Supplementary Appendix

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

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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 (1989) 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, 1989, 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.

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.[1]

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:

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

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

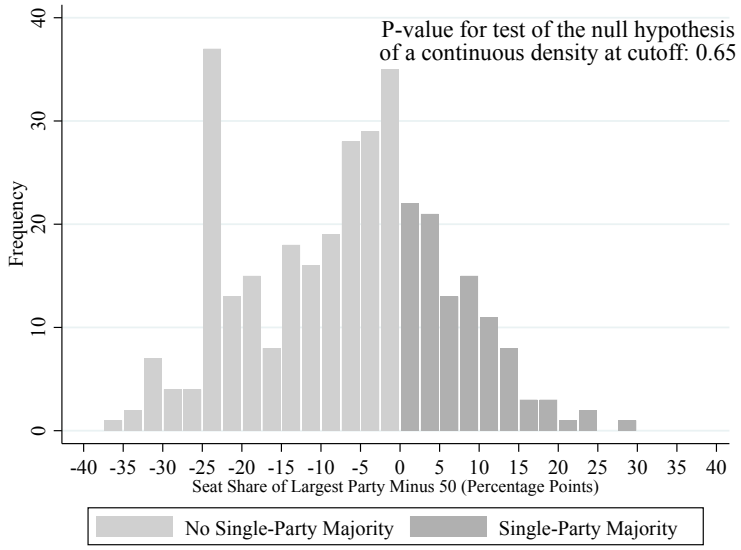
where the other elements are defined analogously with equations (1)–(4).

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.[2] The tests do not reject the null hypothesis of

[1] 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.

[2] 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.

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.

continuity at the cutoff, consistent with the notion of local randomization.

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 I.B. 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.[3] These results suggest that our baseline estimates using the true cutoff value are unlikely to be driven by spurious random variation near the cutoff.

[3]Cutoffs outside $\pm 3.5\%$ once again run into convergence issues since there are insufficient observations to one side of 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 I.B. 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.[4] The triangular kernel assigns the highest weights to observations that are closest to the cutoff. Therefore, it is possible for a small number of observations to have high leverage and a substantial

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

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.[5] 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 RD estimation procedure to each bootstrap sample.

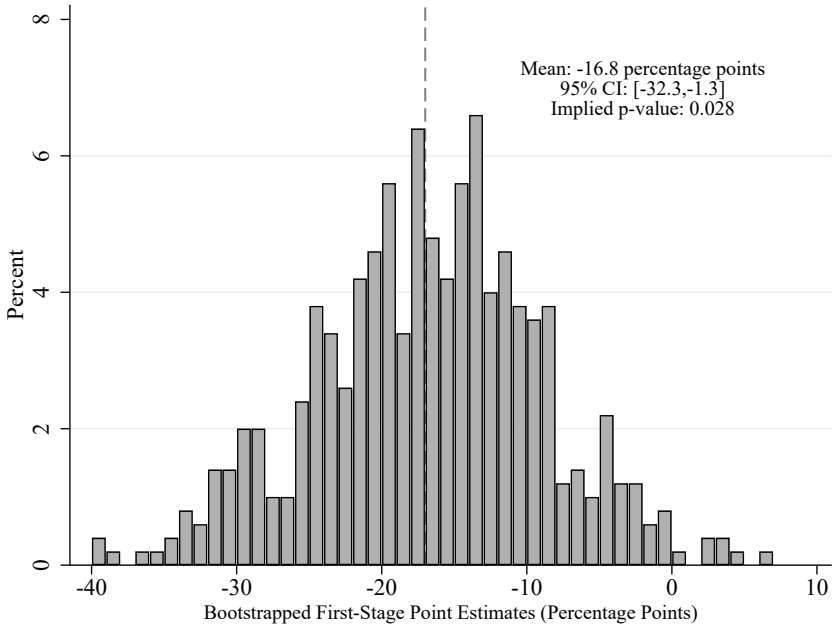
Figure B.2 displays the results of this exercise. Panel (a) displays the first-stage point estimates that result from 500 bootstrap replications of our baseline estimation procedure. The average bootstrapped first-stage point estimate in Figure B.2 is -16.8 percentage points, similar to our baseline 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.

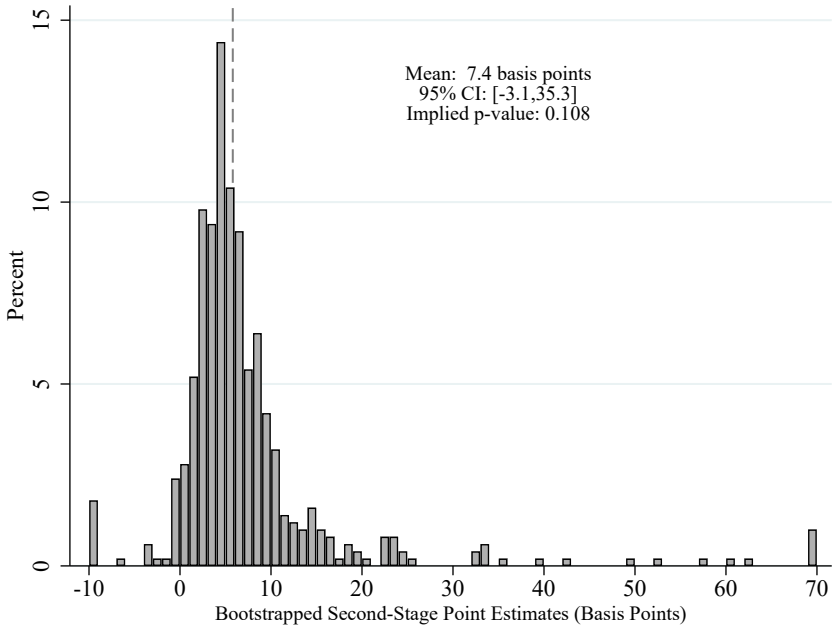
[5] One implication of this procedure is that the confidence intervals need not be centered on the point estimates, which is visible in some panels of Figure 2.

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 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):

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

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, $u_{debt}^{mc}, u_{rates}^{mc}$ as

$$\begin{bmatrix} u_{debt}^{mc} \\ u_{rates}^{mc} \end{bmatrix} \sim N\left(\begin{matrix} 14.9^2 & , & -0.8 \times 14.9 \times 1.3 \\ -0.8 \times 14.9 \times 1.3 & , & 1.3^2 \end{matrix} \right). \tag{B.8}$$

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

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.

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

$$\Delta D^{mc} = \delta^{mc} M^{mc} + \zeta_l^{mc} V^{mc}(1 - M^{mc}) + \zeta_r^{mc} V^{mc} M^{mc} + u_{debt}^{mc}, \tag{B.9}$$

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

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 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.

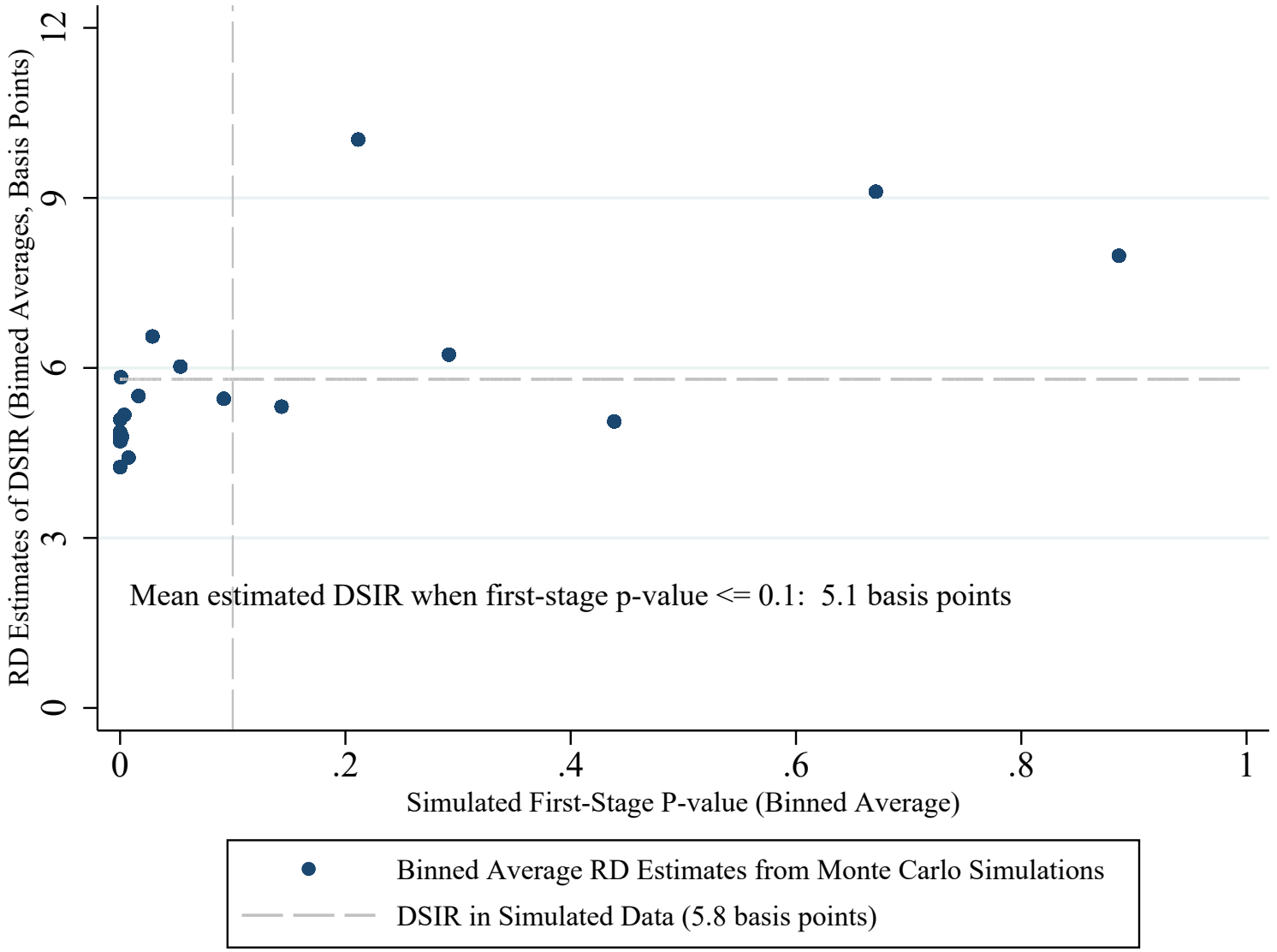
Figure B.3 below displays a binscatter of the results from applying our baseline RD estimation approach to data generated from 500 Monte Carlo simulations of this data generating process. Each dot displays the average first-stage p-value and estimated DSIR for one-twentieth of the simulations (i.e., 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 $W = [\overline{c}-w, \overline{c}+w]$ around the cutoff $\overline{c}$ 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 $V - \overline{c}$ 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 $\overline{c}$. 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:

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

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 $\overline{c}$ 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).[6] Nonetheless, given the small numbers of observations in

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

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.[7]

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.[8] 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

[7] 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.

[8] 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.

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.

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.

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. We have done so (along with their predecessor/sister parties such as the Country Party), consistent with the majority of the political science literature we reviewed (e.g., Sartori, 1976; Budge and Keman, 1990; Siaroff, 2003).

- 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 $V_{it} - \overline{c}_{it}$ 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.[9]

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, 2022). We also drop elections for which there was an IMF intervention program in the following five years.

[9] 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 (2011a), 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 (2011a) 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., 2017a) (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 (2011a).[10] 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.[11] 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, $\Delta \Pi^e_{it}$:

$$\Delta R_{it} = \Delta I_{it} - \Delta \Pi^e_{it}. \tag{D.3}$$

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}.[12]

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, 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, 2021), 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., 2017b).

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 (2020b). For country year combinations where OECD core inflation is not available we fill in with OECD headline inflation (OECD, 2020a), the IMF World Economic Outlook variable for "Inflation, average consumer prices," (International Monetary Fund, 2022c) and inflation from JST respectively (Jordà et al., 2017b). We use core inflation where possible

[10] The underlying data for Reinhart and Rogoff (2011a) are provided in Reinhart and Rogoff (2011c).

[11] For the WEO, we used the historical forecast data in International Monetary Fund (2022b).

[12] 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.

because it is widely considered to be a better predictor of future inflation than all-items inflation.[13]

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).[14] 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" (International Monetary Fund, 2022a). 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.[15]

We also use the economic crisis dummies of Reinhart and Rogoff (2009) provided in Reinhart and Rogoff (2011b), supplemented with the updates available on the Harvard Business School Behavioral Finance and Financial Stability website (Reinhart, 2016). Reinhart and Rogoff (2011a) define crisis indicators for several different types of crises; we create a single crisis indicator equal to one if Reinhart and Rogoff (2011a) code a country-year as having a crisis related to stock markets, the banking sector, currency crises, high inflation, or sovereign debt.[16]

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

[13] 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.

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

[15] Note that the OECD STINT variable contains flags for series breaks. When calculating one-year differences, we only take differences between data series that are from the same source.

[16] Reinhart and Rogoff (2011a) 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 (2011a) code any type of crisis as occurring in a given country-year observation, and which takes value 0 otherwise.

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 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**

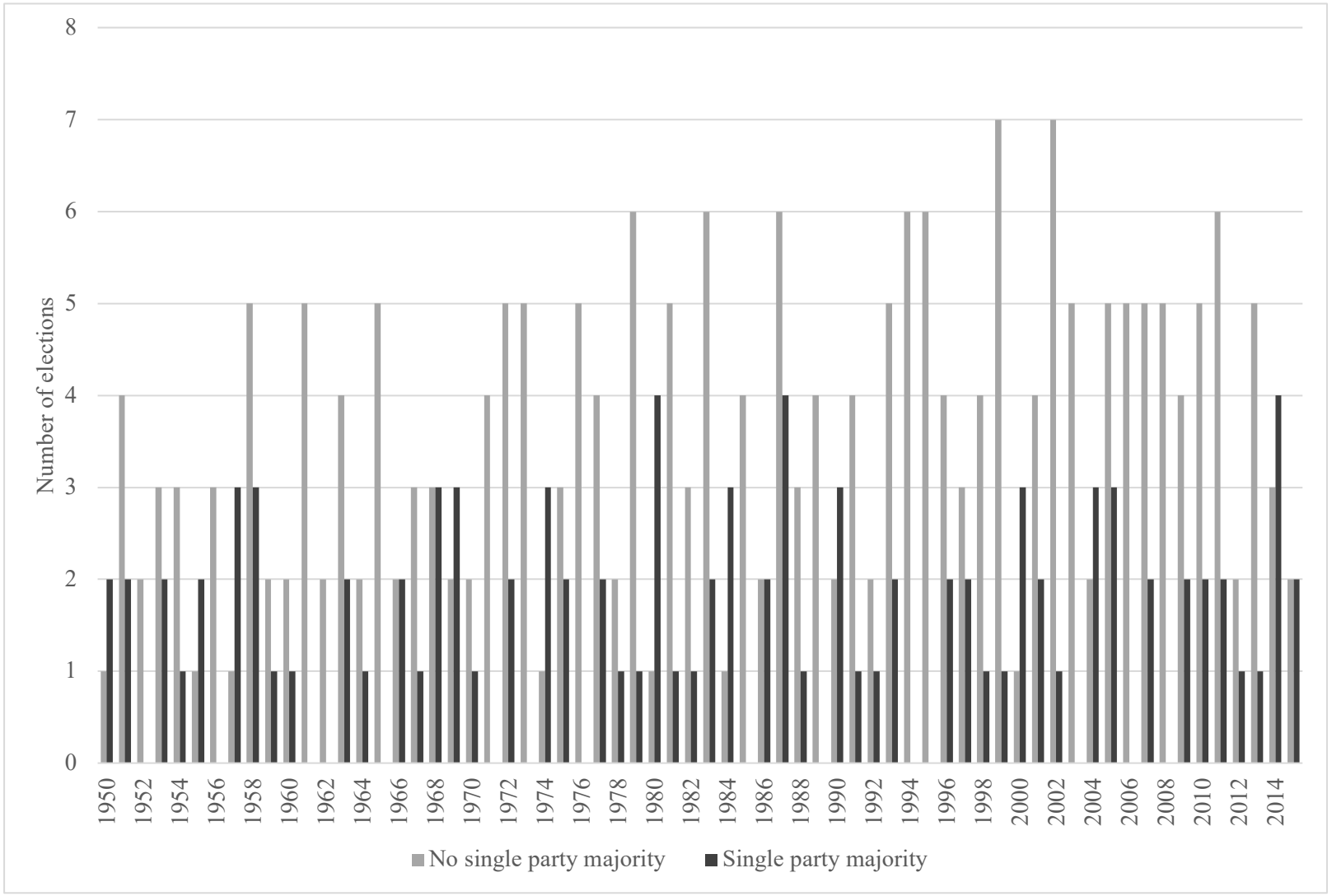
Notes: Bold indicates an election producing a single-party majority. (*) denotes countries with multiple electoral rules over the sample. [b] = Proportional, [c] = Mixed

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. (2017a) 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 I.B, 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 (International Monetary Fund, 2022b). 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-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.

Column (3) instead uses inflation forecasts reported in the historical OECD Economic Outlooks where these forecasts are available (OECD, 1996). 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.[17]

[17]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.

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.[18] The OECD forecasts are available for 226 observations in our main sample, so

[18]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.

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. (2017a) for interest rates, inflation, and public debt.

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

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.

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.

similar to our baseline estimate.[19]

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.

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.[20] 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.[21]

In order to test the sensitivity of the DSIR estimates, we implement a sequence of fuzzy RD designs by estimating a system of two equations, for each horizon $h = 1, 2, ..., H$:

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

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

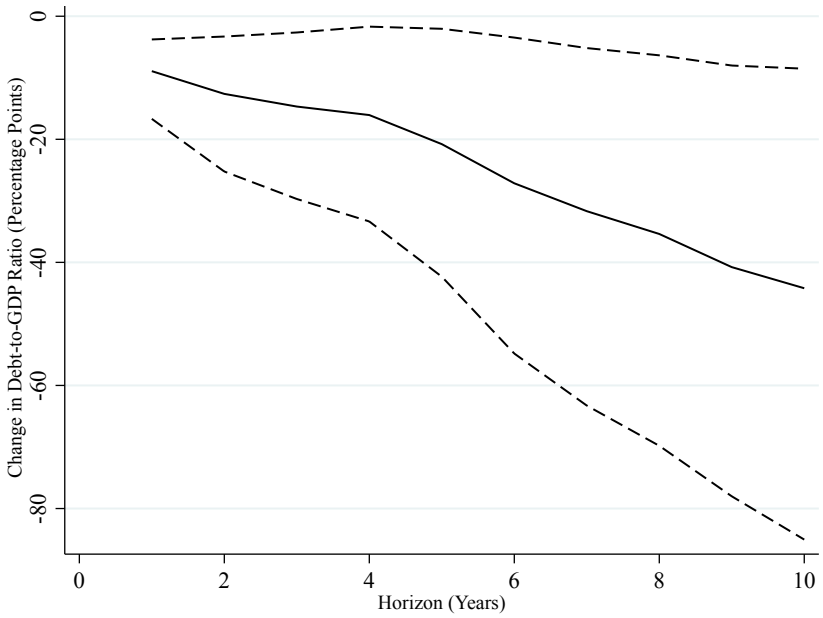
where $\Delta D_{it}^{h} = [D_{i,t+h} - D_{it}]$ is the $h-$year change in the debt-to-GDP ratio for country i between

[19] 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.

[20] 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.

[21] Note that the point estimate in year 5 does not 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



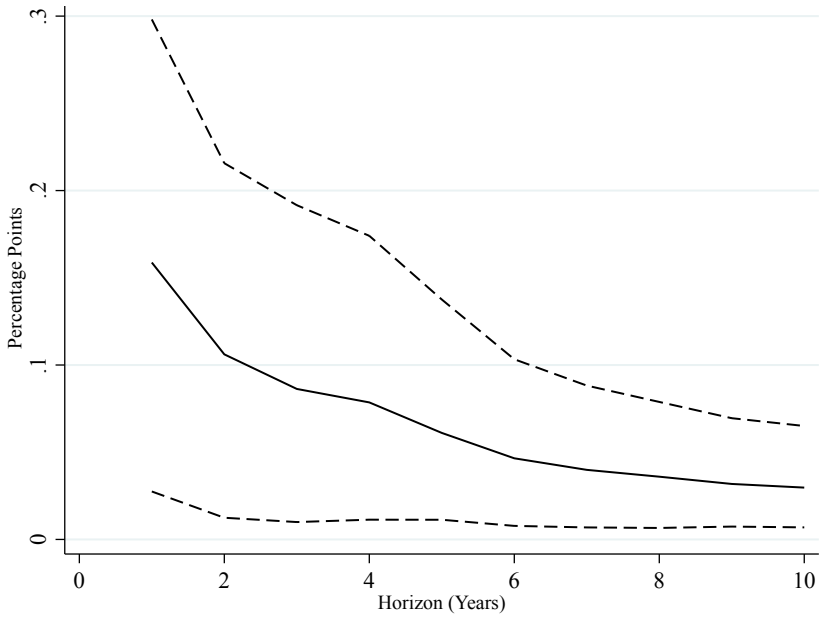
This figure displays the response of the debt-to-GDP ratio over the horizon specified on the horizontal axis from a sharp RD design. The estimation includes the covariates from the baseline specification and the sample is constructed to remain constant for each horizon. Dashed lines show 95% confidence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

election-year t and h years later. All other variables are defined as described in Section I.

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.[22] The decline in the estimated DSIR as we consider 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.

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

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.

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.[23]

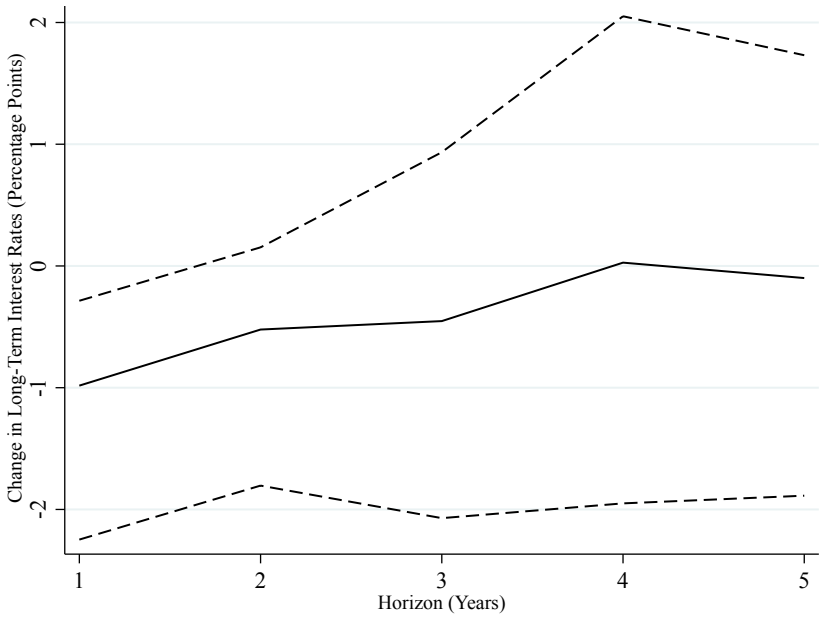
In Figure E.3 we estimate the dynamic effects of interest rate changes in longer horizons. We again construct the chart analogously to Figure 2 using equations (6)–(7). The most significant change in interest rates occurs in the first year following the election, and over time the confidence intervals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

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 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

[23] 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.

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.

regression discontinuity designs given by

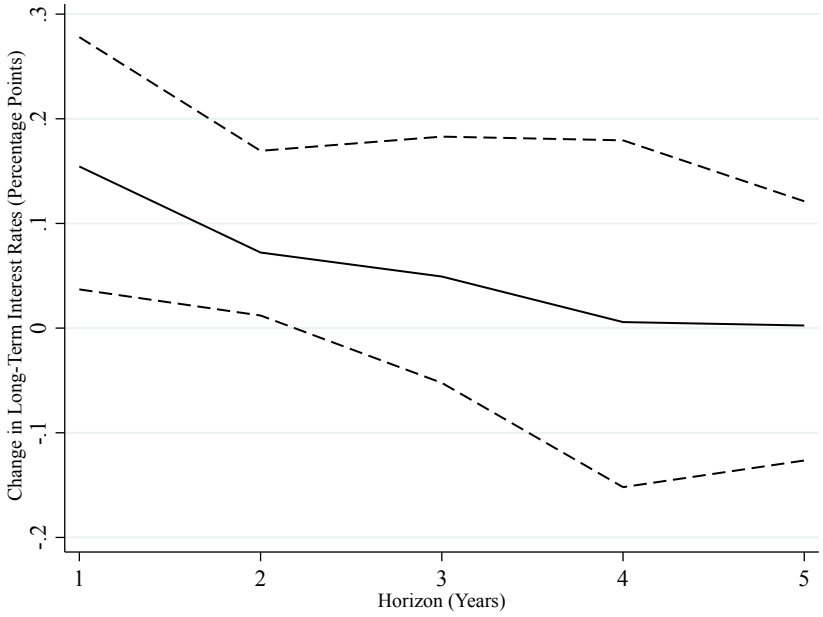
$$\Delta D_{it}^{h} = \gamma + \delta M_{it} + g(V_{it} - \overline{c}_{it}) + \nu_{it} \tag{E.3}$$

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

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:

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

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

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 avoid 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 $\Delta G_{it} = \frac{1}{5}(\sum_{\tau=t+1}^{t+5} G_{i\tau} - \sum_{\tau=t-4}^{t} G_{i\tau})$.

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.

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 $(R-G)$.[24] 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,

[24]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.

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 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.

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