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Electoral thresholds and the success of minor parties

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Abstract

We exploit a natural experiment to study the political consequences of explicit electoral thresholds. The natural experiment in question is an electoral reform in the German federal state of Hesse. In 2001, the state parliament abolished the five percent electoral threshold for local elections. The abolishment of the threshold had, on average, a stronger effect on municipalities with larger councils since *implicit* electoral thresholds are inversely correlated with council size. Using a dataset that includes all 426 Hessian municipalities over the period 1989 to 2011 and exploiting discontinuities in a state law that exogenously maps population to council size, we implement a difference-in-discontinuity design for identification. Our results show that the seat and vote shares of small parties increased in municipalities affected more strongly by the reform. These political effects are primarily due to the reform's psychological rather than the mechanical consequences. We also find that the reform had no effect on voter turnout. These findings suggest that abolishing an existing threshold improves the electoral prospects of smaller parties. It does, however, not increase voter participation.

Keywords: Electoral rules, electoral thresholds, voting

JEL codes: D70, D72, D78

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1 Introduction

A defining characteristic of a democracy is that minorities and special interests can form political parties and gain parliamentary representation. Excessive party competition, however, may lead to legislative fragmentation and political instability.[1] At the core of any electoral system is therefore a trade-off between political representation and legislative cohesion. To achieve legislative cohesion, many countries rely on majoritarian electoral rules. Majoritarianism often ensures that only a small number of parties are able to achieve legislative representation and thereby political influence.[2] As only the party with the largest vote share wins the seats awarded in a constituency, parties that cater to minorities are at a disadvantage and a few large parties typically dominate the political landscape.[3]

Proportional representation systems, on the other hand, call for alternative electoral rules to limit the effective number of parties (Rokkan, 1968).[4] Since electoral districts tend to be large – at times comprising the whole country – and multiple seats are awarded within each district, gaining a relatively small share of votes is sufficient for a party to achieve parliamentary representation. Countries with proportional electoral rules are hence by default more prone to fragmented party systems (Duverger, 1954; Lijphart, 1994; Rae, 1971). Many proportional countries therefore rely on legal electoral thresholds to limit political competition (Lijphart, 1991). Legal thresholds imply that a party may only receive seats in parliament if its overall vote share is above some fixed and relatively high

[1] A large literature studies how electoral laws are able to promote political stability. Lijphart (1968) provides a review of the early contributions.

[2] The US, for example, has a majoritarian system at both the federal and state tiers and only two effective political parties, the Democrats and the Republicans. There are, however, also instances where a majoritarian electoral system does not prevent the emergence of a larger number of effective parties. India is an example with 37 parties in the federal parliament after the 2009 elections.

[3] Seminal contributions on the link between electoral rules and political representation are Duverger (1954) and Rae (1971). Duverger (1954) argues that plurality voting favors a bipartisan system. However, this Duverger's Law has over the years been subject to criticism and been revised to accommodate for several exceptions (Riker, 1982).

[4] The concept of effective parties refers to the number of parties in parliament. It was introduced by Laakso and Taagepera (1979) and is a standard measure of political fragmentation (Lijphart, 1990; Taagepera and Laakso, 1980; Taagepera, 1989).

value. Specific values vary among countries: at the national level, Sweden has a threshold of four, Germany five, and Turkey ten percent.

Despite the presumption that legal electoral thresholds reduce the electoral chances of small parties and thereby foster legislative cohesion, there is little evidence of their causal effect on political outcomes. Existing studies overwhelmingly use cross-country variation and employ empirical methodologies that rely on selection on observables which may lead to biased estimates due to omitted variables.[5] To fill this gap in the literature, we revisit the question of whether electoral thresholds have a causal effect on political outcomes while relying on a credible source of exogenous variation: an institutional reform in the German state of Hesse that involved the abolishment of the five percent legal electoral threshold for local elections as of 2001 combined with a state law that maps population size to council size.[6]

Many countries have held acrimonious debates about the benefits and drawbacks of electoral thresholds, and several of them have adjusted their thresholds recently. In 2014, Germany's federal constitutional court ruled that the three percent electoral threshold for

[5] Haggard and Kaufman (1997) for instance, claim that the electoral threshold in Turkey is an exclusionary mechanism to diminish the electoral prospects of smaller parties. Likewise, Calvo and Micozzi (2005) show how in several Argentinian provinces incumbents relied on electoral thresholds to limit electoral competition. Gebethner (1997) describes the introduction of electoral thresholds in both Romania and Poland in 1991 and 1993, respectively, as means to avoid excessive fragmentation of party politics. Moser (1999) finds that electoral thresholds in newly democratized Eastern-European countries reduce party fragmentation. Moser and Scheiner (2004) study the same question with a larger dataset and find a statistically insignificant effect of thresholds. Remmer (2008) explores the impact of electoral reforms in 18 Latin American countries and finds mixed evidence. Carey and Hix (2011), using a broad dataset of 609 elections in 81 countries, finds some evidence that electoral thresholds reduce party fragmentation. One exceptions is Vatter (2003) who uses subnational data at the level of the Swiss Cantons to find that legal electoral thresholds have no effect on the number of parties represented in parliament.

[6] Our paper hence follows a relatively new literature that uses natural experiments to estimate the causal effect of electoral rules on political outcomes. For example, Fiva and Folke (2014) explore how different methods to map votes to seats in parliamentary systems (d'Hondt vs. a modified Sainte-Lagüe method) affect political outcomes by exploiting a reform in Norway. Fujiwara (2011) uses a regression discontinuity design to establish the effect of single- vs. dual-ballot plurality systems on the vote share of third placed candidates. The setting is Brazilian municipalities and exogenous variation is provided by an electoral law that discontinuously assigns municipalities different voting systems according to their population size. His reduced-form results suggest that third placed candidates receive more votes in dual-ballot systems. Fujiwara (2011) provides further evidence indicating that strategic voting is responsible for the reduced-form effect.

European Union elections is unconstitutional. In New Zealand, the Electoral Commission recommended that the party vote share threshold should be lowered from five to four percent.[7] In Israel, on the other hand, the electoral threshold for the Knesset (national parliament) was continuously increased ever since the country was founded; the highest increase so far occurred as recently as April 2014.[8] It is likely that other countries will hold similar discussions and implement similar reforms in the future. Against this background, it is important to obtain credible causal evidence on the political effects of electoral thresholds. Even though our paper offers only evidence from one relatively small German State, our results are likely to generalize to other developed countries.

The main features of the Hessian electoral reform are the following. Prior to the electoral reform, a party had to win at least five percent of votes in a municipality in order to achieve representation. After the reform, there is no longer an explicit legal electoral threshold. There remains, however, an implicit electoral threshold since parties still need to garner a minimum vote share in order to gain their first seat.[9] Implicit electoral thresholds vary throughout municipalities since their specific value depends on the total number of seats in the municipal council. Whereas in small municipalities the implicit threshold can be as high as the former explicit one, in large municipalities it can be as low as 0.5 percent. Hence, the abolishment of the legal electoral threshold affected municipalities differently, with a stronger effect upon larger ones.

Since municipal council sizes are linked through a state law to municipal population sizes we are able to exogenously map treatment intensity to population brackets in the post-treatment period. Hence, we can use exogenous heterogeneity in treatment intensity to identify the causal effect of electoral thresholds on three main political outcomes: voter

[7] See the Electoral (Adjustment of Thresholds) Amendment Act 2013. The commission additionally recommended that the "one electorate" seat threshold for the allocation of list seats should be abolished.

[8] The explicit electoral threshold increased from 2 to 3.25%.

[9] There are several papers that study the effect of implicit thresholds on political representation, see for example Rokkan (1968), Rae et al. (1971) and Lijphart and Gibberd (1977).

turnout, party seat shares in the local council, and party vote shares. Our sample consists of all 426 Hessian municipalities from 1989 to 2011. This period encompasses three local elections prior and three local elections after the electoral reform. The identification strategy is based on an approach that combines difference-in-difference (diff-in-diff) and regression discontinuity (RD) methods, the difference-in-discontinuity design (diff-in-disc) (Grembi et al., 2012). The idea underlying the diff-in-disc design is to focus on changes in political outcomes only in municipalities close to the population cutoffs at which municipal council sizes are by law allowed to increase, before and after the reform. By combining these two sources of variation, just below and just above the population cutoffs as well as before and after the electoral reform, the estimates are robust to potentially omitted variables, co-treatment, and differential trends.

The diff-in-disc estimates suggest no causal effect of the treatment on voter turnout. On the other hand, there is evidence that the seat and vote shares of small parties increased in municipalities that were exposed to stronger treatments. The seat and vote shares of larger parties decreased accordingly. Furthermore, the increase in the seat share of small parties appears to be a consequence of changing voting patterns rather than a mechanical effect of the abolishment of the legal threshold. As we show in a companion paper, a possible explanation for why mechanical effects were relatively small is that municipalities reduced the number of seats in the council after the electoral reform, which led to an increase in implicit thresholds (Baskaran and Lopes da Fonseca, 2014).

2 Institutional Details

2.1 Local politics

Hesse is divided into 426 municipalities.[10] All municipalities have two important local political institutions: the municipal council and the mayor. The council is the more important institution and the subject of our investigation.[11] Municipal council elections take place at the same date statewide and are contested by several parties.

For the purpose of our analysis, we divide the contesting parties into three categories: large, medium and small. We define the seat and vote share of large parties as the aggregate seat and vote share, respectively, of the two largest national parties, the center-right CDU and the center-left SPD. These two parties usually receive around 30% of the votes in state and national elections. The seat and vote shares of medium parties is the aggregate seat and vote share of the smaller national parties: the market liberal FPD and the environmentalist Green Party. These two parties usually receive up to 10% percent of the votes in national and state elections and are well established in the political mainstream.

Finally, many local and several very small national parties run in municipal elections. We refer to this group of parties as small since they often struggle with the five percent legal electoral threshold. However, among these small parties municipal specific voter initiatives (*Wahlvereinigigungen*) are popular in some municipalities and often receive a substantial fraction of the votes.[12] As there are many small parties, we define in the following the seat and vote share of small parties simply as 100% minus the seat or vote shares of the large and medium parties, respectively.

[10] Figure 1 shows population sizes across the 426 Hessian municipalities

[11] Even though the Hessian mayors are directly elected since the mid-nineties, most important local decisions are still made by the local council. See Hessami (2014) for details on the mayoral office in Hesse.

[12] Note that the label "small" is therefore not always accurate. We use this label for simplicity, but in some cases the "small" parties can be very large. In effect, "small" should be understood as a shorthand for "party that is not important at the national level".

2.2 The *Kommunalwahlreform*

In 1999, the Hessian state parliament passed a law that fundamentally changed the rules that governed local elections from 2001 onward (*Kommunalwahlreform*). First, the length of the legislative period was extended from four to five years. Second, a new voting system called *Kumulieren und Panaschieren* was introduced. Prior to the reform, voters would cast a single vote for their preferred party list. In the new system, voters can cast as many votes as there are seats in the council. They can accumulate up to three votes for an individual candidate or give all their votes to a certain party list. In addition, voters can also drop individual candidates from the list. Third, the five percent legal electoral threshold was abolished. Parties could enter the local council as long as their vote share was sufficient to gain at least one seat.

Unlike the first two elements of the reform, which affected all municipalities equally, the abolishment of the legal electoral threshold had heterogeneous effects. The reason for the effect's heterogeneity is the existence of implicit thresholds. These differ across municipalities and depend on their council size. Intuitively, without an explicit threshold the vote share required to gain at least one seat in a 100 seats council is lower than the one needed to gain the same seat in a 10 seats council (ca. 0.5% vs 5%).[13] Therefore, the abolishment of the legal threshold mattered less for municipalities with smaller councils, both in terms of changing the electoral incentives of voters and in terms of how votes are mapped to seats in the council. This heterogeneity in treatment intensity forms the core of our identification strategy.

[13]The actual value of the implicit threshold for a given party is endogenous and depends inter alia on the vote shares of all other parties. Typically, a vote share that is sufficiently large for half a seat entitles a party to a full seat in the council. See `http://www.wahlrecht.de/kommunal/hessen.html`.

2.3 Electoral thresholds and political outcomes: hypotheses

The expectations underlying the electoral reform was that it would lead to a citizen driven rather than party driven political system at the local level (Vetter, 2009). Anticipated effects included an increase in political participation and competition. Despite these expectations, the consequences of the abolishment are actually ex-ante ambiguous. On the one hand, municipalities with smaller implicit thresholds could experience an increase in turnout after the abolishment since core supporters of minor parties, who may have previously abstained because their preferred party had little chance to enter the council, could finally find it worthwhile to participate in local elections. On the other hand, some supporters of minor parties may also feel that after the reform, their preferred minor party does not depend on their vote to overcome the electoral threshold. Since their participation is no longer deemed as crucial, they might abstain. The overall effect of the reform on turnout is hence ex-ante unclear.

We put forward equally contradictory hypotheses regarding the impact of the reform on party seat and vote shares. *Ceteris paribus* having no legal electoral threshold should mechanically increase the seat share of small parties. However, there are a number of caveats. First, voting patterns might not remain fixed (Moser and Scheiner, 2004). There might be a "psychological" response to the reform (Duverger, 1954; Fiva and Folke, 2014). Vote shares might, on the one hand, change to the benefit of the small parties. Rather than abstaining, supporters of small parties might have chosen to vote for one of the more established parties if there was a non-negligible chance that their preferred small party would fail to overcome the five percent legal threshold prior to the abolishment. Once the threshold was abolished, voting for their most preferred small party might have become more worthwhile for this subset of the electorate (Perea, 2002). On the other hand, voting patterns might change such as to decrease the vote share of small parties. For instance, core supporters of smaller parties might not participate in the election because they believe

that their vote is not as crucial anymore to ensure that their preferred party enters the council. Strategic voting, e. g. supporters of larger parties voting for some smaller parties to facilitate a specific composition of the council, might also decrease, leading to a decline in the vote and seat shares of smaller parties.

3 Empirical strategy

3.1 Difference-in-discontinuity design

There are several attractive features to studying electoral thresholds in our local government setting. First, localities within one sub-federal state are relatively homogeneous (compared to e. g. countries), which reduces the possibility that unobservable heterogeneity leads to biased estimates. Second, the sample size is much larger than in cross-country studies, leading to more precise estimates. Third, the abolishment of the electoral threshold was an exogenous intervention from the perspective of the municipalities as it was imposed by a higher tier of government.

Two additional features of our setting make the identification particularly credible. First, according to a state law, council size in Hesse is a positive and discontinuous (albeit fuzzy) function of municipal population size. Consequently, implicit thresholds and hence treatment intensity – the extent to which a municipality was affected by the abolishment – vary exogenously and discontinuously with population size as well. The maximum and minimum council sizes for each population bracket according to the law are listed in Table 1.[14] Figure 2 plots mean council size for each of the population brackets defined in Table 1 in the pre- and post-treatment period. It is obvious that mean council size is increasing

[14]The law states that council size brackets are determined by the latest available population data when the date for next local election is fixed. This population data is not the same as the annual data published by the state statistical office. For the elections of 2006 and 2011, we obtained the relevant data from the homepage of the statistical office. For the previous elections, we collected the data by hand from various issues of the Hessian government gazette.

between the different brackets, with municipalities typically choosing the largest possible council size in the pre-treatment period.[15] More generally, the probability of a larger council increases discontinuously at the population cutoffs. Consequently, the treatment intensity of abolishing the five percent legal threshold increases discontinuously as well. Thus, a given municipality with e. g. 3000 inhabitants will be affected less by the abolishment of the legal electoral threshold than municipalities with 3001 inhabitants because the former will choose on average smaller councils and thus have larger implicit thresholds.

The second feature that we use for identification is that given the nature of the electoral reform, we have a pre- and post-treatment period: since the legal electoral threshold existed only before 2001, we can rely on within- and not only on between-variation along the population brackets for identification. The presence of these features in our setting, the discontinuities at population cutoffs combined with the electoral reform, enables us to focus on changes in political outcomes at the population cutoffs between the pre- and post-treatment periods, thereby effectively combining RD and diff-in-diff methods. The RD aspect of this approach allows us to control for observable as well as unobservable characteristics of municipalities that may result in differential trends and thereby invalidate a traditional diff-in-diff design. The diff-in-diff aspect addresses concerns regarding co-treatment and manipulation at the relevant population cutoffs which may invalidate a standard RD design (Ade and Freier, 2011). Combining the RD and diff-in-diff designs hence results in a novel design, called difference-in-discontinuity (diff-in-disc), that leads to a 'as good as random' analysis even in settings where either differential trends or co-treatment and manipulation at the population cutoffs cannot be ruled out (Grembi et al., 2012). Furthermore, as the abolishment of the legal electoral threshold was the only aspect

[15]Two municipalities in 1989 have larger council sizes than permissible given their population size (one had 4999 inhabitants and a council size of 31 and the other 9754 and a council size of 37). We drop these two observations from the sample. While we have no definite explanation, we suspect that these two municipalities made use of an exception defined in the Hessian law for local elections that allows municipalities that crossed either of the thresholds from above to keep the council size intended for municipalities in the next threshold for another legislative period.

of the reform having a different impact on municipalities with different population sizes, we are able to isolate its causal effect from the impact of the other changes included in the *Kommunalwahlreform* (e. g. the lengthening of the legislative period from four to five years).[16]

The effect of a stronger treatment at M, the natural log of a given cutoff, can be specified in the context of a diff-in-disc model as:

$$\begin{aligned}\hat{\delta}^M &\equiv \hat{\gamma}^M_{t\geq T} - \hat{\gamma}^M_{t<T} \\ &= \lim_{NLPOP_i \downarrow M} E[y_{i,t}|NLPOP, t \geq T] - \lim_{NLPOP_i \uparrow M} E[y_{i,t}|NLPOP, t \geq T] \\ &- \left(\lim_{NLPOP_i \downarrow M} E[y_{i,t}|NLPOP, t < T] - \lim_{NLPOP_i \uparrow M} E[y_{i,t}|NLPOP, t < T] \right),\end{aligned} \tag{1}$$

where $y_{i,p}$ is each of the political outcomes under study and $NLPOP$ the normalized value of the natural log of population size in municipality i. Population size is normalized by subtracting the log of population $LPOP$ from the log of the threshold, i. e. $NLPOP = LPOP - M$. $\hat{\gamma}^M_{t\geq T}$ and $\hat{\gamma}^M_{t<T}$ are the estimates of the outcomes variables at the cutoff without and with the legal threshold, i. e. in the post- and pre-treatment period, respectively. We are interested in δ^M, the estimate for the treatment effect, which captures the change in the effect of the discontinuity at M between the pre- and the post-treatment periods.

The treatment effect $\hat{\delta}^M$ can be obtained with the following model in a regression framework:

[16] Specifically, the estimates are robust to heterogeneous responses to the other changes in the electoral law. That is, since the analysis is confined to municipalities that are above and below the respective thresholds, the response to the other changes in "treatment" and "control" municipalities will be identical in expectation.

$$
\begin{aligned}
y_{i,t}^{M} =& f(NLPOP) + D_i(\gamma_{t\geq T}^{M} + f(NLPOP)) + I_t(\alpha + f(NLPOP)) \\
&+ D_i(\delta^{M} I_t + I_t f(NLPOP)) + \epsilon_{i,t} \text{ if } |NLPOP| < h,
\end{aligned} \tag{2}
$$

where D_i and I_t are dummy variables, indicating the assignment to treatment and the post-treatment period, respectively. $f(NLPOP)$ is a flexible polynomial of normalized population size which is allowed to have different slopes to the left and right of the normalized population cutoff M and in the pre- and post-treatment periods.

We motivate the diff-in-disc model above by referring to a single normalized population cutoff M. In our case, there are multiple cutoffs at which council size is allowed to change. Rather than analyzing all cutoffs individually, we follow in the baseline regressions the previous literature that uses the RD methodology with multiple population cutoffs and normalize all observations such that they are around a single one (Egger and Koethenbuerger, 2010). This approach has the advantage of a larger sample size. Also, the results can be presented more compactly. In robustness tests, however, we also report results for individual cutoffs. We also follow the previous literature and include in all regressions municipality and election year fixed effects to improve efficiency and reduce finite sample bias (Hoxby, 2000). We estimate this model by local polynomial regression. Our baseline results use a cubic specification and we report results for various bandwidths h around the normalized cutoff. More specifically, we use the following bandwidths: 0.4, 0.5, 0.6 and 0.7.

As indicated above, the identifying assumptions in the diff-in-disc design are arguably less strict than in the RD design. We do not require that there is no co-treatment at the cutoffs. Instead, we only require that the effect of any co-treatments remains constant between the pre- and post-treatment periods. Another assumption that must hold is that the ability or incentives of municipalities to manipulate population size at the cutoff did

not change over the pre- and post-treatment periods. This assumption is plausible as it is unlikely that municipalities would persistently misrepresent their population sizes only to avoid being forced to change their council sizes. A McCrary (2008) style density plot reported in Figure 3 also fails to indicate that incentives for manipulation changed from the pre- to the post-treatment period at the normalized cutoff.[17]

3.2 Diff-in-Disc plots

As a complement to our regression results, we present graphical evidence on the treatment effect based on the specification in Equation 2. We construct the diff-in-disc plots by first dividing normalized population size into bins of size 0.001 within a window of 0.2. Then we calculate the average of the relevant outcome variable y within each bin for the pre- and post-treatment period, i. e. $\overline{y}_{b,t}$ with the index $b = 1, ..., 200$ denoting the bin and $t = 0, 1$ denoting the pre- and the post-treatment period. Then we obtain the difference within each bin in the pre- and post-treatment period $\Delta y_b = (\overline{y}_{b,1} - \overline{y}_{b,0})$. Finally, we plot Δy_b against $NLPOP$ to the left- and the right of the normalized cutoff.

To observe whether there is a discontinuity at the normalized cutoff, we smooth Δy_b with a local polynomial plot of quadratic degree and a bandwidth of 0.1 at both sides of the threshold, using a rectangular kernel and the number of observations within each bin as frequency weights. For presentational purposes, we use the average of the differences for bins of width 0.01 (rather than for the original bin widths). The smooth is constructed based on the original Δy_b, however.

[17] The idea underlying this plot is that if either the ability or the incentives for manipulation changed at the cutoff from the pre- to the post-treatment period, we should observe a discontinuity in the changes in the number of observations close to the cutoff. More specifically, assume that because of the treatment, municipalities systematically start to (mis-) report lower population sizes in order to be able to reduce their council size. Then the increase in observations just below the normalized cutoff from the pre- to the post-treatment period should be significantly higher than the increase in observations just above the threshold.

4 Baseline Results

4.1 Turnout

Figure 4 shows the diff-in-disc plot for the voter turnout. There is no significant discontinuity at the normalized cutoff. The corresponding regression results on the first column of Table 2 confirm this assessment. Coefficient estimates are close to 0 for all bandwidths and insignificant. Overall, it appears that there is no relationship between a stronger exposure to the abolishment of the electoral threshold and voter turnout.

4.2 Seat and vote shares

Figure 5 shows the diff-in-disc plots for the aggregate seat and vote shares of the small parties. In both graphs, there is a noticeable discontinuity at the normalized population cutoff, suggesting a causal increase in the seat and vote shares of the small parties due to the abolishment. In Table 2, we present the corresponding diff-in-disc regressions. The coefficient estimates are consistently positive and always significant across bandwidths.

For both seat and vote shares of small parties, the estimated coefficients are around 3 to 4. As the weighted average increase in treatment strength – the decline in the implicit electoral threshold – at the normalized population threshold is around 0.46, the estimated treatment effects imply that the increase in the seat and vote share of small parties in municipalities with a one percentage point lower implicit threshold after the reform is around 6 to 8 percentage points, respectively.[18]

[18]Crossing the population threshold at 3001 from below implies on average a reduction in the implicit threshold from about 3.33 to 2.17 percentage points, assuming all municipalities choose the highest possible council size. Hence, the intensity of treatment from abolishing the explicit threshold increases by around 1.16 percentage points at the 3001 threshold (recall that the implicit threshold for the first seat is a sufficiently large vote share to gain half a seat) At the next threshold of 5001, the implicit threshold decreases from around 2.17 to 1.61 percentage points. The intensity of treatment increases by around 0.56 percentage points. The same argument applies for all further thresholds. We weight the increase in treatment strength at each threshold with the number of observations within each population bracket when calculating the average size of the treatment.

As a counterpart to the results described above we look into the effect of the abolishment of the legal threshold on the aggregate seat and vote shares of the medium and large parties. Figure 6 shows the diff-in-disc plots for the change in the seat and vote shares of medium parties. There is no discontinuity at the normalized population cutoff. Figure 7 provides the same graphs for large parties. Both subfigures show a negative discontinuity at the normalized cutoff. Thus, it appears that small parties gained seat and vote shares mainly at the expense of large parties.

The corresponding regression results can again be found in Table 2. Coefficient estimates are consistently negative for both the seat and vote shares of medium as well as large parties. Naturally, the combined losses of the medium and large parties match the gains in the seat and vote shares of the small parties. Coefficient estimates are larger for the large parties, particularly for vote shares. Abolishing an electoral threshold in municipalities with a one percentage point lower implicit threshold appears to decrease the seat share of large parties by about 5 to 6 percentage points but cuts the vote share of large parties up to 7 to 8 percentage points.

All in all, these results suggest that small parties did indeed benefit from the abolishment of the five percent legal electoral threshold. They gained both vote and seat share. However, the effect was somewhat stronger for vote than for seat shares.

4.3 Discussion

That the vote shares of small parties increased by a larger amount than their seat shares (and conversely that the seat shares of larger parties decreased less than their vote shares) indicates that the causal impact of the abolishment of the legal electoral threshold was driven primarily by psychological rather than mechanical effects. The rationale for this assessment is as follows. If some small parties enter the council for the first time due to the mechanical effects of the reform, we would expect that the small parties' seat share

increases more than their vote share. However, we observe the opposite. This pattern is to be expected if the small parties benefiting from the treatment were already present in municipal councils before 2001. We indeed show further below that the municipality-specific voter initiatives, who had a strong presence in many councils prior to the reform, were the main beneficiaries of abolishing the electoral threshold in terms of vote and seat shares.

Similarly, an intriguing aspect of baseline findings is that the loss in seat shares of the large parties was less than proportional to their loss in vote shares, suggesting that the mechanical effects of the reform were small. We provide in a companion paper an explanation for this apparent paradox (Baskaran and Lopes da Fonseca, 2014). We show in this paper that Hessian municipalities for which the abolishment of the electoral threshold mattered more reduced their council sizes more strongly. Decreasing council sizes entails an increase in implicit thresholds, which would counteract the impact of the treatment on vote shares, and thereby dampen the effect on seat shares. It hence appears that the mechanical effects failed to materialize because the established parties manipulated council size.

5 Robustness tests

5.1 Polynomials and Bandwidths

In order to test the robustness of the main results described in the previous section we estimate Equation 2 relying on smaller bandwidths h (0.06, 0.07, 0.08, 0.09, 0.1, 0.2) and only a linear polynomial. Regression results are grouped in Table 3. Results for voter turnout are largely negative but statistically insignificant. Coefficient estimates for the seat and vote shares of small parties remain consistently positive and are always significant. Conversely, coefficient estimates for the seat and vote shares of medium and large parties

are always negative. This first robustness test hence supports and reinforces our baseline results. We find significant effects for the average change in vote and seat shares of small and large parties. The results are also stronger and of a higher magnitude for vote than seat shares.

5.2 Placebo tests

As a first set of placebo tests, we let the treatment set in at fake cutoffs and compare the estimated effects with the ones obtained for the correct cutoff. More specifically, we define D_i in Equation 2 such that it is 1 if $NLPOP = -2, -1, 0, 1, 2$ ($NLPOP = 0$ indicates the true threshold). To save space, we summarize the results in graphs. The structure of the plots in Figures 8, 9, 10, and 11 is as follows. For each fake cutoff, we plot the four coefficient estimates obtained by combining the bandwidths and bin sizes used in the baseline regressions. We also indicate the mean value of the coefficient estimates with a red dot.

Figure 8 displays the placebo estimates for voter turnout. For all cutoffs, fake and true, the mean coefficient estimates revolve around zero. In Figure 9, we present the placebo estimates for the seat and vote shares of the small parties. The mean coefficient estimates revolve around zero at every fake cutoff. At the true cutoff, however, there is a large positive jump of the mean coefficient of the seat and vote shares of the small parties. Figure 10 collects the placebo tests for the seat and vote shares of medium parties. Both graphs show that the mean coefficient estimates revolve around zero for the fake and true cutoffs. Finally, Figure 11 presents the same exercise for the seat and vote shares of large parties. In both graphs there is evidence of a noticeable negative jump in the mean coefficient estimate at the true cutoff, while estimates for the fake cutoffs are on average close to zero.

As a second set of placebo tests, we let the treatment begin at a fake treatment year. That is, we limit the sample to the pre-treatment period (1989-1997) and let the treatment

begin in 1993. Figure 12 collects the results. The mean coefficient at the fake cutoffs for each of the outcome measures is indicated with a red dot. For comparison, we indicate the mean estimate at the true cutoff with a blue triangle. Again, we find that the mean coefficient estimates for voter turnout and the seat and vote shares of the medium parties are around zero both at the fake and the true treatment year. For the remaining variables, the graph shows that mean estimates for the fake treatment year are always close to zero, whereas estimates for the true treatment year are further away from zero. Overall, both the placebo test for the fake cutoffs and for the fake treatment year provide further robustness to our baseline estimates

6 Extensions

6.1 Individual cutoffs

Having established the robustness of the baseline results, we extend them in several directions. First, in order to assess whether the baseline results are driven by selected cutoffs, we report results for individual cutoffs. For compactness and since sample sizes are smaller in these regressions we only report results for specifications with a relatively large bandwidth of 0.5 and a quadratic control function. Even though treatment increases with council size, it is at the smallest population brackets that increase in treatment intensity is the largest. Also, the number of observations in the larger brackets is limited. Therefore, here we focus on the first four population cutoffs.

The results are collected in Table 4. Coefficient estimates of the treatment effect at the individual cutoffs for the voter turnout are, in line with previous results, always insignificant. Results for the seat and vote shares of small, medium and large parties also follow in general the baseline results. These results are never significant at conventional values, but this finding is presumably due to the small sample size in these regressions, since the

estimated coefficients are numerically large. According to Baskaran and Lopes da Fonseca (2014) council sizes in municipalities falling into the first population cutoff did not decrease significantly (only larger municipalities reduced their council sizes). It is thus interesting to see that at that same cutoff the increase in the seat share of small parties is more than proportional to the increase in the vote share. The same holds for the decrease in the seat share of large parties which is higher than the decrease in vote share, though to a smaller degree. In the remaining cutoffs where council sizes did significantly decrease we see the contrary.

6.2 Seat shares for individual parties

As an extension of the results for aggregated seat and vote shares, we report in Table 5 the effect of the abolishment of the electoral threshold for individual parties. As expected, the coefficient estimates for the large parties, CDU and SPD, are consistently negative. Yet, they are only significant and of a large magnitude for the CDU. The coefficient estimates for the medium parties, FDP and the Greens, are also negative, but of a much smaller magnitude than for the CDU and never significant.

Finally, we look at municipal-specific voter initiates. As expected, the estimated coefficients for the vote shares of voter initiatives are consistently positive and of a similar magnitude of those obtained for the vote shares of the small parties in the baseline regressions. Voter initiatives, already popular in Hesse, appear thus to have gained the most from the abolishment of the legal electoral threshold, even if the estimated coefficient is only significant for a relatively narrow bandwidth of 0.4.

6.3 Council fragmentation

As a last extension of the results we study the effects of the treatment on council fragmentation. For that purpose we rely on three different measures of council fragmentation: the number of parties in the municipal council, the seat share held by the largest party in the council, and an inverse Herfindahl index.

The regression results are collected in Table 6. There is no significant impact of the abolishment of the legal electoral threshold on council fragmentation, for none of the variables across all bandwidths under analysis. Coefficient estimates are overall positive for both the change in the number of parties in council and the inverse Herfindahl index, and negative for the maximum share of the largest party in council. However there is no robust evidence of a significant impact of the reform on any of the variables. These results imply that council fragmentation did not increase after the reform despite the gain in seat shares of the small parties. This finding is in turn consistent with the notion that only selected small parties, i. e. the voter initiatives, benefited from the reform.

7 Conclusion

We study the relevance of electoral thresholds. Our results indicate that abolishing an explicit electoral threshold increases the seat share of smaller (i. e. non-mainstream) parties at the expense of more established parties. Further analysis indicates that the seat gains of small parties correspond to a similar increase in their vote shares. It hence seems that the expected mechanical effects of the reform failed to materialize. Psychological effects seem to be primarily at play. As suggested by a companion paper, the explanation for a lack of mechanical effects appears to be that established parties reduced council sizes and thereby raised implicit thresholds (Baskaran and Lopes da Fonseca, 2014). Our baseline

results along with the evidence regarding the effect of the individual cutoffs suggest that this explanation is plausible.

Overall, electoral thresholds do appear to have a causal effect on political outcomes. On the one hand, they seem to be a suitable means to achieve legislative cohesion by ensuring that non-mainstream parties receive only a relatively small share of the seats in the legislature. By the same token, however, electoral thresholds reduce the legislative voice of minorities. While we have no normative recommendations regarding the desirability of electoral thresholds, policy makers and voters should be aware that electoral thresholds entail a strong trade off between legislative cohesion and political representation.

Acknowledgments

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Table 1: Population cutoffs for the number of seats in Hessian municipal councils

Population	Council size	Observations
1–3000	11-15	219
3001–5000	15-23	473
5001–10000	23-31	874
10001–25000	31-37	779
25001–50000	37-45	137
50001–100000	45-59	42
100001–250000	59-71	18
250001–500000	71-81	6
500001–1000000	81-93	6
$>$ 1000000	93-105	-

Notes: This table collects the population cutoffs at which municipalities may increase their council size. Municipalities are allowed to choose smaller council sizes. However, the number of seats must be at least as large as the maximum council size allowed for municipalities in the next lower population bracket. For example, municipalities between 5001 and 10000 inhabitants may have up to 31 council seats but must have at least 23 seats.

Table 2: The effect of the abolishment of the electoral threshold on turnout and seat and vote shares. Baseline results.

	BW=0.4	BW=0.5	BW=0.6	BW=0.7
Turnout	-0.141	-0.271	0.119	1.029
	(0.802)	(0.740)	(0.657)	(0.668)
Small parties				
Seat Share	4.913**	3.711*	3.682**	3.027*
	(2.387)	(2.250)	(1.807)	(1.742)
Vote Share	5.028**	3.834*	3.690**	3.143*
	(2.333)	(2.206)	(1.773)	(1.706)
Medium parties				
Seat Share	-2.152	-1.315	-0.589	-0.504
	(1.539)	(1.369)	(1.212)	(1.189)
Vote Share	-1.493	-0.490	-0.022	0.070
	(1.418)	(1.257)	(1.118)	(1.092)
Large Parties				
Seat Share	-2.761	-2.395	-3.093*	-2.523
	(2.253)	(2.138)	(1.742)	(1.673)
Vote Share	-3.536	-3.345	-3.669**	-3.213**
	(2.208)	(2.092)	(1.700)	(1.623)
N	2797	3496	4208	4917

Notes: This table presents diff-in-disc regressions for voter turnout and the seat and vote shares of the small, medium and large parties in Hessian municipal councils. All population cutoffs at which council size is allowed to change are analyzed simultaneously by normalizing population size. Estimates for the average treatment effect of abolishing the election threshold are reported for different bandwidths (0.4, 0.5, 0.6, 0.7) and a cubic polynomial of normalized log population size. Municipality and legislative term fixed effects are included in all models. Standard errors are clustered at the level of a municipality and robust to heteroscedasticity. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

Table 3: The effect of the abolishment of the electoral threshold on turnout and seat and vote shares. Robustness tests with smaller bandwidths.

	BW=0.06	BW=0.07	BW=0.08	BW=0.09	BW=0.1	BW=0.2
Turnout	-0.821	-0.569	-0.387	-0.107	-0.461	0.629
	(1.105)	(0.923)	(0.811)	(0.740)	(0.733)	(0.628)
Small Parties						
Seat Share	4.060	4.817*	4.592*	5.154**	3.634*	3.546**
	(2.982)	(2.766)	(2.396)	(2.220)	(2.101)	(1.664)
Vote Share	4.686	5.303**	5.093**	5.368**	3.843*	3.580**
	(2.931)	(2.700)	(2.356)	(2.188)	(2.091)	(1.639)
Medium Parties						
Seat Share	-0.665	-1.138	-1.646	-1.422	-1.201	-1.485
	(1.986)	(1.816)	(1.669)	(1.569)	(1.513)	(1.105)
Vote Share	-0.995	-1.013	-1.551	-1.103	-0.601	-0.777
	(1.832)	(1.655)	(1.531)	(1.457)	(1.408)	(1.007)
Large Parties						
Seat Share	-3.395	-3.679	-2.946	-3.731*	-2.433	-2.060
	(2.936)	(2.634)	(2.336)	(2.105)	(1.988)	(1.617)
Vote Share	-3.687	-4.290*	-3.541	-4.265**	-3.243*	-2.803*
	(2.819)	(2.554)	(2.275)	(2.045)	(1.952)	(1.562)
N	442	523	591	655	723	1416

Notes: This table presents diff-in-disc regressions for voter turnout and the seat and vote shares of the different groups of parties in Hessian municipal councils. All population cutoffs at which council size is allowed to change are analyzed simultaneously by normalizing population size. Estimates for the average treatment effect of abolishing the election threshold are reported for different bandwidths (0.06, 0.07, 0.08, 0.09, 0.1, 0.2) and a linear polynomial of normalized log population size. Municipality and legislative term fixed effects are included in all models. Standard errors are clustered at the level of a municipality and robust to heteroscedasticity. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

Table 4: The abolishment of the electoral threshold at individual cutoffs.

	T=3001	T=5001	T=10001	T=25001
Turnout	1.952	0.081	0.334	1.645
	(1.444)	(1.032)	(1.007)	(1.432)
Small parties				
Seat share	6.285	3.805	0.798	0.645
	(4.139)	(2.842)	(2.489)	(4.407)
Vote share	5.973	3.919	0.926	0.482
	(4.060)	(2.868)	(2.409)	(4.300)
Medium parties				
Seat share	-0.498	-1.399	0.646	-1.374
	(2.386)	(1.810)	(2.024)	(2.181)
Vote share	-0.243	-1.011	0.625	0.543
	(2.292)	(1.680)	(1.858)	(2.058)
Large parties				
Seat share	-5.787	-2.405	-1.443	0.730
	(4.173)	(2.874)	(2.321)	(3.543)
Vote share	-5.736	-2.907	-1.548	-1.023
	(4.155)	(2.761)	(2.202)	(3.640)
N	625	1105	1122	430

Notes: This table presents diff-in-disc regressions at the individual cutoffs. Estimates for the average treatment effect of abolishing the election threshold are reported for a 0.5 bandwidth and a quadratic polynomial of normalized log population size. Municipality and legislative term fixed effects are included in all models. Standard errors are clustered at the level of a municipality and robust to heteroscedasticity. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

Table 5: The abolishment of the electoral threshold and seat and vote shares of individual parties.

	BW=0.4	BW=0.5	BW=0.6	BW=0.7
CDU				
Seat share	-2.134	-1.824	-2.199*	-1.746
	(1.453)	(1.372)	(1.166)	(1.117)
Vote share	-3.022**	-2.724**	-2.945***	-2.450**
	(1.400)	(1.319)	(1.134)	(1.087)
SPD				
Seat share	-0.627	-0.571	-0.893	-0.777
	(1.627)	(1.547)	(1.275)	(1.223)
Vote share	-0.514	-0.621	-0.725	-0.764
	(1.571)	(1.482)	(1.215)	(1.159)
FDP				
Seat share	-1.312	-0.995	-0.709	-0.416
	(0.879)	(0.786)	(0.681)	(0.675)
Vote share	-0.657	-0.289	-0.187	0.122
	(0.732)	(0.649)	(0.549)	(0.553)
The Greens				
Seat share	-0.840	-0.320	0.120	-0.088
	(1.104)	(0.984)	(0.870)	(0.842)
Vote share	-0.836	-0.202	0.165	-0.052
	(1.054)	(0.941)	(0.834)	(0.808)
Voter Initiatives				
Seat share	4.266*	2.993	2.888	2.296
	(2.432)	(2.258)	(1.828)	(1.763)
Vote share	4.250*	3.033	2.829	2.300
	(2.376)	(2.213)	(1.789)	(1.722)
N	2797	3496	4208	4917

Notes: This table presents diff-in-disc regressions for the seat and vote shares of individual parties. All population cutoffs at which council size is allowed to change are analyzed simultaneously by normalizing population size. Estimates for the average treatment effect of abolishing the election threshold are reported for different bandwidths (0.4, 0.5, 0.6, 0.7) and a cubic polynomial of normalized log population size. Municipality and legislative term fixed effects are included in all models. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

Table 6: The effect of the abolishment of the electoral threshold on council fragmentation.

	BW=0.4	BW=0.5	BW=0.6	BW=0.7
Nr. of parties	0.197	0.116	0.179	0.120
	(0.174)	(0.158)	(0.144)	(0.139)
Max. share	-1.807	-0.307	-0.310	0.146
	(1.781)	(1.621)	(1.349)	(1.322)
Herfindahl i.	1.662	1.029	1.434	0.777
	(1.312)	(1.206)	(1.021)	(1.030)
N	2797	3496	4208	4917

Notes: This table presents diff-in-disc regressions for the different measures of fragmentation in Hessian municipal councils. All population cutoffs at which council size is allowed to change are analyzed simultaneously by normalizing population size. Estimates for the average treatment effect of abolishing the election threshold are reported for different bandwidths (0.4, 0.5, 0.6, 0.7) and a cubic polynomial of normalized log population size. Municipality and legislative term fixed effects are included in all models. Standard errors are clustered at the level of a municipality and robust to heteroscedasticity. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

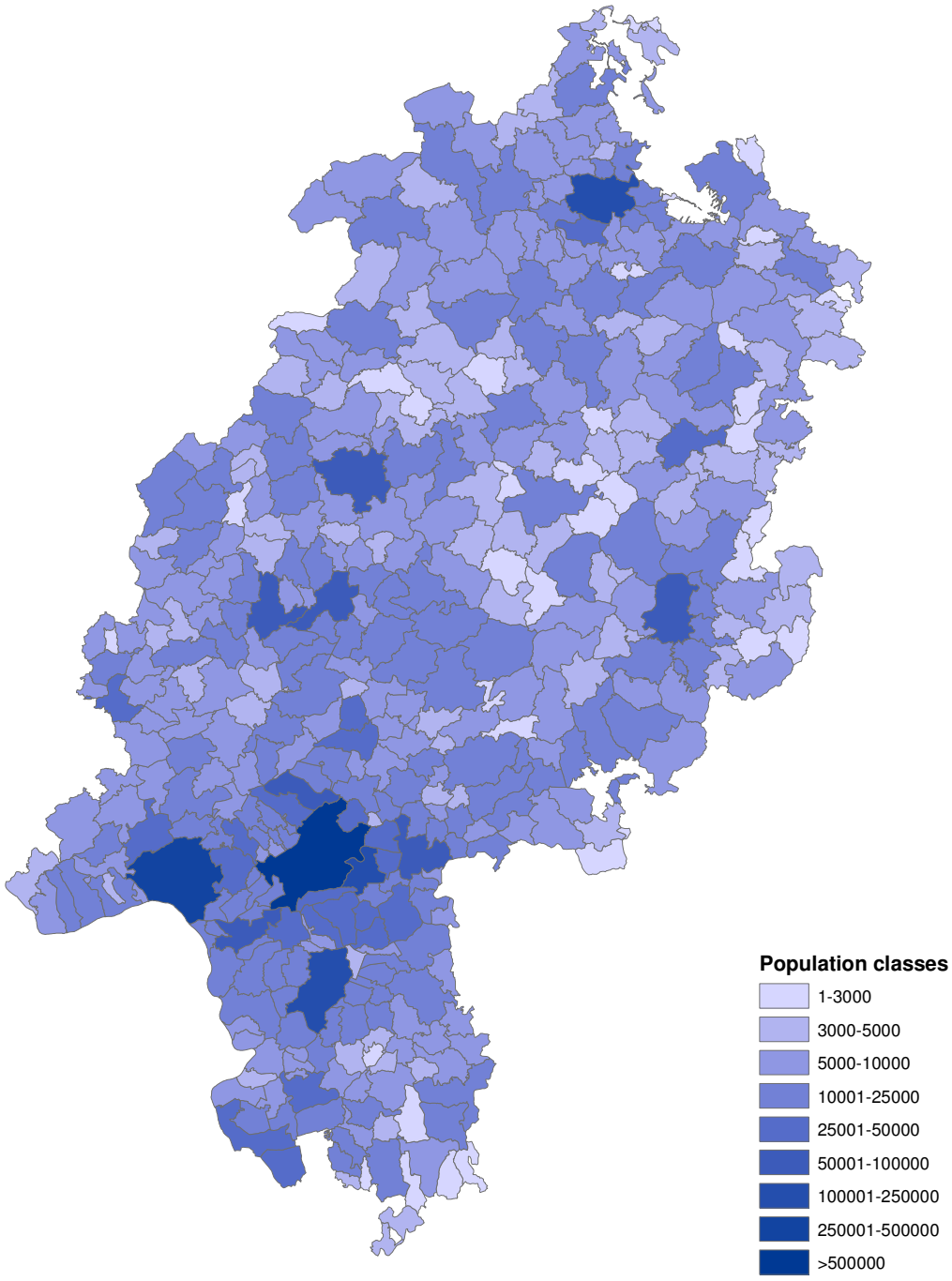
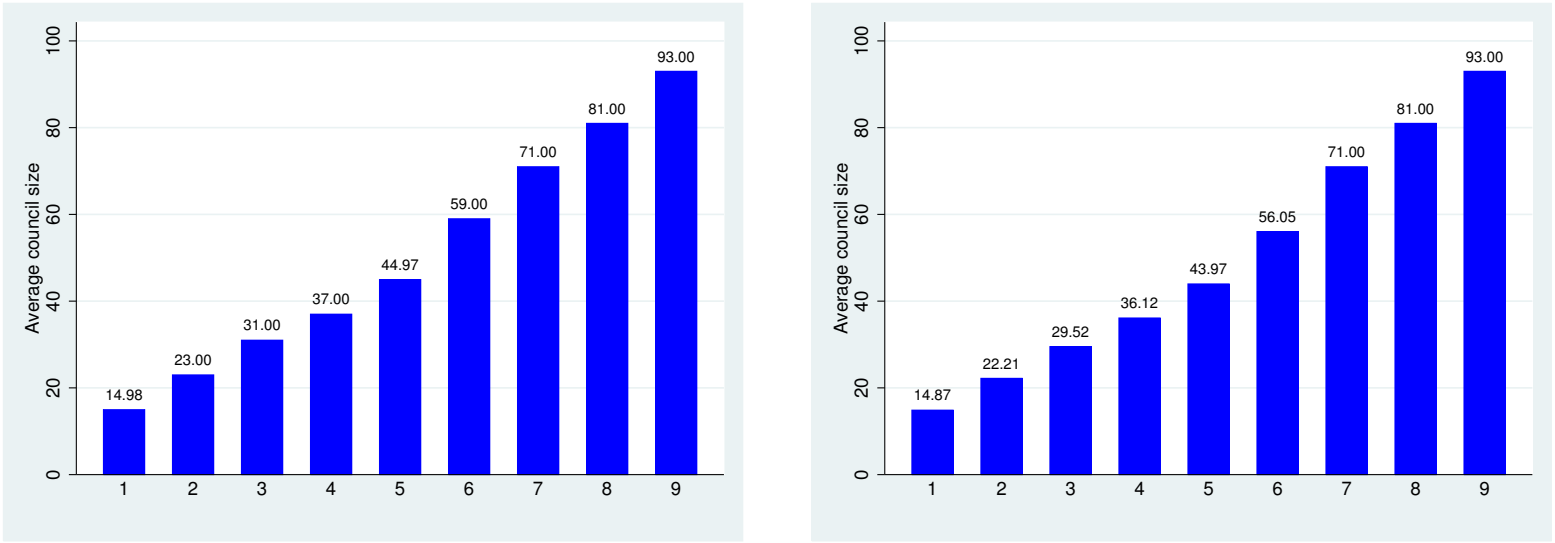


Figure 1: Average population sizes in Hessian municipalities during the sample period.



(a) Before 2001

(b) After 2001

Figure 2: Average council size in different population brackets prior and after the election of 2001. This figure shows the average council size of municipalities in population brackets 1-3000 (1), 3001-5000 (2), 5001-10000 (3), 10001-25000 (4), 25001-50000 (5), 50001-100000 (6), 100001-250000 (7), 250001-500000 (8), 500001-1000000 (9).

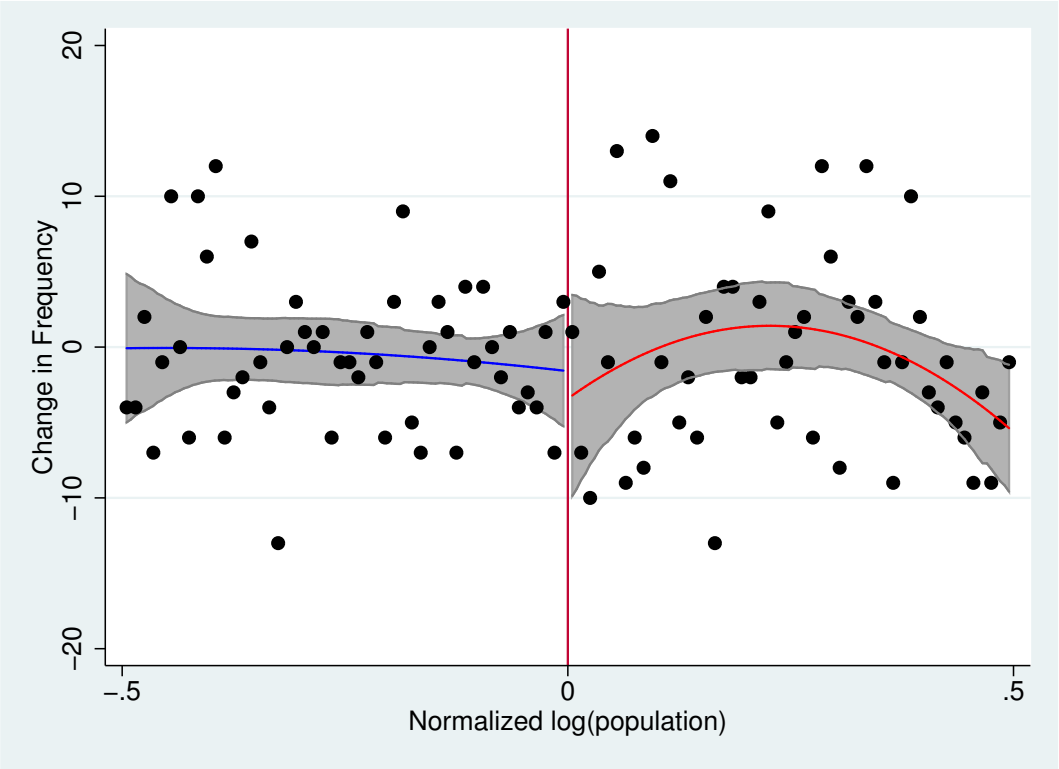
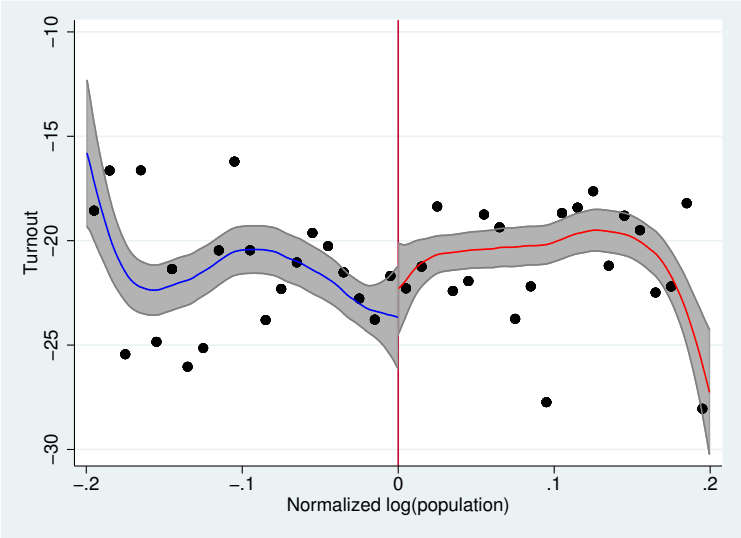
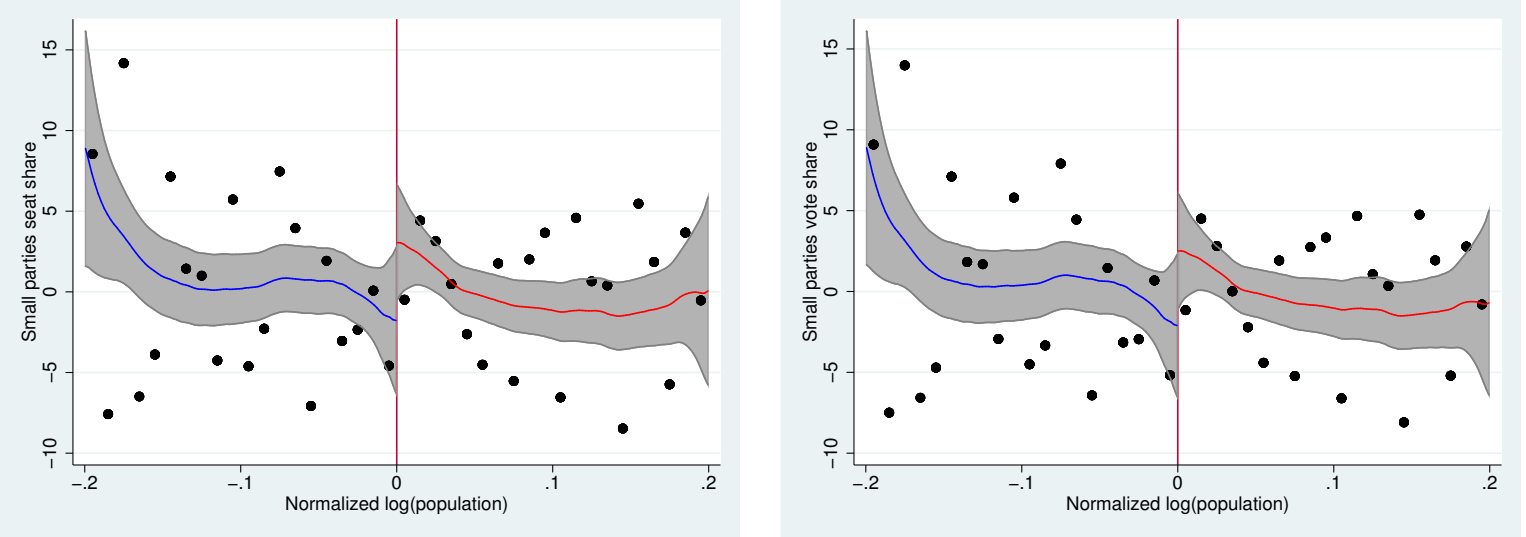


Figure 3: Density plots for change in normalized log population size. This figure presents a density plot for the Diff-in-Disc design in the spirit of McCrary plots (McCrary, 2008). We first divide normalized log population size in bins of width 0.01. Then we calculate the change in the total number of observations within each bin from the pre- to the post-treatment period. Finally, we fit local polynomial plots using a bandwidth of 0.05, a degree of 2, and a rectangular kernel to the number of changes within bins. 95% confidence intervals are indicated in gray.



(a) Turnout

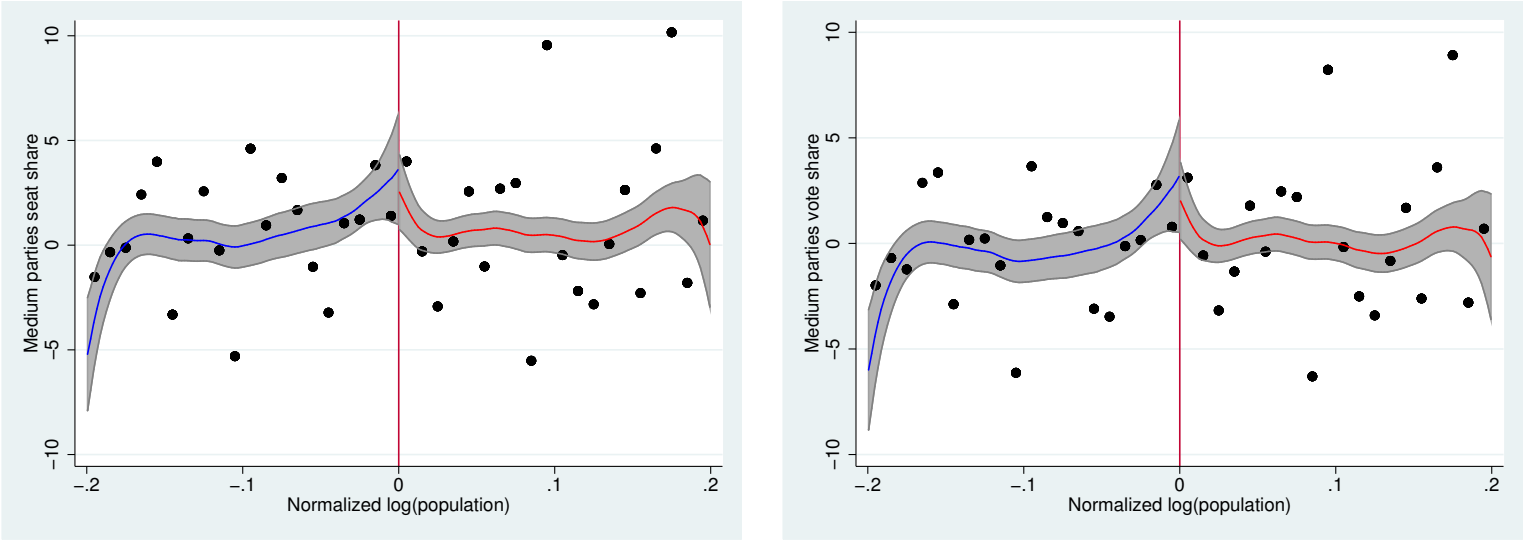
Figure 4: Treatment effect on voter turnout. This figure shows diff-in-disc plots for the change in turnout from the pre- to the post-treatment periods. Observations are averaged within bins of size 0.001. The polynomial plots are constructed using a rectangular kernel, a degree of 2, a bandwidth of 0.1, and the number of observations within bins as frequency weights. 95% confidence intervals are indicated in gray.



(a) Seat shares of small parties

(b) Vote shares of small parties

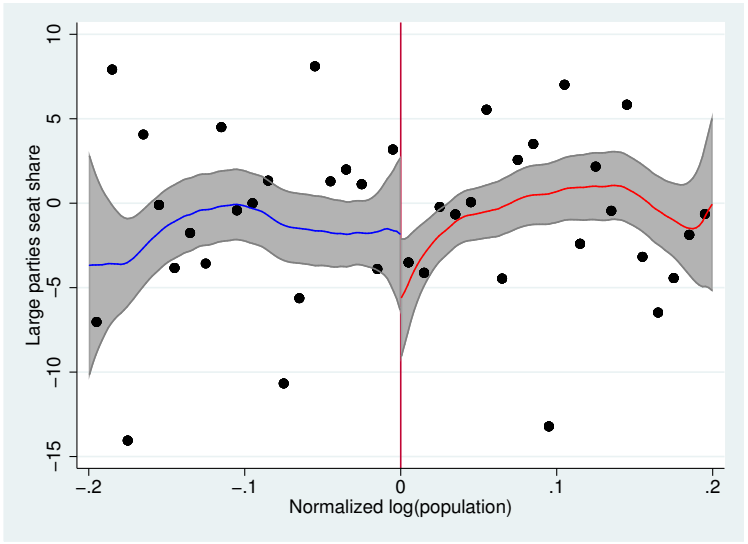
Figure 5: Treatment effect on the seat and vote shares of small parties. This figure shows diff-in-disc plots for the change in the seat and vote shares of small parties from the pre- to the post-treatment periods. Observations are averaged within bins of size 0.001. The polynomial plots are constructed using a rectangular kernel, a degree of 2, a bandwidth of 0.1, and the number of observations within bins as frequency weights. 95% confidence intervals are indicated in gray.



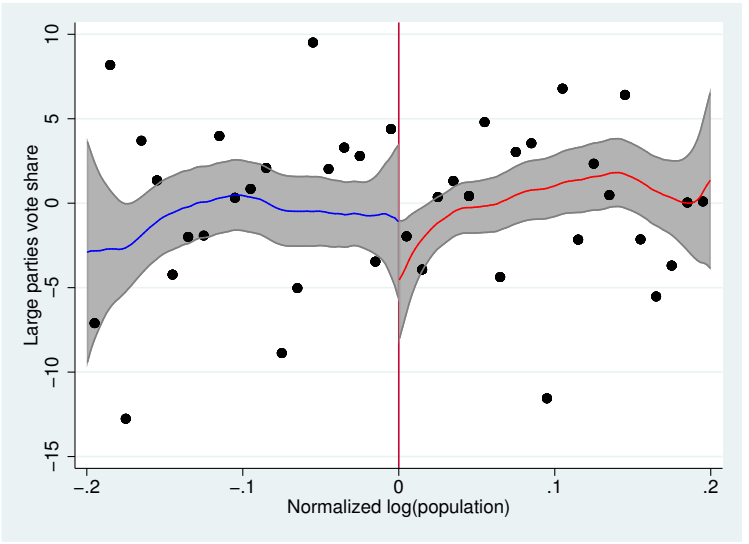
(a) Seat shares of medium parties

(b) Vote shares of medium parties

Figure 6: Treatment effect on the seat and vote shares of medium parties. This figure shows diff-in-disc plots for the change in aggregated seat and vote shares of the medium parties from the pre- to the post-treatment periods. Observations are averaged within bins of size 0.001. The polynomial plots are constructed using a rectangular kernel, a degree of 2, a bandwidth of 0.1, and the number of observations within bins as frequency weights. 95% confidence intervals are indicated in gray.

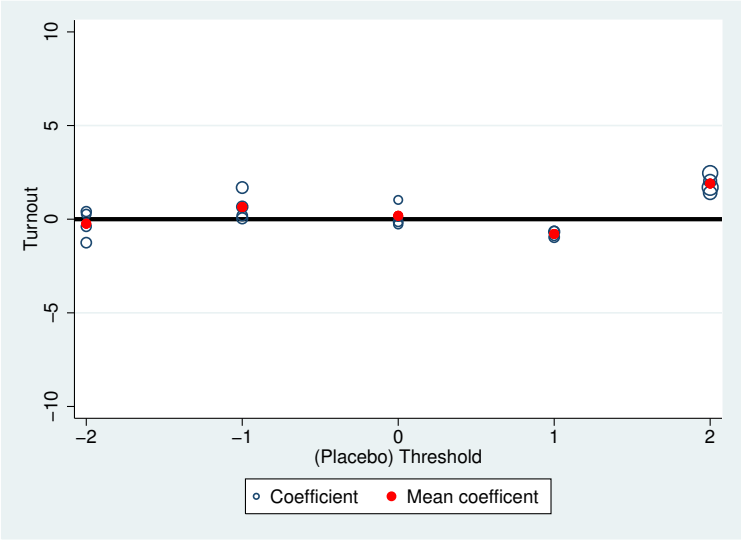


(a) Seat shares of large parties



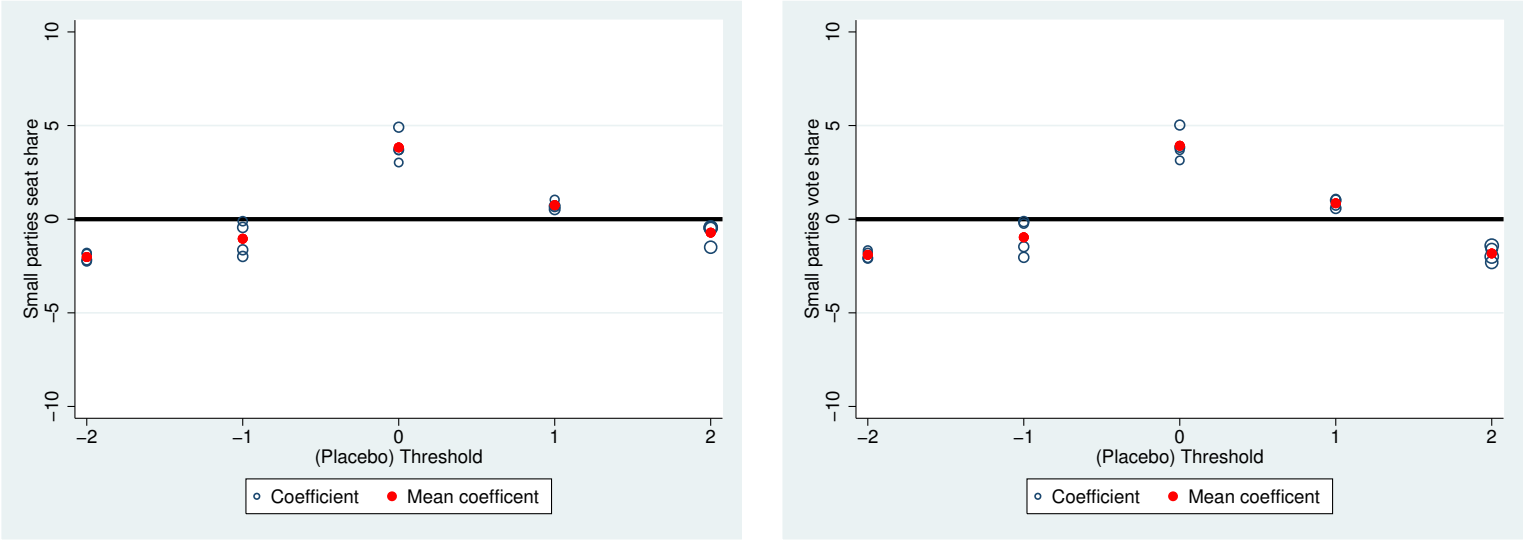
(b) Vote shares of large parties

Figure 7: Treatment effect on the seat and vote shares of large parties. This figure shows diff-in-disc plots for the change in aggregated seat and vote shares of the large parties from the pre- to the post-treatment periods. Observations are averaged within bins of size 0.001. The polynomial plots are constructed using a rectangular kernel, a degree of 2, a bandwidth of 0.1, and the number of observations within bins as frequency weights. 95% confidence intervals are indicated in gray.



(a) Turnout

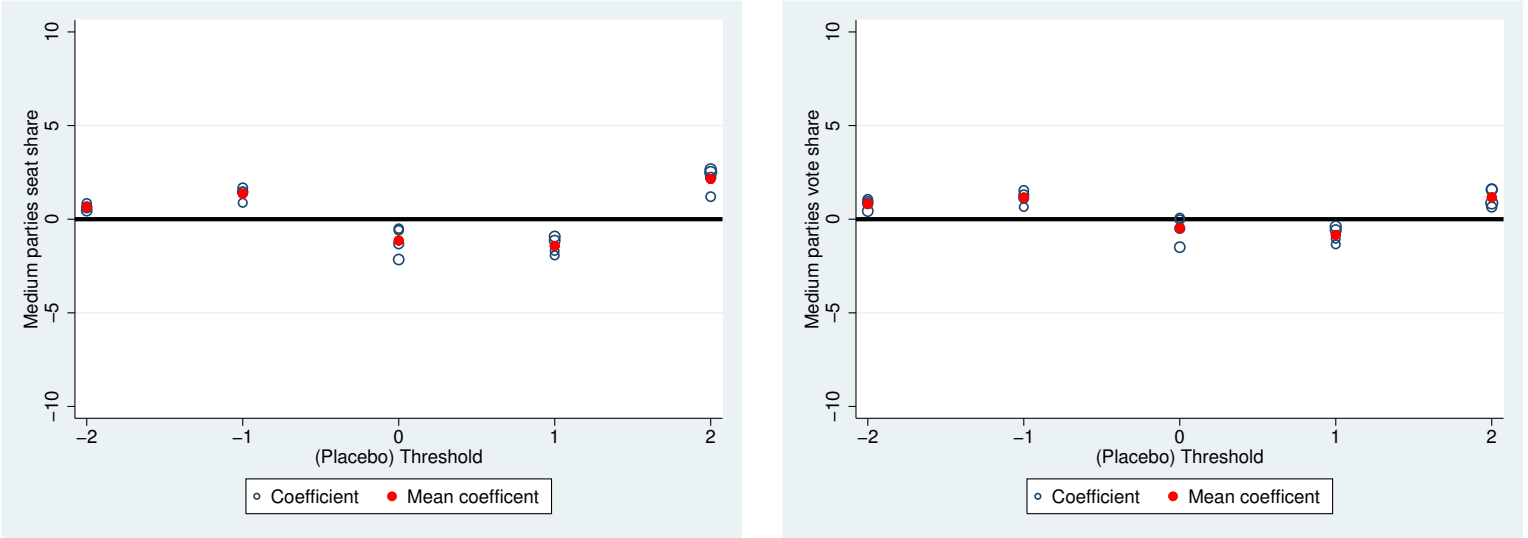
Figure 8: Placebo treatments with fake thresholds – voter turnout. This figure shows coefficient estimates of the diff-in-disc model for turnout with placebo treatments. The size of the dots indicates the standard error of each estimate. The thresholds are redefined such that treatment sets in at NLPOP= -2, -1, 1, 2. For comparison, the coefficient estimates at the true threshold of 0 are also indicated.



(a) Seat shares of small parties

(b) Vote shares of small parties

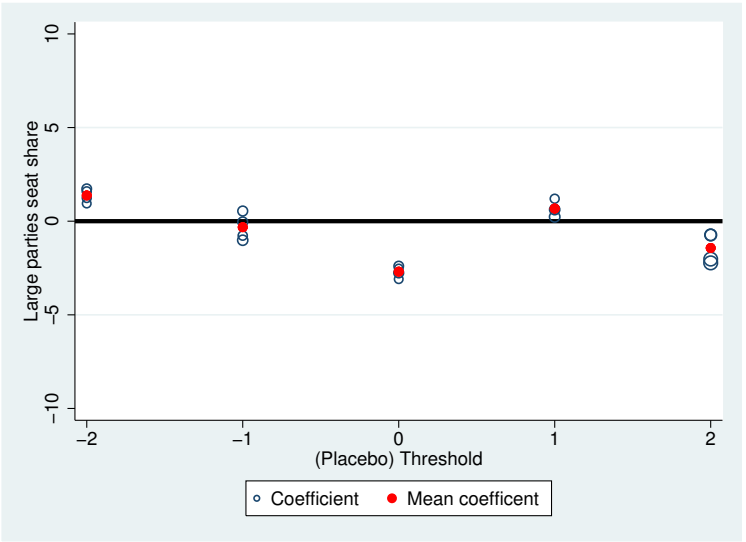
Figure 9: Placebo treatments with fake thresholds – small parties. This figure shows coefficient estimates of the diff-in-disc model for the seat and vote shares of small parties with placebo treatments. The size of the dots indicates the standard error of each estimate. The thresholds are redefined such that treatment sets in at NLPOP= -2, -1, 1, 2. For comparison, the coefficient estimates at the true threshold of 0 are also indicated.



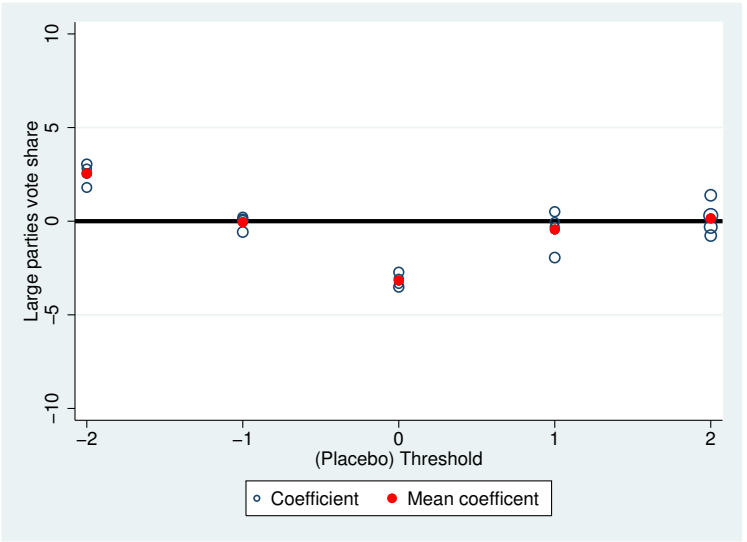
(a) Seat shares of medium parties

(b) Vote shares of medium parties

Figure 10: Placebo treatments with fake thresholds – medium parties. This figure shows coefficient estimates of the diff-in-disc model for the seat and vote share of medium parties with placebo treatments. The size of the dots indicates the standard error of each estimate. The thresholds are redefined such that treatment sets in at NLPOP= -2, -1, 1, 2. For comparison, the coefficient estimates at the true threshold of 0 are also indicated.

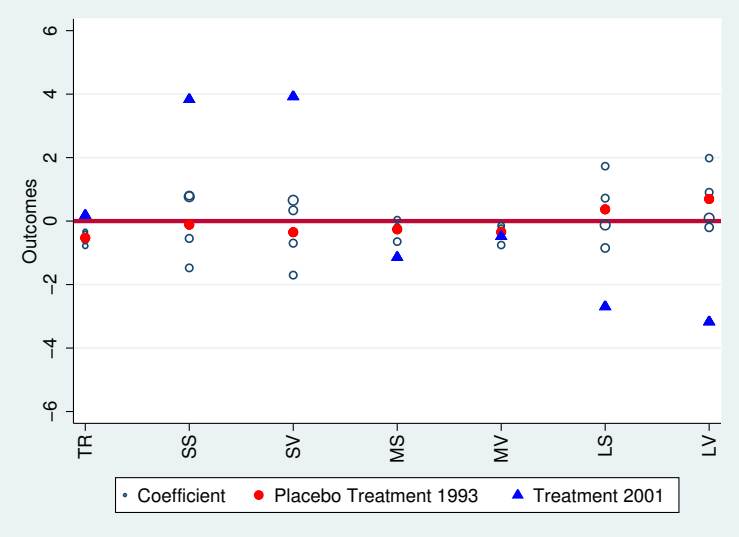


(a) Seat shares of large parties



(b) Vote shares of large parties

Figure 11: Placebo treatments with fake thresholds – large parties. This figure shows coefficient estimates of the diff-in-disc model for the seat and vote share of large parties with placebo treatments. The size of the dots indicates the standard error of each estimate. The thresholds are redefined such that treatment sets in at NLPOP= -2, -1, 1, 2. For comparison, the coefficient estimates at the true threshold of 0 are also indicated.



(a) Seat and vote shares

Figure 12: Placebo treatment for placebo year. This figure shows coefficient estimates of the diff-in-disc model with a placebo treatment defined to begin in 1993. The sample covers the period 1989-1997. The size of the dots indicates the standard error of each estimate. Coefficient estimates are reported for voter turnout (TR), small party seat share (SS), small party vote share (VS), medium party seat share (MS), medium party vote share (MV), large party seat share (LS) and large party vote share (LV). The median estimate at the fake treatment year is indicated with a red dot. The mean estimate at the true treatment year is indicated with a blue triangle.

Table A.1: Summary statistics

Variable		Mean	SD	Min.	Max.	N
Turnout	overall	64.540	12.911	31	93.606	2554
	between		5.560	49.514	79.837	426
	within		11.654	42.579	87.954	5.995
Small party seat share	overall	18.336	15.348	0.000	100.000	2554
	between		13.746	0.000	100.000	426
	within		6.854	-16.447	57.659	5.995
Small party vote share	overall	18.428	15.212	-0.100	100.000	2554
	between		13.645	-0.017	100.000	426
	within		6.754	-16.904	56.178	5.995
Medium party seat share	overall	8.177	7.842	0.000	45.946	2554
	between		6.751	0.000	31.532	426
	within		4.009	-10.318	33.853	5.995
Medium party vote share	overall	8.582	7.771	0.000	46.400	2554
	between		6.823	0.000	30.766	426
	within		3.739	-10.819	33.727	5.995
Large party seat share	overall	73.487	14.434	0.000	100.000	2554
	between		12.594	0.000	100.000	426
	within		7.068	34.164	99.043	5.995
Large party vote share	overall	72.990	14.296	0.000	100.000	2554
	between		12.551	0.000	100.000	426
	within		6.862	35.240	98.373	5.995
Nr. parties	overall	3.782	0.990	1.000	10.000	2554
	between		0.834	1.000	6.833	426
	within		0.535	1.282	7.616	5.995
Max. seat share	overall	47.891	10.314	26.667	100.000	2554
	between		8.828	32.040	100.000	426
	within		5.347	29.612	72.529	5.995
Herfindahl index	overall	62.871	9.335	0.000	100.000	2555
	between		8.448	0.000	100.000	427
	within		4.294	29.601	82.531	5.984
Council size	overall	31.211	9.753	11.000	93.000	2554
	between		9.604	13.667	93.000	426
	within		1.736	24.211	38.211	5.995
Inhabitants	overall	13931.84	36795.72	638	669992	2554
	between		36801.83	727.5	648056.5	426
	within		1084.074	-12745.66	35867.34	5.995