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Working Paper

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Working Paper, No. 2018-08

Provided in Cooperation with:

Department of Economics, University of Massachusetts

Suggested Citation: Girardi, Daniele (2018) : Political shocks and financial markets: Regression-discontinuity evidence from national elections, Working Paper, No. 2018-08, University of Massachusetts, Department of Economics, Amherst, MA

This Version is available at:

<https://hdl.handle.net/10419/202941>

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DEPARTMENT OF ECONOMICS

Working Paper

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

by

Daniele Girardi

Working Paper 2018-08



**UNIVERSITY OF MASSACHUSETTS
AMHERST**

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

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May 3, 2018

Latest version here: <https://umass.box.com/v/politicalshocks>

Abstract

Despite growing interest in the effect of political-institutional factors on the economy, causally identified evidence on the reaction of financial markets to electoral outcomes is still relatively scarce, due to the difficulty of isolating causal effects. This paper fills this gap: we estimate the ‘average treatment effect’ of left-wing (as opposed to conservative) electoral victories on share prices, exchange rates, and sovereign bond yields and spreads. Using a new dataset of worldwide national (parliamentary and presidential) elections in the post-WWII period, we obtain a sample of 954 elections 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 elections. 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, in developing economies, and in the post-1990 period.

1 Introduction

Many interpreted the stock market rally which followed the 2016 US Presidential election as a ‘Trump boom’ or, less optimistically, a ‘Trump bubble’ (Gandel, 2017; Krugman,

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I’m grateful to Michael Ash, Sam Bowles, Arin Dube, Ethan Kaplan, Peter Skott, Suresh Naidu, Fabio Petri, participants to the Fall 2015 Political economy Workshop at the University of Massachusetts Amherst and the 2016 Annual Conference of the International Association for Applied Econometrics (IAAE) for useful comments and suggestions on previous drafts of this paper. Excellent research assistance by Raphael Rocha Gouvea made this work possible. Any errors are of course my own.

2017; Schiller, 2018). Although Donald Trump’s presidency is widely thought to display highly idiosyncratic features, the ‘Trump Boom’ is far from being the only or the most dramatic example of a substantial financial markets’ 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 ‘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 954 elections in which main parties/candidates can be classified as either (center-)left or conservative, *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; Thistlewaite and Campbell, 1960). Intuitively, we compare elections closely won and closely lost by the Left. In presidential elections, the running variable in our RD design is the margin of victory/loss of the (center-)left’s candidate; in parliamentary elections, it is the share of parliamentary seats won by (center-)left parties minus 50%. We test whether the expected values of our financial

¹See 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.

outcomes of interest display a discontinuity at the cutoff which determines electoral victory. Identification is thus based on a ‘smoothness’ assumption, meaning that elections won by a close margin should tend to be quite similar in all respects, except for the color of the winning party/coalition. We implement our RD design through a dynamic specification, to uncover the dynamics of the impacts around our events of interest.

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 financial dynamics around left-wing electoral victories, relative to electoral losses, in all elections (left panel) and in close elections (right panel). 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 substantial reactions of stock and currency (but not government bond) markets.

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 markets is muted (baseline results are summarized in Figure 5). On average, a close Left victory causes real share prices to decrease by 5 to 6 percentage points in the short run (7 to 9 points in a presidential election). The fall is concentrated in the first trading day after the election, in which share prices tend to fall by 2 to 3 percentage points (3 to 5 points in presidential elections). The short-run negative effect on the US dollar value of the domestic currency amounts to around 7 percentage points.

Effects at longer time-horizons (6 to 12 months) are very dispersed, signaling large variability in medium-run outcomes across different experiences. With that caveat in mind, on average across all elections we observe (at least partial) reversal of these negative effects, which may indicate ‘overreaction’ to electoral shocks.

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 radical, in developing countries, and in the post-1990 period. We find no discernible

effect on government bond yields and spreads at any time-horizon in any of these subsamples.

The size of our estimates represents a lower bound for the size of the underlying effects of interest because of anticipation effects. Under the reasonable assumption that in the close elections that we use for identification the ex-ante probability of a left-wing victory perceived by investors is, on average, around 50%, the full underlying effects are roughly twice as large as our estimates.

Our results are confirmed by a number of robustness and falsification tests. We employ alternative criteria for selecting the bandwidth size in our regression-discontinuity specification. We perform falsification tests using placebo thresholds and placebo election dates. We also test whether our results are driven by a limited number of 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.² To the contrary, our results are consistent with models in which different parties pursue different macroeconomic policy goals (Alesina, 1987; Hibbs, 1986).

Perhaps most importantly, our results speak to the relation between capitalism and democracy. The reaction of capital holders to changes in the political context has been

²A recent influential work that provides evidence of policy differentiation is Lee et al. (2004).

seen by several authors 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; CORE Team, 2017, Unit 22). 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 and limited to few case studies.³

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 their 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 be lower by 1.7 percentage points after a Left victory.

Two recent articles have used close and unexpected electoral outcomes as case stud-

³We 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.

ies. 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. 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).⁴

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

⁴The 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.

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 regression-discontinuity design to study financial market effects at the national level. 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’.

3 Data and descriptive evidence

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 954 elections in which main parties/candidates can be classified on the left-right scale *and* data is available for at least one of our financial variables of interest.

3.1 Election results and partisanship

We build a new 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 on the left-right scale from a variety of sources. The resulting dataset includes 1,079 elections for which we are able to define the running variable to be used in our RD design, the (Center-)Left’s margin of victory/loss.

Parliamentary elections For parliamentary elections, our main source is the Manifesto Project Database (Volkens et al., 2017),⁵ which covers 715 parliamentary elections in 56 countries in the 1945-2015 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 seats won by Left and Center-Left parties, which we use as the running variable for parliamentary elections in our RD design (Sec. 4.1). We include in the (center-)left block parties classified by MPD as either ‘Socialist’, ‘Communist’, ‘Social-Democratic’ or ‘Ecologist’. We will also use the MPD policy positions estimates to distinguish between centrist ‘third-way’ left parties and socialist/social-democratic parties in our analysis of heterogeneous effects (Sec. 5.3).

We use the ideological coding provided by the Party Government Dataset (Woldendorp et al., 2011), the Comparative Political Parties Dataset (Swank, 2013) and the Database of Political Institutions (Cruz et al., 2016) to create a dummy variable for whether the government formed after a parliamentary election is led by the Left.⁶ We will use this measure to test the validity of the running variable employed in parliamentary elections, by assessing whether there is a discontinuity in the probability of a Left-led government at the 50% cutoff (Sec. 4.1, Figure 3).

Presidential elections Data on presidential elections is less readily available; we have assembled an original dataset which draws from a variety of sources. Election results (names of candidates, party affiliation and share of votes received) were collected from publicly available national and international sources, for all elections in the 1945-2018 period for which it was possible to find information. In elections determined by an electoral college system (e.g. US), we use electoral college vote shares instead of popular vote shares. We do not, however, include cases in which electoral college votes are expressed by the parliament (e.g. Bolivian elections in which no candidate achieves 50% of the popular vote).

We code presidential candidates as (center-)left or conservative based on existing

⁵Specifically, we use the 2017b version of the MPD, the latest available at the time of writing.

⁶For elections covered by all these three sources (or by any two of them), we define a government as left-wing only if all the available sources code it as such.

sources. For the Latin American elections covered by Baker and Greene (2011), we follow their classification on the left-right scale. For other elections, when the candidate is affiliated with a party that belongs to some worldwide association of political parties, we assign it the ideology of the association (left-wing for ‘Socialist International’, ‘Progressive Alliance’ and ‘Foro de Sao Paulo’; conservative for the ‘International Democrat Union’, the ‘Alliance of Conservatives and Reformists in Europe’, ‘European People’s Party’). When this does not apply, whenever Lansford (2014) provides a clear indication of the position of a candidate (or his/her party) on the left-right scale, we follow that indication. Finally, in cases for which Lansford (ibid.) does not discuss ideology, we use other existing national and international publicly available sources (published books and scholarly articles) to classify the main candidates on the left-right scale whenever possible. Our dataset reports the source of the classification for each of the three most-voted candidates in each election.

Overall sample of elections We exclude presidential elections in parliamentary systems, in which by definition the president does not have substantial executive power (e.g. Austria). When a parliamentary and a presidential election occur in the same month under a presidential system (for example in USA or Chile), we include only the presidential election. The classification of the political system applying to each election (parliamentary, assembly-elected president, presidential) is taken from Cruz et al. (2016), Bormann and Golder (2013), Woldendorp et al. (2011) and Lindberg (2006).

The resulting dataset includes 2,487 elections from 227 countries; of these 1,614 are parliamentary and 873 are presidential elections.⁷ For 1,079 of these elections (423 presidential and 656 parliamentary), we are able to classify the main parties/candidates as either (center-)left or conservative based on existing international and national sources following the procedure outlined above, and therefore to build our running variable, the margin of victory/loss of the Left. For 954 of these elections (621 parliamentary and 333 presidential), data on at least one of our financial outcomes of interest is available.

⁷As explained, this count does not include presidential elections under parliamentary systems, nor parliamentary elections which take place in the same month of a presidential election under a presidential system. Counting also those, the total number of elections in our dataset is 2,874 (929 of which presidential, 1,945 parliamentary).

Descriptive statistics for these elections, which are the ones that we use in estimation, are presented in Table 1(a).

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 substantial number of elections in our sample, in addition to the monthly one. Our main sources are Global Financial Data (GFD) 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 data. Appendix X indicates the stock market index considered for each country, and the source from which we obtained it (when not GFD). We deflate monthly stock market indexes with the Consumer Price Index.⁸

The US dollar value of the domestic currency (our measure of exchange rates) is taken from the monthly dataset of Reinhart (ibid.). For observations that are missing in the latter, but available in the Bank of International Settlement exchange rates database, we use the latter.⁹

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.

To identify country-years with fixed/pegged exchange rates, which therefore cannot be used in estimating exchange rate effects, we use the monthly exchange rate regimes classification provided by Ilzetzki et al. (2017). For country-years for which the latter is not available, we use the yearly classification provided by Klein and Shambaugh (2010).

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

⁸GFD 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.

⁹BIS exchange data were downloaded from <https://www.bis.org/statistics/xrusd.htm> in April 2018. Reinhart (2016) and BIS data provide identical series for all the country-years that are available in both sources.

3.3 Descriptive evidence: average financial dynamics around (close and non-close) Left electoral victories

Figure 2 plots the average dynamics of our financial outcomes of interest around Left electoral victories, relative to Left electoral losses.¹⁰ This descriptive evidence illustrates the challenges associated with estimating the effects of electoral outcomes, and how our identification strategy deals with them. Figures on the left include all elections, while figures on the right include only close elections, defined as elections in which the margin of victory/loss of the Left is not greater than 5%.

The graphs using all elections display little or no reaction of financial markets to electoral outcomes, leading a researcher using a ‘naive’ approach to find little sign of any effect. The graphs using only close electoral outcomes, which are likely to be largely unanticipated and independent of economic conditions, suggest, to the contrary, negative effects on stock and currency markets, while confirming the indication of no effect on Government bond spreads. In the remainder of this paper we build on this intuition and estimate these effects using a dynamic regression-discontinuity approach, broadly confirming the indications of this simple exercise.¹¹

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; Thistlewaite and Campbell, 1960). We implement our RD design through a dynamic specification, to uncover the dynamics of the effects around our events of interest.

Our regression-discontinuity approach achieves causal identification by focusing on

¹⁰Consistent with our RD design, here we consider a parliamentary election as won by the Left if (center-)left parties win at least 50% of parliamentary seats. Of course a presidential election is won by the left if the (center-)left candidate is elected president.

¹¹RD estimates, however, will point to larger exchange rate effects than suggested by the descriptive evidence in Figure 2. Intuitively, the main difference between this exercise and our RD estimations is that here we take unconditional averages and give the same weight to all elections, while the RD estimations will condition on the running variable and give higher weight to elections that are closer to the cutoff. Moreover, in Figure 2 the threshold for inclusion in the sample of close elections (5%) is arbitrary, while the RD estimations will employ optimal bandwidth-selection criteria from the literature.

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 around the threshold. Under this assumption, our RD estimator is able to isolate causal effects – including anticipated effects – and avoid selection bias. Besides being able to identify anticipation effects, the focus on close election is likely to substantially reduce them, as long as close elections are less likely to be largely anticipated than elections won by a large margin.

Our RD approach does have limitations. If investors are able to anticipate with almost certainty close electoral victories, we would find little post-election effects also in the presence of substantial sensitivity to electoral outcomes. If they expect a large Left victory with very high probability, and the result is a close Left victory, they may take this as a surprise in favor of conservative parties if a close victory provides less power than a large one.¹² While perfect anticipation of close electoral outcomes seems unrealistic, this second case, although unlikely to happen on average, is likely to occur in some instances. Two things, however, should be noted. First, also in these cases our RD approach would be able to identify pre-election (anticipated) effects. Second, even confining attention to post-election effects, reassuringly these cases would bias our estimates towards zero, *not* towards finding non-existent effects.

4.1 Forcing variable in presidential and parliamentary elections

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 defined as the margin of victory/loss of the left-wing candidate.¹³ In parliamentary elections, it is calculated as the share of parliamentary seats won by left and center-left parties minus 50%.

While for presidential elections the determination of the forcing variable is rather obvious, for legislative elections it is not: often it is not easy to determine who wins an

¹²Same holds, of course, for a closer-than-expected conservative victory.

¹³When elections are decided in a run-off, we consider only the run-off, not the first round.

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. (2017) as ‘Socialist’, ‘Communist’, ‘Social Democratic’ or ‘Ecologist’ win a majority of parliamentary seats. Of course this definition is not perfect: in some elections left-wing parties can be divided and not willing to form a coalition; in some elections they may be allied with centrist parties, therefore winning power even if our running variable does not reach the threshold. However, as we show below, in most parliamentary elections this definition of a left-wing victory works reasonably well and – most importantly – does generate a discontinuity at the threshold.

It is possible to test empirically whether our running variable for parliamentary elections does provide a discontinuity in the allocation of political power at the cutoff, which can be exploited in estimation. To do this, we assess whether the probability that a Left-led cabinet is formed after the election displays a discontinuity when the share of parliamentary seats won by (center-)left parties crosses the 50% cutoff. Figure 3 displays the result of this test: at the cutoff, the probability of a Left-led government jumps discontinuously and significantly. 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 28 percentage points and significant at the 0.05 significance level. Of course, the ability of forming a Left-led government is not the only possible channel through which a left majority in parliament can influence policy: also when they do not unite to form a government, (center-)left parties may come together on single issues in the legislative process. However, what this result demonstrates is that the share of seats won by (center-)left parties can be used as the running variable for parliamentary elections, given that it does provide a discontinuity in the political power of left parties, that can be used in estimation.

We also test for a discontinuity in the distribution of the forcing variable at the cutoff using the McCrary (2008) test. Such a discontinuity, if significant, may signal the possibility of systematic manipulation of electoral results, which may undermine the RD identifying assumption. The McCrary test finds no significant evidence of manipulation

in parliamentary nor in presidential elections.¹⁴

4.2 Estimation method: dynamic RD 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 RD specification:

$$\Delta y_{i,e,t+h} = \alpha_h + \gamma_h Z_{i,e} + f^h(x_{i,e}) + \epsilon_{i,e,t+h} \quad \text{for } h = -m, \dots, 0, \dots, n \quad (1)$$

$\Delta y_{i,e,t+h}$ is the logarithmic change in the outcome of interest between time $t-1$ and $t+h$ [$\ln(Y)_{i,e,t+h} - \ln(Y)_{i,e,t-1}$]; x is the forcing variable: the margin of victory/loss of the left-wing candidate in presidential systems, the left's share of parliamentary seats minus 50% in parliamentary systems; Z is an indicator equal to 1 if $x \geq 0$ and 0 otherwise; $f()$ is a potentially non-linear function that we approximate through kernel-weighted local linear regression;¹⁵

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. That is why we estimate time fixed-effects in the

¹⁴The discontinuity in the distribution of the margin of victory of the left at the cutoff, estimated through the McCrary (2008) test, is -0.18 with a standard error of 0.20 in parliamentary election, and 0.23 with a standard error of 0.21 in parliamentary election.

¹⁵We employ a triangular kernel. Results are robust to using a rectangular kernel.

whole sample of financial data (including also observations without elections), and then plug the residuals into equation 1.

5 Results

We use the dynamic regression-discontinuity design described by eq.1 to estimate our effects of interest in a time-window around elections. Results indicate that left-wing electoral victories tend to cause stock market values to decrease and the domestic currency to depreciate, in both cases by substantial amounts. Effects at longer time-horizons (6 to 12 months) are characterized by very high dispersion. However, on average across all elections, there is sign of reversal in both variables. Both stock market and exchange rate effects appear stronger and more persistent in presidential systems, in elections in which the Left’s proposed economic policy is more radical, and in the post-1990 period. We don’t find any discernible impacts on Government bond yields at any time-horizon and in any subsample.

5.1 Visual RD 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. Setting $h = 1$ means that we are looking at the 2-months average return between the month before and the month after the election. This is shown in Figure 4, using monthly data on raw returns and including all (parliamentary and presidential) elections. The depicted flexible regression lines are estimated using kernel-weighted local linear regression, with bandwidth selected according to the MSE-criterion.¹⁶

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.

¹⁶As in all baseline estimations presented here, we calculate the MSE-optimal bandwidth using the procedure in Calonico et al. (2014).

5.2 Dynamic RD estimations

To appreciate size, significance and dynamics of the effects, we estimate a set of RD regressions following equation 1, letting h (the time-window) vary from -6 months 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.¹⁷

Figure 5 plots dynamic RD estimates and 95% confidence intervals using monthly data and raw returns in the whole sample (presidential and parliamentary elections). Tables 2 to 4 report results (with h from -6 to +6 months) for all elections, as well as for parliamentary and presidential elections taken separately. For each sample, the Tables report both estimates using raw returns and those using abnormal returns (that is, controlling for common time effects).¹⁸

We find a sizable and significant negative short-term effect on stock market valuations and the US dollar value of the domestic currency. Share prices decrease by 5 to 6 percentage points between the month before the election and the month after. The stock market effect is stronger for presidential elections (7 to 9 percentage points). The exchange rate effect is more gradual. At a 3-months horizon, the effect is around 7.5 percentage points in all elections, with no substantial difference between parliamentary and presidential systems.

Longer-run (6 to 12 months) effects 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) ranges from negative effects as large as -11% to positive effects as large as 18% ; 1-year exchange rate effects range approximately from -15% to $+9\%$;¹⁹ This large dispersion, of course, suggests great caution in commenting average effects. With that caveat in

¹⁷We implement the Calonico et al. (2014) robust bias-corrected estimator using the `rdrobust` package in Stata (Calonico et al., 2017).

¹⁸For space reasons, in the main text we present only the Figures using raw returns; Figures using abnormal returns are reported in Appendix A.

¹⁹We refer to the specification using raw returns here, but similar variability is observed in abnormal returns (Tables 2 and 3).

mind, on average we observe reversal of the negative stock market effect in the whole sample, but not in the sample of presidential elections (Figure 5(a) and Table 2) and partial reversal of the exchange rate effect in the whole sample, although not in the two subsamples.²⁰

We find no effect at any time horizon on Government bond yields and spreads. Panels (c) and (d) of Figure 5 show that the effect is flat and near zero at all time-horizons, and Table 4 confirms that this holds both in presidential and parliamentary elections, on raw yields and controlling for common time effects.

We are able to estimate stock market effects also at a daily frequency for a large enough number of elections (although not for all elections for which we have monthly data). Daily-frequency effects are displayed in Figure 6 and Table 5. We do find some anticipation effects in the days leading to the election – positive and significant coefficients at some pre-election time-horizons, which imply negative changes between day $-h$ and the day of the election – but only in parliamentary elections. However, the bulk of the stock market effect happens in the first trading day after the election, when share prices fall on average by more than 2 percentage points (5 to 6 points in presidential elections).

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).

Presidential vs. parliamentary elections As illustrated above (Section 5.2), estimated stock market effects are substantially higher in presidential systems (Tables 5, 2, 3) than in parliamentary ones. This is likely to reflect, at least in part, our identification

²⁰This is possible because the optimal bandwidth varies across specifications. Therefore, the observations that fall within the optimal bandwidth in the specification using the whole sample are not equal to the union of the observations that fall within the optimal bandwidth in the two subsamples of parliamentary and presidential elections taken separately.

strategy, which focuses on close elections. While we have shown that there is substantial discontinuity in the treatment at the cutoff in parliamentary elections (Figure 3), this discontinuity is certainly stronger for presidential elections. For a parliamentary coalition a close majority is a different outcome from a large one, and controlling almost 50% of the seats is different from controlling (for example) 20% (and may even allow a minority government). In presidential elections, to the contrary, a close victory gives to the winner the same powers as a landslide victory. We thus interpret this result as reflecting our identification strategy, rather than underlying differences caused by different electoral systems (which we cannot exclude nor confirm).

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 policy position estimates of Volkens et al. (2017), and in particular their variables `planeco` and `markeco`. The variable `planeco` measures support for market regulation, economic planning and government control of the economy; `markeco` measures support for a ‘free market economy’ and for a smaller role of the state. We use the difference (`planeco` – `markeco`) as a proxy for economic ideology. We divide elections in two subsamples based on whether the economic ideology of the major Left party (measured by `planeco` – `markeco`) is above or below its median value in the sample. We refer to the first group as elections characterized by a ‘Neoliberal Left’, and to the second group as ‘Interventionist Left’ elections.

In this test we focus only on parliamentary systems, given that Volkens et al. (ibid.) policy position estimates are available only for parliamentary elections. Although imposed by data limitations, the focus on parliamentary elections is beneficial, in that it ensures that results are not contaminated by heterogeneity based on political systems (as discussed above).

Table 6 displays results from this exercise. As expected, the negative stock-market and exchange-rate effects of left-wing electoral victories are stronger and much more persistent in elections in which the Left’s proposed economic policy is more radical.

Stock market effects in the two subsamples have the same direction but different intensity in the short-run, and they diverge even in sign in the medium run. The 1-month

stock-market decrease is stronger by around one percentage point in ‘Interventionist Left’ elections; the 3-months effect is negative but close to zero and non significant for ‘Neoliberal Left’ elections, but significant and substantial (around 9%) in ‘Interventionist Left’ elections. The one-year effect is actually positive and substantial for ‘Neoliberal Left’ elections, negative and substantial for ‘Interventionist Left’ elections – although in both cases very imprecisely estimated and not statistically significant.

The picture is similar – and even more striking – for exchange rate dynamics. The smaller number of observations available (mainly due to the exclusion of elections held under fixed/pegged exchange rate systems), however, suggests caution in interpreting exchange rate results: there are relatively few effective (i.e., within the bandwidth) observations in each subsample. With this caveat in mind, also exchange rate effects are milder and temporary in the ‘Neoliberal Left’ subsample, larger and persistent in the ‘Interventionist Left’ subsample. The 1-year effect in the ‘Interventionist Left’ subsample is remarkably large (around 30%) and significant, although it has to be taken with great caution due to the low number of observations.

Time-varying effects Second, we test whether the effects were stronger in earlier elections or in more recent (post-1990) ones.²¹ Results are reported in Table 7. We find both stock market and exchange rate effects to be stronger in the post-1990 period. This may reflect higher capital mobility in the more recent period. We exclude the 1970 Chile election, which had an extraordinarily high effect on stock market valuations (Girardi and Bowles, 2018), from this test, but the result that effects are stronger post-1990 is robust to including it.

Cross-country heterogeneity Third, we test for different effects in industrialized vs. developing countries. We use OECD membership in 1970 as the (time-invariant) criterion for identifying high-income economies.²² Results are reported in Table 8. Both stock market and exchange rate effects are stronger in non-OECD countries. Also in

²¹We 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.

²²This includes Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany (West Germany pre-1989), Greece, Iceland, Ireland, Italy, Japan, Luxemburg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States.

this test we exclude the Chile 1970 election (Chile was not part of the OECD in 1970, so including this election would strengthen the result of larger effects in developing countries).

6 Robustness and falsification tests

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

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 9. 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 insensitive to the specific bandwidth selection criterion employed.

6.2 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, and suggest that our results may be spurious.

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 50 observations in each side within the bandwidth, to avoid biasing our test against significant findings because of weak statistical power.

Results are reported in Figure 7, which plots the distribution of t-statistics for each financial outcome of interest. Reassuringly, the t-statistics from our baseline estimation at the true threshold (indicated by the vertical dashed lines) are in the tails of the distribution of placebo t-stats, and there is little evidence of a tendency to find significant discontinuities away from the threshold. The ‘true’ t-statistics for the effect on bond yields and spreads are instead confirmed to be non-significant relative to the placebo distribution.²³

6.3 Placebo election dates

As a second falsification test, we try substituting placebo election dates for the true election dates. We estimate equation 1, again setting $h = 1$. We shift our election dates backwards by 36 to 0 months. For a country which has a (true) election e at time t , using the same notation as in eq. 1, we estimate:

$$\Delta y_{i,e,t-s+1} = \alpha_s + \gamma_s Z_{i,e} + f^s(x_{i,e}) + \epsilon_{i,e,t-s+1} \quad \text{for } s = 36, \dots, 0 \quad (2)$$

Figure 8 plots t-statistics for the placebo-effects against s (the number of months by which the election dates have been shifted). Panel (a) uses raw returns and (b) uses abnormal returns. Reassuringly, for both stock market and exchange rate effects, the only statistically significant coefficients (at the 0.05 significance level) are obtained at the true election dates ($s = -1$ and $s = 0$, which both contain true post-election observations), while the time-shifted placebos all produce non-significant results.

²³Figure 7 uses raw returns. Figure A.4 in Appendix A reports results from the same exercise using abnormal returns (ie, returns residualized on time effects).

6.4 Sensitivity to influential observations

To check if our results are 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.²⁴ Specifically, in each regression we exclude the observations with $|DFBeta| > 2/\sqrt{N}$.²⁵

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 10, and indicate that our results are robust to excluding influential observations. We detect many more influential observations in the estimates of the stock market effects (between 21 and 28) than in those of exchange rate effects (between 3 and 7). When excluding those observations, point estimates get smaller, however they remain statistically significant, with even an increase in precision in most cases, and economically relevant.

6.5 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

²⁴For simplicity, we calculate DFBeta coefficients using simple (unweighted) local linear regression within the optimal bandwidth and conventional standard errors, as an approximation to our kernel-weighted local linear regression estimator with robust standard errors. However, when re-estimating eq. 1 after excluding influential observations, we use kernel-weighted local regression and robust standard errors, as in the baseline specifications; so the estimates from this robustness test are directly comparable to those from the baseline specification.

²⁵Using $|DFBeta| > 2/\sqrt{N}$ as the cutoff for defining influential observations is recommended by Belsley et al. (2005 [1980]) and is standard in the literature.

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 RD 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 (147 elections). Notwithstanding the small size, it produces results qualitatively analogous to those found using all the available elections for each variable (results are reported in Table 11). We thus conclude that the different impact on share prices and exchange rates on the one hand, and bond yields on the other, is not driven by the use of partially different samples.

7 Discussion

A negative reaction of share prices and the domestic exchange rate to (center-)left electoral victories may reflect the expectation of policies 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 (Girardi and Bowles, 2018, 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 remarkable variability, which suggests great caution in discussing average medium-run outcomes. With this important caveat in mind, on average we observe (at least partial) reversal of the stock market and exchange rate effects, at least in high income economies and in elections in which the Left's economic policy platform is more free-market oriented. If true, this may imply potential arbitrage opportunities. A possible explanation would be that left-wing coalitions systematically surprise holders of capital after having gained power, by failing to deliver redistribution in favor of capital and/or higher inflation.²⁶ Reversal of the effects could also be seen as a manifestation of the well-documented phenomenon of 'overreaction', which leads to excessive pessimism over bad news and excessive optimism over good ones (Bondt and Thaler, 1985). It should also be noted that limits to arbitrage are substantial in this context: close left-wing electoral victories are relatively rare events, and each displays relevant heterogeneity.

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.

²⁶Of course, as we are measuring effects relative to the counter-factual of a close electoral loss of the Left, it may be conservative coalitions that systematically fail to deliver the expected increases in profitability.

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 5% to 6% in the month following the election (7% to 9% in presidential elections), and the domestic currency to depreciate by around 7% in three months. We have found no significant effects 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. With this caveat in mind, on average we find some evidence of reversal, which may imply 'overreaction' to electoral 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, in developing countries, and in the post-1990 period.

Of course, estimates exploiting close electoral outcomes are bound to underestimate the effects of interest: markets must have discounted some positive probability of a Left victory before the election. Given our focus on close elections, it would appear natural to assume a 50% ex-ante probability of a left-wing victory. Under this assumption, the full underlying effects would be twice as large as the estimate presented here.²⁷

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.

²⁷This calculation ignores risk-aversion, for the sake of simplicity and because of the lack of cross-country data on the average degree of risk aversion in the markets studied here.

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FIGURES

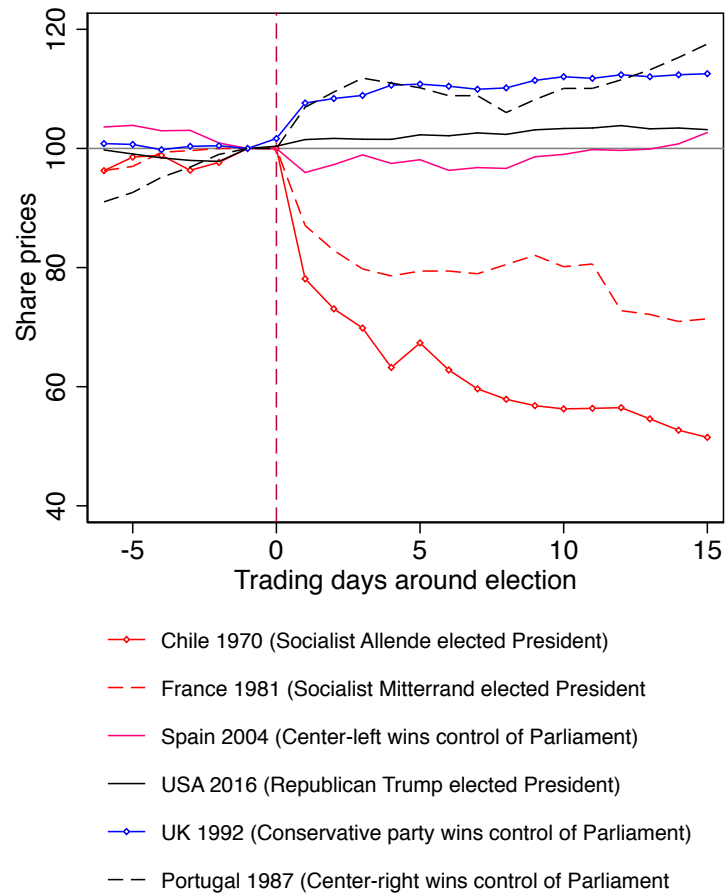


Figure 1: Share prices around selected elections

Share price index normalized to 100 in the day before the election on the vertical axis. Trading days around election on the horizontal axis.

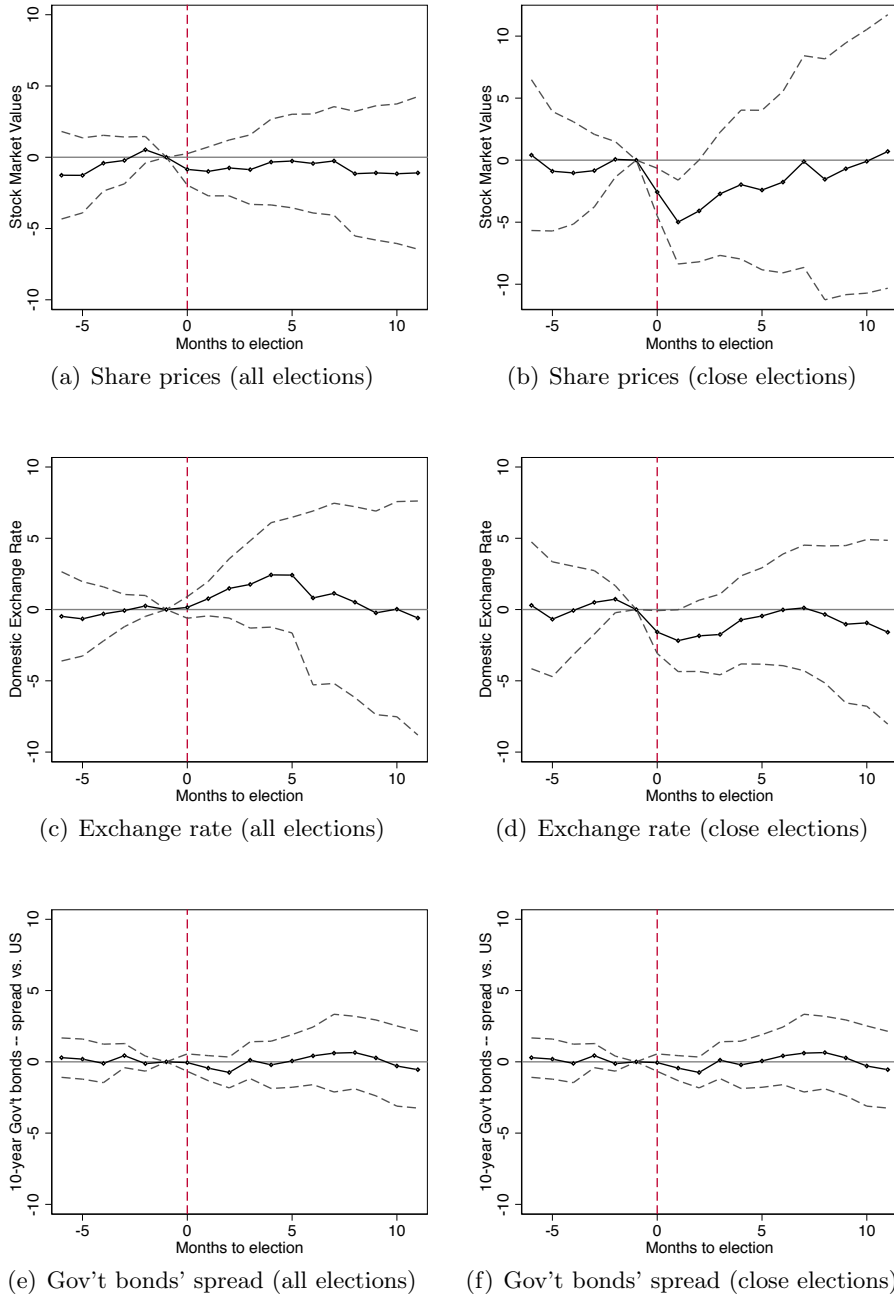


Figure 2: Descriptive evidence: financial dynamics around Left victories (monthly data)

The graphs plot average financial outcomes around (center-)left electoral victories, relative to electoral losses. Outcomes are normalized to zero in the month preceding the election. Months relative to the month of the election on the horizontal axis. Figures on the left include all available elections. Figures on the right include only elections in which the margin of victory/loss of the Left is not greater than $\pm 5\%$ (close elections). Dashed lines are 95% confidence intervals from robust standard errors clustered by country.

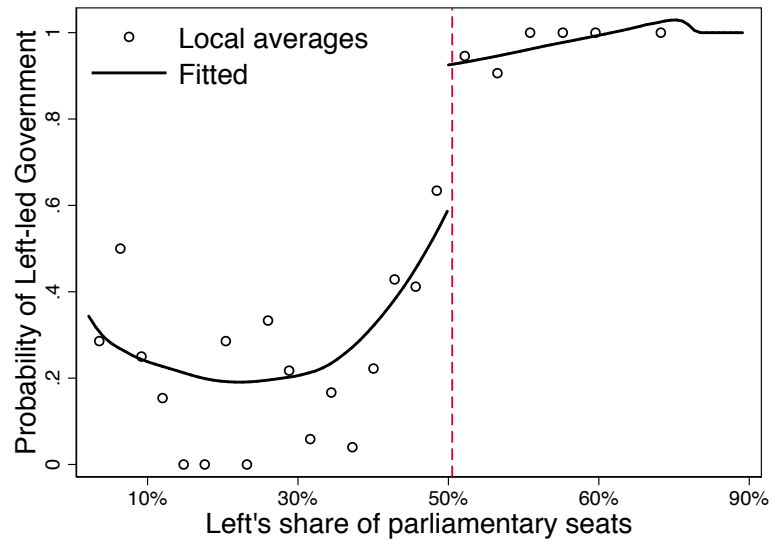
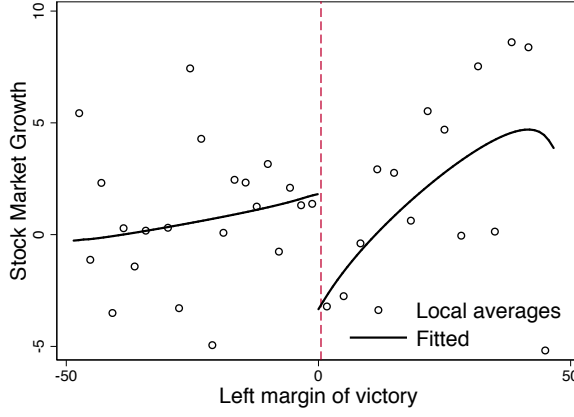
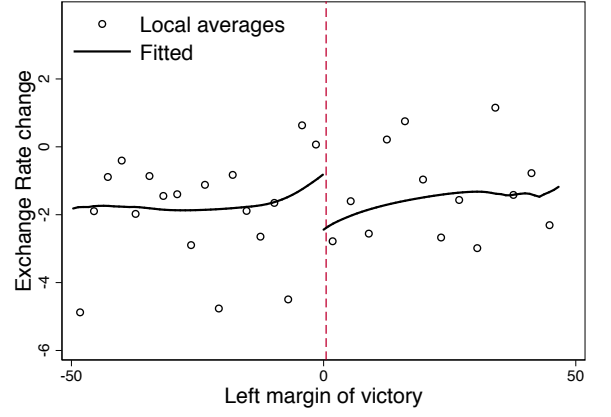


Figure 3: Parliamentary elections: share of seats won by the Left and probability of a Left-led Government (n=405)

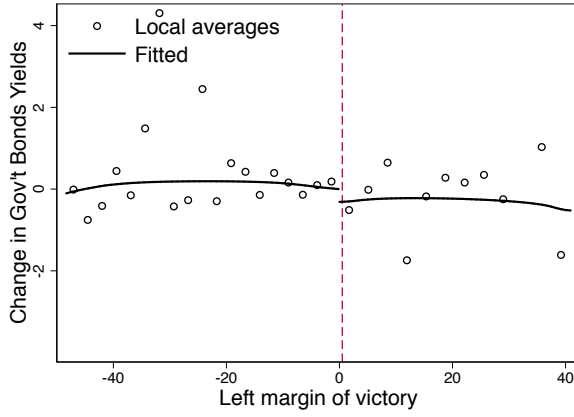
The graph displays the effect of left-wing parties winning a parliamentary majority on the probability that a Left-led Government is formed after the election. The vertical axis displays a dummy variable equal to 1 if a Left-led Government is formed after the election (sources indicated in the main text). The horizontal axis displays the share of parliamentary seats won by parties classified as ‘Socialist’, ‘Communist’, ‘Social-Democratic’ or ‘Ecologist’ by the Manifesto Project Database. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression, with mean squared error-optimal bandwidth.



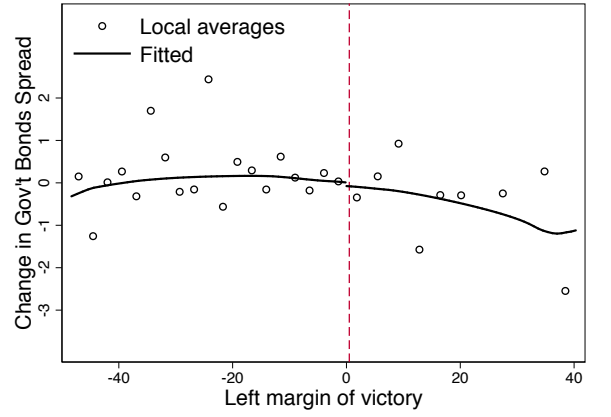
(a) Share Prices



(b) Exchange Rates



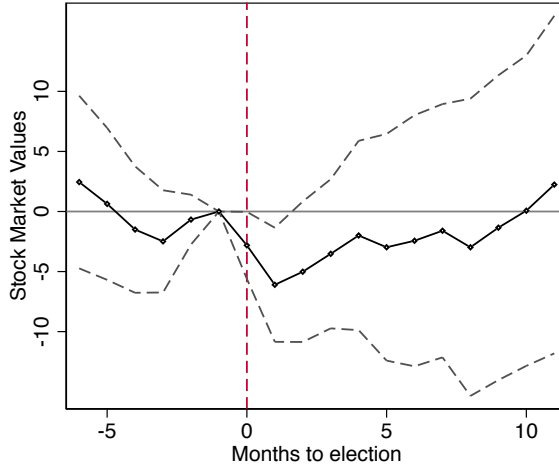
(c) Gov't bonds: real yield



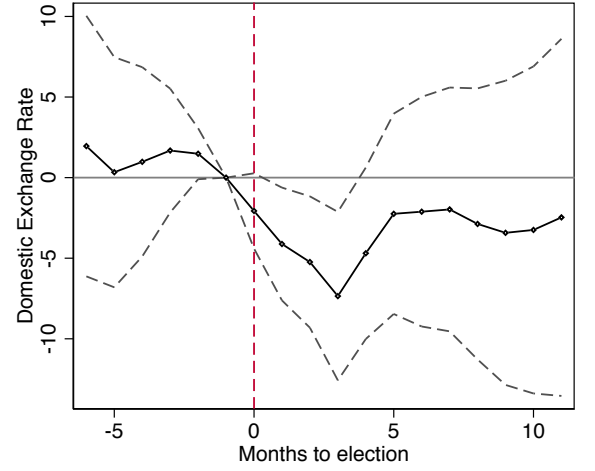
(d) Gov't bonds: spread vs. US

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

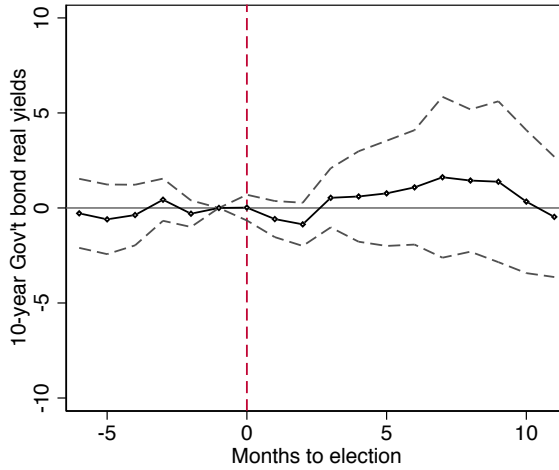
The vertical axis displays the percentage change in the outcome between time $t - 1$ and time $t + 1$, where t is the election month. The horizontal axis displays the Left's margin of victory: the margin of the left-wing candidate in presidential systems; the Left's share of parliamentary seats minus 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 $h = 1$.



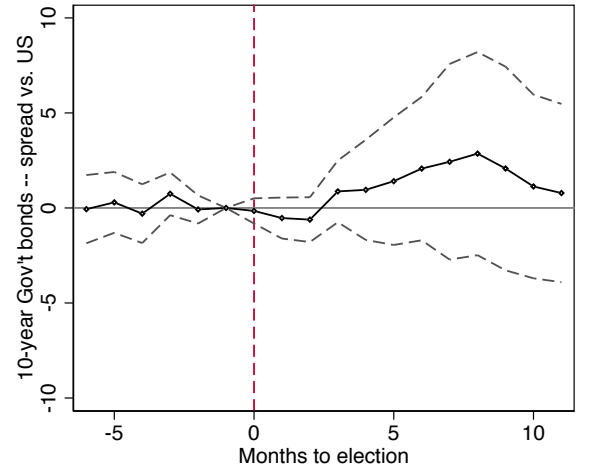
(a) Share Prices



(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
(Dynamic regression-discontinuity estimates; monthly data)

The graph displays the effect of a left-victory on the outcomes of interest in a time-window including 6 months before the election and 12 months after. Estimated through the dynamic regression-discontinuity specification of eq. 1, with h varying from -6 to +12 months. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regressions (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). Dashed lines are 95% confidence intervals from robust bias-corrected standard errors clustered by country.

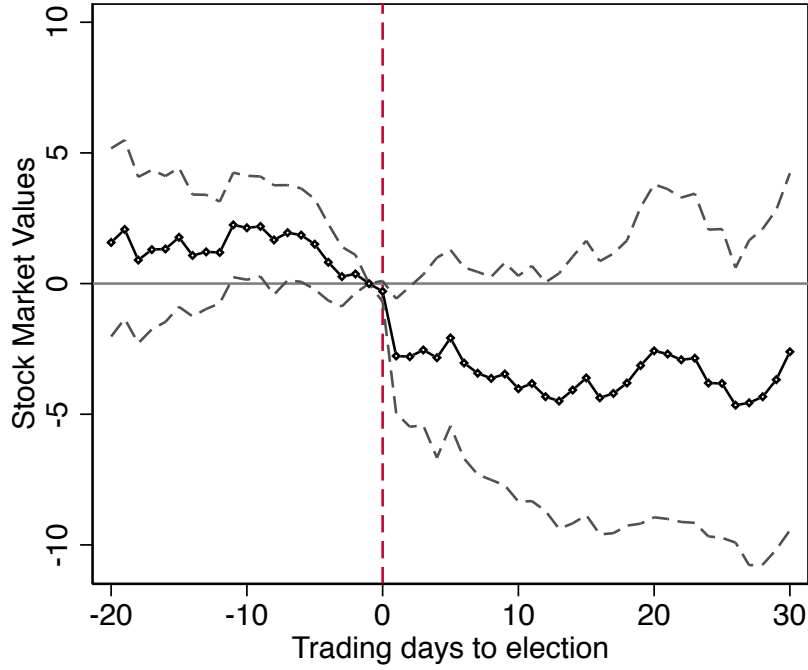


Figure 6: Effect of a left-wing electoral victory on stock market valuations
(Dynamic regression-discontinuity estimates; daily data)

The graph displays the effect of a left-victory on the outcomes of interest in a time-window including 20 days before the election and 30 trading days after. Estimated through the dynamic regression-discontinuity specification of eq. 1, with h varying from -20 to +30 trading days. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regressions (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). Dashed lines are 95% confidence intervals from robust bias-corrected standard errors clustered by country.

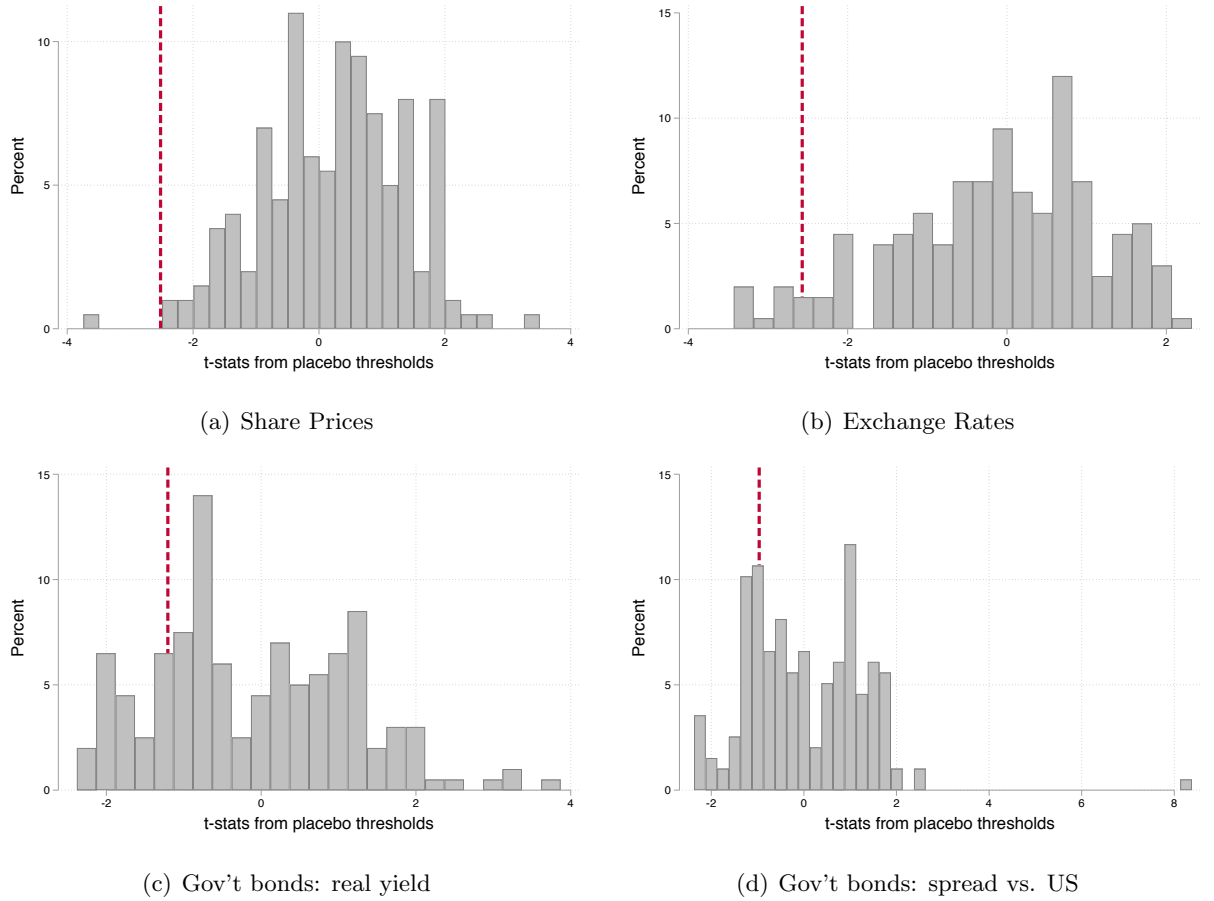
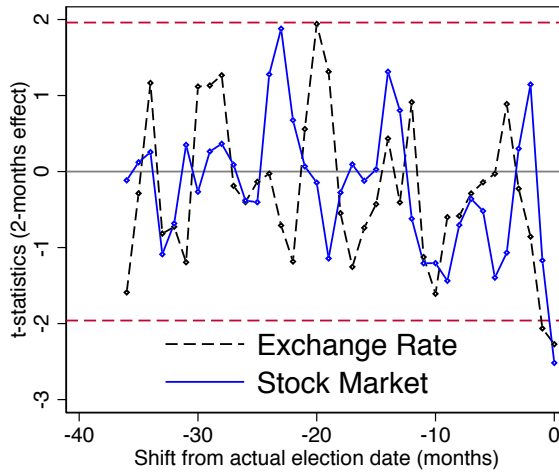
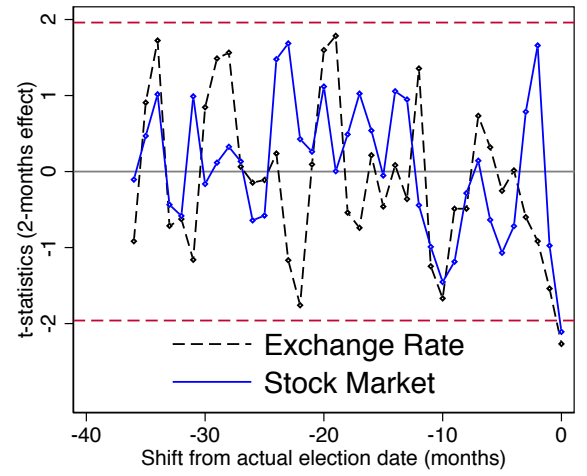


Figure 7: Falsification test with placebo thresholds
(vertical red dotted line = estimate from true threshold)

The figure reports the empirical distribution of t -statistics from our regression discontinuity estimate of the treatment effect (eq. 1, with $h=1$), based on 200 randomly drawn placebo thresholds. Placebo thresholds were 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. 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.



(a) Raw returns specification



(b) Abnormal returns specification

Figure 8: Falsification test with time-shifted placebo election dates

On the vertical axis, t -statistics for the 2-months effect of Left electoral victories (obtained from estimating equation 1, with $h=1$). On the horizontal axis, the number of months by which we have shifted the election dates backward (at -1 and 0 , estimates include true election dates; 0 corresponds to our baseline specification). The t -stats are obtained from robust bias-corrected standard errors (Calonico et al., 2014) clustered by country. In the abnormal returns specification (panel b), time-effects previously filtered-out through a regression of the outcome on month-year dummies.

TABLES

Table 1: Descriptive Statistics

	N	Mean	S.D.	Min	Max
(a) Elections data					
All elections					
Left margin of victory	954	-6.0	29.7	-95.2	100.0
Left victory	954	0.3	0.5	0.0	1.0
Left government	403	0.4	0.5	0.0	1.0
planeco	608	5.5	4.8	0.0	36.1
markeco	608	2.0	2.6	0.0	23.1
Econ. policy (planeco-markeco)	608	3.4	5.7	-23.1	36.1
Political system	676	1.4	0.9	0.0	2.0
Presidential elections					
Left margin of victory	333	5.6	42.1	-95.2	100.0
Left victory	333	0.6	0.5	0.0	1.0
Political system	126	0.0	0.0	0.0	0.0
Parliamentary elections					
Left margin of victory	621	-12.2	17.1	-50.0	46.6
Left victory	621	0.2	0.4	0.0	1.0
Left government	403	0.4	0.5	0.0	1.0
planeco	608	5.5	4.8	0.0	36.1
markeco	608	2.0	2.6	0.0	23.1
Econ. policy (planeco-markeco)	608	3.4	5.7	-23.1	36.1
Political system	550	1.7	0.7	0.0	2.0
(b) Financial data					
Monthly					
Real share prices (2010=100)	50,462	12,028.6	239,335.7	0.01	8,805,724
Exchange rate (USD per unit)	58,924	2.5e+11	5.8e+12	1.45e-11	1.5e+14
Real gov't bond yields	32,881	1.7	15.1	-776.6	159.3
Nominal gov't bond yields	33,364	7.9	7.0	-0.6	161.0
Nominal spread (vs. US)	32,485	2.8	6.9	-9.4	159.3
Real spread (vs. US)	31,986	-0.1	15.0	-766.8	159.3
Fixed/pegged exchange rate	80,190	0.3	0.4	0.0	1.0
Daily					
Share prices (2010=100)	713,278	119.2	2,722.7	0.01	444,298.4

Table 2: Effect of a left-wing electoral victory on stock market valuations (RD Estimates)
- Monthly Data

Outcome: log change in share prices between $t - 1$ and $t + h$						
h (months)	All elections		Presidential elections		Parliamentary elections	
-6	2.45 (3.67)	0.59 (2.88)	-3.36 (8.20)	-2.74 (6.66)	3.31 (4.04)	0.79 (3.17)
-5	0.64 (3.23)	-0.80 (2.50)	-2.63 (7.37)	-1.39 (5.97)	0.27 (3.69)	-1.44 (2.90)
-4	-1.50 (2.69)	-2.52 (2.11)	-3.71 (6.09)	-2.94 (5.16)	0.06 (2.80)	-1.59 (2.24)
-3	-2.49 (2.17)	-2.83* (1.71)	-3.53 (4.39)	-2.74 (3.85)	-0.81 (2.38)	-1.81 (1.79)
-2	-0.67 (1.06)	-1.00 (0.88)	-1.58 (2.17)	-1.42 (1.90)	-0.07 (1.12)	-0.45 (0.86)
-1	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)
0	-2.82** (1.42)	-2.24* (1.35)	-5.96* (3.06)	-4.76 (3.02)	-1.43 (1.57)	-1.35 (1.36)
+1	-6.10** (2.42)	-5.15** (2.44)	-9.18* (4.96)	-7.40* (4.44)	-5.20 (3.43)	-4.93* (2.94)
+2	-5.01* (2.98)	-4.34 (2.66)	-6.71 (5.51)	-4.46 (4.65)	-4.69 (3.97)	-4.50 (3.16)
+3	-3.52 (3.17)	-2.62 (3.10)	-6.37 (6.34)	-4.22 (6.01)	-2.37 (4.05)	-2.79 (3.53)
+4	-2.00 (4.02)	-2.31 (3.78)	-6.71 (8.05)	-4.06 (6.96)	-0.42 (4.52)	-2.45 (4.11)
+5	-2.98 (4.81)	-3.06 (4.12)	-9.36 (9.65)	-6.89 (8.46)	-1.81 (5.05)	-3.39 (4.49)
+6	-2.43 (5.33)	-2.96 (4.75)	-12.51 (11.55)	-9.59 (10.06)	-0.75 (5.63)	-3.13 (4.93)
+12	4.33 (7.31)	2.47 (6.47)	-13.80 (14.85)	-6.88 (10.55)	9.33 (8.36)	2.98 (7.56)
Time FE		✓		✓		✓
Obs	753	753	199	199	555	555
Eff. obs	445	462	115	121	284	292

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 $t + h$. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observations refers to the specification with time-horizon $h = 0$.

Table 3: Effect of a left-wing electoral victory on the domestic exchange rate (Dynamic RD Estimates – monthly data)

Outcome: log change in domestic exchange rate between $t - 1$ and $t + h$						
h (months)	All elections		Presidential elections		Parliamentary elections	
-6	1.96 (4.12)	3.15 (4.45)	-5.60 (4.39)	-2.28 (5.05)	10.61 (7.16)	6.12 (6.31)
-5	0.33 (3.64)	1.67 (3.58)	-1.79 (3.50)	1.94 (4.19)	2.09 (5.29)	2.13 (4.93)
-4	0.98 (3.00)	1.36 (2.89)	0.14 (2.64)	0.23 (3.49)	2.21 (4.58)	2.37 (4.09)
-3	1.68 (1.96)	1.73 (1.97)	3.53 (3.29)	3.73 (3.86)	1.63 (2.35)	1.52 (2.08)
-2	1.48* (0.81)	0.88 (0.86)	1.92 (1.47)	0.81 (1.72)	1.35 (0.86)	1.00 (0.82)
-1	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)
0	-2.07* (1.19)	-1.60 (1.18)	-3.48 (2.75)	-2.76 (2.61)	-2.57** (1.06)	-2.85*** (1.09)
+1	-4.12** (1.79)	-4.08** (1.80)	-3.16 (3.82)	-3.33 (3.44)	-4.54** (1.81)	-4.50** (1.92)
+2	-5.24** (2.08)	-5.20** (2.14)	-4.51 (4.40)	-5.11 (4.19)	-4.30** (2.06)	-3.54* (2.05)
+3	-7.35*** (2.67)	-7.68*** (2.83)	-5.69 (5.36)	-5.58 (5.45)	-6.10** (2.48)	-5.34** (2.58)
+4	-4.69* (2.71)	-4.84* (2.74)	-4.53 (5.65)	-1.92 (5.45)	-7.81*** (2.89)	-6.96** (2.88)
+5	-2.25 (3.17)	-2.12 (3.05)	-3.60 (6.83)	-0.85 (6.72)	-8.93** (3.95)	-7.68** (3.73)
+6	-2.11 (3.63)	-2.58 (3.49)	-3.75 (7.72)	-2.57 (7.45)	-9.12** (4.62)	-6.19 (4.19)
+12	-3.06 (6.13)	-6.00 (5.56)	-8.19 (10.39)	-7.81 (10.65)	-13.23 (8.22)	-10.67 (6.93)
Time FE		✓		✓		✓
Obs	607	607	258	258	350	350
Eff. obs.	315	339	102	113	112	108

Each row represents a separate regression and reports the effect of a left-wing electoral victory on the logarithmic change in the domestic exchange rate (US dollars per unit of domestic currency) between time $t - 1$ and time $t + h$. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observations refers to the specification with time-horizon $h = 0$.

Table 4: Effect of a left-wing electoral victory on 10-year Government bond yields and spreads (RD Estimates)

Outcome: change in bond yields (%) between $t - 1$ and $t + h$						
h (months)	Real yield (All elections)		Real Spread (All elections)		Nominal Spread (All elections)	
-6	-0.28 (0.93)	0.12 (0.89)	-0.06 (0.92)	0.19 (0.91)	0.13 (0.25)	-0.06 (0.19)
-5	-0.60 (0.93)	-0.39 (0.91)	0.29 (0.82)	-0.21 (0.94)	0.35 (0.25)	-0.05 (0.22)
-4	-0.37 (0.81)	0.03 (0.73)	-0.30 (0.79)	-0.46 (0.78)	0.48* (0.25)	0.34 (0.26)
-3	0.43 (0.57)	0.29 (0.51)	0.75 (0.57)	0.29 (0.51)	0.38 (0.25)	0.19 (0.24)
-2	-0.30 (0.36)	-0.30 (0.34)	-0.08 (0.38)	-0.31 (0.34)	-0.07 (0.19)	-0.13 (0.14)
-1	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)
0	0.02 (0.35)	-0.17 (0.33)	-0.16 (0.34)	-0.31 (0.30)	0.41* (0.21)	0.23 (0.20)
+1	-0.58 (0.48)	-0.89 (0.56)	-0.53 (0.55)	-1.04* (0.62)	0.31 (0.30)	0.05 (0.24)
+2	-0.86 (0.58)	-1.03 (0.72)	-0.61 (0.60)	-1.06 (0.70)	0.25 (0.23)	-0.09 (0.26)
+3	0.53 (0.79)	0.25 (0.90)	0.87 (0.83)	0.37 (0.96)	0.62 (0.38)	0.33 (0.38)
+4	0.60 (1.21)	0.33 (1.31)	0.95 (1.35)	0.34 (1.35)	0.12 (0.25)	-0.07 (0.21)
+5	0.77 (1.41)	0.32 (1.54)	1.41 (1.71)	0.41 (1.68)	0.10 (0.30)	-0.12 (0.24)
+6	1.08 (1.54)	0.63 (1.64)	2.07 (1.92)	0.89 (1.84)	0.14 (0.31)	0.01 (0.30)
+12	-0.27 (1.43)	-0.65 (1.40)	1.50 (2.43)	-0.30 (1.99)	-0.59 (0.37)	-0.60 (0.37)
Time FE		✓		✓		✓
Obs.	567	567	549	549	553	553
Eff. Obs.	350	310	258	249	253	259

Each row represents a separate regression and reports the effect of a left-wing electoral victory on the change in yields (or spreads) between time $t - 1$ and time $t + h$. Spreads are calculated as the difference between domestic and US government bonds yields. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observations refers to the specification with time-horizon $h = 0$.

Table 5: Effect of a left-wing electoral victory on stock market valuations (RD Estimates)
- Daily data

h (trading days)	Outcome: log change in share prices between $t - 1$ and $t + h$					
	All elections		Presidential elections		Parliamentary elections	
-6	1.85** (0.91)	0.79 (0.74)	-0.41 (1.53)	-0.18 (1.58)	3.05*** (1.08)	1.68* (0.96)
-5	1.51* (0.88)	0.50 (0.75)	-0.70 (1.64)	-0.54 (1.71)	2.69*** (0.69)	1.13 (0.75)
-4	0.82 (0.75)	0.17 (0.67)	-0.54 (1.57)	-0.57 (1.53)	1.69*** (0.59)	0.60 (0.61)
-3	0.27 (0.58)	-0.10 (0.51)	-1.03 (1.22)	-1.40 (1.23)	0.94* (0.53)	0.33 (0.45)
-2	0.37 (0.37)	-0.04 (0.35)	-0.69 (0.79)	-0.79 (0.74)	0.79** (0.35)	0.29 (0.31)
-1	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)	0.00 (.)
0	-0.30 (0.20)	-0.24 (0.23)	-0.61* (0.36)	-0.36 (0.36)	0.15* (0.08)	-0.01 (0.20)
+1	-2.77** (1.12)	-2.24** (1.07)	-4.68** (2.10)	-3.47* (1.98)	-2.23** (0.90)	-1.92*** (0.66)
+2	-2.80** (1.37)	-2.59** (1.18)	-6.69*** (2.56)	-5.09** (2.38)	-1.32 (1.01)	-1.74** (0.88)
+3	-2.54* (1.47)	-2.70** (1.31)	-6.35** (2.88)	-5.04* (2.76)	-0.65 (1.11)	-1.26 (0.87)
+4	-2.84 (1.95)	-3.29** (1.64)	-8.05** (3.63)	-6.75* (3.47)	-0.66 (1.14)	-1.59 (1.04)
+5	-2.08 (1.72)	-2.95** (1.33)	-6.11* (3.29)	-5.07* (3.02)	-0.73 (1.23)	-1.83* (1.02)
+6	-3.04 (1.87)	-3.13* (1.71)	-7.08* (3.73)	-6.01* (3.46)	-0.89 (1.44)	-2.25* (1.21)
+7	-3.43* (1.98)	-3.24* (1.83)	-6.38 (4.17)	-5.12 (3.87)	-1.21 (1.45)	-1.15 (1.21)
+8	-3.63* (1.98)	-3.08 (1.94)	-6.41 (4.42)	-5.48 (4.08)	-1.58 (1.48)	-1.11 (1.24)
+9	-3.46 (2.17)	-2.78 (1.97)	-6.25 (4.83)	-5.82 (4.29)	-1.94 (1.49)	-2.65** (1.28)
+10	-4.02* (2.20)	-3.12 (2.05)	-5.86 (5.01)	-5.45 (4.38)	-2.68* (1.57)	-2.85** (1.36)
Time FE		✓		✓		✓
Obs	519	519	149	149	419	419
Eff. Obs.	272	268	106	99	88	189

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 $t + h$. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observations refers to the specification with time-horizon $h = 0$.

Table 6: Heterogeneous effect of left-wing electoral victories based on Left’s policy positions (RD Estimates; parliamentary elections)

h (months)	Stock market				Exchange rate			
	Neoliberal Left		Interventionist Left		Neoliberal Left		Interventionist Left	
+1	-4.36 (5.37)	-4.27 (4.69)	-5.70** (2.66)	-5.12** (2.00)	-3.98** (1.70)	-3.23 (2.16)	-5.40* (2.81)	-6.76** (2.63)
+3	-0.39 (6.05)	-0.36 (5.06)	-8.48* (4.79)	-9.67** (4.06)	-2.16 (2.97)	-1.99 (1.92)	-10.43*** (3.52)	-5.02* (2.91)
+6	0.02 (7.39)	0.45 (6.01)	-7.89 (7.60)	-8.50 (6.99)	4.21 (4.13)	1.62 (4.28)	-18.04** (7.41)	-8.46 (5.17)
+12	14.08 (11.07)	13.73 (9.70)	-9.00 (10.77)	-13.11* (7.72)	2.76 (7.14)	6.04 (6.62)	-32.19** (15.06)	-30.18** (13.49)
Obs	285	285	286	286	180	180	181	181
Eff. obs	155	179	125	153	96	84	48	48
Time FE		✓		✓		✓		✓

h (months)	Gov’t Bonds: Real Yields				Gov’t Bonds: Spread vs. US			
	Neoliberal Left		Interventionist Left		Neoliberal Left		Interventionist Left	
+1	0.48 (0.43)	0.50 (0.37)	-0.64 (0.40)	-0.36 (0.70)	1.92** (0.79)	0.67 (0.57)	0.11 (0.45)	-0.17 (0.95)
+3	2.42 (2.03)	2.07 (1.82)	-0.73 (0.51)	-0.67 (0.89)	3.36 (2.39)	2.49 (2.19)	-0.83 (0.78)	-0.49 (0.71)
+6	4.68 (4.82)	4.10 (4.20)	-0.42 (1.15)	2.08 (1.91)	6.69 (6.21)	5.76 (5.51)	1.74 (1.42)	1.91 (1.65)
+12	5.86 (6.59)	4.83 (4.90)	-2.14 (1.96)	1.04 (2.15)	8.80 (8.14)	7.01 (6.26)	3.79* (2.25)	1.51 (2.61)
Obs	235	235	229	229	225	225	222	222
Eff. Obs.	90	85	66	52	52	54	68	50
Time FE		✓		✓		✓		✓

Each row represents a separate regression and reports the effect of a left-wing electoral victory on the logarithmic change in the outcome between time $t - 1$ and time $t + h$. The ‘Neoliberal 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., 2017). Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observations refers to the specification with time-horizon $h = 1$.

Table 7: Heterogeneous effect of left-wing electoral victories: pre and post 1990 (RD Estimates; parliamentary and presidential elections)

h (months)	Stock market				Exchange rate			
	pre-1990		post-1990		pre-1990		post-1990	
+1	-3.38 (3.81)	-2.70 (3.58)	-5.96** (2.70)	-5.89*** (2.25)	-2.99** (1.48)	-2.55 (1.76)	-6.47** (2.80)	-5.87** (2.28)
+3	-0.39 (4.91)	-0.52 (4.26)	-2.21 (4.09)	-3.57 (3.92)	-3.06 (2.81)	-3.50 (3.12)	-10.52*** (4.00)	-10.55** (4.24)
+6	4.93 (6.38)	2.99 (5.61)	-8.76 (7.18)	-12.13* (6.35)	-2.60 (4.93)	-3.87 (5.03)	-6.58 (6.55)	-5.02 (5.98)
+12	10.49 (9.86)	8.89 (8.73)	-2.38 (11.76)	-8.88 (8.78)	3.31 (6.62)	1.20 (6.69)	-13.68 (11.48)	-13.34 (9.87)
Obs	337	337	414	414	237	237	366	366
Eff.obs	235	235	186	190	92	96	174	198
Time FE	✓		✓		✓		✓	

h (months)	Gov't Bonds: Real Yields				Gov't Bonds: Spread vs. US			
	pre-1990		post-1990		pre-1990		post-1990	
+1	-0.80* (0.48)	-0.31 (0.47)	-0.76 (0.96)	-1.29 (1.01)	1.03* (0.58)	0.16 (0.56)	-0.60 (1.00)	-1.41 (1.07)
+3	2.38 (2.07)	2.17 (1.91)	0.87 (1.37)	0.26 (1.35)	3.32 (2.59)	2.42 (2.42)	1.00 (1.40)	0.10 (1.32)
+6	2.77 (2.90)	5.46 (4.75)	-0.35 (2.67)	-0.09 (2.68)	6.87 (6.09)	7.61 (6.89)	0.33 (2.83)	0.16 (2.66)
+12	2.80 (4.14)	2.86 (4.24)	-1.00 (2.32)	-1.08 (2.37)	12.45 (10.09)	8.45 (8.05)	-1.00 (2.38)	-1.11 (2.43)
Obs	269	269	297	297	258	258	290	290
Eff.obs	132	113	197	185	77	88	194	174
Time FE	✓		✓		✓		✓	

Each row represents a separate regression and reports the effect of a left-wing electoral victory on the logarithmic change in the outcome between time $t - 1$ and time $t + h$. The Chile 1970 presidential election, which had an extraordinarily large effect on stock market valuations (Girardi and Bowles, 2018), was excluded. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observations refers to the specification with time-horizon $h = 1$.

Table 8: Heterogeneous effect of left-wing electoral victories based on OECD membership in 1970 (RD Estimates; parliamentary and presidential elections)

h (months)	Stock market				Exchange rate			
	OECD (1970)		non-OECD (1970)		OECD (1970)		non-OECD (1970)	
+1	-4.85 (3.36)	-2.84 (2.45)	-6.12* (3.34)	-5.60* (3.03)	-2.52** (1.15)	-2.75 (1.69)	-6.69* (3.67)	-6.11* (3.18)
+3	-3.78 (4.17)	-1.75 (3.48)	-0.64 (6.05)	-1.49 (5.66)	-1.50 (1.67)	-2.82 (1.97)	-13.69** (5.34)	-12.45** (5.46)
+6	-1.32 (5.85)	-1.72 (4.77)	-10.04 (10.78)	-10.84 (9.10)	2.11 (2.71)	-2.30 (4.21)	-8.38 (7.69)	-5.61 (6.60)
+12	9.31 (8.47)	5.52 (7.54)	-12.28 (15.95)	-8.24 (10.68)	1.94 (4.79)	-4.02 (5.66)	-12.16 (12.50)	-10.72 (10.19)
Obs	478	478	273	273	268	268	335	335
Eff. obs.	299	380	122	122	133	106	118	121
Time FE		✓		✓		✓		✓

h (months)	Gov't Bonds: Real Yields				Gov't Bonds: Spread vs. US			
	OECD (1970)		non-OECD (1970)		OECD (1970)		non-OECD (1970)	
+1	-0.61 (0.37)	-0.65 (0.43)	-1.27 (2.34)	-1.65 (2.15)	0.21 (0.34)	-0.64 (0.44)	-1.36 (2.32)	-1.66 (2.15)
+3	0.19 (0.76)	0.36 (0.99)	4.08 (4.38)	3.04 (4.35)	0.69 (0.91)	0.51 (1.06)	3.58 (4.21)	3.03 (4.36)
+6	0.01 (0.98)	0.39 (1.52)	-0.12 (8.49)	-0.15 (8.57)	2.33 (2.19)	1.26 (2.04)	0.27 (8.57)	-0.16 (8.57)
+12	0.21 (1.56)	0.30 (2.06)	-5.46 (7.35)	-6.32 (7.53)	2.72 (3.06)	0.63 (2.39)	-6.14 (7.29)	-6.32 (7.54)
Obs	456	456	110	110	438	438	110	110
EffObs	216	234	63	71	168	221	66	71
Time FE		✓		✓		✓		✓

Each row represents a separate regression and reports the effect of a left-wing electoral victory on the logarithmic change in the outcome between time $t - 1$ and time $t + h$. OECD (1970) indicates the group of countries that were OECD members in 1970 (Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany (West Germany pre-1989), Greece, Iceland, Ireland, Italy, Japan, Luxemburg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States). Non-OECD (1970) indicates all other countries. The Chile 1970 presidential election, which had an extraordinarily large effect on stock market valuations (Girardi and Bowles, 2018), was excluded. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observations refers to the specification with time-horizon $h = 1$.

Table 9: Effect of a left-wing electoral victory (RD Estimates)
Robustness to alternative bandwidth selection criteria

h (months)	MSE (CCT)		MSE (IK)	CER (CCT)	
	(common)	(two)	(common)	(common)	(two)
Share Prices					
+1	−5.15** (2.44)	−5.05** (2.48)	−5.23** (2.62)	−5.30** (2.52)	−5.04** (2.56)
+3	−2.62 (3.10)	−2.89 (3.08)	−4.10 (3.21)	−3.04 (3.07)	−3.23 (3.08)
+6	−2.96 (4.75)	−3.50 (4.69)	−5.77 (4.92)	−3.87 (4.78)	−4.26 (4.71)
Domestic exchange rate					
+1	−4.08* (1.80)	−3.92 * * (1.68)	−5.66*** (2.15)	−5.11*** (1.91)	−4.36** (1.80)
+3	−7.68*** (2.83)	−5.95** (2.70)	−7.67*** (2.93)	−7.81*** (2.97)	−4.54** (2.22)
+6	−2.58 (3.49)	−2.77 (3.68)	−6.02 (3.68)	−6.30* (3.76)	−5.21 (3.98)
Government bond yields					
+1	−0.89 (0.56)	−1.01* (0.59)	−0.65 (0.48)	−0.99* (0.58)	−0.94 (0.58)
+3	0.25 (0.90)	0.40 (0.93)	0.57 (0.96)	0.32 (0.91)	0.41 (0.92)
+6	0.63 (1.64)	0.59 (1.60)	2.50 (3.11)	0.80 (1.75)	0.75 (1.71)
Government bond spreads					
+1	−1.04* (0.62)	−1.04* (0.62)	−0.70 (0.51)	−0.99* (0.60)	−0.91 (0.60)
+3	0.37 (0.96)	0.32 (0.93)	0.58 (0.99)	0.35 (0.95)	0.48 (0.93)
+6	0.89 (1.84)	0.81 (1.81)	2.55 (3.11)	1.07 (1.98)	1.03 (1.95)
Time FE	✓	✓	✓	✓	✓

Each row represents a separate regression and reports the effect of a left-wing electoral victory on the logarithmic change in the outcome between time $t - 1$ and time $t + h$. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). For each time-horizon considered, we estimate eq.1 through kernel-weighted local linear regressions (triangular kernel), using the bias-corrected procedure of Calonico et al. (2014). 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 in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Effect of a left-wing electoral victory (RD Estimates), excluding influential observations

h (months)	Stock market		Exchange rate	
+1	-3.83*** (1.29)	-3.07** (1.21)	-2.71*** (1.01)	-2.67** (1.18)
Excl. obs.	26	24	5	7
+3	-2.97 (2.37)	-0.52 (2.03)	-7.60*** (2.53)	-2.80 (1.77)
Excl. obs.	24	21	3	5
+6	-1.55 (3.79)	-1.06 (2.83)	-0.98 (2.59)	-1.61 (2.90)
Excl. obs.	26	28	3	3
Time FE	✓		✓	

Each row represents a separate regression and reports the effect of a left-wing electoral victory on the logarithmic change in the outcome between time $t - 1$ and time $t + h$. 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 ‘Excl. obs.’ row. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Robust bias-corrected standard errors clustered by country in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Effect of a left-wing electoral victory, using only elections for which all financial variables are simultaneously available (RD Estimates; monthly data)

h (months)	Stock market		Exchange rate		Gov't bonds yield		Gov't bonds spread	
+1	-5.40 (3.61)	-3.82 (2.88)	-10.26* (5.66)	-7.09* (4.11)	-0.34 (0.73)	-0.44 (0.56)	0.09 (0.64)	-0.44 (0.56)
+3	-6.14 (7.33)	-9.54 (7.52)	-9.21 (6.14)	-5.76 (4.65)	-0.68 (0.95)	-0.45 (0.67)	-0.04 (1.10)	-0.45 (0.67)
+6	-16.83 (13.62)	-11.84 (8.14)	-4.78 (7.32)	-1.73 (5.25)	-0.18 (1.05)	0.03 (0.72)	-0.15 (1.06)	0.03 (0.72)
Obs	147	147	147	147	147	147	147	147
Eff. obs	68	68	85	91	110	114	103	114
Time FE		✓		✓		✓		✓

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

Appendix A. Graphs using abnormal returns

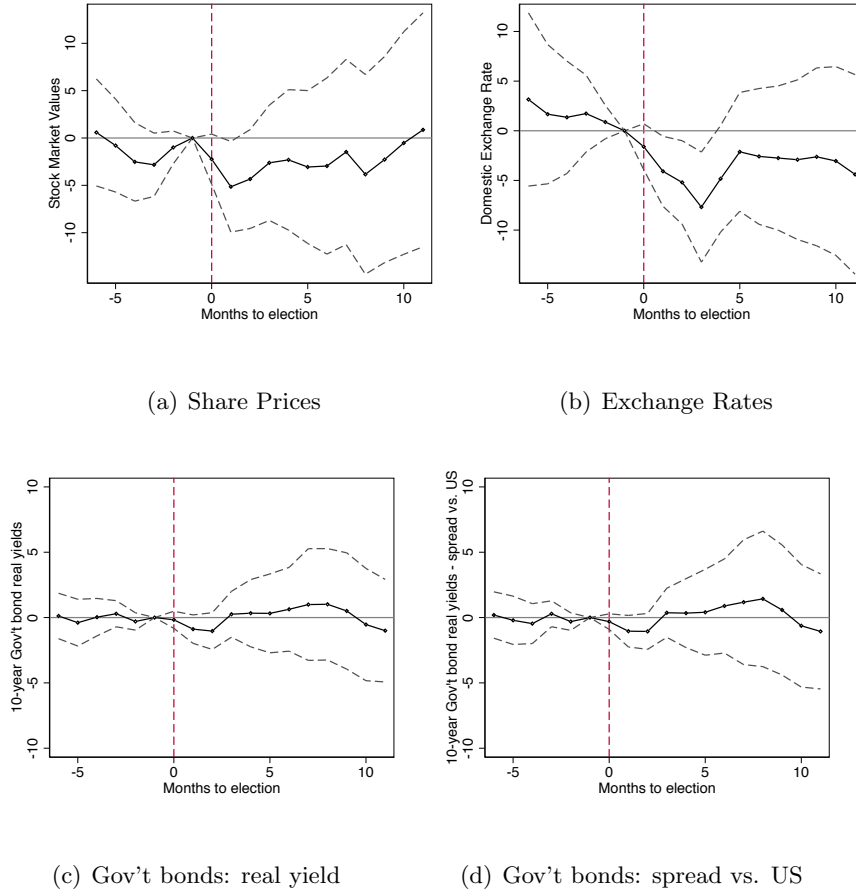


Figure A.1: Effect of a left-wing electoral victory on financial markets (abnormal returns)
(Dynamic regression-discontinuity estimates; monthly data)

The graph displays the effect of a left-victory on the outcomes of interest in a time-window including 6 months before the election and 12 months after. Estimated through the dynamic regression-discontinuity specification of eq. 1, with h varying from -6 to +12 months. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Dashed lines are 95% confidence intervals from robust bias-corrected standard errors clustered by country. Time-effects previously filtered-out through a regression of the outcome on month-year dummies.

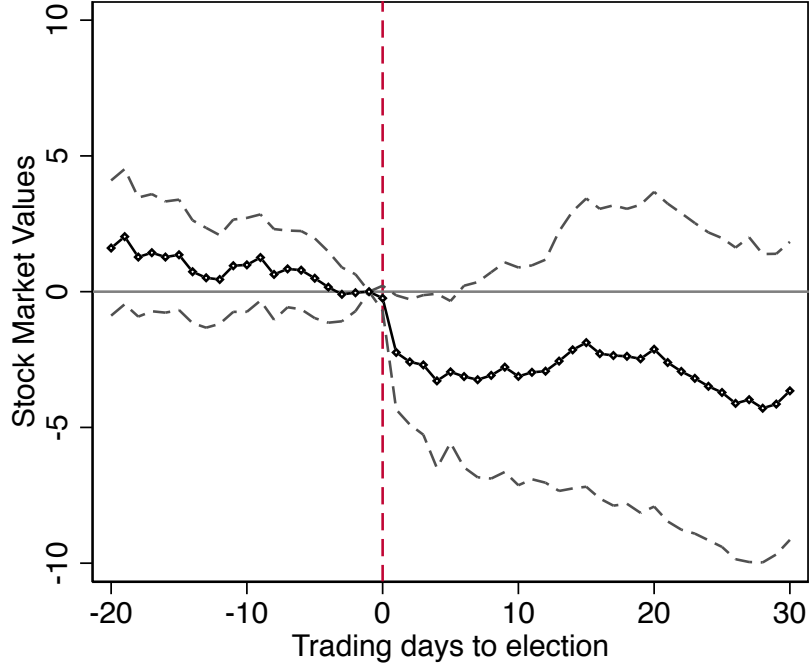
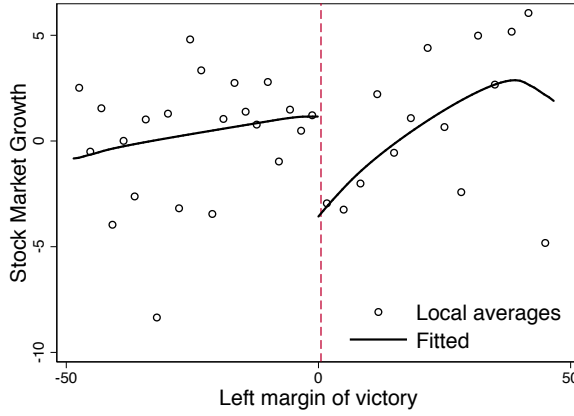
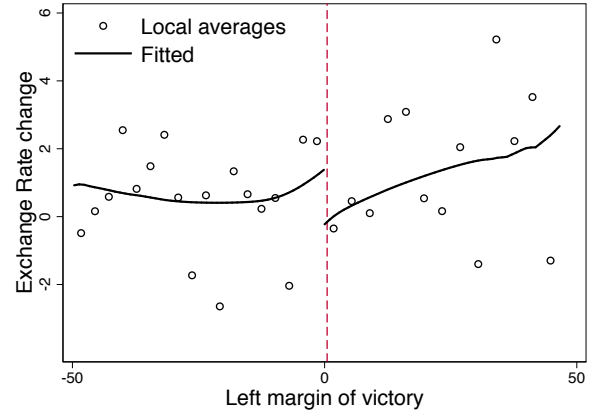


Figure A.2: Effect of a left-wing electoral victory on stock market abnormal returns
(Dynamic regression-discontinuity estimates; daily data)

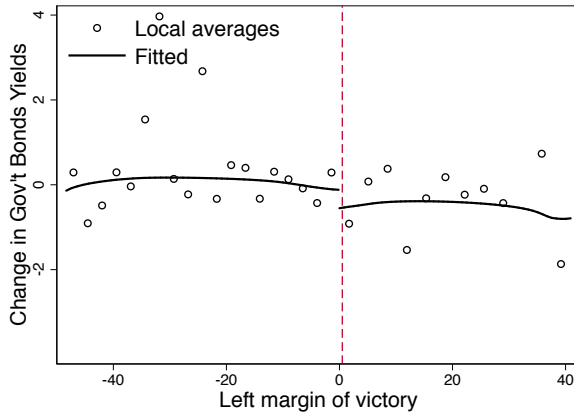
The graph displays the effect of a left-victory on the outcomes of interest in a time-window including 20 days before the election and 30 trading days after. Estimated through the dynamic regression-discontinuity specification of eq. 1, with h varying from -20 to +30 trading days. Coefficients are multiplied by 100 for ease of interpretation (so a coefficient of 1 means a 1% increase in the variable). 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). Dashed lines are 95% confidence intervals from robust bias-corrected standard errors clustered by country. Time-effects previously filtered-out through a regression of the outcome on month-year dummies.



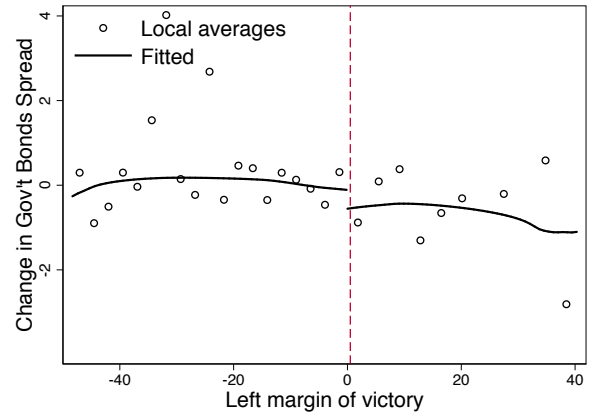
(a) Share Prices



(b) Exchange Rates



(c) Gov't bonds: real yield



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

Figure A.3: 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 $h = 1$.

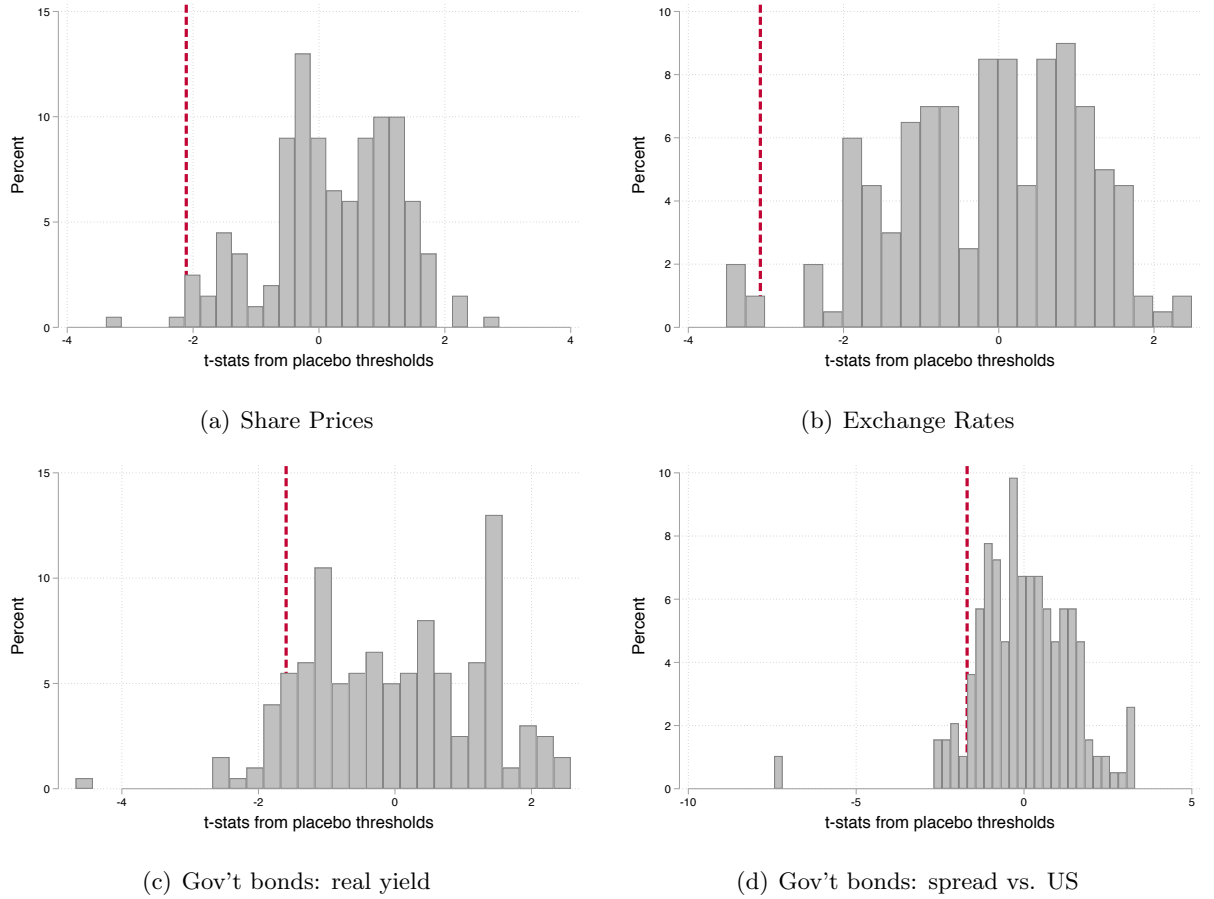


Figure A.4: Falsification test with placebo thresholds – Abnormal Returns
(vertical red dotted line = estimate from true threshold)

The figure reports the empirical distribution of t-statistics from our regression discontinuity estimate of the treatment effect (eq. 6, with $h=1$), based on 200 randomly drawn placebo thresholds. Placebo thresholds were 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. 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. Time-effects previously filtered-out through a regression of the outcome on month-year dummies.