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Does Easy Money Win Elections?

Felix Ward ¹

Florian van Genderen ²

Yao Chen ³

¹ Erasmus University Rotterdam

² Erasmus University Rotterdam

³ Erasmus University Rotterdam

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Yao Chen* Florian van Genderen[†] Felix Ward[‡]

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Abstract

Governments press central banks for lower interest rates on the premise that easy money wins elections. We test this premise for 385 elections in 18 countries, 1900–2020, using exogenous interest rate changes. We find no evidence that monetary easing helps incumbents. At the point estimate, a typical easing leaves the incumbent's vote share unchanged; the one-sided 95 percent confidence bound on the gain is 0.90 percentage points. Of 104 incumbent defeats, such an easing would have reversed none at the point estimate and 15 at the bound, all in close races. Easy money offers incumbents no reliable electoral return.

JEL classification: D72, E52, E58, F42, N10

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*Erasmus School of Economics, Erasmus University Rotterdam; CEPR; Tinbergen Institute; Email: y.chen@ese.eur.nl.

[†]Erasmus School of Economics, Erasmus University Rotterdam; Tinbergen Institute; Email: f.p.t.m.vangenderen@ese.eur.nl

[‡]Erasmus School of Economics, Erasmus University Rotterdam; Tinbergen Institute; Email: ward@ese.eur.nl.

1 Introduction

Governments have long pressed their central banks for lower interest rates. Lyndon Johnson confronted the Federal Reserve chairman, William McChesney Martin, over a rate increase in 1965 (Meltzer, 2009), and Richard Nixon pressed Martin’s successor, Arthur Burns, for easy money before the 1972 election (Abrams, 2006). Those who lost office have sometimes blamed their central bank. George H. W. Bush, defeated in 1992, later said of the Federal Reserve chairman: “Alan Greenspan disappointed me. I think if the interest rates had been lowered more dramatically, that I would have been re-elected president.”¹ Recently, the independence of the Federal Reserve has again come under strain, in public and before the courts: Donald Trump repeatedly demanded lower rates (Bianchi et al., 2023; Drechsel, forthcoming), and his attempt in 2025 to remove a Federal Reserve governor reached the Supreme Court, which blocked it in *Trump v. Cook* (2026).² Behind such episodes lies a premise that has never been tested directly: that easier money wins elections.

Politicians have repeatedly acted as if the premise were true, but economic theory gives reasons to doubt it. Central banks set interest rates on the advice of large staffs of professional economists, so a government that overrides them must do better than that judgment in the eyes of voters. And forward-looking voters have little reason to reward stimulus that will raise inflation once the election has passed (Alesina, 1987; Persson & Tabellini, 2000).³ Whether politicians or theory are right matters for central bank independence. If easy money wins elections, governments have a lasting incentive to press their central banks, even at a cost to the economy. If it does not, that incentive largely disappears.

This paper brings evidence to the question. Drawing on 385 parliamentary and presidential elections in 18 countries between 1900 and 2020, we estimate how exogenous changes in interest rates affect the vote share of incumbent governments. To our knowledge, this is the most comprehensive estimate to date of the electoral effects of easy money. Measuring the effect is hard. Central banks cut rates when the economy weakens, and a weak economy costs incumbents

¹Interview with David Frost, reported in the *Wall Street Journal* (1998).

²*Trump v. Cook*, No. 25A312 (U.S. June 29, 2026). In denying the government’s application to let the removal take effect while the litigation continues, the Court held that the for-cause protection of Federal Reserve governors is constitutional and that a governor is entitled to notice and an opportunity to respond before removal. It left open whether the President may remove Governor Cook for cause.

³Only if voters are myopic, as early political business cycle models assumed (Nordhaus, 1975), can easy money reliably buy votes.

votes (Fair, 1978; Lewis-Beck & Stegmaier, 2000; Nannestad & Paldam, 1994). A simple comparison of interest rates and election results therefore confounds the effect of monetary policy with that of the economic conditions to which it responds. Identifying the causal effect requires rate changes that are not driven by the domestic economy.

The trilemma of international finance supplies the right kind of variation (Jordà et al., 2020). A country that pegs its currency to a base country's and allows capital to move freely must match the base country's interest rate. Its rates then move with conditions abroad, not with its own economy or politics. Britain in 1992 is a case in point. The British Pound had joined the Exchange Rate Mechanism of the European Monetary System in October 1990, tying it to the Deutsche Mark. After German reunification, the Bundesbank kept its rates high to contain inflation in Germany. Britain, with output stagnant after a recession and unemployment still rising, could not set rates for its own conditions: the Bank of England held its base rate at 10.5 percent through the spring of 1992, because a further cut would have put sterling's parity at risk unless German rates fell first (Bank of England, 1992). It was under these imported rates that John Major's Conservative government faced the voters on 9 April 1992. The government was returned to office with almost the same vote share as five years earlier.⁴

This experience is typical. Across our sample, we find no evidence that easy money helps incumbents. A one percentage point increase in the short-term rate over the legislative term changes the incumbent's vote share by 0.01 percentage points, with a 95 percent confidence interval from -1.20 to 1.22 . For a typical easing of 0.91 percentage points over the term, the point estimate implies no gain at all, and the one-sided 95 percent confidence bound on the gain is 0.90 percentage points. The data thus rule out large effects: even at the bound, offsetting the median loss of 2.50 percentage points that incumbents in our sample suffer over a term, the cost of ruling (Nannestad & Paldam, 2002), would take an easing larger than 99 percent of the exogenous easings observed in the sample.

A null result is only as informative as the range of models it survives. We therefore vary the choices on which reasonable analysts might differ, for example whether voters judge monetary policy over the whole term or only by the year before the election (Healy & Lenz, 2014), whether the effect depends on the economy being recessionary or inflationary, and whether results are

⁴The Conservatives won 41.9 percent of the vote, against 42.2 percent in 1987, and a majority of 21 seats (Döring & Manow, 2024).

symmetric for interest rate hikes and cuts. Across a range of alternative specifications, the vote-share gain from a typical easing lies between -0.70 and 0.45 percentage points, and none is statistically distinguishable from zero. No specification, however, rules out modest effects at conventional confidence levels, and such effects can matter where elections are close. Our sample contains 104 elections in which an incumbent lost office, either a parliamentary majority or a presidency. Whereas at the point estimate, a typical easing would have reversed none of these losses, at the one-sided 95 percent bound, 15 losses would have been reversed. Across specifications, an easing at the bound would have reversed between 5 and 19 incumbent losses.

Taken together, our results amount to a bounded null. The most likely electoral return to easy money is close to zero, a large return is ruled out, and a modest return can be decisive only in an exceptionally close race. This is as sharp an answer as a century of elections allows. Between politicians who have acted as if easy money wins elections and a theory that casts doubt on it, the evidence sides with theory: it gives governments little reason to expect that easier money will keep them in office.

This paper contributes to the political economy of central banking. Political business cycle models presume that incumbents gain from pre-election monetary stimulus (Hibbs, 1977; Nordhaus, 1975; Rogoff & Sibert, 1988), but the evidence for monetary interventions as a driving force of political business cycles has been weak (Drazen, 2000). Recent work shows that political pressure on central banks is costly: it increases inflation expectations and raises inflation without raising output (Bianchi et al., 2023; Drechsel, forthcoming). We estimate what this pressure could buy the incumbent in return: the electoral gain that can be expected if monetary policy departed from its course in the incumbent's favor.

We also contribute to the literature on economic voting. Recent work identifies how incumbents' vote shares respond to inflation (Aytaç, McDowell, & Steinberg, 2025), house prices (Choi, Quigley, & Wang, 2025) and stock market wealth (Crane, Koch, & Lin, 2024), and how support for extremist and populist parties responds to financial crises (Funke et al., 2016), inflation surprises (Federle et al., 2024), import competition (Autor et al., 2020) and austerity (Fetzer, 2019; Gabriel, Klein, & Pessoa, 2026).⁵ Monetary policy moves all of these conditions

⁵Federle et al. (2024) also study incumbents, using a binary indicator for whether the incumbent lost vote share, and find weak evidence that inflation surprises reduce incumbent support. We estimate the effect of exogenous monetary policy rather than of realized inflation surprises, and our outcome, the percentage-point change in the incumbent's vote share, recovers the magnitude of the effect and not only its sign.

at once. We estimate its net effect on incumbents' vote shares directly.

Our data also add to the long-run measurement of incumbent support. Cross-country studies of the cost of ruling cover only postwar elections (Kayser & Peress, 2012; Nannestad & Paldam, 2002; Paldam, 1986). Our dataset follows the outgoing incumbent bloc from 1900 onward.⁶ We track the bloc across party mergers, splits and renames under documented rules, and validate its lagged vote shares against the recorded ones.

2 Data and definitions

Our data cover general elections and macroeconomic conditions in 18 of today's advanced economies from 1900 to 2020: Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom and the United States. Each country's series begins in 1900 or at its transition to democracy as dated by Boix et al. (2013), so that only competitive elections enter.⁷ Election results and cabinet compositions for the parliamentary systems come from ParlGov (Döring, 2013, 2016; Döring & Manow, 2024). For the U.S., presidential election results come from the American Presidency Project (Peters & Woolley, 2026), and for France they come from the Centre de Données Socio-Politiques of Sciences Po (Banque de Données Socio-Politiques & Ministère de l'Intérieur, 2014) and the French Ministry of the Interior (Ministère de l'Intérieur, 2022). Macroeconomic data come from Release 6 of the Jordà–Schularick–Taylor Macrohistory Database (Jordà et al., 2017).

Our outcome variable is the percentage-point change in the incumbent's vote share between two consecutive general elections. We define the incumbent at the level of the executive (Powell & Whitten, 1993). In the parliamentary systems, it is the bloc of all parties in the first non-caretaker cabinet formed after the preceding election, and its vote share is the parties' combined share in lower-house elections.⁸ In the United States, and in France since the first direct presidential

⁶Studies of the politics of financial crises record governing parties' vote shares back to the 1870s. Funke et al. (2016) code the government formed after each election and use lower-house votes for presidential systems; Chwiero and Walter (2019) relate banking crises to the vote shares of incumbent parties and coalitions.

⁷Appendix B.1 gives each country's start.

⁸Caretaker cabinets are limited to routine business. Defining the incumbent from the last non-caretaker cabinet of the term yields similar results (specification (17) in Figure 2). The Swiss Federal Council is elected by parliament rather than formed through coalition bargaining, but its partisan composition follows from the parties returned to the lower house, so we treat Switzerland in the same way.

election in 1965, the executive is elected directly. There the incumbent is the party of the sitting president, and its vote share is its share of the national popular vote in the presidential election.⁹ For France, we use first-round vote shares, which, like the other elections in our sample, measure support across the full field of parties.

Measuring the change in vote share requires tracking the incumbent parties across elections. ParlGov party identifiers recover the incumbent parties' combined vote share at the preceding election in 95 percent of parliamentary elections. The exceptions arise from party mergers, splits, changes of name and disbandments, which we resolve by hand under a fixed set of rules (Appendix B.2). After these corrections, the incumbent's lagged vote share as we construct it equals the share the incumbent parties received at the preceding election in all but two elections of the estimation sample (Figure B.1). Incumbency in the French and U.S. elections is coded by hand throughout. Appendix B.1 describes the construction in full.

The estimation sample consists of 385 elections in 18 countries.¹⁰ On average, 2.6 calendar years lie between the two election years of a legislative period. Incumbents typically see their vote share decline over a term, a regularity known as the cost of ruling (Nannestad & Paldam, 2002). In our sample, the median loss is 2.50 percentage points. Tables B.2 and B.3 in the Appendix report summary statistics and the sample by country.

3 Empirical strategy

3.1 The trilemma instrument

A simple comparison of interest rates and election results would be misleading. Central banks cut rates when the economy weakens, and a weak economy costs incumbents votes (Fair, 1978; Lewis-Beck & Stegmaier, 2000; Nannestad & Paldam, 1994). Governments that expect to lose may also press hardest for lower rates. In both cases, rate changes move with the incumbent's

⁹In France, the president's party remains the incumbent in the three periods of cohabitation (1986–88, 1993–95 and 1997–2002), when the prime minister came from the opposing camp, because the presidential election decides the president's own office.

¹⁰Of the elections that meet our sample rules, 386 have a non-missing outcome, treatment and instrument. The estimation sample excludes one of those due to missing controls. Of the 31 U.S. elections that meet our sample rules, 5 enter the estimation sample: as the base country for most of the period, the United States has no trilemma instrument. We exclude legislative periods whose two elections fall in consecutive calendar years (specification (8) in Figure 2 adds them). ParlGov flags its pre-1945 records as subject to revision. Restricting the sample to post-war legislative periods does not change the conclusions either (specification (5) in Figure 2).

prospects for reasons other than monetary policy itself, and the comparison confounds the two. Identifying the effect requires rate changes that neither the domestic economy nor domestic politics drove. The trilemma instrument of Jordà et al. (2020) supplies such changes.

The instrument rests on the trilemma of international finance: a country cannot fix its exchange rate, keep its capital account open and set its own interest rate all at once. A pegged country with open capital markets therefore loses control of its interest rate. Interest parity ties its short-term rate to that of the base country. When the base country moves its rate for reasons of its own, the pegged country has to follow. An imported rate change thus does what political pressure on a central bank would do: it moves monetary policy off the course the central bank would have set for its own economy.

Following Jordà et al. (2020), we first remove the part of base-country rate changes that could have been anticipated. We predict it, $\Delta \hat{r}_{b(i,t),t}$, from the first lags of the growth of the base country's real GDP, consumption and investment, its CPI inflation, the changes in its long-term and short-term rates, and its current account-to-GDP ratio. The remainder passes through to countries whose peg held and whose capital account was open. The instrument thus is

$$Z_{it} = (\Delta r_{b(i,t),t} - \Delta \hat{r}_{b(i,t),t}) \times \text{Peg}_{i,t} \times \text{Peg}_{i,t-1} \times \text{KOPEN}_{i,t}, \quad (1)$$

where i and t index countries and years, $b(i,t)$ is the base country of country i in year t , $\Delta r_{b(i,t),t}$ is the observed change in its short-term rate, and $\text{Peg}_{i,t}$ equals one if the currency of country i is pegged to the base in year t . The lagged indicator requires the peg to have held in the year before as well, which leaves out short-lived pegs that failed soon after they were set. $\text{KOPEN}_{i,t}$ is capital account openness as measured by D. Quinn, Schindler, and Toyoda (2011). Capital controls break the link between domestic and base-country rates. The base country is the country whose currency anchors the peg. We take base countries, peg indicators and base-country rates from the Macroeconomic History Database (Jordà et al., 2017). Before the First World War the base is mostly the United Kingdom. In the interwar period, a composite of the United States, the United Kingdom and France, weighted to follow the gold-standard arrangements of the time. After the Second World War, the United States becomes the base for countries in the Bretton Woods system. From 1973, after the demise of Bretton Woods, Germany becomes the base for the

European countries.¹¹

Variation in the instrument comes from legislative periods in which a country was pegged while its base country moved its policy rate. Of the 385 legislative periods in the estimation sample, 244 contain at least one year in which the peg held in that year and the year before; in 244 of them the base country's rate also moved unexpectedly.¹²

3.2 Estimation and inference

Our outcome is observed once per election, so we sum the annual rate changes and the annual instrument over the calendar years between two elections.¹³ Let $\mathcal{T}_{ie}$ denote the years between election e in country i and the preceding election. The treatment and the instrument are

$$\Delta r_{ie}^{\text{term}} = \sum_{t \in \mathcal{T}_{ie}} \Delta r_{it} \quad \text{and} \quad Z_{ie}^{\text{term}} = \sum_{t \in \mathcal{T}_{ie}} Z_{it}, \quad (2)$$

where Δr_{it} is the annual change in the nominal short-term rate.¹⁴

Using Z_{ie}^{term} as the instrument for $\Delta r_{ie}^{\text{term}}$ we estimate the following equation by two-stage least squares:

$$\Delta v_{ie} = \alpha_i + \gamma_{t(e)} + \beta^{IV} \Delta r_{ie}^{\text{term}} + \mathbf{X}_{ie}' \boldsymbol{\theta} + \varepsilon_{ie}. \quad (3)$$

¹¹Table B.3 in the Appendix lists each country's base countries. Appendix A details the construction of the instrument, including for Ireland, whose base country until 1978 is the United Kingdom.

¹²The exclusion restriction requires base-country rate changes to affect incumbent support only through domestic interest rates. The main threat to this restriction is a trade channel: a base-country tightening may also reduce demand for the pegged country's exports, and hurt the incumbent through lower exports rather than through higher domestic rates. Jordà et al. (2026) examine this channel in theory and in the data. In a small open economy model, they show that without the trade channel, a base-rate shock under a hard peg moves output just as a domestic policy shock of the same size would. In the data, they find the channel small: correcting for it makes the output response somewhat smaller, but it remains sizable. We apply their correction in specification (11) of Figure 2. There we add to the domestic rate change the base-country rate change weighted by the ratio of exports to GDP, which moves the trade channel from the error term into the treatment. The resulting estimate is similar. A second threat to the restriction is common shocks that move base-country rates and home elections together, such as global business cycle developments or inflation shocks. Our control vector includes world output and inflation gaps to account for these (Section 3.2).

¹³We leave out both election years: annual data cannot tell whether a rate change in an election year came before or after the vote. Adding the closing election year leaves the finding unchanged (specification (2) in Figure 2).

¹⁴A missing annual value anywhere in the window sets the sum to missing, and the period drops from the sample. We do not annualize: voters live through the full change in the rate over the term, and the coefficient measures the effect of a one percentage point change between two elections, whatever the length of the term. The finding holds with the standard one-year trilemma instrument (specification (3) in Figure 2).

Δv_{ie} is the percentage-point change in the incumbent's vote share since the preceding election.¹⁵ Country fixed effects α_i absorb persistent differences in how incumbent vote shares evolve. Decade fixed effects $\gamma_{t(e)}$, where $t(e)$ is the decade of election e , absorb slow common shifts, such as the long decline of mainstream parties (Gethin et al., 2022).¹⁶

All controls in $\mathbf{X}_{ie}$ are measured before the legislative period begins, because controls measured during the term could absorb part of the effect we seek. The first group of control variables captures government characteristics: the incumbent's vote share at the preceding election and its square to account for the usual attrition in incumbent vote share over a term, i.e. the *cost of ruling*, which tends to be bigger for those who won large shares (Nannestad & Paldam, 2002). How much incumbents lose also depends on other government characteristics (Nannestad & Paldam, 2002), so we add indicators for coalition cabinets and for governments that took office at the preceding election, and the number of consecutive terms in office. The second group captures where in the business cycle the term begins: the short-term rate and the gaps of real GDP per capita and of CPI inflation, at home and in the world, all in the year before the preceding election.¹⁷ The controls also absorb the instrument's imbalance. At the 5 percent level, the instrument is correlated with the rate level and the world inflation gap at the start of the term; at the 10 percent level, also with the national output and inflation gaps, the number of consecutive terms in office and the indicator for new governments (Table C.1). All of these are among the controls.¹⁸ We also report estimates without any controls.

The errors in our sample are likely to be correlated within countries over time and across countries at similar times. Our standard errors, confidence intervals and first-stage F -statistics

¹⁵ β^{IV} is the effect of rate changes imported from abroad. Voters may attribute such changes to conditions abroad rather than to their government, and judge them differently from an easing they can trace to political pressure. Section 5 returns to this.

¹⁶Calendar-year fixed effects would absorb most of the instrument's variation, because base-country rate changes are common to all countries pegged to the same base: with them, the first-stage F -statistic falls below conventional thresholds (notes to Table C.2).

¹⁷The gaps are deviations from Hodrick–Prescott trends (Hodrick & Prescott, 1997) with the smoothing parameter of 100 common for annual data (Backus et al., 1992). The trends are one-sided, estimated on data up to the year of measurement (Stock & Watson, 1999), so that no gap uses information from later years. World real GDP per capita is cumulated from the growth of real GDP per capita across the countries of the Macroeconomic History Database, weighted by their real GDP, and world inflation is their median CPI inflation. Adding corresponding gaps for fiscal variables, such as public debt and the fiscal balance, as well as for financial variables, such as real credit and an indicator for banking crises, does not change the conclusions.

¹⁸The correlation with the rate level may reflect mean reversion in interest rates (Appendix C.2). The instrument is not correlated, even at the 10 percent level, with the incumbent's previous vote share, coalition cabinets, the world output gap, the gaps of credit, debt and the fiscal balance, or banking crises.

allow for both forms of dependence (Thompson, 2011).¹⁹ We also report Anderson–Rubin intervals, which remain valid when the instrument is weak and use the same variance, and, in the notes to Table 1, a wild cluster bootstrap.

4 Findings

This section answers the paper’s question in three steps. Section 4.1 estimates how an exogenous change in interest rates affects the incumbent’s vote share, and finds no effect that we can detect. Section 4.2 asks whether this null survives alternative specifications, and how large an effect the data cannot rule out. Section 4.3 asks whether an effect of that size could have saved incumbents who lost their majority or the presidency.

4.1 Monetary policy and the incumbent’s vote share

We start with the variation behind the estimate. The instrumental-variable estimate combines two relationships: how the trilemma instrument moves the domestic short-term rate, the first stage, and how it moves the incumbent’s vote share, the reduced form. Figure 1 displays both. The instrument moves the domestic short-term rate strongly (panel a). The incumbent’s vote share does not follow: the elections scatter widely around a line that is close to flat (panel b). The ratio of the two slopes is the instrumental-variable estimate.

Table 1 reports the estimates of equation (3). Column (1) is the OLS benchmark, without the instrument; its estimate, -0.12 , is close to zero. Column (2) is our baseline. In its first stage, a one percentage point cumulative change in the instrument raises the domestic short-term rate by 0.81 percentage points over the term, with an effective F -statistic (Montiel Olea & Pflueger, 2013) of 29. The instrumental-variable estimate implies that a one percentage point exogenous increase in the short-term rate over the term changes the incumbent’s vote share by 0.01 percentage points, with a standard error of 0.57. Column (3) drops all controls: the estimate is -0.06 (0.57), and the first stage remains strong. The null does not depend on the controls.

To see what these numbers mean for an incumbent, consider a typical exogenous easing: a fall in the short-term rate by one standard deviation of its instrument-driven cumulative change, 0.91 percentage points over the term. With a point estimate this close to zero, the informative

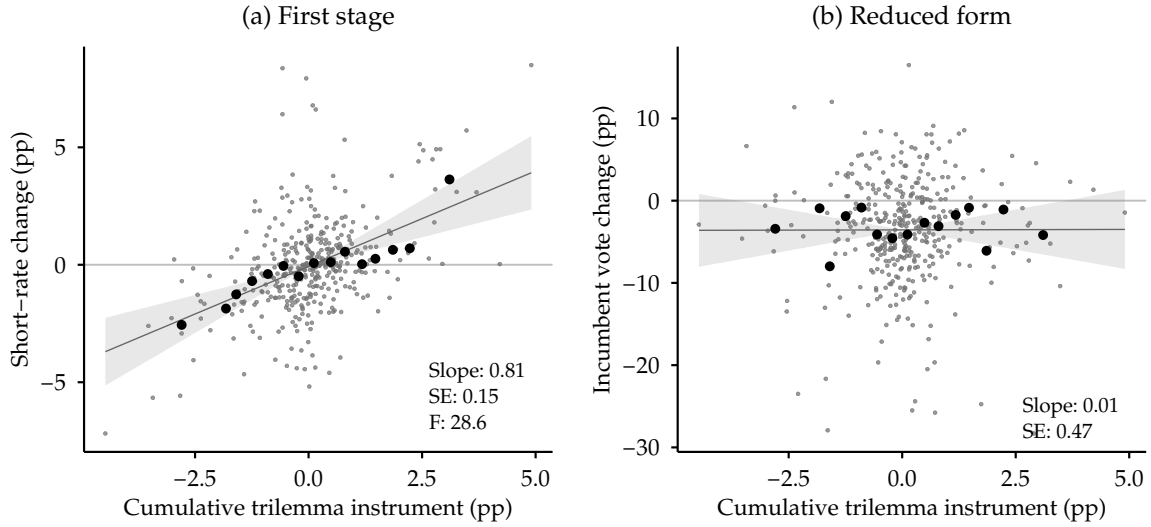
¹⁹Appendix C.1 describes the variance estimator.

Table 1: Monetary policy and the incumbent's vote share

	(1) OLS	(2) 2SLS	(3) 2SLS
<i>Dependent variable: Δ incumbent vote share</i>			
Δ short-term rate (cum.)	−0.117 (0.195)	0.014 (0.574)	−0.060 (0.567)
<i>95% confidence intervals</i>			
Clustered by country and time	[−0.53, 0.29]	[−1.20, 1.22]	[−1.26, 1.14]
Anderson–Rubin	–	[−1.35, 1.29]	[−1.41, 1.19]
<i>First stage: Δ short-term rate (cum.)</i>			
Trilemma IV (cum.)		0.812*** (0.152)	0.878*** (0.163)
<i>F</i> -statistic		28.56	29.12
Controls	Yes	Yes	No
N	385	385	385
Adj. R^2	0.120	0.119	0.052

Notes: Estimates of Eq. (3). The dependent variable is the percentage point change in the incumbent bloc's vote share; the treatment is the cumulative change in the short-term rate over the legislative period, in percentage points. In the 2SLS columns it is instrumented with the cumulative trilemma instrument; the lower panel reports the first stage. All columns include country and decade fixed effects and use the same elections, those with non-missing values of all controls. The controls are measured before the legislative period begins (Section 3). Standard errors (in parentheses), confidence intervals and the first-stage *F*-statistic are clustered by country and time (Thompson, 2011): they allow for any correlation within a country and for correlation across countries within 5 years (Bartlett weights, Driscoll & Kraay, 1998), which common base-country shocks induce, with t_{G-1} critical values, G the number of countries. Anderson–Rubin intervals (Anderson & Rubin, 1949), with the same variance, are robust to weak instruments. A wild cluster bootstrap by country (WRE, Davidson & MacKinnon, 2010; 9,999 replications, weights of Webb, 2023) gives [−1.12, 1.14] for column (2). *F*: effective first-stage *F*-statistic (Montiel Olea & Pflueger, 2013). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, from t_{G-1} .

Figure 1: The trilemma instrument moves interest rates, not incumbent vote shares



Notes: Cumulative change in the short-term rate (panel a) and percentage-point change in the incumbent bloc's vote share (panel b) against the cumulative trilemma instrument over the legislative period, all net of country and decade fixed effects and the controls of Table 1, column (2), with sample means added back. Grey dots: elections; black dots: means of 15 equal-width bins of the instrument. Lines: fitted slopes, the first-stage and reduced-form coefficients of column (2), whose ratio is the instrumental-variable estimate there; shaded: 95 percent confidence bands. Standard errors and the effective first-stage F -statistic are clustered by country and time, as in Table 1.

number is the bound. At the one-sided 95 percent bound, a typical easing raises the incumbent's vote share by at most 0.90 percentage points.²⁰

4.2 Specification uncertainty

The estimate in Section 4.1 rests on one set of specification choices. Here we vary the choices on which reasonable analysts might differ: over which part of the term voters judge monetary policy, which elections enter, which controls and time effects enter, how the treatment is measured, and whether the effect depends on the state of the economy, the direction of the rate change or the clarity of responsibility. None of these choices overturns the null: in every specification, the gain from a typical easing is small and cannot be distinguished from zero. Figure 2 shows the

²⁰The estimate is consistent with what the literature implies. Combining the trilemma-based responses of output and prices to a rate increase (Jordà et al., 2020) with standard elasticities of the incumbent's vote share with respect to growth and inflation (Fair, 2022; Kayser & Peress, 2012), a typical easing raises the incumbent's vote share by 0.13 to 0.39 percentage points at the published point estimates (Table C.5 in the Appendix). Once their sampling uncertainty is taken into account, the implied effect of a one percentage point cumulative tightening ranges from -0.84 to 0.09 percentage points. The range lies inside our confidence interval, and our point estimate implies a smaller gain than any of these calculations. The indirect calculation rests on two assumptions that ours does not need: that voters respond to growth and inflation caused by monetary policy as they do to growth and inflation from any source, and that monetary policy reaches voters through no other channel.

vote-share gain from a typical easing in each specification, numbered as in the paragraphs below. Table C.2 in the Appendix reports the underlying estimates, with its rows numbered the same way.²¹

Timing. Voters may weigh the end of a term more than its beginning (Healy & Lenz, 2014). Specifications (2) to (4) therefore add the closing election year to the window, or replace the cumulative rate change by the change in the year before the election alone, or by the change over the last two years before the election, each instrumented by the corresponding sum of the trilemma instrument.

Sample. Specifications (5) to (8) change which elections enter. Specification (5) keeps only post-WW2 legislative periods, and specification (6) only standard parliamentary systems, without France, Switzerland and the United States. Specification (7) keeps only elections held when the regular term expired. This is because governments can call early elections, and a rate shock could influence when they do.²² Specification (8) adds the terms whose two elections fall in consecutive calendar years, which leave no year between the elections (Section 3.2); for them, it uses the rate change and the instrument of the opening election year.

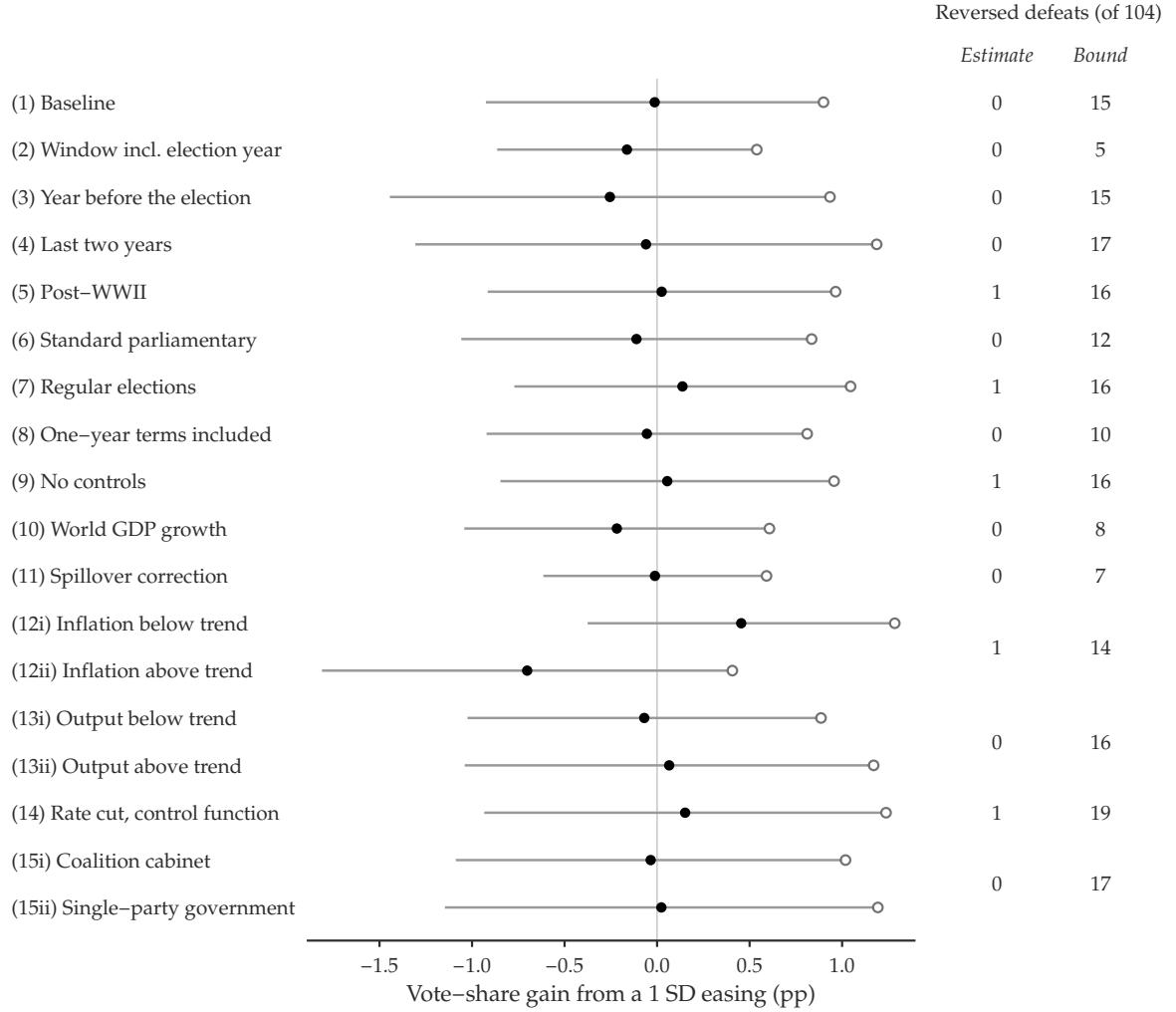
Controls and measurement. Specification (9) drops all controls. Specification (10) replaces the decade fixed effects by world GDP growth over the term (Jordà et al., 2020). Specification (11) addresses the trade channel discussed in Section 3.1: a base-country tightening may also lower demand for the pegged country's exports. Following Jordà et al. (2026), it adds this channel to the treatment:

$$\Delta r_{ie}^{\text{term},s} = \Delta r_{ie}^{\text{term}} + \phi_{ie} \Delta r_{b,ie}^{\text{term}}, \quad (4)$$

²¹In addition, the last three specifications in Table C.2 vary the outcome. They define the incumbent as the bloc of the last cabinet of the term or as the prime minister's party (Fisher & Hobolt, 2010; Kayser & Peress, 2012), or measure its support by seat share. They check our definition of the incumbent (Section 2) rather than the estimation, and are therefore not discussed below. None is significant. The definition by the last cabinet could be selected by the shock if a tightening brought down the cabinet that opened the term. It does not: a rate increase does not affect whether that cabinet is still in office at the election (Table C.3).

²²Table C.3 in the Appendix tests this directly: a rate increase has no statistically significant effect on early elections or on the length of the term.

Figure 2: Monetary easing and incumbent vote shares across specifications



Notes: Vote-share gain of the incumbent from a typical easing, a fall in the short-term rate by 0.91 percentage points over the term (one standard deviation of its instrument-driven cumulative change), by specification, numbered as the rows of Table C.2. Dots: point estimates; circles: one-sided 95 percent bounds; lines: two-sided 90 percent intervals. Standard errors are clustered by country and time, in the state models (12), (13) and (15) by country; specification (14) uses a country-cluster bootstrap. Right: number of the 104 lost offices (85 parliamentary majorities, 19 presidencies) that the gain would have reversed, at the point estimate and at the bound; in the state models, each loss is assigned the coefficient of its own regime.

where $\Delta r_{b,ie}^{\text{term}}$ is the cumulative change in the base country's rate over the same years and ϕ_{ie} is the ratio of exports to GDP in the year before the term. The second term captures the trade channel. Including it in the treatment moves the instrument's effect through exports out of the error term, so the exclusion restriction holds as long as that effect scales with the export share. The instrument is the same as in the baseline.

State dependence. Easing may help an incumbent in a slump and hurt one in an inflationary boom. Specifications (12), (13) and (15) let the effect differ between two regimes $S_{ie} \in \{0, 1\}$:

$$\Delta v_{ie} = \alpha_i + \gamma_{t(e)} + \beta_1^{IV} S_{ie} \Delta r_{ie}^{\text{term}} + \beta_0^{IV} (1 - S_{ie}) \Delta r_{ie}^{\text{term}} + \delta S_{ie} + \mathbf{X}_{ie}' \boldsymbol{\theta} + \varepsilon_{ie}, \quad (5)$$

with $S_{ie} Z_{ie}^{\text{term}}$ and $(1 - S_{ie}) Z_{ie}^{\text{term}}$ as instruments. In specifications (12) and (13), S_{ie} indicates CPI inflation and real GDP per capita above trend in the opening election year.²³ Specification (15) separates single-party governments and presidents from coalitions, which blur responsibility for policy (Powell & Whitten, 1993). The regime indicator then replaces the coalition indicator among the controls. All three regime indicators are measured before the legislative period begins. The rate changes during the term therefore cannot affect which regime an election falls into, so splitting the sample by regime does not select on the treatment. For inflation, the estimates follow the pattern one would expect if voters reward restraint when inflation is high: an easing lowers the incumbent's vote share slightly when inflation is above trend and raises it slightly when inflation is below trend. Neither estimate is significant though, and the difference between them is significant only at the 10 percent level. For output and for the clarity of responsibility, the estimated effect hardly differs between the two regimes.

Direction. Rate cuts and rate increases may affect incumbents differently. Unlike the regimes above, however, the direction of the rate change is part of the treatment itself, so splitting the sample by direction would select on the endogenous treatment. Specification (14) instead estimates

$$\Delta v_{ie} = \alpha_i + \gamma_{t(e)} + \beta^+ \max(\Delta r_{ie}^{\text{term}}, 0) + \beta^- \min(\Delta r_{ie}^{\text{term}}, 0) + \rho \hat{u}_{ie} + \mathbf{X}_{ie}' \boldsymbol{\theta} + \varepsilon_{ie}, \quad (6)$$

where $\hat{u}_{ie}$ is the residual of the first stage of equation (3). Conditioning on $\hat{u}_{ie}$ accounts for the endogeneity of the rate change and hence of its direction (Wooldridge, 2015). Standard errors come from a country-cluster bootstrap that re-estimates both stages. Neither a cut nor an increase has a significant effect, and the two effects do not differ significantly from each other.

Across these choices the null survives. What the choices do change is the width of the

²³Trends are one-sided Hodrick–Prescott trends with $\lambda = 100$, as for the gaps among the controls.

intervals, and with it the largest effect the data cannot rule out: the one-sided 95 percent bounds lie between 0.41 and 1.28 percentage points of vote share. An effect of that size can matter only where an election is close. Section 4.3 asks how often that has been the case.

4.3 Could an easing have saved the incumbent majority?

We now count the elections that a typical easing could have turned. A loss is an election in which the incumbent lost its outright parliamentary majority or the presidency, of which our data contain 104. For each, we compute the vote-share gain the incumbent would have needed to keep its majority or office.²⁴ A loss counts as reversed if a typical easing, a one-standard-deviation fall of 0.91 percentage points in the short-term rate over the term, would have delivered that gain. The right-hand columns of Figure 2 count the reversed losses for each specification, at its point estimate and at its one-sided 95 percent bound.

At the one-sided 95 percent bound, a typical easing reverses 15 of the 104 losses in the baseline (14 percent).²⁵ Across specifications, an easing at the bound would have reversed between 5 and 19 losses. At the point estimate, the baseline reverses none, and no specification reverses more than 1 loss.²⁶

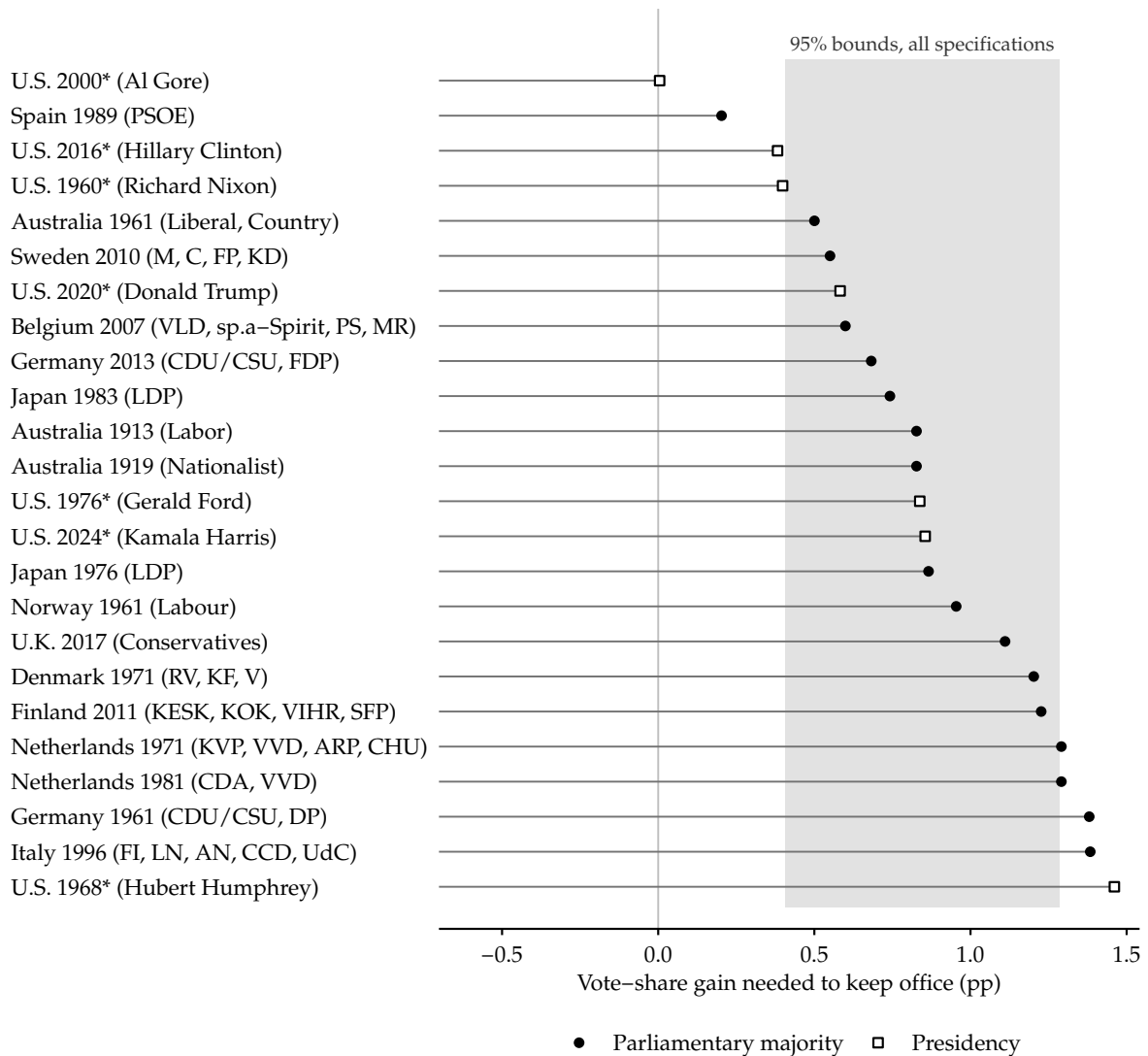
Figure 3 names the elections behind these counts. It ranks the losses that came closest to being reversed by the vote-share gain each needed, against the range of one-sided 95 percent bounds across specifications. The closest is the U.S. presidential election of 2000, decided in Florida by 537 votes, which even small positive point estimates would have reversed. The next closest are parliamentary majorities lost by a few seats and presidencies lost narrowly in the decisive state or round. An easing would have reversed them only if its effect were as large as

²⁴Of the 104 losses, 85 are lost parliamentary majorities, out of 341 parliamentary elections in the estimation sample, and 19 are presidential elections in the United States and France since 1900 that the incumbent president's party lost. For parliaments, we translate the seat shortfall into votes with country-specific seats–votes slopes. For presidencies, the gain needed is half the margin in the decisive contest: the tipping-point state of the U.S. Electoral College, or the decisive round in France.

²⁵A lost majority did not always cost the prime minister's party the premiership. If we count as losses only the lost presidencies and the elections in which the prime minister's party lost the premiership, a typical easing at the bound would have reversed 8 of 70 losses, or 11 percent (Table C.6 in the Appendix).

²⁶For parliamentary majorities, the counts are similar with the seat share as the outcome and with other mappings from votes to seats (Table C.4). An easing comes closest to mattering in U.S. presidential elections: all the presidencies that would have been reversed are American. This reflects how the U.S. presidency is decided, not a larger effect. We apply the same uniform shift in vote shares everywhere, but a U.S. presidency is decided in the state that tips the Electoral College, where the margin is often narrower than the national one. Few U.S. elections enter the estimation sample (Section 2), so for U.S. presidencies the exercise extrapolates an effect estimated mostly in other countries.

Figure 3: Close calls: elections that a one-standard-deviation monetary easing could have reversed



Notes: Lost parliamentary majorities (dots) and lost presidencies (squares), ranked by the vote-share gain the incumbent would have needed to keep office; for presidencies, half the margin in the decisive contest (United States: tipping-point state). In parentheses: the incumbent party or parties, the prime minister's party first, or, for presidencies, the incumbent party's candidate. Shaded band: range of the one-sided 95 percent bounds on the vote-share gain from a typical easing (0.91 percentage points of instrument-driven cumulative rate change) across the specifications of Figure 2. Elections left of the band are reversed at the bound of every specification, elections inside it at the bound of some. Shown: elections whose required gain is at most 1.25 times the largest bound. *Outside the estimation sample.

the upper bound of some or all specifications. Most of the 104 losses fell short by far more than any bound could make up. What the data leave open, then, is a modest effect that decides the occasional close election.

5 Conclusion

Does easy money win elections? For central bank independence, the question is whether a government gains when monetary policy moves off the course its central bank would otherwise have set, in the government's favor. Over more than a century of elections, such departures, imported through the trilemma of international finance, have had no detectable effect on the incumbent's vote share. The data rule out large gains. They do not rule out modest ones, of up to 0.90 percentage points of vote share for a typical easing. An effect of that size would have reversed 15 of the 104 elections in which an incumbent lost its parliamentary majority or the presidency, all of them close races.

For governments that press their central banks, the implication is sobering. The expected electoral return is close to zero, a large return is ruled out, and a modest return could decide only an exceptionally close race.²⁷ The costs of pressure are better established: it raises inflation expectations and inflation without raising output (Bianchi et al., 2023; Drechsel, forthcoming). Voters have judged their governments on many things over the past century. As far as the record shows, easy money has seldom decided their verdict.

²⁷Our estimate concerns rate changes that voters had little reason to attribute to their own government. An easing visibly extracted by political pressure may be judged differently. Voters may credit the government for it, or hold the government responsible for the inflation that follows. The evidence here cannot say which response dominates.

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Online Appendix

to “*Does Easy Money Win Elections?*”

Yao Chen¹ Florian van Genderen² Felix Ward³

¹Erasmus School of Economics, Erasmus University Rotterdam; CEPR; Tinbergen Institute (y.chen@ese.eur.nl).
²Erasmus School of Economics, Erasmus University Rotterdam; Tinbergen Institute (f.p.t.m.vangenderen@ese.eur.nl).
³Erasmus School of Economics, Erasmus University Rotterdam; Tinbergen Institute (ward@ese.eur.nl).

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A Trilemma instrument: construction

We construct the trilemma instrument in equation (1) from Release 6 of the Macrohistory Database (Jordà et al., 2017), following the replication code of Jordà et al. (2020).

Cleaning regression. The predicted base-country rate change comes from a regression of the annual change in the short-term rate of the main base countries on the first lags of the growth rates of their real GDP, consumption and investment, of CPI inflation, of the changes in the long-term and short-term rates, and of the current account-to-GDP ratio. As in Jordà et al. (2020), the regression includes country fixed effects and is estimated separately before the Second World War, for the United States, the United Kingdom and France, and after it, for Germany, the United States and the United Kingdom. The war years 1914–19 and 1939–47 and the German hyperinflation of 1920–25 are excluded from estimation and prediction, so the instrument is missing in those years.

Base countries and pegs. We take peg indicators and base countries from the database, with one exception, Ireland (below). Where the database codes an interwar peg to a composite of the United States, the United Kingdom and France, the base-country rate change is the average over the members of the composite, with the period-specific combinations of Jordà et al. (2020). As in the database, the instrument is zero in years in which a country was not pegged in that year or the year before. It is missing for a base country itself, in the war years, and where an input is missing.

Capital openness. Capital openness is the index of D. Quinn et al. (2011), rescaled to the unit interval and set to full openness after 2012, the last year it covers.

Ireland. The database reports the instrument for Ireland only from 2013. Until March 1979 the Irish pound was held at par with sterling, and Irish short-term rates moved with British rates. We therefore take the United Kingdom as Ireland’s base country from 1952 to 1978. Ireland’s capital openness is the CAP100 index of the current release of D. P. Quinn and Toyoda (2008), which begins in 1950; Irish legislative periods therefore enter the sample from then.

B Voting dataset details

B.1 Construction of the vote-share data

ParlGov records party-level results for every national election and the cabinets that follow each election, including the date a cabinet took office, its parties, the party of the prime minister, and whether it is a caretaker (Döring, 2013, 2016). Coverage begins in 1900, or at a country's full democratic transition as dated by Boix et al. (2013). Where ParlGov reports a single vote share for an alliance of parties, we allocate it to the member parties in proportion to their seats; an example is the Conservative and National Liberal alliance in the United Kingdom from 1950 to 1966. For the United States, we use state and national vote shares from the American Presidency Project (Peters & Woolley, 2026). For France, we combine three files on first-round presidential elections: the 1965–2012 series compiled by Sciences Po, and the 2017 and 2022 series published by the Ministry of the Interior.

The incumbent's current vote share is observed. Its lagged vote share requires tracking parties across elections, which we do with ParlGov party identifiers: each party contesting an election receives the vote share it obtained under the same identifier at the preceding election, and the incumbent's lagged vote share is the sum over the parties of the cabinet formed after that election. Where party mergers, splits, changes of name and disbandments break this link, we resolve the cases by hand under the rules of Appendix B.2, drawing on national election statistics and secondary sources. Appendix B.4 compares the result with the recorded vote shares.

For each country the sample starts with the second election from 1900 onward. In four countries, the earliest covered elections are not comparable to those that follow, and we move the start forward. Germany starts in 1949, because the Weimar Republic had a different executive structure, with both a president and a chancellor. Belgium starts in 1919, because earlier elections renewed only half of the lower house. Sweden starts in 1921, because all earlier elections except 1917 were followed by crown-appointed prime ministers and non-partisan caretaker governments. Portugal starts in 1976, because the 1975 election returned a constituent assembly under a military-led provisional government. Table B.1 lists all elections.

B.2 Hand-coded party changes

Where parties change between elections, we assign incumbency, prime-ministerial status and the lagged vote share by the following rules. Each is illustrated with an example.

- *Change of name.* A party that contests an election under a new name or ParlGov identifier is treated as a merger with a single predecessor under the rules below.
- *Merger of incumbent parties.* The merged party is an incumbent. Its lagged vote and seat shares are the sums of those of its predecessors, and it holds the prime ministership if any predecessor did. Example: Ireland, 1951, when the National Labour Party rejoined the Labour Party. Both were in government.
- *Merger of incumbent and non-incumbent parties.* The merged party inherits incumbency if the incumbent predecessors held a majority of the predecessors' combined vote share at the preceding election. Prime-ministerial status follows the same rule. Its lagged vote and seat shares are the sums of those of all its predecessors, so that the change in its vote share compares like with like. We apply the same rule to a joint list whose members never again run separately. Example: the Netherlands, 1977, when the incumbent KVP and ARP and the non-incumbent CHU, federated since 1975, ran on a single CDA list. They merged formally in 1980. KVP and ARP held 85 percent of the combined vote share, so the CDA list is coded as incumbent, with the lagged vote share of all three.
- *Split of an incumbent party.* Both successor parties inherit incumbency, and the prime ministership goes to the successor the prime minister joins. The predecessor's lagged vote share is divided between the successors in proportion to their seats at the new election. Example: Belgium, 1978, when the incumbent BSP-PSB split into the PS and the SP.
- *Incumbent party leaving parliament.* An incumbent party that loses all its seats, does not stand again or dissolves before the election remains an incumbent. It keeps its lagged vote share, and its vote share at the new election is zero unless it won votes of its own. Its lagged share is not redistributed to other parties. Example: Portugal, 1983, when the incumbent PPM lost its parliamentary representation.
- *Alliances on joint lists.* A joint vote share is allocated to the member parties in proportion

to their seats. Where all members of a list are incumbents, the allocation does not affect the incumbent's vote share, and the list's share counts in full. Example: Finland, 1983, when the incumbent Liberal People's Party ran on the Centre Party's list and won none of its 38 seats, so that the joint vote share goes to the Centre Party.

Italy in 1994 combines several rules. The four parties of the cabinet formed after the 1992 election did not survive the corruption investigations of 1992–93. Christian Democracy dissolved in January 1994 into the Italian People's Party and the Christian Democratic Centre. Under the rule for splits, both are incumbents, and its 1992 vote share is divided between them in proportion to their 1994 seats. The Italian People's Party ran on a joint list with the Segni Pact, whose vote share is divided in proportion to seats. The Liberals disbanded and the Social Democrats won no seat. Both remain incumbents with a vote share of zero. The incumbent's vote share falls from 48.9 percent in 1992 to 17.6 percent in 1994.

Two further corrections need no rule. We correct a few cabinet and prime-ministerial codes in ParlGov. And where ParlGov does not link parties across the two world wars, we take the lagged vote shares from national election statistics.

B.3 Election coverage

Table B.1: Elections covered, by country

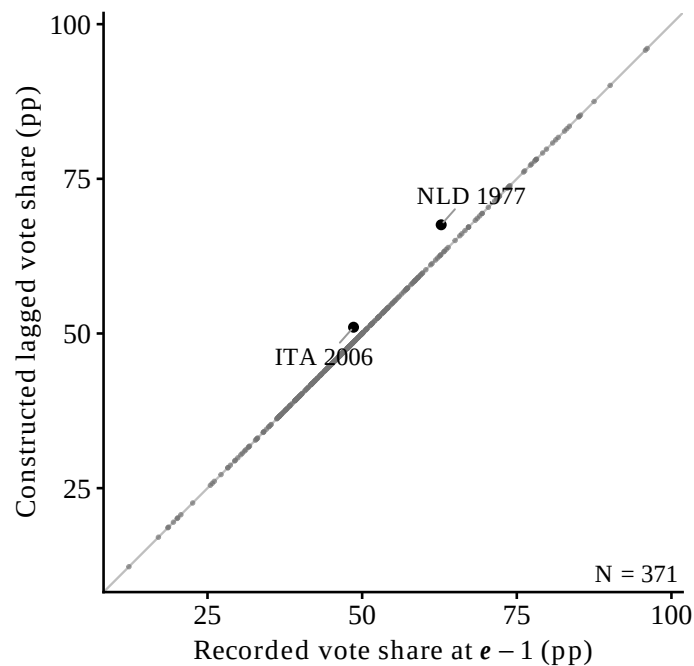
AUS	BEL	CAN	CHE	DEU	DNK	ESP	FIN	FRA	GBR	IRL	ITA	JPN	NLD	NOR	PRT	SWE	USA
(1901)	(1919)	(1900)	(1902)	(1949)	(1901)	(1977)	(1917)	(1965)	(1918)	(1922)	(1946)	(1946)	(1918)	(1900)	(1976)	(1921)	(1900)
1903	1921	1904	1905	1953	1903	1979	1919	1969	1922	(1923)	1948	(1947)	1922	1903	1979	1924	1904
1906	1925	1908	1908	1957	1906	1982	1922	1974	(1923)	1927	1953	1949	1925	1906	(1980)	1928	1908
1910	1929	1911	1911	1961	1909	1986	1927	1981	(1924)	(1927)	1958	1952	1929	1909	1983	1932	1912
1913	1932	1917	1914	1965	(1910)	1989	1929	1988	1929	1932	1963	(1953)	1933	1912	1985	1936	1916
(1914)	1936	1921	1917	1969	1913	1993	(1930)	1995	1931	(1933)	1968	1955	1937	1915	1987	1940	1920
1917	1939	1925	1919	1972	1915	1996	1933	2002	1935	1937	1972	1958	1946	1918	1991	1944	1924
1919	1946	(1926)	1922	1976	1918	2000	1936	2007	1945	(1938)	1976	1960	1948	1921	1995	1948	1928
1922	1949	1930	1925	1980	1920	2004	1939	2012	1950	1943	1979	1963	1952	1924	1999	1952	1932
1925	(1950)	1935	1928	1983	(1920)	2008	1945	2017	(1951)	(1944)	1983	1967	1956	1927	2002	1956	1936
1928	1954	1940	1931	1987	(1920)	2011	1948		1955	1948	1987	1969	1959	1930	2005	1958	1940
(1929)	1958	1945	1935	1990	1924	2015	1951		1959	1951	1992	1972	1963	1933	2009	1960	1944
1931	1961	1949	1939	1994	1926	(2016)	1954		1964	1954	1994	1976	1967	1936	2011	1964	1948
1934	1965	1953	1943	1998	1929	2019	1958		1966	1957	1996	1979	1971	1945	2015	1968	1952
1937	1968	1957	1947	2002	1932	(2019)	1962		1970	1961	2001	(1980)	(1972)	1949	2019	1970	1956
1940	1971	(1958)	1951	2005	1935		1966		1974	1965	2006	1983	1977	1953		1973	1960
1943	1974	1962	1955	2009	1939		1970		(1974)	1969	2008	1986	1981	1957		1976	1964
1946	1977	(1963)	1959	2013	1943		1972		1979	1973	2013	1990	(1982)	1961		1979	1968
1949	(1978)	1965	1963	2017	1945		1975		1983	1977	2018	1993	1986	1965		1982	1972
1951	1981	1968	1967		1947		1979		1987	1981		1996	1989	1969		1985	1976
1954	1985	1972	1971		1950		1983		1992	(1982)		2000	1994	1973		1988	1980
(1955)	1987	1974	1975		1953		1987		1997	(1982)		2003	1998	1977		1991	1984
1958	1991	1979	1979		(1953)		1991		2001	1987		2005	2002	1981		1994	1988
1961	1995	(1980)	1983		1957		1995		2005	1989		2009	(2003)	1985		1998	1992
1963	1999	1984	1987		1960		1999		2010	1992		2012	2006	1989		2002	1996
1966	2003	1988	1991		1964		2003		2015	1997		2014	2010	1993		2006	2000
1969	2007	1993	1995		1966		2007		2017	2002		2017	2012	1997		2010	2004
1972	2010	1997	1999		1968		2011		2019	2007			2017	2001		2014	2008
1974	2014	2000	2003		1971		2015			2011				2005		2018	2012
(1975)	2019	2004	2007		1973		2019			2016				2009			2016
1977		2006	2011		1975					2020				2013			2020
1980		2008	2015		1977									2017			
1983		2011	2019		1979												
(1984)		2015			1981												
1987		2019			1984												
1990					1987												
1993					(1988)												
1996					1990												
1998					1994												
2001					1998												
2004					2001												
2007					2005												
2010					2007												
2013					2011												
2016					2015												
2019					2019												
46	30	35	33	19	46	15	30	10	28	31	19	27	28	32	15	29	31
37	25	21	30	17	36	12	26	9	21	16	18	19	23	29	13	28	5

Notes: Table lists all elections covered for each country. Parentheses mark excluded elections: the first election in each country, and any election held in the same or the following calendar year as the preceding one. Bold marks the elections in the main estimation sample.

B.4 Data validation

We construct the incumbent's lagged vote share by tracking ParlGov party identifiers across elections. Figure B.1 compares it with the share the incumbent parties jointly received at the preceding election, for the parliamentary elections of the estimation sample. After the corrections of Appendix B.2, the two measures differ in only two elections. Both differences follow from the rule for mergers of incumbent and non-incumbent parties, not from a failure to link parties. In the Netherlands in 1977, the governing KVP and ARP ran on a joint CDA list with the CHU, with which they merged in 1980 (Appendix B.2). In Italy in 2006, the governing CCD-CDU merged with Democrazia Europea into the UDC. In both cases the merged party or list is the incumbent, and its lagged vote share includes that of its non-incumbent predecessor. Both elections therefore lie above the 45-degree line.

Figure B.1: Validation of the incumbent's lagged vote share, estimation sample



Notes: Each dot is a parliamentary election. Horizontal axis: combined vote share of the incumbent parties at the preceding election, as recorded for that election. Vertical axis: the incumbent's lagged vote share as constructed from party identifiers, after the corrections of Appendix B.2. Grey dots lie on the 45-degree line; black dots lie off it, and those more than two percentage points off are labelled. Sample: the parliamentary elections of the estimation sample (column (2) of Table 1), that is, without the French and U.S. presidential elections.

Table B.2: Summary statistics

	Mean	SD	P10	Median	P90	N
<i>Outcome, treatment and instrument (per legislative period)</i>						
Δ incumbent vote share (pp)	-3.55	7.13	-12.28	-2.50	4.24	385
Δ short-term rate, cumulative (pp)	-0.05	2.34	-2.75	0.00	2.31	385
Trilemma instrument, cumulative (pp)	0.02	1.21	-1.13	0.00	1.41	385
Years between elections (excl. election years)	2.55	0.96	1.00	3.00	3.60	385
<i>Controls (before the legislative period)</i>						
Incumbent vote share, previous election (%)	48.95	13.57	34.10	47.60	67.42	385
Coalition cabinet (0/1)	0.55	0.50	0.00	1.00	1.00	385
Consecutive terms in office	2.04	1.73	1.00	1.00	4.00	385
New government at previous election (0/1)	0.55	0.50	0.00	1.00	1.00	385
Short-term interest rate (%)	5.48	3.85	1.03	4.66	11.22	385
Output gap (% of trend real GDP p.c.)	-0.16	4.15	-3.63	-0.16	2.99	385
Inflation gap (pp)	-0.00	4.50	-3.11	0.08	2.78	385
World output gap (% of trend world real GDP p.c.)	-0.20	2.90	-3.03	-0.08	2.83	385
World inflation gap (pp)	-0.12	2.69	-1.92	0.12	1.83	385

Notes: Baseline estimation sample (Table 1, column (2)): 385 elections in 18 countries, 1903–2019. Cumulative variables are summed over the years between two elections, excluding both election years. Macroeconomic controls refer to the year before the previous election; Section 3 defines the controls. Gaps are deviations from one-sided Hodrick–Prescott trends (smoothing parameter 100) estimated on data up to the year of measurement. World real GDP per head is cumulated from the GDP-weighted mean growth of real GDP per head, world inflation is the cross-country median of CPI inflation, both across the Macrohistory Database countries.

Table B.3: Estimation sample by country

	Elections	Period	Instrument $\neq 0$	Base country
Australia	37	1903–2019	17	U.K., U.S., Interwar composite
Belgium	25	1925–2019	21	Germany, U.S., Interwar composite
Canada	21	1940–2019	14	U.S.
Denmark	36	1903–2019	28	Germany, U.S., U.K., Interwar composite
Finland	26	1919–2019	18	Germany, U.S., Interwar composite
France	9	1969–2017	9	Germany, U.S.
Germany	17	1957–2017	5	U.S.
Ireland	16	1954–2016	16	Germany, U.K.
Italy	18	1948–2018	14	Germany, U.S.
Japan	19	1960–2017	6	U.S.
Netherlands	23	1922–2017	19	Germany, U.S., Interwar composite
Norway	29	1903–2017	10	U.K., U.S., Interwar composite
Portugal	13	1979–2019	12	Germany
Spain	12	1979–2019	12	Germany
Sweden	28	1924–2018	19	Germany, U.S., Interwar composite
Switzerland	30	1905–2019	15	U.S., U.K., Interwar composite, Germany
United Kingdom	21	1922–2019	6	U.S.
United States	5	1904–1940	3	U.K.
All	385	1903–2019	244	

Notes: Baseline estimation sample (Table 1, column (2)). Period: first and last election. Instrument $\neq 0$: legislative periods in which the cumulative trilemma instrument is non-zero, i.e. the base country's rate moved unexpectedly while the exchange rate was pegged in that year and the year before (Jordà et al., 2017). Base country: base of the pegged years, most frequent first; Germany is the base of the European countries from 1973, including the euro area, and the United Kingdom Ireland's base until 1978. Interwar composite: the gold-standard composite of the United States, the United Kingdom and France (Appendix A).

C Additional results

C.1 Inference

Standard errors, confidence intervals and first-stage F -statistics allow for correlation of the errors within countries over time and across countries at similar times (Thompson, 2011). The variance is the sum of two standard estimators minus what they have in common. The first clusters by country: it allows the errors of a country to be correlated in any way over time. The second, that of Driscoll and Kraay (1998), allows the errors of different countries to be correlated when they are close in time: fully within the same year, and with a weight that declines linearly with the distance between years and reaches zero beyond five years. Both estimators count a country's correlation with itself within five years, so the sum counts it twice, and we therefore subtract it once. With only 18 countries, critical values come from the t distribution with $G - 1$ degrees of freedom, where G is the number of countries.

C.2 Covariate balance

The trilemma instrument is valid if, given the fixed effects, the base-country rate changes that pass through a peg are unrelated to other determinants of the incumbent's vote share. This cannot be tested directly, but it implies that the instrument should not predict conditions determined before the legislative period begins. Table C.1 regresses pre-determined political and macroeconomic variables on the cumulative instrument, with country and decade fixed effects. Coefficients are in standard deviations of the variable per standard deviation of the instrument.

At the 5 percent level, the instrument is correlated with two conditions at the start of the term: the short-term rate and the world inflation gap. At the 10 percent level, it is also correlated with the national output and inflation gaps, the number of consecutive terms in office, and the indicator for governments that took office at the preceding election. It does not predict the incumbent's previous vote share, coalition cabinets, the number of cabinet parties, the world output gap, the gaps of real credit, public debt and the fiscal balance, or banking crises. The correlation with the rate level may reflect mean reversion. High interest rates tend to fall in later years, and under a peg the domestic rate is high when the base-country rate is. The cleaning regression predicts base-country rate changes from lagged changes and other lagged conditions,

so it need not remove such predictable declines. The correlation with the gaps likely reflects the world business cycle, which moves base-country rates as well as output and inflation in the pegged countries. All six variables are among the baseline controls. Without any controls, the estimate is -0.06 (0.57), against 0.01 (0.57) with them (columns (3) and (2) of Table 1), so the imbalance does not drive the result.

Table C.1: Covariate balance: pre-determined conditions and the trilemma instrument

	Coefficient	SE	<i>p</i> -value	N
<i>Political</i>				
Incumbent vote share, previous election	0.016	(0.031)	0.61	385
Incumbent vote share squared	0.017	(0.030)	0.57	385
Coalition cabinet	0.002	(0.015)	0.89	385
Number of cabinet parties	0.011	(0.029)	0.72	385
Consecutive terms in office	-0.057^*	(0.032)	0.09	385
New government at previous election	-0.057^*	(0.029)	0.06	385
<i>Macroeconomic conditions, year before the term</i>				
Short-term interest rate	-0.059^{***}	(0.019)	0.01	385
Output gap	-0.062^*	(0.031)	0.06	385
Inflation gap	-0.061^*	(0.029)	0.05	385
Real credit gap	0.006	(0.048)	0.89	376
Public debt / GDP gap	0.023	(0.057)	0.69	359
Fiscal balance / GDP gap	-0.003	(0.039)	0.94	363
Banking crisis, previous five years	0.051	(0.040)	0.22	385
<i>Global conditions, year before the term</i>				
World output gap	-0.033	(0.059)	0.58	385
World inflation gap	-0.078^{**}	(0.034)	0.03	385

Notes: Coefficient from regressing each variable on the cumulative trilemma instrument over the legislative period, with country and decade fixed effects, standardized to the change in the variable, in standard deviations, per standard deviation of the instrument. All variables are measured before the legislative period begins; the gaps are deviations from one-sided Hodrick–Prescott trends. The world output and inflation gaps are one series for all countries, detrended in the same way: world real GDP per head is cumulated from the GDP-weighted mean growth of real GDP per head, world inflation is the cross-country median of CPI inflation, both across the Macrohistory Database countries. The sample is the baseline estimation sample, restricted to non-missing values of the variables involved. Standard errors are clustered by country and time, as in Table 1; *p*-values from a *t*-distribution with $G - 1$ degrees of freedom. For new government at previous election, the country-and-time variance is not positive in this sample, and the standard error is clustered by country. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C.3 Specification uncertainty

Table C.2: Specification uncertainty: 2SLS estimates of the effect of the short-term rate on the incumbent's vote share

Model	First stage		2SLS		F -statistic	N	Adj. R^2
	$\hat{\gamma}$	SE($\hat{\gamma}$)	$\hat{\beta}^{IV}$	SE($\hat{\beta}^{IV}$)			
(1) Baseline	0.812***	(0.152)	0.014	(0.574)	28.56	385	0.119
<i>Panel I: Timing</i>							
(2) Window including the election year	0.901***	(0.156)	0.178	(0.441)	33.27	384	0.120
(3) Single year before the election	0.599***	(0.076)	0.279	(0.748)	62.36	392	0.123
(4) Last two years before the election	0.707***	(0.144)	0.066	(0.784)	23.95	385	0.119
<i>Panel II: Sample</i>							
(5) Post WW-II	0.783***	(0.137)	−0.027	(0.589)	32.80	294	0.187
(6) Standard parliamentary sample	0.798***	(0.154)	0.122	(0.588)	26.77	341	0.135
(7) Regular elections only	0.853***	(0.114)	−0.150	(0.571)	56.18	296	0.141
(8) Including one-year terms	0.827***	(0.147)	0.060	(0.545)	31.50	408	0.132
<i>Panel III: Controls and measurement</i>							
(9) No controls	0.878***	(0.163)	−0.060	(0.567)	29.12	385	0.052
(10) World GDP growth instead of decade FE	0.893***	(0.140)	0.238	(0.519)	40.47	386	0.064
(11) Spillover correction	1.225***	(0.183)	0.013	(0.379)	44.56	381	0.123
<i>Panel IV: State dependence and direction</i>							
(12) Inflation model						385	0.123
(i) Inflation below trend	1.071***	(0.109)	−0.497	(0.522)	96.78		
(ii) Inflation above trend	0.649***	(0.123)	0.768	(0.697)	27.70		
(13) Output model						385	0.124
(i) Output below trend	0.864***	(0.139)	0.076	(0.601)	38.45		
(ii) Output above trend	0.811***	(0.130)	−0.072	(0.695)	38.78		
(14) Direction model (control function)						385	0.117
(i) Effect of a 1 pp rate increase	0.812***	(0.152)	0.232	(0.442)	28.56		
(ii) Effect of a 1 pp rate cut			0.166	(0.683)			
(15) Clarity of responsibility						385	0.116
(i) Coalition cabinet	0.852***	(0.102)	0.038	(0.662)	69.87		
(ii) Single-party cabinet or president	0.856***	(0.172)	−0.025	(0.736)	24.73		
<i>Panel V: Alternative outcomes</i>							
(16) Incumbent: last cabinet of the term	0.794***	(0.151)	0.491	(0.662)	27.72	371	0.096
(17) Prime minister	0.799***	(0.154)	0.103	(0.448)	26.83	341	0.154
(18) Seat share	0.787***	(0.153)	0.477	(0.897)	26.36	371	0.155

Notes: Each row changes one element of the baseline in row (1), which reproduces column (2) of Table 1; Section 4.2 describes the specifications. Coefficients refer to a cumulative change in the short-term rate over the legislative period of one percentage point; in row (3), to a change of 0.66 percentage points in the year before the election, which is equally typical; in row (14), to a one percentage point increase and a one percentage point cut. All rows include country fixed effects, decade fixed effects (except row (10)) and the controls of Table 1 (except row (9)); in rows (16) to (18) the lagged level of the respective outcome replaces the lagged vote share. Rows (16) to (18) exclude France and the United States, (17) also Switzerland. With calendar-year instead of decade fixed effects, the first-stage F -statistic is 8.76 (not shown). F : effective first-stage F -statistic (Montiel Olea & Pflueger, 2013). Standard errors (in parentheses), stars and F -statistics allow for dependence within countries and across countries in nearby years, as in Table 1, with t_{G-1} critical values; in rows (12), (13) and (15) they are clustered by country, and in row (14) they come from a country-cluster bootstrap that re-estimates both stages (1,999 replications). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.4 Government survival

A null on the vote share could mask an effect on whether the government survives until the election. If tightening brought down cabinets or triggered early elections, the incumbent measured at the end of the term, as in row (16) of Table C.2, would be selected by the shock. Table C.3 estimates the baseline specification with three measures of survival as outcomes: a change of cabinet during the term, an early election, and the length of the term relative to its constitutional maximum. Because the cumulative rate change grows mechanically with the length of the term, these regressions average the treatment and the instrument per year. A rate increase has no statistically significant effect on any of the three: whether the cabinet that opened the term is still in office at the election, whether the election is held early, or the length of the term. Restricting the sample to elections held at the end of the regular term leaves the vote-share estimate unchanged (row (7) of Table C.2).

Table C.3: Monetary policy and government survival

Outcome	Mean	$\hat{\beta}^{IV}$	SE	F	N
(1) Cabinet change during the term	45.3	-1.02	(3.57)	14.27	371
(2) Early election	23.1	3.05	(2.73)	14.62	385
(3) Length of the term (% of the maximum term)	85.2	0.13	(1.36)	11.10	293

Notes: 2SLS estimates with the controls and fixed effects of row (1) of Table C.2, on its elections with the outcome observed (cabinet change: parliamentary elections only). Treatment and instrument are averaged per year of the term; coefficients refer to an average annual rate increase of 0.46 percentage points, as typical as a one percentage point cumulative change. Outcomes in percent: (1) the first and last non-caretaker cabinets of the term differ; (2) the election is held more than 1 year before the end of the maximum term; (3) years since the preceding election, in percent of the maximum term. Standard errors and *F*-statistics as in Table C.2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.5 Lost majorities: seat-share outcome and seat mapping

For parliamentary majorities, the counterfactual of Section 4.3 requires a translation of vote gains into seats. Table C.4 checks this translation in two ways. It replaces the vote-share coefficient by the effect on the seat share, which needs no translation, and it replaces the country-specific seats–votes slopes by three alternatives. Presidencies are decided by votes and need no such translation.

Table C.4: Parliamentary majorities a monetary easing would have retained: seat-share outcome and seat mapping

	Point estimate			One-sided 95% bound		
	β	Vote-share gain (pp)	Retained (of 85)	β	Vote-share gain (pp)	Retained (of 85)
<i>Baseline (country-specific seats–votes slopes)</i>						
Baseline	0.014	–0.01	0 (0%)	–0.984	0.90	9 (11%)
<i>Alternative outcome</i>						
Seat-share outcome ^a	0.213	–0.19	0 (0%)	–1.250	1.14	6 (7%)
<i>Alternative seat mapping</i>						
Seats proportional to votes (1:1)	0.014	–0.01	0 (0%)	–0.984	0.90	5 (6%)
Pooled seats–votes slope	0.014	–0.01	0 (0%)	–0.984	0.90	7 (8%)
Seats–votes slope by electoral system	0.014	–0.01	0 (0%)	–0.984	0.90	8 (9%)

Notes: Lost parliamentary majorities that a typical easing (0.91 percentage points over the term) would have retained, among the 341 parliamentary elections of the estimation sample (without France, Switzerland and the United States), in 85 of which the incumbent bloc lost its outright majority. β : effect of a one percentage point rate increase; gain = $-\beta \times$ easing. Seat share^a: 2SLS with the change in the bloc’s seat share as the outcome, as in Table C.2, row (18), but on these 341 elections. Seat mappings: one-for-one; one pooled slope (1.32); separate slopes for majoritarian (1.52) and proportional systems (1.19). Bounds: $\hat{\beta} - t_{0.95, G-1}$ SE, standard errors as in Table 1 (seat share: clustered by country). ^a In seat-share percentage points.

C.6 Monetary policy, the economy and economic voting

Our estimate measures the effect of monetary policy on the incumbent’s vote share directly. An indirect benchmark follows from two literatures: the effects of monetary policy on output and prices, and the effects of output and prices on votes. Table C.5 chains the two, for the same treatment as ours, a one percentage point cumulative rate increase over the term. Section 4.1 summarizes the result.

Transmission. We take the responses of real GDP per head and of the CPI to a one percentage point increase in the short-term rate from the trilemma local projections of Jordà et al. (2020), Table 4, at horizons zero to four years, for the full sample and for the period after the Second World War. Their instrument is the annual counterpart of ours, so the responses describe the same kind of rate change. We spread the cumulative increase evenly over the L years of the window, $1/L$ percentage points a year, and add up the responses this path implies for real GDP growth in the election year and for the price level at the election. Average annual inflation over the term is the price-level effect divided by the $L + 1$ years of the term, including the election year. Responses beyond four years are held at their four-year value. We average the results over

the window lengths in our estimation sample.

Vote elasticities. We translate growth and inflation into vote shares with three sets of estimates: the presidential and House vote equations of Fair (2022), with the incumbent's vote share responding to election-year growth and to inflation over the term, and the estimates of Kayser and Peress (2012) for parliamentary elections in OECD countries, which include growth only. The implied effect is θ_g times the growth response plus θ_π times the inflation response.

Uncertainty. The intervals of the table propagate the sampling uncertainty of the published estimates by Monte Carlo: 10,000 draws of the impulse responses and of the elasticities from normal distributions with the published standard errors. The published tables give no covariance across horizons. Because local-projection responses at nearby horizons are strongly correlated, we assume a correlation of $0.8^{|h-h'|}$ between horizons h and h' ; treating the horizons as independent would overstate the uncertainty of growth, a difference of adjacent responses. Output and price responses, and the elasticities, are drawn independently. The notes to Table C.5 report the implied effects when growth is measured in the year before the election instead of the election year.

Power. The implied effects are small relative to our standard error. To distinguish an effect of size b from zero with 80 percent power in a two-sided test at the 5 percent level, the standard error must fall to $b/2.8$. Since the standard error scales with the inverse square root of the number of elections, this requires $(2.8 \text{ SE}/b)^2$ times as many elections as we have, 14 to 121 times across the rows of the table.

Table C.5: What the literature implies: a back-of-the-envelope calculation

	Effect on the economy		Vote elasticity		Implied vote effect	
	Growth	Inflation	θ_g	θ_π	Estimate	95% interval
<i>Transmission: Jordà et al. (2020), LP-IV, 1870–2006</i>						
Fair (2022), presidential	−0.65	−0.31	0.71	−0.61	−0.28	[−0.62, 0.09]
Fair (2022), House	−0.65	−0.31	0.37	−0.32	−0.15	[−0.36, 0.08]
Kayser and Peress (2012), OECD	−0.65	−0.31	0.66	0.00	−0.43	[−0.84, −0.09]
<i>Transmission: Jordà et al. (2020), LP-IV, 1948–2006</i>						
Fair (2022), presidential	−0.55	−0.08	0.71	−0.61	−0.34	[−0.59, −0.12]
Fair (2022), House	−0.55	−0.08	0.37	−0.32	−0.18	[−0.35, −0.04]
Kayser and Peress (2012), OECD	−0.55	−0.08	0.66	0.00	−0.36	[−0.72, −0.07]
Our estimate (Table 1, column (2))					0.01	[−1.20, 1.22]

Notes: Vote-share effect of a one percentage point cumulative rate increase over the term implied by published estimates. Growth and inflation: responses of election-year real GDP growth and of average annual inflation over the term (pp), from the trilemma local projections of Jordà et al. (2020), Table 4, with the rate change spread evenly over the term. θ_g , θ_π : vote-share change per percentage point of growth and of inflation. Implied effect: $\theta_g \times \text{growth} + \theta_\pi \times \text{inflation}$. 95% intervals: Monte Carlo over the published standard errors (10,000 draws; correlation 0.8^{|h-h'|} across horizons); for our estimate, the confidence interval of Table 1. With growth in the year before the election, the implied effects range from −0.27 to −0.06.

Table C.6: Losses a monetary easing would have reversed, by definition of a loss

Loss of the . . .	Losses		Reversed		
	All	Presi- dencies	Point estimate	95% bound	Range of bounds
Majority or presidency (baseline)	104	19	0	15	5–19
Premiership or presidency	70	19	0	8	3–10

Notes: Number of losses that a one-standard-deviation easing (0.91 percentage points of instrument-driven cumulative rate change) would have reversed, at the point estimate and at the one-sided 95 percent bound of the baseline. A parliamentary loss is reversed if the vote-share gain exceeds the gain needed to restore the incumbent bloc's outright majority. Premiership: the prime minister's party does not lead the first cabinet after the election. Presidencies count as losses in both rows. Range: one-sided 95 percent bounds across the specifications of Figure 2 with a single coefficient (state models excluded).

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