1311

Discussion Papers

Deutsches Institut für Wirtschaftsforschung 2013

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Quasi-Experimental Evidence from Germany

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IMPRESSUM

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ISSN print edition 1433-0210
ISSN electronic edition 1619-4535

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Signature Requirements and Citizen Initiatives

Quasi-Experimental Evidence from Germany

Felix Arnold Ronny Freier*

July 10, 2013

Abstract: Signature requirements are often used as hurdles to prevent overuse of public referenda. We evaluate the causal effect of lowering signature requirements on the number of observed citizen initiatives. Based on municipality-level data for Germany, we make use of legislative changes at specific population thresholds to build an identification strategy using a regression discontinuity design. We find that reducing the signature requirement by 1 percentage point increases the probability of observing an initiative by 8–10 percentage points. The results are robust to a variety of tests. Importantly, we go into great detail to rule out other potential confounders.

Keywords: signature requirements, citizen initiatives, local referenda, municipality data, regression discontinuity design

JEL classification: H0, H11, H79

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1 Introduction

Direct democratic participation of citizens is often considered a useful complement to purely representative decision making. While some countries, like the US and Switzerland, have used the tools of direct democracy for a long time, other countries, like Germany, and transnational bodies, like the EU, only adopted provisions for citizen initiatives in their constitutions more recently. Generally, one can observe a trend toward more citizen involvement. Critical and reflective citizens are demanding direct control over policy issues that are of importance to them.[1] It is therefore interesting to understand the institutional rules that govern the process of citizen participation.

The literature on citizen referenda mostly focuses on the policy effects of such direct democratic features. For the most part, the literature concludes that direct democracy reduces overall public spending (Feld and Kirchgässner, 2001b) and brings policies closer to the median voter's ideal point (Gerber, 1996). We are first to look not at policy outcomes of direct democracy, but at the adoption of direct democracy in itself as a function of the constitutional rules that govern the process of citizen involvement.

The power of citizen initiatives to alter politicians' behavior depends crucially on the exact design of the law. In theory, a citizen contemplating the initialization of an initiative will compare the benefits and costs of doing so. While it is hard to quantify potential benefits of an initiative (which depend on the probability that the proposal wins), it is relatively straightforward to say something about the costs. By law, the proposer has to collect a certain number of signatures from people that support his cause. Once this requirement is met, the initiative is admissible for a direct vote. Since the collection of signatures is costly, we would expect that a higher signature requirement makes the proposal of an initiative less likely. The idea of the signature requirement is to impose some costs on the proposer to avoid capture of the initiative process by minorities and to prevent overuse. However, a signature requirement that is too high hinders citizen involvement and may lead

[1]For example, the German Pirate Party has all party positions voted on by their members via an online tool called "Liquid Democracy". Also, big infrastructure projects like highways or railroad tracks are often developed jointly with citizen associations to avoid later protests.

to frustration with political elites. The optimal choice of a signature requirement regime is therefore not trivial and important for policymakers.

This paper aims to identify one part of the proposer's cost function. Therefore, we estimate the causal effect of the number of required signatures on the probability that an initiative is proposed. In other words, we ask how many initiatives are prevented from being proposed due to a higher signature requirement, all else equal. Our identification strategy is based on discontinuous changes in the number of signatures required around several population thresholds specified in the constitution.

To this end, we gather data on initiatives on the municipal level in Bavaria from 1995 through 2008. We combine these data with standard demographic variables of the municipalities, including population size in the respective years.

As noted above, the signature requirement in Bavaria is a deterministic function of population size. It ranges from 10% to 3%, going down in steps of 1% at constitutionally specified population thresholds. While a 1% decrease may seem rather small to have an effect on the use of initiatives, consider that on the margin, these changes can have important implications for proposer costs. For example, at the threshold of 50,000 inhabitants, the signature requirement changes from 7 to 6 percent. This implies a marked increase of $\frac{3{,}500-3{,}000}{3{,}500} = 14.29$ percent in the number of required signatures, which is not negligible, especially in the context of convex costs of collecting signatures.

As regards the methodology, we use a regression discontinuity design. By comparing only municipalities close to the thresholds where the signature requirement changes discontinuously, we control for unobserved characteristics that would otherwise make a consistent estimation difficult. Using this empirical strategy, we attempt to guarantee an assignment into treatment and control group that is "as good as random".

However, as Ade and Freier (2011) note, one has to be careful when using population thresholds as an assignment variable in regression discontinuity analyses since endogenous sorting around the thresholds or simultaneous co-treatments at a given threshold can bias the true effect of the treatment. We shall go into great detail in the robustness section to rule out such potential confounders.

Our results suggest that municipalities falling in the area where the signature requirement just dropped to the next lower category have a roughly 8 - 10 percentage point higher probability – depending on the specification we use – of experiencing at least one initiative compared to the municipalities that still have the higher signature requirement. This effect is robust for different window sizes around the cutoff for various polynomial specifications and also for several choices of bandwidth if we use a nonparametric regression approach. Furthermore, placebo tests at fake thresholds or at thresholds where other institutions change (but not the signature requirement) yield no significant estimates, reassuring us that it was indeed the effect of the signature requirement we were measuring in the first place.

The remainder of this paper is organized as follows: Section 2 discusses the literature, before Section 3 describes the institutional setting in Bavaria and the particular setting that enables us to use a quasi-experimental design. Then, Section 4 presents the data and our empirical strategy. Section 5 holds our main results along with several robustness checks and Section 6 concludes the analysis.

2 Literature Review

In this section, we review the related literature. We identify two strands of literature that are of direct relevance. First, we discuss studies that are concerned with citizen initiatives, and here mostly with the policy effects of such initiatives. Second, on the methodological level, we review other papers that also use constitutionally prescribed population thresholds as the basis for a regression discontinuity framework.

Romer and Rosenthal were first to theoretically (Romer and Rosenthal, 1978) and empirically (Romer and Rosenthal, 1979) link the possibility of direct democracy with policy outcomes. They showed that voters can, by means of public referenda, constrain the spending wishes of an agenda-setting, budget-maximizing monopolistic bureaucracy. In the same vein, Tsebelis (2002) argues that referenda add a new veto player to the system which decreases the potential for policy change. Note that, in our case, differences in the size of the signature requirement imply varying strengths of the new veto player. Persson and Tabellini (2002) summarize the theoretical literature on the topic under the keyword “legislative bargaining”.

Evidence from Switzerland, a country where public referenda are frequently used on various governmental tiers and for a number of policy aspects, also suggests that voters use referenda to keep tabs on politicians. Feld and Kirchgässner (2001b) find that expenditures and tax revenues are lower in Swiss cantons where the public has to approve the budget via a referendum. Also, direct democracy seems to correlate negatively with debt accumulation in Switzerland (Feld and Kirchgässner, 2001a). Funk and Gathmann (2005) refine the overall effect on fiscal variables and find that access to direct democracy increases expenditures at the local level while decreasing expenditures at the canton level.

For the US, Matsusaka (1995) compares states that allow for public referenda with purely representative states and finds that spending is about 4% lower in the former. Although the signature requirement is not his primordial interest, the author reports that the tool of initiatives becomes ineffective when the signature requirement is as high as 10%, which is a result that hints at the importance of our research question. Gerber (1996) looks more directly at how the signature requirement affects policy outcomes. Using data on parental consent laws on teenage abortions in US states, she not only shows that initiatives tilt policy toward the median voter, but also finds that legislative policy diverges less from the state's median voter's preferences if the signature requirement is lower. While both papers implicitly highlight the importance of signature requirements for citizen involvement and, ultimately, public policy, we know of no paper that tries to causally estimate the effect of the signature requirement on the number of proposed initiatives. We contribute to the literature by attempting to close this gap.

On a more methodological level, our paper fits into the empirical political economics literature that uses regression discontinuity designs based on constitutionally prescribed population thresholds. Pettersson-Lidbom (2001, 2012) was first to use this particular design in a study for Sweden and Finland in which the number of council members of a municipality was constitutionally defined on various thresholds regarding the size (number of inhabitants) of the municipality. Population thresholds are also used to identify the effects of wage changes for politicians (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2009), the allocation of transfers (Litschig and Morrison, 2010; Brollo, Nannicini, Perotti, and Tabellini, 2009), representative versus direct democracy (Hinnerich and Pettersson-Lidbom, 2010), fiscal rules and policy (Grembi, Nannicini, and Troiano, 2012) and female participation in politics (Campa, 2012).

Finally, for Germany, Egger and Koethenbuerger (2010) follow the analysis by Pettersson-Lidbom (2001), finding a positive effect of council size on government spending in German municipalities, also relying on the fact that council size is a discontinuous function of population size. However, some concerns for the use of population thresholds in regression discontinuity designs are raised by Ade and Freier (2011), also using German data. They show that estimates from this empirical strategy may suffer from multiple simultaneous co-treatments at the same thresholds as well as precise control over the assignment variable. Put differently, municipalities may be able to manipulate the population statistics. In our robustness section (Section 5.2), we run a large number of tests that aim to show that their concerns are of less importance in our study.

3 Institutional Setting

The municipal level in Germany is the lowest of four tiers.[2] In Bavaria there are 2056 independent municipalities. Generally, the affairs of the municipalities are jointly managed by an elected mayor and an elected town council. The responsibilities of the local tier include administration, public order, infrastructure, cultural institutions and public transport.[3]

In the 1990s, most of the German states amended their municipal codes to allow for direct citizen participation in the policy making process. We focus on Bavaria, the state that has seen roughly 40% of all initiative activity in Germany. The Bavarian legislation was introduced November 1st in 1995. It is laid down in Article 18a of the Bavarian municipal code.[4]

For a public referendum to succeed, citizens have to jump two hurdles: In the first step, citizens propose an application for a public referendum (the so-called *Bürgerbegehren*). The proposer of such an initiative then has to collect a minimum

[2]In addition to the federal government, there are 16 states and about 450 counties.

[3]Note that German municipalities are often also in charge of administrating the spending that is being allocated from higher tiers, e.g. for public schools or social services. In those areas, municipalities may have only limited discretion.

[4]See *www.gesetze-bayern.de* for more information.

number of signatures from people that support her cause. Only if this condition is met and the subject of the initiative is applicable for a direct vote[5], the second step of the procedure is initiated: A public referendum, or *Bürgerentscheid*, is held. If a majority of citizens votes in favor of the proposal and this majority corresponds to a sufficiently large fraction of the electorate, the proposal becomes law and has the same standing as a decision taken by the municipal council.

In the analysis, we make use of the fact that the constitutionally prescribed signature requirement for a successful *Bürgerbegehren* varies by municipality size. Policy makers were concerned that it may be more difficult to collect a certain percentage of signatures in a large, socially heterogeneous city than in a small village. Therefore, the state authority made the signature requirement a function of population size.[6] This function is not smooth, but displays discrete jumps at several population thresholds. In Bavaria, the signature requirement varies between 10% for the smallest and 3% for the biggest municipalities. It goes down in 1%-steps at several population cutoffs. Around 500,000 inhabitants, there is a 2% jump, but this only affects the cities of Munich and Nuremberg. The exact key is displayed in table 1.

At first sight, a 1% jump in the signature requirement may seem too small to have a significant effect on the number of proposed initiatives. We argue, however, that these jumps may indeed constitute important shifts. Consider the threshold of 50,000 inhabitants at which the signature requirement changes from 7 to 6 percent. In absolute numbers, this implies that the proposer of an initaitive has to collect 3,000 instead of 3,500 signatures from a population of 50,000. This corresponds to a marked decrease of $\frac{3{,}500-3{,}000}{3{,}500} = 14.29$ percent in the number of required signatures. Furthermore, there is good reason to assume that the cost of collecting signatures is highly convex. Passionate supporters of an idea will sign the petition without hesitation in the beginning; however, as the number of signatures collected increases, it is increasingly difficult to find new voters who

[5]Each state has a list of topics that are eligible for public referenda. Note that one reason that we observe relatively many initiatives in Bavaria is that the state law here allows for many different aspects of local government policy to be decided upon in a public referendum.

[6]Note that other German states have also followed this line of reasoning and introduced similar thresholds in their legislation.

Table 1: Regulation of the Signature Requirement in Bavaria

Number of Inhabitants	Signature Requirement
up to 10,000	10%
up to 20,000	9%
up to 30,000	8%
up to 50,000	7%
up to 100,000	6%
up to 500,000	5%
more than 500,000	3%

Source: Bavarian Municipal Code Article 18a.

are willing to sign the petition. Put differently, the cost of collecting the first 500 signatures is much lower than the cost of collecting the marginal 500 signatures imposed by the increased signature requirement. Therefore, we expect the changes in the signature requirement to have non-trivial effects.

Note that the law also prescribes an alternative route that can lead to a public referendum. Apart from the citizens of a municipality, also the local town council can prescribe a public referendum to be held.[7] Importantly, these council-initiated referenda do *not* require signatures to be collected. While this means that we generally exclude those cases for our main analysis, we use those referenda for a placebo test in the robustness section.

Importantly for our study, the municipal code in Bavaria prescribes a number of institutional rules or constitutional features to depend on population thresholds (see Ade and Freier (2011)). Overall, there are 15 different aspects that are determined at certain thresholds, which in part overlap with the thresholds used in this analysis. We summarize the institutional setting in Table 5 in the appendix. Among other rules, population thresholds are used to define the number of council members, the wages and the type of positions of politicians as well as the set of tasks and fiscal transfers that a municipality is in charge of. This particular setting constitutes an important challenge for the identification of the causal effects

[7]If a council renders a topic to be of general importance for a public referendum, they can directly put this referendum on the public agenda. Within the council, the proposal for a referendum needs 2/3 of all votes.

in this analysis. In the robustness section, we will go into great detail and provide a number of tests to illustrate that the results reported in this paper are indeed driven by the signature requirement rules and not by other discontinuous changes at the same thresholds.

4 Data and Empirical Strategy

4.1 Data and descriptive statistics

To answer our research question, we combine data from two different sources. First, we use a dataset on direct democratic activity in Germany that was jointly developed by the Universities of Wuppertal and Marburg. The nonprofit association *Mehr Demokratie e. V.* makes these data publicly available on its website.[8]

In this dataset, we observe all citizen initiatives in Bavaria from 1995 until 2008. Each initiative is coded as one observation, giving us roughly 2000 data points to start with. Figure 1 in the appendix shows the yearly number of initiatives proposed since 1995. This graphic illustrates that public referenda activity saw an initial spike just after introduction and then quickly leveled off to an average of around 100 referenda per year.

For each proposed initiative in the data set, we observe a number of further variables. Apart from the year of the proposal, we also know the subject of the initiative and the outcome of the process, i.e. whether a referendum was held and if so, whether it was successful or not. Also, having a unique identifier for each municipality in every year, we can match the data with our second data set: the yearly official statistics collected by the Federal Statistical Office. From those data, we can deduce the respective population size in that municipality at that time.[9]

In Bavaria, there are 2056 municipalities (including 25 county-free cities), which we observe over a time period of 14 years (from 1995 to 2008). Since we can only use citizen-proposed initiatives and not council-initiated referenda (for which the signature requirement is not applicable) in our analysis, we are left with about

[8] See *http://www.mehr-demokratie.de/bb-datenbank.html*.

[9] We use the official population figure for the current year as the crucial measure. On the basis of this information we then determine what kind of signature requirement was in place and whether a municipality belongs to the treatment or control group.

1800 observations. Still, enough observations remain such that the unconditional probability of observing an initiative in a given municipality in a given year is about 6%.

For our analysis, the idenfication in the regression discontinuity design will come from observations that lie close to the respective population thresholds where the signature requirement changes. Since it is not ex ante clear what "close" means, we will use different samples ranging from +/- 5% to +/- 25% from the thresholds.[10] Table 2 lists how many observations remain as a function of the sample we choose.

Table 2: Samples used for Estimation

Sample	Window Size	N	N_l (Control)	N_r (Treatment)	Mean(Initiatives)
Discontinuity	5%	704	349	355	0.15
	15%	2477	1278	1199	0.13
	25%	4316	2313	2003	0.13
Whole Sample		28784			0.06

Notes: N denotes the number of municipalities in the respective sample. In total, there are 2056 municipalities (including county-free cities) in Bavaria that we observe from 1995 through 2008, giving us a total of $2056 \cdot 14 = 28784$ observations. N_l corresponds to the number of observations to the left of the cutoff; hence it describes the size of our control group. N_r, analogously, is the size of the treatment group to the right of the cutoff. Mean(initiatives) describes the number of referenda in the respective sample divided by the size of the sample.

The table shows that sample size is significantly reduced if we restrict ourselves to municipalities close to the thresholds. This is even true for the biggest +/- 25% discontinuity sample. This is due to the fact that the distribution of municipality sizes in Bavaria is highly skewed to the right. 50% of all municipalities have less than 2,815 inhabitants. Since the first population threshold for the signature requirement is at 10,000 inhabitants, the majority of municipalities is far even from the first threshold. Note also that the probability of observing an initiative is between 11 and 13 percent in each of the discontinuity samples. This is markedly higher than in the whole sample, also for the fact that these are larger municipalities. Further descriptive statistics (mean, standard deviation, min and max) of all our outcome and explanatory variables can be found in Table 6 in the appendix.

[10]Distance to the threshold is measured in percentage points. For the threshold of 10,000 inhabitants, the +/- 5% sample will include all municipalities that have a population between 9,500 and 10,500 inhabitants.

4.2 Empirical Strategy

To estimate a causal effect of having a lower signature requirement on the number of proposed initiatives, we use a regression discontinuity framework. Following Pettersson-Lidbom (2012), we use population thresholds at which the signature requirements change discontinuosly to identify our treatment effect.

Relying on observations close to the respective population thresholds, we estimate separate regression functions for treatment and control group and interpret the jump at the cutoff where treatment changes from 0 to 1 as the causal effect of the treatment. From a computational perspective, it is convenient to subtract the population threshold from the actual population figure, i.e. to work with $X - c$ rather than with X.

This amounts to estimating regression functions on the left and right hand side of the cutoff:

$$Y = \alpha_{left} + f_{left}(X - c) + \epsilon \quad if \quad X \leq c \tag{1}$$

$$Y = \alpha_{right} + f_{right}(X - c) + \epsilon \quad if \quad X > c \tag{2}$$

where Y is an outcome variable related to initiative activity in the municipality, X is population size and c is the respective (nearest) threshold. $f(\cdot)$ is some polynomial function of $(X - c)$. Several functional forms will be used to estimate the treatment effect robustly. Let D be a dummy indicating treatment. In order to get a direct estimate along with standard errors for the treatment effect $\tau = \alpha_{right} - \alpha_{left}$, it is common to run a pooled regression by including interaction terms between D and X. Thus, with a linear control function $f(\cdot)$, we would estimate the following model:

$$Y = \alpha_{left} + \tau D + \beta_{left}(X - c) + (\beta_{right} - \beta_{left})D(X - c) + \epsilon \tag{3}$$

Note that this specification allows the slope of the regression functions on the two sides of the cutoff to differ. If our theoretical reasoning is true, we would expect the coefficient of interest τ to be positive and significantly different from zero.

One possible disadvantage, however, of estimating the treatment effect parametrically is that observations further away from the cutoff also determine the value of the regression function at the cutoff. This happens because a *global* criterion – the sum of squared residuals – is minimized. Of course, this problem can be alleviated by including higher order polynomials into the regression function, allowing for a more flexible fit. Nevertheless, it is quite common to account for the *local* nature of the regression discontinuity estimator by estimating the model nonparametrically using kernel techniques. We use local linear regressions with a triangular or rectangular kernel, as it is standard in the literature.[11] For a rectangular kernel, this amounts to estimating the same model as in Equation (3), however, now with an additional restriction concerning the bandwidth h, namely that $c-h \leq X \leq c+h$. The important question then is how to chose the bandwidth. We use a data-driven cross validation procedure to find the optimal bandwidth that minimizes the mean squared error of the regression, as suggested by Imbens and Kalyanaraman (2012). The cross-validation criterion is defined as:

$$CV_Y(h) = \frac{1}{N}\sum_{i=1}^{N}(Y_i - \hat{Y}(X_i))^2 \tag{4}$$

This implies an optimal choice for the bandwidth:

$$h_{CV}^{opt} = arg\ \min_h\ CV_Y(h) \tag{5}$$

Note that the optimal bandwidth balances the tradeoff between variance and squared bias. Nevertheless, we will report estimates for different choices of bandwidth as robustness checks.

5 Results

This section is split into three parts. First, we discuss our main results. Then, we examine the validity and robustness of the RDD estimates presented. Finally, we

[11] The advantage of using a rectangular kernel is the ease in interpretation: It is equivalent to estimating a standard regression over a window of width h on both sides of the threshold. Other kernels (for example a triangular one) are generally more efficient for estimation at the boundary, but usually results don't change much. Furthermore, a higher weighting of boundary points (this is what the triangular kernel does) can also be achieved with a rectangular kernel by using a smaller bandwidth. Our results for triangular and rectangular kernels are very similar, so we only report the coefficients from the estimation with a triangular kernel.

highlight a number of tests that aim to exclude confounding mechanisms.

5.1 Main Results

Table 3 shows our main estimates of the treatment effect τ (see eq. (3)). Our dependent variable is a dummy Y_{it} that takes the value of 1 if municipality i experienced at least one initiative in year t. The treatment effect can thus be interpreted as the change in the yearly probability of observing an initiative. We highlight four RDD specifications. Column 1 shows the results in the narrow sample of 5% using only a linear polynomial. Column 2 and 3 extend the sample to 15% and 25% respectively, using quadratic and cubic control functions. In column 4, we implement the nonparametric specification with an optimal bandwidth as described above.

Table 3: Estimates of the Treatment Effect

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%	(4) RDD 25%
Treatment	0.102**	0.081**	0.102**	0.083**
	(0.047)	(0.040)	(0.042)	(0.035)
Control function	Linear	Quadratic	Cubic	Optimal Bandwidth
Specification	Parametric	Parametric	Parametric	Nonparametric
N	704	2477	4316	4316

Notes: Standard errors in parentheses. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01. The dependent variable is a dummy that equals 1 if the municipality in question had at least one initiative. The treatment variable is defined as having a lower signature requirement.

The effect of treatment is positive (as expected) and significant throughout. Depending on window size and control function, the estimates range between 0.08 – 0.10. Having a lower signature requirement thus increases the probability of observing at least one initiative by 8-10 percentage points. Note that this measures an average effect, since we pooled the data for all thresholds.

Figure 2 in the appendix visualizes the jump in the probability of observing an initiative around the cutoff and thereby provides a graphical representation of the regression results from Table 3. In the graph, we plot the deviation in log population from the threshold against the probability of an initiatve in bins of 1%. The graph highlights that the probability difference is immense for observations just left and right to the threshold before it quickly resumes to a lower level.

The point estimates presented in Table 3 and Figure 2 are strikingly large. Given that the unconditional probability of observing an initiative was 15 percent in the 5% discontinuity sample, an increase in probability by 8 percentage points roughly corresponds to a 50% premium on the unconditional probability of observing a referendum in the treatment group.

To complement the above analysis, we also estimate the effect of a lower signature requirement on several other outcome variables related to initiative activity. Mainly, we expect the signature requirement to affect whether a proposal for a referendum is initiated. However, we also care for the fact whether the rule for signatures changes the overall number of initiatives proposed, the number of referenda that are ultimately held as well as the number of initiatives that will lead to a decision.

We present our estimates for the alternative outcomes in Table 4. Panel 1 displays estimates for the number of initiatives as outcome. As the dependent variable now varies between 0 and 3 (and not between 0 and 1), the coefficient of the treatment effect is now larger in size and still highly significant. In Panel 2 we show the results for the number of actual referenda. Not all initiatives will lead to a referendum because the proposed subject may be not eligible for a direct vote, the signature requirement is not met, or other factors intervene in the process. However, also for this outcome we observe positive and significant effects of the lower signature requirement in the order of 5 – 7 %. Finally, Panel 3 takes the number of initiatives leading to a decision as our measure for outcome. Some initiatives are successful although a referendum never takes place. This happens when the municipal council adopts the initiative proposal and issues a new policy in the spirit of the initiative. The council decision then renders a direct vote on the topic obsolete. The outcome variable in Panel 3 captures – in addition to normal referenda – the successful initiatives that were resolved by the city council. Our estimates here are again as large as 10% and significant throughout. We conclude that the signature requirement is important not only at the stage of initiative proposal, but also whether a proposal successfully leads to a political decision.

Table 4: Treatment Effect for Alternative Outcome Variables

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%	(4) RDD 25%
	Panel 1: Dep. Variable - Number of Initiatives			
Treatment	0.162**	0.118**	0.159***	0.122***
	(0.068)	(0.058)	(0.060)	(0.046)
	Panel 2: Dep. Variable - Number of Actual Referenda			
Treatment	0.072*	0.051	0.070**	0.054**
	(0.038)	(0.034)	(0.035)	(0.025)
	Panel 3: Dep. Variable - Number of Initiatives Leading to a Decision			
Treatment	0.096**	0.076*	0.100**	0.075**
	(0.048)	(0.041)	(0.042)	(0.031)
Control function Specification	Linear Parametric	Quadratic Parametric	Cubic Parametric	Optimal Bandwidth Nonparametric
N	704	2477	4316	4316

Notes: Standard errors in parentheses. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The treatment variable is defined as having a lower signature requirement.

5.2 Validity and Robustness of the RDD Results

The regression discontinuity approach outlined above hinges on the crucial assumption that municipalities have no perfect control over the assignment variable that determines treatment. That is, we have to rule out the possibility that municipalities endogenously sort around the thresholds since this would interfere with a *random* allocation of units into treatment and control group. To test whether such a manipulation of the assignment variable is likely in our data we graph a density plot of the assignment variable around the thresholds. For our quasi-experimental design to be valid, we need this density to be continuous.

Figure 3 in the appendix shows a histogram of all municipalities in Bavaria that lie within the range of +/- 30% of their respective population thresholds.[12] As

[12]This implies that for the threshold of 10'000 inhabitants, municipalities from 7,000 to 13,000 inhabitants will make it into the diagram, whereas for the threshold of 50,000, municipalities between 35,000 and 65,000 residents will make it. We have thus a pooled

shown in the graph, we observe no evidence for sorting of municipalities just above (or below) the threshold. The jump at 0 is well within the error margin and the fitted kernel density estimates have almost the same functional value at $X = 0$. Using a formal test (see McCrary (2008)), we cannot reject the null hypothesis of no sorting.[13]

Our results are markedly different to the evidence on sorting for German municipalities presented by Ade and Freier (2011), who use data that include earlier periods. Looking at population data in Bavaria, we note that before 1990, municipalities were able to manipulate population numbers as there was very limited variation and policy makers could easiliy project future population figures. Starting in 1990, however, the municipalities were confronted with an enormous influx of migrants from the former East German states as well as many Ethnic Germans from the former eastern bloc countries of up to $17 - 20\%$ additional population (see Freier, Geys, and Holm (2013)). Given these unexpected shocks, it is understandable that we see no sorting in our data (starting 1995), while Ade and Freier (2011) find clear evidence of manipulation.

Another validity test concerns the balance of predetermined variables in treatment and control group. If assignment to these groups is, as we claim, "as good as random", variables that have been determined before the introduction of direct democracy should be balanced in both groups as a result of the randomization. In particular, we care to show that citizens in municipalities just left and right to the cutoff showed equal amount of political participation before local public referenda were introduced. To that end, we collected data on the municipal elections in 1990. Table 7 in the appendix provides evidence that the predetermined variable tests supports our claim of randomization. The number of valid votes as well as the turnout rates are not significantly different in towns closely above and closely below the population thresholds. Furthermore, also election results seem to be balanced in both groups: Neither of the two big parties (*CDU* and *SPD*) performs significantly better in treatment or control group.

As an initial robustness exercise, we investigate whether our treatment effect varies over time. As we saw in Figure 1, initiative activity in Bavaria peaked just after

presentation of all municipalities that lie close to their respective population threshold.

[13]The test statistic for the McCrary test is 0.053 with a standard error of 0.115.

introduction and then levelled off. It is thus interesting to check for heterogeneous effects over time. Table 8 displays estimates of a model where treatment effect and control function are fully interacted with a time trend of the form $t = year - 1995$. We see that the effect of the lower signature requirement is highest in the years just after the introduction of the initiative. As the coefficient of the time trend variable indicates, the treatment effect then declines over time. With a linear interaction term as displayed in the table, the probability of observing at least one initiative drops by approximately two percentage points per year. Note that the bigger treatment effect in the order of 25 percentage points has to be read with care: In the years just after the introduction of the initiative, also the baseline probability of observing an initiative is much higher than in the whole sample from 1995 to 2008.

As a second robustness check, we run a number of placebo tests at artifical ('fake') thresholds where we would not expect to observe any jump in the outcome variable because no real treatment takes place. In particular, we estimate a series of regressions around cutoffs that lie at some distance to the left and to the right of the real cutoff. These population thresholds do not exist in reality and we thus should not observe significant effects here. Figure 4 in the appendix summarizes the estimates for the treatment effect along with confidence intervals for various placebo thresholds. We report our main estimate at the actual threshold at zero. Then, we run similar regressions for fake thresholds up to a distance of +/- 10% from that true threshold. As highlighted, the results are reassuring, since our estimates are indeed uniquely found at the real threshold. Going away from the threshold to either side, the point estimate becomes small and pivots around zero.

To further show the robsutness of our results, we conduct a battery of specification tests to make sure that our results are stable with respect to changes in functional form and the particular data window. Table 9 in the appendix is an extension of our main results table in section 5.1. Panel 1 displays estimates of the treatment effect for more combinations of window sizes and control functions in the parametric specifications. For the nonparametric specifications, the robustness of the results should also be evaluated with regard to the choice of bandwidth. This is what Panel 2 does.[14] Importantly, the treatment effect is always sizably positive and

[14] With oversmoothing (twice optimal bandwidth), the effect of a lower signature re-

never loses significance.[15]

To see whether the effect of the signature requirement translates to other German states, we conducted the same analysis for the state of North-Rhine Westphalia (NRW).[16] This state has a signature requirement regulation that is very similar to the one in Bavaria.[17] Table 10 displays the estimates from the regressions. Although the effects are insignificant and smaller in size than in Bavaria, they have the correct sign: Also here, a lower signature requirement leads to a higher probability of experiencing an initiative. The lacking significance may be due to the hesitant use of the initiative in North-Rhine Westphalia: Despite similar regulations concerning the signature requirement, we observe about 75% less initiatives here compared to Bavaria.

As a final test for robustness, we check the sensitivity of our results when we leave out individual thresholds. Until this point, we have always pooled the observations at the thresholds of 10,000; 20,000; 30,000; 50,000 and 100,000 inhabitants. Table 11 shows the results of this jackknife exercise. The results are positive throughout and are mostly significant. Notably, the threshold of 50,000 is of some importance as the estimates generally loose significance when we drop this cutoff.[18]

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quirement is generally a bit smaller, whereas the converse is true for undersmoothing (half optimal bandwidth). This is intuitive: Averaging over too many observations (i.e. oversmoothing), some of which may lie further away from the threshold, can obscure the local nature of the treatment effect, rendering its coefficient smaller. This confirms the result of the graphical analysis which highlighted that our estimates are particularly driven by the observations close to the threshold.

[15]As a further robustness test we also checked whether our results are sensitive to the exclusion of particular administrative districts (*Regierungsbezirke*). Bavaria has seven large administrative districts and we excluded each of them and estimated the model only with the remaining six. The results are literally the same, which underscores that the effect is not only driven by a particular subset of towns. The results of this analysis are not included in this paper, however, they are available upon request.

[16]Note that we left out NRW in the main part of the paper because there are notably fewer municipalities and initiative activity was generally less frequent. We also decided against pooling states as the precise institutional framework is state-specific and markedly different (except for the regulation on the signature requirement which uses similar thresholds as Bavaria). Among other things, NRW allows for fewer topics to be voted upon via local public referenda.

[17]Due to the higher density of larger cities in North-Rhine Westphalia, there is an additional threshold at 200000 inhabitants. All the other thresholds are identical to the ones in Bavaria.

[18]The results remain sizable positive even here and they fall within the confidence bands of our main results.

5.3 Excluding alternative mechanisms

As laid out in Section 3, population thresholds in Bavaria are used for a number of different institutional rules. In this section, we explore whether those alternative rules that change at the same thresholds are likely to exert an effect. This is important, because we want to insure that the observed effects are indeed caused by our defined treatment.

First, we estimate the treatment effect around cutoffs where the signature requirement is left unchanged, but however, other important institutions on the municipality level change. As Table 5 shows, the number of seats in the city council as well as the wages and the status of the mayors are discontinuosly determined by population thresholds at 1,000; 2,000; 3,000 and 5,000 inhabitants. Moreover, the threshold at 5,000 inhabitants also changes the fiscal setting of a municipality as towns reveive additional funding to take care of roads and fall under a different code for the fiscal equalization scheme.

We run the same regressions as in the main results Section 5.1 at these different population thresholds. The estimated coefficients are very close to zero and insignificant. Hence, we observe no treatment effect when no treatment takes place, as anticipated. Note that some of the institutions mentioned above also change at other population thresholds *simultaneously* with the signature requirement. For example, city council size not only changes at 1,000; 2,000; 3,000 and 5,000 inhabitants, but also at 10,000; 20,000; 30,000; 50,000; 100,000 and 200,000 inhabitants. Moreover, at 30,000 inhabitants, municipalities get the status of a "larger" city and at 50,000 inhabitants, they can decide to become a county-free city. One could thus argue that this simultaneous treatment of several institutional changes at a given threshold, among which the signature requirement is only one, possibly conceals or biases the true effect of the lower signature requirement. We cannot rule this out. However, we saw beforehand that at the lower population thresholds, where only other institutions than the signature requirement change, no effect could be found. Consequently, it seems reasonable to attribute the significant treatment effect to the signature requirement, because the other institutions had no impact on the outcome variable beforehand.

Egger and Koethenbuerger (2010) and Ade and Freier (2011) are concerned with the effect of council size on fiscal spending in the same Bavarian data (although earlier years). The former paper argues for large spending effects as a result of council size changes at population thresholds. To make sure that our results for the probability of observing an initiative are not indeed driven by council size changes or resulting changes in spending, we test whether directly controlling for these variables makes a difference in our analysis. Note that this is not without problems, because both council size and spending must be viewed as endogeneous, so what we can learn from these models is limited.

Table 13 in the appendix shows that our coefficient of interest is robust to the inclusion of these two crucial control variables. First, we include council size of a municipality as a right hand side variable. Panel 1 shows that the treatment effect survives the inclusion of council size as an additional regressor even though the significance drops slightly.[19] Second, we control for overall spending of municipalities. Spending necessarily varies with population size (larger municipalities spend more) and could also be correlated with the number of initiatives. Panel 2 reveals that the treatment effect is also robust to the inclusion of spending into the model. Third, we check whether the inclusion of the referendum quota as a control variable affects our treatment effect. In Bavaria, not only a majority of the voters has to vote in favor of the proposal, but this majority has to correspond to a sufficiently large fraction of the electorate. This is the so-called "quota", which also varies at several population thresholds.[20] Since it is directly related to proposer costs, one could expect that its omission biases our treatment effect.[21] Panel 3 shows that our model is robust to the inclusion of the the referendum quota. Finally, Panel 4 shows the results of a model that includes all three control variables that were discussed beforehand at the same time. Also here, the treatment effect remains of similar size and significant.

[19]Note that the council size variable is a deterministic function of population size, so it is highly correlated with our regressor. Indeed, the treatment here is only identified because council size changes are of different step sizes at the different thresholds. This high correlation also explains why our estimates are somewhat affected in their level of significance.

[20]Up to 50,000 inhabitants, the majority in favor of the proposal has to correspond to 20% of the electorate. Up to 100,000 inhabitants, 15% are sufficient and with more than 100,000 inhabitants, the quota is 10%. *Source*: Bavarian Municipal Code Art. 18a

[21]The signature requirement and the referendum quota both affect proposer costs. However, they do so at different moments of the initiative process: While the cost for the collection of signatures is incurred ex ante – that is, before the actual vote on the topic – the referendum quota takes its effect ex post.

As a final check, we propose a falsification test. As mentioned before, there exists an alternative way to put a referendum on the political agenda in Bavaria. While we have been concerned with the collection of signatures through a voter initiative, a referendum can also be initiated directly by the local council. Importantly, those council-initiated referenda do not have to fulfill any signature requirements. Now, a natural falsification test is to check whether these council-initiated referenda show different patterns in treatment and control group. If our desgin is valid, no significant differences between treated and control units should be present. To the extent that other discontinuous rules at the population thresholds matter to the referenda process, we would expect the estimates for the council-initiated referenda to show positive effects.

Table 14 in the appendix presents regression discontinuity estimates with council-initiated referenda as the dependent variable. The results confirm our expectations: A lower signature requirement has no influence on the amount of council-initiated referenda. Thus, this variable – which is not affected by the treatment – does not show different effects in treatment and control group.

6 Conclusion

In this paper, we study the causal effect of reducing the signature requirement on the probability of observing a initiative for a public referendum. Based on data for Bavaria, we apply a regression discontinuity design using population thresholds at which the signature requirements are stepwise reduced.

We find that, directly at the thresholds, a drop in the number of signatures required causes the probability of an initiative to increase sizably and significantly. While the baseline probability to observe an initiative in a municipality is about 15 percent in a given year, the lower signature requirement increases this probability by 8-10 percentage points. The effects must therefore be viewed as immense.

Interestingly, the main effect seems to be confined to observations that are relatively close to the threshold. While this is of no concern for the internal validity of our estimates, it raises questions about the external relevance. Because the effects quickly resume to lower levels just after the thresholds, that part of the effect must indeed be attributed to a local effect in which the threshold in itself constitutes a specific treatment. If being close to the threshold (or just above) makes citizens particularly aware that initatives are relatively cheap to propose, it may be in fact the additional awareness rather than the lower costs that makes for part of the effect.

Our results speak to policymakers that aim to design the rules and institutions that regulate features of direct democracy. Generally, we consider descision making under direct democracy desirable because it provides an inherent link between the will of the public and the policy implemented. However, policy makers have long noticed that hurdles such as signature requirements must also be implemented to avoid the overuse of the tools of direct democracy. Our paper highlights that signature requirements are indeed an effective way to steer direct democracy activity. Policymakers must carefully evaluate the effects of those signature requirements with the objective to regulate the amount of local referenda to a level that is neither too high nor too low.

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Appendix

Table 5: Population Thresholds in Bavaria

	Population thresholds at # of inhabitants (in thousands)									
	1	2	3	5	10	20	30	50	100	200
	Panel 1: *Thresholds used in this paper*									
Signature Requirement					x	x	x	x	x	
	Panel 2: *Thresholds defining local institutions*									
Council size	x	x	x	x	x	x	x	x	x	x
Full-time council members					x					
Wage of elected civil servants					x		x	x	x	
Wage of full-time mayors		x	x	x	x		x	x	x	
Wage of part-time mayors	x		x	x						
Referendum quota								x	x	
City districts									x	
Open council					x				x	
Accounting committee				x						
Mayor status				x	x					
	Panel 3: *Thresholds defining budgeting rules*									
County free city								x		
Status of larger city							x			
Vehicle Tax				x						
Fiscal equalization				x	x			x	x	

Notes: *Source*: Table as presented in Ade and Freier (2011, p.8, table 1). For detailed descriptions of the specific rules that apply at those thresholds, consult Ade and Freier (2011, pp.24-25, tables 6 and 7).

Figure 1: Referenda in Bavaria Over Time

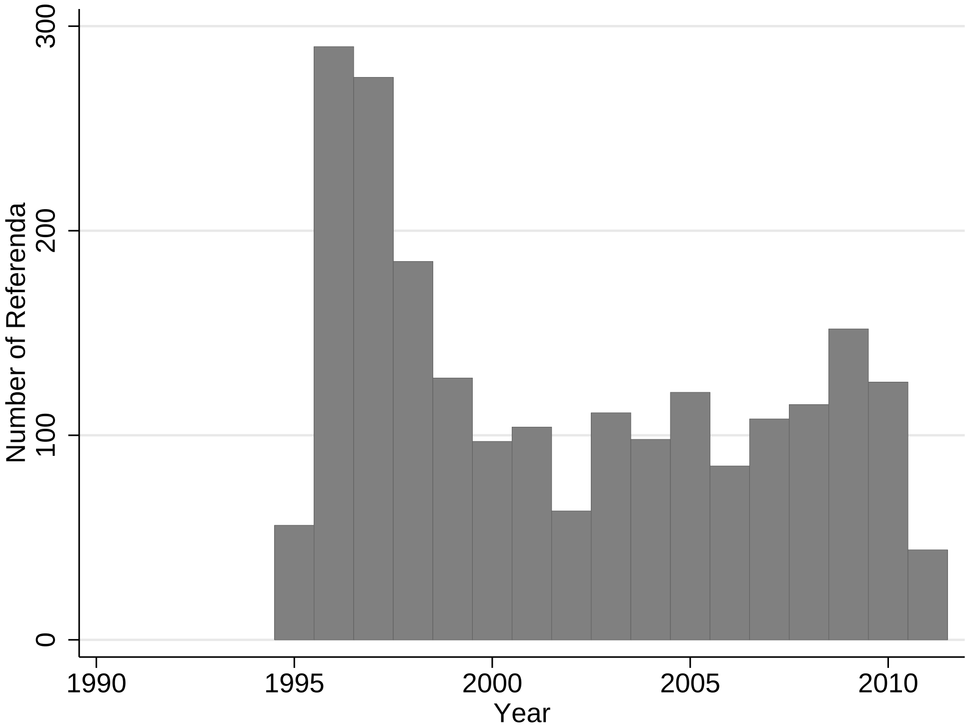


Table 6: Descriptive Statistics for Main Variables of Interest

Variable	Mean	Standard Deviation	Min	Max
	Panel 1: Outcome Variables			
Prob(At Least One Initiative)	.1068119	.3089097	0	1
Number of Initiatives	.1283596	.403027	0	3
Number of Actual Referenda	.0644115	.2757455	0	3
Number of Initiatives Leading to a Decision	.0836423	.3130255	0	3
	Panel 2: Explanatory Variables			
Population	29606.58	33761.22	7790	128380
Council Size	28.44522	9.783652	20	50
Total Expenditures (in 1000 Euros)	49874.93	60490.96	5257.373	526911.4

Notes: The table shows descriptive statistics for all variables of interest in the 25% discontinuity sample. Council-initiated referenda have been dropped from the sample since the signature requirement is not applicable for them.

Figure 2: Graphical Representation of the Main Results

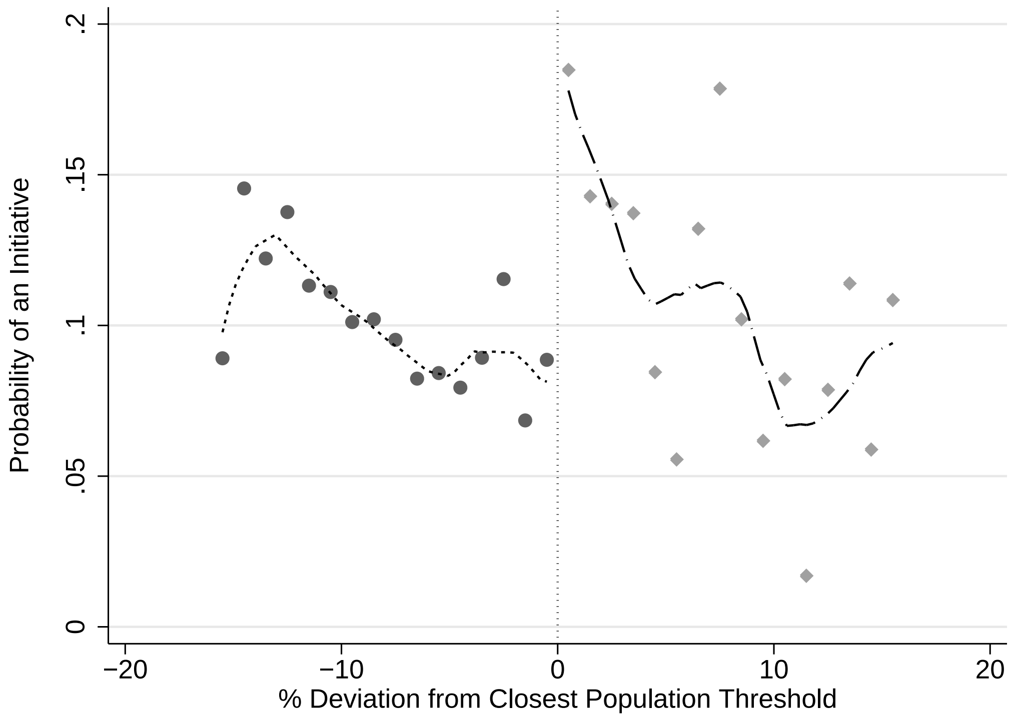
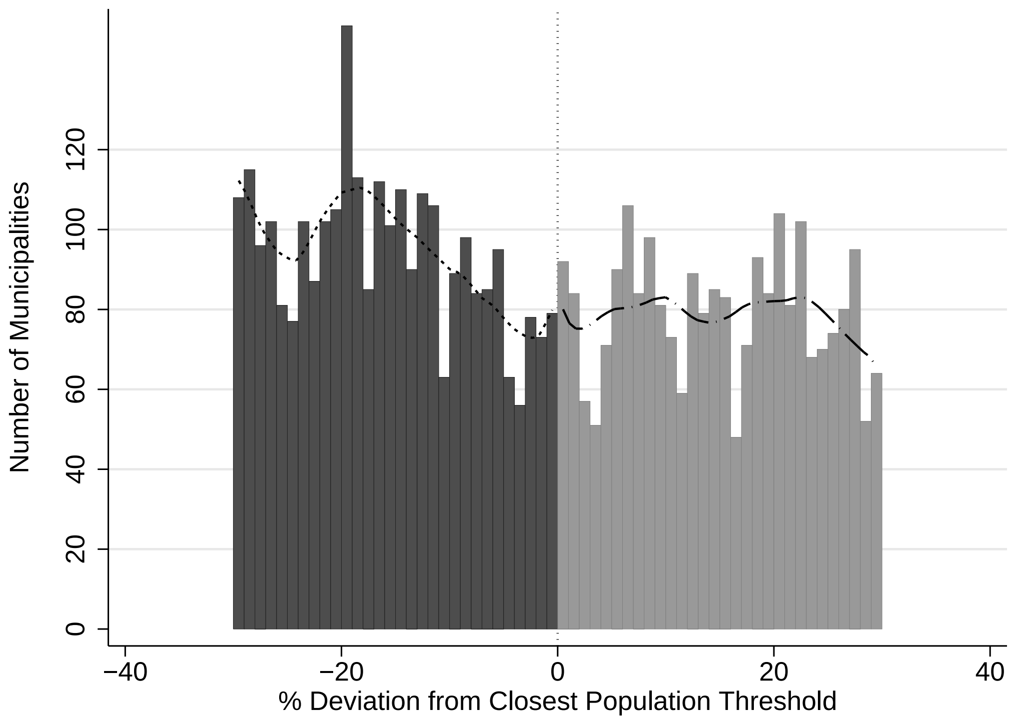


Figure 3: Density of the Assignment Variable around the Threshold



Notes: This figure presents the frequencies of observations in the data according to the deviation of the log population to the nearest population threshold. Each bin in the graph represents an interval of 1 percent. To illustrate any potential discontinuity at the threshold, we superpose a regression (based on local linear regressions) on each side of the threshold. We also calculated the test statistic suggested by McCrary (2008) and found no evidence for a sorting in our data (McCrary test statistic of 0.053 (0.115).). *Source:* Own calculations.

Table 7: RDD Validity - Predetermined Variables

	Predetermined variables from past election in 1990			
	Means in close observations		Pure difference	Estimated difference
	Close below	Close above		
	(1)	(2)	(3)	(4)
Number of valid votes in 1990	12697	14009	1311	-402
	(15090)	(15778)	(1163)	(2160)
Turnout in 1990	0.550	0.546	-0.004	-0.002
	(0.042)	(0.046)	(0.003)	(0.006)
Seats for CSU in 1990	21.32	19.76	-1.563	-3.543
	(38.66)	(38.72)	(2.917)	(5.893)
Seats for SPD in 1990	11.61	13.22	1.609	0.649
	(19.43)	(24.11)	(1.652)	(3.547)
Sample	5 %	5 %	5%	25%
Control function				3rd order

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The respective predetermined variables are indicated in the left column. The first and second column show the means (and standard deviations in parentheses) within the margin of 5% for towns below the threshold (column 1) and towns above the threshold (column 2). In column 3, we present the raw difference of those means. In column 4, we apply the above RDD methodology with the predetermined variable as an outcome variable. We choose a 3rd-order polynomial specification in the sample using the 25% window. *Source*: Own calculations.

Table 8: Robustness: Treatment Effect with Time Trend

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%
Treatment	0.270***	0.226***	0.241***
	(0.100)	(0.086)	(0.089)
Treatment · Time Trend	-0.025**	-0.021**	-0.020*
	(0.012)	(0.010)	(0.011)
Control function	Linear	Quadratic	Cubic
N	760	2236	3612

Notes: Standard errors in parentheses. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is a dummy that equals 1 if the municipality in question had at least one initiative. The treatment variable is defined as having a lower signature requirement.

Figure 4: Treatment Effect at Placebo Thresholds

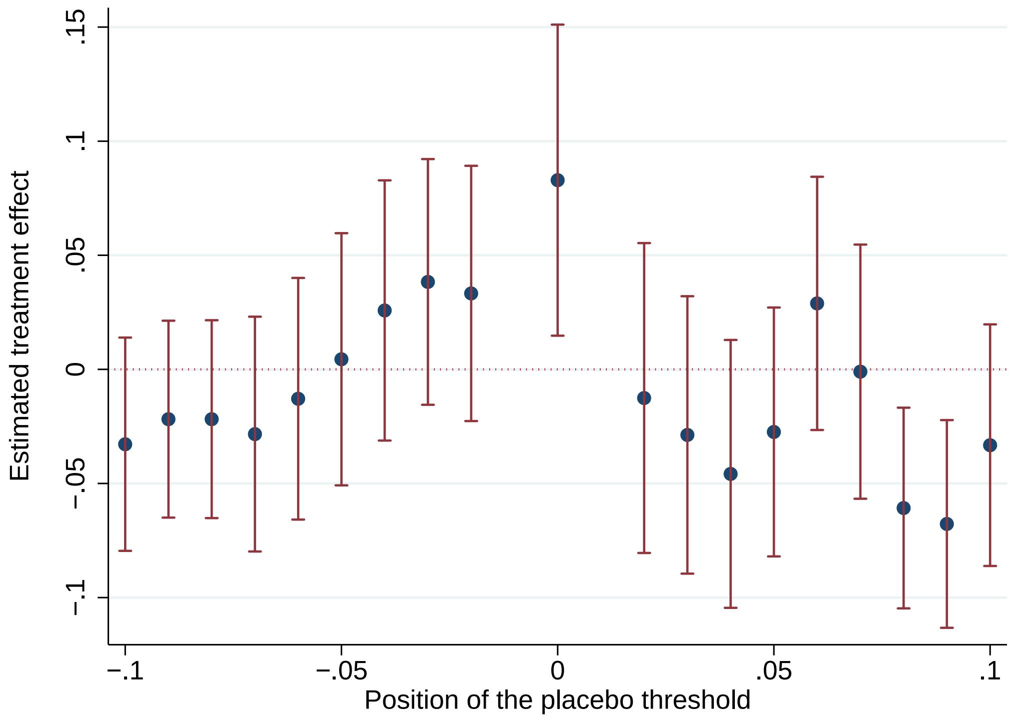


Table 9: Robustness: Treatment Effect for Different Window Sizes and Specifications

	(1) 5% Window	(2) 10% Window	(3) 15% Window	(4) 20% Window	(5) 25% Window	(6) 30% Window
Control Function	Panel 1: Parametric Specifications					
Linear	0.102**	0.078**	0.085***	0.042*		
	(0.047)	(0.033)	(0.026)	(0.023)		
Quadratic		0.097*	0.081**	0.101***	0.094***	
		(0.049)	(0.040)	(0.035)	(0.031)	
Cubic			0.109**	0.102**	0.102**	0.107***
			(0.054)	(0.047)	(0.042)	(0.038)
Bandwidth	Panel 2: Nonparametric Specifications					
Optimal Bandwidth	0.138**	0.098**	0.086**	0.083**	0.083**	0.084***
	(0.063)	(0.042)	(0.038)	(0.034)	(0.035)	(0.031)
Half Optimal Bandwidth	0.153	0.140**	0.130**	0.116**	0.117**	0.102**
	(0.093)	(0.065)	(0.057)	(0.048)	(0.050)	(0.043)
Twice Optimal Bandwidth	0.112**	0.081**	0.084***	0.058**	0.057**	0.041**
	(0.049)	(0.034)	(0.028)	(0.024)	(0.024)	(0.021)
N	704	1614	2477	3418	4316	5183

Notes: Standard errors in parentheses. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01. The dependent variable is a dummy that equals 1 if the municipality in question had at least one initiative. The treatment variable is defined as having a lower signature requirement.

Table 10: Robustness: Treatment Effect in North-Rhine Westphalia

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%	(4) RDD 25%
Treatment	0.020	0.012	0.025	0.014
	(0.038)	(0.031)	(0.032)	(0.022)
Control function	Linear	Quadratic	Cubic	Optimal Bandwidth
Specification	Parametric	Parametric	Parametric	Nonparametric
N	760	2236	3612	3612

Notes: Standard errors in parentheses. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01. The dependent variable is a dummy that equals 1 if the municipality in question had at least one initiative. The treatment variable is defined as having a lower signature requirement.

Table 11: Robustness: Leaving Out Individual Thresholds

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%	(4) RDD 25%
	Panel 1: Leaving out 10'000			
Treatment	0.172**	0.159**	0.210***	0.163***
	(0.084)	(0.070)	(0.073)	(0.056)
N	307	1230	2083	2083
	Panel 2: Leaving out 20'000			
Treatment	0.109**	0.097**	0.103**	0.089***
	(0.050)	(0.044)	(0.045)	(0.033)
N	562	1919	3393	3393
	Panel 3: Leaving out 30'000			
Treatment	0.108**	0.080**	0.102**	0.083**
	(0.047)	(0.041)	(0.042)	(0.034)
N	661	2315	4038	4038
	Panel 4: Leaving out 50'000			
Treatment	0.066	0.053	0.064	0.053*
	(0.045)	(0.039)	(0.040)	(0.028)
N	665	2380	4089	4089
	Panel 5: Leaving out 100'000			
Treatment	0.096*	0.056	0.089*	0.070**
	(0.052)	(0.045)	(0.047)	(0.035)
N	621	2064	3661	3661
Control function	Linear	Quadratic	Cubic	Optimal Bandwidth
Specification	Parametric	Parametric	Parametric	Nonparametric

Notes: Standard errors in parentheses. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01. The dependent variable is a dummy that equals 1 if the municipality in question had at least one initiative. The treatment variable is defined as having a lower signature requirement.

Table 12: Robustness: Effects at Placebo Thresholds of 1000, 2000, 3000 and 5000 Inhabitants

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%	(4) RDD 25%
Treatment	-0.015	-0.003	-0.003	-0.001
	(0.011)	(0.010)	(0.010)	(0.005)
Control function Specification	Linear Parametric	Quadratic Parametric	Cubic Parametric	Optimal Bandwidth Nonparametric
N	4152	12485	21346	21346

Notes: Standard errors in parentheses. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01. The dependent variable is a dummy that equals 1 if the municipality in question had at least one initiative. The treatment variable is defined as being above the (placebo) threshold.

Table 13: Robustness: Including Council Size, Spending and the Referendum Quota as Control Variables

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%
	Panel 1: Including Council Size		
Treatment	0.085*	0.077*	0.092**
	(0.048)	(0.042)	(0.043)
N	650	2299	4007
	Panel 2: Including Spending		
Treatment	0.095**	0.080**	0.100**
	(0.046)	(0.040)	(0.041)
N	704	2477	4316
	Panel 3: Including the Referendum Quota		
Treatment	0.097**	0.084**	0.106**
	(0.046)	(0.040)	(0.042)
N	704	2477	4316
	Panel 4: Including all three		
Treatment	0.114**	0.089**	0.109**
	(0.049)	(0.041)	(0.043)
N	650	2299	4007
Control Function	Linear	Quadratic	Cubic

Notes: Standard errors in parentheses. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is a dummy that equals 1 if the municipality in question had at least one initiative. The treatment variable is defined as having a lower signature requirement.

Table 14: Robustness: Council-Initiated Referenda as Dependent Variable

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%	(4) RDD 25%
Treatment	-0.004 (0.024)	0.012 (0.021)	0.009 (0.022)	0.010 (0.014)
Control function Specification	Linear Parametric	Quadratic Parametric	Cubic Parametric	Optimal Bandwidth Nonparametric
N	704	2477	4316	4316

Notes: Standard errors in parentheses. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is a dummy that equals 1 if the municipality in question held a referendum that was initiated by the city council. The treatment variable is defined as having a lower signature requirement.