Partisan shocks and financial markets: regression-discontinuity evidence from national elections.

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Abstract

We estimate the effect of partisan electoral victories on share prices, exchange rates, and sovereign bond yields and spreads. Using existing data on parliamentary elections and newly collected data on presidential elections, we obtain a sample of 929 worldwide national elections in the post-WWII period, in which main parties/candidates can be classified on the left-right scale based on existing sources and monthly financial data are available. To achieve causal identification, we employ a dynamic regression-discontinuity design, thus focusing on close electoral outcomes. We find that left-wing electoral victories cause significant and substantial short-term decreases in stock market valuations and in the US dollar value of the domestic currency, while the response of sovereign bond markets is muted. Effects at longer time horizons (6 to 12 months) are very dispersed, signaling large heterogeneity in medium-run outcomes. Stock market and exchange rate effects are stronger and more persistent in elections in which the left's proposed economic policy is more radical and in developing economies.

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1 Introduction

The stock market rally which followed the 2016 US Presidential election was interpreted by many as a 'Trump boom' or, less optimistically, a 'Trump bubble' (Gandel, 2017; Krugman, 2017; Schiller, 2018). The (alledged) 'Trump Boom' is far from being the only or the most dramatic example of a substantial financial market movement attributed to a political event. For instance, large stock market crashes followed the close victories of François Mitterrand in France in 1981 (Sachs and Wyplosz, 1986) and, even more dramatically, Salvador Allende in Chile in 1970 (Girardi and Bowles, 2018). Figure 1 illustrates these and some other examples.

Yet, well-identified evidence on the effect of electoral outcomes on financial markets is still scarce and limited to a small number of case studies,[1] reflecting the difficulty of achieving credible causal identification in the presence of simultaneous causality and anticipation effects. Simultaneous causality arises from the strong influence that economic factors exert on political developments (Lewis-Beck and Stegmaier, 2000). Anticipation results from the fact that political changes are often largely predictable, typically on the basis of surveys of voting intentions and expectations, especially when there is a large margin between the competing parties or candidates.

This paper estimates the 'local average treatment effect' of left-wing, as opposed to conservative, electoral victories on share prices, exchange rates and government bond yields in a large sample of elections. We combine a new dataset on national (parliamentary and presidential) elections in the post-WWII period with historical daily and monthly financial data. Our sample includes 929 elections in which the margin of victory/loss of the left can be computed and data is available for at least one of our financial variables of interest.

To identify causal effects, we employ a regression-discontinuity design (Hahn et al., 2001; Imbens and Lemieux, 2008). Intuitively, we compare elections closely won and closely lost by the left. The running variable in our RD design is the margin of vic-

[1] See Girardi and Bowles (2018) on Chile's 1970 presidential election (and subsequent coup); Herron (2000) on the 1992 UK parliamentary election; Knight (2006) on the 2000 US presidential election; Snowberg et al. (2007) on the 2004 US Presidential election; Wagner et al. (2017) on the November 2016 'Trump shock'. See Section 2 for a discussion.

tory/loss of the left. In presidential elections, this is the margin of the left's candidate. In parliamentary elections, as we will explain, it is twice the difference between the share of parliamentary seats won by (center-)left parties and 50%. We test whether the expected values of our financial outcomes of interest display a discontinuity at the cutoff which determines electoral victory. Identification is thus based on a 'smoothness' assumption, meaning that unobserved confounding factors (including ex-ante probabilities) do not display a discontinuity at the threshold. Under this assumption, our RD approach addresses both endogeneity and anticipation effects (more on this in Appendix A).

We implement our RD design through a dynamic specification, to uncover the dynamics of the impacts around our events of interest. While in presidential elections we assume 'perfect compliance', in parliamentary elections our running variable imperfectly (but significantly) predicts a left-wing electoral victory – as measured by the probability that a left-leaning government is formed after the election –giving rise to a fuzzy RD design.

Figure 2 illustrates the identification challenges associated with estimating the effect of electoral outcomes, and our approach to address them. It plots simple averages of share prices around left-wing electoral victories, relative to electoral losses, in all elections (left panel) and in close elections (right panel), with the latter defined as elections in which the margin of victory/loss of the left is not greater than 10%.[2] A 'naive' approach that treats all electoral outcomes as exogenous and unanticipated would lead to the conclusion that financial markets react very little to electoral outcomes. To the contrary, *prima facie* evidence from *close* electoral outcomes, which are likely to constitute news and be independent of macroeconomic conditions, points to a substantial stock market reaction.

Using our dynamic regression-discontinuity specification, we confirm that left-wing electoral victories cause substantial short-term decreases in stock market valuations and the US dollar value of the domestic currency, while the response of sovereign bond mar-

[2]Here, consistent with our RD design, we consider a parliamentary election as won by the left if (center-)left parties win at least 50% of parliamentary seats. A presidential election is won by the left if the (center-)left candidate is elected president. The left margin is defined as explained above and in Section 4.

kets is muted (baseline results are summarized in Figure 5). On average, a close left victory causes real share prices to decrease by 13 to 15 percentage points in the short run. The fall is concentrated in the first trading day after the election, in which share prices tend to fall by 5 to 7 percentage points. The short-run negative effect on the US dollar value of the domestic currency appears more gradual, and amounts to around 10 percentage points in one after-election trimester.

Effects at longer time-horizons (6 to 12 months) are remarkably dispersed, signaling large variability in medium-run outcomes across different experiences. With this important caveat in mind, on average across all elections we observe (at least partial) reversal of the negative stock-market effect, which may suggest 'overreaction' to electoral shocks, but not of the exchange rate effect; however the stock-market effect appears persistent (but still very imprecisely estimated) in elections in which the left's economic platform is more radical and in developing economies.

Indeed, analyzing heterogeneity, we find that stock market and currency effects are stronger and more persistent in elections in which the left's proposed economic policy is more interventionist and in developing countries. Exchange rate effects appear heterogeneous also along a temporal dimension: they are much stronger in the post-1990 period. We find little reaction of government bond yields and spreads, overall and in these subsamples.

Our results are confirmed by various robustness and falsification tests. We employ alternative criteria for selecting the bandwidth size in our RD specification and alternative measures of share prices. We perform falsification tests using placebo thresholds and placebo election dates. We also test whether our results are entirely driven by the few most influential observations, and find that this is not the case.

This paper is the first to provide causally identified evidence on the reaction of financial markets to partisan political shocks from a large sample of national elections. Going beyond single case studies of US elections, on which existing works have mostly focused (e.g. Snowberg et al., 2007; Knight, 2006; Wagner et al., 2017), we contribute more general evidence to the literature on the effect of electoral outcomes. Our research design can be seen as a generalization of case studies which have exploited close elections to study financial market effects, like Girardi and Bowles (2018) on the 'Allende shocks' and Wagner et al. (2017) on the 'Trump shock'.

The evidence we provide is informative on several theoretical issues in macroeconomics and political economy. Our results are inconsistent with the 'policy convergence theorem' (Downs, 1957; Hotelling, 1929), according to which different political coalitions would converge, under competitive pressure, to the same position dictated by the preferences of the median voter.[3] To the contrary, our results are consistent with models in which different parties pursue different macroeconomic policy goals (Alesina, 1987; Hibbs, 1986). More generally, our analysis sheds some light on the macroeconomic effects of political factors.

Perhaps most importantly, our results speak to the relation between capitalism and democracy. The reaction of capital holders to political shocks is seen by several scholars as a major constraint limiting the range of policy options that are feasible in a capitalist economy (Bowles and Gintis, 1986, pp. 88–89; Przeworski and Wallerstein, 1988; Campello, 2015). Although this paper is silent on whether policy platforms are influenced by the expected reaction of financial markets, we do provide empirical backing for the idea that capital holders react substantially to political variation.

The paper is structured as follows. After discussing the previous literature and how we contribute to it (Section 2), we present our dataset (Section 3) and our research design (Section 4). Section 5 presents main results, while in 6 we perform a number of robustness and falsification tests. A discussion of results (Section 7) follows, before conclusions (Section 8).

2 Previous literature on political partisanship and financial markets

Our paper contributes to a recent literature on the effect of electoral outcomes on financial markets. Despite growing interest in the effect of political-institutional factors on economic outcomes, causally identified evidence on this topic is still relatively scarce

[3] A recent influential work that provides evidence of policy differentiation is Lee et al. (2004).

and limited to few case studies.[4]

Some studies have provided interesting aggregate evidence from US and OECD elections, but without an explicit identification strategy to deal with anticipation effects and endogeneity of electoral outcomes, which are therefore likely to affect results. Specifically, Santa-Clara and Valkanov (2003) find that in the US, overall, Democratic presidencies are associated with higher returns, but daily post-election returns are not correlated with election outcomes. Sattler (2013), using a simple event-study approach, shows that in a sample of post-1950 elections in OECD countries, stock returns tend to decline by 1.7 percentage points after a left victory.

Two recent articles have used close and unexpected electoral outcomes as case studies. Girardi and Bowles (2018) focus on the victory of socialist candidate Allende in the 1970 Presidential election in Chile, an episode characterized by remarkably large policy divergence between the competing candidates. Using both daily aggregate data and a new firm-level dataset, they show that Allende's election caused average share prices to fall by as much as one half, with little firm- and sector-level heterogeneity. Wagner et al. (2017) estimate the effect of Trump's victory in the 2016 US presidential election on the cross-section of stock returns. They find that high-tax and domestically focused firms gained value relative to other firms, and that more easily assessed consequences were priced faster than more complex ones.

Other case studies have dealt with anticipation effects by looking at changes in the perceived probability of victory of parties/candidates during the election campaign. For example Herron (2000) studies the 1992 UK parliamentary election, finding a negative correlation between the odds of a Labor victory and average share prices, and inferring that a Labor victory would have reduced stock valuations by 5 to 10 percent. Knight (2006) uncovers a correlation between different types of stocks and the probability of a Bush (as opposed to Gore) victory during the 2000 US presidential campaign. The crucial identification assumption (and main potential limitation) of these studies is that changes in perceived probabilities are assumed to be exogenous to economic conditions.

[4]We are referring here to works that assess partisanship effects. A larger literature has studied the effect of political connections on firms' share prices (e.g. Ferguson and Voth, 2008; Fisman, 2001; Jayachandran, 2006). Dube et al. (2011) estimate the effect of top-secret CIA coup authorizations on the share prices of exposed US firms.

This identification assumption can fail under retrospective economic voting: investors would react to changes in economic conditions by updating their vote expectations, making perceived probabilities endogenous (Snowberg et al., 2007, pp. 824–825).[5]

The study of the 2004 US Presidential election by Snowberg et al. (ibid.) belongs to this latter strand, as it focuses on changes in the perceived probability of a Republican (vs. a Democratic) victory. However, it sidesteps the limitations of previous studies by using higher-frequency financial and prediction markets data, and exploiting exogenous changes in expectations due to the release of flawed exit pool data. They find that investors associated a G.W. Bush presidency with higher stock market valuations and interest rates, as well as a higher price of oil and a stronger dollar. In a less precisely identified but more general exercise, they use prediction markets to obtain a measure of the 'surprise' associated with election results (dummy for Republican victory minus ex-ante probability of Republican victory) in all US Presidential elections from 1880 to 2004. They find a positive correlation between this indicator and post-election daily returns on the $S\&P100$ index, indicating that a Republican victory tends to raise stock market valuations by 3-4 percent.

While a recent literature has used regression-discontinuity to identify the effect of electoral outcomes on various policy variables at the local (municipal and regional) level (Beland, 2015; Ferreira and Gyourko, 2009; Pettersson-Lidbom, 2008), this paper is, to the best of our knowledge, the first to employ a RD design to study financial market effects at the national level.

3 Data

We combine a new dataset on national (parliamentary and presidential) elections in the 1945-2018 period with historical daily and monthly data on stock prices, exchange rates and sovereign bond yields. The resulting sample includes 929 elections in which available information on partisanship allows to build our running variable (the left's margin of

[5] The article by Knight (2006) is arguably less likely to suffer from simultaneity bias, given its focus on cross-sectional variation in returns (some firms and sectors outperforming others), not aggregate effects. However, as noted by Snowberg et al. (2007, p. 809), also in that setting the assumption that changes in the probability of victory of a candidate are exogenous to economic factors may be questionable, due to potential unobservable factors affecting both election prospects and firms' share prices.

victory/loss) and data is available for at least one of our financial variables of interest. This section provides a succinct description of our dataset and sources, while Appendix C provides additional details.

3.1 Election results and partisanship

We build a dataset of worldwide national general (parliamentary and presidential) elections in the post-WWII period. We collect information on election results and the ideological stance of parties and candidates from a variety of sources.

Parliamentary elections For parliamentary elections, our main source is the Manifesto Project Database (Volkens et al., 2018; MPD thereafter), which covers 719 parliamentary elections in 56 countries in the 1945-2017 period. The MPD provides data on the parliamentary seats won by all parties, their ideological classification and quantitative measures of their policy positions on several issues.

We use MPD data to calculate the share of parliamentary seats won by left and center-left parties, which we use to build the running variable for parliamentary elections in our RD design (Sec. 4.1). We include in the (center-)left block all parties classified by MPD as either 'Socialist', 'Social-Democratic' or 'Ecologist'.[6] We also take the MPD policy positions estimates, which will be used to distinguish between 'market-oriented' and 'interventionist' parties in our analysis of heterogeneous effects (Sec. 5.3).

We calculate the left's share of parliamentary seats also from the election and ideology information in Armingeon et al. (2018) and Swank (2013). Reassuringly, the resulting series are strongly correlated with the series obtained from the MPD – in most elections virtually identical. We thus complement the information in the MPD with these two datasets, using them to build the left share of seats in elections not covered by MPD.[7]

We compute the share of left-wing cabinet members in the first government formed

[6]This classification is found in the `parfam` variable in the MPD. Communist parties are included in the 'Socialist' label.

[7]The left share of seats built from the MPD has a correlation coefficient of 0.87 with the left share calculated from Armingeon et al. (2018), and of 0.98 with the one calculated from Swank (2013). See Figure C.1 for a visual comparison. The elections not covered by MPD for which we are able to use the Armingeon et al. (2018) data are 35; the ones for which we use Swank (2013) are 3. Excluding these elections, and leaving only the ones with MPD data, does not affect results in any meaningful way.

after each election from the data in Seki and Williams (2014), Armingeon et al. (2018) and Swank (2013).

The dummy variable for whether the after-election government is left-leaning, to be used in our first-stage regressions, is built from the cabinet members data, defining a government as left-leaning if the share of left-wing cabinet members is at least two-thirds.[8] When the cabinet members data is not available, we follow the ideological coding of Cruz et al. (2016), which uses a cruder measure based on the party affiliation of the chief executive.

Presidential elections Data on presidential elections is less readily available; we have assembled an original dataset which draws from several sources. Election results (names of candidates, party affiliation and share of votes received) were collected from publicly available national and international sources, for the universe of worldwide presidential elections in the Jan1945-Sep2018 period.

We calculate the left margin as the difference between the popular vote share of the first (center-)left candidate and the share of the first non-left candidate.[9] When elections are decided in a run-off, we consider only the run-off, not the first round.

To code presidential candidates as (center-)left or conservative, we employ various existing sources. For the (few) presidential elections covered in the MPD, we employ the MPD classification, following the same criterion that we applied in parliamentary systems (described above). For the 146 (Latin American) elections not covered by MPD but covered by Baker and Greene (2011) or Coppedge (1997), we follow their ideological coding.[10] In the remaining elections, we look at whether a candidate's party belongs to some international association, and assign her the ideology of the association.[11] When

[8]This is a conventional criterion in the literature, sometimes referred to as 'Schmidt-Index', from Schmidt (1992).

[9]The sets of left and non-left candidates are collectively exhaustive in our coding, so either the first left or the first non-left candidate is the president-elect.

[10]In Mexican elections, the MPD-based classification used for parliamentary elections and the Baker and Greene (2011) classification used for presidential ones are inconsistent: the same parties are classified differently based on the two sources. To avoid introducing an inconsistency in the analysis, and considering that the Baker and Greene (ibid.) classification is more fine-grained, we exclude Mexican parliamentary elections. No result is significantly affected by this choice.

[11]Left for Socialist International, Foro de Sao Paulo, Party of European Socialists and Progressive Alliance. Conservative for Liberal International, Centrist/Christian Democrat International, European People's Party, International Democrat Union and Alliance of Conservatives and Reformists in Europe.

this does not apply, we resort to published books or articles which explicitly classify candidates or their parties as (center-)left or conservative. Our elections dataset, available in the replication files, reports the source of the classification for each of the three most-voted candidates in each presidential election.

Overall sample of elections We exclude from the analysis presidential elections in which the president is elected by parliament or an electoral college rather than by popular vote (eg, in Italy or USA), as our running variable would not provide a discontinuity in these cases; presidential elections in purely parliamentary systems, in which the president does not hold substantial executive power (eg, in Austria); parliamentary elections held in the same month of a presidential election under a presidential system (for example in Chile). The classification of the political system applying to each election (parliamentary, semi-presidential, presidential) is taken from Armingeon et al. (2018), Przeworski (2013), Cruz et al. (2016), Bormann and Golder (2013) and Lindberg (2006) (in this order).

The resulting dataset includes 1,445 elections from 135 countries; of these 713 are parliamentary and 732 are presidential elections. For 1,066 of these elections (372 presidential and 694 parliamentary), we are able to compute our running variable, the left's margin, following the procedure and sources described above. For 929 of these elections (650 parliamentary and 279 presidential), data on at least one of our financial outcomes of interest is available. Descriptive statistics for these elections, which are the ones employed in estimation, are presented in Table 1(a). The list of countries in the sample and the number of (parliamentary and presidential) elections that we could use in estimation for each country is provided in Appendix B.

3.2 Share prices, exchange rates, sovereign bond yields

We build a dataset of historical monthly data on stock market prices, exchange rates and sovereign bond yields. For stock price indexes, we are also able to build a daily dataset covering a smaller but still substantial number of elections in our sample, in addition to the monthly one. Our main sources are Global Financial Data (GFD thereafter) for stock prices and bond yields, and Reinhart (2016) for exchange rates. All observations in the monthly dataset are monthly averages.

As a measure of average share prices, we take the broadest available stock market index for each country, resorting to other national and international sources for countries/periods not covered by GFD. Appendix B indicates the stock market index considered for each country. We deflate monthly stock market indexes with the Consumer Price Index.[12] End-of-month share price data are available for a (large) subset of observations, and we will use them in lieu of monthly averages in a robustness test.

The US dollar value of the domestic currency (our measure of exchange rates) is taken from the monthly dataset of Reinhart (2016), which includes both official and parallel (black-market) exchange rates. For observations that are missing in Reinhart (ibid.), but available in the Bank of International Settlement exchange rates database, we use the latter.[13]

We use parallel (instead of official) exchange rates for country-years under an inflexible exchange rate regime. To identify exchange rate regimes we use the classification provided by Ilzetzki et al. (2017) and Klein and Shambaugh (2010).[14]

Data on 10-years government bond yields comes from the GFD database. We use both deflated and nominal yields, and we calculate (real and nominal) spreads relative to US government bonds.

Table 1(b) provides descriptive statistics for our financial outcomes of interest.

4 Regression discontinuity design

To identify the average causal effect of left-wing (as opposed to conservative) electoral victories in our sample, we employ a regression-discontinuity design (Hahn et al., 2001; Imbens and Lemieux, 2008). We implement our RD design through a dynamic specification, to uncover the dynamics of the effects around our events of interest.

[12] GFD provides deflated monthly stock market indexes using CPI data. For cases in which we resort to other sources, we use CPI data from OECD statistics

[13] BIS exchange data were downloaded from `https://www.bis.org/statistics/xrusd.htm` in October 2018. Reinhart (2016) and BIS data provide identical series for all the country-years that are available in both sources.

[14] We consider an exchange rate system as inflexible if either Ilzetzki et al. (2017) or Klein and Shambaugh (2010) (or both) classify it as such. In using Ilzetzki et al. (2017), we consider a peg or a crawling band narrower than ±2% as inflexible (coded as 1 and 2 in their classification).

Our regression-discontinuity approach achieves causal identification by focusing on close elections. We exploit the threshold that determines victory in presidential elections and control of Parliament in legislative elections. Essentially, we test whether the expected value of our outcomes of interest displays a significant 'jump' at this cutoff.

Given our RD strategy, our main identifying assumption is 'smoothness': unobserved confounding factors do not display a discontinuity at the threshold. Under this assumption, our RD estimator is able to isolate causal effects and avoid selection bias. In contrast with traditional event-studies, our dynamic RD estimates are not biased by anticipation effects, as long as ex-ante probabilities, like other confounding factors, do not jump at the threshold (see Appendix A for a more detailed exposition of this point).

Reassuringly, the conditions under which our approach would fail – in the sense of failing to find an effect where there is one, of overestimating the local effect size – appear rather extreme. If investors were able to forecast with certainty any arbitrarily close electoral outcome, we would always obtain a null coefficient, independently of the true effect. This, however, seems unlikely. If the ex-ante probability of a left victory was systematically and substantially *lower* before close left electoral victories relative to close left losses – a possibility that would appear safe to rule out, at least on average – our estimates would have the correct sign but overestimate the magnitude of the effect. If instead there was some discontinuity at the cutoff in ex-ante probabilities, with the ex-ante probability of a left victory being higher before close left victories (as may be possible, at least in principle), our estimates would have the correct sign but *underestimate* the magnitude of the partisan effects. (More on this in Appendix A).

4.1 Forcing variable in presidential and parliamentary elections and fuzzy RD design

Our forcing variable – the variable that determines assignment to treatment in our RD design – is the margin of victory/loss of the (center-)left. In presidential elections, this is straight-forwardly defined as the margin of victory/loss of the left-wing candidate. In parliamentary elections, it is calculated as twice the difference between the share of parliamentary seats won by left and center-left parties and 50%.

While for presidential elections the determination of the forcing variable is rather straightforward, for legislative elections it is not: often it is not easy to determine who wins an election in a parliamentary system. Our choice of the forcing variable for parliamentary elections implies defining a left-wing victory as an election in which parties classified by Volkens et al. (2018) as 'Socialist', 'Social Democratic'or 'Ecologist' hold, together, a majority of parliamentary seats. The distance between the left share of seats and 50% is multiplied by two in order to obtain the margin with respect to non-left parties, thus making the measure comparable with the one used in presidential elections.[15]

Clearly, this running variable can only imperfectly predict (center-)left victories in parliamentary elections. In some elections, for instance, left-wing and center-left parties may not be allied nor willing to form a coalition; in others, they may be part of a stable alliance with some christian-democratic or conservative party. Both these cases would be characterized by little discontinuity in political power at the threshold.

We account for 'imperfect compliance' in parliamentary elections by employing a fuzzy RD design (FRD). We assume that the probability of a left victory in parliamentary elections jumps discontinuously at the cutoff, but by less than one. The overall LATE can then be recovered as the ratio of the jump in the outcome variable to the jump in the probability of treatment at the threshold (Imbens and Lemieux, 2008, p. 619).

Estimating the first-stage relation between the running variable and the probability of treatment (the denominator in the FRD estimator) requires an indicator for whether the left effectively wins a parliamentary election. We use a dummy equal to one if a left-leaning government is formed after the election (built as described in Section 3 and Appendix C).

In presidential elections, instead, we assume 'perfect compliance': we assume that the election of a left-wing president always leads to a left-leaning government. The exclusion of presidential elections in parliamentary systems, in which the president is not the head of the executive, makes the case for this assumption rather compelling.

[15]Formally, this is $x = 2(share_L - 50)$, where $share_L$ is the share of parliamentary seats of left and center-left parties. When this measures crosses the zero cutoff, left parties hold control of parliament and can potentially form a government composed only of (center-)left parties. To see why the distance from 50% must be multiplied by two, consider the simple case in which only two parties are represented in Parliament, a left-wing one and a conservative one. The difference between the shares of the left and conservative parties would be equal to twice the distance between the left share and 50%.

Panel (a) of Figure 3 displays the first-stage relation in parliamentary elections. It shows that the probability that a left-leaning cabinet is formed after the election displays a sizable discontinuity when the share of parliamentary seats won by (center-)left parties crosses the 50% cutoff, using all parliamentary elections for which both the running variable and the left government indicator are available. The size of the discontinuity, estimated through kernel-weighted local linear regression using the robust bias-corrected estimator of Calonico et al. (2014) and clustering standard errors by country, is 30.6 percentage points, with a p-value of 0.019. Panel (b) displays the jump in the share of left-wing cabinet members of the first after-election government (available for a subset of elections), which is also relevant and significant (48.8 p.p., with a p-value of 0.008).

We also test for a discontinuity in the distribution of the forcing variable at the cutoff. Such a discontinuity, if significant, may signal the possibility of systematic manipulation of electoral results, which may undermine the RD identifying assumption. We perform both McCrary (2008) and Cattaneo et al. (2017) tests. We find no evidence of manipulation in parliamentary nor in presidential elections (results reported in Appendix D.1).

4.2 Estimation method: dynamic FRD specification

Consider a country i that has an election e at time t. We estimate the country's financial market reaction over a h-periods horizon through the following dynamic FRD specification:

$$\begin{aligned} D_{i,e} &= \beta Z_{i,e} + g(x_{i,e}) + \eta_{i,e} \\ \Delta y_{i,e,t+h} &= \gamma_h Z_{i,e} + f^h(x_{i,e}) + \epsilon_{i,e,t+h} \\ &\text{for } h = -m, ..., 0, ..., n \end{aligned} \tag{1}$$

The first equation in 1 is the first-stage relation between the left's margin in the election and the probability of a left-leaning government; the second is the reduced-form relation between financial market dynamics and the left's margin. In particular, D is an indicator for whether a left government is formed after the election; x is the forcing variable: the margin of victory/loss of the left, as defined in Section 4.1; Z is an indicator equal to 1 if $x \geq 0$ and 0 otherwise; $\Delta y_{i,e,t+h}$ is the logarithmic change in the outcome of interest between time $t-1$ and $t+h$;[16] $f()$ and $g()$ are potentially non-linear functions, that we approximate through kernel-weighted local linear regression;[17]

For each time-horizon h considered, our parameter of interest is $\frac{\gamma_h}{\beta}$, the local average treatment effect of a left-wing electoral victory.

We employ two main specifications: one that uses *raw returns* as the outcome variable in equation 1, and one that uses *abnormal returns*. The specification using raw returns simply estimates equation 1, with y representing the raw data for the outcome of interest. For calculating abnormal returns, we first regress $\Delta y_{i,e,t+h}$ on time fixed-effects (at the month-year level when using monthly data, at the day-month-year level when using daily data) using the whole panel of financial data, and then use residuals from this regression as the outcome variable in equation 1. This specification controlling for time fixed-effects can be interpreted as using abnormal returns, given that the time effects absorb all common time-varying factors. The reason why we control for time effects in two steps is that there are very few national elections that happen in different countries in the same month (let alone in the same day). It would thus be not only inefficient, but impossible, to estimate time effects jointly with other parameters in equation 1, which uses only observations with elections.

5 Results

We use the dynamic FRD design described by eq.1 to estimate our effects of interest in a time-window around elections.

5.1 Visual evidence

As a first step, we set $h = 1$ in equation 1 and plot observations and flexible regression lines around the threshold, to evaluate visually the presence of a discontinuity in the reduced-form relation. Setting $h = 1$ means that we are looking at the 2-months average

[16] When the outcome variable is already expressed in percentage points (as in the case of bond yields), we take the percentage change, rather than the logarithmic change.

[17] We employ a triangular kernel. Results are robust to using a rectangular kernel.

return between the month before and the month after the election. This is shown in Figures 4, using monthly data on raw returns and including all (parliamentary and presidential) elections. Figures D.2 do the same on abnormal returns. The depicted flexible regression lines are estimated using kernel-weighted local linear regression, with bandwidth selected according to the MSE-criterion.[18]

This exercise reveals a sizable negative discontinuity in post-election stock market growth, and a smaller (but still substantial) one in the post-election change in the value of the domestic currency. There is little evidence of any relevant discontinuity in government bond yields and spreads.

5.2 Dynamic estimations

To appreciate size, significance and dynamics of the effects, we estimate a set of FRD regressions following equation 1, letting h (the time-window) vary from -4 to +12 months. We use monthly data, but in the case of share prices we are also able to look at higher frequency (daily) data. All specifications use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidth, and robust standard errors clustered by country.[19]

Figures 5 plots dynamic FRD estimates and 95% confidence intervals using monthly data and raw returns in the whole sample (pooling presidential and parliamentary elections); figures D.1 use abnormal returns. Tables 2 and 3 report results (with h equal to 1, 2, 6 and 12 months) for all elections, as well as for parliamentary and presidential elections taken separately. For each sample, the tables report estimates using both raw returns and abnormal returns (that is, controlling for common time effects).

We find a sizable and statistically significant negative short-term effect on stock market valuations and the US dollar value of the domestic currency. On average, share prices decrease by 13 to 15 percentage points between the month before the election and the month after. After taking into account 'imperfect compliance' through the FRD estimator, the negative stock market effect appears stronger in parliamentary elections (17

[18] As in all baseline estimations presented here, we calculate the MSE-optimal bandwidth using the procedure in Calonico et al. (2014).

[19] We implement the Calonico et al. (ibid.) robust bias-corrected estimator using the `rdrobust` package in Stata (Calonico et al., 2017).

to 19 p.p.). The exchange rate effect is more gradual. At a 3-months horizon, the effect is around -10 p.p. in all elections. The exchange rate effect appears much stronger and more persistent in presidential elections. We do not find significant pre-trends in any specification using monthly data, which is consistent with our identification assumption that unobserved confounders, including ex-ante probabilities, do not jump at the cutoff.

In contrast with short-run effects, longer-run (6 to 12 months) estimates are very dispersed, signaling wide variation in medium-run outcomes across different experiences. Our 95% confidence interval for stock market effects in all elections at a 1-year horizon ($h = 12$ in equation 1) cannot rule out large positive or negative effects, and this large variability applies to both presidential and parliamentary elections. On average, we observe at least partial reversal of the negative stock market effect in the whole sample, both in raw returns and in abnormal returns. However, of course, the very large confidence intervals discourage from drawing any conclusion from longer-run effects. Moreover, we will see that the average 1-year effect remains negative (but still very imprecisely estimated) in some subsamples.

Medium-run exchange rate effects across all elections display very large variability too. The exception to this pattern is the subsample of presidential elections, in which 1-year exchange rate effects are statistically significant and large (around -50 p.p.), although the relatively small number of observations available when analyzing presidential elections alone suggests some caution also in interpreting this result.

Consistent with the visual evidence of Figures 4, we find little evidence of an effect on Government bond yields and spreads. Panels (c) and (d) of Figure 5 show that the short-run reaction of bond markets is flat and near zero. We do find some positive coefficients (indicating a rise in bond yields, therefore a decrease in their price) at longer time-horizons in presidential elections, but only marginally significant.

We are able to estimate stock market effects also at a daily frequency for a smaller (but still relatively large) number of elections. Daily-frequency effects are reported in the bottom panel of Table 2 and in Figures 6. Consistently with a causal interpretation of our results, the bulk of the stock market effect occurs in the first trading day after the election, when share prices fall on average by 5 to 6 percentage points. At a daily frequency, we do find some small but significant pre-trends in the days immediately before the election when using raw returns (panel a). This may suggest some discontinuity in ex-ante probabilities at the cutoff and therefore underestimation of the effects of interest (see Appendix A). However, these small pre-trends in daily series disappear when controlling for time effects (panel b)

5.3 Heterogeneous effects

Naturally, the treatment effect of (center-)left electoral victories is likely to be heterogeneous, depending on variation in policy platforms, political systems, industrial relations, and socio-economic conditions in general. In what follows, we look at heterogeneity from three perspectives: ideological, temporal (pre- and post-1990) and geographical (high-income vs. developing countries).

Heterogeneity in policy platforms First, we test whether the effect is stronger when the (center-)left's electoral economic platform is more radical. We use the MPD policy position estimates, in particular variables `planeco` and `markeco`. The first measures support for market regulation, economic planning and government control of the economy; the second measures support for a 'free market economy' and a smaller role of the state (Volkens et al., 2018). We compute the difference between the two indicators for the major left party and use it a proxy for the left's economic ideology. We divide elections in two subsamples based on whether the left's economic interventionism is below of above its median value in the sample. We refer to the first group as elections characterized by a 'market-oriented' left , and to the second as 'interventionist left' elections.

In this test we can include only parliamentary elections, given that MPD policy position estimates are not available for presidential ones. This has the advantage of controlling for heterogeneity based on political systems; however, it does reduce the sample size quite significantly. Given that comparability between parliamentary and presidential elections is not an issue in this test, and that our focus here is on the relative difference between the two subsamples rather than the overall effect size, we focus on the reduced-form relation (the second line of eq. 1) in order to increase statistical power.

Table 4 displays results from this exercise. As expected, the negative stock-market and exchange-rate effects of left-wing electoral victories are stronger and more persistent in elections in which the left's proposed economic policy is more radical. The short-run reduced-form coefficient is smaller than 4 percentage points and not significant when the left is 'market-oriented', but around 5 to 8 p.p. and statistically significant in the 'interventionist left' subsample. This qualitative result is confirmed in the subsample with daily frequency.

The 'interventionist left' subsample displays stronger exchange-rate effects too. In this case, the difference in the short-run reduced-form coefficient is smaller–it is around 1 p.p. larger when the left is more radical. The difference in medium-run effects appears much more marked, although 6 and 12-months coefficients are again imprecisely estimated.

Effects on bond yields are not statistically significant in any of the two subsamples, and the point estimates are generally not larger when the left is more radical, which is consistent with government bond yields displaying on average little reaction, at least in parliamentary elections.

Cross-country heterogeneity Second, we test for differential effects in high-income and developing countries. We use the World Bank classification for identifying high-income economies. Results are reported in Table 5. Both stock market and exchange rate effects are much stronger in non-OECD countries. In particular, the exchange rate effect seems to be driven almost only by developing economies. Effects on bond yields are not significant in either group.

Time-varying effects Third, we test whether the effects were stronger in earlier elections or in more recent (post-1990) ones. We choose 1990 as the breakpoint both because of the global political discontinuity represented by the fall of the Soviet Union, and because it allows to retain a reasonably large number of observations in both (pre- and post-) subsamples. Results are reported in Table 6. Daily and monthly stock market effects are somehow (but not dramatically) stronger in the pre-1990 period, but less precisely estimated, possible due to the smaller number of observations. Exchange rate effects, to the contrary, are clearly larger and more precisely estimated post-1990, which may reflect structural changes in international currency markets. The reaction of government bond yields is again not statistically significant in either subsample.

6 Robustness and falsification tests

We perform various robustness and falsification tests. We try alternative bandwidth selection criteria (Sec. 6.1) and alternative measures of share prices (6.2); we perform falsification tests using placebo thresholds (6.3) and placebo election dates (6.4); we try excluding the few most influential observations (Sec. 6.5); finally, we restrict our sample to country-years with non-missing values for all our financial outcomes of interest (6.6).

6.1 Alternative bandwidth selection criteria

We re-estimate our baseline regression-discontinuity specification (eq. 1) using alternative bandwidth selection criteria. Results are reported in Table 7. The first column reports, for the sake of comparison, our baseline results using a MSE-optimal bandwidth selected according to the procedure in Calonico et al. (2014). The second column also uses a MSE-optimal bandwidth, but selects two different bandwidth sizes below and above the threshold. The third column uses the MSE-optimal bandwidth, but employing the procedure in Imbens and Kalyanaraman (2012). The fourth and fifth columns use a CER (coverage error rate)-optimal bandwidth, respectively with a common size and with different sizes on the two sides of the threshold. For all our outcomes of interest, we find results to be largely insensitive to the specific bandwidth selection criterion employed.

6.2 Alternative measures of share prices

In Figures D.3, we estimate stock market effects at a monthly frequency using nominal instead of real valuations, and/or end-of-month values instead of monthly averages. Results are qualitatively unchanged. This demonstrates that effects on real share prices are driven by nominal share valuations, not inflation effects (as shown also by the effect on nominal daily valuations). Moreover, when using end-of-month prices the contemporaneous effect ($h = 0$ in eq. 1) becomes substantially larger. Given that the average share price in the month of the election is almost always contaminated by pre-election observations (which is why we focus on the 1-month time-horizon in our baseline estimations of short-run effects), while the end-of-month observation is not, this fact is consistent with a causal interpretation of our results.

6.3 Placebo thresholds

Our first falsification test investigates the presence of significant discontinuities in our outcomes of interest further away from the true threshold that assigns electoral victory. A tendency to find significant discontinuities in correspondence of placebo thresholds would cast doubts on the 'smoothness' assumption which underlies our RD design.

To do this, we randomly draw 200 placebo thresholds, plot the resulting distribution of t-statistics from the estimation of equation 1 with $h = 1$, and then compare it with the t-statistics obtained at the true threshold. The placebo thresholds are drawn separately on the left and on the right side of the true threshold (100 draws on each side) and only observations from that same side are used in estimation, in order to avoid potential mis-specification due to assuming continuity at the true threshold. We use only placebo thresholds that guarantee at least 25 observations in each side within the bandwidth, to avoid biasing our test against significant findings because of weak statistical power.

Results are reported in Figures D.4, which plot the distribution of placebo-threshold t-statistics for each financial outcome of interest, using both raw and abnormal returns. There is very little evidence of a tendency to find significant discontinuities away from the true threshold. The t-statistics from our baseline estimation at the true threshold (vertical dashed lines) are in the tails of the distribution of placebo t-statistics for stock market and exchange rate effects, but not for bond yields. Consistent with baseline results, the distribution of placebo thresholds suggests a level of significance below 5% for short-run stock market and exchange rate effects, but above 70% for the impact on bond yields.

6.4 Placebo election dates

As a second falsification test, we substitute placebo election dates for the true ones. We estimate eq.1, again setting $h = 1$, after randomly shifting all election dates within the sample. We repeat this procedure 500 times, for each of our outcomes of interest.

Figure D.5 plots the distribution of t-statistics for these placebo effects, comparing them with the t-statistics from the true election dates. Reassuringly, we find very little tendency to find significant effects at placebo election dates, and the t-statistics from the true election dates are in the tails of the placebo distribution for stock market and exchange rate effects, but not for bonds. The distribution of t-statistics from placebo election dates suggests a level of significance below 3% for stock market and exchange rate effects, and above 50% for bond yields effects, thus confirming baseline results.

6.5 Sensitivity to influential observations

To check if our results are entirely driven by few instances of large financial market reactions, we calculate DFBeta coefficients for the effect of left-wing electoral victories, and re-estimate our RD specification (eq.1) after excluding observations with the largest DFBetas. Specifically, in each regression we exclude the observations with $|DFBeta| > 2/\sqrt{N}$.[20]

As expected, influential observations (as identified by the DFBeta coefficients) correspond to well-known cases of close elections characterized by large policy divergence. These include, for example, Chile's 1970 presidential election, closely won by Socialist candidate Allende (Girardi and Bowles, 2018); France's 1981 presidential election, closely won by the communist-socialist coalition supporting Mitterrand (Sachs and Wyplosz, 1986); Portugal's 1979 parliamentary election, in which the center-right coalition Aliança Democrática won a slight majority of parliamentary seats, allowing the formation of a conservative government.

Results from this robustness test are reported in Table D.2, and indicate that our results are robust to excluding influential observations. We detect many more influen-

[20] Using $|DFBeta| > 2/\sqrt{N}$ as the cutoff for defining influential observations is recommended by Belsley et al. (2005 [1980]) and is standard in the literature.

tial observations in the estimates of the stock market effects (between 16 and 27 across different specifications) than in those of exchange rate (between 8 and 14) and bond yields (between 7 and 9) effects. After excluding those influential observations, point estimates for stock market and exchange rate effects get smaller, however they remain statistically significant and economically relevant; effects on government bond yields remain not significant, but become closer to zero and more precisely estimated.

6.6 Common sample

Because of data availability, the samples we use for estimating our stock, currency and bond market effects do not perfectly overlap. A possible concern is that the different effects that we find on our outcomes may be driven by the (partly) different samples used. For instance, if also stock market and exchange rate effects were absent when restricting attention to the (smaller) sample for which bond yields data are available, this would cast doubt on our result that bond markets are unaffected.

To investigate this potential concern, we estimate our baseline dynamic FRD specification, restricting the sample to those elections for which all financial outcomes of interest (share prices, exchange rate, bond yields and bond spreads) are simultaneously available. The resulting sample is, unfortunately, rather small (195 elections, including those outside the optimal bandwidth). Unsurprisingly, given the dramatic reduction in the number of observations, most coefficients lose statistical significance. However the sign of the effects remains the same (results reported in Table D.3).

7 Discussion

A negative reaction of share prices and the domestic exchange rate to (center-)left electoral victories may reflect the expectation of polices that are less favorable to capital and more tolerant of inflation, relative to the counter-factual of a conservative victory. The exchange rate effect may also be driven or exacerbated by resulting capital outflows. Of course, we cannot rule out alternative channels like expectations of lower GDP growth or measures that favor potential entrants over currently existing firms (Snowberg et al., 2007, p. 824).

Quantifying the importance of different potential channels is outside the scope of this paper, and represents a promising avenue for further research, possibly using firm-level data. Girardi and Bowles (2018) provide some empirical assessment of potential channels in their study of Allende's election (which is part of our sample) and subsequent coup in 1970s Chile. They show that the stock market reaction to these events is characterized by a large aggregate effect with small firm- and sector-level variation, and that measures of sensitivity to growth prospects and wage dynamics do not predict price changes after the two events. Based on these tests and a reading of the historical evidence, they argue that the effect was not due to changes in growth prospects nor expected wage policy, but a generalized weakening of private property rights. It would be unwarranted, however, to generalize their considerations and empirical results to all or most of the elections studied in this paper: the episodes they study are arguably unique in the large variation they generate in the political status of private property rights (ibid., pp. 25–26). Expected changes in the share of capital through wage and tax policies are likely to be important in most other elections.

Effects at longer time-horizons display huge variability, so it would be unwarranted to emphasize their interpretation. This fact itself might nevertheless suggest that in the medium-run stock and currency market dynamics have been dominated by factors other than political partisanship, at least on average across our sample. Our analysis of heterogeneity, however, suggests that this might not be entirely true when the left is more interventionist and in developing countries, especially with regard to exchange rate effects.

The absence of significant effects on 10-years government bond yields and spreads may imply that interest rates are not impacted, or that the impact has a different sign in different left-wing electoral victories. Heterogeneity could be due, for example, to different degrees of Central Bank independence. For instance, an independent Central Bank may be expected to raise interest rates in reaction to more expansionary economic policy, but with a lower degree of independence, a government which aims to stimulate the economy may pressure the Central Bank into decreasing interest rates. Moreover, different episodes are likely to differ in the extent of monetary sovereignty and in the propensity of Central Banks to actively control interest rates.

8 Conclusions

Using a dynamic regression-discontinuity design, we have uncovered a substantial reaction of stock and currency markets to electoral outcomes in a large panel of national elections in the 1945-2018 period. We find that close (center-)left electoral victories cause real share prices to decrease by 13 to 15 percentage points in the month following the election, and the domestic currency to depreciate by around 10 p.p. over one quarter. We have found little effect on government bonds' (real and nominal) yields and spreads. Effects at longer time-horizons (6 to 12 months) display great variability, making it hard to assess average medium-run effects.

Stock market and exchange rate effects are stronger and more persistent in elections in which the left's proposed economic policy is more radical and in developing countries. Exchange rate effects, furthermore, are stronger in the post-1990 period.

A natural explanation for the negative reaction of stock and currency markets to left-wing electoral victories is the expectation of policies that are relatively less favorable to capital and more tolerant of inflation, but other potential mechanisms are also possible. Gauging the importance of different potential channels is outside the scope of this paper and represents a promising avenue for further research.

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FIGURES

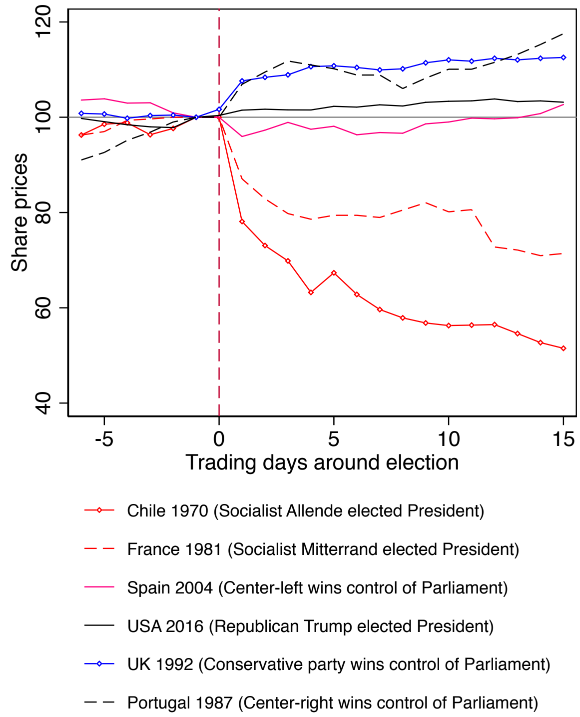
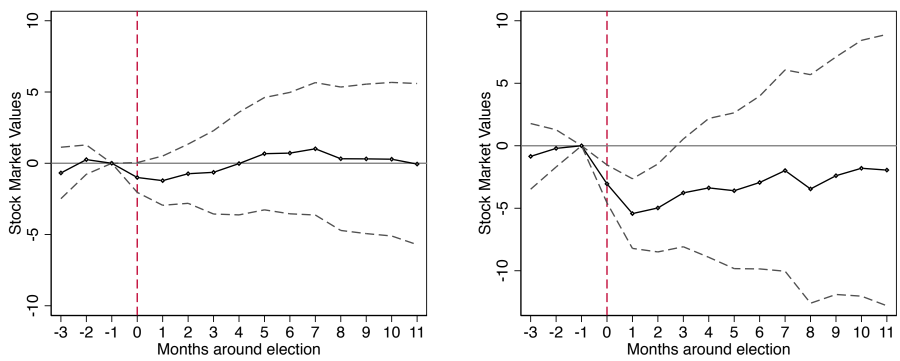


Figure 1: Share prices around selected elections

Note: Nominal share price index normalized to 100 in the day before the election.

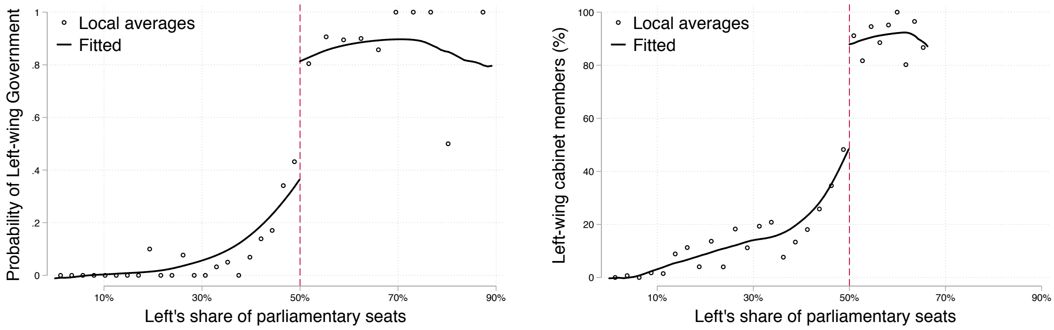


(a) All elections

(b) Close elections

Figure 2: Share prices around left victories (monthly data)

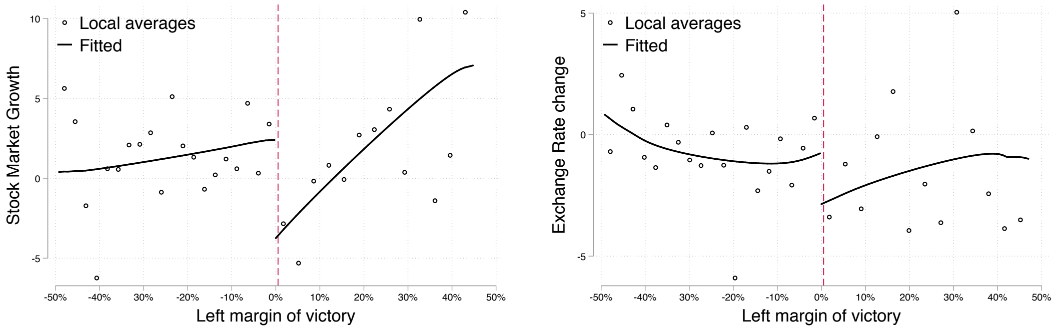
Notes: Average real share prices around (center-)left electoral victories, relative to electoral losses. Normalized to zero in the month preceding the election. Panel (a) includes all available elections. Panel (b) includes only elections in which the margin of victory/loss of the left is not greater than ±10% (close elections). Dashed lines are 95% confidence intervals from robust standard errors clustered by country.



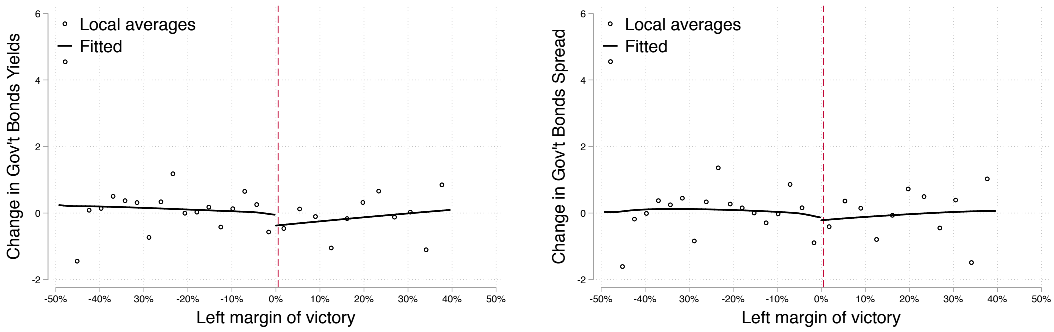
(a) Probability of left-led government (n=637) (b) Left-wing cabinet members (%) (n=520)

Figure 3: First stage: discontinuities at the threshold in parliamentary elections

Notes: Effect of left-wing parties winning a parliamentary majority on (a) the ideology and (b) the share of left-wing cabinet members, of the government formed after the election. The horizontal axis displays the share of parliamentary seats won by parties classified as 'Socialist', 'Social-Democratic' or 'Ecologist' by the Manifesto Project Database. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression, with MSE-optimal bandwidth. We exclude from this graph purely presidential systems.



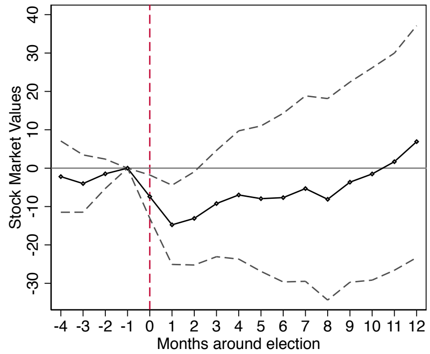
(a) Share Prices (b) Exchange Rates



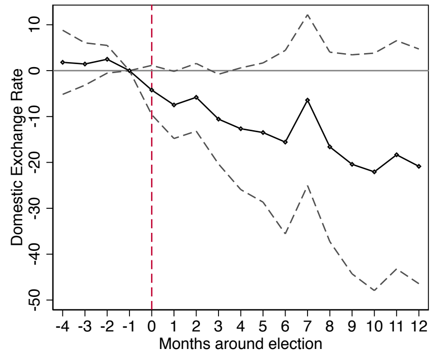
(c) Gov't bonds: real yield (d) Gov't bonds: spread vs. US

Figure 4: Effect of a left-wing electoral victory on financial markets
(Regression-discontinuity estimates; reduced-form relation; monthly data)

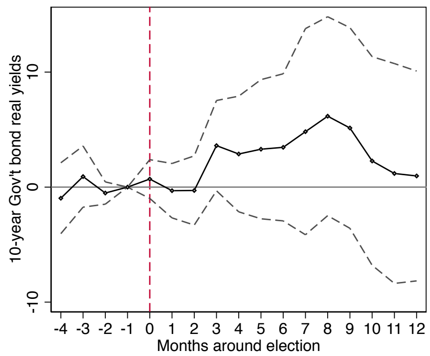
Notes: The vertical axis displays the percentage change in the outcome between time $t-1$ and time $t+1$, where t is the election month. The horizontal axis displays the left's margin of victory (as defined in the main text). Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression, with MSE-optimal bandwidth.



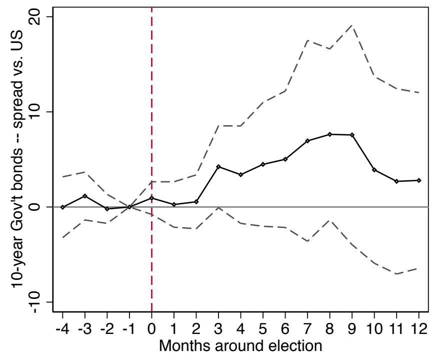
(a) Share Prices



(b) Exchange Rates



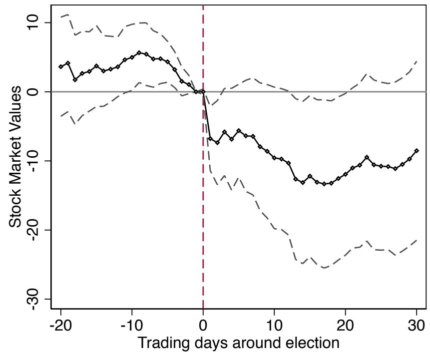
(c) Gov't bonds: real yield



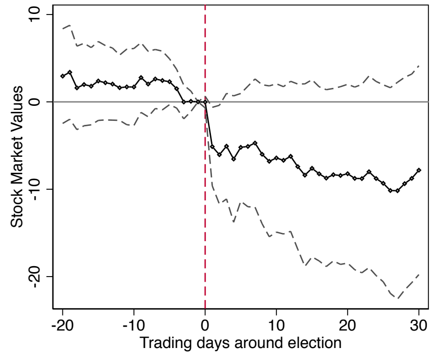
(d) Gov't bonds: spread vs. US

Figure 5: Effect of a left-wing electoral victory on financial markets
(Fuzzy regression-discontinuity estimates; monthly data)

Notes: Effect of a left victory. $t = 0$ *is the month of the election. Fuzzy RD estimates (eq. 1), using the bias-corrected procedure of Calonico et al. (2014). See main text for details. Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Dashed lines are* 95% *confidence intervals from robust bias-corrected standard errors clustered by country.*



(a) Raw returns



(b) Abnormal returns

Figure 6: Effect of a left-wing electoral victory on stock market valuations
(Fuzzy regression-discontinuity estimates; daily data)

Notes: See notes of Figure 5.

TABLES

Table 1: Descriptive Statistics

	N	Mean	S.D.	Min	Max
(a) Elections data					
All elections					
Left margin (%)	929	-15.52	38.57	-100.00	100.00
I[Left Margin> 0]	929	0.30	0.46	0.00	1.00
Political system	929	0.89	0.64	0.00	2.00
Presidential elections					
Left margin (%)	279	6.12	45.17	-96.14	100.00
I[Left Margin> 0]	279	0.52	0.50	0.00	1.00
Political system	279	0.49	0.86	0.00	2.00
Parliamentary elections					
Left margin of victory (%)	650	-24.81	31.07	-100.00	58.33
I[Left Margin> 0]	650	0.21	0.41	0.00	1.00
Left after-election government	647	0.26	0.44	0.00	1.00
Left-wing cabinet members (%)	510	31.92	39.17	0.00	100.00
Policy positions: planeco	589	5.50	4.82	0.00	36.11
Policy positions: markeco	589	2.06	2.66	0.00	23.08
Economic platform (planeco-markeco)	589	3.45	5.76	-23.08	36.11
Political system	650	1.06	0.42	0.00	2.00
(b) Financial data					
Monthly					
Nominal share prices (2010=100)	51,448	97.88	3285.47	1.5e-15	5.9e+05
Real share prices (2010=100)	51,437	2.5e+19	2.3e+21	1.0e-03	2.4e+23
Official exchange rate (USD per unit)	90,667	2.4e+11	5.7e+12	1.4e-11	1.7e+14
Parallel exchange rate (USD per unit)	62,425	2.0e+11	4.1e+12	1.6e-05	1.4e+14
Fixed/pegged exchange rate	90,266	0.69	0.46	0.00	1.00
Real gov't bond yields	33,557	1.60	15.07	-776.58	159.30
Nominal gov't bond yields	34,518	7.69	6.67	-0.56	161.04
Nominal spread (vs. US)	33,582	2.64	6.56	-13.60	159.32
Real spread (vs. US)	32,632	-0.23	14.94	-766.77	159.32
Daily					
Nominal share prices (2010=100)	739,607	164.11	2,721.50	1.1e-12	444,298.44

Notes: The elections data includes only elections in the 1945-2018 period for which the left margin variable could be computed and at least one of our financial outcomes of interest is non-missing. The financial data includes all available country-months for the 1944-2018 period. See main text for the definition of each variable.

Table 2: Effect of a left-wing electoral victory on stock market prices (fuzzy RD estimates; monthly and daily data)

Outcome: log change in share prices between t-1 and t+h						
h (months)	All elections		Presidential		Parliamentary	
+1	-14.76***	-13.13**	-11.49**	-10.13*	-18.90*	-17.78*
	(5.27)	(5.20)	(5.31)	(5.19)	(10.83)	(10.46)
+2	-13.09**	-11.56**	-5.63	-3.72	-15.48	-15.61
	(6.20)	(5.51)	(6.50)	(5.84)	(10.77)	(9.72)
+3	-9.22	-9.22	-3.10	-1.46	-9.58	-11.98
	(7.07)	(6.65)	(8.84)	(8.13)	(11.66)	(10.95)
+6	-7.67	-11.48	-4.81	-7.75	-6.96	-8.50
	(11.21)	(10.48)	(14.37)	(12.86)	(17.62)	(15.08)
+12	6.91	-2.37	-13.15	-6.19	32.94	11.33
	(15.42)	(13.75)	(17.57)	(14.26)	(25.85)	(23.06)
First stage	0.48***	0.48***	1.00***	1.00***	0.27**	0.27**
	(0.10)	(0.10)	(0.00)	(0.00)	(0.13)	(0.13)
Time FE		✓		✓		✓
Obs	743	743	159	159	584	584
Eff. obs	480	488	105	102	346	339
h (days)	All elections		Presidential		Parliamentary	
+1	-6.77***	-5.12**	-4.86*	-3.82	-9.99	-7.64
	(2.37)	(2.29)	(2.49)	(2.39)	(8.84)	(6.28)
+2	-7.36**	-6.05**	-7.01**	-5.71**	-4.94	-5.22
	(3.11)	(2.89)	(3.10)	(2.90)	(6.98)	(5.42)
+3	-5.81*	-5.07	-6.18*	-4.94	-0.53	-3.02
	(3.22)	(3.08)	(3.32)	(3.11)	(5.39)	(4.39)
+6	-6.37	-5.10	-6.87*	-5.19	-3.74	-4.45
	(4.10)	(3.52)	(3.98)	(3.70)	(7.81)	(6.15)
+12	-10.32*	-6.23	-7.22	-5.58	-16.05	-9.09
	(5.31)	(4.38)	(5.60)	(4.84)	(16.86)	(9.69)
First stage	0.48***	0.51***	1.00***	1.00***	0.20	0.20
	(0.11)	(0.11)	(0.00)	(0.00)	(0.19)	(0.19)
Time FE		✓		✓		✓
Obs	568	568	127	127	441	441
Eff. obs	278	258	79	79	181	189

Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the logarithmic change in average share prices between time $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). First stage reports the first-stage in the fuzzy RD estimation, which is jump in the probability of a left victory at the threshold (see main text for definitions). Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Robust bias-corrected standard errors clustered by country. The number of observations refers to the specification with time-horizon $h = 1$. Eff. obs is the number of observations within the MSE-optimal bandwidth. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 3: Effect of a left-wing electoral victory on domestic exchange rate and government bond yields (fuzzy RD estimates; monthly data)

Outcome: log change in USD value of domestic currency between t-1 and t+h

h (days)	All elections		Presidential		Parliamentary	
+1	-7.47**	-9.82***	-10.22**	-12.79***	-4.14	-9.00
	(3.73)	(3.37)	(4.75)	(4.69)	(5.57)	(5.96)
+2	-5.81	-5.36*	-10.19**	-12.84***	-9.62	-9.09**
	(3.76)	(3.11)	(4.41)	(3.80)	(6.76)	(3.67)
+3	-10.56**	-10.12**	-21.18***	-19.45***	-11.99	-14.25
	(4.99)	(4.28)	(6.68)	(7.33)	(9.09)	(8.92)
+6	-15.57	-8.78	-34.18***	-30.50**	-11.99	-17.91
	(10.20)	(8.38)	(12.77)	(12.92)	(11.65)	(12.57)
+12	-20.86	-13.44	-52.45***	-49.59***	-6.08	-7.71
	(13.05)	(11.28)	(15.92)	(18.23)	(14.33)	(13.36)
First stage	0.54***	0.54***	1.00***	1.00***	0.33	0.33*
	(0.11)	(0.12)	(0.00)	(0.00)	(0.21)	(0.20)
Time FE		✓		✓		✓
Obs	682	682	213	213	469	469
Eff. obs	304	296	87	84	147	163

Outcome: change in real bond yields between t-1 and t+h

h (days)	All elections		Presidential		Parliamentary	
+1	-0.31	-0.82	1.23	0.22	-1.28	-1.55
	(1.21)	(1.27)	(2.69)	(2.66)	(0.95)	(1.15)
+2	-0.29	-0.55	2.18	1.21	-0.35	-0.77
	(1.53)	(1.65)	(3.45)	(3.50)	(0.92)	(0.93)
+3	3.61*	3.13	8.78*	7.82*	0.80	0.58
	(2.00)	(2.18)	(4.64)	(4.71)	(1.92)	(2.51)
+6	3.46	2.40	4.02	4.20	4.33	3.05
	(3.26)	(3.33)	(6.47)	(6.61)	(4.49)	(4.80)
+12	0.97	-0.60	-3.00	-3.42	3.83	2.29
	(4.65)	(4.16)	(6.87)	(6.83)	(6.51)	(5.87)
First stage	0.55***	0.55***	1.00***	1.00***	0.40***	0.41***
	(0.10)	(0.10)	(0.00)	(0.00)	(0.11)	(0.12)
Time FE		✓		✓		✓
Obs	569	569	72	72	497	497
Eff. obs	323	312	49	49	278	256

Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in the outcome between time $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). First stage reports the first-stage in the fuzzy RD estimation, which is jump in the probability of a left victory at the threshold (see main text for definitions). Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Robust bias-corrected standard errors clustered by country. The number of observations refers to the specification with time-horizon $h = 1$. Eff. obs is the number of observations within the MSE-optimal bandwidth. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 4: Heterogeneous effect of left-wing electoral victories based on Left's policy positions (RD Estimates; reduced-form relation; parliamentary elections)

	Stock Market (Monthly)				Stock Market (Daily)			
	Market-oriented Left		Interventionist Left		Market-oriented Left		Interventionist Left	
+1	-3.92	-3.72	-5.51*	-7.80***	-0.59	-0.80	-2.50**	-5.43***
	(5.48)	(4.92)	(2.89)	(2.28)	(1.52)	(0.96)	(1.16)	(1.90)
+2	-3.38	-2.41	-8.46*	-11.98***	0.57	0.11	-3.40**	-6.26***
	(6.36)	(5.38)	(4.55)	(3.32)	(1.31)	(1.17)	(1.32)	(1.56)
+6	-7.90	-3.38	-3.37	-9.08	-0.17	-3.15*	1.42	-2.13
	(7.93)	(6.55)	(8.98)	(8.44)	(2.63)	(1.70)	(2.73)	(2.14)
+12	15.21	12.28	-10.09	-16.33*	-0.19	-2.60	-5.55	-9.40***
	(11.36)	(10.10)	(14.26)	(8.97)	(2.43)	(1.87)	(3.46)	(3.27)
Obs	275	275	273	273	202	202	204	204
Eff. obs	136	151	67	71	66	82	43	29
Time FE		✓		✓		✓		✓

	Exchange Rate				Gov't Bonds: Real Yields			
	Market-oriented Left		Interventionist Left		Market-oriented Left		Interventionist Left	
+1	-2.78	-3.37***	-3.52*	-4.67***	1.33*	0.89	-0.57	-0.38
	(2.00)	(1.24)	(1.85)	(1.55)	(0.77)	(0.68)	(0.37)	(0.78)
+2	-2.19	-3.86**	-3.16	-3.52***	1.04	0.43	-0.70	-0.08
	(1.51)	(1.52)	(1.94)	(1.35)	(0.83)	(0.87)	(0.46)	(0.89)
+6	-0.79	-4.78	-10.45**	-7.58	7.47	6.23	1.46	2.07
	(3.71)	(3.39)	(4.98)	(4.63)	(7.07)	(6.02)	(1.57)	(2.20)
+12	1.75	-0.31	-11.17	-8.24	9.74	7.47	0.46	1.36
	(5.07)	(5.71)	(8.93)	(8.59)	(9.37)	(7.49)	(2.43)	(2.56)
Obs	211	211	236	236	229	229	225	225
Eff. Obs.	105	120	77	67	47	49	47	54
Time FE		✓		✓		✓		✓

Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in the outcome between time $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate the reduced-form relation between left margin crossing the threshold and the outcomes of interest, through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). The 'Market-oriented left' specifications include only elections in which the proposed economic policy of the main left party was more pro-market than the median. The 'interventionist left' specifications include only elections in which the proposed economic policy of the main left party was less pro-market than the median. Proposed economic policy proxied by the difference between the variables `planeco-markeco` from the Manifesto Project Database (Volkens et al., 2018). Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Robust bias-corrected standard errors clustered by country. The number of observations refers to the specification with time-horizon $h = 1$. Eff. obs is the number of observations within the MSE-optimal bandwidth. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 5: Heterogeneous effect of left-wing electoral victories based on income level (fuzzy RD Estimates; parliamentary elections)

	Stock Market (Monthly)				Stock Market (Daily)			
	High income		Developing		High income		Developing	
+1	-4.08	-4.18	-20.08*	-19.94**	-4.28**	-2.45*	-6.99	-8.21*
	(5.49)	(4.72)	(10.70)	(9.68)	(1.70)	(1.38)	(4.99)	(4.39)
+2	-4.23	-6.55	-13.85	-14.76	-3.20	-2.57	-9.80	-11.43**
	(7.51)	(5.66)	(11.25)	(11.19)	(2.72)	(2.06)	(6.29)	(5.59)
+6	1.10	-4.73	-25.76	-26.55*	-4.50	-3.24	-9.19	-10.61*
	(11.63)	(11.87)	(16.54)	(15.95)	(4.36)	(3.16)	(6.17)	(6.12)
+12	17.23	3.59	-12.42	-11.63	-8.90	-6.27	-10.18	-11.38
	(25.42)	(16.09)	(23.95)	(22.33)	(6.34)	(4.39)	(7.71)	(7.94)
First stage	0.47**	0.46**	0.71***	0.73***	0.50**	0.50**	0.63***	0.62***
	(0.20)	(0.20)	(0.12)	(0.12)	(0.19)	(0.19)	(0.18)	(0.17)
Obs	500	500	243	243	368	368	200	200
Eff. obs	133	126	128	154	129	118	68	72
Time FE		✓		✓		✓		✓

	Exchange Rate				Gov't Bonds: Real Yields			
	High income		Developing		High income		Developing	
+1	4.57	-1.94	-11.92**	-13.03**	-0.15	-0.20	1.65	0.45
	(3.57)	(2.65)	(5.56)	(5.20)	(0.63)	(0.60)	(4.01)	(4.10)
+2	0.50	-1.48	-6.49	-7.90*	0.16	-0.22	1.91	1.37
	(3.53)	(3.00)	(5.90)	(4.54)	(0.76)	(0.72)	(5.72)	(5.76)
+6	6.59	-6.42	-16.75	-9.65	4.91	4.36	4.76	5.23
	(6.33)	(7.25)	(15.40)	(14.98)	(5.14)	(5.22)	(11.28)	(11.73)
+12	9.14	-0.43	-41.93*	-25.48	2.64	3.96	-6.56	-8.00
	(9.37)	(11.76)	(22.02)	(21.50)	(5.55)	(6.29)	(12.12)	(11.16)
First stage	0.42	0.43*	0.72***	0.72***	0.57***	0.64***	0.75***	0.75***
	(0.26)	(0.25)	(0.13)	(0.13)	(0.13)	(0.15)	(0.18)	(0.18)
Obs	383	383	299	299	460	460	109	109
Eff. Obs.	87	97	131	124	188	152	57	57
Time FE		✓		✓		✓		✓

Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in the outcome between $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). First stage reports the first-stage in the fuzzy RD estimation, which is jump in the probability of a left victory at the threshold (see main text for definitions). High income countries are those classified as such by the World Bank, while developing countries are those classified by the World Bank as low or middle-income. Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Robust bias-corrected standard errors clustered by country. The number of observations refers to the specification with time-horizon $h = 1$. Eff. obs is the number of observations within the MSE-optimal bandwidth. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 6: Heterogeneous effect of left-wing electoral victories: pre and post-1990 (fuzzy RD Estimates; presidential and parliamentary elections)

	Stock Market (Monthly)				Stock Market (Daily)			
	Pre-1990		Post-1990		Pre-1990		Post-1990	
+1	-16.67	-14.63	-13.04***	-13.25***	-7.36	-6.42	-5.48**	-4.33**
	(10.43)	(9.97)	(4.67)	(4.77)	(6.69)	(5.41)	(2.19)	(1.87)
+2	-16.02	-14.01	-10.13*	-9.93	-6.00	-6.70	-7.45**	-6.05**
	(11.05)	(9.94)	(5.86)	(6.12)	(8.36)	(6.99)	(2.96)	(2.63)
+6	-2.96	-3.98	-13.58	-19.17	-5.43	-7.65	-5.52*	-4.28
	(15.39)	(13.46)	(13.78)	(13.56)	(11.55)	(10.12)	(3.19)	(2.85)
+12	22.65	18.80	-9.52	-19.46	-11.17	-8.71	-7.39*	-6.06
	(22.06)	(18.25)	(19.85)	(18.46)	(13.52)	(11.77)	(4.35)	(3.91)
First stage	0.46**	0.46**	0.55***	0.53***	0.59**	0.58**	0.46***	0.47***
	(0.18)	(0.18)	(0.13)	(0.12)	(0.26)	(0.26)	(0.11)	(0.11)
Obs	312	312	431	431	152	152	416	416
Eff. obs	219	220	175	186	63	64	231	239
Time FE		✓		✓		✓		✓

	Exchange Rate				Gov't Bonds: Real Yields			
	Pre-1990		Post-1990		Pre-1990		Post-1990	
+1	-4.99	-7.13	-12.10**	-14.14***	0.90	0.90	-0.26	-1.33
	(5.23)	(5.21)	(5.07)	(4.60)	(0.91)	(1.01)	(2.76)	(2.86)
+2	-3.50	-3.92	-11.60**	-14.73***	0.89	0.75	-0.07	-0.84
	(4.01)	(3.46)	(5.66)	(4.52)	(1.09)	(1.18)	(3.94)	(4.02)
+6	0.28	-10.37	-29.04	-30.25*	11.72	11.98	2.65	2.68
	(9.13)	(9.76)	(19.03)	(17.57)	(9.74)	(10.52)	(7.01)	(6.61)
+12	0.93	-10.17	-41.73*	-39.74**	17.48	12.79	-5.74	-4.87
	(13.98)	(14.08)	(21.71)	(18.57)	(15.37)	(11.86)	(8.49)	(8.58)
First stage	0.49***	0.49***	0.63***	0.64***	0.68**	0.69***	0.47***	0.48***
	(0.17)	(0.18)	(0.13)	(0.13)	(0.26)	(0.26)	(0.15)	(0.15)
Obs	376	376	306	306	257	257	312	312
Eff. Obs.	181	175	129	125	81	76	165	167
Time FE		✓		✓		✓		✓

Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in the outcome between $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). First stage reports the first-stage in the fuzzy RD estimation, which is jump in the probability of a left victory at the threshold (see main text for definitions). Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Robust bias-corrected standard errors clustered by country. The number of observations refers to the specification with time-horizon $h = 1$. Eff. obs is the number of observations within the MSE-optimal bandwidth. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 7: Effect of a left-wing electoral victory (fuzzy RD Estimates)
Robustness to alternative bandwidth selection criteria

Time	MSE (CCT) (common)	MSE (CCT) (two)	MSE (IK) (common)	CER (CCT) (common)	CER (CCT) (two)
Share prices (monthly)					
+1 months	-13.13**	-12.15**	-7.21	-13.63**	-12.69**
	(5.20)	(5.41)	(6.01)	(5.47)	(5.84)
+2	-11.56**	-11.39**	-7.75	-11.97**	-11.90**
	(5.51)	(5.43)	(5.54)	(5.83)	(5.83)
+6	-11.48	-10.36	-13.62	-11.49	-11.93
	(10.48)	(11.28)	(11.23)	(10.41)	(11.29)
Share prices (daily)					
+1 days	-5.12**	-5.43**	-5.42**	-4.91**	-5.14**
	(2.29)	(2.60)	(2.67)	(2.36)	(2.58)
+2	-6.05**	-6.25*	-6.61**	-6.05**	-5.89*
	(2.89)	(3.24)	(3.33)	(2.98)	(3.31)
+6	-5.10	-5.07	-6.48	-5.47	-5.38
	(3.52)	(3.48)	(4.33)	(3.66)	(3.69)
Domestic exchange rate					
+1 months	-9.82***	-8.76***	-12.29**	-9.85***	-9.65***
	(3.37)	(2.99)	(5.18)	(3.55)	(3.09)
+2	-5.36*	-7.34***	-10.34**	-6.51**	-8.55***
	(3.11)	(2.62)	(4.30)	(2.97)	(2.74)
+6	-8.78	-18.22**	-18.98**	-12.18	-19.49**
	(8.38)	(8.39)	(9.60)	(8.80)	(9.17)
Real government bond yields					
+1 months	-0.82	-0.82	-0.33	-0.86	-0.85
	(1.27)	(1.27)	(1.07)	(1.26)	(1.25)
+2	-0.55	-0.87	-0.63	-0.62	-0.70
	(1.65)	(1.71)	(1.32)	(1.64)	(1.65)
+6	2.40	3.83	5.04	2.99	3.99
	(3.33)	(3.67)	(5.13)	(3.57)	(3.80)
Time FE	✓	✓	✓	✓	✓

Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in the outcome between time $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Column titles indicate the criterion used for selecting the bandwidth employed by the RD estimator. MSE (CCT) is the MSE-optimal bandwidth selector using the procedure of Calonico et al. (2014); MSE (IK) is the MSE-optimal bandwidth selector using the procedure of Imbens and Kalyanaraman (2012); CER (CCT) is the CER-optimal bandwidth selector using the procedure of Calonico et al. (2014); ‘common’ means that a unique bandwidth size is used on both sides of the threshold; ‘two’ means that two different bandwidth sizes are used (below and above the cutoff). Robust bias-corrected standard errors clustered by country. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Appendix A. Anticipation effects in our dynamic RD design

Even assuming that its strong identification assumptions hold, a traditional event-study – employing a case study of a single election or aggregating across many episodes – would provide underestimates of the stock market effect of electoral outcomes because of anticipation effects. A measure of ex-ante probabilities would thus be needed, to adjust for anticipation effects and recover the overall effect of interest.

To the contrary, under the (weaker) usual identification assumptions of the regression-discontinuity (RD) design, coefficients from our dynamic RD design (eq. 1 in the main text) provide a correct estimate of the overall effect, without any need to correct for anticipation effects. In this case the key RD identification assumptions include the assumption that ex-ante probabilities, like all other confounding factors, do not jump at the threshold. Intuitively, the required 'continuity of ex-ante probabilities' assumption says that, on average, ex-ante probabilities in arbitrarily close Left victories and Left losses are similar.

To see this, let us start by noting that our overall average treatment effect of interest can be written as

$$ATE^{\star} = E[y(1)_{c,t} - y(0)_{c,t}] \tag{2}$$

where $y(1)_{c,t}$ is (the log of) the level of share prices that would be observed in a country c after an election that took place at time t, under the treatment of a Left electoral victory; $y(0)_{c,t}$ is (the log of) the level of share prices that would be observed under a Left electoral loss.

Also note that the level of share prices before an election can be seen as a weighted average of expected valuations conditional on the two possible election outcomes (the Left win or the Left does not win), with weights given by perceived ex-ante probabilities.[1]

[1] As usual in the literature, in this discussion we abstract from discounting (given the short time period involved) and risk aversion.

We thus have

$$y(i)_{c,t-1} = E_{t-1}(y_{c,t}) = y(1)_{c,t}\pi(i) + y(0)_{c,t}[1-\pi(i)] \qquad \text{for } i = 0,1 \tag{3}$$

where $\pi(i)$ is the ex-ante probability of a Left victory before an election in which outcome i will occur.

Anticipation effects in traditional event-studies Under the strong assumption that electoral outcomes are exogenous to economic conditions,[2] a simple event-study will correctly estimate the post-election change in share prices caused by a partially unanticipated Left victory. Call this effect $E[\Delta y(1)_{c,t}] = E[y(1)_{c,t} - y(1)_{c,t-1}]$.

Eq.3 implies that this estimated price change is equal to the overall effect of interest ($ATE^{\star}$) times the 'surprise':

$$E[\Delta y(1)_{c,t}] = E[y(1)_{c,t} - y(0)_{c,t}][1-\pi(1)] = ATE^{\star}[1-\pi(1)] \tag{5}$$

The overall effect of interest can thus be recovered as the estimated price change divided by the 'surprise':

$$ATE^{\star} = \frac{E[\Delta y(1)_{c,t}]}{1-\pi(1)} \tag{6}$$

An estimate of $\pi(1)$ – the ex-ante probability of Left victory perceived by financial investors before the election – is therefore needed to correct for anticipation effects.

Anticipation effects in our RD design Or RD design exploits knowledge of a 'running variable', the Left margin in the election ($X_{c,t}$), which determines whether the treatment of a Left electoral victory is assigned. Treatment is assigned in country c at time t if $X_{c,t}$ is above the threshold, and is not assigned otherwise. We thus have $D = 1\{X_{c,t} > x_0\}$, where x_0 is the threshold and D is a dummy variable equal to 1 if a Left victory is observed and 0 otherwise.

[2]Formally, this assumption can be written as

$$E[y_{c,e}(0)|D_{c,e} = 1] = E[y_{c,e}(0)|D_{c,e} = 0] \tag{4}$$

where D is a dummy equal to 1 if a 'Left-victory' is observed and 0 otherwise. This implies that there is no selection bias.

The crucial identifying assumption of the RD approach is 'smoothness' or, more precisely, continuity of average potential outcomes at the threshold:

$$E[y(0)|X = x] \text{ and } E[y(1)|X = x] \text{ are continuous in } x \text{ at } x_0 \tag{7}$$

The smoothness assumption of eq. 7, combined with eq. 3, implies that our RD specification (eq.1 in the main text), which looks at average changes in stock prices around elections, correctly estimates the following local average effect:[3]

$$\gamma_{RD} = \lim_{x \downarrow x_0} E[\Delta y|X = x] - \lim_{x \uparrow x_0} E[\Delta y|X = x] =$$

$$= (\lim_{x \downarrow x_0} E[y|X = x] - \lim_{x \uparrow x_0} E[y|X = x])[1 - (\lim_{x \downarrow x_0} E[\pi|X = x] - \lim_{x \uparrow x_0} E[\pi|X = x])] =$$

$$= ATE^{\star}\{1 - (\lim_{x \downarrow x_0} E[\pi|X = x] - \lim_{x \uparrow x_0} E[\pi|X = x])\} \tag{8}$$

where γ_{RD} is the estimated coefficient from our RD specification (eq.1 in the main text), and $\Delta y = y_{c,t} - y_{c,t-1}$.

This makes it clear that the relation between our estimated effect (γ_{RD}) and the overall effect of interest ($ATE^{\star}$) depends on the behavior of ex-ante probabilities *at the threshold*.

Under the reasonable assumption that average ex-ante probabilities, like other confounding factors, do not jump at the threshold, we would have

$$\lim_{x \downarrow x_0} E[\pi|X = x] = \lim_{x \uparrow x_0} E[\pi|X = x] \quad \Rightarrow \quad \gamma_{RD} = ATE^{\star} \tag{9}$$

Our RD specification thus provides an estimate of the overall effect of Left electoral victories, without the need to correct for ex-ante probabilities, *as long as ex-ante probabilities do not jump at the threshold*. Intuitively, this assumption says that, on average, ex-ante probabilities in close Left victories and close Left losses are similar.

What would happen if this assumption failed? If ex-ante probabilities do jump at the threshold, and the average ex-ante probability of Left victory is substantially higher

[3]We focus here on the case with $h = 0$, but the same would apply to any different time-horizon.

before close Left victories relative to close Left losses, we would have:

$$\lim_{x \downarrow x_0} E[\pi|X = x] > \lim_{x \uparrow x_0} E[\pi|X = x] \quad \Rightarrow \quad abs(\gamma_{RD}) < abs(ATE^{\star}) \tag{10}$$

Our estimates would thus have the correct sign but underestimate the magnitude of the effect by a factor equal to $[1 - (\lim_{x \downarrow x_0} E[\pi|X = x]) - (\lim_{x \uparrow x_0} E[\pi|X = x])]$. As long as close electoral victories are harder to predict than large ones, this anticipation-bias is smaller than the anticipation-bias suffered by traditional event studies, because $[(\lim_{x \downarrow x_0} E[\pi|X = x]) - (\lim_{x \uparrow x_0} E[\pi|X = x])] < \pi(1)$.

In the extreme case in which investors are able to forecast with certainty any arbitrarily close electoral outcome, our approach would not be valid, as it would invariably lead to estimating a null effect. In that case we would have that $\lim_{x \downarrow x_0} E[\pi|X = x] = 1$ and $\lim_{x \uparrow x_0} E[\pi|X = x] = 0$. This would imply $\gamma_{RD} = 0$, even if the overall effect of interest $ATE^{\star}$ is actually different from zero. Unsurprisingly, perfect anticipation of all electoral outcomes, no matter how close, would invalidate our approach.[4]

In the (clearly implausible) case in which the average ex-ante probability of Left victory is systematically *lower* before close Left victories relative to close Left losses, our estimates would have the same sign but overestimate the magnitude of the effect (as easily seen by inverting the inequality sign in eq.10).

To sum up, if the 'smoothness in ex-ante probabilities' assumption holds at the threshold, our dynamic RD specification provides a correct estimate of the average treatment effect of interest, without any need to adjust for anticipation effects. Broadly speaking, this assumption says that, on average, ex-ante probabilities are similar before close Left victories and close Left losses. If the assumption fails and ex-ante probabilities do exhibit a positive jump at the threshold, our RD approach would underestimate the magnitude of the effect of interest because of anticipation effects, but the bias would be smaller than the bias of a traditional event-study, as long as close electoral victories are harder to predict than large ones. The assumptions under which our approach would fail or overestimate the magnitude of the effect are instead rather extreme: they would require investors to forecast with certainty any arbitrarily close electoral outcome (in

[4]Of course, perfect anticipation would invalidate also a traditional case study.

which case we would always obtain a null coefficient, independently of the true effect), or the ex-ante probability of Left victory to be systematically and substantially lower before close Left victories relative to close Left losses (in which case we would overestimate the magnitude of the effect).

Appendix B. List of countries and stock market indexes

Table B.1: List of countries, elections and stock market indexes

Ctry	Elections						Stock market index
	All		Parliam.		Presid.		
	tot	use	tot	use	tot	use	
ALB	8	3	8	3	0	0	n.a.
ARG	28	5	26	2	13	3	Buenos Aires SE General Index (IVBNG)
ARM	12	3	6	2	6	1	n.a.
AUS	28	28	28	28	0	0	ASX All-Ordinaries (w/GFD extension)
AUT	33	20	21	20	12	0	Wiener Boersekammer (WBKI)
AZE	12	1	5	1	7	0	n.a.
BEL	22	22	22	22	0	0	Brussels All-Share (w/GFD extension)
BGR	16	10	10	10	6	0	SOFIX
BIH	8	3	8	3	5	0	Sarajevo SE Bosnian Investment Funds
BLR	9	2	4	1	5	1	n.a.
BOL	14	3	10	0	7	3	n.a.
BRA	24	8	14	0	17	8	IBX-100 (IBV pre-1995)
CAF	11	1	6	0	6	1	n.a.
CAN	23	23	23	23	0	0	S&P/TSX 300 CI (w/GFD extension)
CHE	18	18	18	18	0	0	CHE Price Index (w/GFD extension)
CHL	24	14	14	3	12	11	Santiago SE IGPA
CIV	15	2	7	0	9	2	n.a.
CMR	15	2	7	0	9	2	n.a.

Table B.1: List of countries, elections and stock market indexes

Ctry	Elections						Stock market index
	All		Parliam.		Presid.		
	tot	use	tot	use	tot	use	
COG	18	1	13	0	5	1	n.a.
COL	38	8	22	0	18	8	IGBC GI (w/GFD extension)
CRI	20	16	17	0	18	16	IDB data
CYP	17	17	9	9	8	8	CSE All Share CI
CZE	12	9	10	7	2	2	Prague PX
DEU	8	8	8	8	0	0	CDAX CI (w/GFD extension)
DJI	12	3	5	0	7	3	n.a.
DNK	27	27	27	27	0	0	OMX Copenhagen All-Share
DOM	18	4	12	0	14	4	n.a.
ECU	30	8	19	0	16	8	Guayaquil BdV (Quito SE pre-1994)
EGY	21	1	12	0	9	1	Cairo SE EFG General Index
ESP	13	13	13	13	0	0	Madrid SE GI (w/GFD extension)
EST	11	6	7	6	4	0	OECD MEI data
FIN	32	20	20	20	12	0	OMX Helsinki All-Share
FRA	30	25	20	18	10	7	CAC All-Tradable (w/GFD extension)
FRG	11	11	11	11	0	0	CDAX CI (w/GFD extension)
GAB	15	1	8	0	7	1	n.a.
GBR	19	19	19	19	0	0	FTSE All-Share (w/GFD extension)
GEO	14	2	8	1	7	1	n.a.
GHA	10	7	7	0	8	7	GSE CI
GIN	11	4	4	0	7	4	n.a.
GMB	14	2	9	0	8	2	n.a.
GNB	11	1	5	0	6	1	n.a.
GRC	26	20	26	20	0	0	DJ (National Bank pre-1992; Athens CI pre-1978)
GTM	29	1	18	0	12	1	n.a.

Table B.1: List of countries, elections and stock market indexes

Ctry	Elections						Stock market index
	All		Parliam.		Presid.		
	tot	use	tot	use	tot	use	
HND	12	2	11	0	12	2	n.a.
HRV	14	8	9	6	6	2	CROBEX
HTI	10	3	7	0	7	3	n.a.
HUN	16	8	11	8	5	0	OECD MEI data
IRL	31	20	20	20	11	0	ISEQ Overall (w/GFD extension)
ISL	35	21	22	21	13	0	OMX Iceland All-Share
ISR	21	21	20	18	3	3	Tel Aviv All-Share
ITA	18	18	18	18	0	0	BCI (w/GFD extension)
JPN	26	22	26	22	0	0	Tokyo SE (TOPIX) (w/GFD extension)
KAZ	12	4	6	0	6	4	Kazakhstan SE KASE Index
KGZ	13	4	6	0	7	4	Kyrgyz SE
KOR	22	5	10	4	12	1	KOSPI
LKA	23	6	16	0	7	6	Colombo SE All-Share
LTU	15	10	9	7	6	3	OMXV all-shares (Litin-G pre-2005)
LUX	16	13	16	13	0	0	LUXX (w/GFD extension)
LVA	9	9	9	9	0	0	IMF IFS data
MDA	15	3	8	3	7	0	n.a.
MDG	18	5	9	0	10	5	n.a.
MEX	25	8	24	0	9	8	MEX SE IPC
MKD	12	6	9	6	5	0	MBI-10
MLT	12	9	12	9	0	0	Malta SE Index
MNE	13	4	10	4	3	0	MONEX
MNG	17	7	10	0	7	7	MNG SE Top-20
MOZ	7	4	7	0	5	4	n.a.
MWI	9	1	9	0	5	1	n.a.

Table B.1: List of countries, elections and stock market indexes

Ctry	Elections						Stock market index
	All		Parliam.		Presid.		
	tot	use	tot	use	tot	use	
NAM	6	3	6	0	5	3	NAM SE Overall
NGA	12	4	10	0	8	4	NGA SE
NIC	8	2	6	0	8	2	n.a.
NLD	22	22	22	22	0	0	NLD All-Share (w/GFD extension)
NOR	18	18	18	18	0	0	Oslo SE OBX-25 (w/GFD extension)
NZL	24	24	24	24	0	0	NZL SE All-Share
PAN	13	1	13	0	12	1	Panama SE BVPSI
PER	18	5	12	0	13	5	Lima S&P/BVL GI (w/GFD extension)
PHL	19	2	17	0	12	2	Manila SE CI
POL	18	10	12	8	6	2	OECD MEI data
PRT	24	24	15	15	9	9	Oporto PSI-20
PRY	11	4	9	0	11	4	Asuncion SE PDV GI
ROU	16	11	11	6	7	5	Bucharest SE
RUS	13	12	6	6	7	6	MICEX/MOEX (AK&M pre-1997)
SDN	12	2	9	0	7	2	n.a.
SEN	16	6	12	0	8	6	n.a.
SLE	14	3	9	0	7	3	n.a.
SLV	21	5	12	0	10	5	El Salvador Stock Market Index
SRB	16	5	12	5	8	0	Serbia MSCI Standard
SVK	15	10	11	8	4	2	Bratislava SE SAX
SVN	14	7	8	7	7	0	SVN SE SBITOP Blue Chip
SWE	21	21	21	21	0	0	OMX Stockholm All-Share
SYR	17	7	9	0	8	7	n.a.
TUN	11	3	8	0	8	3	n.a.
TUR	21	19	19	18	2	1	Istanbul SE IMKB-100

Table B.1: List of countries, elections and stock market indexes

Ctry	Elections						Stock market index
	All		Parliam.		Presid.		
	tot	use	tot	use	tot	use	
TZA	12	9	9	0	10	9	Dar-Es-Saleem SE
UKR	12	7	7	5	5	2	PFTS OTC Index
URY	17	9	14	0	11	9	Montevideo BdV (URY SE pre-2008)}
USA	36	18	36	18	18	0	S&P 500 CI (w/GFD extension)
UZB	11	2	6	0	5	2	UCI
VEN	19	12	13	0	15	12	Caracas SE GI (w/GFD extension)
ZAF	17	5	17	5	0	0	FTSE/JSE All-Share (w/GFD extension)
ZMB	12	10	8	0	12	10	Lusaka All-Share (LASI)
ZWE	13	2	10	0	5	2	n.a.

Notes: 'tot' is the total number of elections that we have information about; 'use' is the number of elections for which we could calculate the 'left margin' variable *and* data is available for at least one of our financial outcomes of interest (so they are used in estimation). Countries for which we have election data but no election is used in estimation (because we could not calculate the left margin variable or data is not available for any financial outcome) are not included. The stock market index is the one used in the monthly dataset. In some cases this may differ from the one used in the daily dataset, due to data availability reasons. The stock market index used in the daily analysis is reported in the replication files (`stock_index` variable).

Appendix C. Additional information on the elections dataset

This appendix provides additional information on how the key variables in the elections dataset were computed.

C.1 Left margin in presidential elections

To calculate the left margin in each presidential election, we classify the three most-voted presidential candidates as left, conservative or neither.[5] We then take the difference between the vote share of the most-voted left candidate and the vote share of the most-voted non-left candidate. Our dataset, available in the replication files, reports the source of the classification for each of the three most-voted candidates in each presidential election (variables `source_left_first`, `source_left_second`, `source_left_third`). In what follows we provide additional details on how candidates' partisanship was coded.

For 166 Latin American presidential elections, we applied the ideological codings of MPD, Baker and Greene (2011) or Coppedge (1997). The MPD, our main source of partisanship information in parliamentary elections, provides data on 20 Latin American presidential elections. In those 20 elections, we use the same classification applied to parliamentary elections: a presidential candidate is left-wing if her party/coalition is classified by MPD as either 'Socialist', 'Social-Democratic' or 'Ecologist'. 83 of the remaining Latin American presidential elections are included in the Baker and Greene (2011) partisanship coding. For those elections, we follow Baker and Greene (ibid.), which provides a continuous partisanship measure on the left-right scale and thresholds for converting the continuous measure into a discrete coding. A third source of partisanship information in Latin American elections is Coppedge (1997), which covers 800 Latin American parties in 11 countries in parliamentary elections in the 1912-1995 period. When a presidential election is held in the same year of a parliamentary election covered by Coppedge (ibid.), we apply to a candidate Coppedge' partisan coding of her party. In this way we are able to code 63 additional presidential elections which are not covered in either MPD or Baker and Greene (2011). We consider as (center-)left the parties classified by Coppedge as 'Secular Left', 'Secular Center-Left', 'Christian Left'and 'Christian Center-Left'.

In the remaining presidential elections, we look at whether the party of a candidate is affiliated with some partisan international association. When this is the case,

[5] When elections are decided in a run-off, we consider only the run-off, not the first round. In few cases we also consider the fourth most-voted candidate, when she/he obtains a significant vote share.

we attribute to the candidate the partisanship of the international association: left for Socialist International, Foro de Sao Paulo, Party of European Socialists and Progressive Alliance; conservative for Liberal International, Centrist/Christian Democrat International, European People's Party, International Democrat Union and Alliance of Conservatives and Reformists in Europe . When this does not apply, we resort to published books or articles which explicitly classify candidates or their parties as (center-)left or conservative. Lansford's Political Handbook of the World (Lansford, 2017) is our main international source in this regard, while in other cases we resort to country or election-specific articles/books. These are all listed in our dataset in the variables `source_left_first`, `source_left_second` and `source_left_third`.

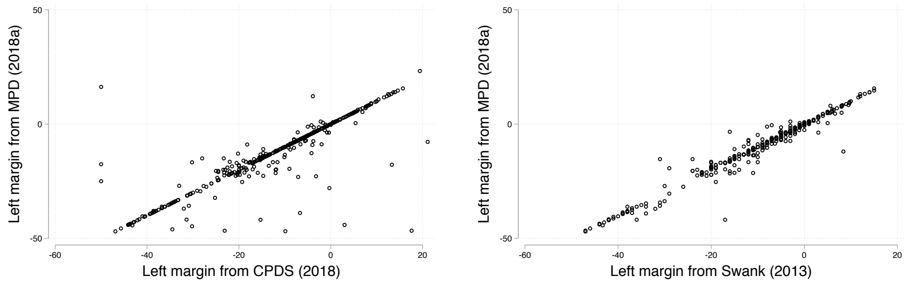
C.2 Left share of parliamentary seats

As explained in the main text, we calculate the left's share of parliamentary seats from the data in the Manifesto Project Database (Volkens et al., 2018), considering as left-wing the parties classified by MPD as 'Socialist', 'Social-Democratic' or 'Ecologist'.

We calculate the left's share of seats also from Armingeon et al. (2018) and Swank (2013). In using Armingeon et al. (2018), we sum the seats of parties classified in this dataset as 'Social-Democratic', 'Left Socialist', 'Communist', 'Post-Communist', and 'Green'. In using Swank (2013), we sum the seats of parties classified by Swank (ibid.) as 'Left'.

Reassuringly, the correlation between the left share of parliamentary seats obtained from these three alternative sources is very strong, in the elections in which they overlap. This is shown in Figures C.1

Cross-checking with these alternative sources, we found and corrected a very small number of mistakes in our main source, the MPD parliamentary data. We correct mistakes in election dates regarding the 1954 election in Ireland and the 1959 election in Israel. More importantly, we also correct five mistakes in the ideological classification (`parfam` variable). These do not appear as ambiguous or difficult calls, but as straightforward mistakes. They are: the Portuguese Social Democratic Party (PSD), which



(a) MPD vs. CPDS (b) MPD vs. Swank (2013)

Figure C.1: Left margin in parliamentary elections, computed from alternative sources

Notes: These graphs plots the left's parliamentary margin variable built from MPD data against the same variable computed from alternative sources.

is mistakenly classified by MPD as center-left (notwithstanding its name, it is universally recognized as a center-right party, affiliated with the conservative Centrist Democrat International and European People's Party); the Portuguese Democratic Renewal Party (PRD), which is mistakenly classified as center-left (it is a centrist party, member of European Democratic Alliance); the Danish Centrum-Demokraterne (CD), which is mistakenly classified as center-left (it is a centrist party, which supported several center-right governments and is affiliated with the conservative European People's Party); the Spanish Catalan Republican Left (ERC), which is classified as a purely regional party but we consider as left-wing (the party presents itself as a left-wing party and has been in coalition with the Socialist Party); the Macedonian Alliance for Macedonia (or Union of Macedonia) coalition (a coalition of parties individually classified as left-wing by the MPD, but itself mistakenly – we believe – classified as liberal). Importantly, we find that these corrections do not affect significantly our main results.

C.3 Ideology of after-election governments in parliamentary elections

We build two measures of partisanship for the governments formed after the parliamentary elections in our sample. The first is the share of left-wing cabinet members. The second is a dummy variable for whether the government is left-leaning. We use the second as the measure of a left-wing electoral victory in parliamentary elections that we use in our fuzzy RD design, because the first is available only for a subset of parliamentary elections.

Share of left-wing cabinet members The Party Government Data Set (PGDS), in the updated version of Seki and Williams (2014), covers the governments of 49 countries in the 1945-2014 period. It provides data on the share of cabinet members of each party (reporting also the party identifier in the MPD) and the date of the most recent parliamentary election. This allows to match this dataset with the MPD, matching each government with the most recent parliamentary election, and calculating the share of cabinet members of parties classified as left-wing by the MPD. We consider only the first government formed after each election. In this way we obtain the left cabinet members variable for 487 of the parliamentary elections in our sample. To extend the coverage of this variable, we calculate this measure also from the Armingeon et al., 2018 and Swank, 2013 government partisanship datasets, applying their partisan coding (which as we have seen is strongly correlated with the MPD coding – Figure C.1). Armingeon et al., 2018 allows to cover other 186 elections, while Swank, 2013 adds 20 elections missing in both PGDS and Armingeon et al. (2018). The left cabinet members variable is thus available for 693 parliamentary elections in our sample, 510 of which can be used in estimation (based on financial data availability).

Partisanship of after-election government This variable is an indicator for whether the first government formed after a parliamentary election is left-leaning. In the elections for which it was possible to build the share of left-wing cabinet members, we build the partisanship variable based on the cabinet members variable. In particular, following the Schmidt-index (Schmidt, 1992), we classify a government as left-leaning if the share of left-wing cabinet members is at least two-thirds. In some elections which Armingeon et al., 2018 covers, but in which the cabinet members data is missing, we build this indicator using the share of government held parliamentary seats as a proxy for the share of cabinet members (this is done only in building the dummy for a left government, not the share of left-wing cabinet members). For the remaining elections, we use the ideological coding provided in the Database of Political Institutions (DPI) dataset (Cruz et al., 2016), which is a cruder measure, based on the partisan affiliation of the chief executive officer (the prime minister in most parliamentary elections). There are only 18 parliamentary elections for which we have the left margin variable and financial data are available, but the partisanship of the after-election government is not available from the sources listed. We build the indicator for these elections by using publicly available information on the party affiliations of the prime minister, and applying the same criterion.

Appendix D. Additional results

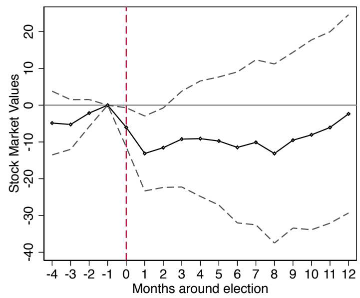
D.1 Manipulation tests

Table D.1: Tests for a discontinuity in the running variable at the threshold

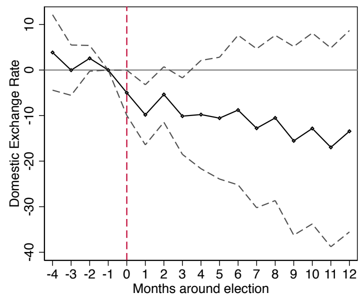
	All		Presidential		Parliamentary	
	McCrary	CJM	McCrary	CJM	McCrary	CJM
T-stat	-1.21	0.77	-0.31	0.57	-0.95	0.49
p-value	0.23	0.44	0.76	0.57	0.35	0.63

Notes: the 'McCrary' column reports the McCrary (2008) manipulation test; the 'CJM' column reports the Cattaneo et al. (2017) test. They both test the null hypothesis of a discontinuity in the distribution of the running variable (the left margin in the election) at the cutoff.

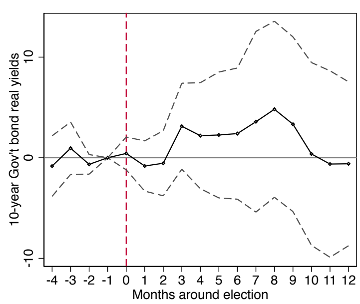
D.2 Graphs using abnormal returns



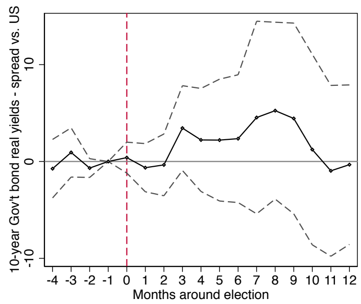
(a) Share Prices



(b) Exchange Rates



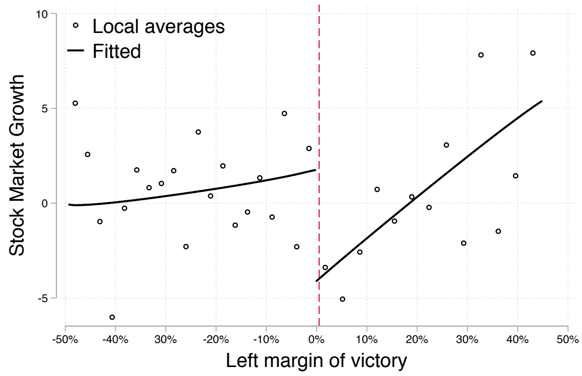
(c) Gov't bonds: real yield



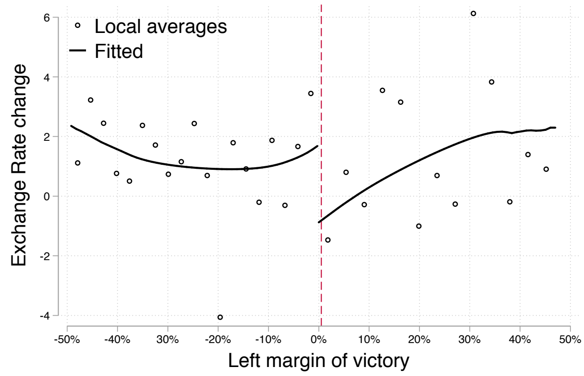
(d) Gov't bonds: spread vs. US

Figure D.1: Effect of a left-wing electoral victory on financial markets (abnormal returns) (Dynamic regression-discontinuity estimates; monthly data)

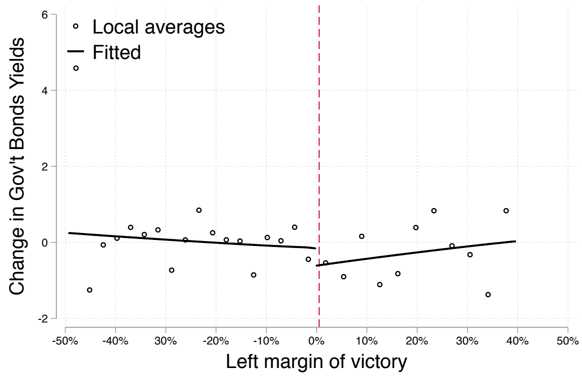
Notes: Effect of a left victory. $t = 0$ is the month of the election. Fuzzy RD estimates (eq. 1), using the bias-corrected procedure of Calonico et al. (2014). See main text for details. Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Dashed lines are 95% confidence intervals from robust bias-corrected standard errors clustered by country. Outcomes residualized on time (month-year) effect.



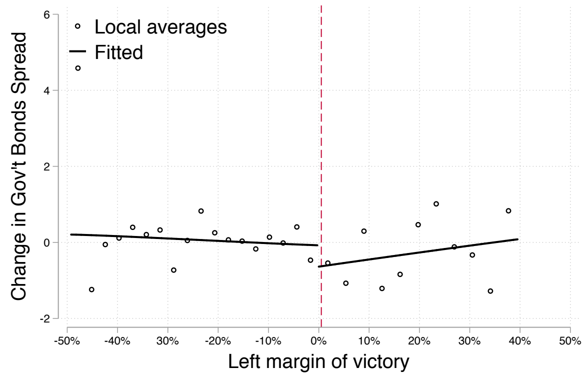
(a) Share Prices



(b) Exchange Rates



(c) Gov't bonds: real yield



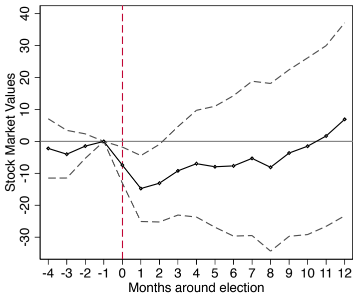
(d) Gov't bonds: spread vs. US

Figure D.2: Effect of a left-wing electoral victory on financial markets – abnormal returns (Regression-discontinuity estimates; monthly data)

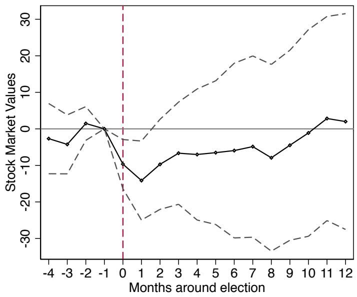
The vertical axis displays the percentage change in the outcome between time $t-1$ *and time* $t+1$*, where* t *is the election month. Time-effects previously filtered-out through a regression of the outcome on month-year dummies. The horizontal axis displays the Left's margin of victory: the margin of the left-wing candidate in presidential systems; the left share of parliamentary seats minus* 50% *in legislative systems. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression, with mean squared error-optimal bandwidth. The graphs correspond to eq. 1, with* $h = 1$.

D.3 Additional robustness and falsification tests

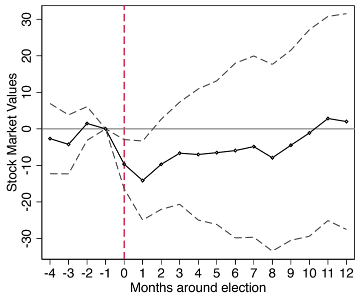
1.Raw returns



(a) Nominal average share prices

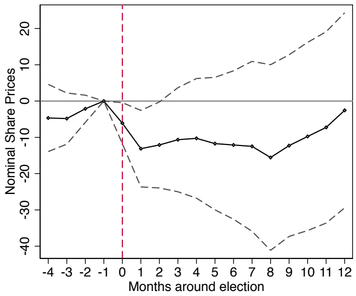


(b) Nominal end-of-month share prices

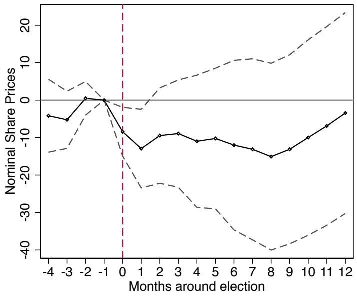


(c) Real end-of-month share prices

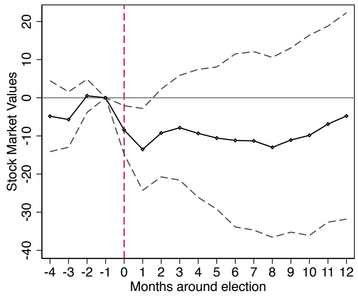
2.Abnormal returns



(d) Nominal average share prices



(e) Nominal end-of-month share prices



(f) Real end-of-month share prices

Figure D.3: Effect of a left-wing electoral victory, using alternative measures of share prices (Dynamic fuzzy RD estimates; monthly data)

Notes: Effect of a left victory. $t = 0$ *is the month of the election. See main text and Table 5 for estimations details.*

Table D.2: Effect of a left-wing electoral victory (fuzzy RD Estimates), excluding influential observations

Months	Stock market		Exchange rate		Real bond yields	
+1	-6.62**	-7.91***	-3.21*	-6.60***	-0.22	-0.40
	(2.69)	(2.85)	(1.83)	(2.04)	(0.45)	(0.52)
Excluded	21	16	14	9	9	9
+2	-13.72***	-5.59	-6.21**	-7.84***	0.05	-0.00
	(4.61)	(3.62)	(2.92)	(2.93)	(0.70)	(0.81)
Excluded	27	20	14	12	8	7
+6	-6.56	-9.49	-1.20	-5.80	0.78	0.90
	(7.13)	(7.04)	(4.78)	(4.16)	(1.05)	(1.26)
Excluded	26	22	8	9	7	8
Time FE		✓		✓		✓

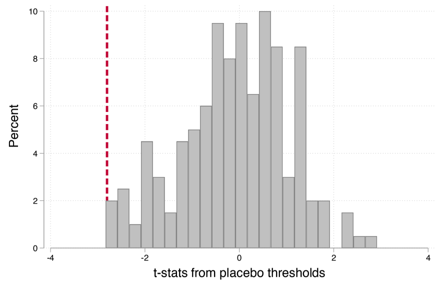
Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in the outcome between time $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). In each regression, we exclude the most influential observations, defined as those with $|DFBeta| > 2/\sqrt{N}$. The number of excluded observations is indicated in the 'Excluded' row. Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Robust bias-corrected standard errors clustered by country in parenthesis. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table D.3: Effect of a left-wing electoral victory, in the subsample in which all financial variables are simultaneously available (fuzzy RD Estimates; monthly data)

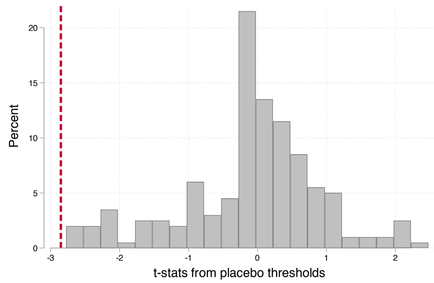
Months	Stock market		Exchange rate		Real bond yields	
+1	-3.27	-1.97	-11.32**	-2.37	-0.60	-1.10
	(5.83)	(5.12)	(5.37)	(2.34)	(1.14)	(0.97)
+2	-4.53	-10.91	-16.03**	-2.24	1.32	0.20
	(12.46)	(10.65)	(7.95)	(3.20)	(2.43)	(1.98)
+6	-19.85	-13.42	-14.34	2.25	1.42	-0.14
	(24.66)	(15.71)	(13.69)	(7.16)	(3.69)	(3.21)
Time FE		✓		✓		✓
Obs	195	195	195	195	195	195
Eff. obs	90	81	106	96	96	88

Notes: each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in the outcome between time $t-1$ and time $t+h$ (t being the time of the election). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regression (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). We restrict the sample to those observations for which all financial variables are available. Coefficients multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). Robust bias-corrected standard errors clustered by country. The number of observations refers to the specification with time-horizon $h = 1$. Eff. obs is the number of observations within the MSE-optimal bandwidth. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

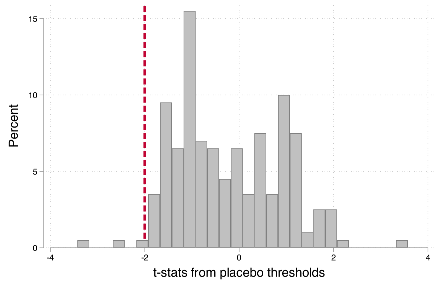
1. Raw returns



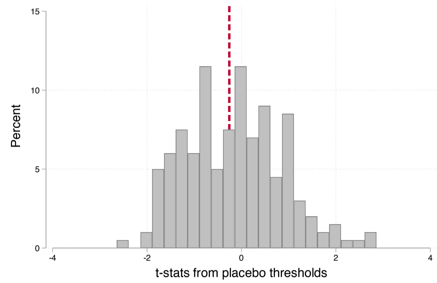
(a) Share prices (monthly)



(b) Share prices (daily)

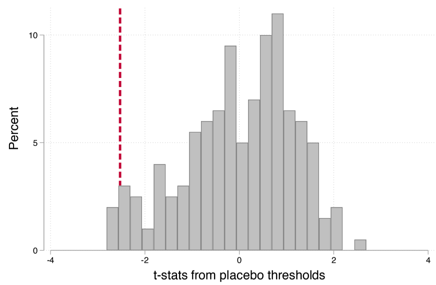


(c) Exchange rate

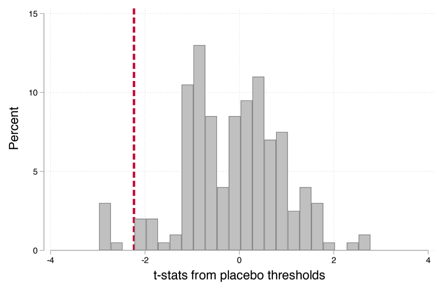


(d) Gov't bond yields

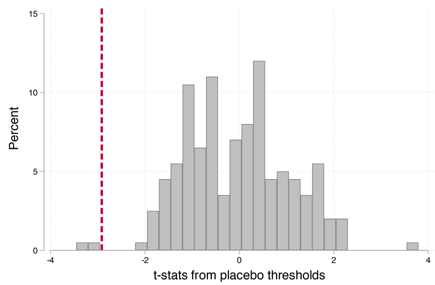
2. Abnormal returns



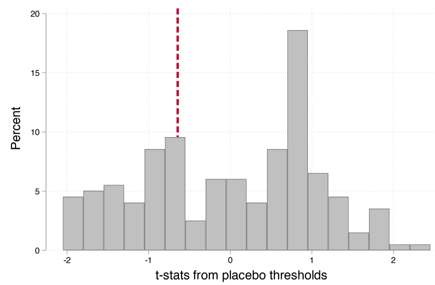
(e) Share prices (monthly)



(f) Share prices (daily)



(g) Exchange Rates

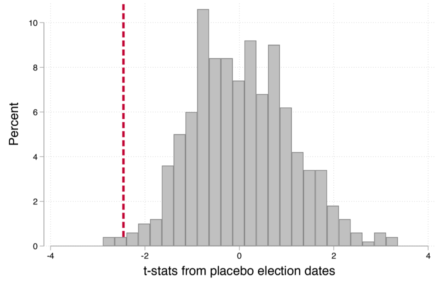


(h) Gov't bonds, real yields

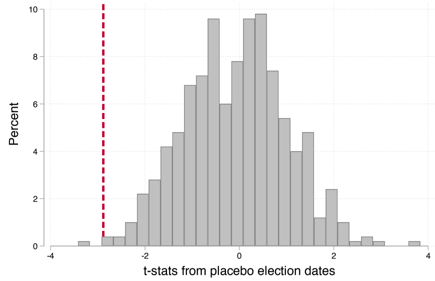
Figure D.4: Falsification test using placebo thresholds
(vertical red dotted line = estimate from true threshold)

Notes: Empirical distribution of t-statistics from our fuzzy regression discontinuity estimate of the treatment effect (eq. 1, with h=1), based on 200 randomly drawn placebo thresholds, drawn separately on the left and on the right side of the true threshold (100 on each side), using only observations belonging to that side, to avoid mis-specification arising from assuming continuity at the true threshold. Vertical red dotted line represents the t-statistics obtained from using the 'true' threshold. The t-stats from robust bias-corrected standard errors (Calonico et al., 2014) clustered by country.

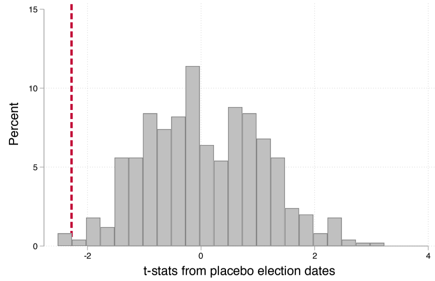
1. Raw returns



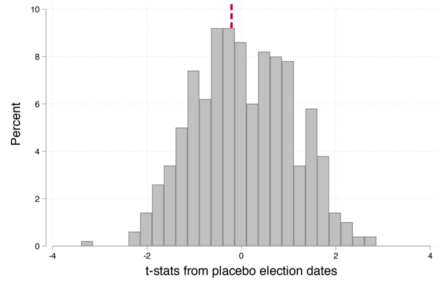
(a) Share prices (monthly)



(b) Share prices (daily)

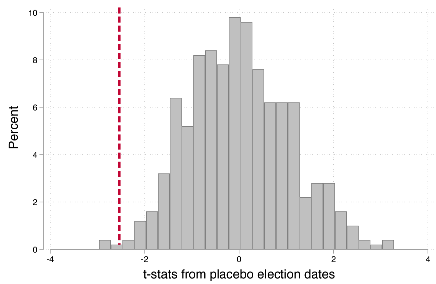


(c) Exchange rate

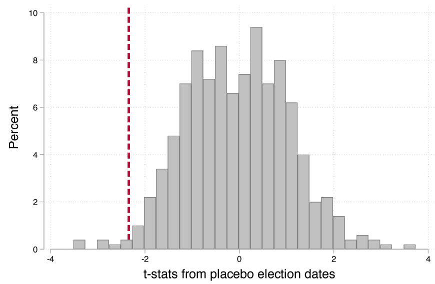


(d) Gov't bond yields

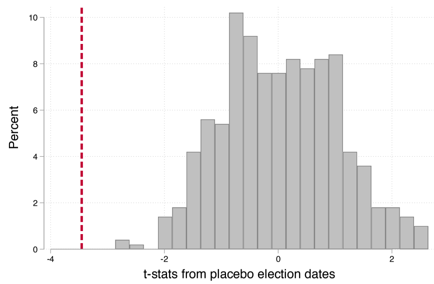
2. Abnormal returns



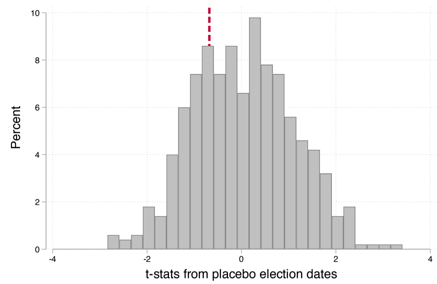
(e) Share prices (monthly)



(f) Share prices (daily)



(g) Exchange rate



(h) Gov't bond yields

Figure D.5: Falsification test using placebo election dates
(vertical red dotted line = estimate from true election dates)

Notes: Empirical distribution of t-statistics from our fuzzy regression discontinuity estimate of the treatment effect (eq. 1, with h=1), based on 500 sets of randomly drawn placebo election dates, which were substituted for the true election dates. In each graph, the vertical red dotted line represents the t-statistics obtained from using the 'true' threshold. The t-stats are obtained from robust bias-corrected standard errors (Calonico et al., 2014) clustered by country.