Public Debt Levels and Real Interest Rates: Causal Evidence from Parliamentary Elections

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Public Debt Levels and Real Interest Rates: Causal Evidence from Parliamentary Elections*

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Abstract

We use close parliamentary elections as natural experiments to estimate the debt sensitivity of interest rates. Relative to an election in which one party barely secures a majority, an election in which no party achieves a majority causes the debt-to-GDP ratio to increase by 17 percentage points, while real interest rates rise by 99 basis points. If elections only impact real rates via debt, our results imply that a one percentage point increase in the debt-to-GDP ratio causes a 5.8 basis point increase in real rates, larger than most previous estimates and suggesting potential reverse causality from rates to debt.

Keywords: national debt, real interest rates, crowding out, regression discontinuity design

JEL Codes: E62, H63

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1 Introduction

Economists have debated whether and to what extent government debt levels affect real interest rates since at least the work of Ricardo (1820). Studying the topic is difficult, however, because of the many factors that may confound the empirical relationship between debt and interest rates. In this paper, we use a regression discontinuity (RD) design that exploits exogenous variation in debt levels resulting from elections in which the largest party barely won or barely missed a parliamentary majority. We estimate that a one percentage point increase in the government debt-to-GDP ratio causes long-term real interest rates to increase by 5.8 basis points. That estimate is larger than most previous estimates, which suggests the potential relevance of reverse causality from interest rates to debt in estimating this relationship.

Our RD design addresses two major endogeneity problems that will tend to obscure any tendency for higher debt levels to raise real rates. First, deficits tend to be counter-cyclical and real interest rates tend to be pro-cyclical in modern economies. Addressing these business-cycle correlations has been a major focus of the literature studying this topic over the past twenty years. Second, there is simultaneity in the determination of debt levels and real interest rates: at the same time that higher government debt levels may drive up real interest rates, low real rates may encourage governments to borrow more freely. We provide some anecdotal evidence that various politicians and economists consider low interest rates to be an argument for higher government borrowing in Appendix A.1. Addressing the potential simultaneity between debt and interest rates does not appear to have been an important focus of the previous literature.

We exploit a plausibly exogenous source of variation in government debt levels in the tendency of single-party majority governments in parliamentary systems to exhibit more fiscal discipline than coalition and minority governments. In their pioneering work documenting this pattern, Roubini and Sachs (1989a) argue that coalition governments suffer from a prisoners' dilemma, in which individual member parties have the incentive and ability to protect their budget priorities from adverse changes, but there are weak enforcement mechanisms to achieve a cooperative outcome featuring greater fiscal discipline. We discuss this argument in more detail and present some case studies from the literature illustrating this relationship in Appendix A.2.

Building on the insight that single-party majorities tend to produce more fiscal discipline, our analysis of the causal effect of public debt on real interest rates proceeds in four main steps:

- First, we estimate that the debt-to-GDP ratio falls by 17 percentage points after an election in which the largest party barely won a majority relative to an election in which the largest party barely missed a majority.
- Second, we estimate that real long-term interest rates fall by 99 basis points after an election in which the largest party barely won a majority relative to an election in which the largest party barely missed a majority.
- Third, we take the ratio of the preceding two effects to estimate that a one percentage-point increase in the debt-to-GDP ratio raises real rates by 5.8 basis points; following Neveu and Schafer (2024), we call this effect the debt sensitivity of interest rates, or DSIR.
- Fourth, we discuss potential threats to our identification strategy and stress the strong assumptions necessary to interpret our estimates as the causal effect of higher debt levels on real interest rates.

Our estimated DSIR of 5.8 basis points is larger than most in the literature. Blanchard (2019) references a “standard (but admittedly rather uncertain as well) back-of-the-envelope number that an increase in debt of 1 percent of GDP increases the safe rate by 2–3 basis points.” The U.S. Congressional Budget Office reports similar estimates (Gamber and Seliski, 2019; Neveu and Schafer, 2024). We therefore interpret our results as suggesting that reverse causality is an important factor to consider when studying the extent to which public debt levels increase real interest rates.

Our paper relates closely to the literature that studies the DSIR. Elmendorf and Mankiw (1999) and Gale and Orszag (2004) provide useful surveys of the evidence for the United States. Laubach (2009) proposes a particularly influential approach. By studying the relationship between projected debt levels and real forward long-term Treasury rates, he is able to insulate the estimates from business-cycle confounding and estimates a DSIR of 3–4 basis points. Using similar approaches, Gale and Orszag (2004) estimate a DSIR of 4–6 basis points, Engen and Hubbard (2004) estimate a DSIR of approximately 3 basis points, Gamber and Seliski (2019) estimate a DSIR of 2–3 basis points, and Neveu and Schafer (2024) estimate a DSIR of 2 basis points. In the finance literature, Krishnamurthy and Vissing-Jorgensen (2012) estimate that a one-standard deviation decrease in the U.S. log debt-to-GDP ratio (approximately 20 percentage points) reduces spreads between long-term Treasury yields and Aaa-rated corporate bond yields by 44 basis points, implying a DSIR of 2.2 basis points.

Our paper also relates to studies of the DSIR in an international context. Those studies face more obstacles than studies focusing on the U.S., however, such as the lack of a long history of consistently defined market interest rates and current and projected levels of debt. Kinoshita (2006) studies OECD countries at a five-year frequency to abstract from business cycle considerations and estimates a DSIR of about 2 basis points. Greenlaw, Hamilton, Hooper and Mishkin (2013) apply panel regression methods to a set of advanced economies to estimate an average DSIR of approximately 4.5 basis points. Ardagna, Caselli and Lane (2007) study OECD countries using a VAR approach and provide evidence that the DSIR is non-linear in the level of debt-to-GDP; they estimate a DSIR of 3.3 basis points for a country with a debt-to-GDP ratio of 119 percent, Italy's level in 2002. Overall, we consider the estimated DSIR in the international evidence to be roughly in line with the U.S. evidence, despite the reasons that they may differ in theory.

Finally, our paper contributes to the literature that relates government deficits and debt levels to the presence or absence of a single-party parliamentary majority. Roubini and Sachs (1989a) estimate that "the difference between a majority government and a minority government... is... 1.5 percentage points of added budget deficit per year," while Roubini and Sachs (1989b) estimate an impact of around 1 percent of GNP using a different sample and methodology. Alesina and Perotti (1995), Alesina, Perotti and Tavares (1998), and Perotti and Kontopoulos (2002) generally support the conclusion that the absence of a single-party majority leads to larger deficits, although De Haan and Sturm (1994) and Perotti and Kontopoulos (2002) both show that such results can be sensitive to the exact coding of different types of governments and regression specifications. Our study complements this literature, which generally predates the widespread adoption of RD designs in economics, by showing that single-party majorities' tendency to be associated with lower debt-to-GDP ratios is likely to reflect a causal relationship.

2 Empirical Approach

2.1 Regression Discontinuity Design

Ideally, we would like to estimate the equation

$$\Delta R_{it} = \iota + \beta \Delta D_{it} + \omega_{it} \tag{1}$$

where the unit of observation is country i at time t, ΔR_{it} represents the change in real long-term interest rates, ΔD_{it} represents the change in the government debt-to-GDP ratio, ι is a constant term, and ω_{it} is a random error. Because government debt levels and real interest rates are determined simultaneously in a complex macroeconomic system, ω_{it} is likely to be correlated with ΔD_{it}, and ordinary least squares estimation of equation (1) is likely to yield an inconsistent estimate of the true DSIR, β.

To address these identification problems, we implement a fuzzy regression discontinuity (RD) design by estimating a system of two equations:

$$\Delta D_{it} = \gamma + \delta M_{it} + g(V_{it} - \overline{c}_{it}) + \nu_{it} \tag{2}$$

$$\Delta R_{it} = \alpha + \tau M_{it} + f(V_{it} - \overline{c}_{it}) + \varepsilon_{it} \tag{3}$$

where ΔR_{it} and ΔD_{it} are defined as in equation (1), but the unit of observation is now country i holding a parliamentary election at time t. V_{it} represents the share of seats won by the largest party in the election, and $\overline{c}_{it}$ represents the threshold necessary for a party to win a parliamentary majority. M_{it} is a dummy variable that captures whether the largest party won a majority in the election:

$$M_{it} = 1[V_{it} \geq \overline{c}_{it}]. \tag{4}$$

ε_{it} and ν_{it} are uncorrelated random errors, and $f(\cdot)$ and $g(\cdot)$ are functions that capture how ΔR_{it} and ΔD_{it} vary with V_{it} away from the cutoff $\overline{c}_{it}$.

The coefficient of interest in equation (2) is δ, which captures the effect of a single-party majority on the change in government debt levels. The coefficient of interest in equation (3) is τ, which captures the effect of a single-party majority on the change in real interest rates. Given estimates $\hat{\tau}$ and $\hat{\delta}$, the estimate of the DSIR $\hat{\beta}$ is calculated as the ratio of the two:

$$\hat{\beta} = \frac{\hat{\tau}}{\hat{\delta}}. \tag{5}$$

We estimate the functions $f(\cdot)$ and $g(\cdot)$ non-parametrically as separate local linear functions on each side of the cutoff, $\bar{c}_{it}$, using triangular kernels and mean square error-optimal bandwidths. Throughout the paper, we report bias-corrected p-values following Calonico et al. (2014), using coverage error optimal bandwidths (Calonico et al., 2020). These choices are guided by the best practices suggested in Cattaneo et al. (2019b). Appendix B examines our estimates' robustness to alternative econometric specifications.

Our baseline and most of our additional specifications include covariates as described in Calonico et al. (2019) to improve the precision of our estimates. The covariates we consider in our baseline analysis are the lagged growth rate of real GDP, the lagged short-term interest rate, a moving average of past inflation, and a lagged indicator variable for an economic crisis. Most of these controls are standard in the macroeconomic literature studying the effects of government fiscal policy (e.g., Auerbach and Gorodnichenko, 2017; Ramey and Zubairy, 2018; Blanchard and Perotti, 2002). The indicator for a lagged economic crisis serves to control for the potential effects of economic crises on debt levels and real interest rates, as emphasized in Reinhart and Rogoff (2009).

2.2 Data Sources and Variable Definitions

Our design requires data on election outcomes, debt-to-GDP ratios, and real interest rates for a panel of countries. The unit of observation in our model is a country-election, and all economic data is measured at the annual frequency. Our baseline sample includes data from 26 countries and 336 election-year observations over the period 1950 to 2015. We begin our analysis in 1950 to avoid any measurement or other confounding issues related to World War II and its short-term aftermath. We describe our main data series, their sources, and their construction briefly here. Appendix D provides further details.

Our main data sources for election outcomes are Seki and Williams (2014), Döring and Manow

(2019), and Inter-Parliamentary Union (2020). We define $(V_{it} - \bar{c}_{it})$ as the share of parliamentary seats won by the largest party in the election minus 50 percentage points. We code M_{it} as one if the largest party won more than 50 percent of the seats but as zero if the largest party won exactly 50 percent of the seats or fewer. Appendix D.1 provides more detail and describes how we combine election data from these sources.

We compare the one-year change in real long-term interest rates to the five-year change in the debt-to-GDP ratio to account for market participants' medium-term expectations of future deficits and debt following an election, rather than current levels only. Feldstein (1986) argues that taking into account anticipated future budget deficits is an essential part of any analysis relating deficits to interest rates. That view has been widely accepted in the literature, which has often focused on expected debt or deficits five years in the future (e.g. Laubach, 2009; Gale and Orszag, 2004; Engen and Hubbard, 2004; Gamber and Seliski, 2019; Neveu and Schafer, 2024). Appendix E discusses our results using different horizons for the change in the debt-to-GDP ratio and real interest rates. Considering longer horizons leads to larger changes in the debt-to-GDP ratio and smaller changes in real interest rates, so the implied DSIR decreases at longer horizons.

Our main sources for government debt-to-GDP ratios are the IMF's Global Debt Database (Mbaye et al., 2018) and the debt series of Reinhart and Rogoff (2011), which together extend from before World War II through 2020 for most of the countries in our sample. We use the central government debt-to-GDP ratio where it is available. We measure the variable ΔD_{it} as the change in the government debt-to-GDP ratio from the year of the election to five years after the election. We supplement these sources with debt data from the IMF's World Economic Outlook or Jordà et al. (2017) as described in Appendix D.

We measure the change in real interest rates ΔR_{it} as the change in nominal interest rates on long-term government bonds, ΔI_{it}, minus the change in a proxy for expected inflation, $\Delta \Pi^e_{it}$. We calculate ΔI_{it} as the one-year change from the year of the election to the year following the election. We follow Council of Economic Advisers (2015) in proxying for expected inflation Π^e_{it} as the five-year unweighted moving average of current and past annual inflation. We describe the construction of our series for the change in real interest rates ΔR_{it} in more detail in Appendix

D.2. We examine our estimates' sensitivity to alternative data sources and definitions of inflation expectations in Appendix E.

2.3 Validity and Interpretation of the Regression Discontinuity Design

Following Lee and Lemieux (2010), a number of assumptions are necessary for our fuzzy RD to estimate the causal DSIR. The first is that the growth of debt-to-GDP ratios changes discontinuously at the threshold for a single-party majority, as seen in our first-stage RD estimates. The second assumption is "monotonicity," which requires that, near the cutoff, a single-party majority always leads to a relative decline in the debt-to-GDP ratio. We cannot test this assumption formally, but the literature on the topic has emphasized this hypothesis.

The third assumption is that there is imperfect sorting of the largest party's parliamentary seat share V around the cutoff $\bar{c}$, or "imprecise control." Lee and Lemieux (2010) show that when this condition holds, the treatment M is "as good as randomly assigned" around the cutoff, i.e., we have "local randomization." We believe this assumption is natural in our context, as it amounts to political parties being unable to choose precisely the number of parliamentary seats that they win.

Local randomization has two implications that are commonly used as falsification tests for the design. The first tests for a discontinuity in the distribution of the share of parliamentary seats won by the largest party around the cutoff for a majority in the spirit of McCrary (2008). We implement the related test of Cattaneo et al. (2018) and Cattaneo et al. (2019a); the test produces a p-value of 0.65, so we cannot reject the null hypothesis of no difference in the density function on either side of the cutoff. The second test is for discontinuities in predetermined variables of interest at the cutoff. We conduct sharp RD tests of the lagged values of several predetermined variables and find that we cannot reject the null hypothesis of no discontinuity at the cutoff for any of them. We present these tests and results, which are consistent with the local randomization assumption, in Appendix B.1, where we also present results from RD specifications using placebo cutoff values for the seat share and discontinuity tests for some additional covariates.

The final identifying assumption is the exclusion restriction, which requires that a single-party majority cannot affect real interest rates except through its effect on the trajectory of fiscal policy, which can be summarized by changes in the debt level. We note that the exclusion restriction allows for the change in fiscal policy to affect other parts of the macroeconomy, which can in turn affect real interest rates, but these changes must be causally downstream from the change in fiscal policy. We examine this assumption in Section 4.

3 Results

Table 1 presents the main results from the fuzzy RD design framework in equations (2)–(4). The top panel contains results from the first stage, which estimates δ, the effect of a single-party majority on the change in the government debt-to-GDP ratio. The middle panel presents the results from the reduced form, which estimates τ, the effect of a single-party majority on long-term real interest rates. The bottom panel presents the main results of interest from the second-stage, the estimated DSIR $\hat{\beta}$, which is calculated as $\hat{\tau}/\hat{\delta}$.

Column 1 presents results from our baseline specification including covariates. The first-stage estimate implies that a single-party majority causes a decline of 17 percentage points in a country's debt-to-GDP ratio from the year of the election to five years later, relative to the counterfactual of no single-party majority. This estimate is statistically significant, with a bias-robust p-value of 0.02. It is also economically significant: the standard deviation of the five-year change in the debt-to-GDP ratio is 15.3 percentage points for the countries used in our baseline estimation post-1950, similar in magnitude to our estimated first-stage effect. Our RD design therefore provides strong support to the previous literature that argues non-single party majorities in parliamentary systems lead to greater public borrowing.[1] Our design removes the ambiguity related to the coding of various types of non-majority governments in the previous literature and strengthens the interpretation of a causal relationship between single-party majorities and greater fiscal discipline.

The estimated coefficient in column 1 of -0.99 in the reduced form equation implies that, relative to the counterfactual, a single-party majority causes real long-term interest rates to decline by 99 basis points from the year of the election to one year later. The estimated effect is again statisti-

[1]Data on government taxes and spending are not as widely available as the data on public debt in our sample. That limitation substantially inhibits our ability to study whether the decline in debt-to-GDP ratios associated with single-party majority governments arises primarily from changes in government spending or changes in tax receipts.

Table 1: Baseline Regression Discontinuity Design Estimates

	(1) **Baseline**	(2) No Covariates	(3) Winsorized
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP Ratios			
Debt-to-GDP ratio (pp)	-17.0	-16.6	-14.6
Cluster-robust standard error	9.0	9.1	7.9
Bias-robust p-value	0.02	0.07	0.02
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates			
Real long-rate (pp)	-0.99	-1.17	-0.94
Cluster-robust standard error	0.47	0.62	0.45
Bias-robust p-value	0.00	0.03	0.01
Second Stage ($\beta = 100 * \tau/\delta$): Debt Sensitivity of Interest Rates (DSIR)			
Real long-rate (bp)	5.8	7.0	6.4
Cluster-robust standard error	2.7	4.7	3.2
Bias-robust p-value	0.02	0.09	0.04
MSE-optimal bandwidth	6.9	6.0	7.0
CER-optimal bandwidth	5.7	4.9	5.8
Number of countries	26	26	26
Number of observations	336	336	336

Each column shows the results from the fuzzy regression discontinuity described in equations (2)–(4). The unit of observation is a country-election. Changes in debt-to-GDP ratios are calculated from the calendar year of an election to five years after the election, and the changes in real interest rates are calculated from the calendar year of an election to the calendar year after the election. Debt-to-GDP ratios and real interest rates are measured in percentage points, while the second-stage effect (DSIR) is measured as basis points impact per one percentage point increase in debt-to-GDP. The covariates in columns (1) and (3) include one lag each of real GDP growth rate, the short-term interest rate, a proxy for the expected inflation rate, and a crisis dummy. The specification in column (3) uses data that has been winsorized at the 1st and 99th percentiles for the changes in debt-to-GDP ratios and real interest rates. Point estimates are calculated using the optimal mean-squared error (MSE) optimal bandwidth, and confidence intervals and p-values are calculated using the coverage-error (CER) optimal bandwidth as recommended by Cattaneo et al. (2019b).

cally and economically significant: the standard deviation of the one-year change in real long-term interest rates is 1.68 percentage points for the countries in our sample.

Finally, the second-stage estimate is the DSIR, which is the ratio of the previous two estimates. It implies that, if single-party majorities affect interest rates only via changes in debt levels, a one percentage-point increase in the debt-to-GDP ratio causes real long-term interest rates to rise by 5.8 basis points, with a bias-robust p-value of 0.02.

Column 2 presents results from our baseline specification without covariates. The first-stage effect is slightly smaller than in column 1, pushing the estimated treatment effect to be slightly larger. The estimated DSIR is 7.0 basis points, so our baseline results are conservative compared with the results from this specification. Without covariates, though, this estimate is less precisely estimated, with a bias-robust p-value of 0.09.

To confirm that our results are not driven by outliers, column 3 presents a specification analogous to column 1, but with values for the changes in real long-term interest rates and the debt-to-GDP ratio winsorized at the 1st and 99th percentiles. The estimated DSIR increases slightly from the baseline estimate in column 1, and it remains statistically significant at standard confidence levels. We conclude that our results are not driven by outliers in the data.

We present and discuss results from a number of additional robustness checks in the Appendix. In Appendix B, we show that specific time periods and country income levels do not appear to drive our results and that our estimates are robust to alternative econometric specifications by considering alternative bandwidths and kernels. In Appendix C, we calculate estimates using a "local randomization" framework instead of the "local polynomial" framework in the RD design. In Appendix E, we present several additional robustness checks related to data sources and construction.

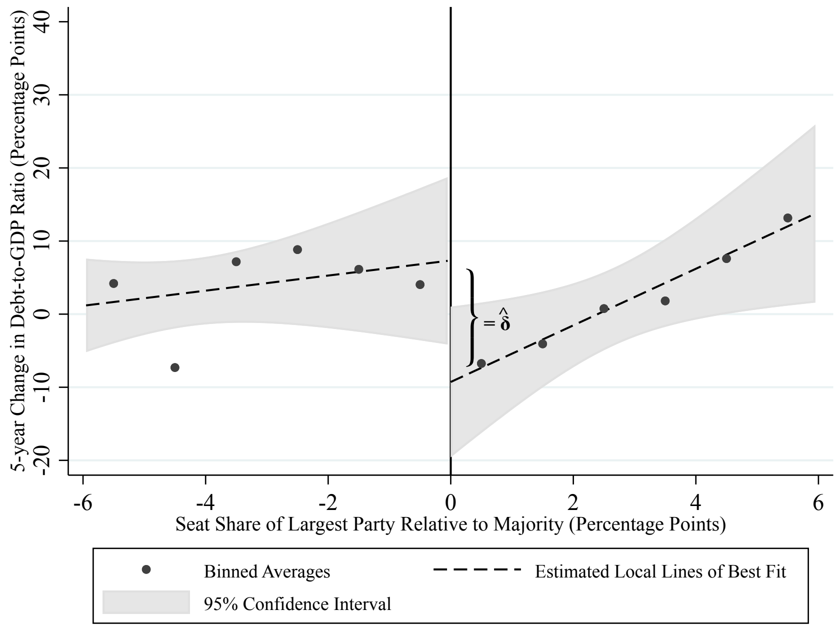
Figure 1 illustrates the estimation procedure in column 2 of Table 1, which, because it omits covariates, has a clearer graphical representation than our baseline specification. Figure 1a plots the five-year change in the debt-to-GDP ratio against the share of seats won by the largest party in a parliamentary election, binned in one-percentage point wide bins. The seat share is normalized so that zero corresponds to a bare majority. The dashed lines show the local linear polynomial $f(\cdot)$, which is estimated separately on each side of the cutoff for a majority, with the 95% confidence intervals shaded in gray. The regression discontinuity nonparametrically estimates the effect of a single-party majority, visually indicated by the dashed lines' vertical distance at the cutoff. The vertical distance in Figure 1a corresponds to the first-stage point estimate of 16.6 percentage points. Figure 1b plots the corresponding figure for the change in real long-term interest rates. The vertical distance between the two dashed lines at the cutoff represents the reduced-form discontinuity of 1.17 percentage points.

The evidence in this section suggests that the DSIR may be larger than most previous studies have estimated, raising the question of the extent to which our estimated DSIR reflects the traditional mechanism of "crowding out" from higher public borrowing versus other potential channels. One common model to think about the effects of government debt on real interest rates is to assume that the economy produces output according to an aggregate production function, and each dollar of government debt displaces, or "crowds out," some fraction of a dollar of productive capital formation (e.g., Ball et al., 1995; Elmendorf and Mankiw, 1999; Engen and Hubbard, 2004; Laubach, 2009; Gamber and Seliski, 2019). The higher marginal product of capital that results from this crowding out effect in turn drives up real interest rates. It is possible to derive a closed form expression for the crowding out effect in this model under a set of simplifying assumptions. Gamber and Seliski (2019) summarize a number of studies that have employed this approach, which generally suggest that the traditional crowding out channel can explain a DSIR of no more than 3.5 basis points. We therefore conclude that a substantial portion of our estimated DSIR of 5.8 basis points is likely to stem from other channels such as lower convenience yields (Krishnamurthy and Vissing-Jorgensen, 2012; Jiang et al., 2024), or increased risks related to inflation, currency depreciation, or default (e.g., Ardagna et al., 2007).

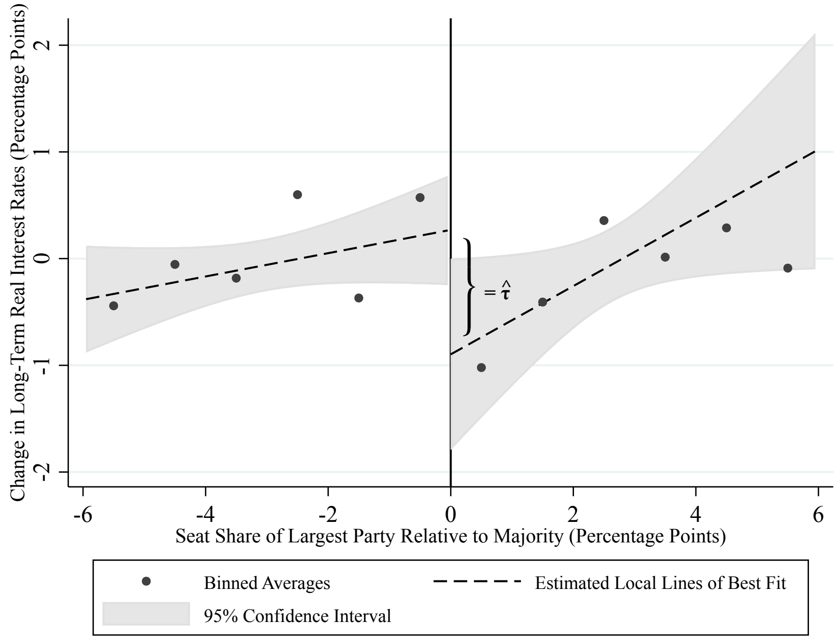
In Appendix F, we also estimate the effect of a higher debt-to-GDP ratio on the difference between long-term interest rates and economic growth rates, or "$R - G$". Our estimates are on the high end or above the estimates in the existing literature.

Figure 1: Single-Party Majorities, Debt-to-GDP Ratios, and Real Long-Term Interest Rates

(a) Single-Party Majorities and Debt-to-GDP Ratios



(b) Single-Party Majorities and Real Long-Term Interest Rates



The horizontal axes represent the seat share of the largest party relative to a bare majority. The vertical axis in panel (a) represents the change in the government's debt-to-GDP ratio from the year of the election to five years after the election, while the vertical axis in panel (b) represents the change in the long-term real interest rate from the year of the election to one year after the election. The dots represent averages for one percentage point wide bins with centroids within the estimation bandwidth. The black lines represent local polynomial estimates based on the specification in column (2) of Table 1, which does not include covariates. The shaded gray regions are 95% confidence intervals for the estimated local polynomials.

4 Examining the Exclusion Restriction and Potential Confounders

In this section, we examine the interpretation of our estimates as the causal effect of government debt levels on real interest rates. First, we consider potential violations of our exclusion restriction, the assumption that the presence or absence of single-party majorities affects real interest rates only through changes in fiscal policy, as summarized by changes in debt levels, and their downstream effects on the macroeconomy. Second, we examine whether our baseline estimates could be confounded by other factors that have been argued in the literature to affect the relationship between government debt levels and real interest rates. Third, we examine whether key electoral, institutional or political features of the countries in our dataset have an important influence on our results.

Figure 2 illustrates the responses of several macroeconomic and political variables to an election in which the largest party barely achieves a single-party majority, relative to barely missing a majority. Each point in the figure's panels shows the estimated coefficient from a sharp (i.e., single-stage) RD design of the form:

$$\Delta Y_{i,t,t+h} = \gamma_h + \delta_h M_{it} + f_h(V_{it} - \overline{c}_{it}) + \nu_{i,t,t+h} \tag{6}$$

$$M_{it} = 1[V_{it} \geq \overline{c}_{it}] \tag{7}$$

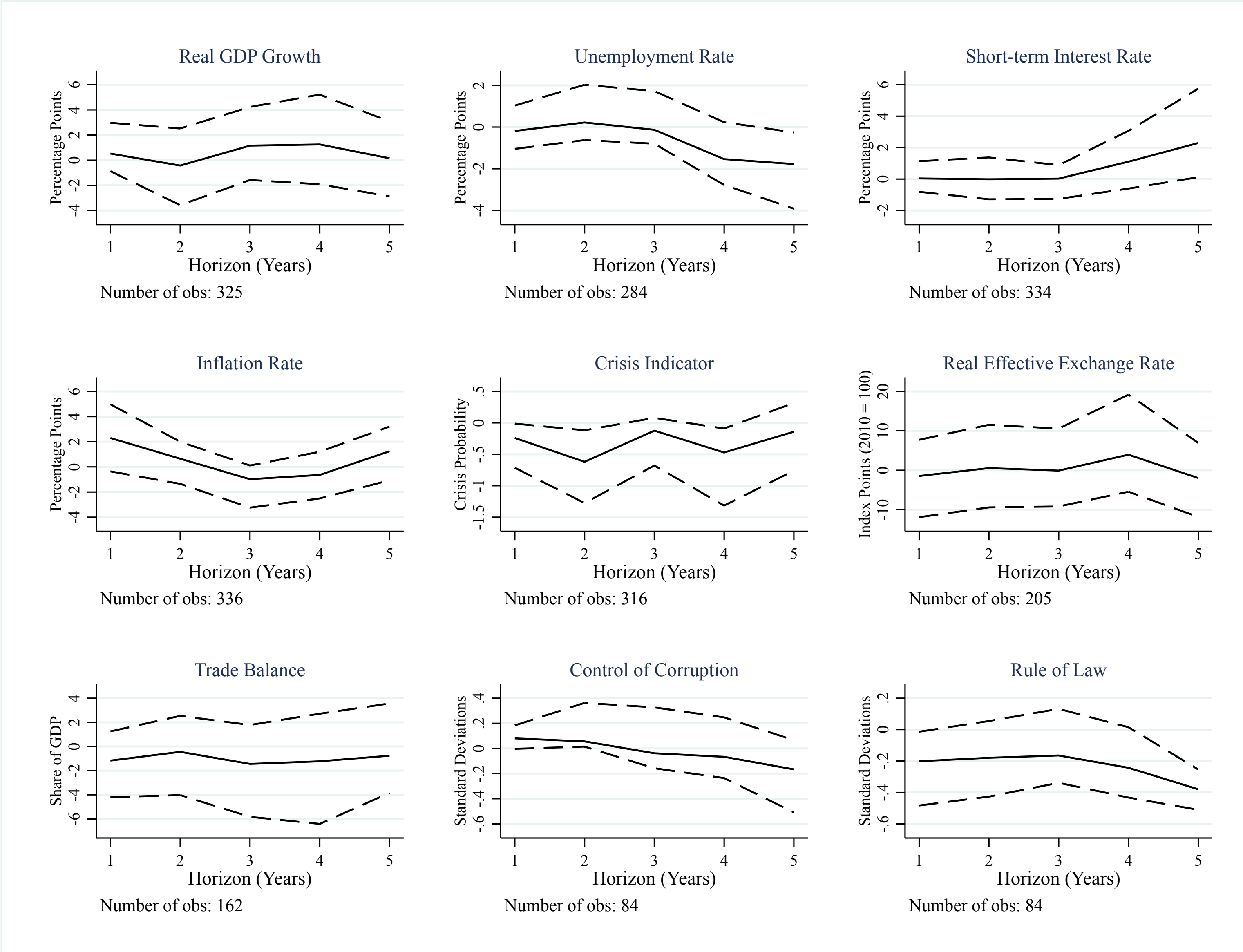
where $\Delta Y_{i,t,t+h} = Y_{i,t+h} - Y_{i,t}$ is the change in a macroeconomic outcome of interest at a horizon of h years after an election at time t in country i, and the other elements are defined analogously with equations (1)–(4). We estimate equations (6)–(7) separately for each horizon h from one to five years following an election for the four macroeconomic variables we use as controls in our baseline specification—the growth rate of real GDP, short-term interest rates, inflation, and an indicator for an economic crisis—plus the unemployment rate, real exchange rate, and the trade balance as a share of GDP. We also consider the response of two of the World Bank's Worldwide Governance Indicators (Kaufmann et al., 2011), "Rule of Law" and "Control of Corruption." We include the same covariates in the estimates of equation (B.1) in Figure 2 as we do in our baseline fuzzy RD specification in column (1) of Table 1.

We interpret the exclusion restriction as implying that close elections can affect real interest rates only by changing the trajectory of fiscal policy and debt levels, but these changes in debt can have downstream effects on macroeconomic outcomes that in turn affect real rates. Therefore, observing meaningful effects on the macroeconomic variables in Figure 2 would not necessarily indicate a violation of the exclusion restriction.

Nonetheless, we do not detect clear-cut effects in Figure 2, suggesting that most macroeconomic variables do not respond in a visibly clear way to the presence or absence of a single-party majority in the five years following an election. The 95-percent confidence regions in the figure almost always span zero, and the point estimates are generally close to zero as well. One exception is that the crisis indicator displays a statistically significant decline in the second year following an election, but we note that we consider nine outcome variables in Figure 2 and five time horizons for each variable, so we would expect some estimated responses to be statistically significant by random chance. The unemployment rate also appears to decline and short-term interest rates appear to rise four to five years after an election, although the confidence bands are wide in those years and continue to encompass zero effect.

Kaufmann et al. (2011) classify the World Bank's Rule of Law and Control of Corruption indicators as the two indicators in the Worldwide Governance Indicators that correspond to "The respect of citizens and the state for the institutions that govern economic and social interactions among them..." Changes in the Rule of Law and Control of Corruption following close elections could therefore indicate that coalition governments versus single-party majorities affect the macroeconomy and real interest rates through channels outside of fiscal policy. Generally speaking, both indices decline slightly following the election of a single-party majority, but the changes are largely statistically insignificant. An exception is that the decline in the Rule of Law index is statistically significant in the fifth year following the election. We note again that we would expect some estimated responses to differ from zero by random chance. Overall, we interpret Figure 2 suggesting that close elections do not have a clear effect on real interest rates through channels outside of fiscal policy or debt.

Figure 2: Responses of Macroeconomic and Institutional Variables to Single-Party Majorities



Note: Each point in the figure corresponds to a sharp RD estimate of the effect of a single-party majority on the macroeconomic outcome in the panel title from one to five years following the election. The RD designs are estimated in changes from the year of the election to h years after the election. All RD specifications include the baseline covariates from column (1) of Table 1. We limit the sample in each RD specification to keep the set of observations included constant across time horizons. Dashed lines display 95-percent confidence intervals.

Our interpretation of the evidence is that the exclusion restriction is likely to be a reasonable approximation in our context, but we acknowledge that it is a strong assumption. As an additional caveat, we note that the DSIR may depend on whether the increase in debt arises due to lower taxes or higher spending. Due to data limitations, we do not examine that question here, but interpret our estimated DSIR as a weighted average of both sets of changes.

Although in principle our RD design should deliver an "as good as locally random experiment," it is nonetheless possible that other factors are confounding our results by random chance in our finite sample. To assess this possibility, we again augment our baseline specification with additional control variables that could confound our estimates.

Table 2 presents the results of our analysis, with the first column reproducing our baseline specification. Column 2 includes a country's lagged debt-to-GDP ratio as an additional control variable to investigate whether the initial level of public debt affects our baseline results. The first-stage and reduced-form point estimates both decline, and the estimated DSIR is slightly larger than the baseline estimate. Column 3 includes the international capital controls dummy of Ilzetzki et al. (2019), which equals one if there is more than one exchange rate in the economy. Column 4 includes the coarse exchange rate regime classification of Ilzetzki et al. (2019).[2] Column 5 includes a dummy variable for whether the country was a member of the Euro Area at the time of the election. The estimated DSIRs in columns 3–5 are all relatively close to our baseline estimate, with the baseline estimate of 5.8 basis points being on the lower end. Finally, in column 6, we include a dummy variable for countries that are currently members of the Organization for Economic Cooperation and Development (OECD). The estimates are very similar to our baseline sample, although this result could reflect the small number (three) of non-OECD countries in our sample.

[2]This measure takes value 1 for the "least flexible" exchange rate regimes, which feature pegs, value 2 for "gradualist adjustment" regimes, value 3 for "broad bands and managed floating" regimes, value 4 for freely floating regimes, and values 5 and 6 for different types of "anchorless" regimes.

Table 2: Examining Potential Confounders

	(1) Baseline	(2) One-Year Lagged Debt-to-GDP Ratio	(3) One-year Lagged Capital Controls	(4) One-Year Lagged Exchange Rate Regime	(5) Euro Area Member	(6) OECD Member
	First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP Ratios					
Debt-to-GDP ratio (pp)	-17.0	-11.7	-14.1	-18.8	-13.9	-17.4
Cluster-robust standard error	9.0	7.5	7.5	8.6	8.9	9.1
Bias-robust p-value	0.02	0.06	0.08	0.02	0.04	0.02
	Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates					
Real long-rate (pp)	-0.99	-0.76	-0.97	-1.07	-0.90	-1.01
Cluster-robust standard error	0.47	0.45	0.44	0.51	0.51	0.47
Bias-robust p-value	0.00	0.02	0.01	0.01	0.02	0.00
	Second Stage ($\beta = 100 * \tau/\delta$): Debt Sensitivity of Interest Rates (DSIR)					
Real long-rate (bp)	5.8	6.5	6.9	5.7	6.5	5.8
Cluster-robust standard error	2.7	4.4	5.1	2.6	3.9	2.6
Bias-robust p-value	0.02	0.09	0.10	0.01	0.08	0.01
MSE-optimal bandwidth	6.9	7.8	5.3	5.4	7.9	6.9
CER-optimal bandwidth	5.7	6.5	4.4	4.5	6.6	5.7
Number of countries	26	26	25	25	26	26
Number of observations	336	336	334	334	336	336

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity that corresponds to the baseline specification in Table 1, including one lag each of the real GDP growth rate, the short-term interest rate, a proxy for the expected inflation rate, and the crisis dummy, but with one alteration in each column except column (1). Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Columns (2) through (6) include the baseline covariates as well as the variables in the column headers. Columns (1), (2), (5), and (6) use data from the baseline specification in Table 1. Columns (3) and (4) supplement the baseline specification data with data on exchange rate arrangements and capital controls from Ilzetzki et al. (2019).

Finally, in Table 3, we examine how some key electoral, institutional or political features of the countries in our dataset influence our results. Column 1 once again reproduces our baseline estimates for convenience.

In column 2, we consider countries' electoral systems. Our estimation strategy relies on the number of seats won by the largest party as its running variable. The literature on coalition governments and fiscal policy focuses on bargaining among parties in parliament, and our discontinuity of interest lies naturally in the share of seats won by the largest party, rather than in the share of votes that it receives. Indeed, there is no reason to expect a discontinuous change in fiscal policy at the 50-percent vote threshold. Nonetheless, countries use a wide variety of rules to allocate parliamentary seats from underlying popular votes. Bormann and Golder (2022) code democratic countries' election rules into three major categories, "majoritarian," "proportional," and "mixed." Majoritarian systems produce single-party majorities significantly more frequently than the other systems in our dataset, so differences between these systems could in principle influence our results. We add fixed effects for majoritarian and proportional electoral systems to our baseline specification in column 2 of Table 3. The resulting estimate of the DSIR is 7.0 basis points, a bit larger than our baseline estimate.

Motivated by similar concerns, in column 3 we exclude observations in which the electoral system features a "majority bonus system." As Bormann and Golder (2022) describe, "This institutional innovation grants the largest party or coalition additional legislative seats to facilitate the government formation process and ensure government stability. ...On the whole, this is a relatively recent institutional innovation that only a handful of countries such as Greece, Italy, and San Marino have adopted in the 2000s and 2010s." These systems could introduce variation in the number of seats won by a political party that does not vary smoothly with its vote share, which could reduce the appropriateness of our "local randomization" assumption. Yet only four observations in our baseline dataset feature a majority bonus system: Italy in 2006, 2008, and 2013, and Greece in 2015. In column 3 of Table 3, we exclude those observations; our estimated DSIR is 6.1 basis points, close to our baseline estimate. We interpret the results in columns 2 and 3 as suggesting that our estimates are not meaningfully affected by differences in electoral systems or procedures for allocating parliamentary seats.

In column 4, we control for the ideology of the government formed after each election in our dataset using a measure of the "Ideological Complexion of Government and Parliament" coded by Seki and Williams (2014).[3] In principle, the ideologies of the governments formed after close elections could vary systematically depending on whether the election produced a single-party majority, which might affect real interest rates independently of debt levels. Yet we find that controlling for the ideological complexion of the government does not meaningfully change our baseline results. In column 5, we exclude observations featuring a balanced budget rule (BBR) because the presence of a BBR may remove the first stage in our fuzzy RD design. We use the coding of Asatryan et al. (2018), who emphasize that BBRs must be constitutional to have meaningful effects on the debt-to-GDP ratio.[4] The estimated first stage from this exercise is slightly larger than in our baseline sample, consistent with the intuition that BBRs might limit election outcomes' effects on debt. The estimated DSIR is very slightly smaller than in our baseline sample.

We interpret the evidence in this section as suggesting that violations of the exclusion restriction or confounding from factors that are not driven by changes in fiscal policy are unlikely to affect our estimate of the DSIR meaningfully. Rather, single-party majorities appear to affect real interest rates primarily through their effects on fiscal policy.

[3]The measure takes values one through five, with a value of one corresponding to "right-wing dominance," two corresponding to "right-center complexion," three corresponding to "balanced situation," four corresponding to "left-center complexion," and five corresponding to "left-wing dominance."

[4]We use the broadest sample of countries coded as having a BBR in Asatryan et al. (2018).

Table 3: Regression Discontinuity Design Estimates–Influence of Electoral, Institutional, and Political Factors

	(1) **Baseline**	(2) Electoral System Effects	(3) Exclude Majority Bonuses	(4) Ideology of Government	(5) Balanced Budget Amendment
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP Ratios					
Debt-to-GDP ratio (pp)	-17.0	-15.3	-16.4	-19.2	-18.0
Cluster-robust standard error	9.0	8.4	9.0	9.3	8.7
Bias-robust p-value	0.02	0.05	0.02	0.03	0.01
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates					
Real long-rate (pp)	-0.99	-1.08	-1.00	-1.12	-0.98
Cluster-robust standard error	0.47	0.48	0.48	0.52	0.47
Bias-robust p-value	0.00	0.01	0.01	0.00	0.01
Second Stage ($\beta = 100 * \tau/\delta$): Debt Sensitivity of Interest Rates (DSIR)					
Real long-rate (bp)	5.8	7.0	6.1	5.9	5.5
Cluster-robust standard error	2.7	3.6	2.9	2.9	2.6
Bias-robust p-value	0.02	0.04	0.02	0.01	0.02
MSE-optimal bandwidth	6.9	6.3	7.0	5.9	7.3
CER-optimal bandwidth	5.7	5.2	5.8	4.9	6.0
Number of countries	26	26	26	24	24
Number of observations	336	336	332	325	308

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity design that corresponds to the baseline specification in Table 1, but with one alteration at a time. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Column (2) adds a set of fixed effects for the major electoral systems as coded by Bormann and Golder (2022). Column (3) drops observations that feature majority bonuses as coded by Bormann and Golder (2022). Column (4) includes an additional control for the ideology of the government from Seki and Williams (2014). Column (5) excludes country-years that featured a Balanced Budget Amendment as coded by Asatryan et al. (2018).

5 Conclusion

The extent to which higher public debt levels increase real interest rates is a longstanding academic question with significant contemporary policy importance. The question remains a topic of active research partly because of the many challenges to estimating the causal relationship.

We use close parliamentary elections as natural experiments to estimate the effects of public debt levels on real long-term interest rates with a fuzzy regression discontinuity design. We first estimate that the failure of an election to produce a single-party majority causes the debt-to-GDP ratio to increase by roughly 17 percentage points over the following five years. Our results thus provide causal support for the literature arguing that single-party governments in parliamentary systems produce more fiscal discipline than coalition and minority governments.

Our primary result is our estimated DSIR, which implies that if single-party majorities affect interest rates only via changes in debt levels, a one percentage point increase in the debt-to-GDP ratio causes long-term real interest rates to increase by 5.8 basis points. That estimate is at the top end of the range in the previous literature. The relatively large effect we estimate highlights the potential relevance of reverse causality from low interest rates toward greater government borrowing.

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APPENDIX FOR ONLINE PUBLICATION

A Qualitative Background

In this section of the Appendix, we provide background for our claims that low interest rates are often interpreted as an argument that it is a good time to borrow and that coalition and minority governments tend to produce less fiscal discipline than single-party majorities.

A.1 Anecdotal Evidence of Reverse Causality between Interest Rates and Debt

We first provide brief anecdotal evidence from politicians, policymakers, and economists suggesting that there is a commonly held belief that low interest rates provide an argument for greater government borrowing. We begin with some statements from prominent politicians:

"We'll get a fund, we'll make a phenomenal deal with the low interest rates and rebuild our infrastructure."—*Republican Presidential Nominee Donald Trump, August 2016* (Trump, 2016)

"If it's borrowing to finance great infrastructure projects, and there's the opportunity to borrow at low rates, to do things for the long-term benefit of the country, then we should do them."—*U.K. Member of Parliament Boris Johnson, June 2019* (Walker, 2019)

"He told me, 'Think big,' because the interest rates are low,"—*Speaker of the House Nancy Pelosi, recounting a conversation with Federal Reserve Chair Jerome Powell, March 2020* (Irwin, 2020)

Next, we offer some quotations from prominent economists suggesting that this belief is also widespread within the economics profession:

"Put these two facts together — big needs for public investment, and very low interest rates — and it suggests not just that we should be borrowing to invest, but that this investment might well pay for itself even in purely fiscal terms."—*Paul Krugman, August 2016* (Krugman, 2016)

"We argue that many—though not all—of the factors that may be contributing to the historically low level of interest rates imply that both federal debt and federal investment should be substantially larger than they would be otherwise."—*Douglas Elmendorf and Louise Sheiner, Summer 2017* (Elmendorf and Sheiner, 2017)

As acknowledged by Elmendorf and Sheiner (2017) and also argued by Garín et al. (2019), the logic that low interest rates make it more advantageous for governments to issue debt depends in principle on the reasons why interest rates are low. Nonetheless, this qualitative evidence illustrates our concern that there may be reverse causality between interest rates and government debt levels that could confound previous estimates of the debt sensitivity of interest rates (DSIR).

A.2 Political Economy Determinants of Public Debt Levels

Roubini and Sachs (1989a) describe the tendency for coalition governments to produce less fiscal discipline as follows:

> Why do coalition governments find it hard to balance the budget? ... First, the individual coalition partners in multi-party governments have distinctive interests and distinctive constituencies. There is no single uniform objective function for the various political parties in the government. There is likely to be a fundamental prisoner's dilemma with respect to budget cuts: all of the partners of the coalition may prefer comprehensive budget cuts to a continuation of large deficits, but each coalition partner may have an incentive to protect its particular part of the budget against the austerity measures. ... Second, individual coalition partners will often have enormous power to prevent a change in the status quo, though they will not typically have the power by themselves to implement a positive program of change. ... Third, the enforcement mechanisms among coalition partners to assure the cooperative outcome will often be very weak. (Roubini and Sachs, 1989a, p. 924)

There are several qualitative case studies supporting this hypothesis. Alesina and Drazen (1991) describe a series of historical episodes illustrating this pattern. Describing interwar France, they note:

> The election of an internally divided "cartel des gauches" in the spring of 1924 initiated an additional period of fiscal instability. An endless debate within the leftist coalition on the imposition of a capital levy and the consequent fiscal inaction implied a further deterioration of the floating-debt problem. ... The successful stabilizations in France (1926) and Italy (1922–1924) coincided with a clear consolidation of power by the right. (Alesina and Drazen, 1991, pp. 1172–1173)

They argue that similar dynamics apply in many other settings:

> Several authors have suggested that the recent increase in the debt/GNP ratios in several OECD economies is due to the failure of weak and divided coalition governments to agree on fiscal-adjustment programs... The cases of Belgium, Ireland, and Italy, the three OECD countries currently with the highest debt/GNP ratio are good examples of this point of view. In several Latin American countries, and particularly in Argentina, the failure to stabilize in the face of endemic inflation has gone hand in hand with continued political polarization and instability and the failure of any group to consolidate its power effectively... (Alesina and Drazen, 1991, p. 1173)

Ihori (2006) describes similar dynamics at work in Japan's recent fiscal policy:

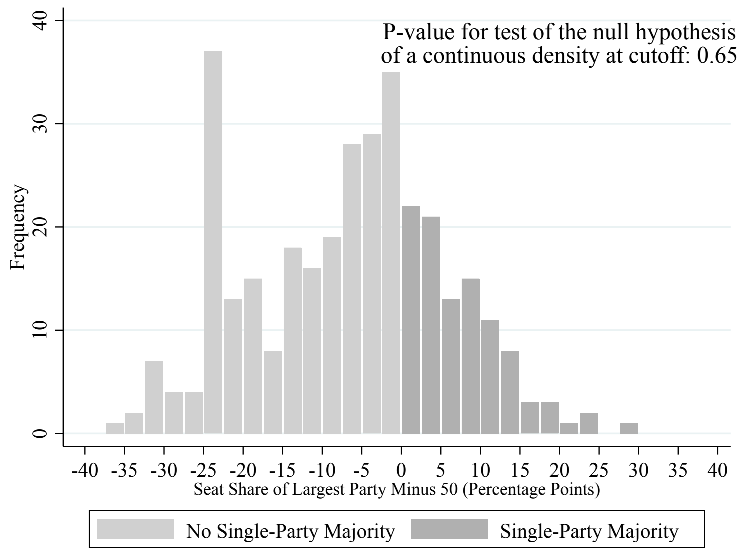
> Third, fiscal consolidation and other structural reforms were put off because of short-term benefits needed by the coalition governments. In the autumn of 1999 Komeito joined the coalition administration of the Liberal Democratic Party and the Liberal Party. As a result, the three parties secured a combined majority in the House of Councilors (Upper House) as well. The overriding objective of the three party coalition at the time was, as stated by Prime Minister Obuchi, to maintain a numerical advantage in the Diet. Given its low public approval ratings, however, the ruling alliance faced a pressing need to produce results quickly to gain public support. This was particularly true of Komeito, which needed even more urgently to deliver short-term achievements because of its emphasis on welfare-related spending. Such political pressures set the stage for free-spending policy. That is why fiscal reform did not make progress under the Obuchi and Mori administrations. (Ihori, 2006, p. 506)

Our study complements the qualitative and econometric literature on coalition governments and debt levels by exploiting quasi-experimental evidence from a regression discontinuity design.

B Additional Validity, Falsification, and Robustness Tests of the RD Design

In this section of the Appendix, we present the results of validity, falsification, and robustness assessments of our RD design.

Figure B.1: Histogram of Seat Share



Note: The figure displays a histogram of frequency counts of the election results used in the baseline estimation in Table 1. A test of the null hypothesis that the density of election results is continuous at the cutoff for a single-party majority, as described in Cattaneo et al. (2018), gives test statistic of -0.46, with a p-value of 0.65, indicating that we cannot reject the null hypothesis at standard confidence levels. We interpret this result as suggesting that the distribution of results does not show evidence of manipulation around the cutoff.

B.1 Validity and Falsification Tests of the RD Design

This section presents results from common falsification tests of the local randomization assumption in the RD design. The first tests for a discontinuity in the distribution of the share of parliamentary seats won by the largest party around the cutoff for a majority. The second tests for discontinuities in predetermined variables of interest at the cutoff. The third tests our main specification using placebo cutoff values for the seat share, and the fourth tests for discontinuities at the cutoff in some additional correlates of election outcomes.

Figure B.1 shows a histogram of the frequency counts of the share of seats won by the largest party relative to a simple majority over our baseline sample. Statistically testing for a discontinuity at the cutoff gives a p-value of 0.65, suggesting that the test cannot reject the null hypothesis of no difference in the density function on either side of the cutoff.[5]

[5]The test-statistic and p-value are implemented using the `rddensity` package (Cattaneo et al., 2018, 2019a) with local polynomials of order one. We used our baseline sample, including only observations that are used in the RD estimation, to estimate the density function.

We next present falsification tests for discontinuities in the predetermined covariates in the spirit of Lee (2008). If the local randomization result holds, there should be no systematic sorting of outcomes that were determined prior to each election. It follows that we should not observe discontinuities in predetermined variables at the cutoff. To test whether we observe such discontinuities, we estimate a series of "sharp" regression-discontinuity tests of the values of six predetermined variables. For each predetermined variable Y, we estimate systems of equations of the form:

$$Y_{it} = \gamma + \delta M_{it} + f(V_{it} - \overline{c}_{it}) + \nu_{it} \tag{B.1}$$

$$M_{it} = 1[V_{it} \geq \overline{c}_{it}] \tag{B.2}$$

where the other elements are defined analogously with equations (1)–(4).

Table B.1 presents results from the sharp RD estimation of equations (B.1) and (B.2). We test using the lagged value of each of six variables: real GDP growth, consumer price inflation, presence of an economic crisis, short-term interest rates, real long-term interest rates, and the debt-to-GDP ratio. This set comprises the four covariates we use in our baseline RD estimation as well as the two main elements of our estimation procedure.[6] The tests do not reject the null hypothesis of continuity at the cutoff, consistent with the notion of local randomization.

[6]We again implement the regression discontinuity tests using the `rdrobust` package of Calonico et al. (2014) using the same options as in our main estimation procedure, but using a sharp design rather than a fuzzy design.

Table B.1: Regression Discontinuity Falsification Tests for Predetermined Covariates and Main RD Outcomes

	(1) Real GDP growth	(2) Inflation rate	(3) Economic Crisis	(4) Short rate	(5) Real long rate	(6) Debt-to-GDP
Sharp RD: Effect of Single-Party Majority on One-Year Lagged Predetermined Covariates						
Coefficient	-0.07	1.15	0.14	1.14	0.77	24.77
Clustered standard error	0.87	1.43	0.27	1.51	0.97	22.79
Bias-robust p-value	0.90	0.31	0.53	0.36	0.28	0.23
MSE-optimal bandwidth	10.4	7.4	6.5	9.1	7.8	6.5
CER-optimal bandwidth	8.6	6.1	5.4	7.6	6.5	5.4
Number of observations	336	336	336	336	335	336
Number of countries	26	26	26	26	26	26

The unit of observation is a country-election. Each column shows the results from a sharp regression discontinuity regression of the variable in the column header on the vote share of the largest party minus the threshold to reach a majority as described in section 2.2. The dataset matches the data used in the baseline regression specification in column (1) of Table 1.

We next present an additional falsification test, in which we replace the true cutoff with placebo cutoff values. Our null hypothesis is that we should not estimate any statistically significant discontinuities at these alternative cutoff values. Table B.2 summarizes the results using placebo cutoffs. We avoid any contamination of our results by the discontinuity at the true cutoff by restricting the sample in each test to a single side of the cutoff. For positive placebo cutoff values, we only consider instances of single-party majorities and for negative placebo cutoff values we consider only instances where there was no single-party majority. Column 4 presents our baseline results, with the cutoff of $c = 0$. Columns 3 and 5 consider cutoffs of $\pm 1.5\%$, the smallest cutoff for which we can estimate our RD design (our sample does not include enough observations on a single side of the cutoff to estimate our RD design at cutoffs of $c < |1.5|$). The remaining columns change the cutoff by increments of one percentage point. The RD results are statistically significant only when we use the correct cutoff ($c = 0$) and are not statistically significant for the placebo cutoffs.[7] These

[7]Cutoffs outside $\pm 3.5\%$ once again run into convergence issues since there are insufficient observations to one side of the cutoff.

results suggest that our baseline estimates using the true cutoff value are unlikely to be driven by spurious random variation near the cutoff.

Table B.2: Regression Discontinuity Falsification Tests for True and Placebo Cutoffs

	(1) c=-3.5	(2) c=-2.5	(3) c=-1.5	(4) c=0	(5) c=+1.5	(6) c=+2.5	(7) c=+3.5
Second Stage ($\beta = 100 * \tau/\delta$): Debt Sensitivity of Interest Rates (DSIR)							
Real long-rate (bp)	-1.9	-22.1	4.6	5.8	1.9	-3.5	37.4
Cluster-robust standard error	3.4	26.8	11.5	2.7	4.2	8.9	138.8
Bias-robust p-value	0.30	0.54	0.52	0.02	0.64	0.61	0.29
MSE-optimal bandwidth	3.0	2.9	4.0	6.9	2.0	1.4	2.4
CER-optimal bandwidth	2.5	2.5	3.3	5.7	1.7	1.2	2.1
No. obs left	33	34	52	85	13	16	24
No. obs right	43	32	15	52	20	11	15

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity that corresponds to the baseline specification in column (1) of Table 1, but with an alternative cutoff for the seat share. Column (4) corresponds exactly to the baseline specification of column (1) in Table 1. The specifications in the other columns change the cutoff to the number shown in the column headers. For negative cutoffs, only election results that do not lead to a single-party majority are included. For positive cutoffs, only election results that do lead to a single-party majority are included.

Table B.3 displays the results of sharp RD cutoff tests analogous to the results in Table B.1 for some additional potential correlates of election results. No discontinuity is statistically distinguishable from zero at standard confidence levels in this set of additional variables.

Table B.3: Regression Discontinuity Design Falsification Tests for Additional Covariates

	(1) Current or Future Crisis	(2) Lagged Exchange Rate	(3) Lagged Capital Control	(4) Current Ideology
Sharp RD: Effect of Single-Party Majority on Additional Covariates				
Coefficient	-0.05	0.25	0.34	0.03
Clustered standard error	0.21	0.60	0.25	0.50
Bias-robust p-value	0.93	0.64	0.13	0.92
MSE-optimal bandwidth	6.5	8.2	6.8	8.4
CER-optimal bandwidth	5.4	6.8	5.6	7.0
Number of observations	336	334	334	325
Number of countries	26	25	25	24

The unit of observation is a country-election. Each column shows the results from a sharp regression discontinuity regression of the variable in the column header on the vote share of the largest party minus the threshold to reach a majority as described in section 2.2. The election dataset matches the data used in the baseline regression specification in column (1) of Table 1. The exchange rate and capital control data in columns (2) and (3) is from Ilzetzki et al. (2019). The ideology of the government variable in column (4) is from Seki and Williams (2014).

B.2 Influence of Time Periods and Country Characteristics

In this section, we consider the robustness of our results to time periods, countries' economic and institutional characteristics, and particular observations. Table B.4 examines the influence of particular time periods. Column 2 drops elections occurring from 2003 to 2010, so that for elections prior to the onset of the Great Recession, the five-year change in the debt-to-GDP ratio ends prior to 2008. The estimated effect of an increase in the debt-to-GDP ratio rises slightly relative to the baseline sample, but the statistical significance of the estimate is weaker, consistent with the smaller sample size. Column 3 drops election-year observations after 2014 so that the five-year change in the debt-to-GDP ratio ends in 2019. This restriction ensures that our results are not biased by the large run-up in public debt associated with the response to the pandemic. The estimates are very similar to the baseline specification, consistent with the fact that only a handful of observations in our sample occurred after 2014. Column 4 adds a set of period effects for observations from the years 1950–1971 and 1972–1990 (we omit a dummy for the latest period, 1991 onward, due to collinearity). These rough geopolitical cutoffs are meant to account for the possibility that our RD specification is picking up the influence of special episodes, such as the European stagflation beginning in the 1970s. The estimated DSIR is 8.1 basis points, larger than our baseline and still significant at the 5-percent level. Similarly, in column 5, we include a set of decade fixed effects. The estimated DSIR is 8.5 basis points, but it is noticeably less precisely estimated. Finally, in column 6, we include a linear time trend as a covariate to account for the possibility that our estimates may simply be picking up two concurrent but causally unrelated time trends. The results are similar to those in columns 4 and 5, with a larger but less precise point estimate than in our baseline specification. We conclude that specific time periods or concurrent trends do not appear to drive our estimate of the DSIR.

Table B.4: Regression Discontinuity Design Estimates - Influence of Time Periods

	(1) Baseline	(2) No Great Recession	(3) No Covid-19	(4) Period FEs	(5) Decade FEs	(6) Time Trend
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP Ratios						
Debt-to-GDP ratio (pp)	-17.0	-13.5	-18.9	-13.5	-11.6	-10.9
Cluster-robust standard error	9.0	7.7	10.0	7.7	7.1	8.3
Bias-robust p-value	0.02	0.04	0.03	0.05	0.22	0.09
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates						
Real long-rate (pp)	-0.99	-0.89	-1.10	-1.09	-0.98	-0.90
Cluster-robust standard error	0.47	0.52	0.50	0.48	0.38	0.50
Bias-robust p-value	0.00	0.03	0.00	0.01	0.00	0.01
Second Stage ($\beta = 100 * \tau/\delta$): Debt Sensitivity of Interest Rates (DSIR)						
Real long-rate (bp)	5.8	6.6	5.8	8.1	8.5	8.2
Cluster-robust standard error	2.7	4.1	2.7	4.2	9.2	5.7
Bias-robust p-value	0.02	0.09	0.02	0.04	0.13	0.12
MSE-optimal bandwidth	6.9	7.7	6.3	6.7	4.9	7.9
CER-optimal bandwidth	5.7	6.4	5.3	5.6	4.1	6.6
Number of countries	26	25	26	26	26	26
Number of observations	336	288	332	336	336	336

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity design that corresponds to the baseline specification in Table 1, but with one alteration at a time. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Column (2) drops elections from 2003 to 2010 so that the five-year change in the Debt-to-GDP ratio ends in 2008, prior to the large increase in debt associated with the Great Recession. Column (3) drops elections after 2014 so that the five-year change in the Debt-to-GDP ratio ends in 2019, prior to the Covid-19 pandemic. Column (4) includes dummies for the periods 1950–1971 and 1972–1990. Column (5) includes decade fixed effects, and column (6) includes a linear time trend.

In Table B.5, we examine the question of whether particular observations may be driving our results. We use a triangular kernel to weight the local linear regressions in our baseline specification, consistent with the result of Fan and Gijbels (1996) that it is the optimal kernel in our context.[8] The triangular kernel assigns the highest weights to observations that are closest to the

[8]Appendix Table B.7 below shows that our estimates are robust to using other kernels.

cutoff. Therefore, it is possible for a small number of observations to have high leverage and a substantial influence on the results. We identify the relative importance of the individual observations in our data set by running a series of 336 RD regressions in which we drop each observation in the baseline sample individually. We then consider the absolute value of the change in the estimated second-stage effect from our baseline estimate.

Table B.5 displays the estimates for the five observations whose absence most affects the baseline estimates: New Zealand in 1991, the United Kingdom in 1950, the United Kingdom in 1974, New Zealand in 1981, and Belgium in 1950. Individually dropping either of the two observations that have the largest individual effects leads to a larger second-stage estimate, as seen in columns 2 and 3, while individually dropping any of the other three observations leads to a smaller estimate, as seen in columns 4 through 6. The largest absolute change in the second-stage point estimate is 2 basis points. We conclude that, although our design and sample size allow for individual observations to have noticeable effects on our estimate, there is not a clear pattern for these influential observations to affect our results in a uniform way.

Table B.6 examines whether countries' level of economic development, as measured by real GDP per capita, influences our results. In column 2, we exclude countries that were outside the top 50 richest countries as measured by real GDP per capita in 1990 in the World Bank's World Development Indicators (World Bank, 2025). In our sample, that restriction excludes Poland, South Africa, Mauritius, and India. In column 3, we drop countries that were outside the top 50 richest as of 2015 as measured by real GDP per capita in 2015 in the World Development Indicators (World Bank, 2025), which excludes Poland, Hungary, Greece, South Africa, Mauritius, and India. In column 4, we include a dummy variable for the top half of countries in our sample as measured by real GDP per capita in 1990. In column 5, we include a dummy variable for the top half of countries in our sample as measured by real GDP per capita in 2015. Our results are fairly stable across these specifications, with estimated DSIRs ranging from 5.5 to 6.5 basis points. We therefore conclude that country income does not appear to have an important influence on our estimate of the DSIR.

Table B.5: Regression Discontinuity Design Estimates–Influence of Particular Observations

	(1) **Baseline**	(2) Excl. New Zealand 1993	(3) Excl. United Kingdom 1950	(4) Excl. United Kingdom 1974	(5) Excl. New Zealand 1981	(6) Excl. Belgium 1950
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP Ratios						
Debt-to-GDP ratio (pp)	-17.0	-15.1	-10.6	-18.6	-21.8	-17.0
Cluster-robust standard error	9.0	10.4	6.9	10.0	8.2	9.4
Bias-robust p-value	0.02	0.07	0.05	0.05	0.00	0.02
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates						
Real long-rate (pp)	-0.99	-1.17	-0.80	-0.79	-0.95	-0.79
Cluster-robust standard error	0.47	0.41	0.50	0.38	0.58	0.49
Bias-robust p-value	0.00	0.00	0.03	0.01	0.03	0.02
Second Stage ($\beta = 100 * \tau/\delta$): Debt Sensitivity of Interest Rates (DSIR)						
Real long-rate (bp)	5.8	7.8	7.5	4.3	4.4	4.6
Cluster-robust standard error	2.7	5.0	5.1	3.2	2.6	2.2
Bias-robust p-value	0.02	0.10	0.12	0.12	0.07	0.01
MSE-optimal bandwidth	6.9	7.2	8.1	5.7	6.5	7.1
CER-optimal bandwidth	5.7	6.0	6.8	4.7	5.4	5.9
Number of countries	26	26	26	26	26	26
Number of observations	336	335	335	335	335	335

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity design that corresponds to the baseline specification in Table 1, but with one alteration at a time. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Each subsequent column drops the observation noted in the column header. These observations had the largest absolute effect on the estimated DSIR out of the 336 observations in our sample.

Table B.6: Regression Discontinuity Design Estimates - Country Income

	(1) Baseline	(2) No Low Income in 1990	(3) No Low Income in 2015	(4) Dummy for Top-Half Income 1990	(5) Dummy for Top-Half Income 2015
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP Ratios					
Debt-to-GDP ratio (pp)	-17.0	-18.9	-18.4	-16.7	-17.4
Cluster-robust standard error	9.0	9.2	9.1	8.8	9.3
Bias-robust p-value	0.02	0.04	0.03	0.01	0.03
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates					
Real long-rate (pp)	-0.99	-1.10	-1.20	-0.91	-1.03
Cluster-robust standard error	0.47	0.51	0.51	0.48	0.47
Bias-robust p-value	0.00	0.01	0.00	0.01	0.01
Second Stage ($\beta = 100 * \tau/\delta$): Debt Sensitivity of Interest Rates (DSIR)					
Real long-rate (bp)	5.8	5.8	6.5	5.5	5.9
Cluster-robust standard error	2.7	3.0	3.2	2.6	2.9
Bias-robust p-value	0.02	0.01	0.01	0.03	0.02
MSE-optimal bandwidth	6.9	5.8	5.8	7.3	6.6
CER-optimal bandwidth	5.7	4.8	4.8	6.1	5.5
Number of countries	26	22	20	26	26
Number of observations	336	325	318	336	336

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity design that corresponds to the baseline specification in Table 1, but with one alteration at a time. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Column (2) drops countries that we classify as being low-income as of 1990 (Poland, South Africa, Mauritius, and India). Column (3) drops countries that we classify as being low-income as of 2015 (Poland, Hungary, Greece, South Africa, Mauritius, and India). Column (4) includes a dummy variable for the richest half of countries in our sample as of 1990, and column (5) includes a dummy variable for the richest half of countries in our sample as of 2015.

B.3 Econometric Robustness

In this section, we consider the robustness of our results to alternative econometric specifications, specifically different choices of bandwidth and kernel. Column 1 of Table B.7 shows our baseline results for comparison purposes, which are based on an optimal MSE bandwidth choice of $[-6.89, +6.89]$ used for point estimation, an optimal CER bandwidth choice of $[-5.72, +5.72]$ used for inference, and the triangular kernel. For a given polynomial order, smaller bandwidths tend to improve the accuracy of the RD estimates by lowering the mis-specification error or bias of the RD estimate. However, because smaller bandwidths include fewer observations in the estimation window, they also tend to increase the variance of the estimated coefficients. The MSE of an estimator is the sum of the variance and the square of the bias of the estimator. The optimal bandwidth we use minimizes the MSE of the local polynomial RD estimate for point estimation, and thus optimizes the bias-variance tradeoff. The CER-optimal bandwidth used for inference is designed to minimize the coverage error. Columns 2 and 3 present RD estimates with double and half the baseline bandwidth, respectively. Halving the bandwidth produces a second stage estimate that is 3 basis points higher than our baseline and remains statistically significant. The estimated effect on real rates is virtually unchanged when we double the bandwidth, but the results are significantly noisier.

Fan and Gijbels (1996) show that under the MSE-optimal bandwidth, the RD estimate has optimal properties with the triangular kernel. Columns 4 and 5 present results for the uniform and the Epanechnikov kernels, respectively. The second stage point estimate remains within a basis point of the baseline specification, but the statistical significance of the estimates is lower than in the baseline specification using both alternative kernels.

Table B.7: Econometric Robustness of Regression Discontinuity Design Estimates

	(1) Baseline	(2) Bandwidth Half	(3) Bandwidth Double	(4) Uniform Kernel	(5) Epanechnikov Kernel
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP Ratios					
Debt-to-GDP ratio (pp)	-17.0	-17.8	-10.2	-10.2	-16.6
Cluster-robust standard error	9.0	8.4	8.4	6.7	8.9
Bias-robust p-value	0.02	0.00	0.04	0.07	0.02
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates					
Real long-rate (pp)	-0.99	-1.58	-0.62	-0.67	-0.92
Cluster-robust standard error	0.47	0.64	0.50	0.44	0.49
Bias-robust p-value	0.00	0.00	0.03	0.05	0.03
Second Stage ($\beta = 100 * \tau/\delta$): Effect of Debt-to-GDP Ratios on Real Interest Rates					
Real long-rate (bp)	5.8	8.9	6.0	6.5	5.5
Cluster-robust standard error	2.7	2.9	5.1	5.4	2.9
Bias-robust p-value	0.02	0.03	0.23	0.17	0.06
Bandwidth for point estimates	6.9	3.4	13.8	9.7	6.9
Bandwidth for inference	5.7	2.9	11.4	8.1	5.7
Number of countries	26	26	26	26	26
Number of observations	336	336	336	336	336

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity that corresponds to the baseline specification in column (1) of Table 1, but with one alteration at a time. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Columns (2) and (3) estimate the local polynomials in the RD with bandwidths that are one-half and double the baseline bandwidth, respectively. Columns (4) and (5) estimate the local polynomials in the RD with a uniform and Epanechnikov kernel, respectively; the baseline specification uses a triangular kernel.

B.4 Strength of First Stage and Statistical Inference

In this section, we examine the strength of the first stage of our fuzzy RD design and its implications for our statistical inference via bootstrapping and Monte Carlo exercises.

The inference in our RD design proceeds in multiple steps. We estimate our fuzzy RD design using the methodology and associated software of Calonico et al. (2014), which we believe is the most widely used and accepted approach in the literature. This methodology involves using a "bias-corrected" effect estimate for statistical inference that is distinct from the point estimate of the effect.[9] Additionally, we calculate point estimates using the optimal mean-squared error (MSE) optimal bandwidth and confidence intervals and p-values using the coverage-error (CER) optimal bandwidth, as recommended by Cattaneo et al. (2019b). This procedure underlies the bias-robust p-values in Table 1 and in all of our fuzzy RD specifications.

Two potential concerns regarding our inference and estimation procedure are that it relies on asymptotic approximations and that it may be unreliable if the first stage is weak. The latter problem has been a subject of significant attention in the instrumental variables literature (e.g., Bound et al., 1995; Olea and Pflueger, 2013; Lee et al., 2022). Although the intuition for fuzzy RD designs corresponds closely to instrumental variables regression, the details of implementation and inference differ in important ways. Cattaneo et al. (2019b) stress that OLS-based inference in RD designs estimated using the local polynomial approach ignores the bias associated with the local polynomial approximation and is therefore inappropriate. Therefore the methods proposed to assess and address weak instruments in Olea and Pflueger (2013) and Lee et al. (2022) unfortunately cannot be applied directly to fuzzy RD designs.

We therefore conduct two alternative exercises to assess the strength of our first stage and its potential effects on our estimates.

Bootstrapping Exercise

First, we conduct a simple bootstrapping exercise. We draw 500 bootstrap samples from our data set. We draw 336 observations with replacement in each replicate sample. We then apply our fuzzy

[9] One implication of this procedure is that the confidence intervals need not be centered on the point estimates, which is visible in some panels of Figure 2.

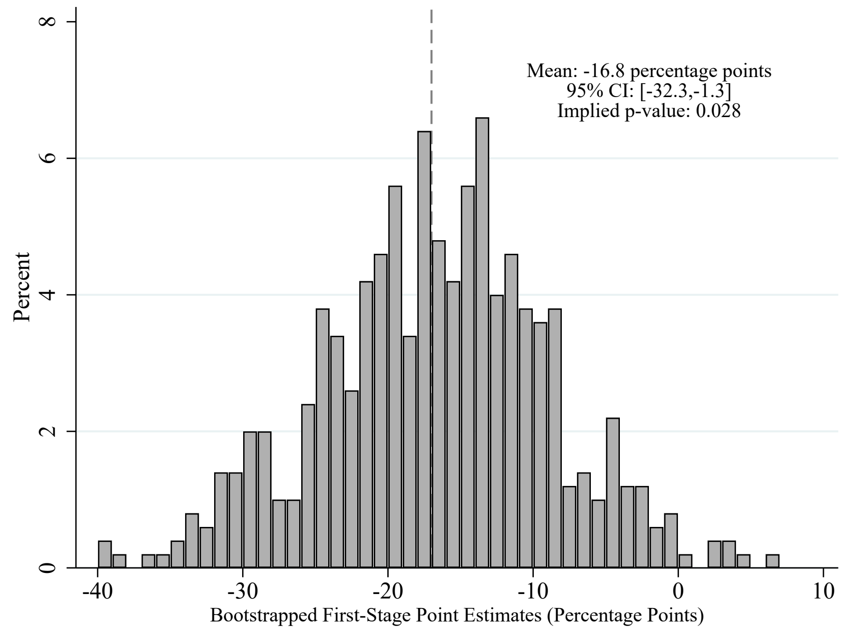
RD estimation procedure to each bootstrap sample.

Figure B.2 displays the results of this exercise. Panel (a) displays the first-stage point estimates that result from 500 bootstrap replications of our baseline estimation procedure. The average bootstrapped first-stage point estimate in Figure B.2 is -16.8 percentage points, similar to our baseline estimate, with a bootstrapped 95% confidence interval of -32.3 to -1.3 percentage points. The implied p-value of the first-stage effect is 0.028. The confidence interval and p-value implied are taken directly from the 500 bootstrapped first-stage point estimates, so they do not rely on a bias correction for inference. They also implicitly mimic the small sample properties of our data. We interpret these results as suggesting that the first-stage effect is statistically significant even without the bias corrected inference procedure in our baseline specification.

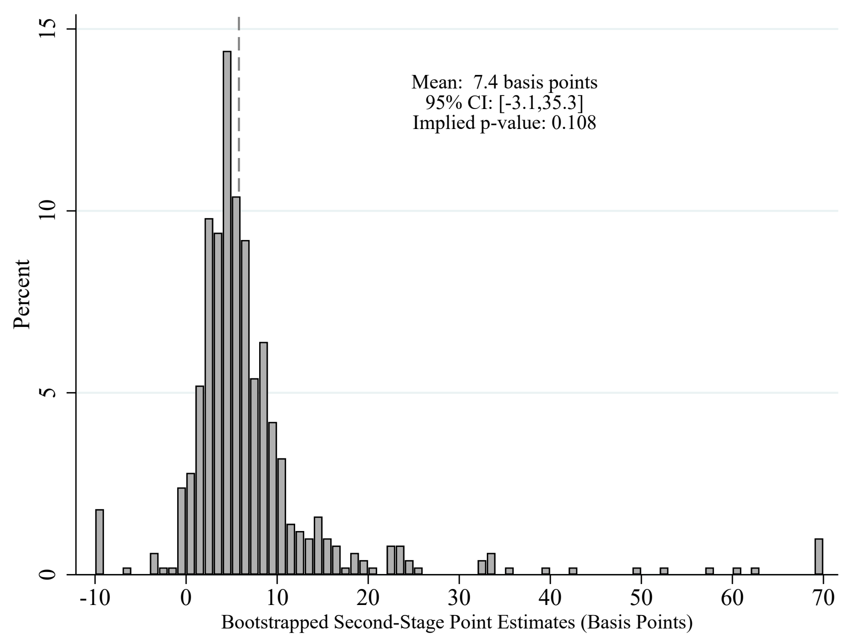
Panel (b) of Figure B.2 displays the second-stage point estimates from our bootstrapping exercise. The average bootstrap estimate of the DSIR is 7.4 basis points, a bit larger than our baseline estimate. Yet the estimates are also more dispersed than suggested by our baseline inference procedure. The bootstrapped 95% confidence interval is -3.1 to 35.3 basis points, and the implied p-value is 0.108. We note again that this confidence interval and p-value do not rely on a bias correction and implicitly reflect the small sample properties of our data. This bootstrapping exercise offers an alternative way to assess the uncertainty surrounding our point estimate of the DSIR.

Figure B.2: Bootstrapped Baseline Point Estimates

(a) Single-Party Majorities and Debt-to-GDP Ratios



(b) Single-Party Majorities and Real Long-Term Interest Rates



Panels (a) and (b) display estimated first-stage and second-stage effects, respectively, from 500 bootstrapped samples according to the baseline specification in column 1 of Table 1. Estimates in panel (a) are winsorized at -40 and 10 percentage points and estimates in panel (b) are winsorized at -10 and 70 basis points for display purposes. Dashed lines shows baseline point estimates of -17 percentage points and 5.8 basis points in panels (a) and (b) respectively. Implied p-values are calculated as twice the proportions of non-negative and non-positive bootstrap estimates in panels (a) and (b), respectively.

Monte Carlo Analysis

We also conduct an additional Monte Carlo study of the relationship between the strength of our first-stage effect and our estimated DSIR in Figure B.3 below. We simulate a data generating process that is meant to mimic roughly the assumed data generating process in our RD design. We then apply our RD estimation procedure to the simulated data and compare the estimated DSIRs to the first-stage p-values.

We simulate the data generating process as follows. We generate normalized seat shares V^{mc} as a normal random variable with mean -6.75 and standard deviation 12.6 percentage points (the values in our baseline sample):

$$V^{mc} \sim N(-6.75, 12.6). \tag{B.3}$$

We code the single-party majority indicator M as

$$M^{mc} = \begin{cases} 0 & \text{if } V^{mc} < 0 \\ 1 & \text{if } V^{mc} \geq 0. \end{cases} \tag{B.4}$$

We assume that the true first-stage effect of a single-party majority on our estimate is a random variable drawn from a normal distribution with the estimated mean and standard deviation from our baseline RD estimate in column 1 of Table 1:

$$\delta^{mc} \sim N(-17.0, 9.0), \tag{B.5}$$

and the first-stage relationship between the seat share and the change in the debt-to-GDP ratio away from the cutoff is governed by local linear polynomials with slopes $\zeta_l^{mc}, \zeta_r^{mc}$ generated as normal random variables with means and standard deviations also taken from our baseline RD estimate (although unreported in Table 1):

$$\begin{aligned} \zeta_l^{mc} &\sim N(0.984, 1.08), \qquad \text{(B.6)} \\ \zeta_r^{mc} &\sim N(3.686, 1.89). \end{aligned}$$

We set the true DSIR in every Monte Carlo simulation equal to our baseline RD point estimate of

5.8 basis points,

$$\tau^{mc} = 5.8 \text{ basis points.} \tag{B.7}$$

Finally, we generate random shocks to debt-to-GDP ratios and real interest rates, $u^{mc}_{debt}, u^{mc}_{rates}$ as

$$\begin{bmatrix} u^{mc}_{debt} \\ u^{mc}_{rates} \end{bmatrix} \sim N \begin{pmatrix} 14.9^2 & , & -0.8 \times 14.9 \times 1.3 \\ -0.8 \times 14.9 \times 1.3 & , & 1.3^2 \end{pmatrix}. \tag{B.8}$$

We set the standard deviation of the debt-to-GDP ratio shock to the standard deviation of the five-year change in the debt-to-GDP ratio (14.9 percentage points) in our baseline sample in column 1 of Table 1. Likewise, we set the standard deviation of the real rate shock to the standard deviation of the one-year change in real interest rates in our baseline sample (1.3 percentage points). We set the standard deviations of the debt-to-GDP ratio and real interest rates shocks equal to the corresponding variables' standard deviations in the data. The correlation between the two shocks cannot be observed in the data, but we set the correlation to a significantly negative value, -0.8, to mimic the stylized fact that the OLS estimate of the DSIR is much smaller than the RD estimate in our data. We present OLS estimates of the DSIR in Table B.8; they are uniformly less than one basis point. The negative correlation between the debt and interest rate shocks mimics the confounding effects from the business cycle and reverse causality from rates to debt that we discuss in the introduction. In unreported results, we find that using alternative values of the correlation between the two shocks of -0.7 or -0.9 yields qualitatively and quantitatively similar results; we find that this correlation is not an important factor in the strength of our first stage.

We then simulate changes in the debt-to-GDP ratio and real interest rates as

$$\Delta D^{mc} = \delta^{mc} M^{mc} + \zeta^{mc}_l V^{mc}(1 - M^{mc}) + \zeta^{mc}_r V^{mc} M^{mc} + u^{mc}_{debt}, \tag{B.9}$$

$$\Delta R^{mc} = \beta^{mc} \Delta D^{mc} + u^{mc}_{rates}. \tag{B.10}$$

Equation (B.9) is intended to parallel the data generating process in equation (2) in the main text. Equation (B.10) is likewise intended to parallel the data generating process in equation (3) in the main text. We note, however, that we do not include a dependence on the vote share V^{mc} or single-party majority indicator M^{mc} in equation (B.10), consistent with our discussion of the exclusion

Table B.8: OLS Estimates of DSIR

	(1) Baseline Sample	(2) Baseline Sample in Bandwidth	(3) Baseline Countries (All Yrs.)	(4) Baseline Countries (Twoway FEs)
DSIR (bp)	0.2	0.4	0.4	0.6
Cluster-robust standard error	0.4	0.5	0.2	0.3
Number of Observations	336	137	1288	1288
Number of Countries	26	19	26	26

This table shows results from ordinary least squares regressions of one-year changes in long-term real interest rates on five-year changes in debt-to-GDP ratios. The unit of observation is a country-election. Data definitions and construction match the baseline regression specification in column (1) of Table 1. The sample in column (1) is the same as the sample in column (1) of Table 1. The sample in column (2) is the set of observations within the MSE-optimal bandwidth in column (1) of Table 1. The sample in columns (3) and (4) is the set of country-years from 1950 to 2015 for the set of countries included in the baseline specification of column (1) of Table 1, i.e., it includes all years for the countries in our baseline sample regardless of whether they held an election. Regressions do not include covariates, with the exception of column (4), which includes a set of year and country fixed effects.

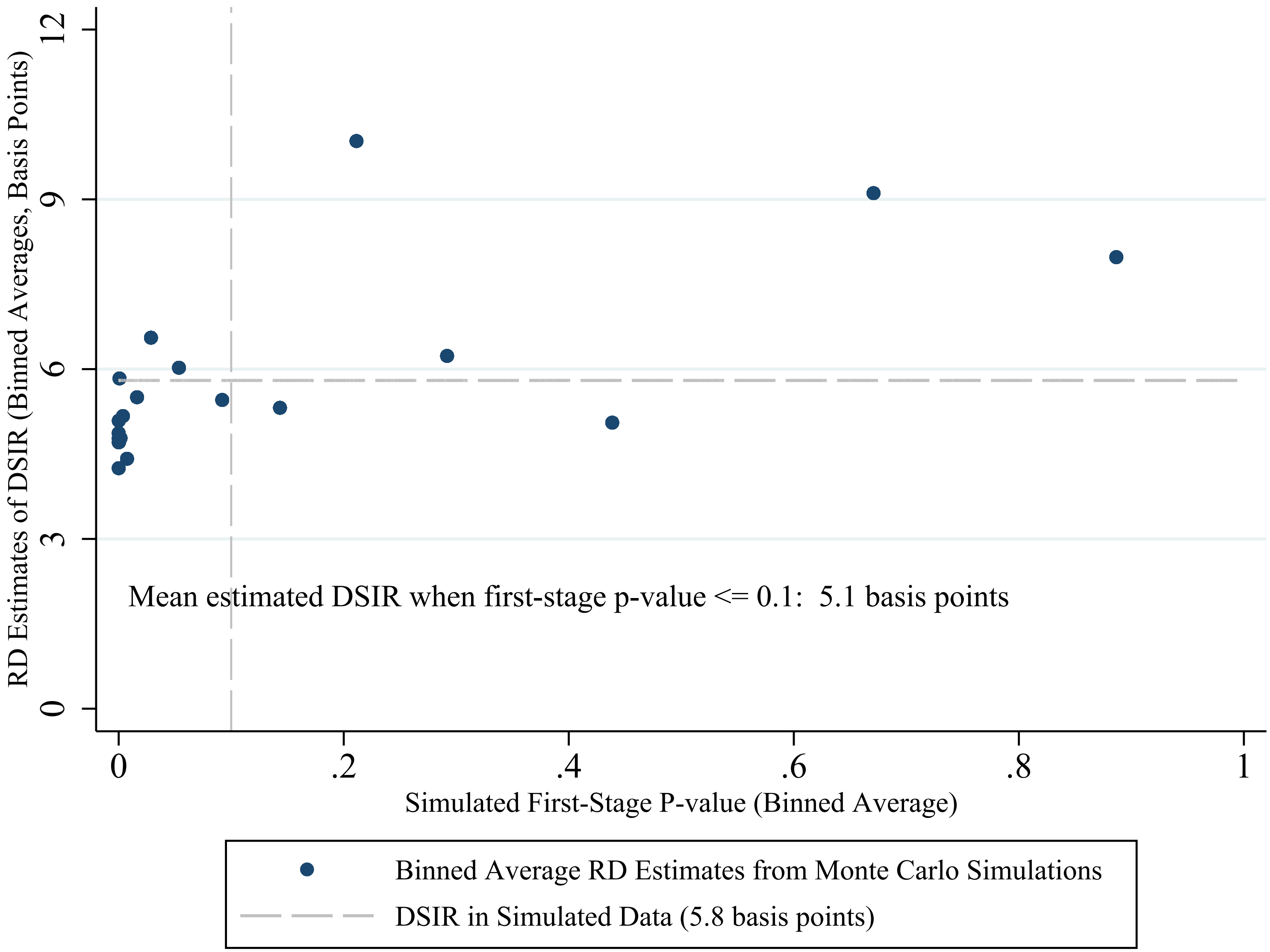
restriction. The change in rates will nonetheless respond to those variables through its dependence on the change in the debt-to-GDP ratio. We generate 336 observations per Monte Carlo simulation. The strength of our first stage will be governed primarily by δ^{mc}, with a larger value of δ^{mc} tending to lead to a stronger first stage.

Figure B.3 below displays a binscatter of the results from applying our baseline RD estimation approach to data generated from 500 Monte Carlo simulations of this data generating process. Each dot displays the average first-stage p-value and estimated DSIR for one-twentieth of the simulations (i.e., 25 simulations each). The mean estimated DSIR is 5.1 basis points for simulations in which the estimated first-stage p-value is less than or equal to 0.10, slightly lower than our baseline estimate.

For simulations with strong first stages, as indicated by low p-values, the estimated DSIRs lie on average below the true effect in the simulated data generating process. The estimated DSIR is the ratio of the reduced form effect to the first-stage effect, i.e., $\hat{\beta}^{RD} = \frac{\hat{\tau}^{RD}}{\hat{\delta}^{RD}}$. If, by random chance, the first-stage estimate $\hat{\delta}^{RD}$ is larger than the true value in the Monte Carlo simulation, the first-stage p-value will tend to be low and the estimated DSIR will tend to be too small. For simulations with weak first stages as indicated by high p-values, the estimated DSIRs are very noisy and tend to be too high. The converse logic to the low p-value case applies, with the additional note that the estimates become more erratic as the first-stage p-values rise above roughly 0.2.

We interpret this simulation evidence as suggesting that our first-stage relationship is likely to be strong enough for our estimated DSIR to be reliable. Additionally, we note that we consider our first-stage estimates to be of independent interest from our second-stage estimates. The complications posed by a potentially weak first stage do not apply to the interpretation of our first-stage estimates as independent parameters of interest.

Figure B.3: Monte Carlo Simulation: RD Estimates of DSIR vs. First-Stage p-value



Note: Each dot shows the average estimated DSIR from 25 replications of the Monte Carlo analysis described in Appendix B.4. The dashed vertical line corresponds to a first-stage p-value of 0.1.

C Local Randomization Evidence on Single-Party Majorities' Effects on Public Debt and Real Interest Rates

In this Appendix, we complement our regression discontinuity evidence on public debt's effects on real interest rates with evidence from a local randomization inference approach. The approach is closely related conceptually to our main RD design, but it employs alternative assumptions and inference procedures.

We follow Cattaneo et al. (2015) in summarizing the main features of the local randomization inference design, applied to our context. The local randomization inference approach begins with the assumption that there is a small window $W = [\bar{c} - w, \bar{c} + w]$ around the cutoff $\bar{c}$ for a single-party majority in which the probability distribution of the vote share of the largest party V is the same for all observations in the window. Furthermore, the vote share of the single largest party is assumed to affect economic outcomes only by determining whether the election produced a single-party majority, not by the distance of the largest party's vote share from the cutoff for a majority. In other words, the single-party majority indicator M is assumed to influence economic outcomes, but the running variable $V - \bar{c}$ is assumed not to have any independent influence on outcomes within the window W. Further, the effect of a single-party majority is assumed to be locally constant within the window W. These assumptions are the main ways in which the local randomization approach differs from the RD design and are stronger than the assumptions in the RD design. The local randomization approach further posits a local stable unit treatment value assumption, which states that the outcome of one election in a country-year within the window W does not affect (or "interfere with") election or economic outcomes in other country-year observations within the window.

Finally, the local randomization approach requires the assumption of a "randomization mechanism" for the observations within the window W that determines whether their largest single-party vote share V is below or above the cutoff for a single-party majority $\bar{c}$. We adopt the (standard) "fixed-margins randomization" mechanism. Suppose there are n total observations within the window W and m observations are "treated," i.e., corresponded to elections that produced a single-party majority. The fixed-margins randomization mechanism posits that every combination of outcomes across elections within W that produces m single-party majorities was equally likely. Let $\mathbb{M}$ denote the vector of ones and zeros collecting the values of the indicator variable M denoting the presence or absence of a single-party majority in each election in the estimation window, and let $\mathbb{m}$ denote a particular realization of $\mathbb{M}$. Under the fixed-margins randomization assumption, the probability distribution of realized single-party majorities across observations within W is assumed to be:

$$Pr(\mathbb{M} = \mathbb{m}) = \binom{n}{m}^{(-1)}. \tag{C.1}$$

Table C.1: Randomization Inference for RD Designs under Local Randomization

	(1) Window Width ± 1	(2) Window Width ± 1.25	(3) Window Width ± 1.5	(4) Window Width ± 1.75	(5) Window Width ± 2
First Stage: Effect of Single-Party Majority on Debt-to-GDP Ratios					
Debt-to-GDP ratio (pp)	-10.8	-11.1	-12.7	-12.4	-11.4
Exact (small sample) p-value	0.35	0.26	0.08	0.04	0.07
Reduced Form: Effect of Single-Party Majority on Real Interest Rates					
Real long-rate (pp)	-1.59	-1.49	-1.01	-0.92	-0.62
Exact (small sample) p-value	0.05	0.04	0.05	0.04	0.11
Bandwidth	1.00	1.25	1.50	1.75	2.00
Number of observations	336	336	336	336	336
Effective number of observations	17	18	28	36	45

The unit of observation is a country-election. Each column shows the results from a local randomization inference approach with the listed symmetric window width in percentage points on each side of the cutoff. Finite-sample inference is not available for the second-stage effects, so we present the first stage and reduced form effects.

Our point estimates for a single-party majority's average treatment effects both on debt-to-GDP ratios and real interest rates (implicitly via the debt-to-GDP channel), are simply the difference in mean outcomes of observations on the two sides of the cutoff $\bar{c}$ and within the window W. In principle, the fixed-margins randomization assumption precisely defines the distributions of those differences in means, allowing for exact finite sample inference. In practice, the cardinality of the set of possible outcome vectors $\mathbb{M}$ will be large, requiring us to conduct inference by sampling from the probability distribution in equation (C.1).[10] Nonetheless, given the small numbers of observations in the windows we consider, it is worth emphasizing that the p-values we report for the first stage and reduced from estimates are valid for finite samples—they do not rely on asymptotic approximations. To our knowledge, the local randomization inference approach has not been adapted to fuzzy RD designs, so we do not report second-stage estimates for our design in Table C.1.[11]

Table C.1 displays estimates from local randomization inference for a succession of narrow windows around the cutoff for a single-party majority. The results in column 1 pertain to a window with width w of 1 percentage point on each side of the cutoff. The width of the window on either side of the cutoff increases by 0.25 percentage points in each subsequent column, with column 5 showing results for a window 2 percentage points wide around the cutoff. The procedure does not make use of covariates, so the sample in every column corresponds to the portion of the sample from column 2 of Table 1 for which the running variable $V_{it} - \bar{c}_{it}$ lies within the estimation window.

The estimated effects of a one percentage point increase in the debt-to-GDP ratio in these windows tend to be larger than or roughly in line with our baseline estimate from column 1 of Table 1.[12] However, with very narrow windows and relatively few observations, the effects are less precisely estimated.

Overall, we view the results from the local randomization exercise as complementary to our main RD evidence. The local randomization results show that even in a narrow window around the cutoff for a single-party majority, without extrapolation away from the cutoff, the presence of such a majority leads to lower debt-to-GDP ratios and lower real interest rates.

[10] We draw 1,000 samples from the distribution to calculate the p-values reported in this section.

[11] It is possible to calculate the implied second-stage effect by taking the ratio of the reduced form effect to the first-stage effect, but finite sample standard errors are not available.

[12] The implied second-stage effects from the local randomization inference approach, i.e., the ratios of the reduced form to first-stage effects, are larger than our baseline point estimate for all window widths except plus or minus two percentage points. The implied second-stage effect declines as we consider successively larger window widths.

D Data Appendix

This section of the Appendix describes our data sources and the construction of our data series in detail. In Section D.1, we describe our procedure for combining our sources of elections data and coding our election outcome variables $V_{it} - \bar{c}_{it}$ and M_{it}. In Section D.2, we describe our sources of economic data and the construction of our various economic variables. In Section D.3, we describe the combined dataset.

D.1 Elections Data

We primarily use data from two sources to construct our measure of the normalized "running variable" $V_{it} - \bar{c}_{it}$, which measures the share of total seats won by the largest party following an election minus the number needed to achieve a bare majority. The primary data set we use comes from Seki and Williams (2014), which in turn updates Woldendorp et al. (2000). Some earlier papers studying the political economy determinants of public debt levels have previously used versions of the Woldendorp et al. (2000) data (e.g. Alesina and Perotti, 1995; Perotti and Kontopoulos, 2002). The secondary data set is the *ParlGov* data set of Döring and Manow (2019). Unfortunately, neither data set on its own is ideally suited for our task.

The main differences between the two data sets are as follows:

- First, Seki and Williams (2014) only record parties that form part of the government following an election. If the party that received the largest number of seats was excluded from the government, it is not recorded in the data set. For instance, at the Norwegian election of 1969, the Labor party won 74 out of 150 seats; the other four parties, which together won a total of 76 seats, formed a coalition excluding the Labor party from power. Seki and Williams (2014) consequently do not record the Labor party's 74 seats in the first government following the election, instead recording the 29 seats won by the Conservative party as the highest individual-party seat total.

- Second, Seki and Williams (2014) code some closely related parties as a unit, whereas the ParlGov data set does not. For instance, Seki and Williams (2014) code Germany's Christian

Democratic Union (CDU) and Christian Social Union (CSU) as a single party, whereas Döring and Manow (2019) do not. In practice, the two parties typically function as a single unit in national politics: as Chase (2018) describes it, "The party names are often hyphenated as CDU-CSU. This reflects the fact that they function as a single group in parliament. ... The majority of Germans most of the time see the CDU-CSU as a single political entity." Similarly, Seki and Williams (2014) treat the United Kingdom's National Liberal Party as being a part of the Conservative party, while Döring and Manow (2019) do not. As Dutton notes, "Historians have not expended much energy in tracing the fortunes of the National Liberal party. Though the party enjoyed a theoretically independent existence for nearly forty years, there is general agreement that it is best seen as a mere adjunct of Conservatism." In contrast, neither data set codes the Liberal Party of Australia and National Party of Australia, commonly known as "The Coalition," as a single party.

- Third, the two data sets appear to make different coding decisions in ambiguous cases concerning whether to include various groups as members of parliament, including representatives from the Australian Capital Territory and Northern Territory in Australia; from the Faroe Islands and Greenland in Denmark; from the Maori electorates in New Zealand; and from West Berlin in pre-unification Germany. We cannot discern a consistent rule governing the coding of those representatives in either data set.

- Fourth, in exploring the differences between the two data sets, we have found what appears to be mis-coded data in both sources.

Our approach to combining the data sets is as follows: we begin with the data set of Seki and Williams (2014). We then re-code the running variable $V_{it} - \bar{c}_{it}$ according to the *ParlGov* figures of Döring and Manow (2019) in cases in which the party winning the most seats was excluded from the government formed after the election; in countries for which Seki and Williams (2014) consistently code two parties in the *ParlGov* dataset as the same party, we preserve that convention. We also take the Döring and Manow (2019) values in cases of a discrepancy when our research indicates that it is clearly preferable to the coding in Seki and Williams (2014). For discrepancies, we cross-reference with data from the Inter-Parlimentary Union (Inter-Parliamentary Union, 2020) along with other online sources. When our research is ambiguous, we retain the coding in Seki and Williams (2014). Finally, for a small number of parliamentary elections not recorded in either Seki and Williams (2014) or *ParlGov*, we use data from the Inter-Parlimentary Union.

We use data on the total number of seats in each country's parliament at the time of the election and the number of seats won by the party winning the most seats to calculate the normalized running variable $V_{it} - \overline{c}_{it}$ as follows:

$$V_{it} - \overline{c}_{it} \equiv \frac{\text{Largest Party Seats}_{it} - \big(\text{int}(\text{Total Seats}_{it}/2) + 1\big)}{\text{Total Seats}_{it}}, \tag{D.1}$$

where $\text{int}(x)$ is the greatest integer less than or equal to x. As previously shown in equation 4, our indicator variable for a single-party majority, M_{it}, is defined as:

$$M_{it} = 1[V_{it} \geq \overline{c}_{it}]. \tag{D.2}$$

A value of 0 for $V_{it} - \overline{c}_{it}$ is associated with a strict majority in this definition, i.e., the largest party must win strictly more than half of the parliamentary seats for the single-party majority indicator M_{it} to be coded as one. For instance, in the 1993 New Zealand general election, the National Party won 50 out of a total of 99 seats, so the value of $V_{it} - \overline{c}_{it}$ is coded as exactly zero, and M_{it} is coded as one. In the 1989 Spanish general election, the Spanish Socialist Workers' Party (PSOE) won 175 out of 350 total seats. $V_{it} - \overline{c}_{it}$ is coded as $\frac{-1}{350}$ in this case, as the PSOE fell one seat short of a majority; M_{it} is coded as zero.[13]

Finally, we apply some additional restrictions to the election data. We drop elections followed by another election within one year and elections that are not classified as "free-and-fair" (V-Dem - processed by Our World in Data, 2024). We also drop elections for which there was an IMF intervention program in the following five years.

[13] Our decision to code elections in which one party wins precisely 50% of the seats as failing to produce a majority is consistent with Seki and Williams (2014).

D.2 Economics Data

We employ a number of data sources to construct our data set. In addition to data on election outcomes, debt-to-GDP ratios, and real interest rates in a panel of countries, we also require some additional variables to implement our specifications using covariates and subsamples.

Our main sources for government debt-to-GDP ratios are Reinhart and Rogoff (2011), which extends from before World War II through 2010 for most of the countries in our sample, and the IMF's Global Debt Database compiled by Mbaye et al. (2018). We use the central government debt-to-GDP ratio where it is available. For country-years without data in either Reinhart and Rogoff (2011) or Mbaye et al. (2018), we supplement with data from the IMF's April 2022 World Economic Outlook (WEO) and the 5th release of the Macrohistory Database (Jordà et al., 2017) (henceforth JST). For our baseline estimates, we take the average of the 5-year change in central government debt-to-GDP from Mbaye et al. (2018) and Reinhart and Rogoff (2011). If neither source has a measure of the change in central government debt, we use the average of general government debt between the same two sources. Finally, if those sources are unavailable, we fill in with the WEO and JST sources respectively. Throughout, we take time differences before combining sources, so the 5-year change is always taken between two data points from the same data source.

We operationalize the change in real interest rates ΔR_{it} as the change in nominal interest rates on long-term government bonds, ΔI_{it}, minus the change in a proxy for expected inflation, $\Delta \Pi_{it}^{e}$:

$$\Delta R_{it} = \Delta I_{it} - \Delta \Pi_{it}^{e}. \tag{D.3}$$

We calculate ΔI_{it} as the one-year change from the year of the election to the year following the election. To avoid measuring spurious jumps in nominal interest rates, we impose that the same series be used in both years in the calculation of ΔI_{it}.[14]

We use multiple data sources to construct our primary series for nominal interest rates on long-term government bonds. Our preferred data sources are the "Long-term interest rates" series in the Organization for Economic Cooperation and Development's Main Economic Indicators (OECD,

[14]In other words, we calculate the differences ΔI_{it} from each data source individually and then splice the differences together across sources, rather than splicing the levels of interest rates together across sources and then calculating the differences.

2019), which refer to government bonds maturing in ten years and the interest rate series for "Government Bonds" published in the International Monetary Fund's International Financial Statistics (International Monetary Fund, 2019), which is listed under the category "Financial, Interest Rates, Government Securities." We take the average one-year change between these two sources as our preferred measure of the one-year change in nominal rates. If long-run interest rates are unavailable from either of these sources, we fill in data from JST (Jordà et al., 2017).

Measures of expected inflation based on surveys or financial market prices are not generally available for the countries and years in our sample, so we construct a proxy. We follow Council of Economic Advisers (2015) in proxying for expected inflation Π^e_{it} as the five-year unweighted moving average of current and past annual inflation. To construct this proxy for inflation expectations, we primarily use the Consumer Price Index for "all items non-food non-energy" (core inflation) published by the OECD (2020). For country year combinations where OECD core inflation is not available we fill in with OECD headline inflation, the IMF World Economic Outlook variable for "Inflation, average consumer prices," and inflation from JST respectively. We use core inflation where possible because it is widely considered to be a better predictor of future inflation than all-items inflation.[15]

In most of our specifications, including our baseline specification, we include additional covariates to reduce the residual variance in the estimates and improve their precision. The covariates we consider in our baseline analysis are the lagged growth rate of real GDP, the lagged inflation rate, the lagged short-term interest rate, and a lagged indicator variable for an economic crisis. We take the growth rate of real GDP from the 2018 release of the Maddison Project (Bolt et al., 2018).[16] For lagged inflation, we use the same series that we did for our measure of inflation expectations. Our primary source for short-term interest rates is the IMF International Financial Statistics variable "Monetary Policy Related Interest Rate." Where this is unavailable, we supplement it with the short-term interest rate variable from JST and finally with the short-term interest rate from the OECD.[17]

[15]In parallel to our procedure for calculating the change in nominal interest rates, we calculate the moving averages Π^e_{it} and their differences $\Delta\Pi^e_{it}$ from each data sources individually and then splice the series of differences together, rather than splicing the levels and then calculating moving averages and differences.

[16]Using real GDP per capita growth instead of real GDP growth produces similar results.

[17]Note that the OECD STINT variable contains flags for series breaks. When calculating one-year differences, we only

We also use the economic crisis dummies of Reinhart and Rogoff (2009) as updated on the Harvard Business School Behavioral Finance and Financial Stability website (Reinhart, 2016). Reinhart and Rogoff (2011) define crisis indicators for several different types of crises; we create a single crisis indicator equal to one if Reinhart and Rogoff (2011) code a country-year as having a crisis related to stock markets, the banking sector, currency crises, high inflation, or sovereign debt.[18]

We begin our analysis in 1950 to avoid any measurement or other confounding issues related to World War II and its short-term aftermath. The most recent elections included in our sample are from 2015. Our baseline sample includes 336 election-year observations from 26 countries that have the necessary data to be included in the analysis.

D.3 Summarizing the Combined Dataset

In this section, we briefly summarize the observations in our combined elections and macroeconomic dataset. Table D.1 lists each of 336 observations in our baseline dataset. The table lists each country in the dataset, its most common of the three major electoral rules as categorized by Bormann and Golder (2022), and each election-year that the country appears in the dataset. Bold year numbers indicate elections that produced a single-party majority, while non-bold year numbers indicate elections that did not produce a single-party majority. Asterisks by country names indicate countries with multiple major types of electoral rules in our sample, again as coded by Bormann and Golder (2022); lettered subscripts after year numbers indicate the electoral rule type when it is not the usual rule. Table D.2 displays the same data only for observations in which the largest party's seat share was within our baseline bandwidth from column 1 of Table 1 of 6.9 percentage

take differences between data series that are from the same source.

[18] Reinhart and Rogoff (2011) define a currency crash as occurring if the domestic currency depreciates by 15% or more relative to the relevant anchor currency. There are 13 instances of currency crises in our baseline sample. An inflation crisis occurs if a country experiences annual inflation rates of 20% or higher. We have 5 instances of inflation crises in our baseline sample. A stock market crisis occurs when there is a cumulative decline in real equity prices of at least 25%. A banking crisis occurs if either of two types of events occur. First, there are bank runs, which lead to the closure, merging, or takeover by the public sector of at least one financial institution. Second, there are no bank runs, but the closure, merging, takeover, or large-scale government assistance of at least one important financial institution begins a sequence of similar outcomes for other financial institutions in the country. A sovereign debt crisis occurs if the central (federal) government defaults by failing to meet a principle or interest payment on debt by the due date. Scenarios in which bank deposits have been frozen or forcibly converted from foreign currency to the local currency are also included as part of a sovereign debt crisis. Because most of these types of crises are quite rare individually, we aggregate them into a single crisis indicator, which takes value 1 if Reinhart and Rogoff (2011) code any type of crisis as occurring in a given country-year observation, and which takes value 0 otherwise.

points. 199 out of 336 elections in our baseline dataset fall within this bandwidth.

Figure D.1 displays annual counts of the numbers elections returning and not returning single-party majorities in our baseline sample by year. The dark bars indicate elections that produced a single-party majority, while the lighter bars indicate elections that did not produce a single-party majority. We classify 103 out of 336 elections in our baseline dataset as producing a single-party majority. The share of elections producing a single-party majority declines modestly over time, from 34% in the period 1950–1971 and 33% in the period 1972–1990 to 27% in the period 1991–2015.

Table D.1: List of Baseline Observations by Country, Election Year, and Election Rule

Country	Usual Electoral Rule	Election years																
Australia	Majoritarian	**1951**	**1954**	**1955**	**1958**	**1961**	**1963**	**1966**	**1969**	**1972**	**1974**	**1975**	**1977**	**1980**	**1983**	**1984**	**1987**	**1990**
			1993	**1996**	**1998**	**2001**	**2004**	**2007**	2010	**2013**								
Austria	Proportional	1994	1995	1999	2002	2006	2008	2013										
Belgium	Proportional	**1950**	1954	1958	1961	1965	1968	1971	1974	1977	1978	1981	1985	1987	1991	1995	1999	2003
			2007	2010	2014													
Canada	Majoritarian	**1953**	**1958**	1963	1965	**1968**	1972	**1974**	**1980**	**1984**	**1988**	**1993**	**1997**	**2000**	2004	2006	2008	**2011**
			2015															
Denmark	Proportional	1950	1953	1957	1960	1964	1966	1968	1971	1973	1975	1977	1979	1981	1984	1988	1990	1994
			1998	2001	2005	2007	2011											
Finland	Proportional	1951	1954	1958	1962	1966	1970	1972	1975	1979	1983	1987	1991	1995	1999	2003	2007	2011
France*	Majoritarian	1951[c]	1956[c]	1958	1962	1967	**1968**	1973	1978	**1981**	1986[b]	1988	1993	1997	**2002**	**2007**	2012	
Germany	Mixed	1953	**1957**	1961	1965	1969	1972	1976	1980	1983	1987	1990	1994	1998	2002	2005	2009	2013
Greece	Proportional	**1996**	**2000**	**2004**	2015													
Hungary	Mixed	2002	**2010**	**2014**														
Iceland	Proportional	1995	1999	2009	2013													
India	Majoritarian	2009																
Ireland	Proportional	1951	1954	**1957**	1961	1965	**1969**	1973	**1977**	1982	1987	1989	1992	1997	2002	2011		
Italy*	Majoritarian	1953[c]	1958	1963	1968	1972	1976	1979	1983	1987	1992	1994[c]	1996[c]	2001[c]	2006	2008	2013	
Japan*	Majoritarian	**1958**	**1960**	**1963**	**1967**	**1969**	**1972**	1976	**1980**	1983	**1986**	**1990**	1993	1996[c]	2000[c]	2003[c]	**2005**[c]	**2009**[c]
			2012[c]	**2014**[c]														
Mauritius	Majoritarian	**2010**	**2014**															
Netherlands	Proportional	1952	1956	1959	1963	1967	1971	1972	1977	1981	1982	1986	1989	1994	1998	2003	2006	2010
			2012															
New Zealand*	Majoritarian	**1975**	**1978**	**1981**	**1984**	**1987**	**1990**	**1993**	1996[c]	1999[c]	2002[c]	2005[c]	2008[c]	2011[c]	2014[c]			
Norway	Proportional	**1953**	**1957**	1961	1965	1969	1973	1977	1981	1985	1989	1993	1997	2001	2005	2009	2013	
Poland	Proportional	2001	2005	2007	2011													
Portugal	Proportional	1976	**1980**	1983	1985	**1987**	**1991**	1995	1999	2002	**2005**	2015						
South Africa	Proportional	**1999**	**2004**	**2009**	**2014**													
Spain	Proportional	1979	**1982**	**1986**	1989	1993	1996	**2000**	2004	2008	**2011**							
Sweden	Proportional	1952	1956	1958	1960	1964	**1968**	1970	1973	1976	1979	1982	1985	1988	1991	1994	1998	2002
			2010	2014														
Switzerland	Proportional	1951	1955	1959	1963	1967	1971	1975	1979	1983	1987	1991	1995	1999	2003	2007	2011	
United Kingdom	Majoritarian	**1950**	**1951**	**1955**	**1959**	**1964**	**1966**	**1970**	**1974**	**1979**	**1983**	**1987**	**1992**	**1997**	**2001**	**2005**	2010	**2015**

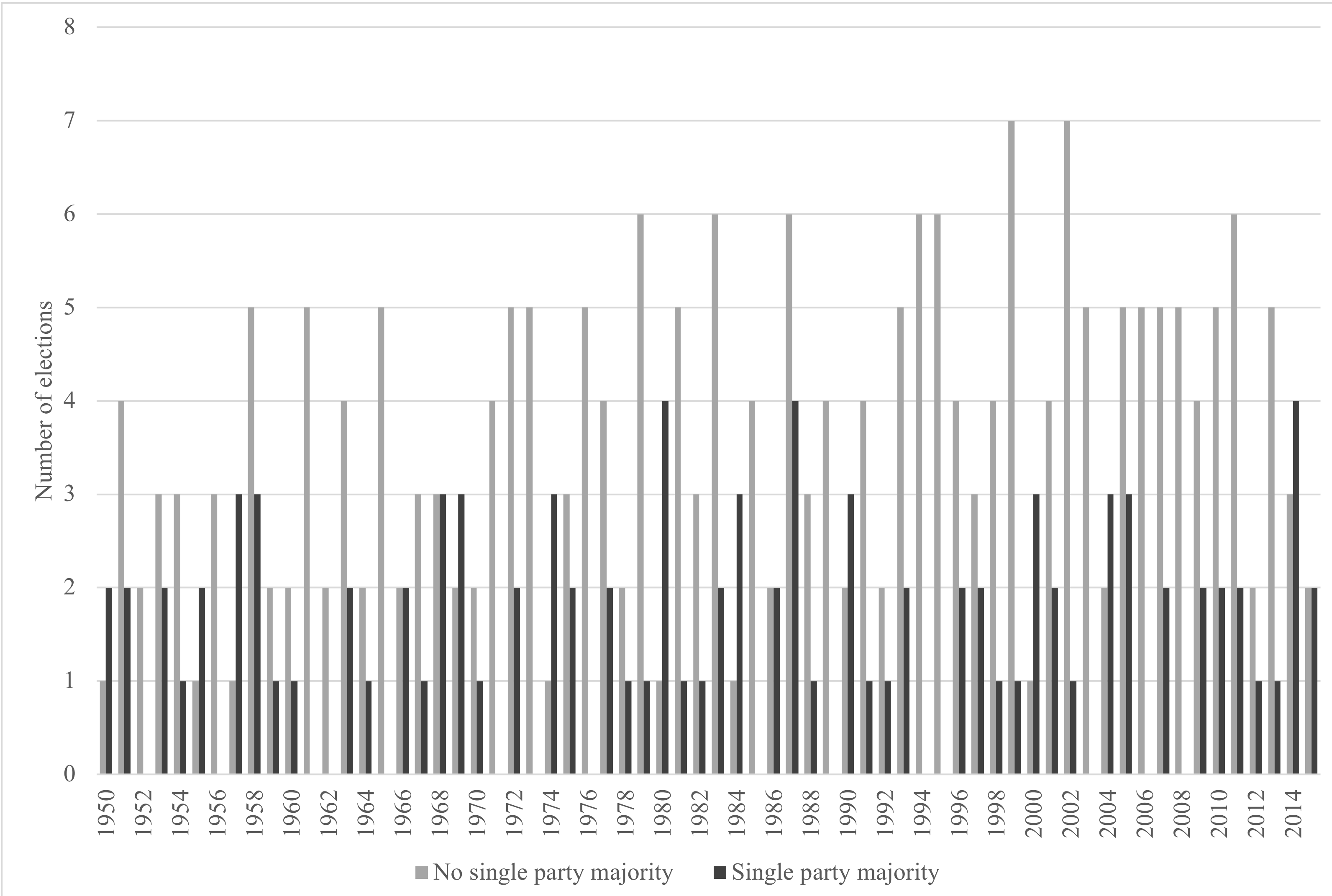
Notes: Bold indicates an election producing a single-party majority. (*) denotes countries with multiple electoral rules over the sample. [b] = Proportional, [c] = Mixed

Table D.2: List of Observations within Baseline Bandwidth by Country, Election Year, and Election Rule

Country	**Usual Electoral Rule**	**Election years**													
Australia	Majoritarian	**1951**	**1954**	**1961**	**1969**	**1972**	**1974**	**1984**	**1990**	**1993**	**1998**	**2001**	**2007**	2010	
Austria	Proportional														
Belgium	Proportional	**1950**	1954	1958	1961										
Canada	Majoritarian	1963	1965	**1974**	**1980**	**1997**	2004	2008	**2011**	**2015**					
Denmark	Proportional	1960	1964												
Finland	Proportional														
France*	Majoritarian	1962	**1981**	1988	1993	**2007**	2012								
Germany	Mixed	1953	**1957**	1961	1965	1969	1972	1976	1980	1983	1990	1994	1998	2013	
Greece	Proportional	**1996**	**2000**	**2004**	2015										
Hungary	Mixed	2002													
Iceland	Proportional														
India	Majoritarian	2009													
Ireland	Proportional	1951	1954	**1957**	1961	1965	**1969**	1973	**1977**	1982	1987	1989	1997	2002	2011
Italy*	Majoritarian	1953[c]	1958	2008	2013										
Japan*	Majoritarian	**1967**	**1972**	1976	**1980**	1983	**1990**	1993	1996[c]	2000[c]	2003[c]				
Mauritius	Majoritarian														
Netherlands	Proportional														
New Zealand*	Majoritarian	**1978**	**1981**	**1993**	2008[c]	2011[c]	2014[c]								
Norway	Proportional	**1953**	**1957**	1961	1965	1969	1977	1985							
Poland	Proportional	2001	2007	2011											
Portugal	Proportional	**1980**	1995	1999	2002	**2005**	2015								
South Africa	Proportional														
Spain	Proportional	1979	**1986**	1989	1993	1996	**2000**	2004	2008	**2011**					
Sweden	Proportional	1952	1956	1958	1960	1964	**1968**	1970	1973	1976	1979	1982	1985	1988	1994
Switzerland	Proportional														
United Kingdom	Majoritarian	**1950**	**1951**	**1955**	**1964**	**1970**	**1974**	**1979**	**1992**	**2005**	2010	**2015**			

Notes: Bold indicates an election producing a single-party majority. (*) denotes countries with multiple electoral rules over the sample. [b] = Proportional, [c] = Mixed

Figure D.1: Count of Election Results by Year



E Data Robustness

E.1 Alternative Data Sources and Definitions

Compiling our data requires making several choices. In Table E.1, we test the sensitivity of our baseline estimates to some of our decisions about data sources. Column (1) reproduces our baseline estimates. Column (2) reports estimates in which the 5-year change in debt is calculated as a percentage of the election-year GDP, rather than as the five-year change in the debt-to-GDP ratio. If GDP growth rates were different for single-party and coalition governments, the change in the debt-to-GDP ratio could be driven by changes in GDP rather than changes in the debt levels. The estimates in column (2) are very similar to our baseline estimates, however, suggesting this is not an important issue.

Columns (3) and (4) test the sensitivity to which data sources we prioritize. In column (3), rather than taking the median across our "preferred" data sources for debt-to-GDP and long-term interest rates, we primarily use the IMF global debt database and IMF international financial statistics, only using other sources when these sources are missing. The estimates from this specification are again very similar to the baseline, with a marginally larger DSIR of 7.3 basis points. In column (4), we exclusively use the macrohistory database compiled by Jordà et al. (2017) as our source for long-term interest rates, inflation, and debt-to-GDP ratios. Restricting to only the JST data increases the DSIR to 7.6 basis points. Despite the somewhat smaller sample size, the estimate in this specification remains highly statistically significant.

One challenging aspect of constructing our data is measuring inflation expectations, which are needed to measure real long-term interest rates. Consistently and accurately measuring inflation expectations over the many decades and many countries in our sample is not straightforward. Table E.2 explores the sensitivity of our results to alternative measures of inflation expectations. As described in Section 2.2, our baseline specification follows Council of Economic Advisers (2015) in using a simple adaptive inflation forecast, the five-year unweighted moving average of current and past inflation, to measure inflation expectations. Column (2) instead uses the professional inflation forecasts from the IMF World Economic Outlook where these measures are available. These forecasts were published twice a year (spring and fall vintages), covering the year of the forecast as well as five years into the future starting in 1990. In column (2), when constructing real long-term interest rates, we measure expected inflation in a given year as the WEO-forecasted average rate of inflation over the next 5 years (averaging the spring and fall WEO forecasts). These forecasts are only available for 150 observations, approximately half our sample, so we continue to use the 5-

Table E.1: Regression Discontinuity Design Estimates with Alternative Data Definitions

	(1) Baseline	(2) Base-year GDP	(3) IMF Data Preferred Sources	(4) Macrohistory Database
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP				
Debt-to-GDP ratio (pp)	-17.0	-17.3	-16.0	-22.5
Cluster-robust standard error	9.0	9.5	8.5	13.2
Bias-robust p-value	0.02	0.05	0.03	0.08
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates				
Real long-rate (pp)	-0.99	-1.07	-1.02	-1.54
Cluster-robust standard error	0.47	0.48	0.47	0.45
Bias-robust p-value	0.00	0.01	0.00	0.00
Second Stage ($\beta = 100 * \tau/\delta$): Effect of Debt-to-GDP Ratios on Real Interest Rates				
Real long-rate (bp)	5.8	6.3	6.4	7.6
Cluster-robust standard error	2.7	3.5	3.1	4.7
Bias-robust p-value	0.02	0.04	0.01	0.04
MSE-optimal bandwidth	6.9	5.8	6.4	5.1
CER-optimal bandwidth	5.7	4.8	5.4	4.3
Number of countries	26	26	26	26
Number of observations	336	335	336	279

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity that corresponds to the baseline specification in column (1) of Table 1, but with an alternative data source or definition. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Columns (2) defines the five-year change in debt as a percentage of GDP in the election year rather than considering the change in the debt-to-GDP ratio. Column (3) uses the IMF's Global Debt Database Central Government Debt and International Financial Statistics as the preferred sources for the debt and interest-rate variables respectively, filling in missing values as specified by the data appendix. Column (4) exclusively uses data from the Macrohistory database of Jordà et al. (2017) for interest rates, inflation, and public debt.

year historical moving average of realized inflation when the WEO forecasts are unavailable. Using the WEO forecasts when available does not significantly change our estimate of the DSIR.

Table E.2: Regression Discontinuity Design Estimates with Alternative Inflation Expectation Measures

	(1) Baseline	(2) IMF WEO Forecast	(3) OECD Economic Outlook Forecast	(4) All Inflation Forecasts	(5) Realized Forward Inflation
First Stage (δ): Effect of Single-Party Majority on Debt-to-GDP					
Debt-to-GDP ratio (pp)	-17.0	-17.9	-17.7	-17.8	-18.2
Cluster-robust standard error	9.0	9.0	9.0	9.0	9.3
Bias-robust p-value	0.02	0.02	0.02	0.02	0.05
Reduced Form (τ): Effect of Single-Party Majority on Real Interest Rates					
Real long-rate (pp)	-0.99	-1.18	-1.09	-1.26	-0.18
Cluster-robust standard error	0.47	0.46	0.72	0.78	0.32
Bias-robust p-value	0.00	0.00	0.05	0.04	0.91
Second Stage ($\beta = 100 * \tau/\delta$): Effect of Debt-to-GDP Ratios on Real Interest Rates					
Real long-rate (bp)	5.8	6.6	6.2	7.1	1.0
Cluster-robust standard error	2.7	3.1	3.4	3.4	1.9
Bias-robust p-value	0.02	0.03	0.04	0.02	0.92
MSE-optimal bandwidth	6.9	6.4	6.5	6.5	5.3
CER-optimal bandwidth	5.7	5.3	5.4	5.4	4.4
Number of countries	26	26	26	26	26
Number of observations	336	336	336	336	336

The unit of observation is a country-election. Each column shows the results from a fuzzy regression discontinuity that corresponds to the baseline specification in column (1) of Table 1, but with an alternative measure of inflation expectations used to construct real long-term interest rates. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Column (2) uses the average forecast of the inflation rate over the five years following the election from the IMF's World Economic Outlook for the 150 observations where these forecasts are available. Column (3) exclusively uses the inflation forecasts from the OECD EO as the measure of inflation expectations. Column (4) uses the average rate of ex-post realized inflation in the five years following the election as a measure of inflation expectations.

Column (3) instead uses inflation forecasts reported in the historical OECD Economic Outlooks where these forecasts are available. The OECD Economic Outlooks are published twice yearly starting in 1967, once mid-year and once at the end of each year. The longer history of the published OECD forecasts provides greater coverage of our baseline dataset than do the WEO forecasts. Yet the forecast horizon for the OECD forecasts is much shorter than the horizon of the IMF WEO forecasts, and the data availability varies from report to report in the earliest vintages.[19] Unlike the IMF WEO, which provides forecasts 5 years into the future, the OECD EO forecast horizon is quite limited; recent editions forecast only a couple of years, and the earliest vintages provide only 6- or 12-month forecasts. If long-run inflation expectations are anchored, using a very short forecast horizon will introduce additional volatility relative to longer-term expectations. Column (3) uses the average of the mid-year and year-end forecasts as a measure of inflation expectations.[20] The OECD forecasts are available for 226 observations in our main sample, so we again supplement with the 5-year historical moving average of realized inflation when OECD forecasts are unavailable. The estimated DSIR using the OECD forecasts when available is again similar to our baseline estimate.[21]

Column (4) combines both sets of professional forecasts, using the moving average when neither is available. The longer forecast horizon of the WEO inflation forecasts makes this source our preferred data source for the 150 observations where it is available. We then fill in with the OECD inflation forecasts for an additional 83 observations. Finally, for the remaining 103 observations we use the 5-year moving average of inflation. The results using this combined measure are broadly consistent with the previous measures, with a slightly larger point estimate of the DSIR of 7.1 basis points.

[19]For recent vintages, we access the forecast for the personal consumption expenditure price index (PCP) through the OECD's API https://www.oecd.org/en/data/insights/data-explainers/2024/09/api.html. For older vintages, we hand-code inflation forecasts from scanned PDF reports available from the OECD website https://www.oecd.org/en/topics/sub-issues/economic-outlook.html. In these older reports, we use the private consumption deflator or consumer price deflator when available and supplement with the GDP deflator.

[20]For the end-of-year forecasts, typically released in December, we take the one year difference between the year of the election and the prior year, rather than differencing the year after the election to the year of the election. The precise timing of the forecast release date at the end of the year motivates this timing convention to bracket the election between forecasts. We keep the same timing convention for the mid-year forecasts that we use for the remainder of our economic data. Making the timing of the mid-year forecasts consistent with the end-of-year forecasts does not qualitatively change the results.

[21]In unreported results, we have estimated our RD design without supplementing the OECD inflation forecasts with our adaptive expectations measure, dropping observations for which the OECD forecasts are missing. The estimated DSIR increases to 9.0 basis points, primarily because the estimated first-stage effect shrinks to 12.4 percentage points. Yet these results are less precisely estimated than our baseline estimates, because the number of observations within the bandwidth drops roughly in half.

Finally, column (5) takes an alternative approach, using ex-post realized inflation as a measure of inflation expectations. If we invoke the assumption of rational expectations, realized inflation should be a consistent estimator for inflation expectations, although the extent to which this relationship holds in practice is unclear (e.g., Goodspeed, 2025). Using realized inflation as a measure of inflation expectations leads to a smaller estimate of the DSIR that is not statistically distinguishable from zero.[22] Overall, the results in Table E.2 suggest that our estimated DSIR is somewhat sensitive to measures of inflation expectations. However, the alternative point estimates bracket our baseline estimate of the DSIR.

E.2 Changes in the Debt-to-GDP Ratio at Different Horizons

In our baseline specification presented in Table 1, we considered the effects of the change in the debt-to-GDP ratio over a five-year window after each election. Expected debt levels or deficits five years in the future have been commonly considered in the literature (e.g., Laubach, 2009; Gale and Orszag, 2004; Engen and Hubbard, 2004; Gamber and Seliski, 2019; Neveu and Schafer, 2024), so we consider that horizon the most appropriate for comparing our results to other estimates. In this section, we summarize the estimated effects using different horizons to estimate the change in the debt-to-GDP ratio in the first stage of the RD design.

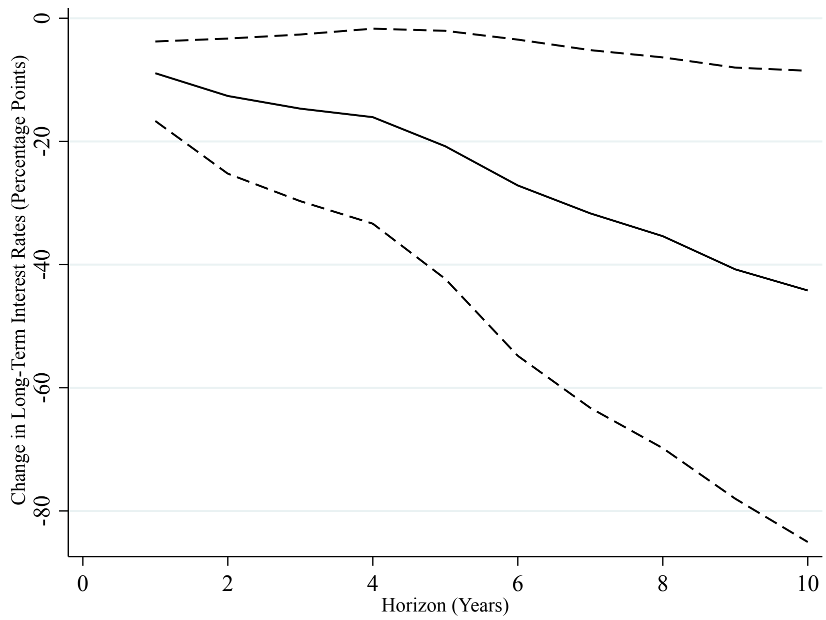
Figure E.1 shows the dynamic response of the debt-to-GDP ratio to Single-Party Majority governments. This chart is constructed analogously to Figure 2 using equations (6)–(7). Coalition governments are associated with statistically significant higher debt at every horizon, but the magnitude of this effect steadily increases with the horizon considered.[23]

In order to test the sensitivity of the DSIR estimates, we implement a sequence of fuzzy RD

[22] This approach is operationalized by using the five-year unweighted moving average of inflation from the year of the election to four years after the election. Using a ten-year moving average instead similarly produces a near-zero estimate of the DSIR.

[23] Note that the point estimate in year 5 does identically match the first-stage estimates in the baseline specification. This difference is due to a slightly sample restriction ensuring that a consistent sample is used throughout Figure E.1 at each horizon and the fact that the optimal bandwidths are calculated for a sharp RD with the debt-to-GDP ratio as the dependent variable, rather than the bandwidths appropriate for the fuzzy RD used in the baseline specification.

Figure E.1: Response of Debt-to-GDP to Single-Party Majorities



This figure displays the response of the debt-to-GDP ratio over the horizon specified on the horizontal axis from a sharp RD design. The estimation includes the covariates from the baseline specification and the sample is constructed to remain constant for each horizon. Dashed lines show 95% confidence intervals.

designs by estimating a system of two equations, for each horizon $h = 1, 2, ..., H$:

$$\Delta D_{it}^{h} = \gamma + \delta M_{it} + g(V_{it} - \bar{c}_{it}) + \nu_{it} \tag{E.1}$$

$$\Delta R_{it} = \alpha + \tau M_{it} + f(V_{it} - \bar{c}_{it}) + \varepsilon_{it} \tag{E.2}$$

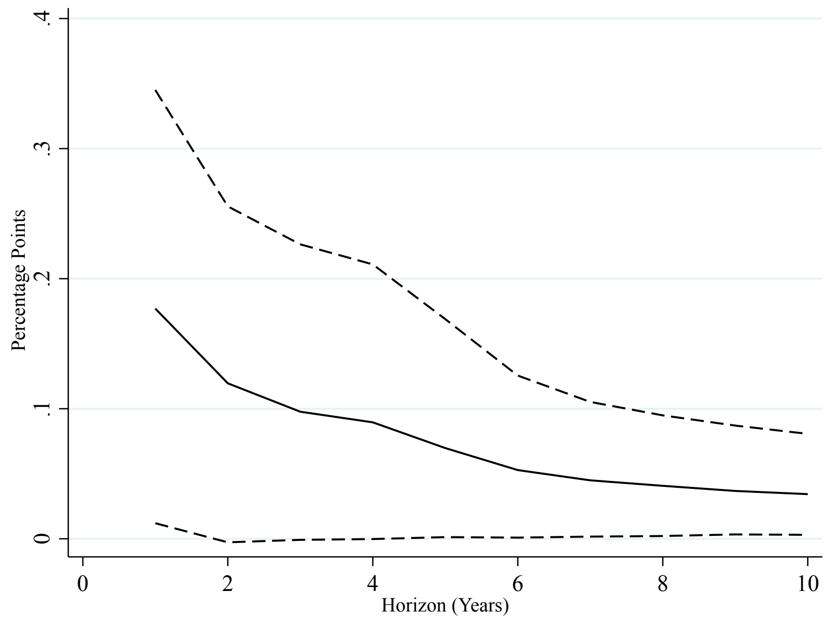
where $\Delta D_{it}^{h} = [D_{i,t+h} - D_{it}]$ is the $h-$year change in the debt-to-GDP ratio for country i between election-year t and h years later. All other variables are defined as described in Section 2.

Figure E.2 presents the estimated second-stage effect of a one percentage point increase in the debt-to-GDP ratio on real long-term interest rates at horizons $h = 1, 2, ..., 10$. Each horizon represents a separate RD design estimate. Using a one-year horizon suggests that a one percentage point increase in the debt-to-GDP ratio leads to an almost 16 basis point increase in real long-term interest rates, whereas using a three-year horizon leads to a smaller estimated effect of about 9 basis points. At a horizon of seven years, the estimated second-stage effect is 4 basis points and at a ten-year horizon it is around 3 basis points.[24] The decline in the estimated DSIR as we consider

[24]To maintain comparability between different horizon, we restrict our sample to 303 observations with debt-to-GDP

longer horizons reflects the tendency of changes in the debt to be autocorrelated over time, so that the first-stage effect (i.e., single-party majorities' effects on the debt) will be larger at longer horizons.

Figure E.2: Effect on Real Rates for a 1% Increase in Debt-to-GDP Over Different Horizons



This figure displays the second stage point estimate from a fuzzy RD design using the one-year change in real long-term interest rates and the change in debt-to-GDP ratios over the horizon specified on the horizontal axis. The estimation is based on the specification in equations E.1 and E.2. Dashed lines indicate 95% confidence intervals.

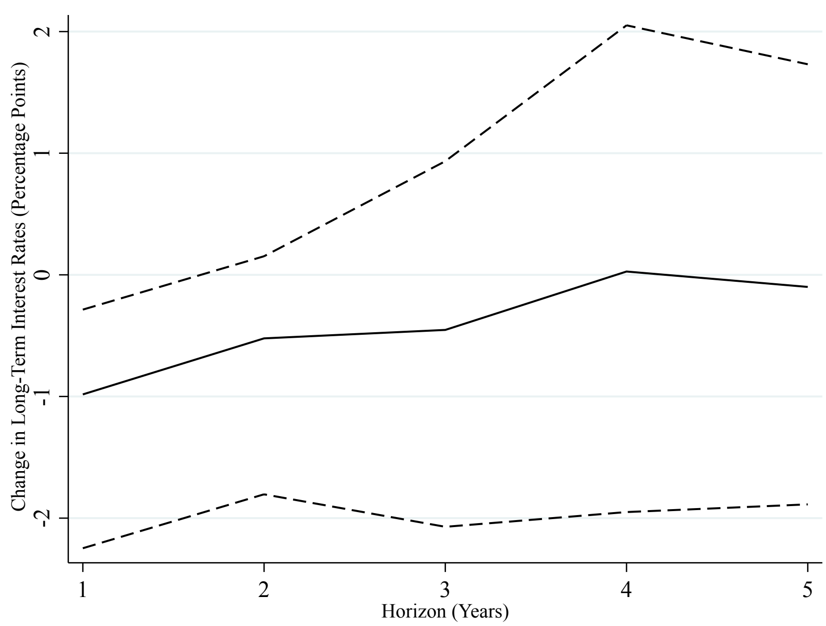
data for each time horizon. We use `rdrobust` (Calonico et al., 2014) to estimate the effect at each time horizon, selecting the optimal bandwidth at each horizon.

E.3 Interest Rate Changes at Different Horizons

Our baseline specification measures interest rate changes using a 1-year change in the long-term real interest rate. We believe that this narrow 1-year window is the most appropriate for measuring the change in the long-term interest rate as financial markets are forward looking and should immediately incorporate future changes in fiscal policy.[25]

In Figure E.3 we estimate the dynamic effects of interest rate changes in longer horizons. We again construct the chart analogously to Figure 2 using equations (6)–(7). The most significant change in interest rates occurs in the first year following the election, and over time the confidence intervals widen and the estimated effect attenuates to zero.

Figure E.3: Response of Real Long-term Interest Rates to Single-Party Majorities



This figure displays the response of the real long-term interest rate over the horizon specified on the horizontal axis from a sharp RD design. The estimation includes the covariates from the baseline specification and the sample is constructed to remain constant for each horizon. Dashed lines show 95% confidence intervals.

If financial markets price in changes from close elections within a year, estimating changes over longer horizons will simply add noise rather than useful extra information, diminishing our ability to detect an effect. This concern appears in some of the recent literature that uses high

[25] In principle, we would want to estimate changes in the interest rate in even smaller windows around the exact date of the election, but the annual frequency of our historical data prevent us from doing so.

frequency event studies to estimate the effects of fiscal policy on financial variables (e.g., Phillot, 2025; Wiegand, 2025). The attenuation of the interest rate response and larger standard errors are consistent with forward-looking financial markets and the overall volatility of long-term interest rates.

Finally, we conduct an additional exercise in which we use the same horizon to estimate the change in the debt-to-GDP ratio and real interest rates. Specifically, we estimate a series of fuzzy regression discontinuity designs given by

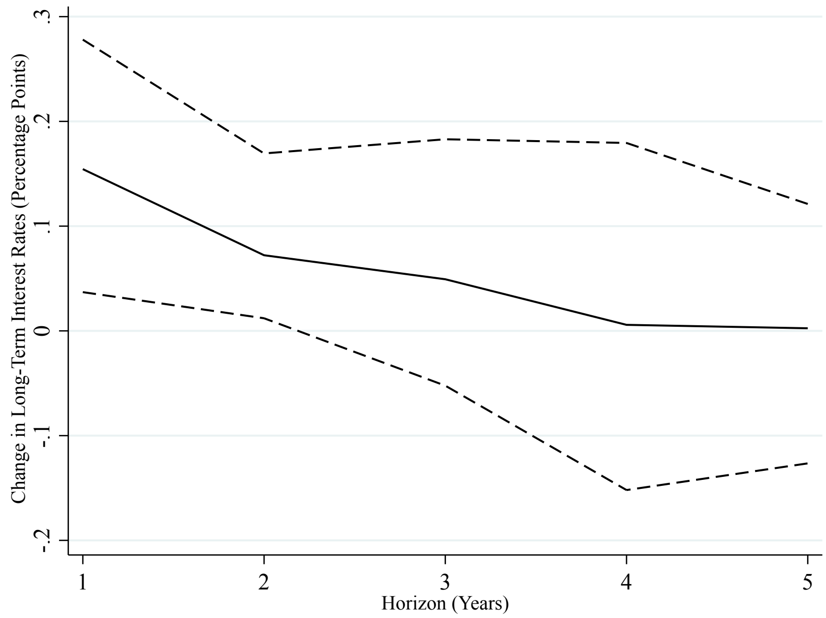
$$\Delta D_{it}^{h} = \gamma + \delta M_{it} + g(V_{it} - \overline{c}_{it}) + \nu_{it} \tag{E.3}$$

$$\Delta R_{it}^{h} = \alpha + \tau M_{it} + f(V_{it} - \overline{c}_{it}) + \varepsilon_{it} \tag{E.4}$$

where $\Delta D_{it}^{h} = [D_{i,t+h} - D_{it}]$ is the $h-$year change in the debt-to-GDP ratio and $\Delta R_{it}^{h} = [R_{i,t+h} - R_{it}]$ is $h-$year change in the real long-term interest rates for country i between election-year t and h years later.

Figure E.4 plots the point estimates and 95% confidence intervals estimated by equations (E.3) and (E.4) for $h = 1, 2, ...5$. For horizons of 1 and 2 years, the estimated DSIR is larger than the point estimates from the baseline specification and highly statistically significant. Using a 3-year horizon, the estimated DSIR of 4.9 basis points is similar to our baseline estimate, although it is no longer statistically significant at standard confidence levels. For longer horizons, the point estimates attenuate towards zero, consistent with the estimated long-term interest rate response shown in Figure E.3.

Figure E.4: Estimates of the Debt Sensitivity of Interest Rates with a Fixed Horizon for Debt and Interest Rates



This figure displays the estimated DSIR when the same horizon is used for both the debt-to-GDP ratio and real long-term interest rate. The estimation includes the covariates from the baseline specification and the sample is constructed to remain constant for each horizon Dashed lines show 95% confidence intervals.

F Implications for Debt Sustainability

Our estimates of the DSIR may have implications for the possibility of fiscal policies in which increased debt can be sustainably rolled over without a future increase in taxes. Blanchard (2019) argues that such opportunities exist when the real rate R is less than the economic growth rate G, i.e., $(R - G) < 0$. In his framework, the relationship between R and G does not depend on the level of debt. Mian et al. (2022) analyze a setting in which the gap between interest rates and economic growth rates, $(R - G)$, depends on the debt-to-GDP ratio. They denote the sensitivity of $(R - G)$ to the debt level, $\frac{\partial(R-G)}{\partial \log(D)}$, as φ. They show that when $\varphi > 0$, such an opportunity requires that $(R - G) < -\varphi$, which is more stringent than Blanchard's requirement that $(R - G) < 0$. A larger φ therefore implies fewer circumstances in which increased debt can be sustainably rolled over without a future increase in taxes.

In Table F.3, we use our fuzzy RD design to estimate φ. Mian et al. (2022) note that φ can be estimated as either $D_0 \frac{\partial(R-G)}{\partial D}$, where D_0 is the initial debt level, or as $\frac{\partial(R-G)}{\partial \log(D)}$. Following their logic, we estimate φ using two different fuzzy RD approaches. In columns 1 and 2 of Table F.3, we estimate the equations:

$$\Delta D_{it} = \gamma + \delta M_{it} + g(V_{it} - \bar{c}_{it}) + \nu_{it} \tag{F.5}$$

$$D_{it}\Delta(R_{it} - G_{it}) = \alpha + \tau M_{it} + f(V_{it} - \bar{c}_{it}) + \varepsilon_{it}. \tag{F.6}$$

In columns 3 and 4, we estimate the equations:

$$\Delta \ln(D_{it}) = \gamma + \delta M_{it} + g(V_{it} - \bar{c}_{it}) + \nu_{it} \tag{F.7}$$

$$\Delta(R_{it} - G_{it}) = \alpha + \tau M_{it} + f(V_{it} - \bar{c}_{it}) + \varepsilon_{it}, \tag{F.8}$$

where in both specifications the right-hand side variables have the same definitions as in equations (2)–(4). $\hat{\varphi}$ is estimated as $\hat{\tau}/\hat{\delta}$. G_{it} represents the growth rate of real GDP in country i and year t. The theoretically relevant growth rate, G, for estimating φ is the long-run trend growth rate of real GDP. We therefore use moving averages of G_{it} to a void capturing short-run business cycle fluctuations. Columns 1 and 3 measure $\Delta G_{it} = \frac{1}{3}(\sum_{\tau=t+1}^{t+3} G_{i\tau} - \sum_{\tau=t-2}^{t} G_{i\tau})$, while columns 2 and 4 instead measure $\Delta G_{it} = \frac{1}{5}(\sum_{\tau=t+1}^{t+5} G_{i\tau} - \sum_{\tau=t-4}^{t} G_{i\tau})$.

Our estimates of φ in Table F.3 range from 2.8 to 8.3, suggesting that a 1 percent increase in the debt-to-GDP ratio leads to a 2.8–8.3 basis point increase in $(R - G)$.[26] The estimates of φ are higher using five-year average growth rates versus three-year averages. All of the estimates are on the high end or above the estimates of φ that Mian et al. (2022) report from the existing literature, which range from 0.8 to 3.1, with most estimates lying between 1.1 and 2.5. Mian et al. (2022) use the average φ of 1.7 as their preferred estimate.

Our larger estimates of φ suggest that there may be fewer circumstances in which debt can be sustainably rolled over without a future increase in taxes than the previous literature would suggest, because the condition $(R - G) < -\varphi$ is less likely to hold the larger is φ. To illustrate this point, we consider the relationship between R and G during the period 1990 to 2007. Focusing on

[26]Note that the interpretation of these magnitudes differs from the interpretation of the estimates in Tables 1–2, because φ measures the effect of a 1 *percent* change in the debt-to-GDP (i.e., it is a semi-elasticity), whereas β measures the effect of a 1 *percentage point* increase in the debt-to-GDP ratio.

Table F.3: Sensitivity of the Interest Rate Minus Growth Rate $(R-G)$ to Public Debt Levels

	(1)	(2)	(3)	(4)
	$D_0 \frac{\partial(R-G)}{\partial D}$		$\frac{\partial(R-G)}{\partial \ln(D)}$	
	3-year average	5-year average	3-year average	5-year average
Second Stage (φ): Effect of Debt-to-GDP Ratios on $(R-G)$				
R-G semielasticity	2.8	8.3	4.9	7.9
Cluster-robust standard error	2.6	3.3	4.6	4.9
Bias-robust p-value	0.11	0.00	0.14	0.05
MSE-optimal bandwidth	5.4	4.9	9.0	9.0
CER-optimal bandwidth	4.5	4.1	7.4	7.5
Number of countries	26	26	26	26
Number of observations	336	325	336	325

The unit of observation is a country-election. Each columns reports the effect of a 1 percent increase in the debt-to-GDP ratio on the difference between the real interest rate and real growth rate $(R-G)$. In columns (1) and (2), the dependent variable is specified as the level of debt in the year of the election, D_0, multiplied by the difference in the change in the real interest rate and real growth rate. Columns (3) and (4) report specifications in which the difference in the change in the real interest rate and real growth rate are used as the dependent variable and the percent change in the debt-to-GDP ratio is measured as the 5-year change in the log of the debt-to-GDP ratio. We omit the first-stage and reduced form estimates because the different variable definitions render the units incomparable across columns.

that period minimizes the effects of the zero lower bound on nominal interest rates, which became more prominent after the global financial crisis of 2008, and which Mian et al. (2022) show can complicate this analysis.

Twenty-two countries in our sample had real interest rate and debt data for nearly that entire period, with average values of $(R-G)$ ranging from -4.7 to 1.6 percent. Of those countries, nine displayed average values of $(R-G)$ that suggested there were opportunities for sustainable debt rollovers assuming φ equals 0, while only five did so assuming φ equals 1.7. However, calibrating φ to the lower end of our estimates (2.8) leaves only two such opportunities, and using the midpoint across our four estimates (5.5) eliminates even those opportunities. Focusing on the period 2010 to 2020 does not change this basic pattern: calibrating φ to 1.7 suggests there are nine countries in the relevant sample with opportunities for sustainable debt rollovers, but calibrating φ to 5.5

suggests there is only one.