. (8) is base on the subsample excluding euro area countries in the years they are members of the euro area.

**Table D5: ADDITIONAL EVIDENCE 2: INFLOWS**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High	Middle	Low	Interacted	Pre '05	Post '05	Interacted	Without Eurozone
Election	0.788 (0.557)	0.750 (0.837)	2.768** (0.922)	0.773 (0.807)	1.328* (0.677)	0.255 (0.482)	1.434* (0.727)	0.995* (0.551)
Election x low income				1.373 (1.123)				
Election x high income				0.116 (0.979)				
Election x 2005							-0.902 (0.790)	
$KCI_{c,t-1}$	-0.189*** (0.034)	-0.272*** (0.047)	-0.334*** (0.073)	-0.243*** (0.031)	-0.434*** (0.059)	-0.315*** (0.030)	-0.243*** (0.031)	-0.248*** (0.033)
Observations	820	979	157	1,956	880	1,076	1,956	1,744
Country FE	YES							
Year FE	YES							
R <sup>2</sup>	0.128	0.143	0.234	0.138	0.233	0.174	0.151	0.151

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at country level. Ordinary least squares regression. The dependent variable is the change in capital controls on inflows. Year and country fixed effects are always controlled for. (1)–(3) is based on subsamples of high-, middle, and low-income countries. (4) is based on the full sample and the interactions between the election indicator and two indicators taking on value 1 for low- and high-income countries respectively. Middle-income countries are the reference group. (5) is restricted to the pre-2005 sample, (6) is the subsample since 2005. (7) is based on the full sample and an interaction between the election indicator and an indicator taking on value 1 for all years since 2005 is controlled for. (8) is base on the subsample excluding euro area countries in the years they are members of the euro area.

**Table D6:** DISAGGREGATED MEASURES OF CAPITAL CONTROLS ON OUTFLOWS

	(1) Collective investments	(2) Money market	(3) Equity	(4) Guarantees	(5) Derivatives
Election	4.945*** (1.016)	2.782*** (0.802)	2.986*** (0.918)	2.395** (1.050)	2.117* (1.161)
Observations	1,912	1,901	1,938	1,919	1,833
R-squared	0.059	0.050	0.047	0.046	0.033
	(6) Financial credits	(7) Direct investment	(8) Bonds	(9) Commercial credits	(10) Real estate
Election	2.083* (1.074)	1.883 (1.253)	1.917** (0.841)	0.968 (0.734)	0.414 (0.841)
Observations	1,916	1,925	1,683	1,920	1,922
R-squared	0.043	0.037	0.049	0.031	0.039
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered at country level. Ordinary least squares regression. Year and country fixed effects are always controlled for. Disaggregated measures of changes in capital controls on outflows in ten subcategories as defined by Fernández et al. (2016) are used as dependent variables.

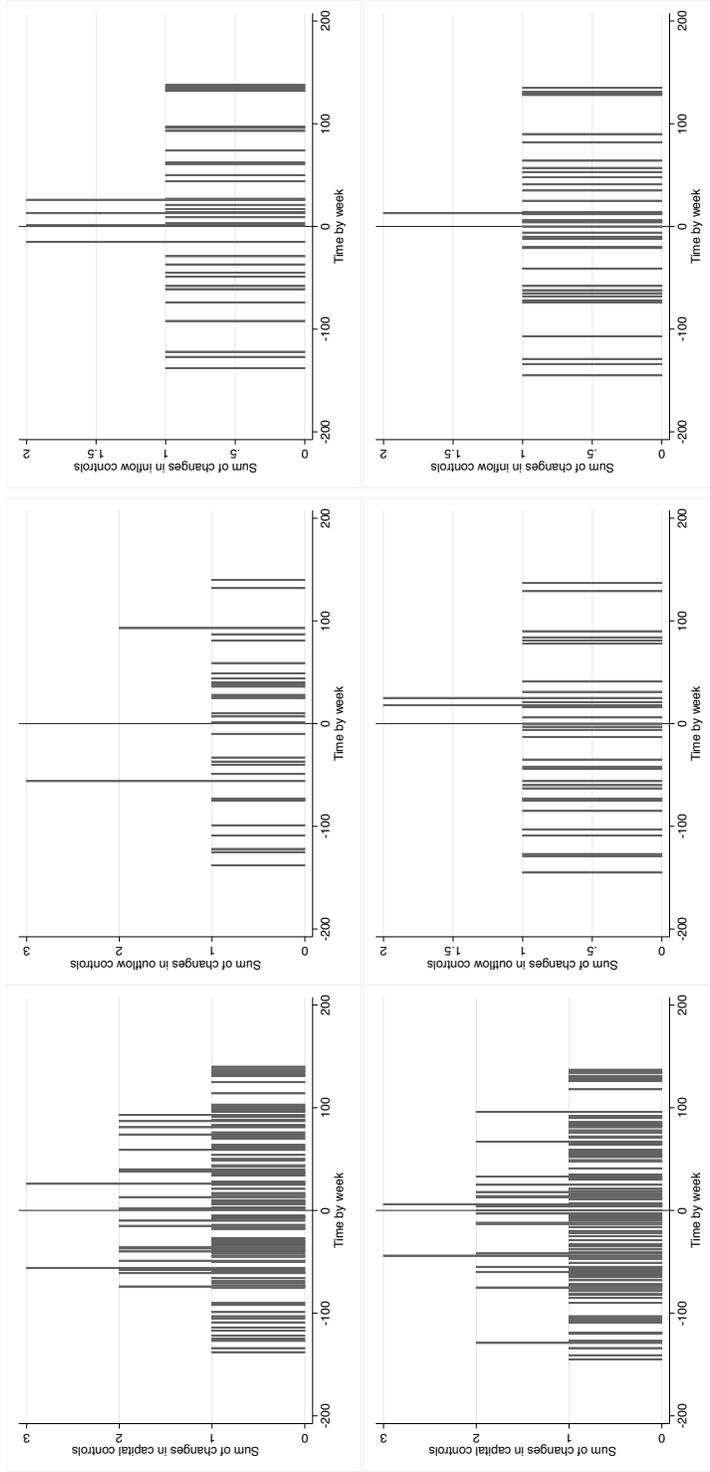
**Table D7:** DISAGGREGATED MEASURES OF CAPITAL CONTROLS ON INFLOWS

	(1)	(2)	(3)	(4)	(5)
	Collective investments	Money market	Equity	Guarantees	Derivatives
Election	2.341** (0.943)	2.504*** (0.867)	1.948** (0.820)	1.927* (1.089)	0.683 (1.099)
Observations	1,893	1,911	1,933	1,916	1,792
R-squared	0.040	0.034	0.038	0.042	0.040
	(6)	(7)	(8)	(9)	(10)
	Financial credits	Direct investment	Bonds	Commercial credits	Real estate
Election	0.674 (1.446)	0.077 (0.981)	-0.537 (1.030)	-0.420 (1.101)	-0.976 (0.860)
Observations	1,943	1,955	1,692	1,940	1,917
R-squared	0.038	0.044	0.038	0.033	0.035
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered at country level. Ordinary least squares regression. Year and country fixed effects are always controlled for. Disaggregated measures of changes in capital controls on inflows in ten subcategories as defined by Fernández et al. (2016) are used as dependent variables.

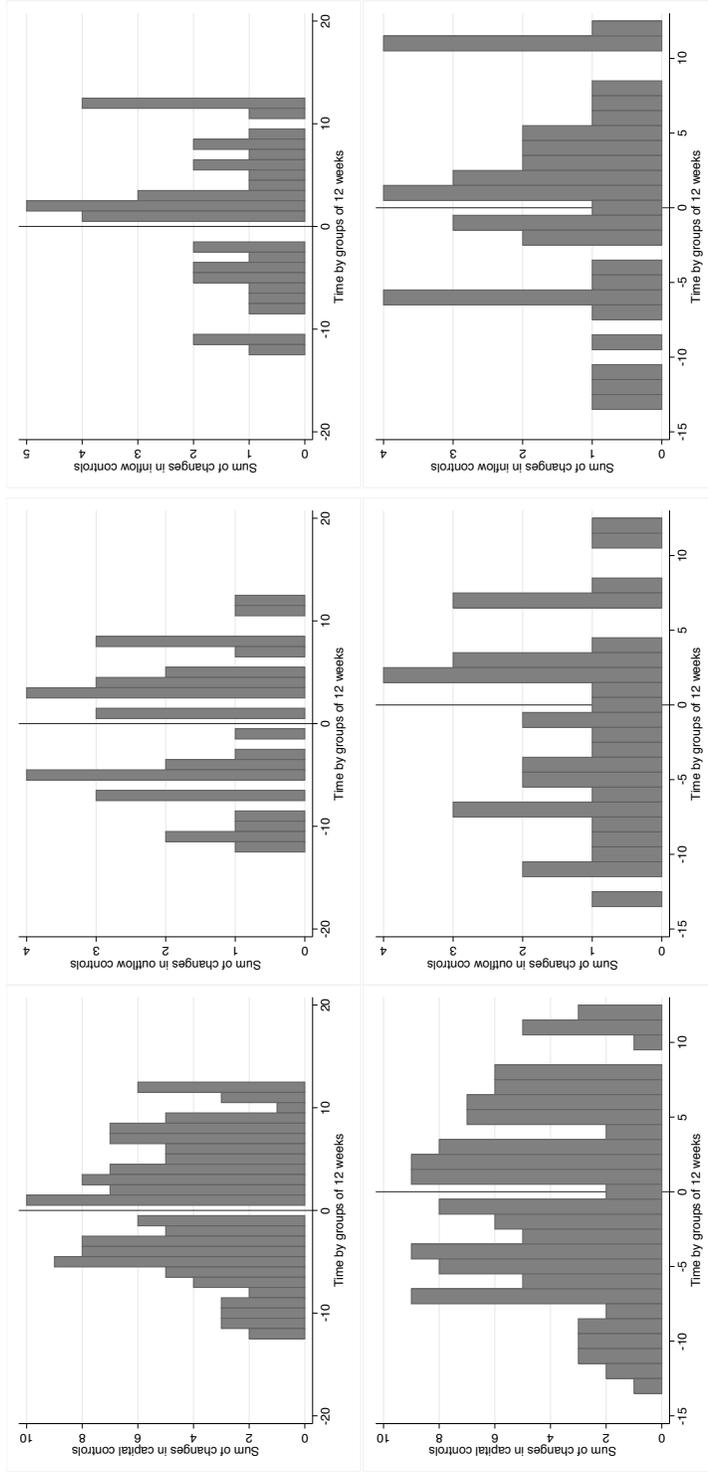
## E Figures

**Figure F1:** Changes in restriction by week



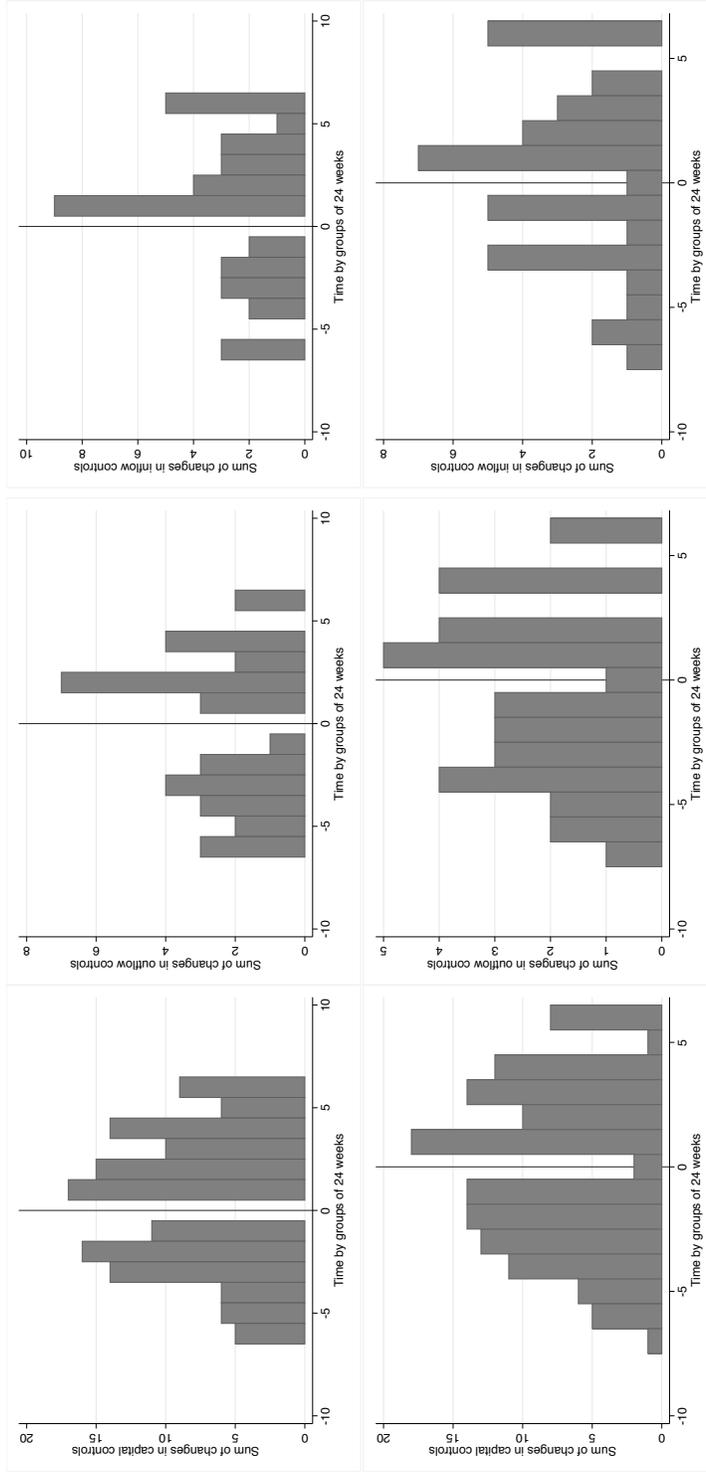
NOTE: Based on data from Forbes, Fratzscher and Straub (2015). The upper panels show the number of changes in restrictions by week around elections on overall capital controls (left), outflows (center) and inflows (right). The lower panels show the number of changes in restrictions by week around the week of government formation on overall capital controls (left), outflows (center) and inflows (right).

**Figure F2: By 12 weeks**



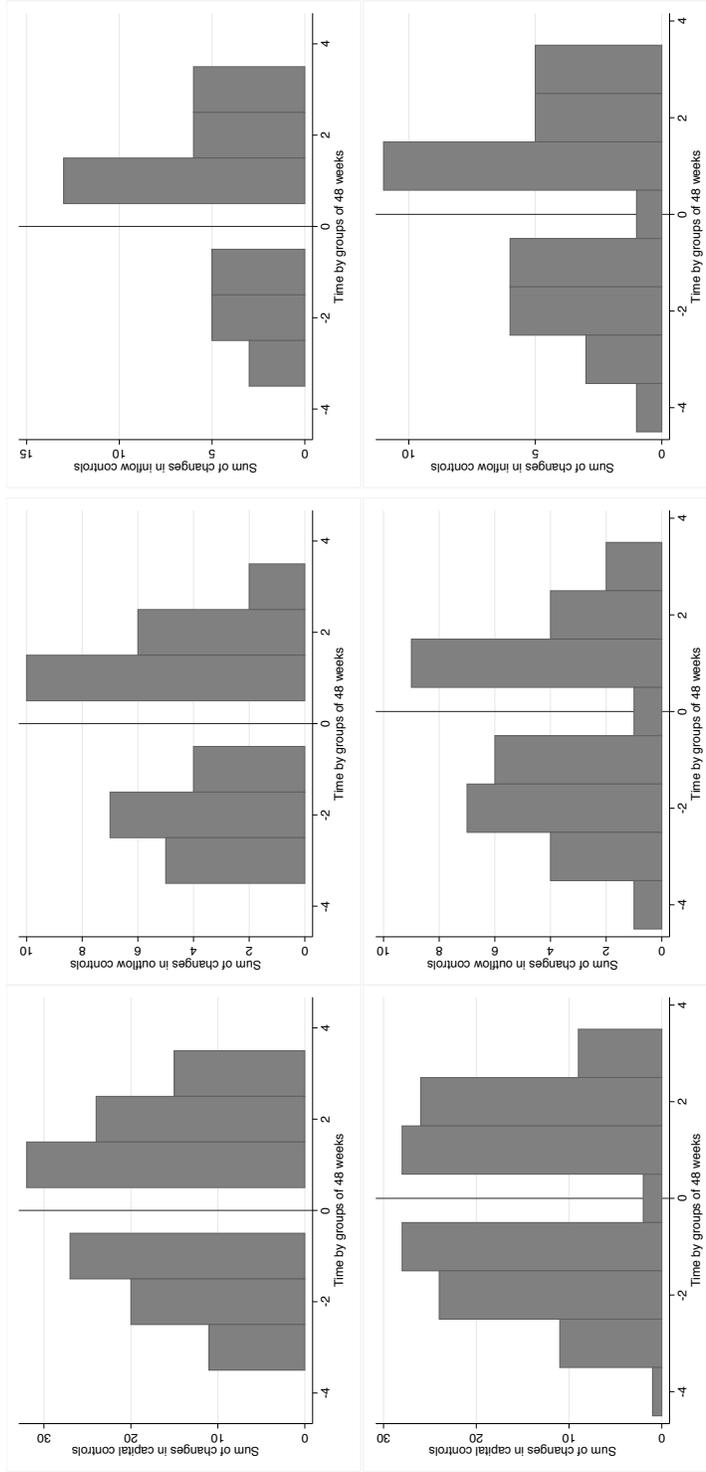
NOTE: Based on data from Forbes, Fratzscher and Straub (2015). The upper panels show the number of changes in restrictions by 12 week periods around elections on overall capital controls (left), outflows (center) and inflows (right). The lower panels show the number of changes in restrictions around the week of government formation on overall capital controls (left), outflows (center) and inflows (right).

**Figure F3: By 24 weeks**



NOTE: Based on data from Forbes, Fratzscher and Straub (2015). The upper panels show the number of changes in restrictions by 24 week periods around elections on overall capital controls (left), outflows (center) and inflows (right). The lower panels show the number of changes in restrictions around the week of government formation on overall capital controls (left), outflows (center) and inflows (right).

**Figure F4: By 48 weeks**



NOTE: Based on data from Forbes, Fratzscher and Straub (2015). The upper panels show the number of changes in restrictions by 48 week periods around elections on overall capital controls (left), outflows (center) and inflows (right). The lower panels show the number of changes in restrictions around the week of government formation on overall capital controls (left), outflows (center) and inflows (right).