Government Spending and Voting Behavior

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Abstract

Does government spending on public goods affect the vote choice of citizens? On the one hand, voters have been characterized as "fiscal conservatives" who may turn toward conservative parties when government spending goes up. On the other hand, increased spending signals that the economy is doing well, which makes progressive parties a more viable option. To adjudicate between both hypotheses, this paper draws on a natural experiment, which created exogenous variation in government spending. A discontinuity in the 2011 German census meant that some municipalities saw a significant, unforeseen increase in budgets. Using a well-powered regression discontinuity coupled with a difference-in-differences design, we show that the increase in budgets and subsequent spending on public goods benefited left-leaning parties. To parse out the causal channel, we rely on panel evidence and demonstrate that residents in treated municipalities viewed their economic situation more favorably, which led them to switch to progressive parties.

Word count: 10,000

1 Introduction

What are the electoral consequences of public goods spending? The fact that public goods are slow to produce and have unclear distributional effects make them rather unlikely candidates to affect the vote choice of citizens. Yet, historic evidence from the New Deal demonstrates that public goods spending can have a lasting impact on the electoral landscape. America's largest public investment program produced a profound political realignment, splitting the country into so-called New Deal "liberals" and "conservatives"[1]—terms that dominate U.S. politics to this day (Domhoff and Webber, 2011; Schickler, 2013). If public goods spending, indeed, changes partisan loyalties, which parties benefit and why?

How public goods spending affects partisan proclivities is theoretically unclear. On the one hand, voters have been characterized as "fiscal conservatives" who move to anti-spending parties when government spending increases (Peltzman, 1992a). Related, if public preferences for spending are stable, an increase in government spending, ceteris paribus, should reduce demand for additional spending and thus benefit fiscally conservative parties (Wlezien, 1995). On the other hand, increased spending may signal to voters that the economy is doing well, which makes left-leaning parties a more attractive option. Increased spending may also lead voters to appreciate the public goods being provided and thus spur an appetite for additional investment (Korpi and Palme, 1998).

To make headway on these conflicting theoretical predictions, this paper makes use of a natural experiment. The 2011 German census applied a new, registry-based population estimation method in municipalities below 10,000 inhabitants. Municipalities above the threshold, by contrast, were assessed using a more traditional, survey- and registry-based method. As a result, small municipalities saw population changes significantly overestimated, resulting in a relative population gain (on paper). Population figures, in turn, form the basis for state fiscal transfers to municipalities. By changing population figures,

[1] In what follows, we use the term "left", "liberal" or "progressive" to capture parties in favor of increasing public spending. Details are provided in Section 3.3.

the census therefore exposed small municipalities to an exogenous increase in transfers from the state and a subsequent rise in government spending.

We take advantage of this natural experiment in order to explore the relationship between public goods spending and citizens' vote choice. Using a regression discontinuity coupled with a difference-in-differences design, we provide two pieces of evidence. First, we show that the shock to population figures led to a sizable increase in transfers from states to local municipalities. Focusing on non-earmarked transfers—which municipalities can spend at their own discretion—we estimate that the new census method raised transfers by 6 percent compared to municipalities where the traditional survey- and registry-based method was applied. Using data on public tenders, we show that the money was spent on highly visible public goods, including the renovation of schools and the funding of public swimming pools.

Second, we show that the exogenous increase in government spending on public goods raised the vote share of pro-spending parties by 1.3 percentage points between the 2013 and 2017 federal elections (RI p-value of 0.029). We find similar treatment effects in state and municipal elections, where pro-spending parties gain between 1 and 3 percentage points. Crucial for our case: we also demonstrate that the effect is not mediated by incumbent status: increased spending did *not* benefit incumbents. The evidence thus implies that public goods spending can engender broad changes to citizens' vote choice to the benefit of the political left.

We ensure the robustness of the headline finding in five ways. First, we show that the results are robust to a variety of different RD bandwidths. Second, we randomly draw 1,000 pseudo RD cutoffs and show that there are no effects on citizens' vote choice. Third, we create an alternative placebo test by exploiting the fact that one federal state did not use the census to determine transfers to municipalities. Reassuringly, we find that the state saw *no* changes in voting behavior. Fourth, we estimate donut hole RD regressions—leaving out observations just around the cutoff—and demonstrate that results are unchanged. Fifth, we use the federal elections preceding the census change as a placebo test and confirm that there are no effects on citizens' vote choice.

Why did the exogenous increase in government spending nudge citizens' vote choice toward the political left? To explore the causal mechanism, we proceed in two steps. First, we assess whether the increase in spending changed voters' preferences or whether it merely changed who turned out to vote. To tease both channels apart, we draw on panel evidence. Tracking the same respondents before and after the spending increase allows us to show that there was, indeed, a change in preferences. In 2017—the year of the first federal election after the spending increase—respondents in treated municipalities are 5 percentage points more likely to identify with progressive parties relative to the control group. What is more, we also document that the spending increase had no effect on turnout. Both pieces of evidence imply that government spending changed voters' preferences, rather than simply changing who turned out to vote.

Second, we explore why government spending led voters to endorse the political left. Evidence on the precise causal mechanisms linking public goods spending and preferences is necessarily tentative. To explore the two aforementioned causal channels—that is, spending functioning as a signal of economic growth versus spending creating demand for additional investment—we draw on local-level survey evidence. We show that residents in treated municipalities report greater subjective satisfaction with the economy. The evidence is thus in line with a theoretical model whereby increased public goods spending signals to voters that the economy is doing well, paving the way for progressive voting behavior. By contrast, we find no evidence that voters want more public investments. Government spending hence does not spur demand for "more of the same." Interestingly, we also find *no* effect on residents' incomes. The relation between spending and voting thus seemingly functions via individual perceptions about economic wellbeing, rather than real dollars in residents' purses.

Our paper adds to three important debates in political science. First, we add to a literature on redistribution. Canonical models of political economy stipulate that transfers are set by the median voter who determines the tax rate on the basis of her position in the income distribution (Meltzer and Richard, 1981). Our evidence opens a second pathway. When governments increase public goods spending, voters interpret this as a signal for economic wellbeing and then turn to parties on redistributive platforms. Importantly, this channel functions independent of individual incomes. As stated, we find no effect of public goods spending on average incomes. The traditional Meltzer and Richard-channel—whereby an increase in average incomes leads the median voter to demand more redistribution (assuming a right-skewed distribution)—is thus mute in our setting. And, still, voters demand more redistribution because they perceive the economy as booming (Abou-Chadi and Kayser, 2017).

Second, we provide an explanation for why left-leaning parties favor increased government spending on public goods. At first glance, public goods are not a progressive tool because all citizens—rich or poor—benefit (Epple and Romano, 1996). The case is different for transfers, which directly redistribute money from rich to poor. For this reason, one may expect left-leaning parties to prefer transfers over public goods provision. Conservative parties, by contrast, might favor public goods provision over transfers, at least as long as public goods are socially more efficient than a private provision (Samuelson, 1954). Our evidence, however, shows that voters interpret public goods as a signal of economic growth, leading the median voter to espouse redistribution. The fact that we do *not* see increased demand for public goods further underlines this point. Left-leaning parties, thus, have a good reason to endorse public goods provision because it ultimately leads voters to demand more redistribution.

Third, we add to a literature on policy feedback (Pierson, 1993; Weaver and Lerman, 2010). A large literature has made the case that increased welfare state spending ties citizens to the state and thus increases support for it (Korpi and Palme, 1998). One theoretical explanation for this relationship is that welfare state spending reduces individual risk (Gingrich and Ansell, 2012). Our paper points to an alternative feedback channel. We do not find that increased public goods spending creates demand for more public investments. There is thus no immediate feedback loop. Rather, there is a spillover effect whereby public goods spending leads voters to demand more redistribution because they perceive the economy to be on an upward trajectory. As such, the welfare state can "reproduce [its] own legitimacy" (Jaeger, 2009, 726) by investing in public goods.

2 Theoretical Background

How does government spending on public goods affect voters' preferences? In the following, we lay out competing logics how increased public goods spending can either increase or decrease support for progressive parties. While definitions of progressive / left and conservative / right are not without conceptual difficulties, we follow Tavits and Letki who write "[b]ecause the Left prefers more government control of the economy and the Right advocates reliance on the market, leftist governments are expected to produce a bigger government" (2009, 555). We thus conceptualize progressive parties as pro-spending and conservative parties as opposed to spending (Blais, 2006; Faricy, 2015). The characterization accurately describes our study context (Hayo and Neumeier, 2019; Potrafke, 2013), which we will corroborate below by drawing on party manifestos (Section 3.3).

Our theoretical discussion focuses on public goods provision, that is, state investments which are non-excludable and, at least partly, non-rival (e.g., roads, hospitals or public schools). The reason for focusing on public goods spending is twofold. First, while much has been written about the provision of welfare, we know comparatively little about the ways in which government spending on non-rival public goods affects voters' preferences. Second, our empirical focus is on municipal governments, which typically have no discretion over welfare spending (though they administer them).

Before delving into the theoretical arguments, we must note that we focus on the effect of public goods spending on voters' preferences. We do not explore effects on pro-incumbent voting behavior. While the latter is undoubtedly an interesting question, it has received significant scholarly attention (Anderson, 2007; Lewis-Beck and Paldam, 2000). Moreover, as we show below, we do not find any effects of government spending on incumbents. Our theoretical focus is therefore on the understudied effect of public spending on voters' preferences, independent of incumbency status. Finally, our theoretical considerations start from an exogenous increase in public spending, which was the case in our empirical setting. The theoretical mechanisms, however, could also be applied to a reduction in spending.

2.1 Spending and conservative voting

How can an exogenous increase in public goods spending lead citizens to shift their vote to conservative parties? We discuss two channels in turn.

Spending in a supply-demand framework. A first logic linking an increase in public goods spending to conservative voting arises when considering canonical models of public goods provision. According to the Samuelson condition, it is socially optimal to provide a public good as long as the benefits to citizens are greater than the costs of providing it (Samuelson 1954). Democratic governments have any reason to provide this optimal level in order to maximize support at the ballot box. If a government provides too little [much] of the public good, the median voter will switch to a competitor that increases [reduces] the public good. A similar argument is made by Wlezien in his "public as thermostat"-model (1995). The author hypothesizes that preferences for spending are fixed and voters punish parties from deviating from their ideal points. An increase in spending therefore reduces demand for spending and may then lead some individuals to switch to conservative parties, which—in our context—advocate for reducing public spending. Wlezien writes: "changes in preferences are negatively related to spending decisions, whereby the public adjusts its preferences for more spending downward (upward) when appropriations increase (decrease)" (1995, 981).[2]

Spending as a sign of inefficiency. A second logic linking an increase in public spending to conservative voting focuses on preferences for fiscal prudence or, put differently, the conjecture that voters are "fiscal conservatives." As much is argued by Peltzman (1992b). While there are a good reasons for the median voter to advocate for redistribution from rich to poor (Meltzer and Richard, 1981), the case is different for public goods. The

[2]The hypothesis also corresponds to the canonical Meltzer and Richard model. When incomes are unequally distributed, the median voter will enact a tax rate such that she receives transfers from the rich (Meltzer and Richard, 1981). An increase in public goods provision—which are funded via taxes and benefit everyone—therefore reduces transfers to the poor.

provision of public goods is known to be slow. Public goods also need to be funded by raising taxes, which create a deadweight loss (Battaglini and Coate, 2008). Partly as a result, Peltzman argues that the median voter is a "flinty-eyed fiscal conservative" whose "basic objection is to spending, not just the part financed by taxes" (1992b, 329). If the median voter is, indeed, opposed to public goods provision—that is, not just the level (see above), but *any* spending—an exogenous increase in public spending may lead her to switch to conservative parties.

2.2 Spending and progressive voting

How may an exogenous increase in public goods spending lead citizens to shift their vote to pro-spending parties? We discuss two channels in turn.

Spending as a signal of economic growth. A first logic linking increased public goods spending to progressive voting owes to the informational value of government investments. If the government increases public goods spending, this may be interpreted as a signal that the economy is doing well. "Economists," write Alesina et al. (2008, 1006), typically prescribe "that tax rates and discretionary government spending as a fraction of GDP ought to remain constant over the business cycle." In reality, however, government spending tends to go up during economic booms and down in recessions (Kaminski et al., 2004). The reasons for the spending cycle are manifold. Talvi and Vegh (2005), for instance, argue that the presence of surpluses—owing to an improved economic situation—increases the government's propensity to spend. When exposed to increased spending, the median voter may therefore infer that the economy is on an upward trajectory. This, in turn, may translate into an increased willingness (and financial leeway) to fund additional public projects or to redistribute from the rich to the poor, which should benefit pro-spending parties.

Spending as a driver of demand for public goods. A second logic linking increased government spending to progressive voting arises when considering the aforementioned debate on policy feedback loops. A large literature has made the case that welfare spending leads to more demand for redistribution. As Jaeger (2009, 726) puts it, "welfare regimes produce and reproduce their own legitimacy" (see also, Korpi and Palme 1998). One theoretical explanation for this relationship is that welfare state spending reduces individual risk (Gingrich and Ansell, 2012). A similar logic can also be applied to public goods provision. The construction of a public swimming pool, a hospital or a pre-school creates, as Jaeger (2009, 726) writes, "socially and culturally embedded institutions [...] and social belief systems" (see also, Hall, 1986). Therefore, increased government spending on public goods may spark additional demand for similar investments and thus benefit pro-spending parties.

3 Design

We have pointed out conflicting logics how an increase in government spending on public goods may affect the vote choice of citizens. To adjudicate between the different hypotheses, we make use of a natural experiment created by the 2011 German census.

3.1 Treatment: 2011 census

Germany conducts regular censuses, starting with the 1816 census in the Kingdom of Prussia. Besides characterizing the German population, the key purpose of the census is to provide accurate population figures. Perhaps the most important use of the census-based population estimates is to determine the size of transfers from the state level to local municipalities. Most tax revenues in Germany—e.g. income, capital gains or value-added taxes—accrue at the state and federal level. Local municipalities are only allowed to collect property taxes (which are comparatively low) and a fraction of corporate taxes. Yet, municipalities fulfill key functions of government. This includes highly visible tasks such as administration (unemployment agencies and city offices), construction (schools, hospitals, kindergartens), and infrastructure (roads, electricity, water), to name a few.

The size of transfers from the state level is thus highly relevant and directly affects citizens. And, the size of the transfers is (partly) determined on the basis of census population estimates.

Table 1: Discontinuity in 2011 German Census

	Small municipalities ($\leq$ *10,000 inhabitants*)	**Large municipalities** ($>$ *10,000 inhabitants*)
1987 Census	Survey (control)	Survey (control)
2011 Census	Registry (treated)	Survey* (control)

Notes: The Table summarizes the empirical design by showing the different estimation procedures used above and below 10,000 inhabitants in the 1987 and 2011 census, respectively. NB: the survey-based data collection method in 2011 used a stratified population survey to correct errors in the registry data (Christensen et al., 2015). The cutoff (10,000 inhabitants) was determined on the basis of registry counts from the year 2009.

Importantly, while German censuses have historically used the same methodological strategy across the country, the 2011 census created a consequential discontinuity by employing two distinct methods. The change in methods and our empirical design is provided in Table 1. Until 2011, communities below and above 10,000 inhabitants were subject to the same population estimation procedure. Specifically, the 1987 census (the last full count) conducted surveys in all German municipalities. The estimates were periodically updated using registry counts.[3] In 2011, however, the census bureau decided to use a different estimation procedure in communities below 10,000 inhabitants. Communities above 10,000 inhabitants, by contrast, were administered using traditional surveys, which were used to clean the registry data (Christensen et al., 2015). For this reason, we conceptualize communities below 10,000 inhabitants as "treated," given that their population was estimated using a new and unanticipated method. Taken together, Table 1 shows that there are two key comparisons one can draw, which we make use of in the empirical section. First, one can compare communities in 2011 below and above

[3]In Germany, citizens are required to register with their local municipality when they change their place of residence. Municipalities collect this information in population registers.

the threshold (discontinuity design). Second, one can compare communities across time, given that 1987—and the years after—can serve as a baseline where the same estimation methods were used.

3.2 Sample: German municipalities

The overall population consists of all 11,301 German municipalities. In the empirical analysis, we focus on the sample of municipalities that lie around the cutoff of 10,000 inhabitants in order to afford a regression discontinuity coupled with a difference-in-differences design. As stated above, Germany has 16 federal states. One state, Rhineland-Palatinate, does not base the calculation of transfers to its municipalities on census figures. We therefore exclude the state and its municipalities from the analysis and use them as a placebo test in the Robustness section. A second state, Baden-Württemberg, decided to ease the burden of the 2011 census by applying it as late as 2016. In the main sample, we therefore also exclude Baden-Württemberg and its municipalities from the sample, but show that results are robust to its inclusion. Finally, three federal states in Germany are so-called city states (Berlin, Hamburg and Bremen). These states consist of only one municipality, which lies far beyond the 10,000 inhabitant cutoff and are therefore not in the sample. In sum, the sample includes municipalities from 11 federal states and covers most areas of Germany An overview of the implementation of the census across states is given in Table A1.

3.3 Data

To explore whether government spending affects citizens' vote choice, we rely on four sources of data. All variables are summarized in Table A2.

First, we collected municipality population figures as well as census population estimates from the German Statistical Office (see Münnich et al., 2016). This data forms the basis of the empirical analysis, given that we must first confirm that the 2011 census did, indeed, lead to an exogenous population increase in the recorded population in some municipalities, but not others. For our purpose, we collected population figures for the year 2009 (register) as well as 2011 (census). We use the 2009 figures as they, in turn, formed the basis for the cutoff used by the census bureau.

Second, we collected data on municipal finances, namely, transfers from state governments, debt and spending. The data come from the Bertelsman Foundation (Bertelsmann, 2015) and span the years 2010 to 2016. Transfers from the state to local municipalities are, i.a., formed on the basis of the income, corporate, capital gains and value-added tax. Some transfers are earmarked. We focus on non-earmarked transfers that local municipalities can spend at their own discretion.[4] We normalize all municipal finance variables by dividing the total amount in each municipality by the pre-census population.

Third, we collected data on voting behavior. Germany is a federal system in which elections take place at the federal, state and municipal[5] level. We collected data at all three levels before and after the 2011 German census went into effect, which, as stated, ranged from 2013 to 2016 (see Table A1). Theoretically, there is no clear reason to prefer one level as the main outcome. Fiscal transfers are distributed by the federal government, while state governments decide on the precise allocation to municipalities. Finally, municipal governments spend the resulting budget windfalls. We therefore report results across all three elections in the empirical section.

To capture pro-spending voting behavior, we must classify German parties into a pro-spending and an anti-spending camp. Such a classification will undoubtedly involve a degree of arbitrariness. To tackle this problem, we use three different classifications. First, we aggregate the German political system into three groups: a progressive (i.e., pro-

[4] There is no way to centrally access municipal governments' books. We are therefore not in a position to assess what kinds of projects were funded. However, as we detail in Section 4.2, we obtained data on public tenders in the year 2019 to provide descriptive statistics what projects municipal governments typically invest in.

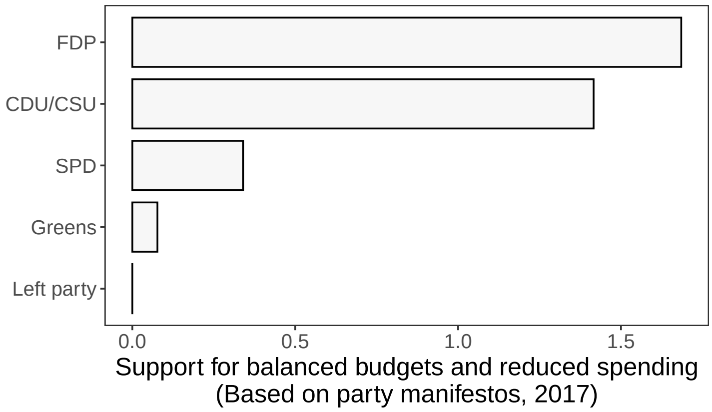
[5] At the municipal level, there are both mayoral as well as council elections. We focus on the latter as the former is not consistently reported across municipalities. Specifically, mayoral elections are only centrally reported in two states, which would reduce the sample size significantly and not allow us to implement our highly local design.

spending), a center and a conservative camp (i.e., anti-spending). Pro-spending parties comprise the Greens (*Bündnis 90/Die Grünen*) and the Left Party (*Die Linke*). This set captures parties that clearly advocate for increased spending and a more active role of the state. For instance, the two parties are outspoken critics of Germany's debt brake (Potrafke et al., 2016). Center parties comprise the Social Democrats (*Sozialdemokratische Partei Deutschlands*; SPD) and the Christian Democratic Union (*Christlich Demokratische Union*; CDU). This set captures two parties that are neither explicitly pro-spending nor against it and jointly introduced the debt brake in 2009 (Simone et al., 2018). Finally, anti-spending parties comprise the Free Democrats (*Freie Demokratische Partei*; FDP) and the Alternative for Germany (*Alternative für Deutschland*; AfD). The latter set clearly advocates for the reduction of deficits and spending more broadly. Second and alternatively, we dichotomize the German political system into a progressive (i.e., pro-spending) and a conservative camp (i.e., anti-spending). Here, left-leaning parties comprise the SPD, the Greens and the Left party, while right-leaning parties comprise the CDU, the FDP, and the AfD. Third, we also report results across all parties because any party arguably has a different "taste for spending." Here, the spending spectrum arguably runs as follows: FDP → AfD → CDU → SPD → Green → Left. In the empirical section, we prioritize the first classification, but results are robust across all three.

To verify that the proposed classification accurately captures preferences for government spending, we rely on data from party manifestos. In doing so, we utilize the classification scheme proposed by the Comparative Manifesto Project (CMP, Volkens et al., 2020). Using human coders, the CMP scores party manifestos according to several pre-defined dimensions. To capture parties' spending preferences, we make use of the CMP's *Economic Orthodoxy* dimension, which measures calls for "reduction of budget deficits", "retrenchment in crisis" as well as "thrift and savings in the face of economic hardship." In Figure 1, we rank German parties according to their economic orthodoxy score and show that the ranking corroborates our proposed classification.

Fourth, we collected background variables on municipalities' social and economic context. We use these variables to assess balance around the 10,000 inhabitant cutoff prior

Figure 1: Pro-spending preferences of German parties



Notes: The Figure ranks Germany's major parties according to Economic Orthodoxy, based on 2017 party manifestos (Volkens et al., 2020). The dimension captures support for balanced budgets and decreased government spending. NB: The sample does not include the AfD.

to treatment (Table A3; more below) as well as to improve the precision of the empirical models by adding the variables as controls. We obtained the data from two sources, the 2011 census and the German Statistical Office. From the census, we obtained information on the number of foreign born residents per capita, unemployment rates, population density as well as the share of population older than 65 years.[6] In addition, we obtained background information on household size and the share of households with married couples. Finally, we collected variables on the share of owner-occupied residences as well as the share of newer residences and residences larger than 100 m^2. From the German Statistical Office, we obtained data on GDP per capita and land values, which are measured at the county level.

3.4 Empirical strategy

In order to estimate the effect of the new 2011 census data collection method on the key outcomes of interest—government spending and citizens' vote choice—we couple a regression discontinuity with a difference-in-differences design. We model the outcome as a function of the pre-census population—a binary variable that indicates which side of the

[6]While the 2011 census population figures were subject to errors, there is no indication that this is also the case for information on the foreign-born population or unemployment rates.

cutoff a given municipality is on. To account for potential pre-treatment differences, we operationalize the main outcome as the change in voting behavior for pro- / anti-spending parties from 2013 to 2017.[7]

When estimating the effect of the census on citizens' vote choice, we follow the framework outlined by Calonico et al. (2014). Using a local polynomial of order $p = 1$ and a triangular kernel, we fit two local linear regressions above and below the 10,000 inhabitant threshold for all observations within the MSE-optimal bandwidth h_{MSE}. We regress the outcomes on a constant and $(X_i - 10,000)$, where X_i is the pre-census population. From the two regressions, we obtain intercepts $\hat{\mu}_+$ (for municipalities above the cutoff) and $\hat{\mu}_-$ (for municipalities below the cutoff). The sharp RD point estimate is then defined as the difference in the intercepts:

$$\hat{\tau}_{\text{SRD}} = \hat{\mu}_- - \hat{\mu}_+$$

In line with Calonico et al. (2014) we report results based on the MSE-optimal bandwidth h_{MSE}. We use a separate bandwidth b_{MSE} to construct robust-bias corrected confidence intervals. The respective optimal bandwidths for the different outcomes are shown in Table A4. Further details on the estimation procedure are provided in Section A.2.

When estimating the effect of the census on municipal finances (transfers, debt and spending), we again compare communities above and below the cutoff, but use a two-way fixed effects panel estimator. This allows us to make full use of the temporal dimension of the data given that we observe the outcomes in every year (not just in 2013 and 2017, as is the case for the voting outcomes). The difference between the panel and RD estimator lies in the fact that the former does not differentially weight observations near the cutoff (cf. Calonico et al., 2014). Our estimating equation for municipal finance outcomes is as follows:

$$Y_{ijt} = \alpha_{ij} + \gamma_t + \tau \left[\mathbb{1}(\text{Population}_{ij}^{2009} < 10000)\mathbb{1}(t \geq 0)\right] + \varepsilon_{ijt}$$

[7] Alternatively, our strategy can be considered to be a two-period difference-in-difference design in which we explicitly model the effect of the pre-census population and include weights that depend on distance from the cutoff.

Here, Y_{ijt} is a municipal finance outcome (transfers, debt or spending) measured in year t for municipality i in state j. Municipalities are considered treated if their pre-census population is below 10,000 inhabitants ($\text{Population}_{ij}^{2009} < 10000$) and if the state j as already started using the census population figures ($t \geq 0$). We transform the data such that $t = 0$ is the first period in which the census population figures were applied (see Table A1 for an overview, this varies by state). We include municipality and time fixed effects, and cluster standard errors by municipality. To approximate the RD design used for the voting outcomes, we estimate the model for a number of bandwidths that limit the sample to municipalities close to the 10,000-inhabitant cutoff.

3.5 Assumptions

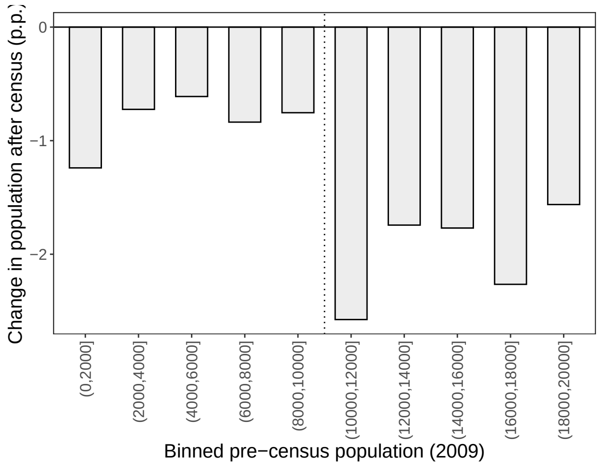
For the RD design to be credible, we require that (1) municipalities are not able to sort into treatment, (2) potential outcomes are continuous around the cutoff, and (3) there are no rivaling treatments that coincided with the 2011 census. To save space, we discuss all three assumptions at length in Section A.1. First, we show statistically that there is no sorting around the cutoff and we argue that sorting was impossible given that municipalities could not mistake their population count. Second, we confirm pre-Census balance across treated and control municipalities. Third, we rule out alternative treatments in the year 2013 at the 10,000 inhabitant cutoff.

4 Results

4.1 Effect on population figures

We begin by scrutinizing the first stage, that is, the effect of the census on 2011 population figures. In Figure 2A, we plot the population change reported in the census, conditional on pre-census municipality population (see also, Christensen et al. (2015)). The pre-census population is based on register counts dated to December 31, 2009, which we show on the x-axis and which formed the basis for the 2011 census cutoff. On the y-axis, we display the relative change in population. The Figure shows that the average population change is negative, i.e., most municipalities lost residents. Importantly, however, the reported decrease in population is disproportionally smaller for treated municipalities—i.e., those exposed to the new registry estimation method.

Figure 2: Effect of 2011 census on population change



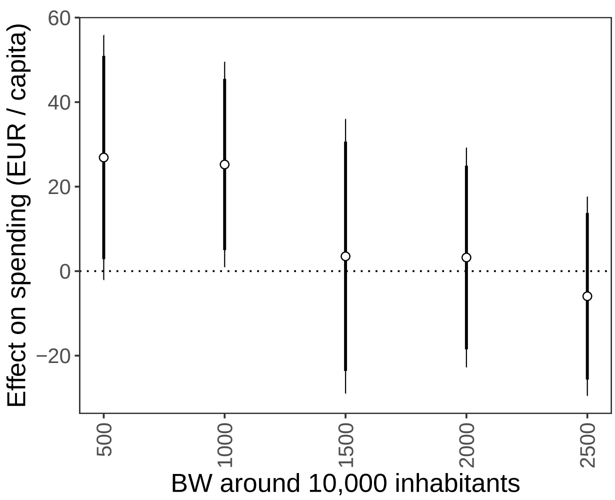
Notes: The Figure shows average population changes in the 2011 census conditional on municipality population in 2009 in percentage points across the indicated bins. Negative values on the y-axis indicate a population decrease.

We quantify the jump at the 10,000 inhabitant threshold in Figure A.2, using a simple differences in means analysis to estimate the effect of the new census method on population changes. We find that treated municipalities experience population declines that are about 1.2 to 1.8 percentage points smaller than municipalities that were not exposed to the new estimation method. In sum, treated municipalities gained a sizeable and unanticipated number of recorded residents relative to what was to be expected on the basis of the survey-based estimation method. Put differently, elected officials in treated communities should have seen the population decline in line with the control group. The new method, however, meant that treated municipalities received additional inhabitants.

4.2 Effect on government spending

Did the unanticipated increase in population size lead to an increase in government spending in treated municipalities? As stated, the size of municipality budgets is a function of transfers from the state, which are partly calculated on the basis of population figures. To quantify the increase in transfers, Figure A.3 reports coefficients and 95 percent confidence intervals from the benchmark panel specification using transfers as the outcome. The Figure confirms a noticeable effect of the positive population shock (induced by the new 2011 census method) on per-capita transfers of roughly 10 Euros per year per person. As Table A2 shows, this translates into an increase in transfers by 6 percent—a sizeable improvement of municipal budgets. Importantly, the estimates are similar across all indicated RD bandwidths (though the precision of the estimates varies as a function of the sample size).

Figure 3: Effect of 2011 census on government spending



Notes: The Figure shows point estimates and 90 / 95 percent confidence intervals from five panel models around the indicated bandwidths, regressing municipal per-capita spending on the treatment indicator. We consider the difference in spending between the last pre-treatment year and the first year for which census estimates were used to allocate transfers.

Importantly, a rise in transfers does not necessarily translate into increased spending.

It could be the case, for example, that municipalities used the windfall to pay off debt. This is not the case, however. As much is shown in Figure 3, where we use municipal spending as the outcome variable. The Figure demonstrates that treated municipalities around the cutoff engaged in a significant increase in per-capita spending of roughly 25 Euros. Interestingly, the estimate is thus slightly greater than the aforementioned 10 Euros in increases in transfers. The estimate is particularly pronounced at the cutoff, underlining the necessity for the RD design. In Figure A.20 we also confirm that municipalities did not use the windfall to pay off debt. If anything, they engaged in more borrowing (though estimates are imprecise). The evidence thus underlines that the census increased spending noticeably.

What did local governments spend the windfall on? The books of municipal governments are not open to the public, unfortunately. To still characterize the public investments, we scraped all current municipal tenders, which are centrally advertised by the German government on the website `www.service.bund.de`. Details on the scraping procedure and the data are provided in A.3.[8] The data allows us to describe which capital projects municipalities commonly spend their budgets on. Table A7 demonstrates that municipal tenders most frequently contain references to the construction, renovation and upkeep of public schools, day cares, public gyms and pools. Municipal governments thus invested the budgets in highly visible public goods. As a result, a large share of the local population, directly or indirectly, benefitted from the increase in spending. Finally, in Table A6 we furthermore show that municipalities which received more transfers and spent more money were also, on average, more likely to list public tenders.

4.3 Effect on citizens' vote choice

Did the increase in government spending affect citizens' vote choice? Table 2 presents the results of the benchmark RD model. The outcome is citizens' vote choice in federal, state and municipal elections. As can be seen, the exogenous shock to government spending

[8]NB: We cannot construct a panel of municipal tenders for the entire period of study since data on past tenders only goes back to 2017.

increased the vote share of left-leaning parties significantly. Compared to municipalities that did not experience a rise in spending, the change in federal voting behavior from 2013 to 2017 (Column 1) was about 1.2 percentage points greater in municipalities that witnessed more government spending.[9] Interestingly, Figure A.5 shows that the gains for left-leaning parties predominantly stem from centrist parties. Right-wing parties, by contrast, see no substantively meaningful changes.

Table 2: Effect of spending on citizens' vote choice

	DV: ΔLeft-wing vote share (percentage points)		
	Federal election	State election	Municipal election
Census shock	1.22**	2.15*	2.40**
	(0.61)	(1.34)	(1.14)
Controls	Yes	Yes	Yes
Opt. bandwidth (h)	2,548	2,890	2,496
N	596	659	540

Note: The Table shows the results of the benchmark RD model, regressing the difference in left-wing vote shares (percentage points) between the elections before and after the census was applied on the treatment indicator. We present results for federal, state and municipal elections. We estimate separate optimal bandwidth, depending on the outcome. Standard errors are shown in parentheses. More details on the models are given in Table A4 in the SI. See Section 3.4 for more details on the RD estimation. ***p < .01; **p < .05; *p < .1

Did the increase in government spending also affect citizens' vote choice in elections in state and municipal elections? Table 2 shows that effect sizes are, if anything, larger in lower-level elections. In both state and municipal elections, left-leaning parties gain roughly 2.5 percentage points as a result of the increase in government spending. While the differences between the federal and state / municipal estimates are not themselves statistically significant, the larger effect size in more local elections is theoretically plausible given that the money was spent by municipal governments and provided by the state.

[9]Note that the results for voting in federal elections derives from optimal bandwidths between 2,200 and 2,700 inhabitants. These bandwidths are larger than the ones in which we observe strong effects on spending (see Figure 3). Reassuringly, however, Figure A.8 shows that the voting effects are most pronounced for municipalities between 9,000 and 11,000 inhabitants. This confirms that the main result is driven by municipalities where spending increased the most. We elaborate on this further in Section A.5.

4.4 Mediation

Before turning to robustness tests and mechanisms, we must address one important quibble with our empirical design. While the effect of the census on vote choice is causally identified, we cannot know with certainty whether the effect is due to increased government spending. In essence, our design takes the form of a mediation analysis, invoking sequential ignorability. We believe, however, that this assumption is rather plausible in our setting. The 2011 census was a technocratic happenstance. The average citizen does not follow the statistical procedures of the census bureau. What is more, the impact of the census on population estimates was not a highly politicized occurrence. To bolster this argument, in Section A.4 we use newspaper data to show that the difference between the two estimation methods received only moderate media attention and only right after the census was released (in 2013). Leading up to the 2017 election—one of our key outcomes—the new method and its consequences was barely discussed in the media. It is thus no stretch to assume that the effect of the census on voting behavior operates through a spending channel.

One way to quantitatively buttress this assumption is to implement a mediation analysis. In doing so, we rely on the framework proposed by Imai et al. (2011), which allows us to decompose the total effect of the 2011 census into the average direct effect (ADE) and the average causal mediation effect (ACME), where changes in local government spending are the mediator. Table 3 presents the results of this procedure. We find evidence that the effects of the 2011 census were, indeed, mediated by increased government spending, as both the ACME and the proportion of the total effect that was mediated are positive and significantly different from zero. We must emphasize, however, that mediation analyses require strong assumptions (the mediator must be independent of the potential outcomes given treatment assignment and pre-treatment covariates).

Table 3: Mediation analysis

Quantity	**Estimate**	**CI**	$h_{\mathbf{MSE}}$	n
Total effect (p.p.)	1.086	[0.374, 1.968]	2,288	475
ADE (p.p.)	0.955	[0.237, 1.861]	2,288	475
ACME (p.p.)	0.131	[0.019, 0.34]	2,288	475
Proportion mediated (0–1)	0.114	[0.021, 0.432]	2,288	475

Note: The Table reports results from a causal mediation analysis (See Section 4.4 for details). The mediator is the change in local government spending in Euro/capita between 2011 and 2016. The outcome is the change in the left-wing vote share between the 2013 and 2017 federal elections. All quantities are measured in percentage points, except for the proportion mediated, which is measured on a scale from 0 to 1. The mediation models include the same pre-treatment covariates as the main model.

4.5 Robustness

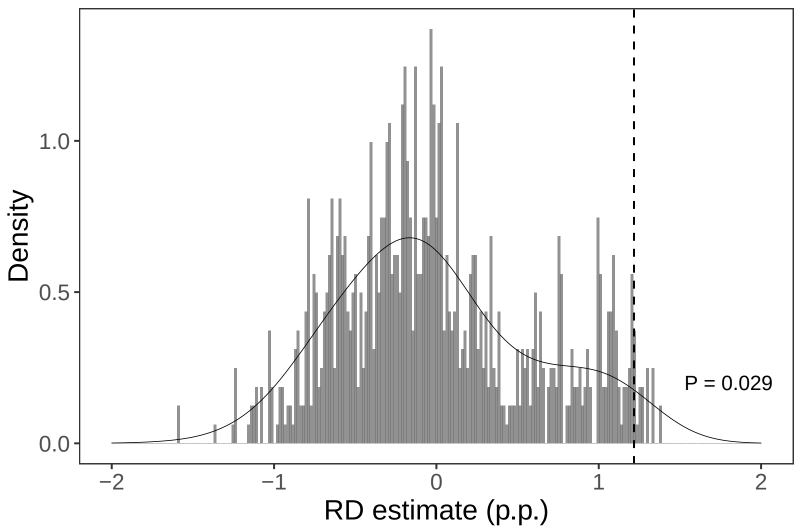
Before turning to mechanisms, we briefly present seven tests to underline the robustness of the main effect on citizens' vote choice.

First, all headline results are robust to different classifications of the electoral system. Notably, the results hold when simply dividing the party space into left- and right-leaning. As Figure A.10 shows, left-leaning parties (here, including the center-left SPD) gain roughly 1.3 percentage points as a result of the spending increase. Moreover, Figure A.11 shows results for all parties separately. By far the largest coefficient is found for the Left Party. This is reassuring inasmuch as the party—being the successor of the East German Socialist Unity Party—is the most unequivocal measure of pro-spending voting behavior. What is more, the party is consistently in favor of increased redistribution, not just any increase in government spending—a finding we revisit below.

Second, the results are also robust to splitting Germany in an East and West sample. As Figure A.16 shows, we observe that the estimates in West Germany mirror the aggregate results. Point estimates in the East German sample are closer to zero. However, the smaller sample leads to relatively noisy estimates, as reflected by the larger standard errors.

Third, to address the concern that RD designs crucially depend on the chosen bandwidth, in Figure A.13 we estimate the benchmark model across a number of different bandwidths. The Figure shows that the treatment effect is remarkably stable across bandwidths ranging from 1,000 up to 5,000 inhabitants. The estimate is largest and most precisely estimated right at the cutoff (1,000 inhabitants), where the design is arguably most credible and where the strongest effects on spending are found (see Footnote 9 and Figure 3). Still, the effect is statistically significant and substantively sizeable even when including the largest bandwidth of 5,000 inhabitants. This builds trust that the benchmark estimate is not the result of a particular bandwidth. The results also hold when including the state of Baden-Württemberg, which applied the census population figures just before the 2017 election.

Figure 4: Randomization inference using placebo cutoffs



Notes: The Figure plots the distribution of 1,000 sharp RD point estimates (using the benchmark RD model) for 1,000 randomly chosen cutoffs in the interval from 5,000 to 15,000 inhabitants. The outcome is the difference in vote shares for left-leaning parties between the 2013 and 2017 federal elections. The vertical dashed line is the sharp RD point estimate based on the actual cutoff of 10,000 inhabitants as reported in Figure A.5. The two-tailed p-value is 0.029

Fourth, we create a permutation test by drawing 1,000 randomly chosen pseudo-RD cutoffs between 5,000 and 15,000 inhabitants, i.e., cutoffs that do not reflect any treatment. We then re-estimate the benchmark RD model around each pseudo treatment. Figure 4 displays the distribution of the treatment effects across the 1,000 specifications. Reassuringly, the distribution is squarely centered around zero. The actual observed treatment effect—estimated around the true census cutoff—stands in sharp contrast to this distribution. Our treatment estimate is more extreme than 97 percent of all pseudo-estimates, implying a two-tailed p-value of 0.029. Put differently, we had a less than 3 percent chance to observe a treatment effect of this magnitude by chance.

Fifth, we make use of the fact that one state, Rhineland-Palatinate, does not use the census to determine the size of transfers to municipalities. We therefore should *not* see a treatment effect in this state. Reassuringly, Figure A.18 shows that the estimated effect of the census treatment on left-wing party vote shares is zero. The evidence thus builds trust that the treatment effect we observe is due to the census, rather than due to unrelated trends that affect all states.

Sixth, to address the concern that municipalities select into treatment (which, as stated, seems highly implausible in our setting), we make use of the so-called donut RD procedure (Eggers et al., 2015). Specifically, we exclude municipalities that lie very close to the cutoff. The results, presented in Figure A.14, show that this procedure virtually does not change the main finding. The effect is detectable across a variety of donut-specifications, leaving 25 to 150 municipalities out of the sample. Accounting for sorting therefore does not change the substantive conclusions from above. The evidence also further assuages concerns about a potentially compounded treatment. Were other treatments taking place at the 10,000 cutoff (which we ruled out above), the potentially associated sorting can be addressed using the donut RD procedure.

Seventh, we use the period from 2009 to 2013 as an additional placebo test. The census went into effect after 2013. There should thus be *no* difference in citizens' vote choice in municipalities below and above 10,000 inhabitants from 2009 to 2013. We confirm this conjecture in Figure A.19. We find no significant effects of the census treatment on any of the voting behavior outcomes. The evidence thus showcases that the municipalities on either side of the cutoff are not on different trends prior to the treatment. Unfortunately, we cannot, however, repeat this procedure for elections going further back in time as German municipalities are often redrawn and there is no straightforward mapping from 2005 municipalities to 2013 / 2017.

5 Mechanisms

We have shown that increased public spending benefits progressive parties in federal, state and municipal elections. A natural follow-up question concerns the causal channel that brings about this effect. We consider two interrelated mechanisms in turn.

5.1 Persuasion vs. mobilization

In a first step, we scrutinize whether the change in vote choice is a result of mobilization or persuasion. Put differently, we ask whether increased spending simply changes who goes to vote or whether it changes who people vote for. To answer this question, we present two pieces of evidence.

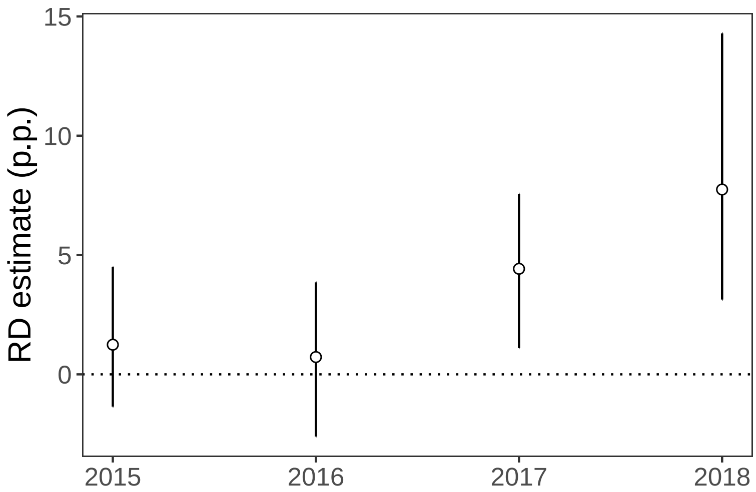
Panel evidence. A first strategy to tease apart the mobilization and persuasion channels is to rely on panel evidence before and after the unanticipated spending increase took effect. The idea is that we observe one person before and after the spending increase in local communities, thus creating a within-subjects design. Germany has a long-operating panel survey called the Socioeconomic Panel (SOEP). Fortunate for our case: the panel includes over 25,000 representatively sampled residents who are surveyed once a year. Within the subset of municipalities around the 10,000 inhabitant cutoff, the panel still includes roughly 1,200 individuals, which affords a solid degree of support to draw inferences. One question in the panel captures respondents' preferred party. Specifically, the item reads: "*Many people in Germany lean toward a specific party over longer periods of time, despite sometimes voting for a different party. How about you: Do you lean toward a specific party in Germany?*"

Figure 5 confirms the headline finding from above.[10]. Individuals in treated municipalities are significantly more likely to lean toward left-leaning parties in the years after local governments increased spending on public goods. The individual-level estimate is

[10]See Section A.2.1 in the SI for more detailed discussion of the estimation strategy used for the SOEP data

roughly 5 percentage points in 2017—the year when the federal election took place. Given that the evidence is at the individual-level (we take the difference within individuals), the finding points toward a persuasion mechanism: Increased public spending led respondents to switch toward progressive parties. Interestingly, the effect is only visible 1-2 years after the budget increases took place, a sensible finding inasmuch as spending takes time to hit the ground. The evidence also shows that the individual-level estimate is larger than the aggregate estimate. This suggests that potential mobilization mechanisms, if anything, work in the opposite direction.

Figure 5: Effect of 2011 census on identification with left-leaning parties (panel evidence)



Notes: The Figure shows point estimates and 95 percent confidence intervals from four RD models using the benchmark specification. The outcome is the difference in likelihood of stating that SOEP respondents tend toward left-wing parties between 2013 and the year given on the x-axis (in percent).

Turnout. A second, less compelling way to tease the persuasion and mobilization channels apart is to look at turnout. If the increase in municipal budgets also increases turnout, it is arguably less convincing to infer a persuasion mechanism. After all, changes in turnout typically have differential effects across the political spectrum (Gomez et al., 2007). If there is no effect on turnout, it suggests that similar people turned out to vote. Changes in vote choice are thus likely due to a persuasion mechanism. We must caution, however, that a constant level of turnout does not imply that the same people vote. There may thus be differential mobilization across parties, which we would not pick up in aggregate turnout results—highlighting the importance of the panel evidence. Still and reassuringly so, Table A4 in the SI shows that the increase in spending had no effect on turnout. The evidence thus complements the panel-survey finding that the increase in spending likely shifted individual preferences.

5.2 Why does spending benefit pro-spending parties?

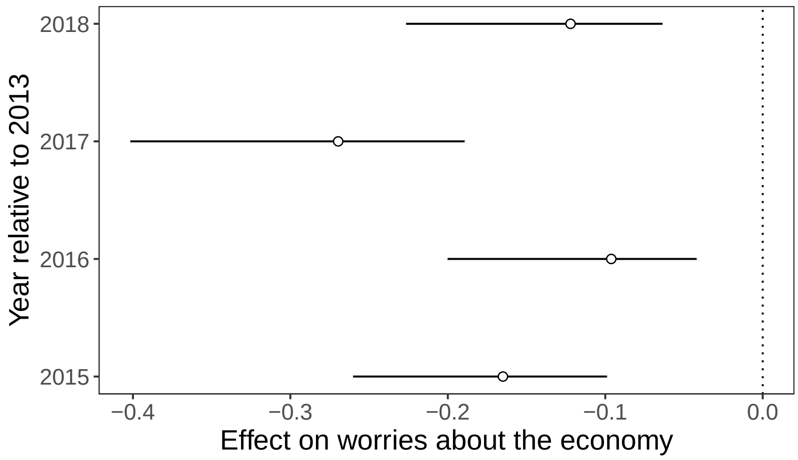
Having made the case that public spending shifts voters' preferences to left-leaning parties, we now turn to the theoretically most interesting question: Why is this the case? Put differently, what individual-level mechanism can account for the fact that increased spending leads citizens to switch to left-leaning parties? Above, we have pointed to two channels. First, public spending may act as a signal of economic growth, which makes redistributive parties a more viable option. Second, increased spending may beget demand for additional spending as citizens get used to the public goods being provided.

While it is difficult to tease both mechanisms apart, we collected survey evidence for both channels. To capture the "spending as economic signal"-channel, we rely on evidence from the aforementioned panel survey. Specifically, the SOEP asks individuals to what degree they are worried about their economic situation. To capture the "spending increases demand for investment"-channel, we gained access to a survey item from the public opinion firm *CIVEY*. The firm conducts large opt-in online surveys based on river sampling, which allowed us to obtain a sufficient sample size around the cutoff. The item read "What should the government do with budget surpluses?" One answer choice was "invest," which we use as an indicator that respondents demand more public investment. One drawback of the item is that it is a cross-sectional measure taken in 2019 / 20, six years after the treatment.

We present results in Figures 6 and 7. The survey evidence points toward the first channel. Figure 6 shows that treated individuals became *less* worried about the economy. Arguably, individuals interpreted increased spending as a signal that the economy is doing well, which makes redistributive parties an attractive option. By contrast, we find no evidence that treated respondents demand more investments. As much is shown in Figure 7. Taken together, the survey evidence thus supports the first channel: Increased spending seems to function as a signal that the economy is on an upward trajectory, which makes it economically feasible to vote for parties on redistributive platforms. By contrast, it does not seem to be the case that spending drives demand for more investments—as would be suggested by the literature on policy feedback loops.

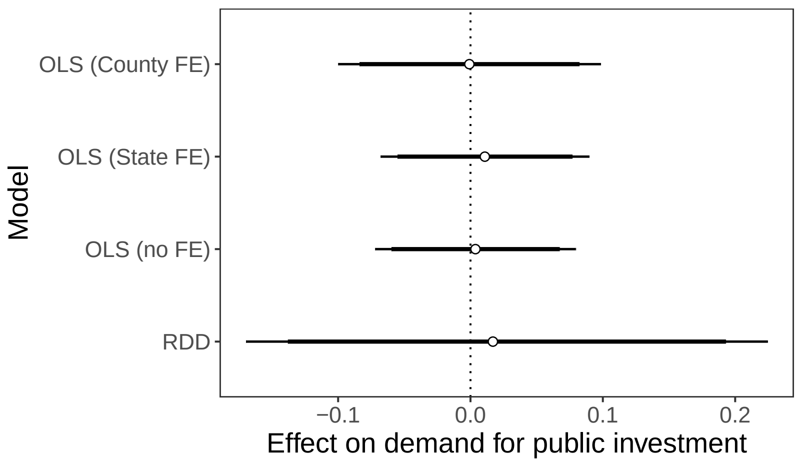
Two additional results support our main proposed mechanism, namely, that public goods spending is interpreted by voters as a signal of economic well-being, paving the way for increased redistribution, not additional investments. First, as stated above, we observe the strongest treatment effect for the Left party. Across the German party space, the Left party is most stringently in favor of redistribution and places less of a focus on public investment. The latter is most vocally endorsed by more centrist parties, especially the Social Democrats. Second, in Figure A.22 we show that the increase in public goods spending did *not* improve incomes. Were this the case, the canonical Meltzer and Richard model would predict a *decrease* in redistribution because the median voter would move closer toward the mean. Taken together, the evidence thus implies that public goods spending changes voters' perceptions about the scope with which society can support the poor.

Figure 6: Effect of 2011 census on worries about the economy



Notes: The Figure shows point estimates and confidence intervals from four RD models. The outcome is the within-person difference in how much respondents worry about the situation of the economy between the years given on the y-axis and 2013. The data source is the German Socio-Economic Panel.

Figure 7: Effect of 2011 census on demand for public investment



Notes: The Figure shows point estimates and 90 / 95 percent confidence intervals from four regression models. The outcome is a survey item (provided by the survey firm CIVEY) indicating that respondents believe government surpluses should be invested. All models are cross-sectional since the data was collected in 2019/2020 (after the treatment occurred). We estimate three OLS models for all municipalities within 5,000 inhabitants of the 10,000 inhabitant cutoff. The models include either state fixed effects, county fixed effects or no fixed effects. In addition, we estimate a cross-sectional version of the benchmark RD specification. All models are based on the sample where Baden-Württemberg is excluded.

6 Alternative explanations

Before concluding, we briefly rule out two alternative explanations for our main finding.

6.1 Incumbency

One may be concerned that the effect of government spending on vote choice is simply an incumbency effect. Four pieces of evidence rule out this channel. First, given our RD design, the share of left- and right-leaning incumbents should be similarly distributed around the cutoff. Our design therefore holds incumbency constant—at least at the local level. Second, to still corroborate that there is no incumbency effect, we coded which parties were in power in the respective states as well as at the federal level[11] and then created an incumbency variable. Figure A.7 shows that there is *no* positive effect on incumbents, both in state and in federal elections. Third and related, at the federal level the government at the time was comprised of the centrist SPD and CDU. As Figure A.5 shows, however, the two parties did not gain due to the census. Last, as we outline above, we confirmed the changes in vote choice using individual-level panel evidence (see Figure 5). This analysis thus further rules out that the observed effect is driven by voters rewarding incumbents for increased spending.

6.2 Envy

A second concern is that voters in control communities may have reacted to the increase in government spending in treated communities with envy. Transfers across communities are a zero-sum game. Put differently, the gain in transfers in treated communities partly came at the expense of control communities. It may therefore be the case that control communities simply punished left-leaning parties in order to reduce overall spending. Four pieces of evidence rule out this channel. First, in Figure A.6 we show that control communities did *not* punish left-leaning parties. Indeed, from 2013 to 2017 the vote share

[11]Note that we do not have data on the affiliation of mayors; more details above.

of left-leaning parties remained constant in control communities, while it rose in treated communities. Second, Figure A.4 shows that transfers to all municipalities—treated or control—rose from 2012 to 2016. Control communities thus also saw increases in spending, which were in line with budgetary expectations. Third, if control communities wanted to punish treated communities for the increases in spending, they could only realistically do so by affecting state and federal policy-making, the levels from which municipalities derive their budgets. The fact that we also see changes in municipal elections thus makes it implausible that control communities punished the left to reduce the spending of treatment communities. Finally, if anything, the gains of left-leaning parties stem from centrist parties (see Figure A.5). Changes on the political right are few across all levels of election.

7 Conclusion

This paper has assessed whether government spending on public goods affects citizens' vote choice. Our empirical setting was Germany, where the 2011 census created exogenous variation in municipal spending. Analyzing voting behavior in federal, state and municipal elections, we found that the unanticipated spending shock increased the vote share of left-leaning parties by 1 to 2 percentage points. Evidence from a panel suggests that the effect is due to persuasion, not differential mobilization. In order to explain the causal channel, we relied on survey evidence and showed that citizens in treated municipalities expressed more positive views about the economy. The finding suggests that government spending can function as a signal that the economy is on an upward trajectory, which makes redistributive parties a more viable option.

Two pivotal questions were beyond the scope of our paper and may be fruitful avenues for future research. First, we have made the case that increased public goods spending leads voters to view the economy favorably, which makes redistribution feasible. We did not find any feedback effect whereby voters demand additional public investments. One crucial unanswered question is whether voters would react similarly if governments increased spending on transfers. Arguably, such an increase is neither readily observable by voters, nor does it send a positive signal about the economy. One might therefore expect voters to stay put when governments increase transfers or to, in fact, move toward conservative parties—assuming the median voter's preferred level of redistribution is already implemented. Alternative mechanisms are imaginable, however. We therefore invite future research to assess the effects of alternative forms of government spending on voting behavior.

A second unresolved question concerns the precise manner in which politicians invest windfalls (Berry et al., 2010). Unfortunately, we were not in a position to gain access to the books of municipal governments, which remain closed to the public. We were thus unable to explore what kinds of investments municipalities made—save a descriptive analysis of advertised tenders. While such decisions are undoubtedly endogenous, they do offer a rich opportunity to better understand what kind of investments voters value. Do highly visible projects yield the greatest payoff? Or are projects targeted toward swing-voters most likely to yield electoral rewards? These questions demonstrate the need to better understand the ways in which governments invest resources in public goods and how voters react to this. There will, undoubtedly, also be differences across parties (Bickers and Stein, 2000), which marks a final avenue for future research.

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A Supplementary Information (Online Only)

Contents

Table A1: Census implementation by state

State	**Applies census**	**Year census applied**
Schleswig-Holstein	Yes	2014
Lower Saxony	Yes	2014
North Rhine-Westphalia	Yes	2014
Hesse	Yes	2014
Rhineland-Palatinate	No	–
Baden-Württemberg	Yes	2016
Bavaria	Yes	2014
Saarland	Yes	2014
Brandenburg	Yes	2013
Mecklenburg-Vorpommern	Yes	2013
Saxony	Yes	2013
Saxony-Anhalt	Yes	2014
Thuringia	Yes	2014

Notes: The Table contains information on when federal states first adopted the 2011 census population estimates as the basis for transfers to municipalities. Note that we exclude city states (Berlin, Hamburg, and Bremen) who are well outside the RD sample.

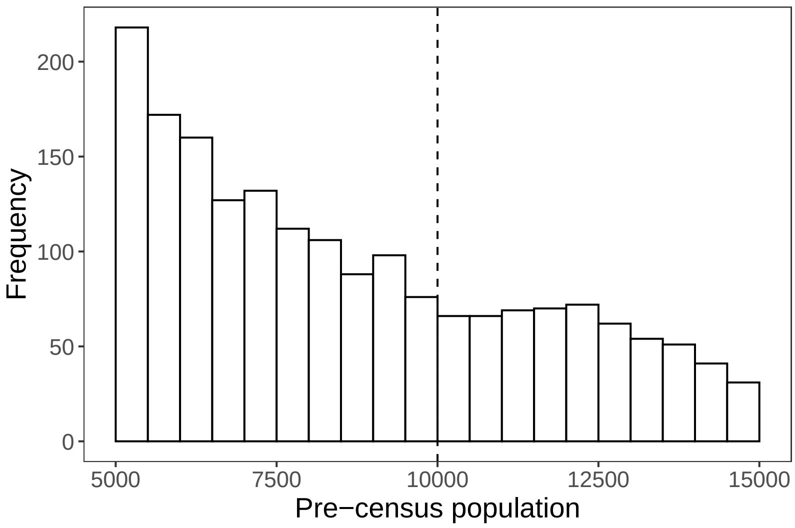
A.1 RD Assumptions

For the RD design to be credible, we require that (1) municipalities are not able to sort into treatment, (2) potential outcomes are continuous around the cutoff, and (3) there are no rivaling treatments that coincided with the 2011 census. We discuss all three[12] assumptions in turn.

A.1.1 Sorting

Are there discontinuous changes in the density of the running variable in the neighborhood around the cutoff? Sorting could happen if municipalities somehow anticipated the census discontinuity, and tried to maximize future transfers by (fraudulently) changing their registry population figures such that communities remained just shy of 10,000 inhabitants. The fact that the census employed two different methods was known prior to data collection.[13] Importantly, however, public officials cannot change inhabitant figures at will. What is more, it is unlikely that local officials were able to predict the census discontinuity as well as its eventual consequences. Still, to confirm that there are no discontinuous jumps, Figure A.1 presents the density of the running variable for municipalities between 5,000 and 15,000 pre-census inhabitants. Reassuringly, there are no discontinuities in the number of municipalities as the pre-census population crosses the cutoff. To formally test this assumption, we use a local polynomial density estimator and obtain an insignificant p-value of 0.8. We thus have no evidence of sorting. Had municipal officials somehow been able to predict the beneficial consequences of the census discontinuity, one would expect a larger share of municipalities just below the 10,000-inhabitant cutoff. This is not the case.

Figure A.1: Density of the running variable



Notes: The Figure displays a histogram of the running variable, i.e., the pre-census population figures in 2009 based on registry data. The dashed vertical line represents the cutoff, which determined the census data collection method. Each bin corresponds to a 500-inhabitant range.

[12] A fourth assumption is that the forcing variable has positive density around the cutoff, which Figure A.1 confirms.

[13] It is unclear whether the change in methods was known three years in advance, which would have been necessary to give communities sufficient time to (fraudulently) change the 2009 registry figures, which formed the basis of the 2011 census.

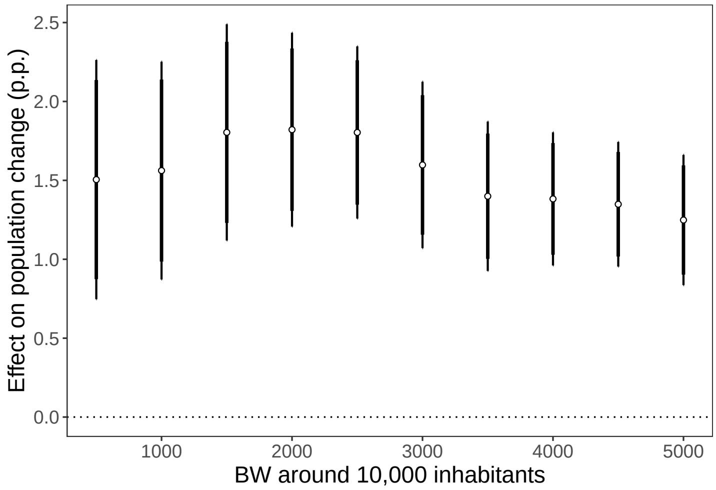
A.1.2 Balance

Are potential outcomes isomorphic around the cutoff? To corroborate that assignment to treatment is "as-if random," we evaluate covariate balance for all available pre-census variables across the treatment and control group. Table A3 shows that there is excellent balance, including for variables such as migration, employment, population density, turnout, land value and GDP. The only outcome with noticeable differences is the result of left / right-leaning parties in the 2009 election. Importantly, however, this bias works against the empirical finding we present below. That is, it is a well-known fact that smaller communities, in Germany, tend to be more conservative, which Table A3 confirms. The headline finding below, however, shows that the census had an effect in the opposite direction: it *raised* the vote share of left-leaning parties in smaller communities. The main estimate below is thus likely a lower bound. In any case, the imbalance underlines the necessity to account for pre-treatment voting behavior. As stated, we achieve this by first-differencing the electoral outcomes and by controlling for all variables provided in Table A3.

A.1.3 Compounded treatments

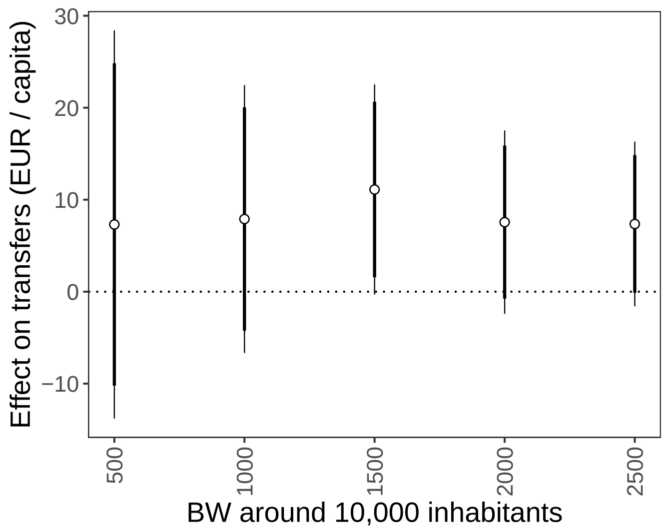
A final important concern in RD designs is that the treatment may be compounded. That is, the 10,000 inhabitant cutoff may not only have led to a discontinuity in the 2011 census population estimates, but may also coincide with other treatments, which are applied at this cutoff. Importantly, we are not aware of any other treatment that was applied at the 10,000 cutoff in the year 2011. It is worth repeating that the alternative treatment would need to apply not just at the same threshold, but also at the same time, which is rather unlikely. What is more, German municipalities between 5,000 and 20,000 inhabitants fall into the same administrative category (*Kleinstadt*; small town; Haas n.d.). Significant political reforms thus affect municipalities in the same manner above and below the cutoff. Moreover, the design not only uses the cutoff to make inferences, but also controls for pre-treatment imbalances (difference-in-differences). If treated municipalities differed systematically from control communities, this would show up in pre-treatment imbalances, which we have tried to rule out above and which we account for by i) first-differencing the outcome variable (in the case of electoral outcomes), and ii) by estimating a two-way fixed effects panel model (in the case of municipal finances). Finally, as we elaborate on below, we also test whether there are any differences in voting behavior between the federal elections of 2009 and 2013 (Figure A.19)—when *no* census treatment took place—and show this is not the case. The evidence thus further bolsters that no other treatments, other than the 2011 census, were applied at the 10,000 inhabitant threshold.

Figure A.2: Effect of 2011 census on population change rate



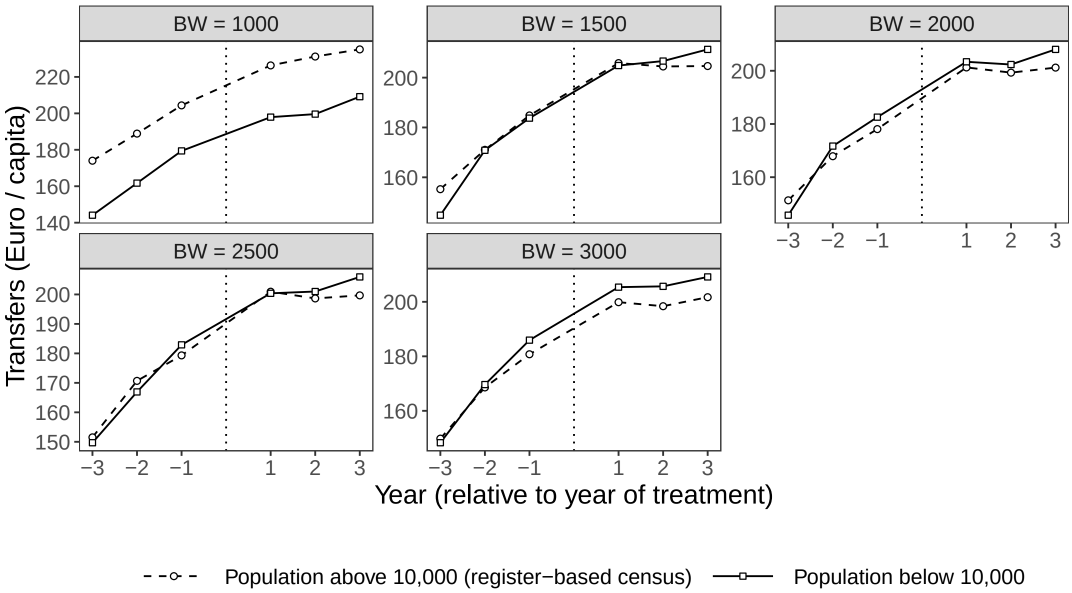
Notes: The Figure shows differences in means in the rate of population change, comparing pre- and post-census population figures for treated and control municipalities. We present estimates and 90 / 95 percent CIs. We present estimates for different bandwidths bandwidths around the 10,000 inhabitant cutoff, as indicated in the x-axis. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants.

Figure A.3: Effect of 2011 census on per-capita transfers



Notes: The Figure shows coefficients and 90 / 95 percent confidence intervals of panel regressions of per-capita transfers on the treatment dummy restricting the sample to the indicated bandwidths around the 10,000 inhabitant cutoff (x-axis). We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants.

Figure A.4: Trends in per-capita transfers to municipalities



Notes: The Figure shows average per-capita transfers in Euros from the state to municipalities exposed to the new census method (treated; dashed line) relative to those exposed to the survey-based method (not treated; solid line). We show trends for five different bandwidths. Census figures were first used to allocate transfers either in 2013 or 2014. Therefore, the x-axis indicates the year relative to the first year that the census population figures was used, which is labeled '1'.

Table A2: Summary statistics

Variable	Mean	Median	SD	Min	Max
Population					
Population in 2009	8778.74	8249.00	2797.83	5001.00	14990.00
Post-census population (2011)	8714.23	8248.00	2806.75	4833.00	23568.00
Pre-treatment covariates					
Foreign born / 1000 capita (2011)	39.60	31.02	33.91	0.35	327.80
Age 65+ / capita (2011)	0.21	0.21	0.04	0.10	0.36
Employment / 1000 capita (2011)	353.41	364.63	70.75	0.00	487.40
Unemployment / capita (2011)	0.03	0.02	0.02	0.01	0.10
Households: prop. married couples (2011)	0.53	0.53	0.06	0.31	0.69
Households: prop. 2+ members (2011)	0.70	0.71	0.05	0.48	0.84
Residences: prop. owner-occupied (2011)	0.56	0.58	0.11	0.19	0.81
Residences: prop. 100+ sqm area (2011)	0.49	0.51	0.14	0.12	0.81
Residences: prop. built 2000 or later (2011)	0.11	0.10	0.05	0.02	0.37
Population density / km2 (2011)	251.20	167.00	274.12	20.00	2422.00
Out-migration / 1000 capita (2011)	55.35	52.27	23.92	27.39	604.77
Land value (county-level, 2012)	76.00	52.02	86.28	6.26	618.87
GDP / capita (county-level, 2012)	27021.79	25308.00	10183.62	16446.00	98581.00
Municipal finances					
Municipal spending (2012, EUR/capita)	1254.24	1179.00	414.97	668.00	7624.00
Municipal spending (2014, EUR/capita)	1378.07	1309.26	474.13	0.00	11985.73
Municipal spending (2016, EUR/capita)	1513.22	1443.03	490.61	816.41	9694.43
Transfers to municipalities (2012, EUR/capita)	161.36	161.50	111.88	0.00	688.00
Transfers to municipalities (2014, EUR/capita)	194.93	196.09	136.53	0.00	973.17
Transfers to municipalities (2016, EUR/capita)	203.52	203.01	145.75	0.00	799.44
Municipal debt (2012, EUR/capita)	948.37	713.00	916.37	0.00	7597.00
Municipal debt (2014, EUR/capita)	950.74	739.12	896.30	0.00	7282.56
Municipal debt (2016, EUR/capita)	952.66	721.13	889.18	0.00	8237.59
Electoral outcomes (%)					
Turnout (2009)	68.80	70.66	8.07	42.33	84.62
Right party vote share (2009)	51.78	51.81	9.96	25.81	81.59
Left party vote share (2009)	41.88	42.52	10.58	14.61	67.73
Turnout (2013)	69.74	70.66	6.72	43.23	82.73
Right party vote share (2013)	50.18	49.54	8.69	27.26	77.24
Left party vote share (2013)	38.33	39.02	9.45	13.94	63.43
Turnout (2017)	75.23	76.70	6.87	46.35	87.59
Right party vote share (2017)	46.06	46.29	8.14	27.87	74.45
Left party vote share (2017)	34.26	34.25	7.76	16.04	57.74

Notes: the Table contains summary statistics for all variables that we use. We consider the subset of municipalities between 5,000 and 15,000 inhabitants, equaling the largest bandwidth shown in Figure A.13. For the municipal finance outcomes, we only show the indicated years, rather than all years from 2010–2016. In addition, all municipal finance outcomes use the 2009 (pre-census) population as the denominator.

Table A3: Pre-treatment balance

Outcome	$\hat{\tau}_{\mathbf{SRD}}$	**CI**	**P-val**	$h_{\mathbf{MSE}}$	$b_{\mathbf{MSE}}$	n
Age 65+ / capita (2011)	-0.214	[-0.58, 0.064]	0.116	3213	5358	818
Employment / capita (2011)	0.019	[-0.133, 0.167]	0.822	1898	3207	458
Unemployment / capita (2011)	-0.037	[-0.427, 0.284]	0.694	3033	4785	755
Foreign born / capita (2011)	0.563	[0.096, 1.237]	0.022	3070	4863	771
Households: prop. married couples (2011)	0.057	[-0.276, 0.358]	0.800	2536	3865	629
Households: prop. 2+ members (2011)	0.107	[-0.244, 0.392]	0.650	2651	4123	653
Residences: prop. owner-occupied (2011)	-0.057	[-0.218, 0.091]	0.418	1875	2814	465
Residences: prop. 100+ sqm area (2011)	-0.089	[-0.364, 0.136]	0.372	1990	3057	488
Residences: prop. built 2000 or later (2011)	-0.026	[-0.32, 0.253]	0.817	5128	7533	1444
Population density / km2 (2011)	0.013	[-0.387, 0.486]	0.825	3923	5905	1016
Out-migration / capita (2011)	-0.021	[-0.229, 0.158]	0.720	3685	6197	919
Right party vote share (2009)	0.394	[0.091, 0.826]	0.015	2356	4169	582
Left party vote share (2009)	-0.402	[-0.854, -0.085]	0.017	2170	3841	534
Turnout (2009)	-0.059	[-0.401, 0.255]	0.662	3719	5755	976
Land value (county-level, 2012)	0.244	[-0.248, 0.848]	0.283	3750	5843	965
GDP / capita (county-level, 2012)	0.251	[-0.206, 0.806]	0.245	3382	5515	854

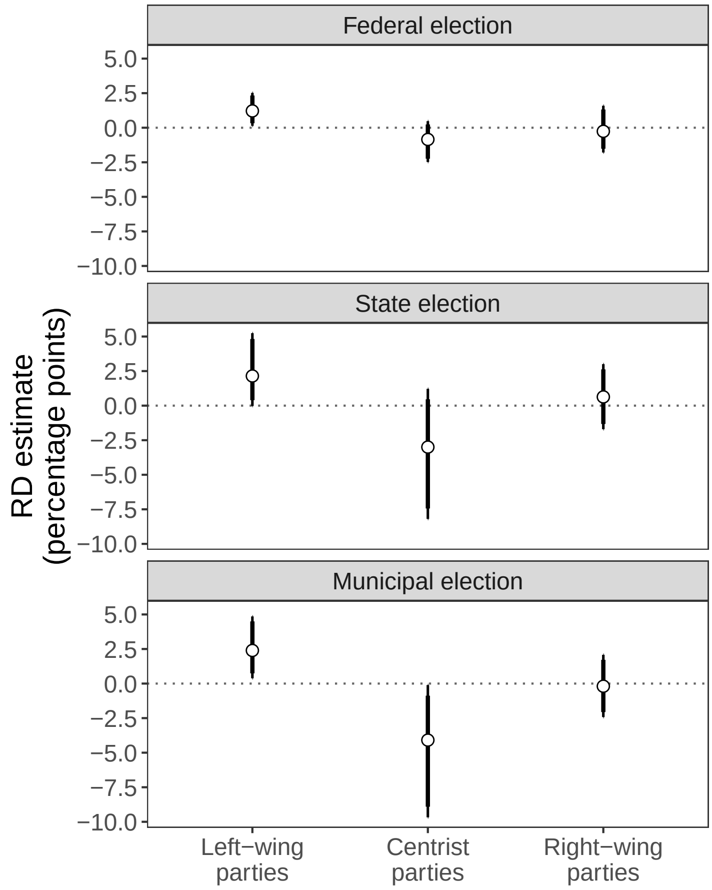
Notes: The Table shows coefficients, CI and p-values of the benchmark RD regression of the indicated pre-treatment covariates on the treatment indicator. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants.

Table A4: Main RDD results

Outcome	**Sample**	$\hat{\tau}_{\mathbf{SRD}}$	**95% CI**	**P-val**	**P-val (RI)**	$h_{\mathbf{MSE}}$	$b_{\mathbf{MSE}}$	n
Turnout	B-W excluded	-0.32	[-1.45, 0.84]	0.60	0.52	3716	5574	919
Turnout	Full sample	0.05	[-0.88, 1.19]	0.77	0.91	2935	4450	899
Left-wing parties	B-W excluded	1.22	[0.13, 2.52]	0.03	0.03	2548	3878	596
Left-wing parties	Full sample	0.87	[-0.03, 2.04]	0.06	0.16	2676	4301	800
Centrist parties	B-W excluded	-0.85	[-2.48, 0.48]	0.18	0.06	2657	4046	623
Centrist parties	Full sample	-0.80	[-2.3, 0.32]	0.14	0.28	2404	3942	710
Right-wing parties	B-W excluded	-0.26	[-1.79, 1.59]	0.91	0.74	2498	3834	584
Right-wing parties	Full sample	-0.04	[-1.31, 1.63]	0.83	0.96	2213	3645	647

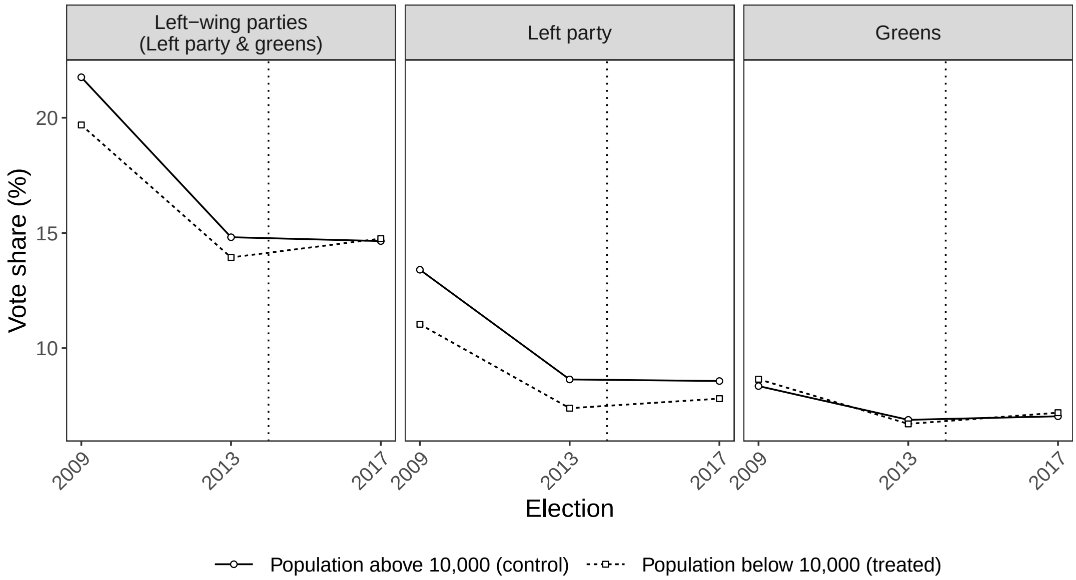
Note: The Table shows the key RD results and parameters for the indicated outcomes and samples, including the treatment effect, confidence interval, p-value, randomization inference p-value, as well as the bandwidths used to estimate treatment effects and confidence intervals. We consider a municipality to be treated if its pre-census population is below 10,000. The estimates for the sample that excludes Baden-Wuerttemberg are the ones shown in Figure A.5.

Figure A.5: Effect of spending on voting behavior



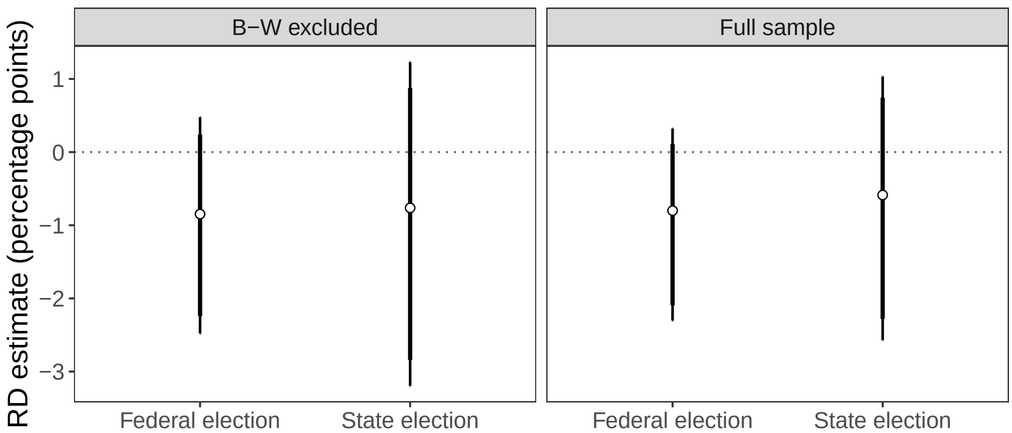
Notes: The Figure plots coefficients and robust bias-corrected 90 / 95 percent CIs of the benchmark RD model, regressing the difference in vote shares (percent) for the indicated political camps, respectively, between the last pre-census election and the first post-census election, on the treatment indicator. More details on the models are given in Table A4 in the SI. For more details on the estimation, see Section 3.4.

Figure A.6: Trends in voting behavior



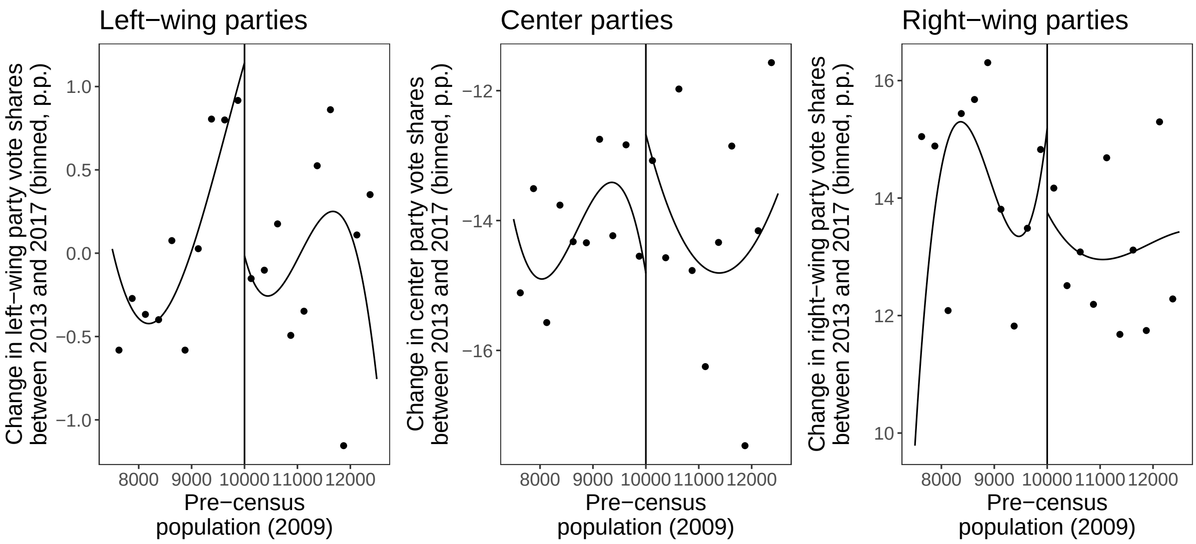
Notes: The Figure shows average vote shares over time, comparing treated (below 10,000 inhabitants, dashed line) to control (above 10,000 inhabitants, solid line) municipalities. We show trends for our main outcome, the combined vote share of the Left and Green parties, as well as for the two parties individually. We consider all municipalities within 1,000 inhabitants around the 10,000 inhabitant cutoff. Analogous to the main RD specification, we use weights from a triangular kernel, where municipalities further from the cutoff receive lower weights. The dashed vertical line marks the point in time when the census figures first affected transfers and spending.

Figure A.7: Effect of 2011 census on incumbent vote share in federal and state elections



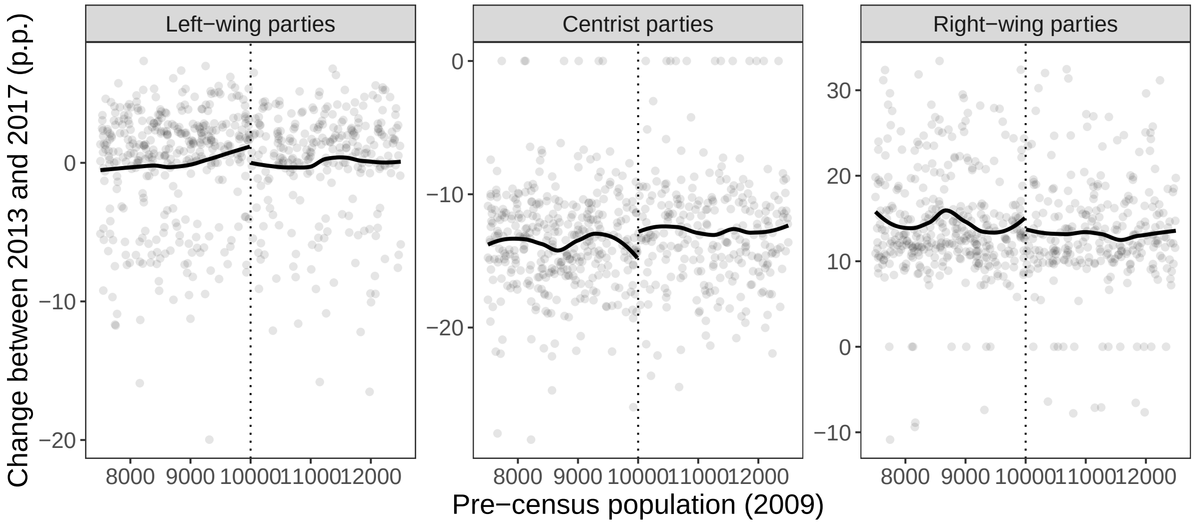
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RD model in which we regress the change in vote share for the pre-treatment incumbent on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. Note that state election dates differ between states. The right-hand side panel reports the full sample; the left-hand-side panel excludes the state of Baden-Württemberg.

Figure A.8: Regression discontinuity plots for the three main outcomes



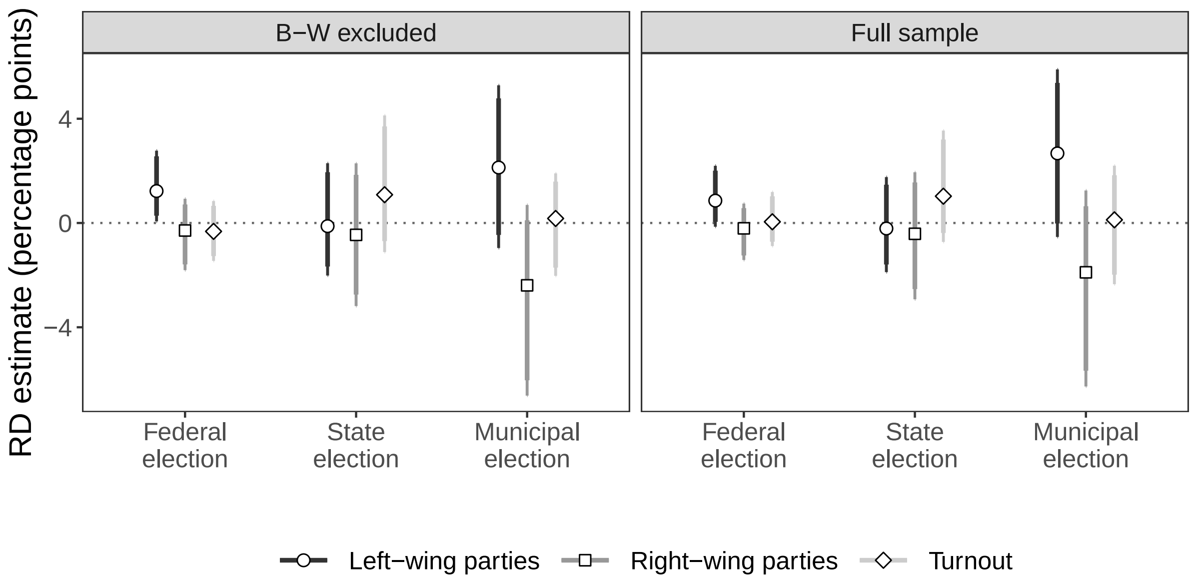
Notes: The figures show binned changes in vote shares and turnout between 2013 and 2017. We use a fourth-order polynomial to visually approximate the conditional means of vote share and turnout changes. Note that we consider a municipality to be treated if its pre-census population is below 10,000 inhabitants, i.e. the effect that we describe is the change in vote shares as we move from the right-hand side of the cutoff to the left-hand side.

Figure A.9: Regression discontinuity plots for the three main outcomes



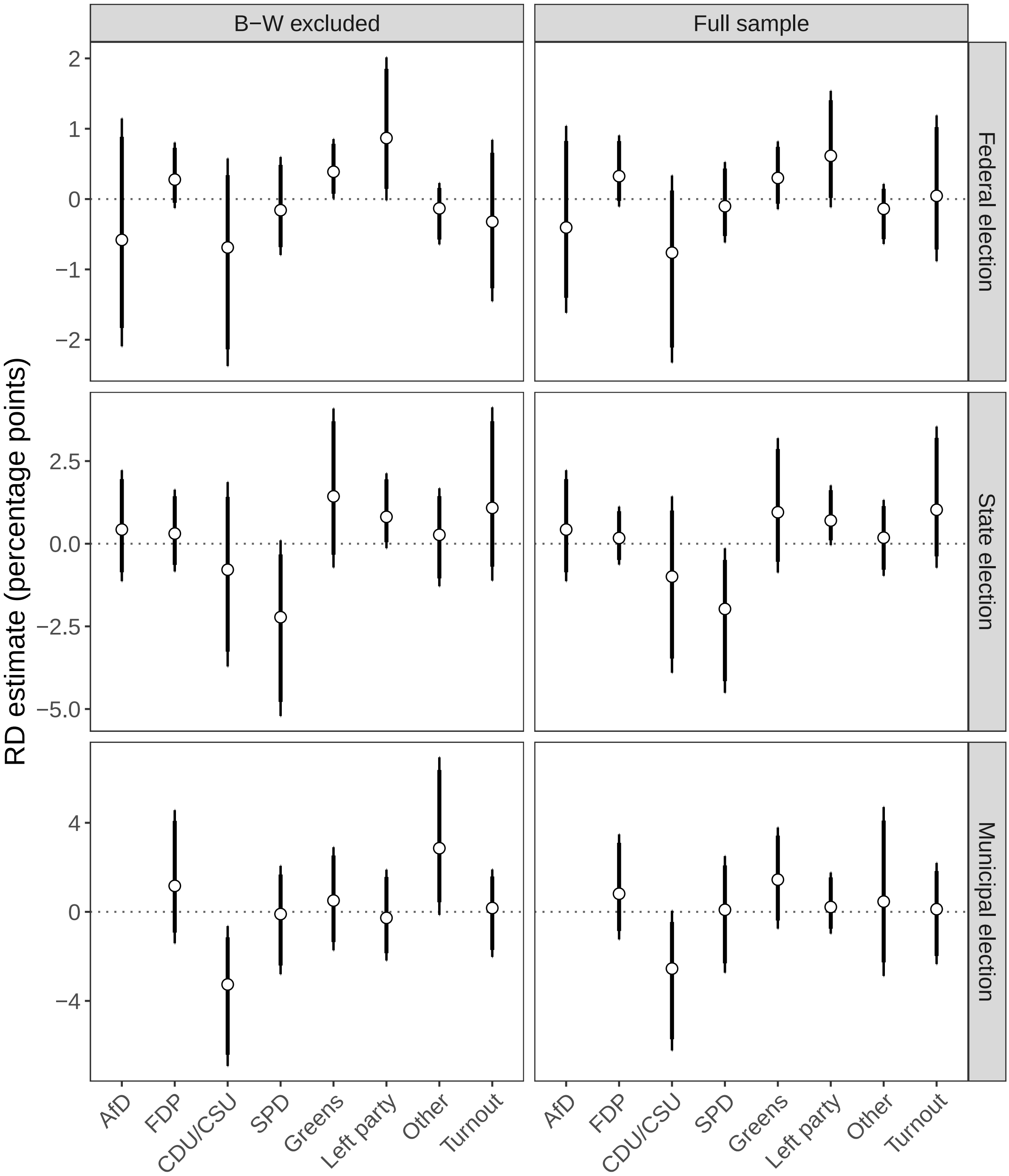
Notes: The figures show changes in vote shares and turnout between 2013 and 2017. We use a fourth-order polynomial to visually approximate the conditional means of vote share and turnout changes. Note that we consider a municipality to be treated if its pre-census population is below 10,000 inhabitants, i.e. the effect that we describe is the change in vote shares as we move from the right-hand side of the cutoff to the left-hand side.

Figure A.10: Effect of 2011 census on municipal and state election behavior, using left/right aggregation



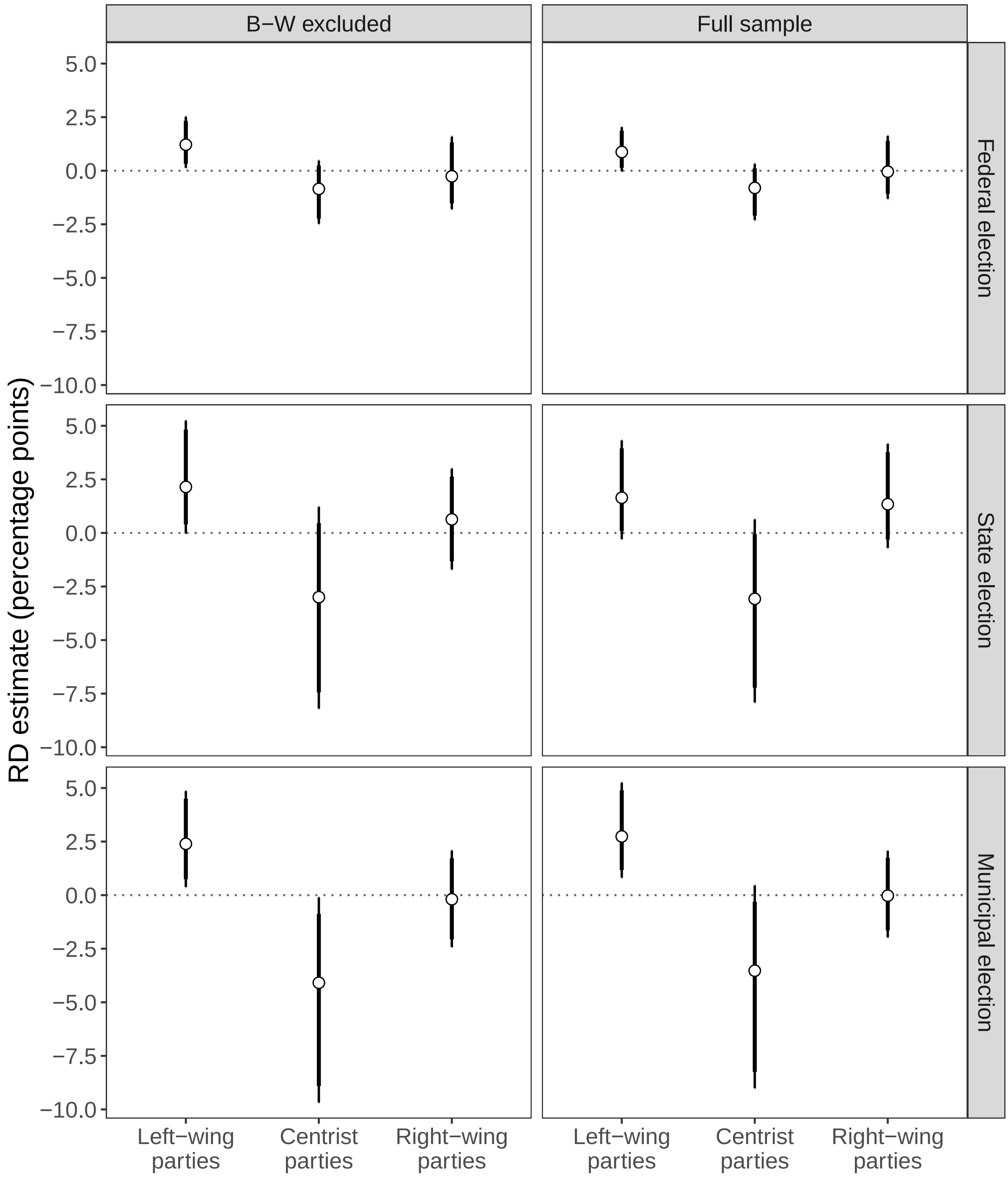
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RD model in which we regress the indicated electoral outcomes on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. Note that state and municipal election dates differ between states. The lower panel reports the full sample; the upper panel excludes the state of Baden-Württemberg.

Figure A.11: Effect of 2011 census on three elections, by party



Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RD model in which we regress the indicated electoral outcomes on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. Note that state and municipal election dates differ between states. The lower panel reports the full sample; the left-hand side panels exclude the state of Baden-Württemberg.

Figure A.12: Effect of 2011 census on three elections, left/right/center

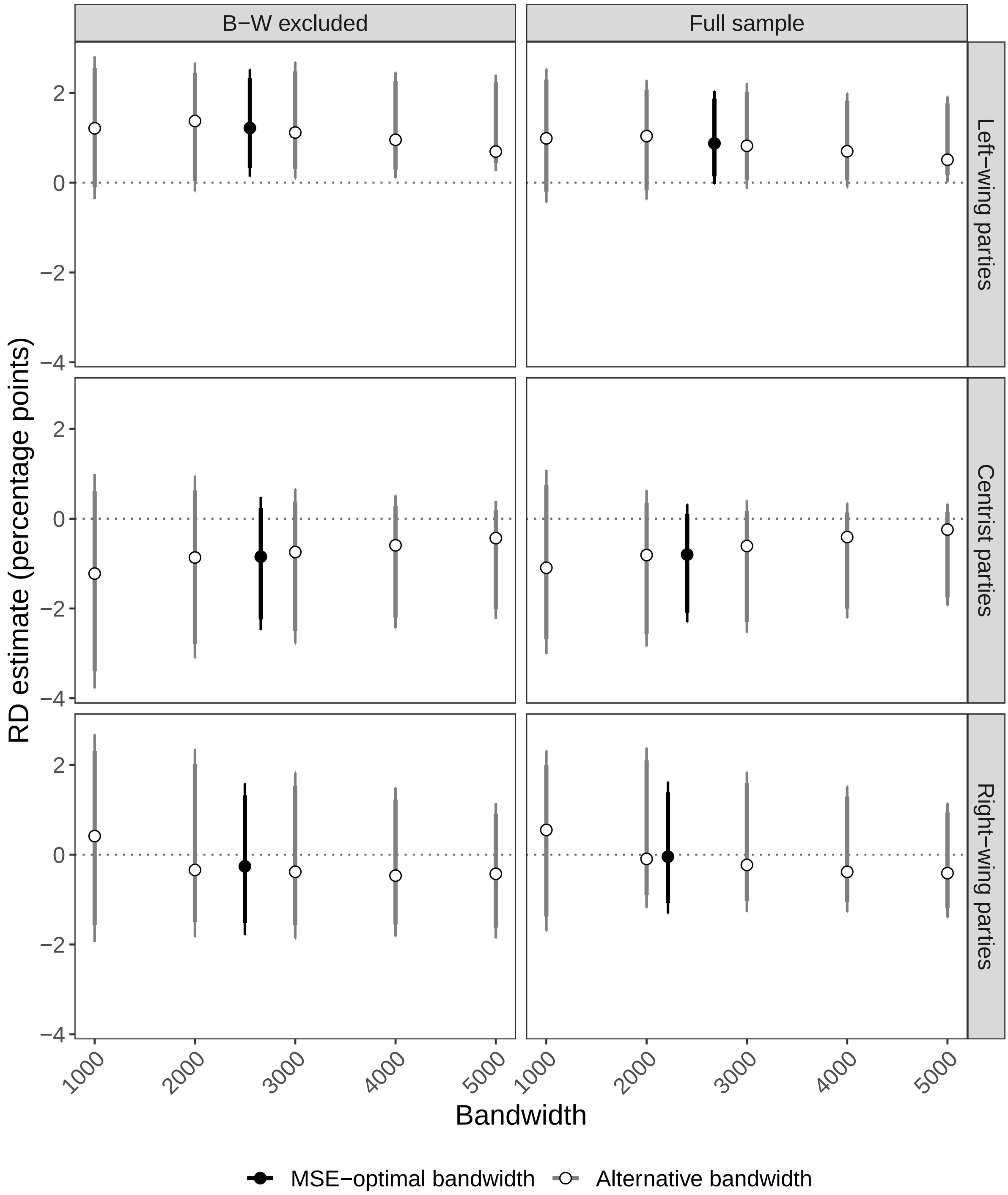


Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RD model in which we regress the indicated electoral outcomes on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. Note that state and municipal election dates differ between states. The lower panel reports the full sample; the left-hand side panels exclude the state of Baden-Württemberg.

Table A5: RDD Results for three elections, by party

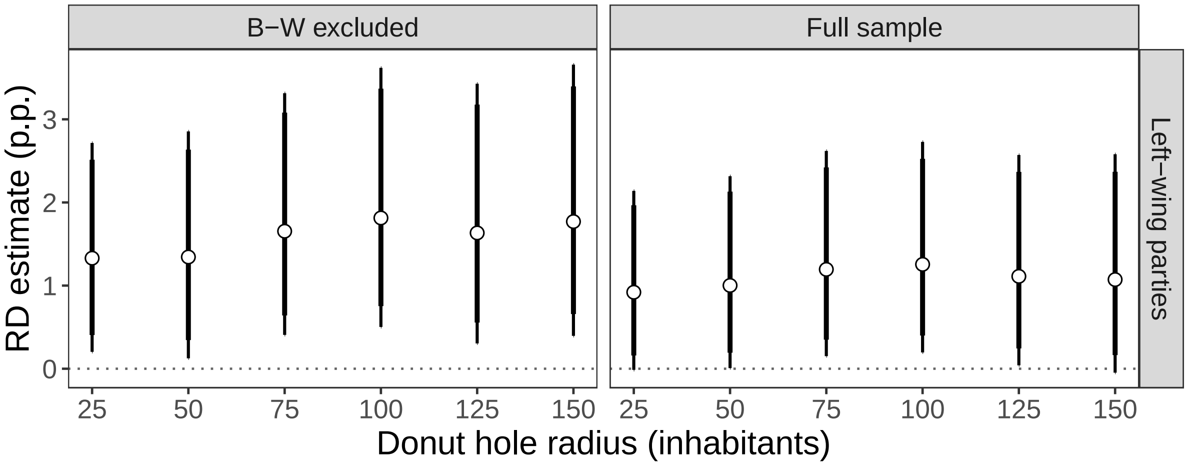
Outcome	**Sample**	$\hat{\tau}_{\mathbf{SRD}}$	**95% CI**	**P-val**	$h_{\mathbf{MSE}}$	$b_{\mathbf{MSE}}$	n
Federal elections							
AfD	B-W excluded	-0.58	[-2.09, 1.15]	0.57	2630	3931	612
AfD	Full sample	-0.40	[-1.62, 1.04]	0.67	2667	4052	795
FDP	B-W excluded	0.28	[-0.13, 0.8]	0.15	3418	6079	840
FDP	Full sample	0.33	[-0.11, 0.91]	0.12	2264	3885	666
CDU/CSU	B-W excluded	-0.69	[-2.37, 0.58]	0.23	2686	4347	632
CDU/CSU	Full sample	-0.76	[-2.33, 0.34]	0.14	2362	4153	699
SPD	B-W excluded	-0.16	[-0.8, 0.6]	0.78	2921	4509	700
SPD	Full sample	-0.10	[-0.62, 0.53]	0.88	3056	4836	937
Greens	B-W excluded	0.39	[0.01, 0.85]	0.05	3211	4974	773
Greens	Full sample	0.30	[-0.14, 0.82]	0.17	2956	4496	907
Left party	B-W excluded	0.87	[-0.02, 2.02]	0.05	2314	3659	536
Left party	Full sample	0.61	[-0.12, 1.54]	0.09	2414	3789	717
Other	B-W excluded	-0.13	[-0.65, 0.23]	0.35	2222	4038	517
Other	Full sample	-0.14	[-0.64, 0.22]	0.33	1981	3585	574
Turnout	B-W excluded	-0.32	[-1.45, 0.84]	0.60	3716	5574	919
Turnout	Full sample	0.05	[-0.88, 1.19]	0.77	2935	4450	899
State elections							
AfD	B-W excluded	0.43	[-1.13, 2.22]	0.52	4188	6644	276
AfD	Full sample	0.43	[-1.13, 2.22]	0.52	4188	6644	276
FDP	B-W excluded	0.31	[-0.84, 1.64]	0.53	3501	5322	818
FDP	Full sample	0.17	[-0.63, 1.12]	0.58	4599	7251	1458
CDU/CSU	B-W excluded	-0.79	[-3.71, 1.87]	0.52	3086	4570	711
CDU/CSU	Full sample	-0.99	[-3.9, 1.43]	0.36	2499	3916	724
SPD	B-W excluded	-2.22	[-5.21, 0.1]	0.06	2904	4567	663
SPD	Full sample	-1.97	[-4.51, -0.14]	0.04	2495	4291	719
Greens	B-W excluded	1.43	[-0.72, 4.09]	0.17	3001	4513	691
Greens	Full sample	0.95	[-0.87, 3.19]	0.26	2820	4343	820
Left party	B-W excluded	0.81	[-0.14, 2.13]	0.08	2440	4207	538
Left party	Full sample	0.70	[-0.04, 1.77]	0.06	2375	4353	675
Other	B-W excluded	0.27	[-1.29, 1.68]	0.80	3140	4785	723
Other	Full sample	0.18	[-0.97, 1.32]	0.76	3459	5309	1037
Turnout	B-W excluded	1.08	[-1.12, 4.13]	0.26	2228	3752	495
Turnout	Full sample	1.03	[-0.73, 3.55]	0.20	2189	3958	614
Municipal elections							
FDP	B-W excluded	1.17	[-1.41, 4.57]	0.30	2544	4342	280
FDP	Full sample	0.81	[-1.24, 3.48]	0.35	3068	5252	540
CDU/CSU	B-W excluded	-3.26	[-6.92, -0.64]	0.02	1377	2723	281
CDU/CSU	Full sample	-2.55	[-6.23, 0.05]	0.05	1500	2787	393
SPD	B-W excluded	-0.10	[-2.8, 2.07]	0.77	2152	3396	449
SPD	Full sample	0.09	[-2.73, 2.51]	0.93	2052	3140	552
Greens	B-W excluded	0.51	[-1.73, 2.9]	0.62	3141	4695	334
Greens	Full sample	1.45	[-0.75, 3.79]	0.19	3990	6134	705
Left party	B-W excluded	-0.27	[-2.19, 1.89]	0.89	2804	4402	259
Left party	Full sample	0.22	[-0.98, 1.77]	0.57	2443	4027	388
Other	B-W excluded	2.86	[-0.14, 6.94]	0.06	2229	4132	382
Other	Full sample	0.46	[-2.88, 4.71]	0.64	2713	4429	646
Turnout	B-W excluded	0.17	[-2.03, 1.9]	0.95	4099	6404	967
Turnout	Full sample	0.12	[-2.35, 2.2]	0.95	3568	5610	1050

Figure A.13: Main results (bandwidth sensitivity)



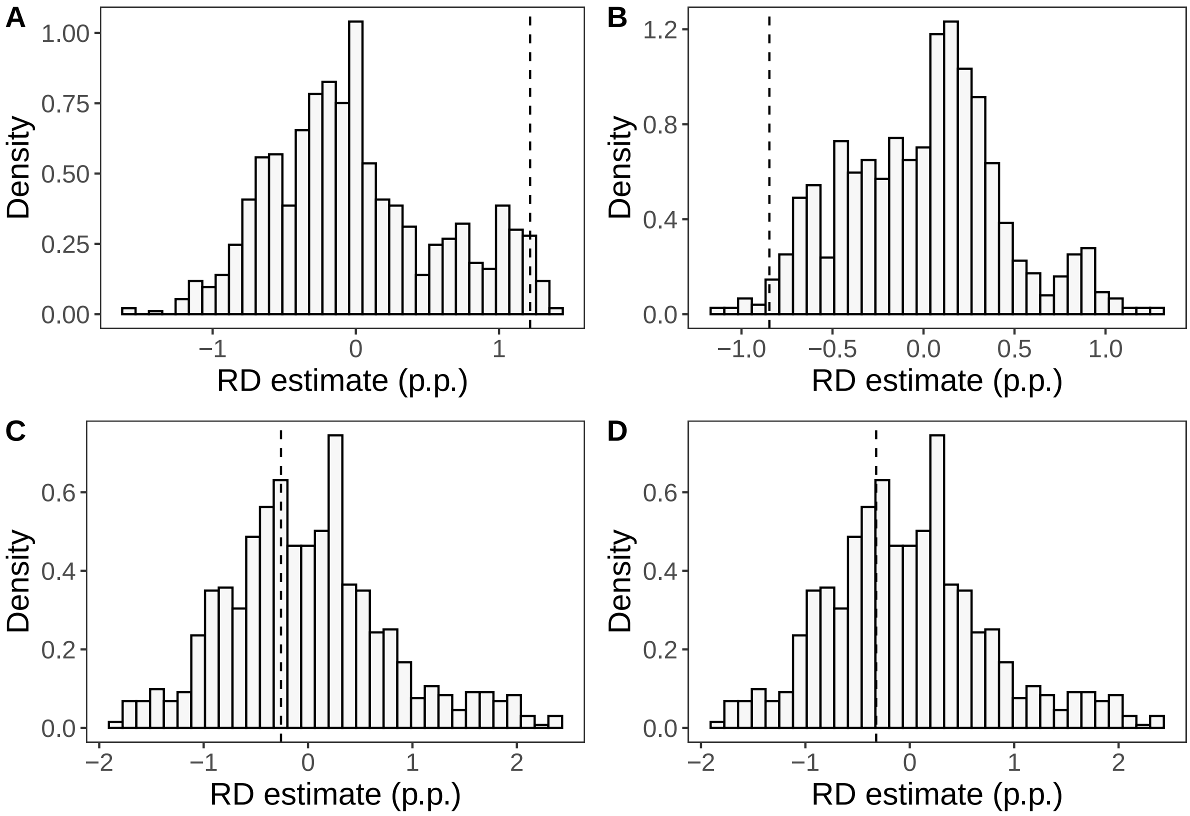
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RDD model in which we regress the indicated electoral outcomes (difference between 2013 and 2017 federal election; in percent) on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. We vary the RD bandwidth around the 10,000-inhabitant cutoff, which is given on the x-axes. Estimates based on the MSE-optimal bandwidth (filled circles) use separate bandwidths h_{MSE} and b_{MSE} for the point estimates and the confidence intervals (see Table A4 and the discussion in Section 3.4). For all other bandwidths h, we choose $h = b$. For this reason, confidence intervals will not be the same when even when h is close to to h_{MSE}.

Figure A.14: Main results (donut hole approach)



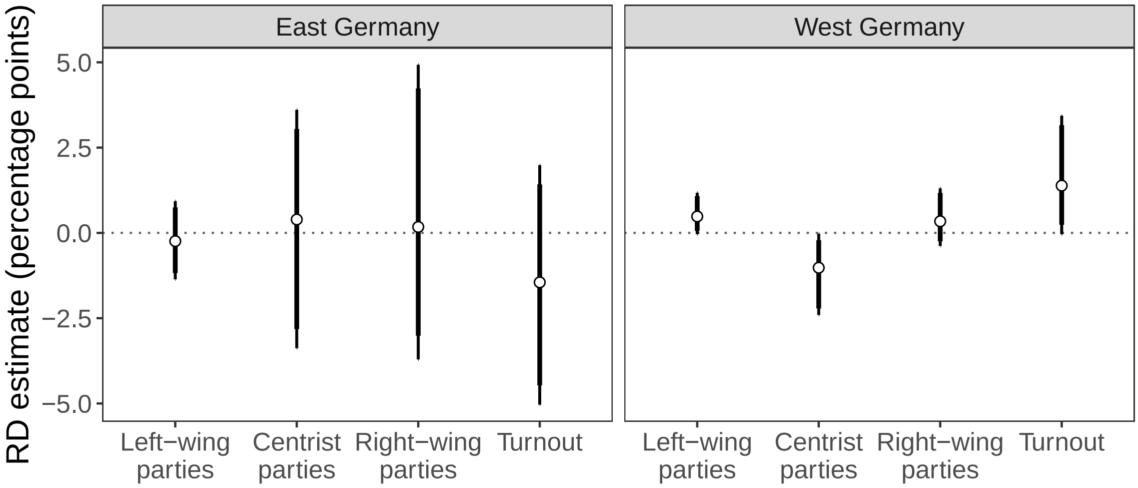
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RDD model in which we regress left-party vote share (difference between 2013 and 2017 federal election; in percent) on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. We exclude municipalities just around the 10,000 inhabitant cutoff. The radius determining whether a municipality is excluded is given on the x-axis. All estimates are based on the MSE-optimal bandwidth. We use separate bandwidths h_{MSE} and b_{MSE} for the point estimates and the confidence intervals (see also Table A4 and the discussion in Section 3.4).

Figure A.15: Placebo cutoffs



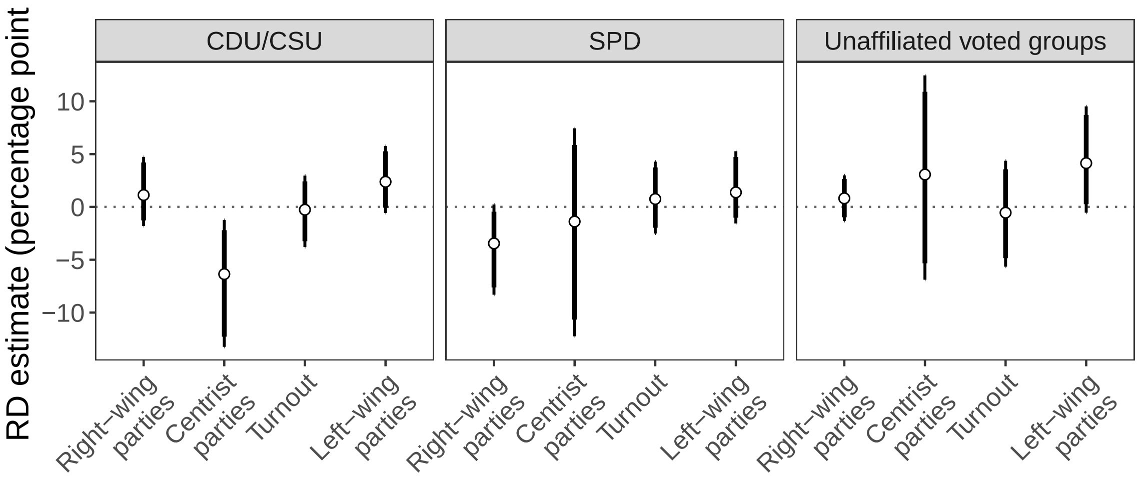
Notes: The Figure shows the distribution of 1,000 sharp RD point estimates for 1,000 randomly selected cutoffs in the interval from 5,000 to 15,000 inhabitants. We use the benchmark RD model. The vertical dashed line is the sharp RD point estimate based on the actual cutoff of 10,000 inhabitants, as given in Figure A.5. The outcomes are left party vote shares (panel A), center party vote shares (panel C), right party vote shares (panel E) and turnout (panel G). All panels are based on the sample the excludes Baden-Württemberg.

Figure A.16: Effect of 2011 census on voting behavior in East and West Germany



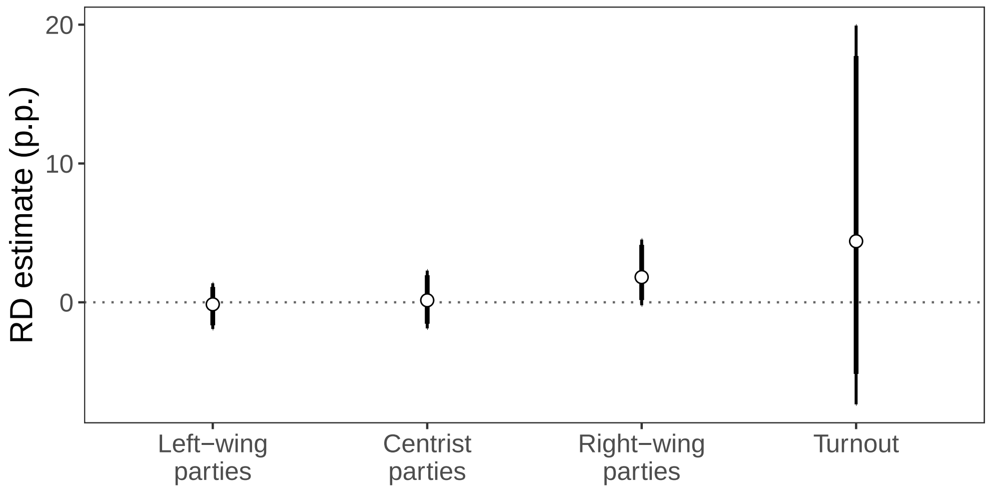
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RDD model in which we regress the indicated electoral outcomes (difference between 2013 and 2017 federal election; in percent) on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. We split the sample into East and West German municipalities.

Figure A.17: Effect of 2011 census on voting behavior, conditional on municipal incumbent



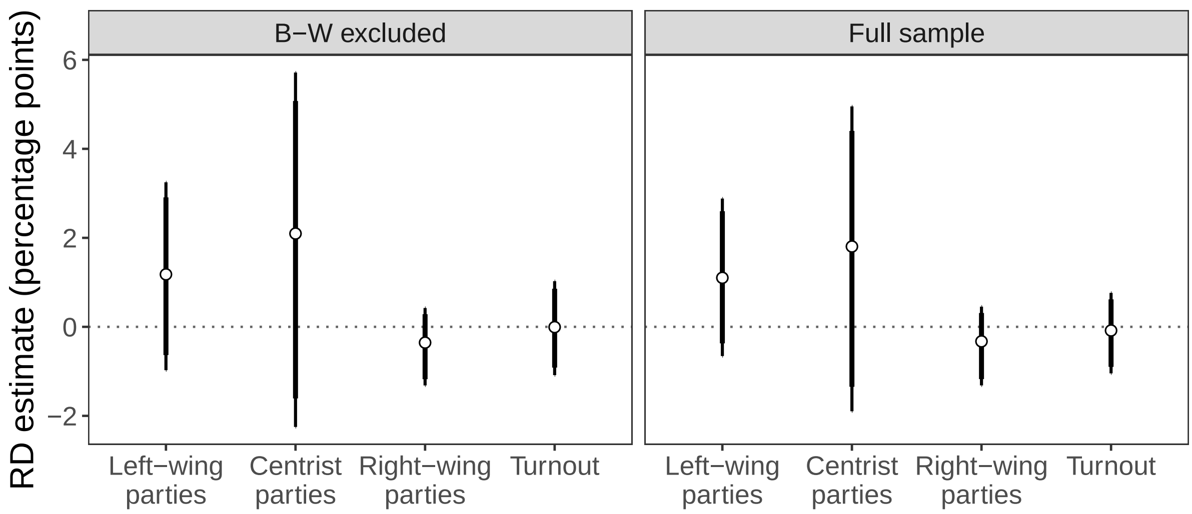
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RDD model in which we regress the indicated electoral outcomes (difference between 2013 and 2017 federal election; in percent) on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. We split the sample into three groups, defined by the strongest party in the municipal council prior to the release of the new census population figures.

Figure A.18: Effect of 2011 census on voting behavior in Rhineland-Palatinate (placebo)



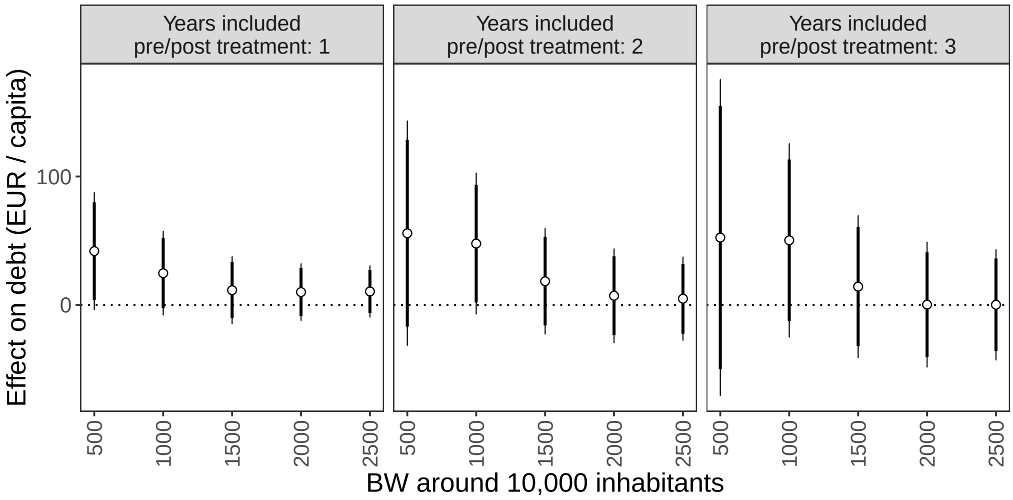
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RDD model in which we regress the indicated electoral outcomes (difference between 2013 and 2017 federal election; in percent) on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. The sample is restricted to the state Rhineland-Palatinate, where the 2011 census was *not* applied.

Figure A.19: Effect of 2011 census on 2009–2013 voting behavior (placebo)



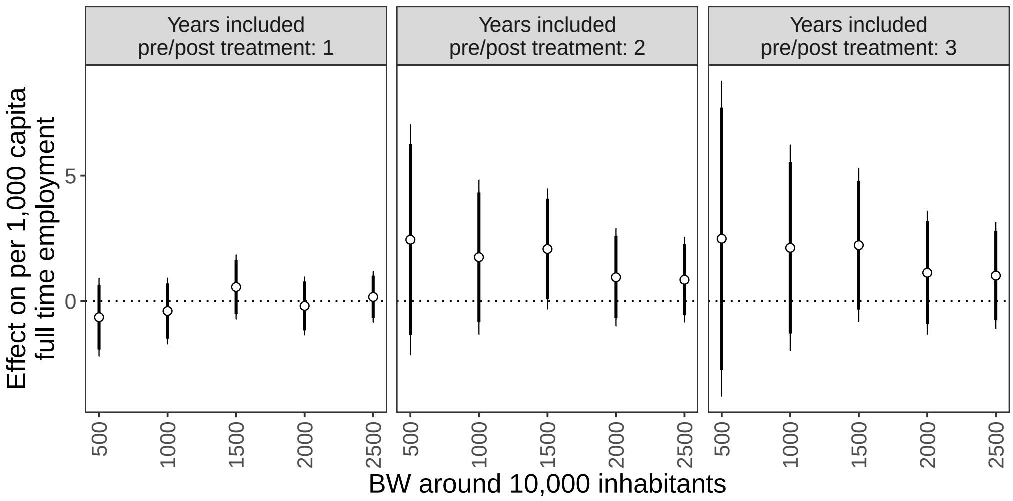
Notes: The Figure shows point estimates and 90 / 95 percent robust bias-corrected confidence intervals from the benchmark RDD model in which we regress the indicated electoral outcomes (difference between 2009 and 2013 federal election; in percent) on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. The left panel includes the preferred sample (excluding Baden-Württemberg); while the right panel includes the full sample (including Baden-Württemberg).

Figure A.20: Effect of 2011 census on municipal debt



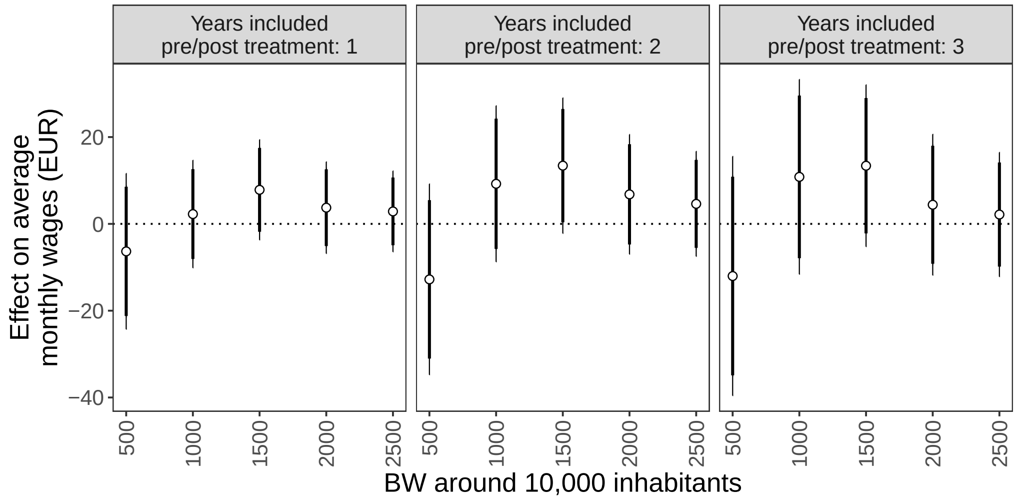
Notes: The Figure shows point estimates and 90 / 95 percent confidence intervals (thick and thin lines, respectively) from the benchmark panel models in which we regress municipalities' debt (in Euro/capita) on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. We report models for five different RD bandwidths around the 10,000 inhabitant cutoff (given on the x-axes). In addition, we vary the number of years before and after the treatment included in the model (shown in panel headings) in order to account for debt easing, which may dilute the treatment effect.

Figure A.21: Effect of 2011 census on full time employment



Notes: The Figure shows point estimates and 90 / 95 percent confidence intervals (thick and thin lines, respectively) from benchmark panel models in which we regress full time employment per 1,000 capita on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. We report models for five different RD bandwidths around the 10,000 inhabitant cutoff (given on the x-axes). In addition, we vary the number of years before and after the treatment included in the model (shown in panel headings).

Figure A.22: Effect of 2011 census on average monthly wages



Notes: The Figure shows point estimates and 90 / 95 percent confidence intervals (thick and thin lines, respectively) from benchmark panel models in which we regress average monthly wages on the treatment dummy. We consider a municipality to be treated if its pre-census population is below 10,000 inhabitants. We report models for five different RD bandwidths around the 10,000 inhabitant cutoff (given on the x-axes). In addition, we vary the number of years before and after the treatment included in the model (shown in panel headings).

A.2 Details on RDD estimation

As stated in Section 3.4, we use the approach proposed by Calonico et al. (2014) to estimate the effects of the budget shock. In this section, we provide additional details concerning the specifics of the RD design.The RD estimation choosing the bandwidth around the cutoff h, the choice of the kernel function K and the polynomial order p. For all models, we set $p = 1$, i.e., we estimate local linear models. As recommended by Calonico et al. (2015), we use a triangular kernel. The choice of the optimal bandwidth is related to the bias-variance trade-off that occurs when one varies the bandwidth. Using the MSE-optimal bandwidth h_{MSE} "results in a point estimator τ_{SRD} that is not only consistent but also has minimal asymptotic MSE" (Calonico et al., 2015, 71). To avoid specification searching, we select the MSE-optimal bandwidth h_{MSE} conditional on the outcome at hand. We only report robust bias-corrected confidence intervals, which are based on a separate MSE-optimal bandwidth b_{MSE}. We report the sharp RD estimate $\hat{\tau}_{\text{SRD}}$ that is based on the optimal bandwidth h_{MSE}. Finally, we add a number of covariates to increase the precision of the estimates, which are reported in Table A2.

A.2.1 Panel survey data estimates

When estimating the effect of the new census population figures on party ID and attitudes using the SOEP panel data, we rely on a modified version of the baseline RD specification. We again use a first-differenced outcomes in conjunction with the RD design described in Section A.2. We run separate models for all years after the census was implemented, using 2013 as the baseline. This means that the outcome in Figures 5 and 6 is defined as $Y_{i,t} - Y_{i,2013}$ for $t \in \{2015, 2016, 2017, 2018\}$, where i indexes municipalities. This leaves us with four different estimates, one for each possible value of t. These estimates are the ones presented in Figures 5 and 6.

A.3 Municipal tendering data

As an additional analysis, we gather data on municipal tendering. The German government maintains a central website where all public government tenders are listed, including projects tendered by municipal governments[14]. We scraped the 10,000 most recent tenders listed on the website as of Jun 20, 2020. Note that this data only includes active tenders. While the federal government also maintains a list of recently completed public tenders, this list only goes back until 2017. Therefore, it is not possible to construct a panel of municipal-level tendering activity that goes back to the time period before the census population figures were applied. We then subset the data to projects tendered by municipalities, excluding projects tendered by county, state or federal government offices. Each tender includes a one-sentence description of what is being tendered. We collect these descriptions and then count the twenty most frequent terms that appear in those descriptions, after removing stop words and legal terms common to all tenders. The results are shown in Table A7. Here, we list the original German term, a translation as well as the number of tenders that a term appears in. We observe that, most commonly, tenders are about new construction or renovation of buildings for public services that municipalities provide to citizens. These services include kindergartens, schools, the fire department, public pools as well as public gyms.

In a second step, we regress a binary indicator for whether municipalities list any tenders on 2016 transfers and spending on the municipal level, controlling for population and state. In Table A6, we show that higher transfers and spending are associated with a greater likelihood of listing tenders.

Table A6: Probability of listing tenders conditional on transfers/spending

	DV: Any tenders listed (0/1)			
Transfers (Euro, scaled)	0.058	0.117^{**}		
	(0.047)	(0.059)		
Spending (Euro, scaled)			0.048	0.191^{**}
			(0.081)	(0.084)
Controlling for pop.	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
N	1,625	1,625	1,625	1,625
R^2	0.051	0.102	0.050	0.103

Note: We estimate a linear probability model, where the outcome is whether a given municipality lists any tenders on the public government tendering portal. The independent variables are transfers (first two models) and spending (models 3 and 4), measured in 2016. We always control for municipality population. The independent variables are standardized. We considers municipalities within a 2,500-inhabitant bandwidth around the 10,000 inhabitant threshold.$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

A.4 Newspaper coverage of census errors

In a supplementary analysis, we use the *Nexis Uni* portal to investigate the frequency at which the new census methods and the resulting population changes were covered in German news media. We proceed as follows: first, we use the following query to find articles that cover census and the population changes around the 10,000-inhabitant cutoff that we analyze: "(zensus OR volkszaehlung) AND fehler", which translates to "(census OR population census) AND error/mistake". We use the 'error/mistake' wording, since the German cities claimed that the census published incorrect population figures for the larger municipalities. Second, we download and clean all articles we find using the query described above.

[14]https://www.service.bund.de/Content/DE/Ausschreibungen/Suche/Formular.html?nn=4641514

Table A7: Most frequent terms in municipal tenders

Term	Translation	No. of mentions	Rel. freq. of mentions (%)
Neubau	New construction / new building	295	10.04
Sanierung	Renovation	286	9.74
Lieferung	Delivery (of goods or services)	206	7.01
Kindergarten	Kindergarten / Preschool	191	6.50
Grundschule	Elementary school	137	4.66
Schule	School	86	2.93
Feuerwehr	Fire department	75	2.55
Beschaffung	Procurement / acquisition (of goods or services)	64	2.18
Erweiterung	Extension / enlargement	59	2.01
Umbau	Conversion / modification (of a building)	54	1.84
Erneuerung	Renewal / renovation	50	1.70
Gymnasium	High school	49	1.67
Gesamtschule	Comprehensive school (German school type)	46	1.57
Bau	Construction	34	1.16
Hallenbad	Indoor pool	33	1.12
Sporthalle	Gym (refers to public or school gyms)	33	1.12
Malerarbeiten	Painting works	31	1.06
Sanitaer	Sanitation systems	31	1.06
Elektroarbeiten	Electrical work / installation	29	0.99
Fenster	Windows	29	0.99

Note: The table shows the most frequent terms in 2,937 active municipal tenders, as of June 20, 2020. We include a translation of the German terms as well as clarifying comments in parentheses.

Finally, we count the annual number of 'true positives', i.e. the articles that both match the query and actually cover the new census method.

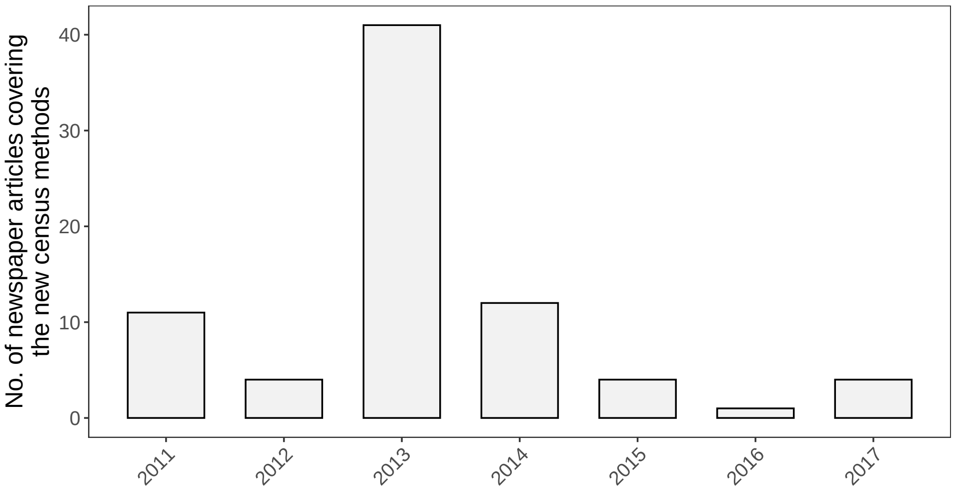
In Figure A.23, we present the number of newspaper articles by year. Not surprisingly, the the new census methods received the greatest attention in 2013, when the new census figures were first made public. Articles prior to 2013 commonly deal with problems with the implementation of the census, e.g. issues with mailing questionnaires or gaps in population registers.

A.5 Spending effects and optimal bandwidths

Our main results are based on optional bandwidths between 2,200 to 2,700 inhabitants around the cutoff. As shown in Figure 3, we observe the most pronounced effects on government spending for municipalities between 9,000 and 11,000 inhabitants, considerably closer to the cutoff than the bandwidths used for the main voting specifications. While initially different, these two fact can be reconciled in three ways.

First, as shown in Figure A.8, we observe the largest changes in vote shares right at the cutoff, i.e. for the same municipalities where we observe increased spending. This suggests that changes in voting and changes in spending happen for the same municipalities. Second, we use the standard triangular kernel to weight observations in the main RD specifications (Calonico et al., 2015). These weights decrease as we move further away from the 10,000-inhabitant cutoff. As a result, the results we observe are mainly driven by municipalities that are very close to the cutoff. Third, we show that the RD point estimates actually increase in size as we move closer to the cutoff (see Figure A.13), again indicating that the results are driven by municipalities closest to the cutoff. Taken together, the results seem to mostly stem from municipalities closer to the cutoff. Since we use the standard approach of choosing the appropriate bandwidth in a data-driven manner, we refrain from selecting an ad-hoc bandwidth that is closer to the cutoff, and may better correspond to the results in the spending analysis. However, we do show that the results hold and may even be stronger for smaller bandwidths, as presented in Figure A.13.

Figure A.23: Newspaper articles on the census discontinuity, by year,



Note: We show the number of news articles that discuss the discrepancy between the two census methods, by year.