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

Gabriel Ehrlich
University of Michigan

Aditi Thapar†
University of Michigan

November 1, 2020

Abstract

We use close elections in parliamentary democracies as natural experiments to estimate public debt levels' effects on real interest rates. We first estimate that an election in which no party achieves a parliamentary majority causes the debt-to-GDP ratio to increase by 21 percentage points over the following five years relative to an election in which one party barely secures a majority. We next estimate that real interest rates rise by a relative 119 basis points following such an election, implying that a one percentage point increase in the debt-to-GDP ratio causes a 5.6 basis point increase in real rates. That effect is larger than most previous estimates in the literature, suggesting the potential importance of simultaneity in the determination of real rates and government debt levels.

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

JEL Codes: E62, H63

*We thank seminar participants at the Congressional Budget Office, the International Online Public Finance Seminar Series, and the University of Michigan for helpful comments, and especially William Gale for a helpful discussion of the paper. We additionally thank Andres Blanco, Simon Gilchrist, James Hines, John Leahy, Ryan Nunn, and David Ratner, Matthew Shapiro, and Kent Smetters for helpful comments. We thank Shane Reed for outstanding research assistance, and Dimitrije Ruzic for outstanding research assistance and input on an early version of this project. Portions of this paper were completed while Dr. Thapar was visiting the Federal Reserve Bank of Kansas City as part of CSWEP's Summer Economic Fellows Program. Thapar is grateful to the National Science Foundation for their support under grant SES-1919362.

†Please email the authors at gehrlich@umich.edu or athapar@umich.edu.

"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)

"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."—*Economics Nobel Laureate Paul Krugman, August 2016* (Krugman, 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)

1 Introduction

Economists have debated whether and to what extent government debt levels affect real interest rates at least since the work of Ricardo (1820). The recent increase in debt-to-GDP ratios across many economies brought about by the COVID-19 pandemic gives this question renewed urgency. Studying the topic is difficult, however, because of the many possible factors that may confound the empirical relationship between debt and interest rates. In this paper, we use a regression discontinuity (RD) design that exploits exogenous variation in debt levels resulting from elections in which the largest party barely won or barely missed a parliamentary majority. We estimate that a one percentage point increase in the government debt-to-GDP ratio causes long-term real interest rates to increase by 5.6 basis points, a substantially larger effect than most previous estimates.[1]

Our RD design addresses two major endogeneity problems that will tend to obscure any tendency for higher debt levels to raise real rates in observational data. The first problem is that deficits tend to be counter-cyclical and real interest rates tend to be pro-cyclical in modern economies; this problem could be called "automatic fiscal endogeneity." Addressing these business-cycle correlations has been a major focus of the literature over the past twenty years. The second major potential source of endogeneity is simultaneity in the determination of debt levels and real interest

[1] To be clear, we began work on this paper in 2017, and we do not intend the paper to offer any position on the appropriate extent of fiscal stimulus during the COVID-19 pandemic.

rates, or "discretionary fiscal endogeneity": at the same time that higher government debt levels may drive up real interest rates, low real rates may induce governments to borrow more freely.[2] Addressing the potential problem of discretionary fiscal endogeneity does not appear to have been an important focus of the previous literature.[3]

The sentiment that low interest rates mean it is a good time for the government to borrow appears to cross both ideological lines and international borders. If this logic affects governments' borrowing decisions, the resulting discretionary fiscal endogeneity will tend to confound observational estimates of debt levels' effects on real interest rates.[4] An exogenous source of variation in government debt levels will then be necessary to account for the potential reverse causality.

We locate a plausibly exogenous source of variation in government debt levels in the tendency of single-party majority governments in parliamentary systems to exhibit more fiscal discipline than coalition and minority governments. In their pioneering work documenting this pattern, Roubini and Sachs (1989a) describe it 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

[2] We thank William Gale for suggesting the terms automatic and discretionary fiscal endogeneity to us.

[3] For instance, Laubach (2009) states, "One major obstacle in obtaining empirical estimates is the need to isolate the effects of fiscal policy from the many other factors affecting interest rates. The most obvious of these factors is the state of the business cycle. ... Of course, there are many conceivable factors that jointly determine fiscal variables and interest rates, and it is unlikely that a reduced-form regression would ever completely overcome this endogeneity problem, but focusing on long-horizon expectations is an important step in the right direction."

[4] In principle, the logic that low interest rates make it more advantageous for governments to issue debt depends on the reasons why interest rates are low (e.g. Garín et al., 2019). At least currently, however, most of the likely reasons for low real interest rates do appear to militate in favor of higher government debt levels. For instance, in their careful survey Elmendorf and Sheiner (2017) conclude, "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." We are not aware of any reasons that low interest rates would signal that it is a *worse* time for the government to borrow.

mechanisms among coalition partners to assure the cooperative outcome will often be very weak.

Our empirical strategy proceeds in four main steps:

- First, we estimate that five years after an election, the debt-to-GDP ratio in countries where the party winning the most seats barely achieved a majority is 21 percentage points lower than in countries where the largest party barely failed to do so. We thus provide strong causal evidence in support of the thesis of Roubini and Sachs (1989a) and most, but not all, of the related literature, which predates the widespread adoption of regression discontinuity designs in econometrics.

- Second, we estimate that real long-term interest rates fall by an average of 1.2 percentage points after an election in which the largest party barely won a majority relative to an election in which the largest party barely missed a majority.

- Third, we take the ratio of the preceding two parameters to estimate that a one percentage-point increase in the debt-to-GDP ratio raises real rates by 5.6 basis points.

- Fourth, we examine potential sources of confounding and violations of the exclusion restriction that justifies interpreting our estimates as the causal effect of debt levels on real long-term interest rates. We argue that the evidence does not suggest meaningful problems on either of those fronts.

The effect of government debt levels on real interest rates that we estimate is substantially larger than most estimates in the literature. For instance, Blanchard (2019) references a "standard (but admittedly rather uncertain as well) back-of-the-envelope number that an increase in debt of 1 percent of GDP increases the safe rate by 2–3 basis points." We therefore interpret our results as suggesting that discretionary fiscal endogeneity is an important factor to consider when studying the extent to which public debt levels increase real interest rates.

A natural question concerns the mechanisms that give rise to the large effect of public debt on real interest rates that we estimate. We present some limited evidence that the traditional channel of public debt "crowding out" productive investment is unlikely to explain an effect of the size that we estimate, and that therefore a risk channel is also likely to play a role. An important limitation of our identification strategy is that, because of the small sample of elections in our study, we cannot examine the mechanisms that generate the effect that we estimate with the rigor that we would like. We view this topic as an important avenue for future research.

2 Literature Review

This paper relates closely to two literatures: first, the literature that attempts to estimate the effects of public debt levels on the interest rates for that debt, and second, the literature that studies the political economy determinants of government debt levels through the lens of the type of government formed following parliamentary elections.

2.1 Public Debt and Interest Rates

Elmendorf and Mankiw (1999) and Gale and Orszag (2004) provide useful surveys of the large literature studying the effects of public deficits or debt on interest rates. Gale and Orszag (2004) divide the literature into three main categories: studies that examine the effects of expected future deficits, typically as measured from published forecasts; studies that use vector autoregressions (VARs) to account for expectations of the fiscal trajectory; and studies that examine the impact of current deficits or debts. We focus our discussion of studies of the United States' experience on the first approach in light of its dominance in the modern literature.[5] The high data requirements of this approach typically render applying it outside of the relatively recent history of the United States infeasible, so we discuss alternative approaches in the international context.

The studies that focus on anticipated future debt levels have tended to find that a one-percentage point increase in the expected future U.S. debt-to-GDP ratio causes an increase in real long-term

[5] Gale and Orszag (2004) argue that when practical, this approach is the most reliable of the three. First, as noted by Feldstein (1986): "... it is wrong to relate the rate of interest to the concurrent budget deficit without taking into account the anticipated future deficits. ... Similarly, it is inappropriate to relate the interest rate to the value of the government debt without taking into account the anticipated future budget deficits." Second, as shown by Bernheim (1987), Cohen and Garnier (1991), Elmendorf (1993), and Hall and Thapar (2019) VAR-based forecasts of future fiscal variables are less accurate than forecasts produced by government and private sector forecasters. Third, as Elmendorf and Mankiw (1999) point out, "Measurement error in the proxies for expectations biases the estimated coefficients toward zero."

Treasury interest rates of between 2 and 5 basis points.[6] Laubach (2009) proposes a particularly influential approach, to study the relationship between projected debt levels and real forward long-term Treasury rates, rather than contemporaneous rates. He argues that using forward rates insulates the estimates from automatic fiscal endogeneity, or confounding from the state of the business cycle. He estimates that a one percentage-point increase in the projected debt-to-GDP ratio causes a 3 to 4 basis point increase in real long-term Treasury rates. Using similar approaches, Gale and Orszag (2004) estimate an effect of between 4 and 6 basis points, Engen and Hubbard (2004) estimate an effect of approximately 3 basis points, and Gamber and Seliski (2019) estimate an effect of 2 to 3 basis points. These estimates are roughly consistent with the description in Blanchard (2019) of a "back-of-the-envelope number that an increase in debt of 1 percent of GDP increases the safe rate by 2–3 basis points," although there are certainly estimates in the literature above 3 basis points.

Studies focusing on international evidence have faced additional obstacles, including for most countries the lack of a long history of consistently defined market interest rates and current and projected levels of debt. Faini (2006) studies the effects of fiscal policy on interest rates in 10 countries in the European Monetary Union (EMU) using a reduced form approach grounded in Keynesian theory. He estimates that "The coefficient on the EMU fiscal balance suggests that a 1% fall in the primary surplus boosts real interest rates by 41 basis points. ... the debt stock plays no statistically significant role at the country level, but once again is quite significant for EMU as a whole..." Kinoshita (2006) studies a panel data set including 19 OECD countries at a five-year frequency to abstract from business cycle considerations. He estimates that "... a one percentage point increase in the government debt-to-GDP ratio raises the real long-term interest rate by about 2 basis points." When he allows for heterogeneity in the effect across countries, he finds a larger average effect of 4 to 5 basis points. Ardagna et al. (2007) study a panel of 16 OECD countries.

[6]Results from similar study designs reported in terms of the deficit rather than the debt find that a one-percentage point increase in the expected future deficit-to-GDP (or GNP) ratio causes an increase of between 15 and 43 basis points; see, e.g., Elmendorf (1993), Canzoneri et al. (2002), Gale and Orszag (2004), Laubach (2009), and Gamber and Seliski (2019). An ongoing discussion in the literature concerns whether deficits or debts are a more appropriate metric. We focus on changes in the debt-to-GDP ratio in this study because of data availability considerations. As Laubach (2009) and Gamber and Seliski (2019) among others point out, the persistence in the deficit-to-GDP ratio means that the larger effects typically estimated for the deficit-to-GDP ratio are consistent with the smaller effects estimated for the debt-to-GDP ratio.

Because projections of future debt levels and deficits are not available for all of the countries in their panel, they use a VAR system to estimate expectations. They estimate a non-linear effect of public debt levels on real interest rates, with effect sizes that translate to 0.4 basis point of a one-percentage point increase in the debt-to-GDP ratio for a country with a pre-existing ratio of 58 percent (the U.S. level in 2002) and a 3.3 basis point effect for a country with a pre-existing debt-to-GDP ratio of 119 percent (Italy's level in 2002). Likewise, Greenlaw et al. (2013) study a panel of 20 advanced countries using standard panel regression methods. They report, "We find a significant relation between debt loads and borrowing costs, with a one-percentage-point increase in debt as a percent of GDP being associated with a 4.5-basis-point increase in the yield on 10-year government bonds in a linear specification. We also find strong evidence of nonlinearities. Interest rates rise much more quickly at higher debt levels..." Rachel and Summers (2019) relate estimated neutral interest rates to government debt levels in the United States, Canada, the Euro Area, and the United Kingdom using pooled regression, fixed effects regression, and so-called "between" regression (using economy-level averages). Only the "between" regressions indicate that debt levels have a positive effect on real interest rates, which they interpret as suggesting that "the secular downward trend in interest rates, which coincided with increasing government debt, dominates these econometric estimates."

Our study adds to this literature by exploiting quasi-experimental evidence from a regression discontinuity design, which suggests government debt levels have a larger effect on interest rates than has generally been estimated. The quasi-exogenous variation in debt levels generated by the design should isolate the effects of debt levels on interest rates from potential confounding both from automatic and discretionary fiscal endogeneity. Our design's ability to address discretionary fiscal endogeneity (simultaneity in the determination of debt levels and real interest rates) may explain why our estimates are higher than the effects that have typically been estimated in the literature.

One limitation of our approach, which is common to quasi-experimental designs generally and especially to RD designs, is that we exploit only a subset of the variation in the treatment that is likely to be exogenous, reducing the potential for confounding in our estimates but reducing our statistical power. This trade-off is especially severe in our setting because of our small sample size. Therefore, in most cases it is impractical for us to study potential non-linearities or the mechanisms that generate our estimated effect using subsample analysis.

A final note on our paper's relationship to the literature on public debt's effects on real interest rates concerns its connection to the influential theory of Ricardian Equivalence pioneered by Barro (1974). As Elmendorf (1996) describes the theory, "That proposition concerns changes in the financing of government spending, *holding the amount of that spending constant*." In contrast, our empirical approach estimates the effects of changes in debt-to-GDP ratios without distinguishing between changes in government revenues and changes in government expenditures. We consider our estimates to reflect the weighted average effect of changes in government debt-to-GDP ratios arising from the two sources. While we believe this estimated average effect has important real-world relevance, we note that, like much of the literature, our empirical strategy does not provide a direct test of the theory of Ricardian Equivalence. Gale and Orszag (2004) provide one of the few attempts to estimate the effects of government spending and revenues separately; their estimates do not indicate substantially asymmetric effects of changes in projected revenues and primary outlays on forward real interest rates.

2.2 Single-Party Majorities and Public Debt Levels

This paper also relates to substantial literature that relates government deficits and debt levels to the structure of parliamentary governments, such as whether the government comprises a single-party majority, a multi-party coalition, or a parliamentary minority.[7] Roubini and Sachs (1989a) estimate that in industrial democracies from 1975–86, "the difference between a majority government and a minority government... is... 1.5 percentage points of added budget deficit per year."[8] They accompany their econometric estimates with a nuanced discussion of the reasons this pattern might arise and historical examples illustrating the mechanisms. Notable empirical studies that are generally supportive of Roubini and Sachs' conclusion include Alesina and Perotti (1995), Alesina et

[7]That literature is only a small part of the literature that studies the political economy determinants of public debt levels, doing justice to which would be beyond the scope of this paper.

[8]Roubini and Sachs (1989b) estimate an impact of around 1 percent of GNP using a different sample and methodology.

al. (1998), and Perotti and Kontopoulos (2002). An additional result in this literature, however, is that the estimated extent of this difference can be sensitive to the exact coding of different types of government (e.g., coalitions with various numbers of parties, minority and caretaker governments, etc.) and to different regression specifications.[9] Alesina and Drazen (1991) and Velasco (1999) provide formal theoretical models for the pattern highlighted by Roubini and Sachs (1989a) might arise, focusing on the fragmentation in the decision-making process brought about by coalition governments; Alesina and Drazen (1991) also provide a series of historical case studies illustrating the empirical regularity.

Our study complements this literature, which has generally used reduced form methods, by providing causal estimates that provide strong support for its main conclusions. The first stage of our RD design shows that the public debt-to-GDP ratio rises substantially in the five years following an election in which the party winning the largest number of seats just failed to achieve a parliamentary majority relative to an election in which the largest party barely won a majority. The quantitative, seat-based nature of our design prevents us from differentiating between the various types of non-single-party majority governments (e.g., minority governments, coalition governments with few parties, coalition governments with many parties), but it also removes the potential ambiguity associated with coding various types of governments.

3 Empirical Approach

Our core empirical strategy in this paper is to use a fuzzy regression discontinuity design to estimate the effects of government debt levels on real interest rates. Section 3.1 summarizes the regression discontinuity design and estimation, Section 3.2 summzarizes the data sources and variable definitions, and Section 3.3 discusses some common checks for the validity of the design.[10]

[9]Roubini and Sachs (1989a,b) distinguish between one-party majority governments, coalitions with two parties, coalitions with three or more parties, and minority governments. Edin and Ohlsson (1991) argue that minority governments rather than coalition governments drive their results. De Haan and Sturm (1994) and Perotti and Kontopoulos (2002) show that Roubini and Sachs' results can be sensitive to the exact coding of different types of governments and regression specifications. Our approach sidesteps those issues by comparing elections that barely produce a single-party majority with those that barely do not, without distinguishing between the resulting types of government. Grilli et al. (1991) find that when government durability and stability are considered alongside the presence of a majority government, durability is predictive of higher debt levels while the other two factors are not.

[10]Appendix D provides additional detail on the data, and Appendix A contains some further discussion of the validity of the RD design.

3.1 Regression Discontinuity Design

Ideally, we would like to estimate the equation:

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

where the unit of observation is country i at time t, ΔR represents the change in real long-term interest rates, ΔD represents the change in the government debt-to-GDP ratio, and ω_{it} is a random error. Because government debt levels and real interest rates are determined simultaneously in a complex macroeconomic system, ω is very likely to be correlated with ΔD, and ordinary least squares estimation of equation (1) is likely to yield an inconsistent estimate of public debt's true effect on real interest rates, β.

We therefore instead implement a fuzzy regression discontinuity (RD) design by estimating a system of two equations:

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

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

where ΔR and ΔD are defined as in equation (1), but the unit of observation is now country i holding a parliamentary election at time t. V represents the share of seats won by the largest party in the election, normalized as described in Section 3.2 below, and $\overline{c}$ represents the normalized seat threshold necessary for a party to win a parliamentary majority. M is a dummy variable that captures whether the largest party won a majority in the election:

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

ε and ν are uncorrelated random errors, and $f(\cdot)$ and $g(\cdot)$ are functions that capture how R and D vary with V away from the cutoff $\overline{c}$.

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

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

A large literature studies the estimation of RD designs. We estimate the functions $f(\cdot)$ and $g(\cdot)$ non-parametrically as separate local linear functions on each side of the cutoff $\bar{c}$ using triangular kernels and mean square error-optimal bandwidths.[11] The p-values we report in the paper are constructed using the procedure of Calonico et al. (2014), which corrects for the potential bias of the RD estimator in constructing the confidence intervals for the estimates of τ and δ using quadratic local polynomial estimates.

Most of our specifications, including our baseline specification, include covariates in the RD design, which we handle as described in Calonico et al. (2019). In contrast to the motivation for including additional covariates in many other designs for estimating causal effects, our motivation ot include them in the RD design is not to control for potential confounding, which, as we will discuss in Section 3.3 below, ought to be absent if pre-treatment covariates are not discontinuous at cutoff for a single-party majority.[12] Instead, 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, and a lagged indicator variable for an economic crisis.

The first two control variables, real GDP growth and inflation, are standard in the macroeconomic literature studying the effects of government fiscal policy. For instance, Auerbach and Gorodnichenko (2017) include both variables as controls in their baseline specification. Ramey and Zubairy (2018) control for real per capita GDP divided by trend GDP in their base specification and include inflation as an additional control variable in a robustness check. Blanchard and Perotti (2002) argue that because government budgeting is typically conducted in nominal terms and personal income tax brackets are indexed with a lag, "...inflation shocks are likely to affect

[11]Table 9 in Appendix A.3 examines the estimates' robustness to alternative econometric specifications.

[12]In Section 5.2, we do perform a series of robustness checks in which we include additional control variables to assess the potential for confounding in our results, but that is not our motivation for including covariates in our baseline specification.

both spending and taxes." Our third control variable, the indicator for a lagged economic crisis, serves to control for the potential effects of economic crises on debt levels and real interest rates, a theme emphasized in Reinhart and Rogoff (2009). Furthermore, economic crises may cause political turnover, as measured by Prime Ministerial resignations, falls of government, and subsequent election results (Chwieroth and Walter, 2010).

3.2 Data Sources and Variable Definitions

To implement our RD design, we require 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. The unit of observation in our model is a country-election. All economic data is measured at the annual frequency. We begin our analysis in 1950 to avoid any measurement or other confounding issues related to World War II and its short-term aftermath. Our baseline estimation sample includes data from 25 countries and 308 election-year observations over the period 1950 to 2010.

We use a number of data sources to construct our baseline data set. We summarize the data series, their sources, and their construction briefly here. Appendix D at the end of the paper provides further detail.

Our main data source for election outcomes is Seki and Williams (2014), who extend the data set of Woldendorp et al. (2000) to include 48 countries covering the period 1945–2014.[13] We supplement the data in Seki and Williams (2014) with data from Döring and Manow (2019). Appendix D.1 describes how we combine data from the two sources and how we code our running variable $(V_{it} - \overline{c}_{it})$ and our indicator for a single-party majority, M_{it}. Briefly, $(V_{it} - \overline{c}_{it})$ is defined as the share of parliamentary seats won by the largest party in the election minus 50 percentage points. We code M_{it} as one if the largest party won any number of seats more than exactly 50 percent, but we code it as zero if the largest party won exactly 50 percent of the seats; in other words, we require a strict majority for M_{it} to be coded as one.

Our main source for government debt-to-GDP ratios is Reinhart and Rogoff (2011), which ex-

[13]Some other 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).

tends from before World War II through 2010 for most of the countries in our sample. We use the central government debt-to-GDP ratio where it is available. We operationalize the variable ΔD_{it} as the change in the government debt-to-GDP ratio from the year of the election to five years after the election, a choice we discuss further below.

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, $\Delta \Pi^e_{it}$. We calculate ΔI_{it} as the one-year change from the year of the election to the year following the election. We follow Council of Economic Advisers (2015) in proxying for expected inflation Π^e_{it} as the five-year unweighted moving average of current and past annual inflation. We describe our construction of our series for the change in real interest rates ΔR_{it} in substantially more detail in Appendix D.2.

Our choice to compare the one-year change in real long-term interest rates to the five-year change in the debt-to-GDP ratio is meant to account for market participants' medium-term expectations of future deficits and debt following an election, rather than current levels only. Feldstein (1986) argues that taking into account anticipated future budget deficits is an essential part of any analysis relating deficits to interest rates. That view has been widely accepted in the literature, which has often focused on expected debt or deficits five years in the future (e.g. Laubach, 2009; Gale and Orszag, 2004; Engen and Hubbard, 2004; Gamber and Seliski, 2019). Appendix C discusses our results using different horizons for the change in the debt-to-GDP ratio.[14]

The variables we have discussed above are sufficient to run our simplest RDD specification, but as noted, in most of our specifications, we include the lagged growth rate of real GDP, the lagged inflation rate, and a lagged indicator variable for an economic crisis as additional covariates. We take the growth rate of real GDP from the 2018 release of the Maddison Project (Bolt et al., 2018), the inflation series from Reinhart and Rogoff (2011), and the economic crisis dummies of Reinhart and Rogoff (2011).[15]

[14]We note as well that Engen and Hubbard (2004) argue that economic theory suggests that the level of debt should influence the level of interest rates, while the change in debt should influence the change in interest rates. Our specification here is consistent with their preferred specification.

[15]The authors define crisis indicators for several different types of crises; we create a single crisis indicator that takes value one if Reinhart and Rogoff (2011) code a country-year as having a crisis related to stock markets, the banking sector, currency crises, high inflation, or sovereign debt.

In Section 4.4, we use a separate data set for all macroeconomic variables from the "macrohistory database" of Jordà et al. (2017). That data set contains all of the economic variables necessary to conduct our core analysis for a panel of 16 parliamentary democracies. In Section 5, we include additional covariates in our RD design; we describe the sources of those additional covariates along with our discussion of those specifications in that section.

3.3 Validity and Interpretation of the Regression Discontinuity Design

A number of identifying assumptions are necessary for us to interpret our fuzzy RD estimates as the causal effect of government debt levels on real interest rates. Hahn et al. (2001) provided an early formalization of those conditions; our discussion here will generally follow Lee and Lemieux (2010). The first necessary condition is that the growth of debt-to-GDP ratios changes discontinuously at the threshold for a single-party majority. Our first-stage estimates indicate that this discontinuity is empirically large and statistically significant, consistent with most of the earlier literature on the topic. The second necessary condition is "monotonicity," which requires that, near the cutoff, a single-party majority always leads to a decline in the debt-to-GDP ratio relative to no single-party majority. In the case of a binary treatment variable, this condition is equivalent to requiring no "defiers" for whom the intent-to-treat disincentives treatment. We cannot test formally that there are no defiers in our data set, but the previous literature has focused empirically and theoretically on the hypothesis that non-single-party majority governments exhibit less fiscal discipline (e.g. Roubini and Sachs, 1989a; Alesina and Drazen, 1991).

Another assumption for the validity of the regression discontinuity design is that there is imperfect sorting of the largest party's parliamentary seat share V around the cutoff $\bar{c}$. Lee and Lemieux (2010) term this condition "imprecise control," and show that when this condition holds the treatment M is "as good as randomly assigned" around the cutoff. We believe that the "imprecise control" assumption is natural in our context, as it amounts to the requirement that political parties cannot precisely choose the number of parliamentary seats that they win in the vicinity of the cutoff for a majority. This local randomization result additionally has two implications that are commonly used as falsification tests for the design. First, the density of the running variable V

should be continuous at the cutoff (McCrary, 2008). Second, the observed covariates should be continuous at the cutoff (Lee, 2008). We present results from these tests in Section 3.3.1 below, showing that the results are consistent with the running variable being locally randomized around the cutoff for a single-party majority.

The final assumption for the interpretation of our fuzzy RD results as recovering the causal effect of government debt levels on real interest rates β is the exclusion restriction that a single-party majority cannot affect real interest rates except through its effect on debt levels. Formally, this is equivalent to the requirement that M_{it} should be excluded from equation (1). The exclusion restriction is not formally testable, but we provide a detailed examination of its plausibility in our context in Section 5.

3.3.1 Manipulation and Covariate Balance Tests of the RD Design

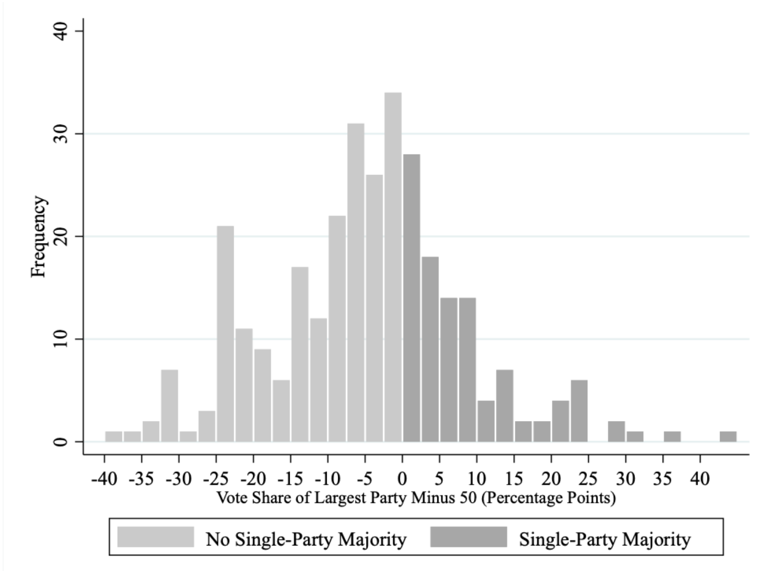
This section presents results from two common falsification tests of the local randomization assumption in the RD design. The first tests for a discontinuity in the distribution of the share of parliamentary seats won by the largest party around the cutoff for a majority. The second tests for discontinuities in predetermined variables of interest at the cutoff.

Figure 1 provides visual evidence and a formal test consistent with the notion of local randomization at the cutoff. The top panel 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 from 1950–2010, while the bottom panel shows the estimated kernel density of the same variable. The histogram and kernel density both appear continuous near the cutoff. The bottom panel of Figure 1 also reports the results from a formal test of the null hypothesis that there is no discontinuity at the cutoff.[16] The density is calculated separately on each side of the cutoff, and the shaded area represents the 95% confidence interval of the density function. The p-value of 0.73, indicated below the figure, implies that the test cannot reject the null hypothesis of no difference in the density function of observations above and below the cutoff.

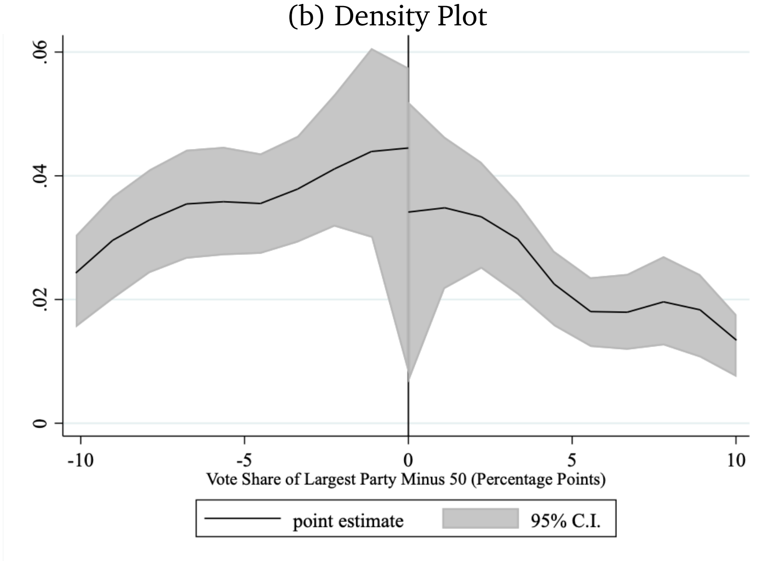
[16] Both the kernel density plot and the test results are implemented using the `rddensity` package (Cattaneo et al., 2018, 2019) with local polynomials of order one. We used our baseline sample, including only observations that are used in the RD estimation, to estimate the density function. Using all of the observations in the $1950-2010$ sample does not change the results of the density manipulation test.

Figure 1: Histogram and Density of Vote Share

(a) Histogram



(b) Density Plot



Coefficient: -0.48
P-value: 0.73

Panel (a) displays a histogram of frequency counts of the election results used in the baseline estimation in Table 1. Panel (b) displays kernel density estimates of the corresponding density of election results. The coefficient and p-value reported below Panel (b) are from a test of the continuity of the density at the cutoff, as described in Cattaneo et al. (2018).

If the local randomization result holds, there should be no systematic sorting of outcomes that were determined prior to each election, and we should not observe discontinuities in predetermined variables at the cutoff. To test this implication of local randomization, we conduct sharp RD tests of two lags each of five predetermined variables: real GDP growth, consumer price inflation, economic crisis indicators, real long-term interest rates, and the debt-to-GDP ratio. For each of these variables, we cannot reject the null hypothesis of no discontinuity at the cutoff. These results are again consistent with the notion of local randomization. Table 6 in Appendix A presents the results of these tests, while figure 4 plots each covariate's behavior around the cutoff graphically.

4 Regression Discontinuity Evidence on Public Debt's Effects on Real Interest Rates

In this section we present and discuss results from our regression discontinuity estimates of public debt levels' effects on real interest rates. Section 4.1 presents our baseline RD estimates, Section 4.2 presents related graphical estimates, and Section 4.3 discusses the results' interpretation in an economic context. Section 4.4 examines the results' stability across alternative samples and presents the results using an alternative data set for the macroeconomic variables. Appendix A.3 discusses the results' robustness to alternative econometric specifications. In Section 5 below, we examine the assumed exclusion restriction that allows us to interpret our results as the causal effect of changes in the public debt-to-GDP ratios on real long-term interest rates.

4.1 Baseline Results: Statistical Evidence of Public Debt's Effects on Real Rates

Table 1 presents the main results from the fuzzy RD design framework in equations (2)–(4) of Section 3.1. The top panel contains results from the first stage of the RD approach, which estimates the effect of a single-party majority on the change in the government debt-to-GDP ratio, represented by δ in equation (2). The middle panel of the table presents the results from the reduced form of the fuzzy RD approach, which estimates the effect of a single-party majority on long-term real interest rates, represented by τ in equation (3). The bottom panel presents the main results of interest from the second-stage of the RD approach, the effect of government debt on real interest rates β, which is estimated as the ratio $\hat{\tau}/\hat{\delta}$.

Column 1 presents results from our baseline specification, which includes lagged real GDP growth, lagged inflation, and a lagged economic crisis indicator as covariates in the RD design. The estimated coefficient of -21.39 in the first stage implies that a single-party majority causes a decline of 21.39 percentage points in a country's debt-to-GDP ratio from the year of the election to five years later, relative to the counterfactual of no single-party majority. This estimate is highly statistically significant, with a bias-robust p-value of 0.02.[17] It is also economically large: the standard deviation of the five-year change in the debt-to-GDP ratio is 15.11 percentage points for the countries used in our baseline estimation over the period 1950–2010. The estimated coefficient of -1.19 in the reduced form equation implies that a single-party majority causes real long-term interest rates to decline by 1.19 percentage points from the year of the election to one year later (relative to the counterfactual). The estimated effect is again statistically significant at the 2 percent level and economically large: the standard deviation of the one-year change in real long-term interest rates is 1.65 percentage points for the countries in our sample over the years 1950–2010. Finally, the coefficient of 0.056 in the second stage implies that a one percentage-point increase in the debt-to-GDP ratio causes real long-term interest rates to rise by 5.6 basis points. The bias-robust p-value on that estimate is 0.014, implying that the estimate is statistically significant at conventional levels.

Column 2 includes the United States in the sample, using results from elections for the House of Representatives to construct the running variable $V_{it} - \bar{c}_{it}$. Our baseline sample excludes the United States from the analysis because its system of government is not parliamentary, and it has never featured a multi-party or coalition government (Seki and Williams, 2014). The United States is also the only nation for which Seki and Williams (2014) code parties for the upper house of the legislature, perhaps an indication of the U.S. Senate's unusually powerful role.[18] To be clear, we believe that the economic effects of public debt levels on real interest rates in the United

[17] We note again that the p-value comes from the bias-correction procedure proposed by Calonico et al. (2014), which involves estimating a higher-order local polynomial in the calculation of the p-value than in estimating the size of the discontinuity at the cutoff. Calonico et al. (2014) show that this procedure is robust to potentially incorrect inference associated with local linear regressions at the boundary of the estimation window.

[18] White (2003) claims that the U.S. Senate is the "Only upper house in the world more powerful than the lower house."

States should be similar to the effects in the parliamentary democracies in our baseline sample; we exclude the United States from our baseline sample simply because it fits less clearly with our identification strategy. Nonetheless, Roubini and Sachs (1989b) include the United States in their analysis, and they note that, like the United States, France and Finland also have mixed parliamentary-presidential systems. Including the United States in the sample lowers the estimated second-stage effect slightly, to a 5.2 basis points increase in real long-term interest rates per one percentage point increase in the debt-to-GDP ratio. The estimate remains statistically significant at conventional confidence levels.

Column 3 presents results from our baseline specification without covariates.[19] The estimate of a single-party majority's effect on relative real long-term interest rates is very similar to the estimate from column 1, but the estimated effect on debt-to-GDP ratios in the first-stage equation is smaller, at 15.41 percentage points. That smaller first-stage effect translates into a larger estimated second-stage effect of a 7.9 basis points increase in real long-term interest rates per one percentage point increase in the debt-to-GDP ratio. The estimated effect is less precise, however, with a p-value of 0.056. Indeed, improving the statistical significance of our estimates is our primary motivation for including the covariates in our baseline specification.Finally, we note that the results in our baseline sample are conservative compared with the results from this specification.

To confirm that our results are not driven by outliers, column 4 of Table 1 presents a specifications analogous to column 1, but with values for the changes in real long-term interest rates and the debt-to-GDP ratio winsorized at the 1st and 99th percentiles.[20] The estimated second-stage effect increases slightly from the baseline estimate in column 1, and it remains statistically significant at standard confidence levels. We conclude that our results are not driven by outliers in the data.

[19]The sample also includes three observations for Iceland, which was excluded from the baseline sample in column 1 because it is missing data for the economic crisis indicator covariate.

[20]We winsorize the top 1% and bottom 1% of each variable individually. The winsorization is conducted using only data that is used in the estimation. The 1st- and 99th-percentile changes for the debt-to-GDP ratio are -30.3 and 47.8 percentage points, respectively (on a five-year basis). The 1st- and 99th-percentile changes for the long-term real interest rate are -3.9 and 4.3 percentage points.

Table 1: Baseline Dataset - Regression Discontinuity Design Estimates

	(1) **Baseline** With Covariates	(2) Include US With Covariates	(3) No Covariates	(4) Winsorized With Covariates
First Stage: Effect of Single-Party Majority on Debt-to-GDP Ratios				
Debt-to-GDP ratio	-21.39	-19.14	-15.41	-18.50
Cluster-robust standard error	10.13	9.62	9.96	8.26
Bias-robust p-value	0.02	0.02	0.07	0.01
Reduced Form: Effect of Single-Party Majority on Real Interest Rates				
Real long-rate	-1.19	-0.99	-1.22	-1.15
Cluster-robust standard error	0.57	0.56	0.60	0.53
Bias-robust p-value	0.02	0.04	0.02	0.01
Second Stage: Effect of Debt-to-GDP Ratios on Real Interest Rates				
Real long-rate	0.056	0.052	0.079	0.062
Cluster-robust standard error	0.023	0.023	0.041	0.027
Bias-robust p-value	0.014	0.028	0.056	0.020
Bandwidth	6.36	6.64	7.26	6.35
Number of Observations	308	336	311	308
Number of countries	25	26	26	25

The data is from the baseline sources described in Section 3.2. The unit of observation is a country-election. Changes in debt-to-GDP ratios are calculated from the calendar year of an election to five years after the election, and the changes in real interest rates are calculated from the calendar year of an election to the calendar year after the election. Long-term real interest rates are calculated by subtracting a five-year moving average of inflation from nominal interest rates on long-term government securities. Debt-to-GDP ratios and real interest rates are measured in percentage points. The covariates in columns (1), (2), and (4) include one lag each of the inflation rate, the real GDP growth rate, and the crisis dummy. The specification in column (2) includes data from the United States and is not winsorized. The specification in column (4) uses data that has been winsorized at the 1st and 99th percentiles for the changes in debt-to-GDP ratios and real interest rates.

4.2 Graphical Evidence of Public Debt's Effects on Real Interest Rates

Figure 2 illustrates the estimation procedure in column 3 of Table 1, which has the clearest graphical representation of our specifications. Figure 2a plots the 5-year change in the debt-to-GDP ratio against the share of seats won by the largest party in a parliamentary election as blue dots, each corresponding to a one-percentage point wide bin.[21] The seat share is normalized so that zero corresponds to a bare majority as described in Section 3.2. The solid black lines show the local linear polynomial $f(\cdot)$, which is estimated separately on each side of the cutoff for a majority.[22] The solid black lines' vertical distance at the dashed line corresponding to the cutoff indicates the estimated first-stage discontinuity of 15.41 percentage points. Figure 2b plots the corresponding figure for the change in real long-term interest rates. The vertical distance between the two solid black lines at the cutoff represents the reduced-form discontinuity of 1.22 percentage points. The RD plots in figures 2a and 2b illustrate the essential logic of our empirical design, but visually there is substantial variation in the data surrounding the estimated lines of best fit, consistent with the marginally statistically significant p-value of 0.056 from the corresponding regression. Controlling for lagged covariates, as we do in our baseline specification in column 1 of table 1, reduces that statistical variation substantially.

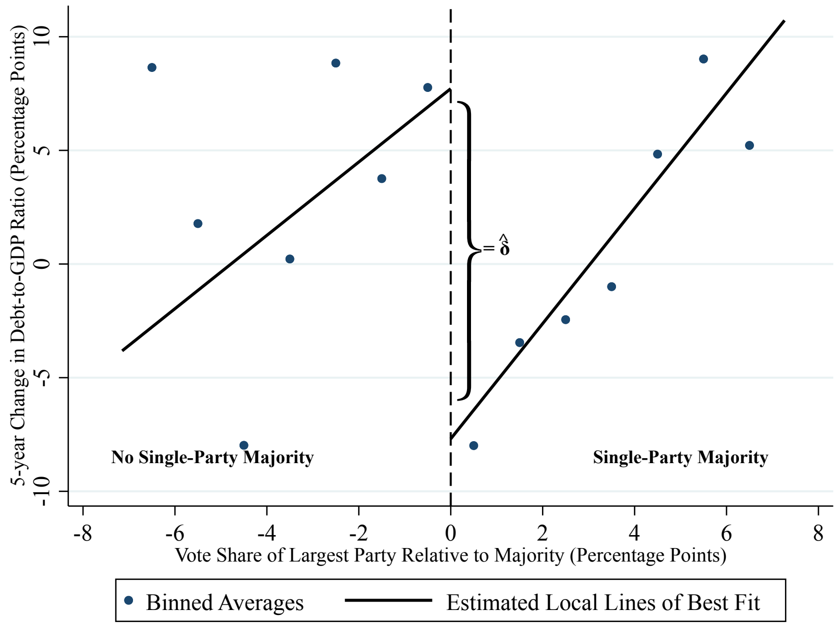
The slope of the estimated local polynomial $g(\cdot)$ in Figure 2a is potentially puzzling from the standpoint of the explanation given in Roubini and Sachs (1989b) and the related literature for why single-party majority governments should exhibit more more fiscal discipline than coalition and minority governments. That explanation suggests that coalition governments use increased spending or decreased taxation to maintain discipline among the coalition partners. One might expect that argument to extend away from the cutoff for a single-party majority, so that coalitions in which no party is close to commanding a single-party majority should exhibit less fiscal discipline than coalitions in which one party is close to a majority, and single-party governments with a large majority should exhibit more discipline than single-party governments with a small majority. In that case, the estimated local polynomials in Figure 2a ought to slope downward, whereas in actuality

[21]The bins contain an average of ten observations each.

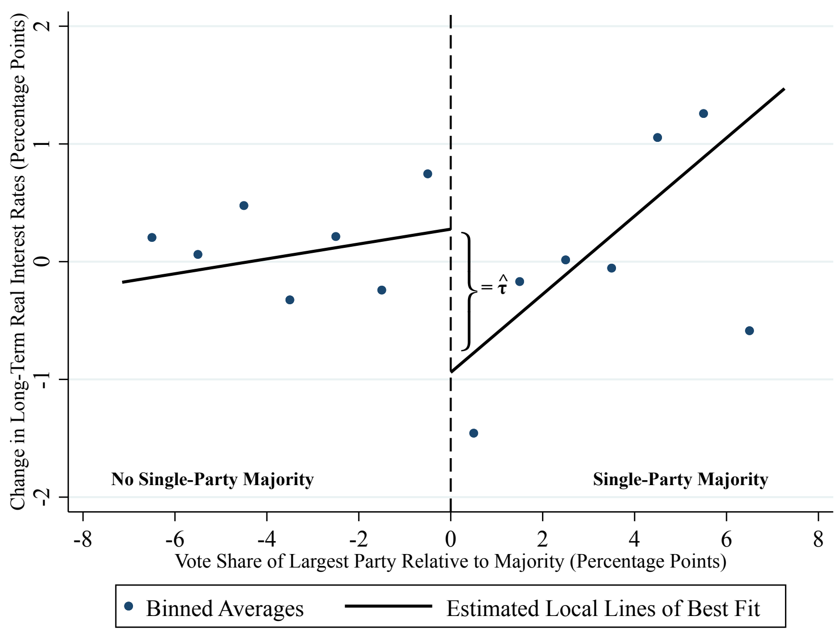
[22]Figure 2 displays only bins with centroids inside the bandwidth used for estimation, as only data inside the bandwidth is used to estimate the discontinuities and standard errors.

Figure 2: Single-Party Majorities, Debt-to-GDP Ratios, and Real Long-Term Interest Rates

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



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



The horizontal axis represents the vote share of the largest party relative to a bare majority. The vertical axis in panel (a) represents the change in the government's debt-to-GDP ratio from the year of the election to five years after the election, while the vertical axis in panel (b) represents the change in the long-term real interest rate from the year of the election to one year after the election. The blue dots represent averages for one percentage point wide bins with centroids within the estimation bandwidth; each bin contains an average of ten observations. The black lines represent local polynomial estimates based on the specification in column (3) of Table 1, which does not include covariates.

they slope upward, especially to the right of the cutoff as the size of the single-party majority increases beyond the bare minimum.

The precise relationship between public debt levels and the strength of single-party majorities is not directly relevant to our empirical design, but at the risk of straying outside of our paper's main focus, we do note that the pattern observed in Figure 2a is arguably consistent with the model proposed by Tabellini and Alesina (1990). As they summarize their model:

> Suppose that there is uncertainty about the future composition of public spending, because the identity of future majorities is still unknown. Then, whereas the majority who runs a budget deficit also chooses how to allocate the debt proceeds, the allocation of the burden of repaying the debt is not under its control. Under appropriate conditions this asymmetry prevents the current majority from fully internalizing the costs of budget deficits, the more so the greater is the difference between its preferences and the expected preferences of the future majority.

Their model assumes that the preferences of the median voter change over time in a stochastic way due to variations in voter turnout, the composition of the voter pool, etc. The preferences of the median voter in a "blow-out" election that produces a large single-party majority may differ substantially from the expected preferences of future median voters. In that case, it may not be surprising that large majorities accumulate substantial debt: future elections may feature voters with a very different set of preferences, and accumulating debt allows the current majority's to impose its preferences partially on those voters. In contrast, very close elections may feature median voters with preferences closer to those expected of future median voters, reducing the incentive to accumulate debt. Of course, the focus on the median voter in Tabellini and Alesina (1990) takes us away from the considerations of institutional arrangements and government structure that provide our identification strategy.

Despite the preceding argument, to ameliorate potential concerns about the slope of the estimated polynomial $g(\cdot)$, Appendix B provides evidence from a local randomization inference design that the estimated slopes do not drive our main results. The local randomization inference strategy examines a very narrow window around the cutoff, in which changes in debt-to-GDP ratios and real long-term interest rates are assumed to depend on whether or not an election produced a single-party majority, but not on the distance of the largest party's seat share from the cutoff. The point estimates of a one percentage point increase in the debt-to-GDP ratio's effect on long-term real interest rates presented in Appendix B are uniformly larger than our baseline RD results, and four out of five estimates are highly statistically significant. We therefore conclude that our main results are not driven by the estimated slopes of the local polynomials $f(\cdot)$ and $g(\cdot)$ away from the cutoff for a single-party majority.

4.3 Discussion

A common benchmark model to think about the effects of government debt on real interest rates is an aggregate production function approach (Ball et al., 1995; Elmendorf and Mankiw, 1999; Engen and Hubbard, 2004; Laubach, 2009; Gamber and Seliski, 2019). In this theoretical or model-based approach, the economy is assumed to produce output according to a Cobb-Douglas aggregate production function, and one dollar of government debt is assumed to displace, or "crowd out," some fraction of a dollar c of productive capital formation. The higher marginal product of capital that results from this crowding out effect in turn drives up real interest rates. If factors are paid their marginal products and public debt is risk-free, it is straightforward to show that the steady-state effect of an increase in the debt-to-GDP ratio on real interest rates is given by:

$$\frac{\partial r}{\partial D/Y} = \frac{\alpha(1-\alpha)c}{k^2} \tag{6}$$

where r is the real interest rate, D/Y is the debt-to-GDP ratio, α is capital's share in the aggregate production function, c is the fraction of capital formation crowded out by issuance of public debt, and k is the economy's steady-state capital-to-output ratio.

Gamber and Seliski (2019) summarize a number of studies, listed above, that have employed this approach. Using standard parameterizations of the U.S. economy, assuming government debt crowds out capital dollar-for-dollar, this approach implies that a one percentage point increase in the debt-to-GDP ratio will increase real rates by approximately 2 to 3.5 basis points. Assuming less than full crowding out because of capital inflows or increased private saving to offset government borrowing will reduce that estimated effect, to approximately 1.4 to 2.1 basis points. Therefore, it would be difficult for the traditional crowding out channel on its own to drive the effect we estimate, that a one percentage point increase in the debt-to-GDP ratio leads to a 5.6 basis point increase in real rates.

The benchmark crowding out model's inability to produce the effect that we estimate suggests to us that part of the effect is likely to be driven by a risk premium channel. Although Alesina et al. (1992) estimate that default risk on government debt is "very small," Ardagna et al. (2007) note there are a number of potential risks related to government debt, including not only default but also inflation and currency depreciation. These risks may be more pertinent in our international context than they are in the literature that examines the effects of debt on real rates in the United States. In our view, the important nonlinearities in the effects of debt on real rates estimated by Ardagna et al. (2007) and Greenlaw et al. (2013) suggest that the risk premium is an empirically important channel by which government debt levels affect real interest rates, consistent with the effects we estimate being larger than the traditional channel of crowding out can produce. Tedeschi (2019) argues that the risk channel may be empirically relevant in the United States as well, even though default risk is likely to be absent. Likewise, Kitchen (1996) argues that higher U.S. deficits appear to raise real interest rates in part via an inflation risk premium.

How applicable, then, are our estimates likely to be to modern economies such as the United States, Germany, or Japan today? The literature typically advances the view that those countries' bonds are perceived as "safe haven" assets (Laubach, 2009; Bernoth and Erdogan, 2012; Flavin et al., 2014; Kopyl and Lee, 2016). In that case, our estimated effect may overstate the likely current effects of public debt levels on real rates in those economies. That said, we do believe that the problem of discretionary fiscal endogeneity that we discussed in the introduction is likely to be just as operative in today's modern economies as in other times and places. If so, the estimated effects of public debt levels on real interest rates from empirical approaches that do not account for that source of endogeneity are likely to be too small.

4.4 Results from Alternative Samples

The small size of our baseline sample restricts our ability to properly analyze the stability of our baseline estimates across various subsamples. We do, however, apply our baseline estimation specification across several subsamples and an alternative dataset to analyze the sensitivity of our baseline results to different samples. We discuss these results of this exercise in detail in Appendix A.2. Here, we briefly summarize the alternative samples we present in Appendix A.2, which are as follows:

- Our baseline sample excluding the Great Recession period;
- Our baseline sample excluding observations in which the election was followed by new elections within one year;
- Our baseline sample dropping the five most influential observations, as described in more detail in Appendix A.2; and
- An alternative data set, the macrohistory database compiled by Jordà et al. (2017), which features different economic data sources and a different set of countries and years than our baseline sample.

Our results are not very sensitive to alternate samples, and our baseline estimate of the effect of an increase in the debt-to-GDP ratio on real long-term interest rates is conservative relative to most of the alternative samples that we consider.

5 Examining the Exclusion Restriction and Confounding

In this section, we further examine the interpretation of our estimates as the causal effect of changes in the debt-to-GDP ratio on long-term real interest rates. We first examine the exclusion restriction, which in our context requires that whether or not the largest party in an election barely achieves or fails to achieve a single-party majority does not affect real long-term interest rates except through its effect on the debt-to-GDP ratio. We examine a set of potential factors that could violate the exclusion restriction in section 5.1. In section 5.2, we then examine whether our baseline estimates could be confounded by other factors that have been argued in the literature to affect the relationship between government debt levels and real interest rates.

5.1 Exclusion Restriction

Unfortunately, the exclusion restriction is a fundamentally untestable assumption. Here, we examine some possible violations of the exclusion restriction and provide evidence that these potential violations do not drive our result. We interpret this evidence as suggesting that the exclusion restriction is a plausible approximation of reality in our context.

Before presenting the evidence, it is worth clarifying that the presence of additional factors further along the causal pathway from debt-to-GDP ratios to real interest rates does not itself violate the exclusion restriction. For instance, suppose that the absence of a single-party majority causes an increase in debt, which in turn precipitates an economic crisis. In that case, the economic crisis would be a mediator of the effects of debt, rather than a violation of the exclusion restriction that might confound our causal estimates.

5.1.1 Single-Party Majorities and Economic/Political Conditions

In this section, we consider four potential factors that could violate our exclusion restriction: economic crises; macroeconomic performance more generally; monetary policy reactions; and the ideology of the elected governments. Our basic approach to assessing these potential violations of the exclusion restriction in this section is to include a measure of each potential confounder as an additional control variable in our RD design. Because controlling for these potential confounders does not change our baseline estimates appreciably, we argue that these factors are unlikely to violate the exclusion restriction. In a supplementary analysis in appendix section A, we also document that these additional covariates do not exhibit discontinuities at the cutoff for a single-party majority. In section 5.1.2 below, we consider an additional potential factor that could violate the exclusion restriction, but for which insufficient data is available for us to follow our standard strategy of including the factor as an additional control variable.

Table 2 presents the results from our investigations of potential violations of the exclusion restriction. The first column of the table reproduces our baseline estimate to facilitate comparison; each subsequent column includes one additional control variable representing a factor that could in principle violate our exclusion restriction.

Columns 2 and 3 of Table 2 examine potential confounding from general economic conditions by including contemporaneous and one-year future indicators of economic crises and real GDP growth, respectively, as control variables in addition to our baseline covariates.[23] The estimates in both columns are nearly identical to our baseline estimates, suggesting that economic crises and economic growth more generally are unlikely to be important confounders of our baseline results.

In column 4, we include the levels of contemporaneous and short-term nominal interest rates to proxy for potential monetary responses to different election results. Our measure of short-term interest rates comes from Jordà et al. (2017), so the sample size is accordingly smaller than our baseline sample. Once again, the results are quite close to our baseline results, suggesting that differential monetary policy responses to differing election results do not confound our estimates.[24]

In column 5, we control for the ideology of the government formed after each election in our dataset using a measure of the "Ideological Complexion of Government and Parliament" coded by Seki and Williams (2014).[25] In principle, the ideologies of the governments formed after close elections could vary systematically depending on whether the election produced a single-party majority, and those varying ideologies might affect real interest rates independently of public debt levels. As we show in column 5, however, controlling for the ideological complexion of the government does not meaningfully change our baseline results.

We interpret the evidence in this section as suggesting that changes in general economic conditions or in broad government policy stances are unlikely to violate the exclusion restriction, or in other words, to confound the the effect we estimate of public debt levels on real interest rates.

[23]Column 2 uses the economic crisis designations of Reinhart and Rogoff (2009) as updated in Reinhart and Rogoff (2011), which are the same indicators we use as a lagged covariate in our baseline specification.

[24]If we include the contemporaneous and one-year ahead change in short-term nominal interest rates, the results are again similar.

[25]Seki and Williams (2014) is an update of the measure in Woldendorp et al. (2000). The measure takes values one through five, with a value of one corresponding to "right-wing dominance," two corresponding to "right-center complexion," three corresponding to "balanced situation," four corresponding to "left-center complexion," and five corresponding to "left-wing dominance."

Table 2: Regression Discontinuity Design Estimates With Different Covariates (Examining the Exclusion Restriction)

	(1) Baseline	(2) Current and Future Crisis	(3) Current and Future Real GDP growth	(4) Current and Future Short Rates	(5) Ideology
	First Stage: Effect of Single-Party Majority on Debt-to-GDP Ratios				
Debt-to-GDP ratio	-21.39	-20.88	-21.47	-30.92	-22.43
Clustered standard error	10.13	9.75	10.53	11.00	10.10
Bias-robust p-value	0.02	0.02	0.02	0.00	0.01
	Reduced Form: Effect of Single-Party Majority on Real Interest Rates				
Real long-rate	-1.19	-1.11	-1.16	-1.69	-1.20
Clustered standard error	0.57	0.60	0.41	0.56	0.56
Bias-robust p-value	0.02	0.03	0.00	0.00	0.02
	Second Stage: Effect of Debt-to-GDP Ratios on Real Interest Rates				
Real long-rate	0.056	0.053	0.054	0.055	0.054
Clustered standard error	0.023	0.024	0.021	0.016	0.022
Bias-robust p-value	0.014	0.023	0.012	0.001	0.012
Bandwidth	6.36	6.62	6.43	6.43	5.90
Number of Observations	308	307	308	232	308
Number of countries	25	25	25	16	25

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 column (1) of Table 1, but with the additional covariates referenced in the column headers. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Columns (2) through (6) include the baseline covariates as well as the variables in the column headers. Columns (1), (2), and (3) use data from the baseline dataset in Table 1. Column (4) supplements the baseline specification data with data on current and future short-term real interest rates from the JST dataset described in Section 3.2. Column (5) supplements the baseline specification data with data on the the ideology of the government from Seki and Williams (2014).

5.1.2 Single-Party Majorities and Economic Policy Uncertainty

Another possible violation of the exclusion restriction is the potentially confounding effect of policy uncertainty on the causal link between debt-to-GDP ratios and real interest rates. As an example, suppose that the election of a single-party majority government systematically raises uncertainty about the future path of economic policy, and that higher uncertainty leads to a decline in real interest rates. In such a scenario, we would be unable to determine whether the relative change in real interest rates following a close election stems from the relative change in debt levels or from the relative change in uncertainty.[26] Unfortunately, the limited span of data in the most commonly used measure of Economic Policy Uncertainty, constructed by Baker et al. (2016), prevents us from taking the approach in section 5.1.1 to assess this possibility by adding a measure of policy uncertainty to our baseline specification.[27]

We therefore take an alternative approach to examine this possibility. We first document the effects of close election results on economic policy uncertainty by estimating a sharp regression discontinuity design, using the annual change in the Economic Policy Uncertainty (EPU) index of Baker et al. (2016) as the dependent variable and the normalized seat share of the largest party in an election as the running variable. The EPU is available for 14 of the countries in our baseline data set; its coverage begins at different points in time for different countries, with the index for Sweden beginning the earliest in 1976.

In principle, a close election that produces a single-party majority could lead to a relative increase or a relative decrease in policy uncertainty relative to an election that does not produce a majority. The shorter duration of coalition and minority governments and the likelihood of new elections could both generate higher uncertainty. On the other hand, single party majorities may have more ability to enact their policy agendas, which may increase the scope for substantial changes in policy and increase uncertainty relative to the status quo. Alesina and Drazen (1991) and Velasco (1999) both present models emphasizing the greater difficulty of achieving decisive policy reform with fragmented decision making, as often seen in coalition governments.

[26]Again, we note that if the relative change in the debt-to-GDP ratio itself causes the change in uncertainty, there is no violation of the exclusion restriction.

[27]The EPU quantifies policy-related economic uncertainty, largely based on newspaper coverage of policy-related news.

We estimate that elections that produce a single-party majority lead to a substantial increase in economic policy uncertainty relative to elections that do not produce such a majority. Table 3 shows our estimate that a single-party majority raises the EPU by 76.3 index points in the year following the election relative to the absence of a single-party majority. That effect is nearly three standard deviations of the one-year change in the EPU and is highly statistically significant.[28]

Table 3: Regression Discontinuity Estimates: Economic Policy Uncertainty and Single-Party Majority

	One-year change in EPU
RD Estimation	
Coefficient	76.29
Clustered standard error	
Bias-robust p-value	0.01
Bandwidth	2.60
Number of observations	90
Number of countries	14
Summary Statistics	
Mean	0.68
Standard deviation	26.25

The EPU index is taken from Baker et al. (2016). The election data set matches the data used in the baseline regression specification in column (1) of Table 1. We show results from a sharp regression discontinuity regression of the one-year change in EPU on the vote share of the largest party minus the threshold to reach a majority as described in Section 3.3.

To assess whether single-party majorities' effects on economic policy uncertainty could confound our estimate the effects of public debt levels on real interest rates, in the second step of our analysis we estimate a panel Vector Autoregression (VAR) to evaluate the effects of a shock to economic policy uncertainty on macroeconomic outcomes. We include three variables in the VAR: the log of the EPU index, the real long-term interest rate, and the growth rate of real GDP.[29] We

[28]We include all available data for the countries in our baseline estimation for this exercise, extending through 2014, when the Seki and Williams (2014) election data ends. Including data only through the end of our baseline elections sample in 2005 leads to insufficient observations for the estimation of clustered standard errors in the RD design. Even when using the longer data set, we are limited to only 90 observations.

[29]The growth rate of real GDP corresponds exactly to the covariate used in our baseline estimation in Table 1. We also use the same series for nominal interest rates to construct real long-term interest rates. Because we use the levels of long-term real interest rates in the panel VAR, rather than their first differences as in the RD design, though, we use only the inflation rates reported by Reinhart and Rogoff (2011) to construct real rates. We make that choice for the same reason that we use only the inflation rates from Reinhart and Rogoff (2011) to construct the covariate in our baseline

estimate the panel VAR at an annual frequency from 1976 to 2004 for the 13 countries in our baseline data set for which we also have data for the EPU. The VAR includes one lag of each variable in the system.

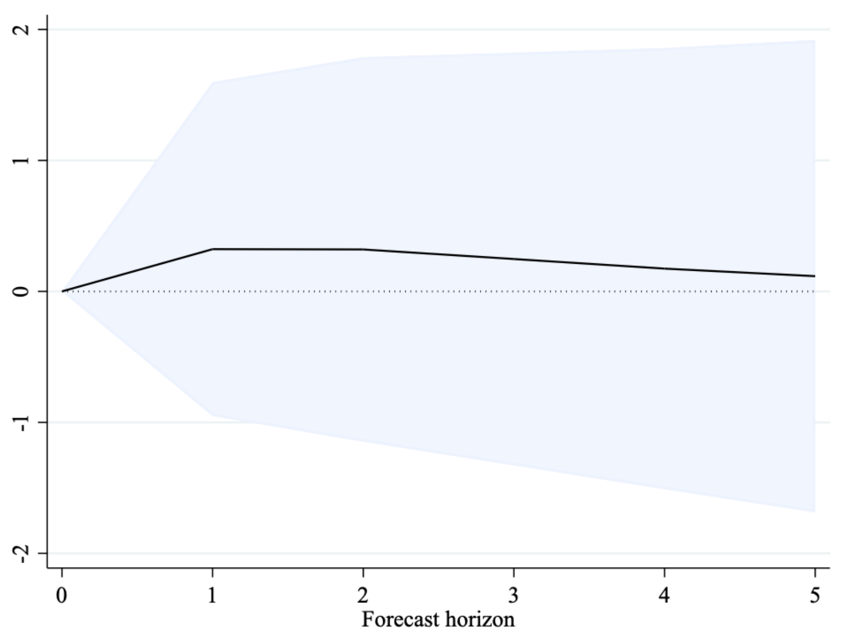
Figure 3 illustrates the estimated response from our panel VAR of real long-term interest rates to a one percentage point increase in economic policy uncertainty. We present results from the reduced form VAR, without any structural assumptions regarding the ordering of the variables. The shock we consider is therefore more accurately considered a forecast error for policy uncertainty rather than a structural shock to the economy.[30] The point estimate of the impulse response function of long-term real interest rates rises from its normalized value of zero to 32 basis points in the year following the shock, and it then declines slowly toward zero, remaining positive five years after the shock. That said, in no year is the impulse response close to significantly different from zero at the 10% confidence level. Our estimates of public debt's effect on real interest rates measure the change in rates from the calendar year of an election to the following year, so the portion of the impulse response function in Figure 3 that speaks to potential confounding from policy uncertainty is the change in rates from year zero to year one, in which the point estimate for the impact on real rates rises.

We interpret the two sets of results in this section as indicating that, to the extent that changes in economic policy uncertainty confound our estimates, they bias the estimated effect of public debt on real interest rates *downward* (i.e., toward zero). We document that an election that produces a single-party majority leads to a relative increase in economic policy uncertainty. We also present (much noisier) estimates that a shock to economic policy uncertainty increases real interest rates. The potential uncertainty channel therefore implies that a single-party majority should lead to *higher* real interest rates. On the contrary, our reduced form baseline estimates in Table 1 imply that a single-party majority causes real long-term interest rates to *decline* by 119 basis points in the year following the election. Although the VAR results relating changes in policy uncertainty to real

specification: doing so allows us to avoid the series breaks produced by splicing together the level of inflation across different data sources. In the RD design, when we are able to calculate changes in inflation only from consistent series, and then splice the changes together from the alternative data sources, we are able to avoid that problem.

[30]We focus on the reduced form because we do not believe that the timing restrictions implicit in a structural VAR using a Cholesky decomposition are justified at an annual frequency. However, if we do perform such a decomposition, regardless of whether the policy uncertainty index is ordered first or last, the point estimate of the impulse response function for real interest rates increases from the year of the uncertainty shock to one year later.

Figure 3: Impulse Response Function: Effect of Economic Policy Uncertainty on Real Long-Term Interest Rates



This figure presents the impulse response of real long-term rates to a shock to economic policy uncertainty. The panel VAR is estimated using the level of the real long-term interest rate using the baseline data set, the real GDP growth rate, and the log of the Economic Policy Uncertainty index of Baker et al. (2016). The VAR is estimated for 13 countries over the $1976 - 2004$ period. The EPU data set begins in 1984 while the baseline data ends in 2010.

interest rates are too noisy to draw firm conclusions, we therefore believe this potential channel for confounding is unlikely to drive our results.

5.2 Confounding

An important question regarding our baseline analysis is whether the relationship we estimate between public debt levels and real interest rates is confounded by other economic factors. For instance, the evidence of nonlinearity in the relationship between debt levels and real rates in Ardagna et al. (2007) and Greenlaw et al. (2013) points to the role of fiscal space; Warnock and Warnock (2009) and Tedeschi (2019) emphasize the role of international capital flows in setting real interest rates, pointing to the role of international capital movement and exchange rate regimes. Although in principle our RD design should deliver an "as good as locally random experiment," it is nonetheless possible that these other factors are confounding our results by random chance. We also view an analysis of whether our baseline results are sensitive to controlling for these factors as informative about the likely stability of our results across different sets of macroeconomic conditions and policy arrangements.

In this section, we explore some important potential confounders either by augmenting our baseline estimation specification with additional control variables or by restricting our sample to subsets of observations. A major limitation of our methodology in this regard is that our small baseline sample size means that our ability to conduct subsample analysis is limited. Table 4 presents the results of our explorations, with the first column once again reproducing our baseline specification for comparison.

Column 2 of table 4 includes a country's lagged debt-to-GDP ratio as an additional control variable to investigate whether the initial level of public debt affects our baseline results. The second stage point estimate rises very slightly relative to the baseline, but the results are largely similar. We interpret this result as suggesting that countries' different levels of pre-election fiscal space are unlikely to confound our baseline results.

Columns 3 and 4 include control variables for countries' lagged capital control and exchange rate regimes, respectively. In a similar spirit, column 5 drops country-year observations for which the country was a member of the Euro Area, which features a fixed exchange rate and free capital movement within the area. Column 3 includes the international capital controls dummy of Ilzetzki et al. (2019), which equals one if there is more than one exchange rate in the economy, indicating the presence of some controls on the movement of capital. Column 4 includes the coarse exchange rate regime classification of Ilzetzki et al. (2019).[31] The estimated effect of public debt levels on real rates in column 3 is larger than the baseline estimate, but still within one standard deviation, while the estimated effect in column 4 is very slightly lower than our baseline estimate. Likewise, the estimated effect in column 5 is very close to our baseline estimate.

We interpret the insensitivity of our estimated results to controlling for exchange rate regimes or international capital controls and to excluding Euro Area members as suggesting that our estimated

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

effects are relatively constant across different exchange rate and international capital movement regimes. This result is potentially surprising given the importance of international capital flows to mitigate the effects of the traditional "crowding out" channel by which public borrowing may raise real interest rates (Gamber and Seliski, 2019; Gale and Orszag, 2004; Warnock and Warnock, 2009). As we noted in section 4.3, however, the effect size that we estimate is larger than the conventional crowding out channel can explain. We therefore view these results as consistent with the notion that a risk channel may play an important role in driving our estimated effects.

Finally, in column 6, we restrict our sample to the subset of countries that are currently members of the Organization for Economic Cooperation and Development (OECD).[32] The estimated effect of public debt on real interest rates is 5.1 basis points per percentage increase in the debt-to-GDP ratio, slightly smaller than, but again within a standard deviation of, our baseline estimate. We interpret this result as suggesting that our baseline estimate is not driven by countries with less developed economies.

Overall, the results in this section indicate that our baseline estimate of public debt's effect on real interest rates does not appear to be confounded by by measures of fiscal space, exchange rate flexibility, or capital controls. We caution, however, that our methodology is not ideally suited for detecting such potential confounding.

[32] The only non-OCED countries in our baseline sample are India and South Africa.

Table 4: Regression Discontinuity Design Estimates With Different Covariates (Examining Potential Confounders)

	(1) Baseline	(2) One-Year Lagged Debt-to-GDP Ratio	(3) One-year Lagged Capital Controls	(4) One-Year Lagged Exchange Rate Regime	(5) Drop Euroarea Country-Years	(6) Only OECD Countries
First Stage: Effect of Single-Party Majority on Debt-to-GDP Ratios						
Debt-to-GDP ratio	-21.39	-19.05	-16.28	-22.86	-17.18	-21.00
Clustered standard error	10.13	8.06	7.80	9.61	9.46	10.15
Bias-robust p-value	0.02	0.01	0.02	0.00	0.04	0.02
Reduced Form: Effect of Single-Party Majority on Real Interest Rates						
Real long-rate	-1.19	-1.18	-1.12	-1.23	-0.94	-1.08
Clustered standard error	0.57	0.54	0.56	0.60	0.55	0.58
Bias-robust p-value	0.02	0.02	0.02	0.01	0.04	0.03
Second Stage: Effect of Debt-to-GDP Ratios on Real Interest Rates						
Real long-rate	0.056	0.062	0.069	0.054	0.055	0.051
Clustered standard error	0.023	0.027	0.030	0.022	0.027	0.022
Bias-robust p-value	0.014	0.019	0.022	0.012	0.035	0.020
Bandwidth	6.36	6.31	6.88	5.72	7.77	6.58
Number of Observations	308	308	303	303	289	289
Number of countries	25	25	24	25	24	23

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. Columns (2) through (4) include the baseline covariates as well as the variables in the column headers. Columns (5) and (6) restrict the sample as specified in the column headers. Columns (1), (2), (5), and (6) use data from the baseline specification in Table 1. Columns (3) and (4) supplement the baseline specification data with data on exchange rate arrangements and capital controls from Ilzetzki et al. (2019).

6 Conclusion

The extent to which higher public debt levels increase real interest rates is a longstanding academic question with significant contemporary policy importance. For instance, Gamber and Seliski (2019) note that "The relationship between debt and interest rates plays a key role in the Congressional Budget Office's economic and budget projections (especially long-term projections) and for dynamic analyses of fiscal policy, where the sensitivity of interest rates with respect to changes in the level of debt is vitally important." The question remains a topic of active research partly because of the many challenges to estimating the relationship accurately.

We use close parliamentary elections as natural experiments to estimate the effects of public debt levels on real long-term interest rates with a fuzzy regression discontinuity design. We first estimate that the absence of a single-party majority following an election causes the debt-to-GDP ratio to increase by roughly 21 percentage points over the following five years. Our results thus provide strong causal support for a previous literature arguing that single-party governments in parliamentary systems produce more fiscal discipline than coalition and minority governments.

Our primary result is our estimate that a one percentage point increase in the debt-to-GDP ratio causes long-term real interest rates to increase by 5.6 basis points, at the top end of the range of estimates in the previous literature. We believe that the relatively large effect that we estimate suggests that discretionary fiscal endogeneity, or the simultaneous determination of government borrowing and real interest rates, is an empirically important factor to consider when estimating debt levels' effects on rates.

A limitation of our approach is that history provides relatively few observations with which to estimate our regression discontinuity design. We therefore lack the statistical power to study potential non-linearities in the relationship between debt levels and interest rates, or the mechanisms that drive our baseline results, with the precision that we would like. Those questions remain important topics for additional research.

References

Alesina, Alberto and Allan Drazen, "Why are Stabilizations Delayed?," *The American Economic Review*, 1991, *81* (5), 1170–1188.

_ **and Roberto Perotti**, "Fiscal expansions and adjustments in OECD countries," *Economic policy*, 1995, *10* (21), 205–248.

_ **, Mark De Broeck, Alessandro Prati, and Guido Tabellini**, "Default risk on government debt in OECD countries," *Economic policy*, 1992, *7* (15), 427–463.

_ **, Roberto Perotti, and José Tavares**, "The political economy of fiscal adjustments," *Brookings Papers on Economic Activity*, 1998, *1998* (1), 197–266.

Ardagna, Silvia, Francesco Caselli, and Timothy Lane, "Fiscal discipline and the cost of public debt service: some estimates for OECD countries," *The BE Journal of Macroeconomics*, 2007, *7* (1).

Auerbach, Alan J and Yuriy Gorodnichenko, "Fiscal stimulus and fiscal sustainability," Technical Report, National Bureau of Economic Research 2017.

Baker, Scott R., Nicholas Bloom, and Steve Davis, "Measuring Economic Policy Uncertainty," *Quarterly Journal of Economics*, 2016, *131* (4).

Ball, Laurence, N Gregory Mankiw et al., "What do budget deficits do?," in "Proceedings-Economic Policy Symposium-Jackson Hole" Federal Reserve Bank of Kansas City 1995, pp. 95–119.

Barro, Robert J, "Are government bonds net wealth?," *Journal of political economy*, 1974, *82* (6), 1095–1117.

Bernheim, B Douglas, "Ricardian equivalence: An evaluation of theory and evidence," *NBER macroeconomics annual*, 1987, *2*, 263–304.

Bernoth, Kerstin and Burcu Erdogan, "Sovereign bond yield spreads: A time-varying coefficient approach," *Journal of International Money and Finance*, 2012, *31* (3), 639–656.

Blanchard, Olivier, "Public debt and low interest rates," *American Economic Review*, 2019, *109* (4), 1197–1229.

_ **and Roberto Perotti**, "An empirical characterization of the dynamic effects of changes in government spending and taxes on output," *the Quarterly Journal of economics*, 2002, *117* (4), 1329–1368.

Bolt, Jutta, Robert Inklaar, Herman de Jong, and Jan Luiten van Zanden, "Maddison Project Database, version 2018. "Rebasing 'Maddison': new income comparisons and the shape of long-run economic development"," 2018.

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik, "Robust nonparametric confidence intervals for regression-discontinuity designs," *Econometrica*, 2014, *82* (6), 2295–2326.

_ , _ , Max H Farrell, and Rocio Titiunik, "Regression discontinuity designs using covariates," *Review of Economics and Statistics*, 2019, *101* (3), 442–451.

Canzoneri, Matthew B, Robert E Cumby, Behzad Diba et al., "Should the European Central Bank and the Federal Reserve be concerned about fiscal policy?," *Rethinking stabilization policy*, 2002, pp. 29–31.

Cattaneo, Matias D, Brigham R Frandsen, and Rocio Titiunik, "Randomization inference in the regression discontinuity design: An application to party advantages in the US Senate," *Journal of Causal Inference*, 2015, *3* (1), 1–24.

_ , Michael Jansson, and Xinwei Ma, "Manipulation testing based on density discontinuity," *The Stata Journal*, 2018, *18* (1), 234–261.

_ , _ , and _ , "Simple local polynomial density estimators," *Journal of the American Statistical Association*, 2019, (just-accepted), 1–11.

Chase, Jefferson, "Bavaria's Christian Social Union: What you need to know," https://www.dw.com/en/bavarias-christian-social-union-what-you-need-to-know/a-39192183 09 2018. Accessed March 3, 2020.

Chwieroth, Jeffrey M and Andrew Walter, "Financial crises and political turnover: a long run panoramic view," in "annual meeting of the International Political Economy Society, Harvard University, November" 2010, pp. 12–13.

Cohen, Darrel and Olivier Garnier, "The impact of forecasts of budget deficits on interest rates in the United States and other G-7 countries," *Federal Reserve Board*, 1991.

Council of Economic Advisers, "Long-term Interest Rates: A Survey," https://obamawhitehouse.

archives.gov/sites/default/files/docs/interest_rate_report_final.pdf 2015.

Döring, Holger and Philip Manow, "Parliaments and governments database (ParlGov): Information on parties, elections and cabinets in modern democracies," http://www.parlgov.org/ 2019.

Edin, Per-Anders and Henry Ohlsson, "Political determinants of budget deficits: Coalition effects versus minority effects," *European Economic Review*, 1991, *35* (8), 1597–1603.

Elmendorf, Douglas W, "Actual budget deficit expectations and interest rates," Technical Report, Harvard-Institute of Economic Research 1993.

_ , "The effects of deficit-reduction laws on real interest rates," 1996.

_ **and Louise M Sheiner**, "Federal budget policy with an aging population and persistently low interest rates," *Journal of Economic Perspectives*, 2017, *31* (3), 175–94.

_ **and N Gregory Mankiw**, "Government debt," *Handbook of macroeconomics*, 1999, *1*, 1615–1669.

Engen, Eric M and R Glenn Hubbard, "Federal government debt and interest rates," *NBER macroeconomics annual*, 2004, *19*, 83–138.

Faini, Riccardo, "Fiscal policy and interest rates in Europe," *Economic Policy*, 2006, *21* (47), 444–489.

Fan, Jianqing and Irene Gijbels, *Local polynomial modelling and its applications*, Vol. 66, CRC Press, 1996.

Feldstein, Martin S, "Budget deficits, tax rules, and real interest rates," 1986.

Flavin, Thomas J, Ciara E Morley, and Ekaterini Panopoulou, "Identifying safe haven assets for equity investors through an analysis of the stability of shock transmission," *Journal of International Financial Markets, Institutions and Money*, 2014, *33*, 137–154.

Gale, William G and Peter R Orszag, "Budget deficits, national saving, and interest rates," *Brookings Papers on Economic Activity*, 2004, *2004* (2), 101–210.

Gamber, Edward and John Seliski, "The Effect of Government Debt on Interest Rates," *CBO Working Paper Series*, 2019, (2019-01).

Garín, Julio, Robert Lester, Eric Sims, and Jonathan Wolff, "Without looking closer, it may seem cheap: Low interest rates and government borrowing," *Economics Letters*, 2019, *180*, 28 – 32.

Greenlaw, David, James D Hamilton, Peter Hooper, and Frederic S Mishkin, "Crunch time: Fiscal crises and the role of monetary policy," Technical Report, National Bureau of Economic Research 2013.

Grilli, Vittorio, Donato Masciandaro, and Guido Tabellini, "Political and monetary institutions and public financial policies in the industrial countries," *Economic policy*, 1991, *6* (13), 341–392.

Haan, Jakob De and Jan-Egbert Sturm, "Political and institutional determinants of fiscal policy in the European Community," *Public choice*, 1994, *80* (1-2), 157–172.

Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw, "Identification and estimation of treatment effects with a regression-discontinuity design," *Econometrica*, 2001, *69* (1), 201–209.

Hall, Matthew and Aditi Thapar, "The Economic Effects of Government Spending: The Importance of Controlling for Anticipated Information," *Working Paper*, 2019.

Homer, Sidney and Richard Sylla, *A history of interest rates*, Wiley Finance, 2005.

Ilzetzki, Ethan, Carmen M. Reinhart, and Kenneth S. Rogoff, "Exchange arrangements entering the twenty-first century: which anchor will hold," *Quarterly Journal of Economics*, 2019, *134* (2), 599–646.

International Monetary Fund, "International Financial Statistics," https://data.imf.org/?sk=4C514D48-B6BA-49ED-8AB9-52B0C1A0179B 2019. Accessed December 30, 2019.

Irwin, Neil, "How Jerome Powell's Unconventional Career Path Prepared Him for This Crisis," https://www.nytimes.com/2020/04/02/upshot/powell-career-path-crisis-coronavirus.html 2020.

Jordà, Òscar, Moritz Schularick, and Alan M Taylor, "Macrofinancial history and the new business cycle facts," *NBER Macroeconomics Annual*, 2017, *31* (1), 213–263.

Kinoshita, Noriaki, *Government debt and long-term interest rates* number 2006-2063, International Monetary Fund, 2006.

Kitchen, John, "Domestic and international financial market responses to Federal deficit announcements," *Journal of International Money and Finance*, 1996, *15* (2), 239–254.

Kopyl, Kateryna Anatoliyevna and John Byong-Tek Lee, "How safe are the safe haven assets?," *Financial Markets and Portfolio Management*, 2016, *30* (4), 453–482.

Krugman, Paul, "Time to Borrow," `https://www.nytimes.com/2016/08/08/opinion/time-to-borrow.html` 2016.

Laubach, Thomas, "New evidence on the interest rate effects of budget deficits and debt," *Journal of the European Economic Association*, 2009, *7* (4), 858–885.

Lee, David S, "Randomized experiments from non-random selection in US House elections," *Journal of Econometrics*, 2008, *142* (2), 675–697.

__ **and Thomas Lemieux**, "Regression discontinuity designs in economics," *Journal of economic literature*, 2010, *48* (2), 281–355.

McCrary, Justin, "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of econometrics*, 2008, *142* (2), 698–714.

OECD, "Long-term interest rates (indicator).," `https://doi.org/https://doi.org/10.1787/662d712c-en` 2019. Accessed December 30, 2019.

__ , "CPI: All items non-food non-energy," `https://doi.org/https://doi.org/10.1787/eee82e6e-en` 2020. Accessed January 4, 2020.

Perotti, Roberto and Yianos Kontopoulos, "Fragmented fiscal policy," *Journal of Public Economics*, 2002, *86* (2), 191–222.

Rachel, Lukasz and Lawrence Summers, "On Secular Stagnation in the Industrialized World," in "Brookings Papers on Economic Activity BPEA Conference Drafts, March 7," Vol. 8 Brookings Institution Press 2019.

Ramey, Valerie A and Sarah Zubairy, "Government spending multipliers in good times and in bad: evidence from US historical data," *Journal of Political Economy*, 2018, *126* (2), 850–901.

Reinhart, Carmen M. and Kenneth S. Rogoff, *This Time is Different: Eight Centuries of Financial Folly*, Princeton: Princeton University Press, 2009.

__ **and** __ , "From Financial Crash to Debt Crisis," *American Economic Review*, August 2011, *101* (5), 1676–1706.

Ricardo, David, "Essay on the funding system," *Encyclopaedia Britannica*, 1820.

Roubini, Nouriel and Jeffrey D Sachs, "Political and economic determinants of budget deficits in the industrial democracies," *European Economic Review*, 1989, *33* (5), 903–933.

— **and Jeffrey Sachs**, "Government spending and budget deficits in the industrial countries," *Economic policy*, 1989, *4* (8), 99–132.

Seki, Katsunori and Laron K Williams, "Updating the Party Government data set," *Electoral Studies*, 2014, *34*, 270–279.

Tabellini, Guido and Alberto Alesina, "Voting on the Budget Deficit," *The American Economic Review*, 1990, *80* (1), 37–49.

Tedeschi, Ernie, "Deficits are Raising Interest Rates. But Other Factors are Lowering Them.," `https://medium.com/bonothesauro/deficits-are-raising-interest-rates-but-other-factors-are-lowering-them-6d1e68776b7a` 2019. Accessed September 26, 2020.

Trump, Donald, "Interview with Stuart Varney (3:00-3:10)," `http://video.foxbusiness.com/v/5068153187001/?#sp=show-clips` 2016.

Velasco, Andres, "A model of endogenous fiscal deficits and delayed fiscal reforms," in "Fiscal institutions and fiscal performance," University of Chicago Press, 1999, pp. 37–58.

Walker, Peter, "Johnson Says he is prepared to increase public borrowing," `https://www.theguardian.com/politics/2019/jun/30/johnson-prepared-increase-public-borrowing` 2019.

Warnock, Francis E and Veronica Cacdac Warnock, "International capital flows and US interest rates," *Journal of International Money and Finance*, 2009, *28* (6), 903–919.

White, Michael, "House rules: Upper chambers around the world," *The Gaurdian*, Feb 2003.

Woldendorp, Jaap, Hans Keman, and Ian Budge, *Party government in 48 democracies. Composition–duration–personnel*, Dordrecht etc., Kluwer Academic Publishers, 2000.

Young, Alwyn, "Consistency without inference: Instrumental variables in practical application," 2019.

APPENDIX FOR ONLINE PUBLICATION

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

In this section of the Appendix, we present the results of additional validity, falsification, and robustness assessments of the regression discontinuity evidence we present in the body of the text on public debt's effects on real interest rates.

A.1 Validity and Falsification Tests of the RD

In Section 3.3.1, we presented two falsification tests of the local randomization assumption of the local RD design. We discuss three additional falsification and robustness tests here.

We first 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 5: Regression Discontinuity Design Falsification Test 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
Real long-rate	-0.02	0.02	0.17	0.06	-0.02	0.04	-0.25
Clustered standard error	0.13	0.21	0.67	0.02	0.04	0.08	0.44
Robust p-value	0.74	0.22	0.71	0.01	0.77	0.40	0.55
Bandwidth	3.54	5.08	3.17	6.36	1.57	2.98	2.50
No. obs on left	39.00	58.00	40.00	71.00	14.00	28.00	29.00
No. obs on right	46.00	34.00	18.00	54.00	15.00	19.00	12.00

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 column (1) of Table 1, but with an alternative cutoff for the vote 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 5 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 cutoff $c = 0$. 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 increase (decrease) 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.[33] These results suggest that our baseline estimates using the true cutoff value are unlikely to be driven by spurious random variation near the cutoff.

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. For each predetermined covariate Y, we estimate systems of equations of the form:

$$Y_{it} = \gamma + \delta M_{it} + f(V_{it} - \bar{c}_{it}) + \nu_{it} \tag{7}$$

$$M_{it} = 1[V_{it} \geq \bar{c}_{it}] \tag{8}$$

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

Table 6 presents results from the sharp RD estimation of equations (7) and (8). We test two lags each of five predetermined variables: real GDP growth, consumer price inflation, presence of an economic crisis, real long-term interest rates, and the debt-to-GDP ratio. This set comprises the three covariates we use in our baseline RD estimation as well as the two main elements of

[33] Cutoffs outside ± 3.5 once again run into convergence issues since there are insufficient observations to one side of the cutoff.

our estimation procedure.[34] In nine of ten cases, the test does not approach rejecting the null hypothesis of no discontinuity at the cutoff, consistent with the notion of local randomization. The test indicates a discontinuity in the one-year lagged economic crisis indicator variable at the 11 percent significance level. We believe this result is most likely attributable to random noise: we perform ten RD tests in Table 6, so one of them would be expected to indicate a statistically significant discontinuity at the 10 percent significance level even if the local randomization result holds. As noted by Lee and Lemieux (2010), "If there are many covariates ... some discontinuities will be statistically significant by random chance." Nonetheless, the near statistical significance of this test result is an additional reason that we include the lagged crisis indicator as a covariate in our baseline specification.

In figure 4, we present graphical evidence that the predetermined covariates and correlates of the outcomes we consider in table 6 are roughly continuous around the cutoff. The figure depicts two lags each of the five predetermined variables considered in table 6: the debt-to-GDP ratio, the real long-term interest rate, real GDP growth, consumer price inflation, and the economic crisis indicator. While there are some visual discontinuities in the predetermined covariates, table 6 and the test statistics displayed above each panel show that they are not statistically significant at standard confidence levels.

In Sections 5.1.1 and 5.2, we include various additional control variables in the RD design in order to examine the exclusion restriction and potential confounding. Table 7 displays the results of sharp RD cutoff tests analogous to the results in table 6 for the additional variables considered in those sections. No discontinuity is statistically distinguishable from zero at standard confidence levels in this set of additional covariates.

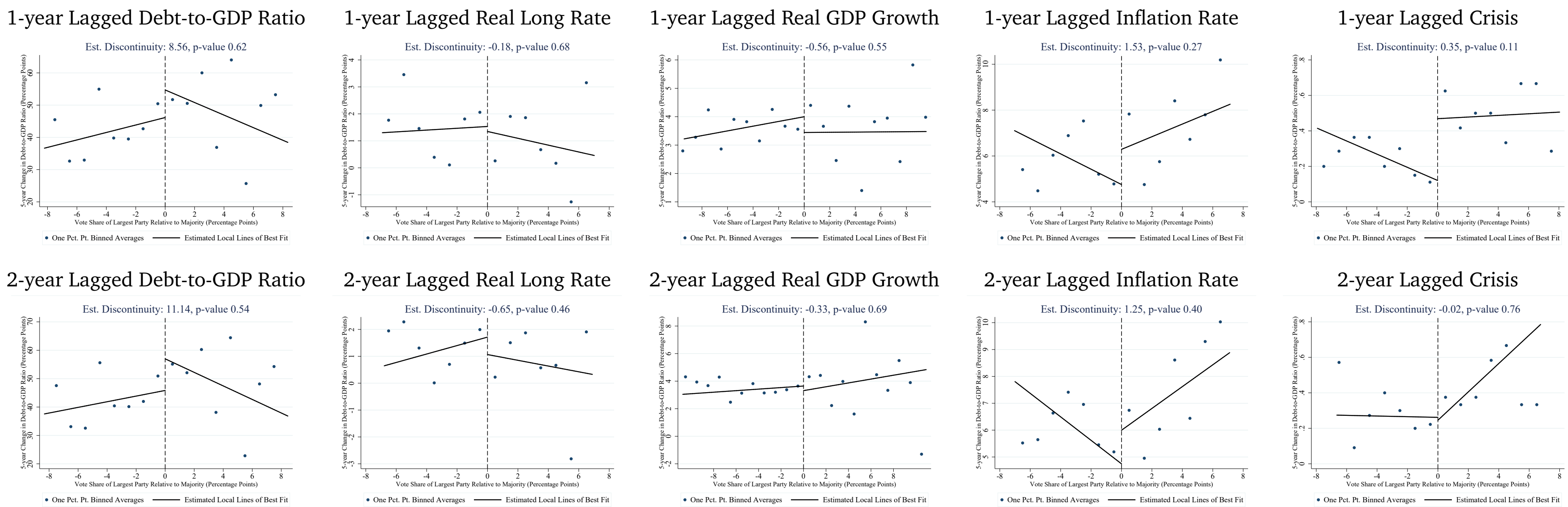
[34]We use the inflation series from Reinhart and Rogoff (2011) as our covariate because it has substantially more complete coverage than the core inflation measure from OECD (2020). We again implement the regression discontinuity tests using the `rdrobust` package of Calonico et al. (2014) using the same options as in our main estimation procedure, but using a sharp design rather than a fuzzy design.

Table 6: Regression Discontinuity Design Falsification Tests for Predetermined Covariates

	(1) Real GDP growth	(2) Inflation rate	(3) Economic Crisis	(4) Real long rate	(5) Debt-to-GDP
One-Year Lagged Predetermined Covariates					
Coefficient	-0.56	1.53	0.35	0.36	8.56
Clustered standard error	0.82	1.67	0.22	1.19	23.99
Bias-robust p-value	0.55	0.27	0.11	0.86	0.62
Bandwidth	9.55	7.14	8.05	6.66	8.34
Number of Observations	308	308	308	305	308
Number of countries	25	25	25	25	25
Two-Year Lagged Predetermined Covariates					
Coefficient	-0.33	1.25	-0.02	-0.41	11.14
Clustered standard error	1.14	1.68	0.23	1.34	24.30
Bias-robust p-value	0.69	0.40	0.76	0.66	0.54
Bandwidth	10.91	7.11	6.75	6.47	8.45
Number of Observations	308	308	308	301	306
Number of countries	25	25	25	25	25

The unit of observation is a country-election. Each column shows the results from a sharp regression discontinuity design 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 3.2. The dataset matches the data used in the baseline regression specification in column (1) of Table 1.

Figure 4: Predetermined Covariates



The horizontal axis of each panel represents the vote share of the largest party relative to a bare majority. The vertical axis in each panel represents the level of the variable indicated in the title one or two years prior to the election year. The blue dots represent averages for one percentage point wide bins with centroids within the estimation bandwidth. The black lines represent local polynomial estimates based on the specification in the corresponding column of Table 6.

Table 7: Regression Discontinuity Design Falsification Tests for Additional Covariates

	(1) Contemporaneous Crisis	(2) One-Year Ahead Crisis	(3) Lagged Exchange Rate	(4) Lagged Capital Control	(5) Lagged JST Short Rate	(6) Contemporaneous Ideology
Coefficient	0.25	0.03	0.20	0.19	1.16	0.11
Clustered standard error	0.17	0.18	0.66	0.19	2.38	0.50
Bias-robust p-value	0.12	0.99	0.65	0.27	0.49	0.77
Bandwidth	6.21	8.30	6.32	11.86	5.60	9.36
Number of Observations	308	307	303	303	233	308
Number of countries	25	25	24	24	16	25

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 3.2. The election dataset matches the data used in the baseline regression specification in column (1) of Table 1. The exchange rate and capital control data in columns (3) and (4) is from Ilzetzki et al. (2019). The short-term interest rates in column (5) are from the JST dataset described in Section 3.2. The ideology of the government data in column (6) is from Seki and Williams (2014).

A.2 Alternative Sample Stability

In this section, we elaborate on our brief discussion of results from alternative samples in Section 4.4 and demonstrate that our baseline results are not sensitive to examining several alternative samples and an alternative data set. The small size of our baseline sample, at 308 observations, limits the extent of subsample analysis we are able to conduct. We do, however, compare our baseline analysis to four alternative samples in table 8. The first column of the table reproduces our baseline estimates from column 1 of Table 1 for comparison. We use the same baseline specification for all four alternative samples.

Column 2 of Table 8 excludes the Great Recession period from the sample. Specifically, we consider only election years through 2002, so that the five-year change in the debt-to-GDP ratio ends in 2007. The estimated effect of an increase in the debt-to-GDP ratio rises very slightly relative to the baseline sample, but the statistical significance of the estimate is weaker, consistent with the smaller sample size.[35]

Column 3 drops election-year observations that were followed by new elections within one year, on the notion that a shorter interval between elections might indicate that the resulting government did not have a chance to pass a budget. The second-stage point estimate is larger than in the baseline sample, but the statistical precision of the estimate is substantially lower, perhaps reflecting the loss of over one-sixth of the baseline sample.

Column 4 examines our estimates' sensitivity to especially important observations. By their nature, RD designs examine variation in a narrow window around a given cutoff, so that a subset of observations is used to estimate the effect of interest. Further, as is standard, 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.[36] The triangular kernel assigns the highest weights to observations that are closest to the cutoff. In practice, therefore, it is possible for a small number of observations to have high leverage and an outsize influence on the results.[37] We identify

[35]Adding a dummy for the Great Recession period, instead of dropping the observations, raises the estimated effect of an increase in the debt-to-GDP ratio to 6.4 basis points. In this case, we do not lose any statistical significance relative to our baseline sample due to the unchanged sample size.

[36]Appendix Table 9 shows that our estimates are robust to using other kernels.

[37]Young (2019) argues that similar problems can occur in instrumental variables regressions, stating "In published papers, statistically significant IV results generally depend upon only one or two observations or clusters..."

the relative importance of the individual observations in our data set by running a series of 308 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 10 in Appendix A.4 displays the estimates for the five observations whose absence most changes the baseline estimates.[38] Dropping two of the five observations leads to a smaller second-stage estimate, while dropping the other three observations leads to a higher estimate. The largest absolute change in the second-stage point estimate is 1.5 basis points. In column 5 of table 8, we drop all five of the most important observations. The point estimate of the second-stage effect rises a bit from the baseline estimate, to a 6.5 basis points increase in real long-term interest rates per one percentage point increase in the debt-to-GDP ratio. The bias-robust p-value is also larger, however, at 0.053.

Overall, we view this exercise as indicating that although individual observations do have meaningful effects on our point estimates, it is not the case that the main results are influenced in a uniform direction by highly-influential observations. It is natural that individual observations have a noticeable effect on our estimates in light of our small sample and our RD design. We find it reassuring that the influential observations we identify have varied influences on our baseline estimate and that dropping the most influential observations actually increases the estimated second-stage effect.

Finally, column 5 shows that our results are robust to using an alternative data set for our macroeconomic variables.[39] Specifically, we use the historical macro financial data set compiled by Jordà et al. (2017), or "JST" hereafter. One advantage of using this data set is that all of the macroeconomic series were constructed by researchers other than ourselves. Therefore, the results will not be sensitive to our own particular choices among alternative data sources. Another advantage is that debt-to-GDP ratios in the JST data set extend through 2016, which allows us to consider elections through 2011; the debt-to-GDP ratios in Reinhart and Rogoff (2011) extend through 2010, limiting us to considering elections through 2005 in our main data set.

A disadvantage of using the JST data set is that it includes substantially fewer countries than our

[38]Those observations are the United Kingdom in 1951 and 1974, New Zealand in 1981 and 1993, and Ireland in 1982.
[39]Our data sources for the election outcomes do not change.

main data set. The data set comprises 17 countries, including the United States, which we exclude from our baseline analysis. We cluster our standard errors at the country level, so the reduction in the number of countries in the analysis matters for inference in addition to the (proportionally more modest) reduction in the number of individual observations.

Column 5, using the JST data set, produces a second-stage point estimate that a one percentage point increase in the debt-to-GDP ratio increases real long-term interest rates by 6.2 basis points. This effect is less precisely estimated than in our baseline data set, however, with a robust p-value of 0.053. We interpret these results as indicating that the relatively large effect of public debt on real interest rates we have estimated in our main data set is unlikely to be driven by the specific choices we made in assembling our baseline data set. These results also suggest that our baseline estimates would likely be robust to extending the baseline data set later in time through the period following the Global Financial Crisis.[40]

[40]Dropping the Great Recession period, similar to the analysis in Column 2, our second-stage point estimate falls to 5.5 basis points to, while adding a dummy for the Great Recession period raises our point estimate to 6.9 basis points. However, our results are no longer statistically significant at conventional levels in both cases.

Table 8: Regression Discontinuity Design Estimates - Subsample Stability

	(1) Baseline 1950-2010	(2) Drop Great Recession 1950-2007	(3) Longer than 1-year 1950-2010	(4) Drop Sensitive Dates 1950-2010	(5) JST Dataset 1950-2016
First Stage: Effect of Single-Party Majority on Debt-to-GDP Ratios					
Debt-to-GDP ratio	-21.39	-16.44	-17.59	-21.11	-26.95
Clustered standard error	10.13	8.92	10.22	10.76	8.80
Bias-robust p-value	0.02	0.03	0.04	0.03	0.00
Reduced Form: Effect of Single-Party Majority on Real Interest Rates					
Real long-rate	-1.19	-0.93	-1.46	-1.37	-1.69
Clustered standard error	0.57	0.56	0.57	0.44	0.51
Bias-robust p-value	0.02	0.04	0.00	0.00	0.00
Second Stage: Effect of Debt-to-GDP Ratios on Real Interest Rates					
Real long-rate	0.056	0.057	0.078	0.065	0.063
Clustered standard error	0.023	0.030	0.040	0.033	0.030
Bias-robust p-value	0.014	0.042	0.071	0.053	0.001
Bandwidth	6.36	7.84	6.58	6.27	6.65
Number of Observations	308	292	250	303	275
Number of countries	25	25	25	25	16

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) truncates the elections data in 2002 so that the five-year change in the Debt-to-GDP ratio ends in 2007, prior to the onset of the Great Recession. Column (3) drops election dates that led to governments that lasted less than one year. As discussed in Section A.4, Column (4) drops the five election dates that result in the largest absolute change in our second-stage point estimate. Column (5) uses data from the macrohistory database of Jorda et al. (2017) described in Section 3.2.

A.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 9 shows our baseline results for comparison purposes, which are based on an optimal MSE bandwidth choice of $[-6.36, +6.36]$ and the triangular kernel.[41] 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, and thus optimizes the bias-variance tradeoff. Columns 2 and 3 present RD estimates with double and half the baseline bandwidth, respectively. The estimated effect on real rates is virtually unchanged when we double the bandwidth but the results are slightly noisier, with a p-value of 0.046. Halving the bandwidth produces a second stage estimate that is 2 basis points higher than our baseline and is highly statistically significant.

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 around 5–6 basis points per one percentage point increase in the debt-to-GDP ratio in both specifications, but the statistical significance of the estimates is somewhat lower than in the baseline specification using both alternative kernels.

[41] We estimate the RD design using the `rdrobust` package of Calonico et al. (2014).

Table 9: Econometric Robustness of Regression Discontinuity Design Estimates

	(1) Baseline	(2) Bandwidth Double	(3) Bandwidth Half	(4) Uniform Kernel	(5) Epanechnikov Kernel
First Stage: Effect of Single-Party Majority on Debt-to-GDP Ratios					
Debt-to-GDP ratio	-21.39	-13.59	-27.24	-14.49	-17.43
Clustered standard error	10.13	9.26	11.26	9.11	9.44
Robust p-value	0.02	0.02	0.00	0.05	0.03
Reduced Form: Effect of Single-Party Majority on Real Interest Rates					
Real long-rate	-1.19	-0.75	-2.04	-0.72	-0.95
Clustered standard error	0.57	0.52	0.72	0.56	0.56
Robust p-value	0.02	0.01	0.00	0.09	0.05
Second Stage: Effect of Debt-to-GDP Ratios on Real Interest Rates					
Real long-rate	0.056	0.055	0.075	0.049	0.055
Clustered standard error	0.023	0.031	0.024	0.033	0.028
Robust p-value	0.014	0.046	0.001	0.106	0.043
Bandwidth	6.36	12.71	3.18	7.14	7.33
Number of Observations	308	308	308	308	308
Number of countries	25	25	25	25	25

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 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 double and one-half 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.

Table 10: Regression Discontinuity Design Estimates Excluding Sensitive Dates

	(1) Baseline	(2) UK 1951	(3) UK 1974	(4) New Zealand 1981	(5) Ireland 1982	(6) New Zealand 1993
Real long rate	0.056	0.065	0.044	0.042	0.068	0.071
Clustered standard error	0.023	0.028	0.025	0.024	0.029	0.034
Robust p-value	0.014	0.019	0.072	0.055	0.019	0.031
Bandwidth	6.36	6.78	6.54	8.39	6.53	6.44
Seat share relative to majority		1.28	0.16	0.00	-1.81	0.00
Number of Observations	308	307	307	307	307	307
Number of countries	25	25	25	25	25	25

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 column (1) of Table 1, but with one election date dropped at a time. Column (1) corresponds exactly to the baseline specification of column (1) in Table 1. Columns (2) through (6) drop country-election observations that are referenced in the column header.

A.4 Sensitive Dates

The final robustness analysis we conduct is to evaluate the our results' sensitivity to each country-election observation. Our baseline analysis includes only 308 observations, which allows us to conduct this exercise easily.

We drop one observation at a time and re-estimate our baseline fuzzy RD design, presented in equations 2–4, 308 separate times. Table 10 presents our baseline results, in column 1, along with the results for the five specifications that cause the largest absolute change in our second-stage point estimate when dropped. The column headers for columns 2 through 6 indicate which observation was dropped, and the columns are ordered chronologically. Each observation featured a close election, as indicated in the row "Vote share relative to majority," which displays the normalized running variable expressed in percentage points.[42]

The estimated second-stage effects in Table 10 range from 4.2 to 7.1 basis points. Three of the regressions produce larger point estimates than in our baseline specification, while two regressions

[42]The triangular kernel assigns the greatest weight to observations close to the cutoff. Hence, it is not surprising that the five most influential observations also featured very close election outcomes.

produce smaller estimates. The regressions produce less statistically significant point estimates, with column (2), which drops the United Kingdom election of October 1974, producing a bias robust p-value of 0.07. In each column, however, the estimated effect remains statistically significant at the 10-percent confidence level.

B Local Randomization Evidence on Public Debt's Effects on Real Interest Rates

In this section, 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 approach, but it employs alternative assumptions and inference procedures. The results of the local randomization approach demonstrate that our RD results do not arise from extrapolation away from the cutoff for a single-party majority via the local polynomials $f(\cdot)$ and $g(\cdot)$ in equations (2) and (3). Instead, there are economically large and statistically significant discontinuities in debt-to-GDP ratios and real interest rates that can be detected in a narrow window around the cutoff for a single-party majority without extrapolation via local polynomials.

The local randomization evidence is valuable because of the potential tension between the slopes of the estimated local polynomials illustrated in Figure 2, which displays our main RD plots, and the explanation given by Roubini and Sachs (1989a) for why single-party majority governments should exhibit more fiscal discipline than coalition and minority governments. The local randomization evidence reassuringly clarifies that the local polynomials are not an essential driver of our main results.

We follow Cattaneo et al. (2015) in summarizing the main features of the local randomization inference design, adapted for concreteness to our context and implementation. The local randomization inference approach begins with the assumption that there is a 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 $V - \bar{c}$ is assumed not to have any independent influence on outcomes within the window W. Further, the effect of a single-party majority is assumed to be locally constant within the window W. The two preceding 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 (SUTVA), 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. The approach also maintains the assumption of the exclusion restriction from the RD design, which states that the presence or absence of a single-party majority affects real interest rates only through its effect on the debt-to-GDP ratio, rather by other channels.

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

$$Pr(\mathbb{M} = \mathbb{m}) = \binom{n}{m}^{(-1)}. \tag{9}$$

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

Table 11: 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	-14.804	-14.365	-13.862	-12.306	-9.628
Bias-robust p-value	0.148	0.076	0.032	0.030	0.078
Reduced Form: Effect of Single-Party Majority on Real Interest Rates					
Real long-rate	-1.988	-1.399	-1.075	-0.975	-0.646
Bias-robust p-value	0.006	0.012	0.000	0.008	0.098
Second Stage: Effect of Debt-to-GDP Ratios on Real Interest Rates					
Real long-rate	0.134	0.097	0.078	0.079	0.067
Bias-robust p-value	0.006	0.012	0.000	0.008	0.098
Bandwidth	1.00	1.25	1.50	1.75	2.00
Number of Observations	311	311	311	311	311
Effective Number of Observations	17	22	32	42	49
Number of countries	26	26	26	26	26

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. The randomization inference procedure reports the same bias robust p-values in the reduced form and second stage.

mean outcomes of observations on the two sides of the cutoff $\bar{c}$ and within the window W. We calculate the second-stage treatment effect analogously to the RD design, by dividing the estimated reduced form effect of a single-party majority on real interest rates by the first-stage effect on the debt-to-GDP ratio. 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 (9).[43] Nonetheless, given the small numbers of observations in the windows we consider, it is worth emphasizing that the p-

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

values we report are valid for finite samples—they do not rely on asymptotic approximations.

Table 11 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 3 of table 1 for which the running variable $V_{it} - \overline{c}_{it}$ 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, the estimate from column 3 of table 1, and uniformly larger than our baseline estimate from column 1 of table 1. The second-stage finite sample p-values easily allow us to reject the null hypothesis that the debt-to-GDP ratio has no effect on real interest rates at the 95% confidence level, except in column 5, which has a p-value of 0.097. One possible reason for the weaker statistical significance in column 5 is that the stringent assumptions underlying the local randomization approach, most importantly that the distance of the largest party's vote share from the cutoff does not affect economic outcomes, may be less likely to hold as the estimation window becomes wider.

Overall, we view the results from the local randomization exercise as strongly complementary to our main RD evidence. The local randomization results show that even in a narrow window around the cutoff for a single-party majority, the presence of such a majority leads to lower debt-to-GDP ratios and lower real interest rates. The debt-to-GDP ratio's implied effect on real interest rates is larger than our baseline estimate. Most importantly, these results demonstrate that extrapolation away from the cutoff via local polynomials is not essential to produce the results we estimate in the RD design.

C Estimates using changes in the debt-to-GDP ratio at different horizons

In our baseline specification presented in Table 1 of Section 4.1, 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), so that horizon is likely 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.

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

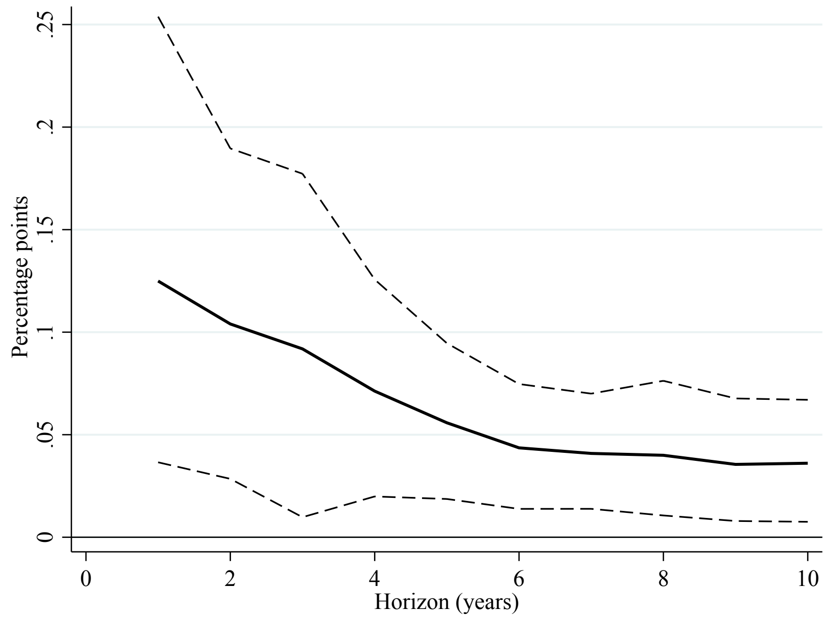
$$\Delta D_{it}^{h} = \gamma + \delta M_{it} + g(V_{it} - \bar{c}_{it}) + \nu_{it} \tag{10}$$

$$\Delta R_{it} = \alpha + \tau M_{it} + f(V_{it} - \bar{c}_{it}) + \varepsilon_{it} \tag{11}$$

where $\Delta D_{it}^{h} = [D_{i,t+h} - D_{it}]$ is the $h-$year change in the debt-to-GDP ratio for country i between election-year t and h years later. All other variables are defined as described in Section 3.

Figure 5 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 a 12 basis point increase in real long-term interest rates, whereas using three-year horizon leads to a smaller estimated effect of about 9 basis points. At a horizon of six to seven years, the estimated second-stage effect stabilizes around 4 basis points.

Figure 5: Effect on Real Rates for a 1% Increase in Debt-to-GDP Over Different Horizons (90% confidence interval)



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 10 and 11.

D Data Appendix

This 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 two 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 present tables summarizing various features of our data set.

D.1 Elections Data

We 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 76 seats in the first government following the election, instead recording the 29 seats won by the Conservative party as the highest individual-party seat total.

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

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

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

Our approach to combining the data sets is as follows: we begin with the data set of Seki and Williams (2014). We then re-code the running variable $V_{it} - \bar{c}_{it}$ according to the *ParlGov* figures of Döring and Manow (2019) in cases in which the party winning the most seats was excluded from the government formed after the election; in countries for which Seki and Williams (2014) consistently code two parties in the *ParlGov* dataset as the same party, we preserve that convention. We also take the Döring and Manow (2019) values in cases of a discrepancy when our research indicates that it is clearly preferable to the coding in Seki and Williams (2014). Finally, when our research is ambiguous, we retain the coding in Seki and Williams (2014).

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 $V_{it} - \bar{c}_{it}$ as follows:

$$V_{it} - \bar{c}_{it} \equiv \frac{\text{Largest Party Seats}_{it} - \big(\text{int}(\text{Total Seats}_{it}/2) + 1\big)}{\text{Total Seats}_{it}}, \tag{12}$$

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:

$$M_{it} = 1[V_{it} \geq \bar{c}_{it}]. \tag{13}$$

A value of 0 for $V_{it} - \bar{c}_{it}$ 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 $V_{it} - \bar{c}_{it}$ 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. $V_{it} - \bar{c}_{it}$ 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.[44]

Table 12 lists the 27 countries and the election years that we use in this paper. As noted above, we exclude the United States from our baseline sample because it does not have a parliamentary system of government, but we do show a specification including it to analyze the robustness of our results. Iceland is also excluded from our baseline sample because it lacks information for one of our baseline covariates, but we include it in the analogous specification without covariates.

D.2 Economics Data

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

Tables 12–15 in Appendix D.3 summarize our data in detail. The unit of observation in our regressions is a country-election. We measure all economic data at an annual frequency. Our baseline estimation includes data from 25 countries spanning 308 election-year observations from 1950 to 2010.[45]

Our main source for government debt-to-GDP ratios is Reinhart and Rogoff (2011), which extends from before World War II through 2010 for most of the countries in our sample. We use the central government debt-to-GDP ratio reported in Reinhart and Rogoff (2011) where it is available. Central government debt is not reported for the Netherlands, Poland, and Romania, so we use "general government debt" for those countries. As noted, we operationalize the variable ΔD_{it}

[44]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).

[45]There are three country-years in our baseline estimation sample that contain two elections: Denmark in 1953, the United Kingdom in 1974, and Ireland in 1982. Our baseline specification includes two observations each for those country-years, for which only the value of the running variable differs, reflecting the different outcomes of the two elections in each year. An unreported specification that retains only one election date in each of these cases by dropping the election date that corresponds to the resulting government with the shorter duration leads to a somewhat larger estimated second-stage effect than in our baseline sample, which remains statistically significant at standard confidence levels.

as the change in the government debt-to-GDP ratio from the year of the election to five years after the election; Appendix C shows results considering alternative horizons for the change in the debt-to-GDP ratio.

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, $\Delta \Pi^e_{it}$:

$$\Delta R_{it} = \Delta I_{it} - \Delta \Pi^e_{it}. \tag{14}$$

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}.[46]

We use multiple data sources to construct our primary series for nominal interest rates on long-term government bonds. Our preferred data source is 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. If that series is missing data, we next use the interest rate series for "Government Bonds" published in the International Monetary Fund's International Financial Statistics (International Monetary Fund, 2019), which is listed under the category "Financial, Interest Rates, Government Securities." Our next source in order of preference is the interest rate series for "Long-Term Government Bonds" reported in Homer and Sylla (2005); if that data is missing, we supplement it where possible with interest rates for a specific class of government bonds from the same source.

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 must construct a proxy. We use two primary data sources to construct our proxy for expected inflation. The first is the Consumer Price Index for "all items non-food non-energy" (core inflation) published by the OECD (2020). The second is the inflation data contained in Reinhart and Rogoff (2011), which was collected from various sources and generally corresponds to all-items CPI inflation in recent history. We follow Council of Economic Advisers (2015) in proxying for expected inflation Π^e_{it} as the five-year

[46] 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.

unweighted moving average of current and past annual inflation. We use core inflation in country-years when it is available, and if not we use the all-items measure from Reinhart and Rogoff (2011). We use changes in core inflation where possible because core inflation is more stable from year to year and is a better predictor than all-items inflation, leading to more precise estimates.[47]

The variables we have discussed so far are sufficient to run our simplest RDD specification, but 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, 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).[48] We use the inflation series in Reinhart and Rogoff (2011) when we use inflation as a covariate because it has substantially better coverage (nearly complete in our sample) than the OECD's core measure. Splicing together the level of inflation from multiple sources introduces spurious series breaks, adding noise to the estimates, so we avoid it when we include the level of inflation rather than the change. We also use the economic crisis dummies of Reinhart and Rogoff (2009) as updated in Reinhart and Rogoff (2011). Reinhart and Rogoff (2011) define crisis indicators for several different types of crises; we create a single crisis indicator that takes value one if Reinhart and Rogoff (2011) code a country-year as having a crisis related to stock markets, the banking sector, currency crises, high inflation, or sovereign debt.[49]

We begin our analysis in 1950 to avoid any measurement or other confounding issues related to

[47] 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 of the two data sources individually and then splice the series together across the two sources, rather than splicing the levels and then calculating differences.

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

[49] Reinhart and Rogoff (2011) define a currency crash as occurring if the domestic currency depreciates by 15% or more relative to the relevant anchor currency. There are 13 instances of currency crisis 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. Or second, there are no bank runs, but the closure, merging, takeover, or large-scale government assistance of at least one important financial institution begins a sequence of similar outcomes for other financial institutions in the country. A sovereign debt crisis occurs if the central (federal) government defaults by failing to meet a principle or interest payment on debt by the due date. Scenarios in which bank deposits have been frozen or forcibly converted from foreign currency to the local currency are also included as part of a sovereign debt crisis. Because most of these types of crises are quite rare individually, we aggregate them into a single crisis indicator, which takes value 1 if Reinhart and Rogoff (2011) code any type of crisis as occurring in a given country-year observation, and which takes value 0 otherwise.

World War II and its short-term aftermath. The debt-to-GDP ratios in Reinhart and Rogoff (2011) end in 2010, and we examine the 5-year change in the debt-to-GDP ratio, so the most recent elections included in our sample are from 2005. Our baseline sample includes 308 election-year observations from 25 countries that have the necessary data to be included in the analysis.[50] Appendix D provides summary statistics and coverage details for the data used in the analysis.

D.2.1 Alternative Data Set

As a complement to our main analysis, we also conduct our core analysis using a separate data set for all macroeconomic variables from the "macrohistory database" of Jordà et al. (2017). We use Release 4 of the data set. That data set contains all of the economic variables necessary to conduct our core analysis for a panel of 17 countries, of which we use 16 in most specifications because we exclude the United States from our baseline sample. One advantage of the alternative data set in Jordà et al. (2017) is that all of the series have been produced by other researchers, so it does not reflect our own, potentially idiosyncratic, choices. Another advantage is that the debt-to-GDP ratios in the data set extend through 2016, whereas the debt-to-GDP ratios in Reinhart and Rogoff (2011) end in 2010. A disadvantage is that because the alternative data set contains fewer countries than our main data set, the estimates using the Jordà et al. (2017) data set are substantially less precise.

D.3 Data Summary

Tables 12−15 below summarize the data used in our analysis. Table 12 tabulates the election-years by country used in our baseline estimation while Table 13 tabulates the years for which data is available for each variable used in this paper. Tables 14 and 15 provide summary statistics of all the variables used in the paper for all available data or only the election-year observations used in our RD design estimation, respectively.

[50] Data for lagged real GDP growth and lagged inflation are available for the country-years that have complete data for the change in real rates and the change in debt-to-GDP ratios; data for lagged crisis indicators is available for all country-years except for three otherwise valid election-year observations for Iceland. The election results for those observations were all far from the cutoff for a single-party majority, so their inclusion or exclusion does not have a large impact on the results.

Table 12: Election Years Used in Baseline Estimation

country	Election Year
Australia	1951 1954 1955 1958 1961 1963 1966 1969 1972 1974 1975 1977 1980 1983 1984 1987 1990 1993 1996 1998 2001 2004
Austria	1966 1970 1971 1975 1979 1983 1986 1990 1994 1995 1999 2002
Belgium	1958 1961 1965 1968 1971 1974 1977 1978 1981 1985 1987 1991 1995 1999 2003
Canada	1953 1957 1958 1962 1963 1965 1968 1972 1974 1979 1980 1984 1988 1993 1997 2000 2004
Denmark	1950 1953 1953 1957 1960 1964 1966 1968 1971 1973 1975 1977 1979 1981 1984 1987 1988 1990 1994 1998 2001 2005
Finland	1987 1991 1995 1999 2003
France	1951 1956 1958 1962 1967 1968 1973 1978 1981 1986 1988 1993 1997 2002
Germany	1957 1961 1965 1969 1972 1976 1980 1983 1987 1990 1994 1998 2002 2005
Greece	1993 1996 2000 2004
Hungary	2002
Iceland*	1995 1999 2003
India	1951 1957 1962 1967 1971 1977 1980 1984
Ireland	1954 1965 1969 1973 1977 1981 1982 1982 1987 1989 1992 1997 2002
Italy	1953 1958 1963 1968 1972 1976 1979 1983 1987 1992 1994 1996 2001
Japan	1955 1958 1960 1967 1969 1972 1976 1979 1980 1983 1986 1990 1993 1996 2000 2003 2005
Netherlands	1952 1956 1959 1963 1967 1971 1972 1977 1981 1982 1986 1989 1994 1998 2002 2003
New Zealand	1951 1954 1957 1960 1963 1966 1969 1972 1975 1978 1981 1984 1987 1990 1993 1996 1999 2002 2005
Norway	1953 1957 1961 1965 1969 1973 1977 1981 1985 1989 1993 1997 2001 2005
Poland	2001
Portugal	1976 1979 1980 1983 1985 1987 1991 1995 1999 2002 2005
South Africa	1953 1958 1961 1966 1970 1974 1977 1981 1987 1989 1994
Spain	1979 1982 1986 1989 1993 1996 2000 2004
Sweden	1952 1956 1958 1960 1964 1968 1970 1973 1976 1979 1982 1985 1988 1991 1994 1998 2002
Switzerland	1951 1955 1959 1963 1967 1971 1975 1983 1987 1991 1995 1999 2003
Turkey	1950 1954 1957 1961 1965
United Kingdom	1950 1951 1955 1959 1964 1966 1970 1974 1974 1979 1983 1987 1992 1997 2001 2005
United States**	1950 1952 1954 1956 1958 1960 1962 1964 1966 1968 1970 1972 1974 1976 1978 1980 1982 1984 1986 1988 1990 1992 1994 1996 1998 2000 2002 2004

*: Iceland is missing data for crisis indicators so it is only included in column (3) of Table 1 and all columns of Table 11.
**: United States is only included in column (2) of Table 1.

Table 13: Data Availability of Key Variables from $1950 - 2010$

country	Debt-to-GDP		5-Year Change in Debt-to-GDP		Real Long-Rate		1-Year Change in Real Long-		Lagged Inflation Rate		Lagged Real GDP per capita		Election Outcomes	
	First	Last	First	Last	First	Last	First	Last	First	Last	First	Last	First	Last
Australia	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1951	2013
Austria	1950	2010	1950	2005	1965	2010	1965	2009	1950	2011	1950	2014	1953	2013
Belgium	1950	2010	1950	2005	1951	2010	1951	2009	1952	2011	1950	2014	1950	2014
Canada	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1953	2011
Denmark	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1950	2011
Finland	1950	2010	1950	2005	1987	2010	1987	2009	1950	2011	1950	2014	1951	2011
France	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1951	2012
Germany	1951	2010	1951	2005	1950	2010	1950	2009	1951	2011	1950	2014	1953	2013
Greece	1950	2010	1950	2005	1986	2010	1986	2009	1950	2011	1950	2014	1950	2012
Hungary	1991	2010	1991	2005	2000	2010	2000	2009	1952	2011	1950	2014	1990	2014
Iceland	1950	2010	1950	2005	1992	2010	1992	2009	1950	2011	1952	2014	1953	2013
India	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1951	2009
Ireland	1950	2010	1950	2005	1952	2010	1952	2009	1950	2011	1950	2014	1951	2011
Italy	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1953	2013
Japan	1954	2010	1954	2005	1950	2010	1950	2009	1950	2011	1950	2014	1952	2014
Netherlands	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1952	2012
New Zealand	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1951	2014
Norway	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1953	2013
Poland	1990	2009	1990	2004	2001	2010	2001	2009	1951	2011	1952	2014	1989	2011
Portugal	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1976	2011
South Africa	1950	2009	1950	2004	1950	2010	1950	2009	1950	2011	1950	2014	1953	1994
Spain	1950	2010	1950	2005	1978	2010	1978	2009	1950	2011	1950	2014	1977	2011
Sweden	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1952	2014
Switzerland	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1951	2011
Turkey	1950	2010	1950	2005	1950	1969	1950	1968	1950	2011	1950	2014	1950	2007
United Kingdom	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1950	2010
United States	1950	2010	1950	2005	1950	2010	1950	2009	1950	2011	1950	2014	1950	2012

Table 14: Summary Statistics by Country: Using All Available Data from 1950 − 2010

country	Debt-to-GDP			5-Year Change in Debt-to-GDP			Real Long-Rate			1-Year Change in Real Long-Rate			Lagged Inflation Rate			Lagged Real GDP per capita growth		
	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N
Australia	30.90	25.02	61	-7.23	8.40	56	1.80	3.91	61	0.13	1.40	60	5.59	3.59	62	2.06	1.74	65
Austria	31.63	19.99	61	3.74	6.39	56	3.38	1.28	46	-0.02	0.79	45	6.45	10.31	62	3.42	3.40	65
Belgium	77.68	24.15	61	2.03	15.67	56	3.40	1.90	60	0.03	1.06	58	3.53	2.37	60	2.39	2.09	65
Canada	57.91	16.54	61	-3.48	13.72	56	2.82	2.19	61	0.09	1.03	60	4.07	2.75	62	2.01	2.28	65
Denmark	37.58	24.03	62	-0.53	18.26	57	3.63	2.36	62	-0.08	1.15	61	4.91	2.92	63	2.14	2.45	66
Finland	23.21	17.53	61	1.40	15.64	56	4.22	1.95	24	-0.16	1.02	23	6.64	5.98	62	2.80	3.22	65
France	34.36	19.12	61	4.04	7.40	56	1.11	6.38	61	0.51	2.37	60	6.79	7.96	62	2.55	2.26	65
Germany	18.29	11.34	60	2.97	4.19	55	1.85	2.95	59	0.12	1.52	57	4.57	3.39	61	3.23	3.73	65
Greece	52.26	42.07	62	9.79	13.67	57	1.65	2.68	22	0.06	1.37	20	14.64	31.89	63	3.00	4.09	66
Hungary	91.68	37.41	20	-27.30	30.16	15	-0.05	3.26	11	0.86	1.47	10	8.24	7.88	60	2.20	3.61	65
Iceland	28.08	22.16	61	5.07	17.54	56	3.32	3.95	19	0.13	2.34	18	16.05	15.26	62	2.63	4.04	63
India	34.35	5.89	61	1.30	6.74	56	-0.40	2.83	42	-0.01	2.06	40	6.74	3.15	62	2.92	3.20	65
Ireland	66.26	23.34	62	-0.74	19.49	57	2.29	2.39	54	-0.08	1.49	52	6.22	4.81	57	3.17	3.17	66
Italy	68.05	33.69	61	6.66	11.41	56	2.05	3.51	61	0.21	2.05	59	6.99	6.55	62	2.82	2.89	65
Japan	55.05	54.27	57	15.65	17.98	52	-0.67	10.18	59	1.04	4.32	57	9.83	33.89	62	4.14	3.77	65
Netherlands	65.35	20.13	61	-4.87	14.77	56	2.12	2.42	61	0.06	1.01	60	3.99	2.21	62	2.34	2.34	65
New Zealand	50.63	17.87	61	-5.44	11.36	56	1.44	2.88	61	0.06	1.13	60	5.99	4.30	62	1.71	3.50	65
Norway	25.32	5.41	61	-0.77	6.86	56	1.82	2.44	61	0.00	1.03	60	4.79	2.74	62	2.61	1.88	65
Poland	53.19	17.48	20	-11.33	20.62	15	2.32	1.33	10	0.26	0.78	9	27.83	50.65	61	2.56	3.46	63
Portugal	27.00	20.81	61	5.08	9.22	56	0.12	3.94	60	0.06	1.85	58	7.97	8.15	62	3.05	3.36	65
South Africa	41.45	7.72	60	-2.20	6.75	55	2.01	2.41	61	0.03	1.03	60	7.75	4.51	62	1.19	2.33	65
Spain	30.13	13.64	61	-0.28	11.01	56	1.97	3.22	33	0.26	1.40	32	7.57	4.74	62	3.17	3.36	65
Sweden	38.79	18.89	61	0.72	16.78	56	1.77	2.32	61	0.01	1.15	60	5.30	3.09	62	2.19	2.23	65
Switzerland	17.97	8.85	59	-1.12	6.85	52	1.32	1.12	61	0.02	0.75	60	2.73	1.67	62	1.61	2.47	65
Turkey	28.93	12.40	58	2.79	13.86	50	-1.81	4.30	20	-0.34	1.96	19	30.48	27.37	62	3.18	4.33	65
United Kingdom	66.18	41.26	62	-10.18	17.17	57	2.21	2.75	62	0.00	1.51	61	5.56	4.16	63	1.98	1.99	66
United States	53.44	15.24	61	-0.03	10.72	56	2.23	1.76	61	0.08	0.90	60	3.91	2.35	62	1.97	2.42	65
All	43.28	29.61	1558	0.50	14.49	1418	1.83	3.79	1314	0.11	1.66	1279	8.34	16.50	1666	2.56	3.08	1755

Table 15: Summary Statistics by Country: Actual Data Used in Baseline Estimation from $1950 - 2010$

country	Debt-to-GDP			5-Year Change in Debt-to-GDP			Real Long-Rate			1-Year Change in Real Long-Rate			Lagged Inflation Rate			Lagged Real GDP per capita growth		
	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N	mean	std. dev	N
Australia	33.03	24.11	22	-7.27	7.76	22	1.39	4.00	22	0.18	1.55	22	6.06	3.62	22	2.08	1.62	22
Austria	34.47	20.16	12	5.25	5.57	12	3.76	1.16	12	-0.14	0.86	12	3.76	1.78	12	2.69	2.18	12
Belgium	73.90	27.32	15	4.29	18.50	15	3.49	2.20	15	-0.09	1.06	15	4.25	2.89	15	3.37	1.96	15
Canada	55.80	14.91	17	-2.05	12.53	17	3.29	1.29	17	0.13	1.01	17	3.89	3.04	17	2.76	2.05	17
Denmark	35.44	24.87	22	3.17	19.68	22	3.96	2.56	22	-0.05	0.79	22	5.57	2.93	22	2.08	2.34	22
Finland	38.76	21.94	5	5.22	28.73	5	4.67	1.91	5	0.17	0.68	5	3.64	2.26	5	2.33	2.12	5
France	29.14	15.32	14	2.38	7.13	14	0.61	7.42	14	0.98	2.15	14	7.39	8.99	14	2.92	1.63	14
Germany	17.63	10.10	14	3.35	5.09	14	2.64	2.64	14	-0.14	1.28	14	4.46	3.05	14	2.63	2.69	14
Greece	105.87	5.52	4	6.07	8.58	4	2.94	2.50	4	-1.29	1.13	4	9.87	6.40	4	2.44	2.35	4
Hungary	61.38	.	1	-0.04	.	1	-2.62	.	1	1.61	.	1	12.31	.	1	4.01	.	1
Iceland	47.73	9.80	3	1.32	25.94	3	4.87	2.29	3	1.15	0.82	3	4.10	2.21	3	2.52	3.01	3
India	31.75	4.21	8	1.61	4.15	8	-1.03	2.74	8	1.14	2.31	8	6.31	3.79	8	0.37	3.96	8
Ireland	74.08	22.39	13	0.18	21.50	13	2.58	2.69	13	-0.69	1.75	13	7.76	5.10	13	3.36	2.73	13
Italy	67.76	34.85	13	5.49	11.08	13	2.68	2.48	13	-0.25	1.51	13	7.03	5.29	13	2.93	3.04	13
Japan	49.31	46.49	17	15.74	18.72	17	1.63	2.61	17	0.18	1.33	17	4.00	3.69	17	4.07	3.31	17
Netherlands	63.49	18.36	16	-3.32	13.24	16	2.37	2.35	16	-0.08	0.86	16	4.19	2.16	16	1.98	2.30	16
New Zealand	52.27	17.34	19	-4.40	12.53	19	1.27	3.07	19	0.19	1.54	19	6.28	4.49	19	1.93	3.45	19
Norway	25.72	5.32	14	-0.28	6.93	14	1.71	2.39	14	0.20	0.72	14	5.09	2.70	14	3.56	1.69	14
Poland	37.24	.	1	10.52	.	1	0.76	.	1	-0.73	.	1	12.80	.	1	4.59	.	1
Portugal	25.69	20.98	11	12.09	9.18	11	-1.87	4.74	11	-0.42	2.05	11	14.83	9.26	11	1.54	3.64	11
South Africa	41.69	7.69	11	-0.23	6.26	11	1.33	1.80	11	0.24	0.89	11	8.28	5.25	11	1.29	1.84	11
Spain	33.44	15.47	8	6.53	12.66	8	1.70	3.78	8	0.91	1.74	8	9.15	6.36	8	2.48	2.16	8
Sweden	37.53	19.97	17	2.10	17.00	17	1.76	2.32	17	0.18	0.92	17	6.14	3.10	17	2.17	1.79	17
Switzerland	17.58	9.68	13	-0.77	7.20	13	1.18	1.07	13	0.12	0.51	13	2.86	1.70	13	1.44	2.50	13
Turkey	20.09	4.17	5	4.33	6.80	5	-1.41	4.93	5	-1.41	1.76	5	5.60	4.85	5	1.01	5.72	5
United Kingdom	75.99	52.72	16	-12.51	22.45	16	1.78	3.34	16	-0.34	2.22	16	5.69	3.86	16	2.56	2.22	16
United States	51.89	14.79	28	-0.68	10.85	28	2.31	1.87	28	0.04	0.81	28	4.08	2.47	28	2.23	2.13	28
All	45.55	29.56	339	1.10	14.63	339	2.00	3.24	339	0.05	1.38	339	5.79	4.71	339	2.43	2.55	339