Center for European, Governance and Economic Development Research
cege

Discussion Papers

Number 177 – December 2013

# ELECTORAL THRESHOLDS AND POLITICAL OUTCOMES: QUASI-EXPERIMENTAL EVIDENCE FROM A REFORM IN GERMANY

Thushyanthan Baskaran, Mariana Lopes da Fonseca

GEORG-AUGUST-UNIVERSITÄT GÖTTINGEN

**ISSN:** 1439-2305

# Electoral thresholds and political outcomes: quasi-experimental evidence from a reform in Germany

Thushyanthan Baskaran*†, and Mariana Lopes da Fonseca††

†University of Göttingen
thushyanthan.baskaran@uni-goettingen.de
††University of Göttingen
mariana.da-fonseca@wiwi.uni-goettingen.de

**Abstract**

In 2001, the state parliament of the German federal state of Hesse abolished a 5 percent legal electoral threshold for local elections. This reform had a stronger effect on municipalities with larger councils because *implicit* electoral thresholds decrease with council size. Exploiting discontinuities in a state law that exogenously maps population to council size, we implement a difference in discontinuity design to study the political consequences of abolishing an electoral threshold. The dataset covers all 426 Hessian municipalities over the period 1989-2011. Our results suggest that the seat and vote shares of small parties increased in municipalities that were affected more strongly by the abolishment. In addition, municipalities exposed to stronger treatments reduced their council size, presumably to limit political competition.

**Keywords**: Electoral rules, electoral thresholds, voting
**JEL codes**: D70, D72, D78

*Corresponding author: Thushyanthan Baskaran, Department of Economics, University of Goettingen, Platz der Goettinger Sieben 3, 37073 Goettingen, Germany, Tel: +49(0)-551-395-156, Fax: +49(0)-551-397-417.

# 1 Introduction

A defining characteristic of a democracy is that even small minorities and narrow special interests can form political parties and thereby gain parliamentary representation. Excessive party competition, however, can 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, some countries rely on majoritarian electoral rules. Majoritarianism often ensures by default that only a small number of parties can gain legislative representation and thereby political influence.[2] Given that only the party with the largest vote share wins the seats awarded in a constituency, parties that cater to small minorities are at a disadvantage and a few large parties typically dominate the political landscape.[3] 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.[4]

Countries that have proportional electoral rules, on the other hand, often employ mechanisms that are specifically designed to limit the effective number of parties. Since electoral districts tend to be large –sometimes even comprising the whole country– and multiple seats are awarded within each district, gaining a small share of votes is sufficient for a party to gain parliamentary representation. Countries with proportional electoral rules are hence by default more prone to legislative fragmentation (Duverger, 1954; Lijphart, 1994; Rae, 1971). A mechanism that many proportional countries therefore adopt to limit political competition are legal electoral thresholds (Lijphart, 1991). With a legal electoral threshold, a party may only receive seats in parliament if its overall vote share is above some fixed and relatively high value. The specific value varies between countries: at the national level, Sweden has a threshold of 4 percent, Germany 5 percent, and Turkey 10 percent.

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

[2] 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).

[3] 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.

[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).

Despite the presumption that legal electoral thresholds limit legislative fragmentation, 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.[5] However, observational cross-country analyses may lead to biased estimates due to omitted variables. More specifically, it is difficult to separate the effect of electoral thresholds from that of voter preferences and other unobservable variables since whether to introduce an electoral threshold and its size are endogenous policy choices that are made either at the constitutional stage or by a sufficiently large parliamentary majority. But since voters approve constitutions and elect legislative majorities, it is possible that more cohesive societies or, alternatively, divided countries introduce higher thresholds.

We ask in this paper whether electoral thresholds have a causal effect on political outcomes. In contrast to previous studies, we rely on a credible source of exogenous variation: an institutional reform in the German state of Hesse that involved the abolishment of the 5 percent legal electoral threshold for *local* elections as of 2001.[6]

Prior to this reform, a party had to win at least 5 percent of votes in a municipality in order to gain any seats in that municipality's council. After the reform, there is no longer an explicit electoral threshold. There remains, however, an implicit threshold since parties must still have a minimum vote share to gain their first seat.[7] The implicit threshold varies between municipalities because its specific value depends on the total number of seats in the municipal council, which in turn is linked to municipal population size. In

[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 no statistically significant 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 exception for this literature are Vatter (2003), who use subnational data at the level of the Swiss Cantons. They find that legal electoral thresholds have no effect on the number of parties represented in parliament.

[6] Our paper thus follows a relatively new literature that uses natural experiments to estimate the causal effect of electoral rules on political outcomes. A related study is Fiva and Folke (2011) who 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.

[7] 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).

small municipalities, the implicit threshold can be as high as 5 percent, while in large municipalities the implicit threshold can be as low as 0.5 percent. Hence, the abolishment of the explicit 5 percent electoral threshold affected municipalities differently, with a stronger effect on larger ones.

We rely on this heterogeneity in treatment intensity to identify the causal effect of electoral thresholds on four outcome variables: the seat and vote shares of "small" parties, council fragmentation, and council size. Our sample covers all 426 Hessian municipalities over the period 1989-2011. This period encompasses three local elections prior and three local elections after the electoral reform. Our identification strategy is based on an approach that combines difference-in-difference (DD) and regression discontinuity (RDD) methods, the difference in discontinuity design (Diff-in-Disc) (Grembi et al., 2012). The idea underlying the Diff-in-Disc design is to focus only on changes in political outcomes in municipalities close to the population cutoffs at which municipal council size is allowed to increase according to an exogenous state law. By relying only on changes in political outcomes in municipalities just below and just above the relevant population cutoffs, the estimates are robust to potentially omitted variables or differential trends.

The Diff-in-Disc estimates suggest that the seat share of small parties increased in municipalities that were exposed to stronger treatments, i. e. where after the abolishment of the explicit threshold the implicit ones were lower. Yet, council fragmentation did not rise significantly because only certain small parties benefited from the decline in the seat share of the national parties. Also, the increase in the seat share of small parties was a consequence of changing voting patterns rather than a mechanical effect of the abolishment of the threshold, i. e. vote shares of small parties increased by the same amount as seat shares. There is hence evidence that abolishing an explicit threshold increases electoral competition and benefits small parties, but it does so mostly indirectly through psychological rather than mechanical effects. This interpretation is confirmed by our analysis of how the abolishment of the electoral threshold affected council size. We find that local politicians in municipalities where the abolishment mattered more because they had relatively larger councils reduced council sizes, presumably to increase implicit thresholds and thereby limit political competition.

The remainder of this paper is structured as follows. We describe some institutional details in the next section. Section 3 discusses the link between electoral thresholds and political outcomes theoretically. In Section 4, we describe the empirical strategy. Section 5

presents the baseline results. In Section 6, we collect some robustness tests and extensions. Section 7 concludes.

# 2 Institutional details

The setting for our analysis is the German State of Hesse. Hesse has about six million inhabitants who live in 426 municipalities. Municipal population sizes differ considerably. There are, on the one hand, municipalities with less than 1000 and, on the other hand, the city of Frankfurt with more than 600,000 inhabitants. Figure 1 shows a map of Hesse and indicates the average population sizes of municipalities during the sample period.

Inhabitants in every municipality elect the council in elections held at the same date. The council is the most important political institution in a Hessian municipality. It decides, inter alia, on various municipal taxes, user fees, and on the provision of municipal public goods and services.[8]

Several parties contest the local elections. These are, first, the center-right CDU and the center-left SPD. These two parties typically receive 30 percent or more both in national and state elections. Second, the Green Party and the FDP. The characteristic feature of the Green Party is its emphasis on environmental issues. It is considered to be left of center regarding economic and liberal regarding social issues (i. e. immigration) and tends to form coalitions with the SPD. The FDP, on the other hand, emphasizes economic liberty. It is considered to be right of center with respect to economic issues and liberal with respect to social issues. It tends to form coalitions with the CDU. The Green Party and the FDP receive typically up to ten percent of the votes.

In addition to the four large national parties, there are a number of non-mainstream parties that contest local elections. First, small national parties which can be either centrist, far-left, or far-right with respect to economic and social issues. Second, municipal specific voter initiatives (*Wahlvereinigigungen*) often contest local elections by fielding a list of candidates. The smaller parties often struggle with the five percent threshold, even though some voter initiatives are very successful in their respective municipalities. Despite the success of voter initiatives in some municipalities, we refer to all non-mainstream parties as "small" parties in this paper.

[8]The other important political institutions is the mayor. There was a reform in the way mayors were elected in 1992. As of 1993, the mayor is directly elected by municipal inhabitants in regular elections. However, even though the mayor is directly elected, she continues to have little formal power vis-a-vis the council.

The rules governing local council elections in Hesse differed significantly before and after 2001. Until 2001, local elections took place every four years. Citizens were allowed to cast one vote for their favored party list. Parties would then be allocated seats in the council according to the Hare-Niemeyer procedure. All candidates placed sufficiently high on their respective lists would receive a seat. However, even if a party had a sufficiently large vote share to gain one or more seats in the council, it would not receive a seat if its vote share was below 5 percent.

In 1999, the 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 extend from four to five years. Second, the law introduced a new voting system called *Kumulieren und Panaschieren*. In this system, voters may cast as many votes as there are seats available in the council. Up to three votes can be cumulated and given to individual candidates. Alternatively, voters are allowed to give all their votes to a certain party list, but they can also drop individual candidates from the list. Third, the 5 percent electoral threshold was abolished. Parties could enter the parliament if they had sufficient votes to gain at least one seat.

The first two elements of the reform – the lengthening of the legislative period and the possibility of *Kumulieren und Panaschieren* – affected all municipalities equally. But the third element – the abolishment of the 5 percent threshold – had heterogeneous effects. This aspect of the reform affected municipalities with a large municipal councils more strongly because in addition to possible explicit legal electoral thresholds, there are always implicit thresholds. In a council with 100 seats, for example, a vote share of around 0.5 percent would be sufficient for a party to gain a council seat if there was no 5 percent threshold.[9] But if the council has for example only 20 seats, a party has to receive around 2 to 3 percent of the votes to get a seat even if there is no explicit 5 percent threshold. Consequently, the abolishment of the 5 percent 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.

[9]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`.

# 3 The link between electoral thresholds and political outcomes

Studying electoral thresholds in a local setting has several attractive features. First, localities within one sub-federal state are more homogeneous than different countries, which reduces the possibility that unobservable between-country heterogeneity leads to biased estimates. Second, the sample size is much larger than in cross-country studies, leading to more precise estimates. Third, given that the reform in Hesse abolished an existing threshold, we have a pre- and post-treatment period and can therefore rely on within rather than only on between variation for identification. Fourth, the abolishment of the electoral threshold was an exogenous intervention from the perspective of Hessian municipalities since it was imposed by the state tier. Finally, given that the council size is a discontinuous (albeit fuzzy) function of municipal population size defined by a state law, the intensity of treatment also varies exogenously with population size.

In view of these advantages of the Hessian setting, we analyze the effect of abolishing the electoral threshold on four outcome variables. First, the aggregated seat shares of the small parties, defined as 100 percent minus the seat share of the four national parties (CDU, SPD, FDP, and the Green Party). Second, the small parties' aggregated vote share, again defined as 100 percent minus the vote shares of the four national parties. Third, the degree of council fragmentation, which we measure with an inverse Herfindahl index.[10] Fourth, the size of the municipal council, i. e. the total number of council seats. Municipalities can either increase or decrease the size of the council for the next election within the thresholds defined in Table 1 (see below for more details). However, they cannot change the size of the current council.

The expectation underlying the reform of 1999 was that the abolishment (and the other reforms in the *Kommunalwahlreform* as well) would benefit smaller parties and thus foster political competition. That is, given fixed voting patterns and fixed council sizes, having no legal electoral threshold should mechanically increase the seat shares of smaller and decrease the seat share of larger parties. However, voting patterns must not remain fixed (Moser and Scheiner, 2004). They might change such as to increase the seat shares of small parties. Prior to the abolishment, 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

[10] More specifically, we calculate council fragmentation as follows: $\text{council fragmentation}_{i,t} = \left(1 - \sum_{p,i,t}(\text{Seat share}_{p,i,t})^2\right)$ where $p$ denotes a given party in municipality $i$ in year $t$

small party would fail to overcome the 5% threshold. Once the threshold was abolished, voting for their preferred small party might have become more worthwhile for this subset of the electorate (Perea, 2002). Hence, the abolishment might have had, in addition to any mechanical effects, what we call in the following psychological effects (Duverger, 1954; Fiva and Folke, 2011).

The effect of abolishing the threshold on council fragmentation is indeterminate as well. Much depends on how the abolishment affects seat shares. If the seat share of the large parties increases at the expense of both the mid-sized and the small parties, council fragmentation should decrease. Conversely, if the seat share of the small parties increases, the effect on council fragmentation will depend on whether all smaller parties benefit equally or if voters implicitly coordinate on a few of the smaller parties so that these parties receive a relatively large fraction of the available seats.

Finally, both the effect of the abolishment on seat shares and council fragmentation depends upon how council sizes evolve in the aftermath of the reform. In principle, the abolishment should have no effects on the number of seats since it did not spell out any changes with respect to the organization of the council. But if the mainstream parties want to counteract the abolishment and limit electoral competition, then one possibility is to reduce the size of the council and hence increase the implicit threshold. If so, the abolishment of the electoral threshold should have led to smaller councils.

# 4 Empirical strategy

## 4.1 Difference in discontinuity design

We use the heterogeneity in the treatment effect of abolishing the electoral threshold in 2001 to identify the ensuing political consequences. As indicated previously, municipalities with larger councils are exposed to a stronger treatment. However, the size of the council is set by the municipality and can therefore be changed between the pre- and post- treatment period. Yet, while Hessian municipalities can determine their council sizes they are not completely free in doing so.

A state law maps maximum and minimum council sizes to municipal population size.[11] The relevant cutoffs are listed in Table 1. For example, a municipalities with 5000 inhabitants must choose a council size between 15 and 23 seats while municipalities with 5001 inhabitants must have between 23 to 31 seats. Hence, Hessian municipalities with e. g. 5001 inhabitants will tend to have larger councils than municipalities with 5000 inhabitants. More generally, the probability of a larger council increases discontinuously at the population cutoffs. Figure 2 maps 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 between the different brackets. Typically, municipalities choose the largest possible council size.[12]

Even though the relationship between population size and council size is fuzzy, it is clear that there is a positive and discontinuous relationship between both variables. Therefore, the treatment intensity of abolishing the five percent threshold will increase discontinuously in population size. More specifically, a given municipality with e. g. 3000 inhabitants will be affected less by the abolishment of the electoral threshold than municipalities with 3001 inhabitants because the former will choose on average smaller councils and thus have larger implicit thresholds.

Given that the implications of abolishing the electoral threshold are stronger for larger municipalities, a possible empirical methodology to identify the causal effect of electoral thresholds on political outcomes is a difference in difference design where treatment intensity varies in the population size of a municipality. Finkelstein (2007) implements a similar design to estimate the effect of health care reforms in the US. In her application, treatment intensity varies in the number of privately insured inhabitants in a state prior to the introduction of Medicaid.

One crucial assumption of a difference in difference design is, however, that treatment and control groups would have experienced similar trends if no treatment had been applied. The corresponding assumption in our case is that municipalities that were applied relatively

[11]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.

[12]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.

weaker treatments would have experienced the same trends in political outcomes as the municipalities that were applied stronger treatments. Graphs collected in the appendix for the four outcome variables suggest that this assumption is not generally valid (see Figure A.1).

One possibility to address this problem is to explicitly allow for differential trends. Given the large number of population brackets, however, we opt for an alternative strategy. The first observation on which our alternative approach is based is that the formula that maps population to councils size as described by Table 1 is discontinuous. As noted previously, municipalities to the right of each of the cutoffs have in expectation larger councils than municipalities to the left of the thresholds. Since treatment intensity increases in council size, and the probability of a larger council increases discontinuously in population size, we could implement a fuzzy RDD (Lee and Lemieux, 2010) using only post-treatment data. That is, the effect of a stronger treatment at $M$, the natural log of a given threshold, in the post-treatment period $t \geq T$ can be defined as follows:

$$\hat{\gamma}^{M}_{t \geq T} = \lim_{NLPOP_i \downarrow M} E[y_{i,t} | NLPOP, t \geq T] - \lim_{NLPOP_i \uparrow M} E[y_{i,t} | NLPOP, t \geq T], \qquad (1)$$

where $y_{i,p}$ is either of the four political outcomes in the post-treatment period $t \geq T$ and $\hat{\gamma}^{M}_{t \geq T}$ is the estimate for the treatment effect. $NLPOP$ is the normalized value of the natural log of the relevant population figure such that $NLPOP = LPOP - M$.

The treatment effect $\hat{\gamma}^{M}_{t \geq T}$ can be obtained with the following general RDD model in a regression framework:

$$\begin{aligned} y^{M}_{i,t \geq T} =& \gamma^{M}_{t \geq T} D_i + f(NLPOP) + D_i f(NLPOP) + \epsilon_i \\ & \text{if } |NLPOP| < h, \end{aligned} \qquad (2)$$

where $D_i$ is a dummy that is 1 if $NLPOP \geq 0$ and 0 else. $f(NLPOP)$ is a flexible polynomials of normalized population size which is allowed to have different slopes to the left and right of a normalized cutoff $M$. This type of RDD model can be estimated by local polynomial regression using different polynomials and bandwidths $h$.

One difficulty in implementing a RDD as defined by Equation 2 in our setting is that most cutoffs are not only relevant for council size but also for other institutional features relevant for municipalities. In particular, several of the cutoffs in Table 1 are relevant for equalization transfers as well, so that co-treatment cannot be excluded (Baskaran, 2012).

Consequently, a simple RDD is not suitable in our setting. But since we have data on political outcomes and population size in pre-treatment periods, we can account for co-treatments and possible manipulation at the cutoffs by studying to what extent the effect of the cutoffs on political outcomes varied between the pre- and the post-treatment periods, i. e. by focusing on changes in political outcomes between the pre- and post-treatment periods at the cutoffs.

Given a logarithmized cutoff $M$, a Diff-in-Disc model can be specified as

$$\begin{aligned}\hat{\delta}^M &\equiv \hat{\gamma}^M_{t\geq T} - \hat{\gamma}^M_{t<T} \\ &= \lim_{NLPOP_i \downarrow M} E[y_{i,t}|NLPOP, t \geq T] - \lim_{NLPOP_i \uparrow M} E[y_{i,t}|NLPOP, t \geq T] \\ &- \left( \lim_{NLPOP_i \downarrow M} E[y_{i,t}|NLPOP, t < T] - \lim_{NLPOP_i \uparrow M} E[y_{i,t}|NLPOP, t < T] \right),\end{aligned} \tag{3}$$

In a regression framework, the corresponding model is:

$$\begin{aligned}y^M_{i,t} =& f(NLPOP) + D_i(\gamma^M_{t\geq T} + f(NLPOP)) + I_t(\alpha + f(NLPOP)) \\ &+ D_i(\delta^M I_t + I_t f(NLPOP)) + \epsilon_{i,t} \text{ if } |NLPOP| < h,\end{aligned} \tag{4}$$

where $I_t$ is a dummy indicating the post-treatment period. This specification is an extension of the standard RDD model specified in Equation 2. This model allows the control function to vary both to the left and the right of the cutoff $M$, between the pre- and post-treatment periods, and within treated municipalities in the pre- and post-reform periods. We are interested in the the estimate for $\delta^M$ which captures the change in the effect of the discontinuity at $M$ between the pre- and the post-treatment periods.

We motivated the Diff-in-Disc model above by referring to a single cutoff $M$. In our case, there are multiple cutoffs. Rather than analyzing all cutoffs individually, we follow in the baseline regressions the previous literature that use the RDD methodology with multiple population cutoffs and normalize all observations such that they are around a single cutoff (Egger and Koethenbuerger, 2010). This approach has the advantage of a larger sample size and the results can be presented more compactly. In robustness tests, however, we also report results for individual cutoffs.

The identifying assumptions in the Diff-in-Disc design are arguably less strict than in the RD design. Notably, 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. The other crucial assumption 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 appears plausible as it is unlikely that municipalities would persistently misrepresent their population sizes only to avoid being forced to change their council sizes. Moreover, given that most municipalities choose the highest possible council size in the pre-treatment period, they were allowed to reduce their council sizes anyway and therefore had no incentives for manipulation. Finally, a McCrary (2008) style density plot[13] reported in Figure A.2 in the appendix also fails to indicate that incentives for manipulation changed from the pre- to the post-treatment period at the normalized cutoff.

To establish the robustness of the baseline estimates, we always report results for various bandwidths around the normalized cutoff and polynomials of the control function. More specifically, we use the following bandwidths: 0.5, 0.4, 0.3, 0.2, 0.15, 0.1. Note that already a bandwidth of 0.3 is fairly large given that most population cutoffs are close to each other.[14] With respect to polynomials of normalized population size, we use up to a quartic specification. We 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).

## 4.2 Diff-in-Disc plots

In addition to regression results, we also present graphical evidence on the treatment effect based on the specification in Equation 4. We construct the Diff-in-Disc plots by first dividing the control function, $NLPOP$, into bins of size 0.02 within a window of 0.5. 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 $b = 1, ..., 50$ denoting the bin and $t = 0, 1$

[13]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.

[14]We also experimented with the data driven bandwidth selection procedure by Imbens and Kalyanaraman (2011). However, this procedure was developed for cross-sectional RD designs and may result in inappropriate suggestions if the arbitrary initial bandwidth is wrong. In our application, it typically suggested unreasonably large optimal bandwidths. We therefore establish the robustness of the results by reporting estimates for different but relatively narrow bandwidths.

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 the difference in $y$ between the pre- and post-treatment periods, i. e. $\Delta y_b$, against $NLPOP$ to the left- and the right of the normalized cutoff. We then smooth the observations with a local polynomial plot of quadratic degree and a bandwidth of 0.5 at both sides of the threshold, using a rectangular kernel and the number of observations within each bin as frequency weights.

# 5 Results

## 5.1 Seat share of small parties

Subfigure (a) of Figure 3 shows Diff-in-Disc plots for the aggregated seat share of the small (non-mainstream) parties. There is a noticeable discontinuous increase in the seat share of the small parties at the normalized cutoff. In Table 2, we present the corresponding Diff-in-Disc regressions. The coefficient estimate for the seat share of the small parties is consistently positive (with one exception) and in several cases significant.

These results indicate that the small parties benefited at the cost of the mainstream parties from the abolishment of the electoral threshold. While the coefficient estimates vary between specifications, most of them are around 3 to 5.

The weighted average increase in treatment strength, i. e. the decline in the implicit electoral threshold, at the normalized population threshold is around 0.38.[15] Consequently, abolishing an explicit legal threshold increases the seat share in municipalities with a 1 percentage point lower implicit threshold by about 7.9 to 13.2 percentage points. These are relatively large effects, but as shown below they are mostly driven by the smallest population brackets where the increase in treatment intensity is the largest.

[15]Crossing the population threshold at 3001 from below implies on average a reduction in the implicit threshold from about 3.33 to about 2.17 percentage points, assuming that 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 around 1.61 percentage points. The intensity of treatment increases by around 0.56 percentage points. The same argument applies for all further thresholds. We weigh 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.

## 5.2 Vote shares of small parties

What lies behind the increase in the seat share of smaller parties? Does it come from a mechanical application of the new electoral rules or from a change in voting patterns? To answer this question, we study how the electoral reforms affected the aggregated vote share of the small parties.

Subfigure (b) of Figure 3 presents the graphical evidence for aggregated vote shares. This figure resembles the one for seat shares: there is a noticeable positive discontinuity in the vote share of the small parties at the normalized threshold.

Table 3 presents the corresponding regression results. According to these results, it appears that the entire increase in the seat share of small parties was due to changing voting patterns. The estimated effect of the normalized cutoff is typically slightly larger than in the equivalent regressions for seat shares. The coefficient estimates are again around 3 to 5, which implies that the increase in the vote share of smaller parties in municipalities with a 1 percentage point lower implicit threshold was in the same ballpark as the increase in seat shares, i. e. around 8 to 13 percent points. These are again large effects, but as shown below primarily driven by the smaller thresholds.

That vote shares increase by roughly the same amount as seat shares indicates that the psychological effects of the reform were more important than the mechanical effects. The rationale for this assessment is as follows. If some small parties enter the council for the first time because of the mechanical effects of the reform, we would expect that the small parties' seat shares increase more than their vote shares. However, we observe the opposite. Such opposite effects are to be expected if it were those small parties that had been already present in most councils – in particular the voter initiatives – that had primarily benefited from the reform. In this case, we would expect that vote shares increase by the same amount or slightly more than seat shares since implicit thresholds are still binding.

## 5.3 Council fragmentation

In Subfigure (c) of Figure 3, we present the Diff-in-Disc plot for council fragmentation. This figure shows that the polynomial smooth has no significant discontinuity at the cutoff. The corresponding regression results in Table 4 point in the same direction. The Diff-in-Disc estimates are often positive but only once significant. Overall, the results imply that council fragmentation did not increase after the reform despite the gain in seat shares for the small parties. This finding is consistent with the notion that only selected small

parties – in particular voter initiatives – benefited from the reform. If small parties that were already present in most councils benefited the most, we would not expect a significant increase in council fragmentation.

### 5.4 Council size

As argued above, the intensity of treatment varies according to the size of the municipal council. But council size is itself an endogenous variable because the mainstream parties can reduce it prior to the next election in order to attenuate the effects of the reform.

To explore this issue, we present Diff-in-Disc plots and regressions with council size as the dependent variable. Figure 3 presents the graphical evidence. There is a noticeable drop in the council size at the normalized cutoff for municipalities that experienced a stronger treatment. In Table 5 we collect the regressions. Consistent with the graphical evidence, the effect of abolishment is consistently negative and highly significant for all bandwidths and polynomials. Council size after the abolishment decreases in municipalities that were exposed to a stronger treatment by about 2 to 3 seats. Scaled by the average increase in treatment intensity, these estimates imply that municipalities with a 1 percentage point lower implicit threshold reduce the size of their council by about 5.2 to 7.9 seats.

Hence, it appears that without the changes in voting patterns smaller parties would not have benefited much from the abolishment of the electoral threshold since politicians from the more established parties colluded to raise the implicit electoral threshold.

## 6 Robustness tests and extensions

### 6.1 Placebo tests

As a first set of placebo tests, we let the treatment set in at fake cutoffs and compare the estimated effects with those estimated for the correct cutoff. More specifically, we define $D_i$ in Equation 4 such that it is 1 if $NLPOP = -2, -1, 0, 1, 2$ ($D_i = 0$ indicates the true threshold). We estimate the regressions for all combinations of bandwidths and polynomials reported in the baseline estimates. To save space, we summarize the results in graphs.

The structure of the plots in Figure 4 is as follows. For each fake cutoff, we plot the 18 coefficient estimates obtained by combining the bandwidths and bin sizes used in the baseline regressions. Then we indicate the median value of the coefficient estimates.

In subfigure (a) of Figure 4, we present the placebo estimates for the seat share of the small parties. We find for the seat share of the small parties that the median coefficient estimate at every fake cutoff revolves around 0. At the true cutoff of 0 there is a noticeable increase. Subfigure (b) presents corresponding graphs for vote shares. The conclusions are similar: while the coefficient estimates for the vote share of smaller parties are close to 0 at the fake cutoffs, they increase significantly at the true cutoff.

Subfigure (c) presents the results for council fragmentation. As previously, the placebo tests confirm the baseline findings. The average coefficient estimate are typically around 0, both at the true and the fake cutoffs, and in one case (at $D_i = -1$) even larger than at the true cutoff.

We finally collect in subfigure (d) the placebo estimates for the size of the council. Here, too, the placebo tests confirm the baseline estimates. The average coefficient estimate at the fake cutoffs revolves around 0, but decreases noticeably at the true cutoff. Overall, this first set of placebo tests supports the baseline results.

As a second set of placebo tests, we let the treatment set in at a fake treatment year. More specifically, we limit the sample to the pre-treatment period (1989-1997) and let the treatment set in in 1993. We collect the results in a Figure 5. The median coefficient at the fake cutoffs for each of the four outcome measures is indicated in red. For comparison, we also indicate the median estimate at the true cutoff with a blue dot.

We find that the median estimate for the seat shares of the small parties is around 2 percent for the fake treatment year. While larger than 0, the estimates are smaller than the median estimate for the true treatment year. Also, the placebo estimates seem to vary a lot, with some estimates being very large – i. e. up to a value of 6 – and having correspondingly large standard errors.

The median coefficient estimate for the vote share of small parties for the fake treatment year is also positive, but smaller than in the placebo seat share regressions. But as in the seat share regressions, the vote share estimates seem to suffer from a lot of variability.

While it is possible that for these two outcome variable, even the Diff-in-Disc design does not fully account for differential trends, the fact that the estimated treatment effects are noticeably smaller for the placebo than for the true treatment year indicates that the abolishment of the threshold had indeed a positive effect on the seat and vote shares of the small parties.

For council fragmentation and council size, the estimates at the fake treatment year are close to 0. Hence, these placebo regressions unambiguously confirm the baseline results.

## 6.2 Individual cutoffs

Are the baseline estimates driven by only selected cutoffs? To answer this question, 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.

The results are collected in Table 6. In line with the baseline results, the coefficient estimates for the seat share of small parties at each of the individual cutoffs are consistently positive, but display economically significant values only at the first two thresholds, a value of 6.285 at the 3001 and 3.805 at the 5001 inhabitants cutoffs, respectively. While these two coefficients are not statistically significant either, presumably because of the smaller sample size, they display large t-statistics.

Given that the implicit threshold is about 1.16 percentage points smaller to the right than to the left of the 3001 inhabitants cutoff, a 1 percentage point stronger treatment (i. e. a 1 percent point lower implicit threshold) increases the vote share of the small parties by about 5.4 percentage points. The economic effect at the 5001 cutoff is similar. When the estimated coefficient is scaled by the strength of the treatment, which is about 0.56 percentage points, we can conclude that an increase in the treatment strength by 1 percentage point results in an expansion of the seat share of small parties by about 6.8 percentage points. While still large, these estimates imply smaller treatment effects than those derived from the baseline estimates.

The results for vote shares once again indicate that the changes in seat shares appear to be driven primarily by psychological effects on voting behavior. That is, the coefficient estimates for the vote shares of small parties displays the same pattern as the coefficients for the seat shares.

Regarding council fragmentation, we find no consistent effect of the reform as in the baseline results. The coefficients change signs at the various thresholds and are never significant.

Finally, the average coefficient estimates for council size at the individual population cutoffs, too, confirm the baseline findings. In particular, for all cutoffs except the first the coefficient estimates are negative and significant. As discussed above, politicians may have an incentive to reduce council size in order to counteract the effects of the abolishment of the explicit electoral threshold. However, they might have little leeway to do so if the council is already small, which in turn might be the reason why we fail to observe a reduction of the council size at the lowest cutoff.

### 6.3 Seat shares for individual parties

As an extension of the results for aggregated seat shares, we report in Table 7 the effect of the abolishment of the electoral threshold on individual party seat shares. The coefficient estimates for the large conservative party, the CDU, are consistently negative (with two exceptions) and sometimes significant. The coefficient estimates range around -2. The estimate for the large left-wing party, the SPD, is also typically negative, but is of smaller magnitude than for the CDU. Similarly, the coefficient estimates for the small conservative party, the FDP, is typically negative as well, but of a smaller magnitude than those for the CDU. The estimates for the green party are close to 0. Overall, it appears that the CDU has suffered the most from the abolishment of the electoral threshold. The SPD and FDP, while also experiencing a reduction in their seat shares, were harmed less.

Who gained from the electoral losses of the mainstream parties? While there are many small parties at the local level in Hesse, the most important ones, as alluded above, are municipal-specific voter initiatives. We therefore relate the abolishment of the threshold to the aggregated seat shares of these initiatives. The estimated coefficients are consistently positive, sometimes significant, and of similar magnitude than those obtained for the seat shares of the small parties in the baseline regressions. Consequently, the voter initiatives appear to have gained the most from the reform.

## 7 Conclusion

We study whether electoral thresholds are effective. Our results indicate that abolishing an explicit electoral threshold increases the seat share of smaller parties at the expense of more established national parties. Further analysis indicates that the seat gains of the small parties emerge primarily because of psychological rather than mechanical effects of the reform. The seat shares of the small parties increase roughly by the same amount as vote shares. One reason why mechanical effects fail to materialize seems to be that politicians from the more established parties replaced the explicit threshold with higher implicit ones by reducing the size of the council. Overall, electoral thresholds appear to have a causal effect on political outcomes.

These findings show, on the one hand, that electoral thresholds might 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 legal electoral thresholds, our results suggest that policy makers and voters should be aware they entail a strong trade off between legislative cohesion and political representation.

## Acknowledgments

We thank Sebastian Blesse for excellent research assistance. We are also grateful to various staff members from the Hessian state statistical office for providing the electoral data.

## References

Baskaran, T. (2012). The flypaper effect: evidence from a natural experiment with Hessian municipalities. Mimeo (University of Goettingen).

Calvo, E. and J. P. Micozzi (2005). The governor's backyard: A seat-vote model of electoral reform for subnational multiparty races. *The Journal of Politics 67 (4)*, 1050–1074.

Carey, J. M. and S. Hix (2011). The electoral sweet spot: Low-magnitude proportional electoral systems. *American Journal of Political Science 55 (2)*, 383–397.

Duverger, M. (1954). *Political Parties: Their Organization and Activity in the Modern State*. New York: Wiley.

Egger, P. and M. Koethenbuerger (2010). Government spending and legislative organization: quasi-experimental evidence from Germany. *American Economic Journal: Applied Economics 2*, 200–212.

Finkelstein, A. (2007). The aggregate effects of health insurance: evidence from the introduction of medicare. *Quarterly Journal of Economics 122*, 1–37.

Fiva, J. H. and O. Folke (2011). Mechanical and psychological effects of electoral reform. CESifo Working Paper 3505.

Gebethner, S. (1997). Free elections and political parties in transition to democracy in Central and Southeastern Europe. *International Political Science Review 18 (4)*, 381–399.

Grembi, V., T. Nannicini, and U. Troiano (2012). Policy responses to fiscal restraints: a difference in discontinuity design. IGIER Working Paper.

Haggard, S. and R. R. Kaufman (1997). The political economy of democratic transitions. *Comparative Politics 29*, 263–283.

Hoxby, C. (2000). The effects of class size on student achievements: new evidence from population variation. *Quarterly Journal of Economics 115*, 1239–1985.

Imbens, G. and K. Kalyanaraman (2011). Optimal bandwidth choice for the regression discontinuity estimator. *Review of Economic Studies forthcoming*.

Laakso, M. and R. Taagepera (1979). Effective number of parties: a measure with application to West Europe. *Comparative Political Studies 12*, 3–27.

Lee, D. S. and T. Lemieux (2010). Regression discontinuity designs in economcis. *Journal of Economic Literature 48*, 281–355.

Lijphart, A. (1968). Typologies of democratic systems. *Comparative Political Studies 1*, 3–44.

Lijphart, A. (1990). The political consequences of electoral laws, 1945-85. *American Political Science Review 84*, 481–496.

Lijphart, A. (1991). Constitutional choices for new democracies. *Journal of Democracy 2*, 72–84.

Lijphart, A. (1994). *Electoral systems and party systems*. Oxford: Oxford University Press.

Lijphart, A. and R. W. Gibberd (1977). Thresholds and payoffs in list systems of proportional representation. *European Journal of Political Research 5*, 219–244.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: a density test. *Journal of Econometrics 142*, 698–714.

Moser, R. G. (1999). Electoral systems and the number of parties in postcommunist states. *World Politics 51*(3), 359–384.

Moser, R. G. and E. Scheiner (2004). Mixed electoral systems and electoral system effects: controlled comparision and cross-national analysis. *Electoral studies 23*, 575–599.

Perea, E. A. (2002). Individual characteristics, institutional incentives and electoral abstention in western europe. *European Journal of Political Research 41*, 643–673.

Rae, D. W. (1971). *The political consequences of electoral laws*. New Haven: Yale University Press.

Rae, D. W., V. J. Hanby, and J. Loosemore (1971). Thresholds of representation and thresholds of exclusion: An analytic note on electoral systems. *Comparative Political Studies 3*, 479–488.

Remmer, K. L. (2008). The politics of institutional change: Electoral reform in Latin America, 1978-2002. *Party Politics 14 (1)*, 5–30.

Riker, W. H. (1982). The two-party system and Duverger's law: An essay on the history of political science. *American Political Science Review 76 (4)*, 753–766.

Rokkan, S. (1968). Electoral systems. In *International Encyclopedia of the Social Sciences*. Macmillan.

Taagepera, R. (1989). Empirical thresholds of representation. *Electoral Studies 8 (2)*, 105–116.

Taagepera, R. and M. Laakso (1980). Proportionality profiles of West European electoral systems. *European Journal of Political Research 8*, 423–446.

Vatter, A. (2003). Legislative party fragmentation in Swiss cantons: A function of cleavage structures or electoral institutions? *Party Politics 9 (4)*, 445–461.

Table 1: Population thresholds 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 thresholds 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.

Table 2: The abolishment of the election threshold and seat share of small parties.

| | BW=0.5 | BW=0.4 | BW=0.3 | BW=0.2 | BW=0.15 | BW=0.1 |
|---|---|---|---|---|---|---|
| Linear | -0.575 | 0.168 | 1.604 | 4.032** | 2.296 | 4.345* |
| | (1.593) | (1.503) | (1.478) | (1.746) | (1.969) | (2.261) |
| Quadratic | 2.386 | 3.197* | 5.445** | 2.174 | 2.914 | 6.936* |
| | (1.547) | (1.914) | (2.236) | (2.782) | (3.198) | (3.695) |
| Cubic | 4.502* | 5.871** | 2.818 | 4.404 | 5.096 | 3.733 |
| | (2.459) | (2.589) | (3.087) | (3.743) | (4.235) | (4.395) |
| N | 3070 | 2474 | 1893 | 1260 | 970 | 640 |

Notes: This table presents difference-in-discontinuity regressions for the seat share of the non-mainstream (small) parties in Hessian municipal councils. All population thresholds 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 (0.5, 0.4, 0.3, 0.2, 0.15, 0.1) and increasingly flexible polynomials (linear to cubic) 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 abolishment of the election threshold and vote share of small parties.

| | BW=0.5 | BW=0.4 | BW=0.3 | BW=0.2 | BW=0.15 | BW=0.1 |
|---|---|---|---|---|---|---|
| Linear | -0.492 | 0.215 | 1.539 | 4.034** | 2.652 | 4.476** |
| | (1.570) | (1.482) | (1.452) | (1.732) | (1.962) | (2.271) |
| Quadratic | 2.386 | 3.186* | 5.443** | 2.647 | 3.123 | 7.694** |
| | (1.527) | (1.882) | (2.209) | (2.747) | (3.153) | (3.618) |
| Cubic | 4.501* | 5.909** | 3.584 | 4.511 | 5.557 | 4.933 |
| | (2.419) | (2.543) | (3.046) | (3.679) | (4.192) | (4.372) |
| N | 3070 | 2474 | 1893 | 1260 | 970 | 640 |

Notes: This table presents difference-in-discontinuity regressions for the vote share of non-mainstream (small) parties in Hessian municipal councils. All population thresholds 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 (0.5, 0.4, 0.3, 0.2, 0.15, 0.1) and increasingly flexible polynomials (linear to cubic) 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 4: The abolishment of the election threshold and the fragmentation of the council.

| | BW=0.5 | BW=0.4 | BW=0.3 | BW=0.2 | BW=0.15 | BW=0.1 |
|---|---|---|---|---|---|---|
| Linear | -1.223 | -0.693 | -0.026 | 1.277 | -0.019 | 1.942 |
| | (1.043) | (1.055) | (1.033) | (1.076) | (1.152) | (1.370) |
| Quadratic | 0.447 | 0.947 | 1.976 | 0.266 | 1.523 | 0.780 |
| | (1.076) | (1.130) | (1.246) | (1.381) | (1.560) | (1.946) |
| Cubic | 1.873 | 2.405* | 0.970 | 1.600 | 0.754 | -1.019 |
| | (1.322) | (1.428) | (1.514) | (1.776) | (2.016) | (2.389) |
| N | 3070 | 2474 | 1893 | 1260 | 970 | 640 |

Notes: This table presents difference-in-discontinuity regressions for the fragmentation of the council. All population thresholds 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 (0.5, 0.4, 0.3, 0.2, 0.15, 0.1) and increasingly flexible polynomials (linear to cubic) 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 5: The abolishment of the election threshold and the number of council seats.

| | BW=0.5 | BW=0.4 | BW=0.3 | BW=0.2 | BW=0.15 | BW=0.1 |
|---|---|---|---|---|---|---|
| Linear | -1.762*** | -2.051*** | -2.066*** | -2.386*** | -2.917*** | -3.471*** |
| | (0.325) | (0.330) | (0.375) | (0.491) | (0.510) | (0.647) |
| Quadratic | -2.158*** | -2.206*** | -2.470*** | -3.350*** | -3.608*** | -3.719*** |
| | (0.408) | (0.496) | (0.590) | (0.692) | (0.774) | (0.964) |
| Cubic | -2.242*** | -2.572*** | -3.266*** | -3.887*** | -4.001*** | -4.096*** |
| | (0.597) | (0.683) | (0.752) | (0.901) | (1.029) | (1.284) |
| N | 3070 | 2474 | 1893 | 1260 | 970 | 640 |

Notes: This table presents difference-in-discontinuity regressions for the size of the council (number of seats). All population thresholds 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 (0.5, 0.4, 0.3, 0.2, 0.15, 0.1) and increasingly flexible polynomials (linear to cubic) 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: Effect of abolishment at individual thresholds.

| T=3001 | T=5001 | T=10001 | T=25001 |
|---|---|---|---|
| **Small parties' seat share** | | | |
| 6.285 | 3.805 | 0.798 | 0.645 |
| (4.139) | (2.842) | (2.489) | (4.407) |
| **Small parties' vote share** | | | |
| 5.979 | 3.918 | 0.922 | 0.480 |
| (4.060) | (2.868) | (2.409) | (4.299) |
| **Council fragmentation** | | | |
| -0.747 | 1.277 | 0.784 | -2.671 |
| (2.731) | (1.768) | (1.583) | (1.877) |
| **Council size** | | | |
| -0.116 | -3.419*** | -2.663*** | -4.479*** |
| (0.487) | (0.765) | (0.657) | (1.121) |

Notes: This table presents difference-in-discontinuity regressions at the following individual population thresholds: 3001, 5001, 10001, and 25001. Estimates for the average treatment effect of abolishing the election threshold are reported for a bandwidth of 0.5 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 the municipality and robust to heteroscedasticity. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

Table 7: The abolishment of the election threshold and seat shares of individual parties.

| | BW=0.5 | BW=0.4 | BW=0.3 | BW=0.2 | BW=0.15 | BW=0.1 |
|---|---|---|---|---|---|---|
| **CDU** | | | | | | |
| Linear | 1.201 | 0.405 | -1.023 | -2.131* | -1.508 | -1.994 |
| | (1.112) | (1.059) | (1.063) | (1.192) | (1.312) | (1.406) |
| Quadratic | -1.047 | -1.670 | -2.468* | -0.864 | -0.858 | -2.804 |
| | (1.119) | (1.326) | (1.411) | (1.650) | (1.844) | (2.469) |
| Cubic | -2.325 | -2.688* | -1.008 | -1.728 | -2.509 | -4.138 |
| | (1.521) | (1.602) | (1.798) | (2.243) | (2.631) | (3.206) |
| **SPD** | | | | | | |
| Linear | 0.148 | -0.024 | -0.268 | -0.783 | 0.044 | -1.449 |
| | (1.128) | (1.074) | (1.095) | (1.245) | (1.455) | (1.654) |
| Quadratic | -0.834 | -0.982 | -1.508 | 0.061 | -0.730 | -2.334 |
| | (1.180) | (1.401) | (1.599) | (1.973) | (2.140) | (2.341) |
| Cubic | -1.228 | -1.273 | -0.404 | -0.700 | -0.688 | 2.736 |
| | (1.751) | (1.822) | (2.194) | (2.432) | (2.791) | (2.918) |
| **FDP** | | | | | | |
| Linear | -0.460 | -0.442 | -0.333 | -0.556 | -0.499 | -1.449 |
| | (0.479) | (0.495) | (0.573) | (0.703) | (0.795) | (0.969) |
| Quadratic | -0.315 | -0.399 | -0.691 | -0.869 | -1.377 | -1.883 |
| | (0.616) | (0.729) | (0.853) | (1.030) | (1.169) | (1.375) |
| Cubic | -0.526 | -0.852 | -1.199 | -2.332* | -2.223 | -2.433 |
| | (0.858) | (0.966) | (1.109) | (1.352) | (1.518) | (1.896) |
| **Green Party** | | | | | | |
| Linear | -0.313 | -0.106 | 0.020 | -0.563 | -0.333 | 0.547 |
| | (0.693) | (0.731) | (0.774) | (0.908) | (0.985) | (1.273) |
| Quadratic | -0.190 | -0.145 | -0.778 | -0.502 | 0.051 | 0.087 |
| | (0.832) | (0.964) | (1.083) | (1.271) | (1.498) | (1.877) |
| Cubic | -0.423 | -1.059 | -0.207 | 0.356 | 0.325 | 0.102 |
| | (1.111) | (1.265) | (1.423) | (1.788) | (2.017) | (2.651) |
| **Voter initiatives** | | | | | | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Linear | -0.955 | -0.310 | 0.990 | 3.230* | 1.776 | 3.918 |
| | (1.564) | (1.463) | (1.456) | (1.774) | (2.005) | (2.451) |
| Quadratic | 1.840 | 2.502 | 4.697** | 1.913 | 2.539 | 6.203 |
| | (1.535) | (1.927) | (2.273) | (2.856) | (3.338) | (3.914) |
| Cubic | 3.780 | 5.174* | 2.393 | 3.804 | 3.671 | 2.067 |
| | (2.465) | (2.649) | (3.162) | (3.899) | (4.447) | (4.787) |
| N | 3070 | 2474 | 1893 | 1260 | 970 | 640 |

Notes: This table presents difference-in-discontinuity regressions for the seat share of individual parties. All population thresholds 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 (0.5, 0.4, 0.3, 0.2, 0.15, 0.1) and increasingly flexible polynomials (linear to quartic) 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%(***).

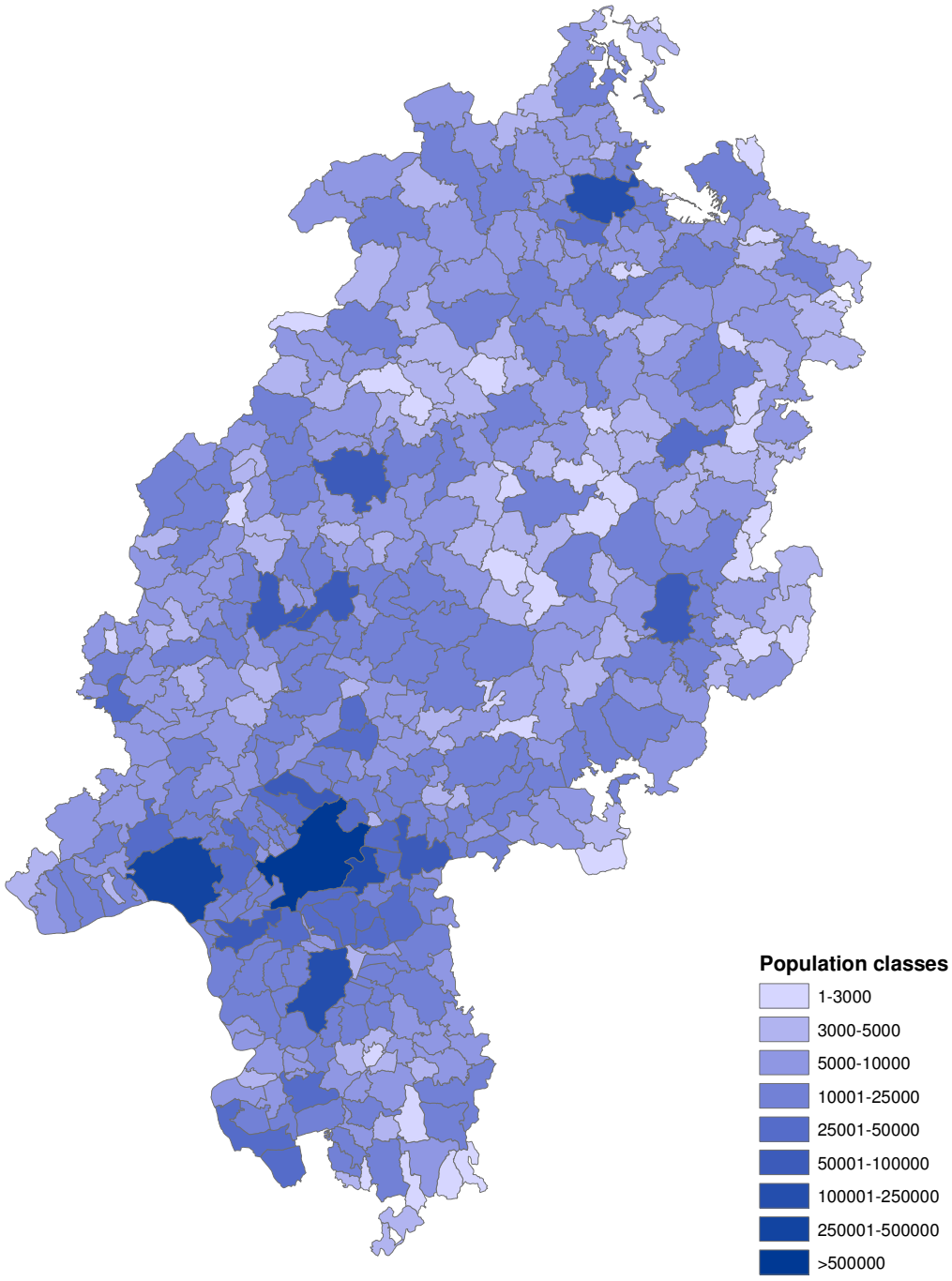
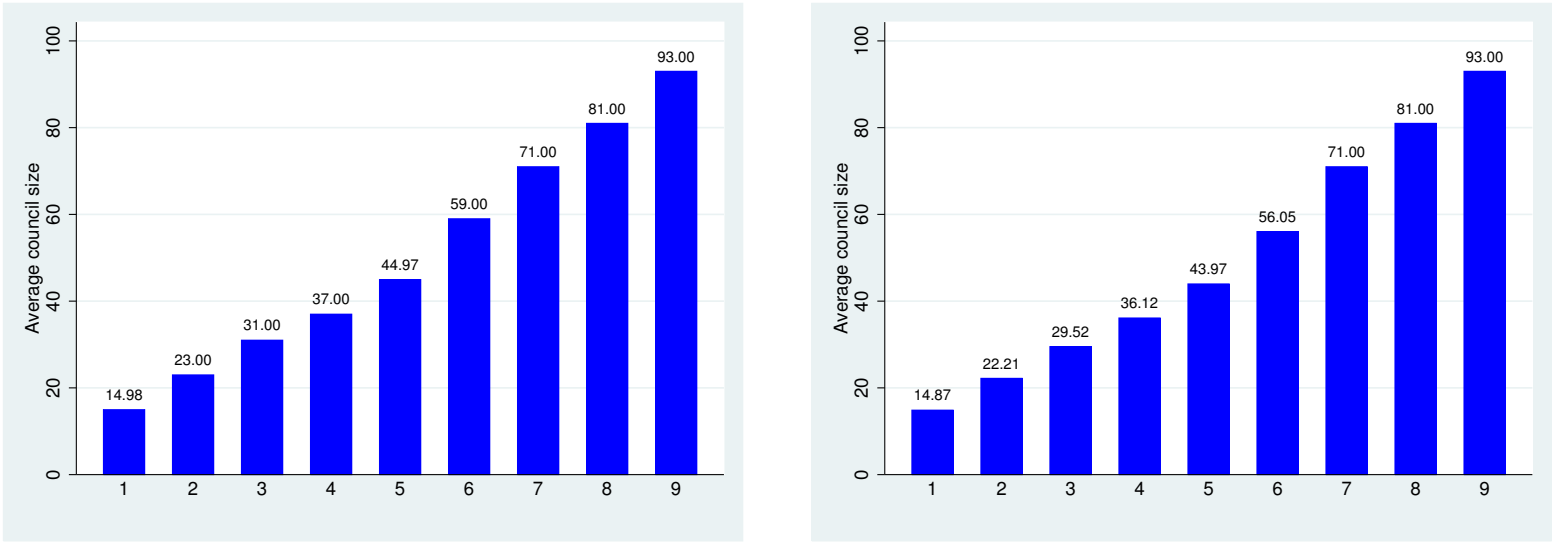


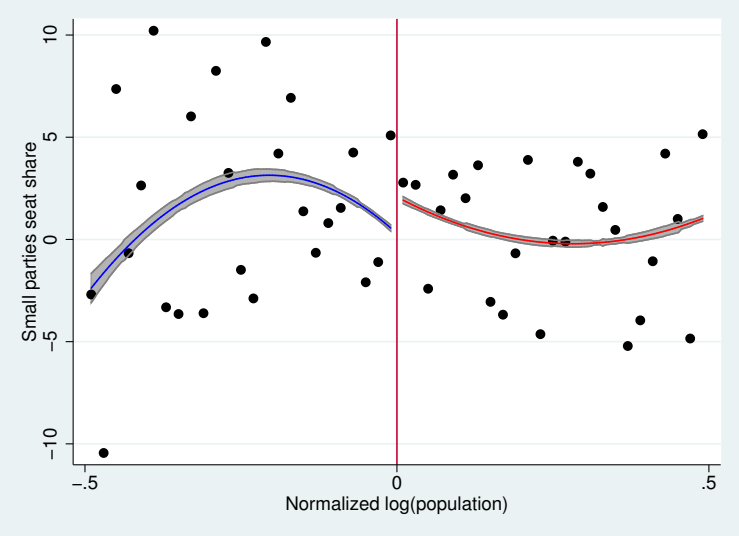

Figure 1: Average population sizes in Hessian municipalities during the sample period.



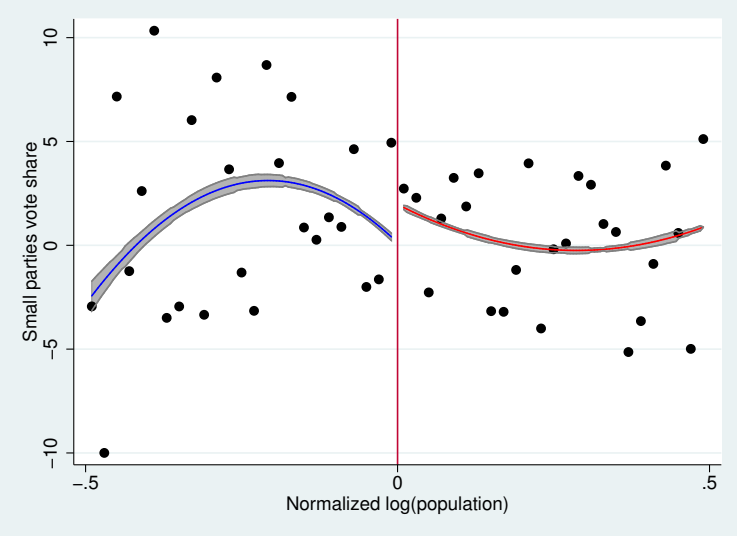

(a) Before 2001

(b) After 2001

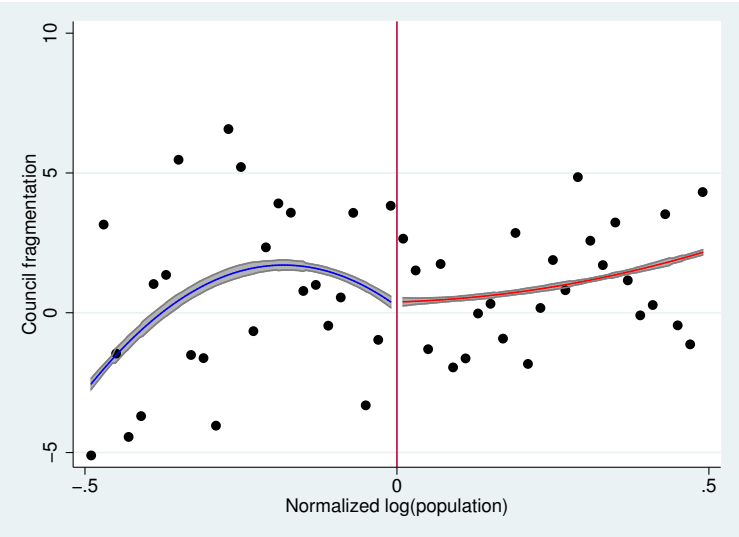
Figure 2: Average council size in different population brackets prior and after the election of 2001. This figure shows the average council size of municipalities in population brackets 1-3000 (1), 3001-5000 (2), 5001-10000 (3), 10001-25000 (4), 25001-50000 (5), 50001-100000 (6), 100001-250000 (7), 250001-500000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



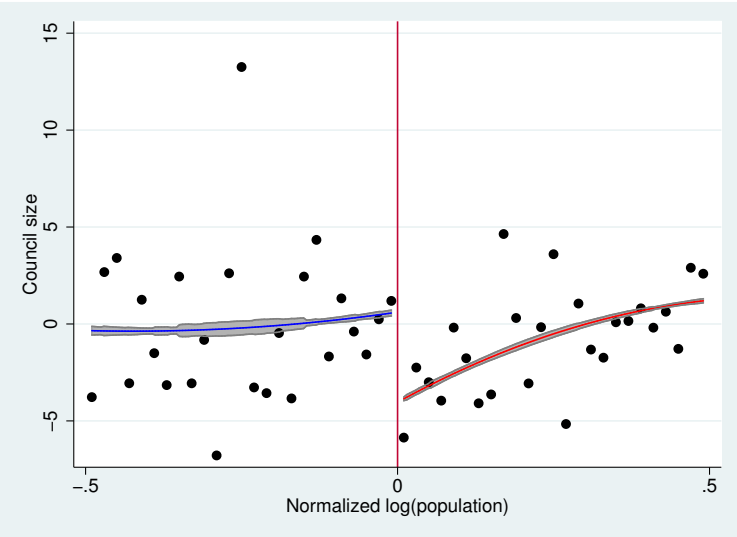

(a) Seat shares




(b) Vote shares

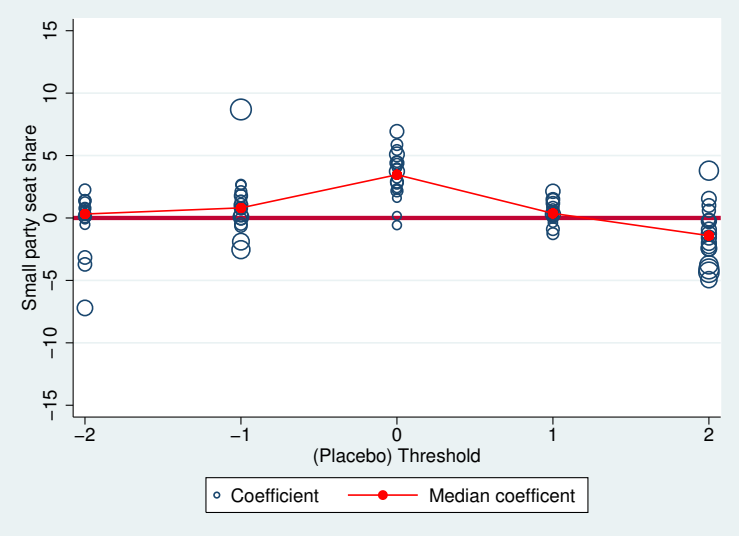



(c) Council fragmentation

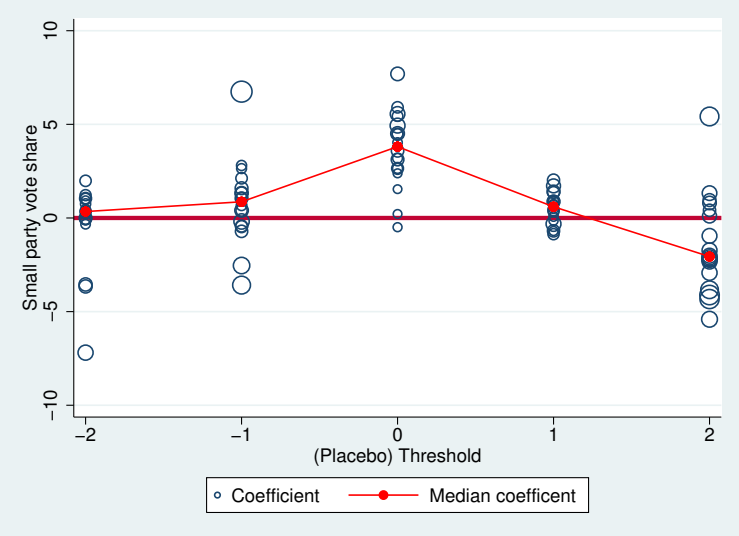



(d) Council size

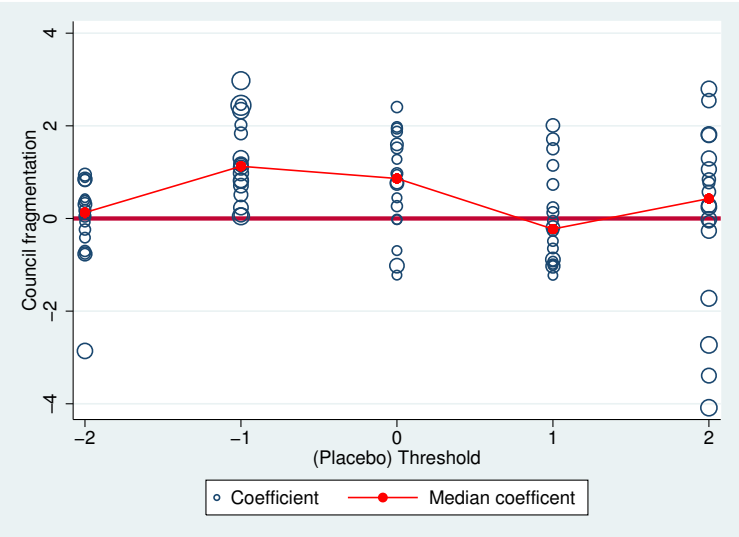
Figure 3: Abolishment of electoral thresholds and political outcomes. This figure shows Diff-in-Disc plots for the change in aggregated seat and vote shares of the small parties, council fragmentation, and council size from the pre- to the post-treatment periods. Observations are averaged within bins of size 0.02. The polynomial plots are constructed using a rectangular kernel, a degree of 2, a bandwidth of 0.5., and the number of observations within bins as frequency weights.



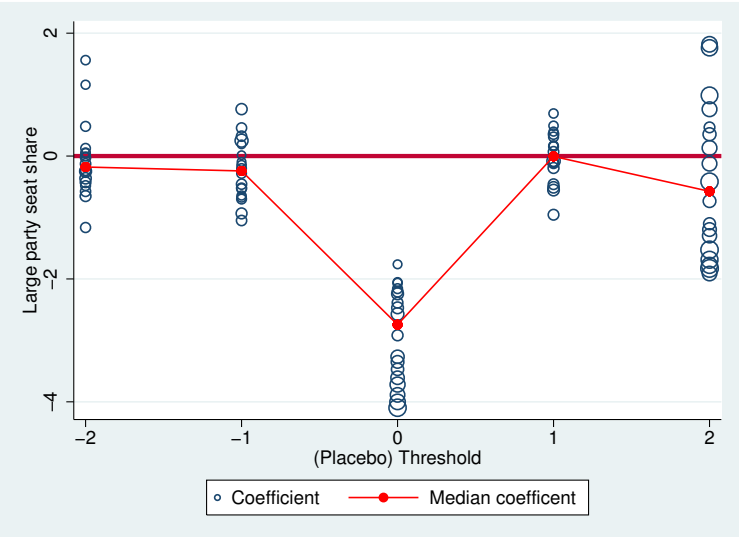

(a) Seat shares




(b) Vote shares




(c) Council fragmentation




(d) Council size

Figure 4: Placebo treatments with fake thresholds. This figure shows coefficient estimates of the Diff-in-Disc model for the seat and vote share of small parties, council fragmentation, and council size 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 D= -2, -1, 1, 2. For comparison, the coefficient estimates at the true threshold of 0 are also indicated.

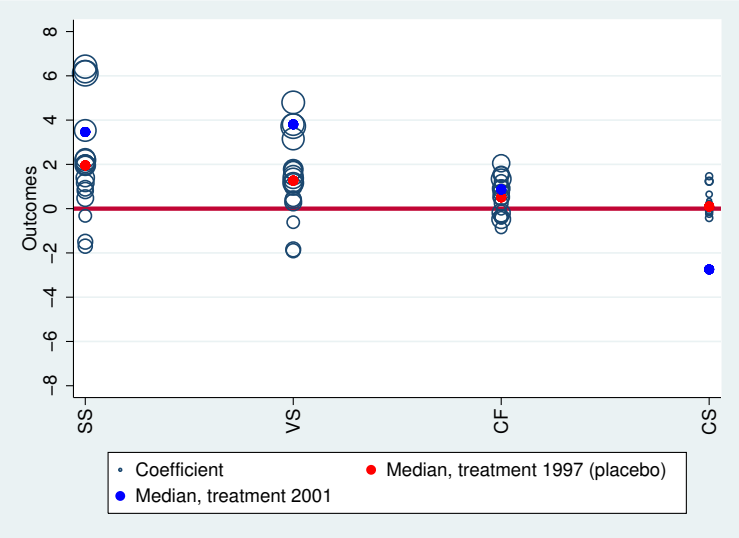
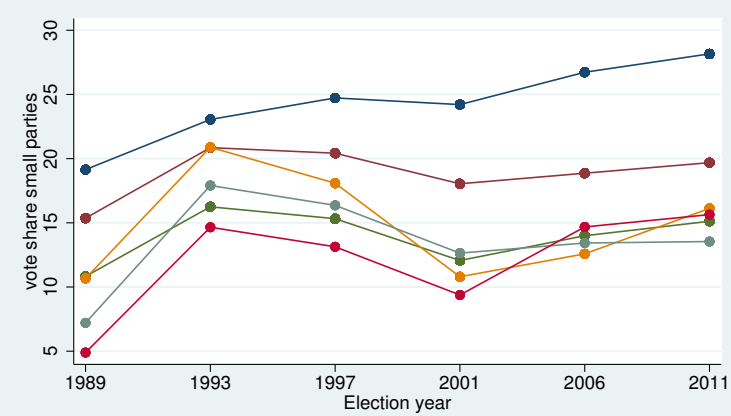


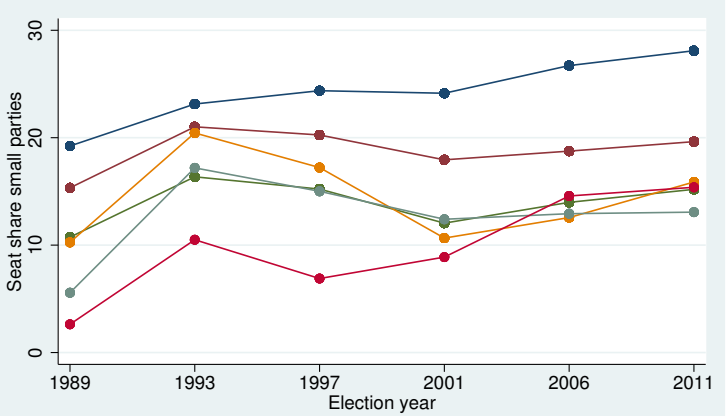

Figure 5: Placebo treatment for placebo year. This figure shows coefficient estimates of the Diff-in-Disc model with a placebo treatment defined to set in 1993. The sample covers the period 1989-1997. The size of the dots indicates the standard error of each estimate. Coefficient estimates are reported for the small party seat share (SS), small party vote share (VS), council fragmentation (CF), and council size (CS). The median estimate at the fake treatment year is indicated with a red dot. The median estimate at the true treatment year is indicated with a blue dot.

Table A.1: Summary statistics

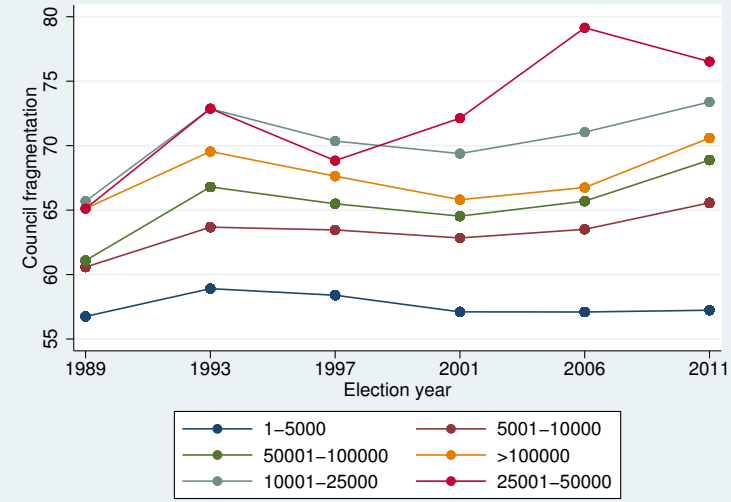
| Variable | | Mean | SD | Min. | Max. | N |
|---|---|---|---|---|---|---|
| Small party seat share | overall | 18.336 | 15.348 | 0.000 | 100.000 | 2554 |
| | between | | 13.746 | 0.000 | 100.000 | 426 |
| | within | | 6.854 | -16.447 | 57.659 | 5.995 |
| Small party vote share | overall | 18.428 | 15.212 | -0.100 | 100.000 | 2554 |
| | between | | 13.645 | -0.017 | 100.000 | 426 |
| | within | | 6.754 | -16.904 | 56.178 | 5.995 |
| Council fragmentation | 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 | 16311.080 | 125764.500 | 638.000 | 6092891.000 | 2555 |
| | between | | 296469.300 | 727.500 | 6092891.000 | 427 |
| | within | | 1083.861 | -10366.420 | 38246.580 | 5.984 |



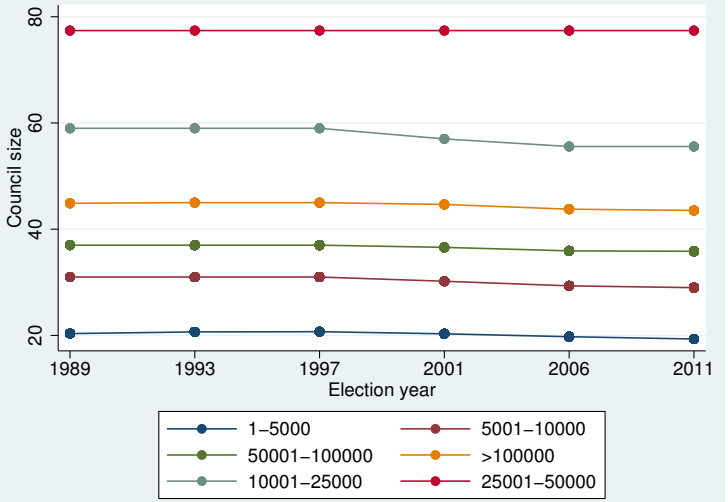

(a) Small parties' seat share




(b) Small parties' vote share




(c) Council fragmentation




(d) Council size

Figure A.1: Development of political outcome variables over time in municipalities within different population brackets.
This figure shows how the seat and vote shares of small parties, council fragmentation, and council size have evolved in Hessian municipalities over the period 1989-2011. Data are averaged for municipalities located in different population brackets.

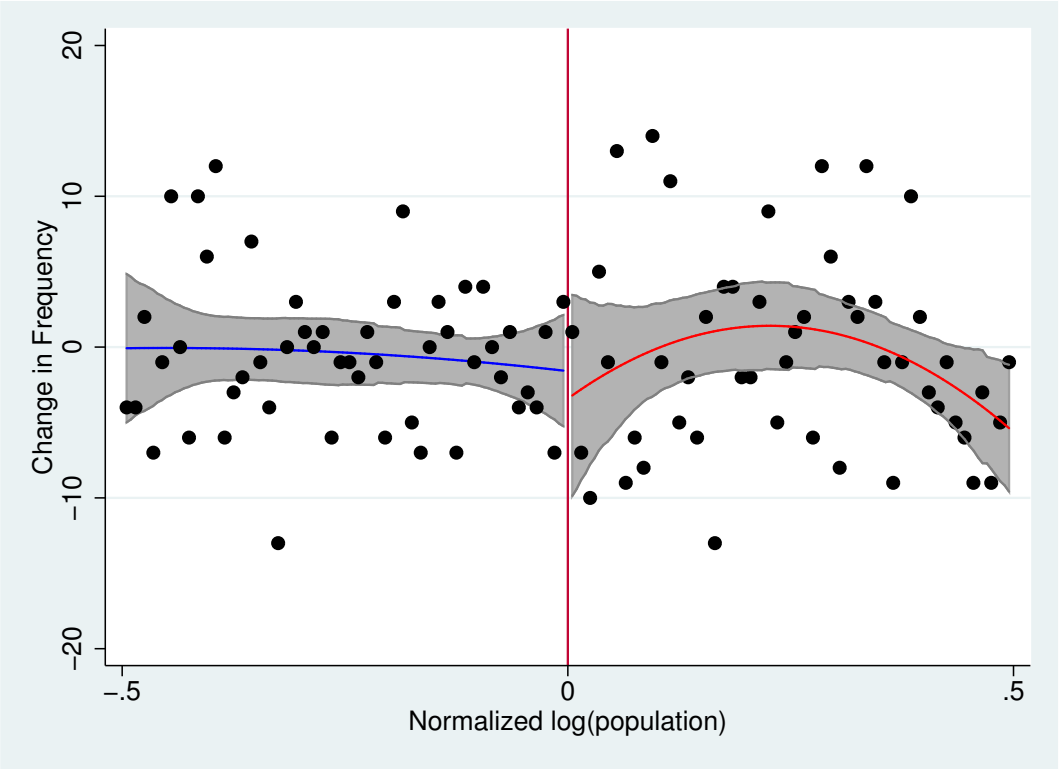



Figure A.2: Density plots for change in normalized log population size. This figure presents a density plot for the Diff-in-Disc design in the spirit of McCrary plots (McCrary, 2008). We first divide normalized log population size in bins of width 0.01. Then we calculate the change in the total number 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. We also plot 95% confidence intervals.