ELECTORAL COMPETITION AND ENDOGENOUS POLITICAL INSTITUTIONS: QUASI-EXPERIMENTAL EVIDENCE FROM GERMANY

Thushyanthan Baskaran
Mariana Lopes da Fonseca

GEORG-AUGUST-UNIVERSITÄT GÖTTINGEN

Electoral competition and endogenous political institutions: quasi-experimental evidence from 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

Do established parties change political institutions to disadvantage smaller, non-mainstream parties if the latters' electoral prospects improve? We study this question with a natural experiment from the German federal state of Hesse. The experiment is the abolishment of an explicit electoral threshold (the so called "five percent hurdle") for local elections in 2001 by the Hessian state parliament. The abolishment improved the electoral prospects of smaller parties at local elections, but local politicians from large mainstream parties had the ability to adjust municipal political institutions in such a way as to counteract the increased competitiveness of smaller parties. One such institutional adjustment is to reduce the size of the local council and thereby raise implicit electoral thresholds. Using a dataset that covers all 426 Hessian municipalities over the period 1989-2011, we document with a difference-in-discontinuity design that municipalities where the electoral competitiveness of smaller parties improved more because of the abolishment of the explicit threshold, reduced their council size more. Hence, established parties appear to erect barriers to entry by adjusting political institutions once new political formations become viable electoral alternatives.

Keywords: Electoral rules, Electoral thresholds, Political competition

JEL codes: D70, D72, D78

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1 Introduction

Even if all constitutional authority is supposed to derive from the will of the people, elites wield significant influence over the political process. In many countries, it is the political elite that decides how electoral districts are shaped, how votes are translated into seats, and how many seats the legislature has. Since the rules that govern the constitutional life of a country, its political institutions, can be reshaped by those who currently hold the reigns of power, it seems plausible that they would adjust these rules in an ad hoc fashion to maintain their position at the top of the political order. The political elite, in short, may change institutions in such a way as to erect new barriers to entry for new parties once they become a credible electoral threat (Doron and Maor, 1991).

Anecdotal evidence suggests that mainstream parties adopt measures to disadvantage new political formations if the latter become too successful. After the success of Ross Perot's Campaign in the 1992 US presidential race, for example, third party candidates were prevented by the Commission on Presidential Debates – which is controlled by both the Republican and Democratic parties – from participating in future presidential debates, a measure that likely diminished the electoral prospects of non-mainstream parties.

While such anecdotal evidence can be found easily, the question is whether it is indicative of a general phenomenon. This paper is one of the first to offer credible empirical evidence on whether established parties adjust political institutions to disadvantage smaller parties. To do so, it makes use of a natural experiment in the German State of Hesse: an electoral reform for local elections passed by the Hessian state parliament in 1999 and implemented in 2001 (when the first election after the reform was held). The professed purpose of the reform was to increase the degree of political competition at the local level. Its most important aspect was the abolishment of an explicit electoral threshold, the so called "five-percent hurdle". Before this threshold was abolished, parties only received seats in the local

council if their vote share surpassed at least five percent. Consequently, smaller parties were handicapped in the pre-reform period: first because they were prevented from receiving seats to which they were entitled to given their vote share; second because voters might have been reluctant to vote for small parties in the first place given the non-negligible chance that their vote would be "wasted" if their preferred party did not overcome the electoral threshold.

After the reform, smaller parties became a more viable electoral alternative because they required, in general, a much lower vote share than five percent to gain their first council seat and thereby legislative representation. The question we ask in this paper is how the established political parties reacted to the exogenous increase in the electoral competitiveness of smaller parties: did they change the prevailing local political institutions to put smaller parties back at a disadvantage?

We show that at least one municipal political institution was adjusted after the reform: the size of the local council. By reducing the number of council seats, which can be done through a two-third council majority, the mainstream parties raised implicit thresholds – the minimum vote share that a party has to gain to receive at least one seat – and thus made it harder for smaller parties to gain their first council seat.

Since all Hessian municipalities were subject to the abolishment of the explicit electoral threshold, there is no obvious control group against which changes in council size from the pre- to the post-treatment period could be evaluated. Our identification strategy to uncover the causal effect of the abolishment on council size relies therefore on differences in the intensity of treatment. Municipalities that had a larger council were affected more strongly by the abolishment of the electoral threshold than those with smaller councils because municipalities with smaller councils have higher implicit thresholds to begin with. That is, the mainstream parties had a stronger incentive to reduce the size of the council if the current council size in their municipality was large. They are, however, not completely

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free in doing so. A state law relates municipal population sizes discontinuously to minimum and maximum council sizes. We exploit the discontinuous nature of the link between population size and council size to implement a variant of the regression discontinuity design (RDD), the difference-in-discontinuity design (Diff-in-Disc).

This paper contributes to the literature on political institutions, and specifically to the literature on their determinants. A strand of this literature assumes a historical perspective and analyzes how secular changes in political regimes, most notably the shift to popular democracy, were determined by conscious decisions of the elite (Acemoglu and Robinson, 2000; Aghion et al., 2004; Acemoglu and Robinson, 2006). On the other hand, the literature on the determinants of contemporaneous and arguably less decisive adjustments of political institutions within generally democratic societies is scarce. One of the few studies are Hayo and Voigt (2010, 2013) who analyze with cross-country regressions why countries witness constitutional change. They find that political factors, e. g. whether there is an internal armed conflict, determine how countries transition from a parliamentary to a presidential form of government (or vice versa). Another study is Ticchi and Vindigni (2010) who find with cross-section regressions that countries with higher income inequality are more likely to have majoritarian electoral rules.

Even rarer are studies on how political institutions are adjusted by established elites to counter threats from new political formations. Drometer and Rincke (2014) find that in the US, states which were affected more strongly by the Voting Rights Act of 1965 tightened ballot access restrictions to hinder new entrants into the political market.[1] Similarly, Trebbi et al. (2008) show that US cities systematically changed electoral rules after the adoption of the Voting Rights Act to limit minority representation.[2] Our paper primarily

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[1]Lee (2013) shows that US House incumbents respond to the threat of third party candidates within their constituency by adjusting how they vote in roll call votes. However, he does not study adjustments of political institutions.

[2]Doron and Maor (1991) and Bellettini et al. (2012) offer relevant theoretical analyses, showing for example how incumbent politicians can use bureaucratic red tape to disadvantage new entrants.

contributes to this small literature and is the first that looks at a setting other than the US. It documents that political institutions are indeed endogenous and thereby suggests that evidence from studies that take political institutions as exogenous is questionable (Trebbi et al., 2008). From a policy perspective, our results also suggest that adjustments of the wider political system by established elites should be taken into account when designing reforms aimed at increasing political competition.

Our paper is furthermore relevant for the literature that studies the consequences of legislature size and legislative fragmentation for fiscal outcomes. Relying on common pool theories, Gilligan and Matsusaka (2001), for example, find that larger US state legislatures spend more. Similar evidence for US local governments is offered by Baqir (2001). The related literature on legislative fragmentation and fiscal outcomes finds that increased fragmentation leads to higher spending and deficits (Borge, 2005). However, most studies in this literature operate under the assumption that the size of the legislature is essentially exogenous. We show that legislature size responds to political developments. Our results hence vindicate recent contributions on the effect of council size on fiscal outcomes that attempt to exploit quasi-experimental variation in council size for identification (Egger and Koethenbuerger, 2010; Pettersson-Lidbom, 2012).

2   Institutional details

2.1   Political setting

The setting for our analysis is the German federal State of Hesse. This state has about six million inhabitants who live in 426 municipalities. Municipal population sizes vary: there are, on the one hand, municipalities with less than 1000 inhabitants 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 a local council in elections held at the same date throughout the state. 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. The other important political institutions is the mayor. The mayor used to be appointed by the council (council-manager system), but as of 1993 she is directly elected by municipal inhabitants. Yet even after the reform, Hessian municipalities do not employ a full fledged mayor-council system as the competencies of the mayor remain limited (Hessami, 2014). We discuss the implications of the reform in the electoral rule for mayoral elections for our empirical strategy in Section 6.1.

Several parties contest the local council elections. First, the center-right CDU and the center-left SPD. These two parties typically receive 30 percent or more of the votes in both 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 free market economics. 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 mainstream parties, there are a number of smaller 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*) which often contest local elections by fielding a list of candidates.

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2.2   Electoral rules and the *Kommunalwahlreform*

The rules governing local council elections in Hesse differed 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 five percent, i. e. there was an explicit electoral threshold.

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 extended 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, and most importantly, the five percent electoral threshold was abolished. Parties could enter the parliament if they surpassed the *implicit threshold*, i. e. 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* – had in all likelihood similar and relatively limited effects across all municipalities (see Section 6 for a more thorough discussion of these aspects of the reform). But the third element – the abolishment of the five percent threshold – had decidedly heterogeneous effects. This aspect of the reform affected municipalities that had large municipal councils more strongly because they have smaller implicit thresholds. In a council with e. g. 100 seats a vote share of around 0.5 percent would be sufficient for a

party to gain its first council seat if there was no five percent threshold.[3] 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 five percent threshold. Consequently, the abolishment of the five percent threshold mattered less for municipalities with smaller councils. This heterogeneity in treatment intensity forms the core of our identification strategy below.

With the reform of 1999, and in particular the abolishment of the five percent threshold, the state legislature intended to improve the competitiveness of smaller parties and thereby foster political competition. Indeed, 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. In addition, voting patterns must not remain fixed (Moser and Scheiner, 2004). They will likely change such as to increase the seat shares of small parties even further. 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 small party would fail to overcome the five percent threshold. Once the threshold was abolished, voting for their preferred small party became more worthwhile for this subset of the electorate (Perea, 2002). In short, the abolishment had in all likelihood, in addition to any mechanical effects, what many authors call psychological effects (Duverger, 1954; Fiva and Folke, 2014).[4]

2.3   Council size

In each legislative period, municipalities can choose the size of their council for the next legislative period within certain ranges depending on population cutoffs that are defined in

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[3]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.

[4]We study the consequences of the reform for vote and seat outcomes in a companion paper (Baskaran and Lopes da Fonseca, 2014). The results suggest that certain small parties benefited from the reform.

the Hessian local government code.[5] Since the cutoffs are defined in the local government code (HGO) and not in the electoral law, they were not affected by the electoral reform in 1999. Table 1 notes the minimum and maximum council sizes for each population bracket according to the HGO.[6] Municipalities must choose a council size within the bracket that is relevant for them. Hence, a municipality with e. g. 1 to 3000 inhabitants must have at least 11 and at most 15 council seats while a municipality with 3001 to 5000 inhabitants must have at least 15 and at most 23 council seats

Up until one year before the next election, the current council is allowed to change the size of the next council with a two-third majority as long as final council size remains within the ranges listed in Table 1. One way through which the established parties can therefore counteract the abolishment of the five percent threshold is to lower the size of their council and thereby raise implicit thresholds for the next council. Figure 2 plots average council size in Hessian municipalities over time. There is a clear break in the trend for council size in 1997. Before 1997, average council sizes increases continuously. The reason for this continuous increase is presumably that Hessian municipalities were witnessing positive population growth, as depicted in the second plot in the graph, so that several municipalities were crossing the council size cutoffs from below and therefore were allowed to adopt larger councils.[7] From 1997 onward, however, council size has been continuously declining even as population size has been continuing to grow.

The coinciding of the reform with the start of the decline in council size is suggestive. However, it cannot be interpreted causally. For example, it is possible that even as av-

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[5]See See Art. 38 of Hessian Local Government Code (*Hessiche Gemeindeordnung*, HGO)

[6]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.

[7]Apart from "natural" demographic developments, the increase in population size of Hessian municipalities in the early nineties might be due to immigration from Eastern Germany following the German Reunification.

erage population size grew after 1997, those municipalities that reduced their council size witnessed shrinking populations. One way to control for the effect of population growth on changes in council size is to plot the development of average council size within each of the population size brackets defined by the equalization law. Figure 3 shows that before 2001, essentially all municipalities chose the largest possible council size for their bracket, either because local politicians wanted to maximize their chances of receiving a seat in the council or because the wording in the relevant article of the local government code suggests the highest possible council size as the default[8], or both.[9] There were also virtually no adjustments from 1989 to 1997, which in turn suggests that the increasing average council size in Hesse as documented in Figure 2 was indeed due to increasing population size. The only exception is a small decline in average council size from 1993 to 1997 in the lowest bracket. After 2001, however, average council sizes began to decline for all but the three highest brackets (into which altogether only five large cities fall). For example, average council sizes in municipalities that have between 50001 and 100000 inhabitants is about 3.5 seats smaller in 2011 than in 1997.

Yet, it would still be premature to relate the decline in council sizes causally to the reform. There might have been unobserved trends in Hesse that have caused municipalities to reduce their council size after 1997 but which were not systematically related to the electoral reform of 1999. For example, Hessian municipalities may have encountered fiscal difficulties and cut council sizes to reduce costs. Alternatively, the belief that smaller councils are more efficient might have gained traction throughout the state for some un-

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[8]Paragraph 1 of the relevant article states that the number of representatives in a municipalities should be as listed in Table 1. Paragraph 2 states that municipalities *may* choose the council size listed for the previous bracket, or some number in between.

[9]Two municipalities had 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) in 1989. 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 electoral law that allows municipalities that crossed any of the thresholds from above to keep the council size intended for municipalities in the next higher threshold for another legislative period.

observed reason after 1997. In short, the presence of unobserved trends cannot be ruled out. Since all Hessian municipalities were affected by the reform, there is also no obvious control group that would offer a counterfactual with which the causal effect of the reform on council size could be easily identified. The remainder of this paper is therefore concerned with identifying treatment and control municipalities within a quasi-experimental framework.

3   Empirical strategy

3.1   Difference in discontinuity design

To causally relate the reform of the electoral law to reductions in council size, we focus on the abolishment of the five percent threshold. In particular, we rely on the fact that the abolishment did not affect all Hessian municipalities equally. Municipalities that were affected less offer a reasonable counterfactual for those that were affected more strongly, as long as it can be ensured that both sets of municipalities were subject to similar trends. As indicated by our previous discussion on implicit thresholds, municipalities with larger councils were exposed to a stronger treatment because their implicit thresholds are lower. Therefore, after 2001 municipalities with larger councils should have reduced their council size more than those with small councils if local politicians use adjustments in council size to limit competition from small, non-mainstream parties.

In principle, we could compare changes in council size in municipalities that had in the pre-treatment period large councils with changes in municipalities that had small councils to uncover how local politicians react to an increase in the competitiveness of small parties. There are, however, two problems with this approach. The first is that council size is under the direct control of municipalities and therefore an endogenous variable. It is therefore questionable whether council size in the pre-treatment period can be used as an exogenous

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explanatory variable. For example, municipalities where local politicians dislike political competition might have chosen small councils already in the pre-treatment period (even though this does not seem to be the case given the evidence in Figure 3), making it more difficult to reduce council size further in the post-treatment period. Another problem is that municipalities with different pre-treatment council sizes differed in other characteristics as well, most clearly in their population size. It is hence possible that they were subject to different trends, either with respect to their council size or with respect to the political competitiveness of smaller parties. Municipalities that had larger councils in the pre-treatment period hence might have lowered their council size more strongly for reasons unrelated to the reform.

Given these difficulties when using pre-treatment council size to determine treatment intensity, we adopt the following research design. We rely on the fact that the probability of a larger council increases discontinuously at the population cutoffs as defined in Table 1. Figure 3 shows that mean council size is indeed increasing between the different brackets. Typically, municipalities chose the largest possible council size in the pre-reform period. Even though the relationship between population size and council size is fuzzy at the cutoffs, since the council size that municipalities to the left and the right of each cutoff are allowed to adopt overlap to some extent, 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, in expectation, increase discontinuously at the population cutoffs. 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 in both the pre- and the post-treatment period.

Since treatment intensity increases in council size, and the probability of a larger council increases discontinuously in population size, we can implement a fuzzy RDD (Lee and

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Lemieux, 2010) using pre- and post-treatment data to identify the causal effect of the abolishment of the electoral threshold. The basic idea is to estimate the effects of the population cutoffs on council size for the pre- and post-treatment periods, and then to observe whether there is a difference in the effect of the cutoffs between the two periods. If the effect of the cutoffs has changed between the pre-and the post-treatment period, we can reasonably ascribe the change to the reform of 1999.

In other words, we study whether municipalities that have population sizes barely above either of the cutoffs defined in Table 1, and therefore in expectation discontinuously larger councils, reduce their council size more strongly than municipalities with population sizes barely below the cutoffs, and therefore discontinuously smaller councils. Since RDD designs rely on local randomization, this design implicitly accounts for differential trends between municipalities that were exposed to stronger and weaker treatments.

More formally, the effect of a stronger treatment at M, the natural log of a given threshold, in period t can be defined as follows:

$$\hat{\gamma}_t^M = \lim_{NLPOP_i \downarrow 0} E[y_{i,t}|NLPOP, t] - \lim_{NLPOP_i \uparrow 0} E[y_{i,t}|NLPOP, t], \tag{1}$$

where $y_{i,t}$ is council size in period t and $\hat{\gamma}_t^M$ 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 $\hat{\gamma}_t^M$ can be obtained with the following general RDD model in a regression framework:

$$\begin{aligned} y_{i,t}^M = &\gamma_t^M D_i + f(NLPOP) + D_i f(NLPOP) + \epsilon_i \\ &\text{if } |NLPOP| < h, \end{aligned} \tag{2}$$

where D_i is a dummy that is 1 if $NLPOP \geq 0$ and 0 else. $f(NLPOP)$ is a flexible polynomial 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.

An estimate for the effect of the abolishment of the electoral threshold on council size is the difference in the treatment effects in the pre- and post-treatment period:

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\begin{aligned}
\hat{\delta}^M \equiv{} & \hat{\gamma}^M_{t \geq T} - \hat{\gamma}^M_{t < T} \\
={} & \lim_{NLPOP_i \downarrow 0} E[y_{i,t}|NLPOP, t \geq T] - \lim_{NLPOP_i \uparrow 0} E[y_{i,t}|NLPOP, t \geq T] \\
& - \left(\lim_{NLPOP_i \downarrow 0} E[y_{i,t}|NLPOP, t < T] - \lim_{NLPOP_i \uparrow 0} E[y_{i,t}|NLPOP, t < T] \right),,
\end{aligned}
\tag{3}
$$

where $t \geq T$ denotes the post- and $t < T$ the pre-treatment period.

The corresponding model in a regression framework 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 RD design specified in Equation 2 and is labeled Diff-in-Disc design by Grembi et al. (2012). It 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 δ^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 uses the RDD methodology with

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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. In addition, the results can be presented more compactly. In robustness tests, however, we also report results for individual cutoffs to establish that our more compact analysis is reasonable.

We report results for various bandwidths around the normalized cutoff and polynomials of the control function. More specifically, we use the following bandwidths: 0.5, 0.3, 0.2, 0.15, 0.1, and 0.05.[10] With respect to polynomials of normalized population size, we use up to a cubic specification. To control for common trends in the pre- and post-treatment period, we include legislative term fixed effects (rather than only dummies for the pre- and post-treatment periods). We also follow the previous literature and include in all regressions municipality fixed effects to improve efficiency and reduce finite sample bias (Hoxby, 2000).

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 (Ade and Freier (2011) show that co-treatment can be a major problem in standard RDDs with population thresholds). Instead, we only require that the effect of any co-treatments remains constant between the pre- and post-treatment periods. For example, one particular reason for potential co-treatment is that several of the cutoffs in Table 1 are relevant for equalization transfers (Baskaran, 2012). However, the stipulations surrounding the intergovernmental transfer scheme were not changed from the pre- to the post-treatment period, and thus the effect of different transfers receipts at the population thresholds on council size should remain constant in both periods.

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[10]We 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.

The other crucial assumption is that the ability or incentives of municipalities to manipulate population size at the cutoff did not change from the pre- to the 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 chose 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[11] reported in Figure 4 also fails to indicate that incentives for manipulation changed from the pre- to the post-treatment period at the normalized cutoff.

3.2 Diff-in-Disc plots

In addition to regression results, we also present graphical evidence on the treatment effect. We construct RDD plots for the pre and post-treatment periods and a Diff-in-Disc plot by first dividing the control function, $NLPOP$, into bins of size 0.02 within a window of 0.5. Then we calculate average council size 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 $t = 0, 1$ denoting the pre- and the post-treatment period.

For the RDD plots, we smooth the observations with a local polynomial plot of quadratic degree and a bandwidth of 0.5 at both sides of the normalized cutoff, using a rectangular kernel. For the Diff-in-Disc plot, we first obtain the difference within each bin in the pre- and post-treatment period $\Delta y_b = (\overline{y}_{b,1} - \overline{y}_{b,0})$, then plot the differences in y against $NLPOP$

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[11]Standard McCrary plots are inappropriate for our design as we are interested in the difference of the effect at the normalized population cutoff in the pre- and the post-treatment period. The idea underlying our Diff-in-Disc variant of the McCrary 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.

to the left- and the right of the normalized cutoff, and finally smooth the differences with a local polynomial plot of quadratic degree and a bandwidth of 0.5 at both sides of the normalized cutoff, using a rectangular kernel and the number of observations within each bin as frequency weights.

4 Baseline results

4.1 Graphical evidence

Figure 5 presents the graphical evidence. Subfigure (a) shows the effect of the normalized threshold on council size in the pre-treatment period. There is a large positive discontinuity. Municipalities to the left of the normalized threshold have councils that are about 10 seats larger than municipalities to the left. Subfigure (b) shows the effect of the normalized threshold in the post-treatment period. There is still a positive discontinuity, but it is markedly smaller. Average council size to the right of the threshold is only about 6 seats higher than to the right. Finally, subfigure (c) presents the Diff-in-Disc plot, which explicitly plots the change in the effect of the normalized threshold from the pre- to the post-treatment period. There is a negative discontinuity of about 4 seats. Hence, municipalities that were exposed to a stronger treatment reduced the number of seats more strongly.

4.2 Regression results

In Table 2 we collect the regression results. Consistent with the graphical evidence, the effect of abolishment is consistently negative and highly significant for almost all bandwidths and polynomials. Council size after the abolishment decreases in municipalities that were exposed to a stronger treatment by about 3 to 4 seats. The weighted average increase in

treatment strength, i. e. the decline in the implicit electoral threshold, at the normalized population threshold is around 0.46.[12] Scaled by the average increase in treatment intensity, these estimates imply that municipalities reduce the size of their council by about 7 to 8 seats after the abolishment of the five percent hurdle for every percent point lower implicit threshold. According to back of the envelope calculations, reducing the council size from e. g. 23 to 15 members increases implicit thresholds from about 2.2 to about 3.3 percentage points[13], i. e. by about 50%. This increase in implicit thresholds is presumably sufficiently large to keep several small parties out of the council that would otherwise have been entitled to a seat.

5 Robustness tests and extensions

5.1 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 at the correct cutoff. If the coefficient estimates in these placebo tests are significant, it is possible that municipalities adjusted their council size for reasons other than that the abolishment of the electoral threshold. We define D_i in Equation 4 such that it is 1 if $NLPOP = -2, -1, 0, 1, 2$ ($NLPOP = 0$ indicates the true threshold). We estimate the regressions for all combinations of bandwidths and

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[12]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 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.

[13]Note that there is no exact formula for implicit thresholds. For the above calculations, we assume that parties are entitled to a seat if they have enough votes for "half a seat".

polynomials reported in the baseline estimates. To save space, we summarize the results in graphs.

The structure of the plot in Figure 6 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 and indicate their median value. We find that 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.

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 1993. If the treatment effect in the baseline regressions is not due to the reform but due to pre-existing trends, these placebo estimates should be significantly negative. We collect the results in a Figure 7. The median coefficient at the fake cutoffs is indicated in red. For comparison, we also indicate the median estimate at the true cutoff with a blue dot. The estimates at the fake treatment year are close to 0, in contrast to the median estimate at the true threshold. Hence, these placebo regressions, too, confirm the baseline results.

5.2   Event-study plot

Another way to evaluate whether the treatment effect found in the baseline regressions is due to pre-existing trends is to conduct an analysis in the spirit of an event-study. Consequently, we run traditional RDD regressions for each election year separately and estimate how the normalized cutoff is related to council size in each year. Given our baseline results, we expect the RDD coefficients for the 2001 election to be substantially smaller than the RDD coefficients for the 1997 election. If there were pre-existing trends, however, we should also observe declining RDD coefficients from 1989 to 1993 and from 1993 to 1997. A persistent decline in the RDD coefficients up to the 2001 election would

suggest that at least part of the estimated treatment effect for the reform in 2001 is due pre-existing trends.

More formally, we estimate the following parametric RDD separately for $t = 1989, 1993, 1997, 2001, 2006, 2011$:

$$y_{i,t}^M = \gamma_t^M D_i + f(NLPOP) + D_i f(NLPOP) + \epsilon_{i,t} \text{ if } |NLPOP| < h, \qquad (5)$$

where all relevant variables are defined as in Equation 4.

We estimate the model for the bandwidths and polynomials used for the baseline estimates. To save space, we collect the results in a graph. Figure 8 reports for each election year the 18 coefficients estimates for the various bandwidths and polynomials. The size of circles indicate the standard errors. We indicate the median coefficient estimate with a red dot. We observe a small decline in the median RDD estimate from 1989 to 1993. A large number of estimated coefficients for 1989 are also quite large, but they are estimated imprecisely. The small decline from 1989 to 1993 in the RDD coefficients is presumably a statistical artifact due to a changing composition of treatment and control groups between the two elections. Given the substantial change in population size during this time period in Hesse (see Figure 2), some municipalities that were located below the normalized threshold in 1989 probably transitioned above the threshold in 1993, which in turn might affect the RDD coefficient estimates. Other explanations, especially deliberate responses of the established parties to some political events, are less likely because the descriptive evidence in Figure 3 indicates that almost all municipalities chose the highest possible council sizes both in 1989 and in 1993. That local politicians deliberately reduced council sizes would be inconsistent with this descriptive evidence.

We find no decline in the RDD coefficient from 1993 to 1997. In 2001, there is, as expected, a substantial drop in the coefficient estimates. This drop persists to the 2006

election. By 2011, however, the RDD coefficient is as large as in 1993 and 1997. In our companion paper, we show that even though selected smaller parties benefited from the electoral reform – i. e. they saw a significant increase in their vote and seat shares – overall council fragmentation did not increase significantly (Baskaran and Lopes da Fonseca, 2014). It is hence possible that the established parties, having observed for two consecutive elections that consequences of the electoral reform were manageable, decided to expand the council size again before the 2011 election.[14]

Overall, there was a significant decline in the effect of the normalized discontinuity on council size just around the "event" of the reform in 2001. Furthermore, in the election immediately before and after the 2001 election, we observe no significant decline in the RDD coefficients. These results hence indicate that the treatment effect found in the baseline regressions was not due to pre-existing trends.

5.3   Individual cutoffs

Are the baseline estimates driven by only selected cutoffs? Does the effect of the various cutoffs on council size differ? To answer these questions, we report results for the four smallest individual cutoffs (sample sizes are too small for the larger cutoffs). For compactness and since sample sizes are smaller, we only report results for specifications with a relatively large bandwidth of 0.5.

The results are collected in Table 3. We find for all cutoffs except the first that the coefficient estimates are negative and significant. That we observe no effects at the lowest cutoff is plausible if politicians in smaller municipalities have little leeway or interest to reduce the council size. In particular, local politicians in small municipalities might feel that they may harm their own chances of winning a seat too much if they reduce the size

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[14]A much more thorough description of the electoral consequences of the reform can be found in Baskaran and Lopes da Fonseca (2014).

of the council further. At the 5001 and 10001 cutoffs, we observe significant but fairly moderate reductions: council size declines by 2 to 4 points. At the 25001 inhabitants threshold, we observe somewhat stronger effects. Council size decreases by about 4 to 6 seats.

5.4 Percent changes

One further concern with the baseline estimates is that municipalities to the right of the normalized cutoff might appear to cut more seats in absolute terms than those to the left even if the abolishment of the electoral threshold was irrelevant when there are common trends that lead to the same relative reductions at the left and the right of the cutoffs. That is, the relative decline in council size to the left and the right of the normalized cutoff might be the same even if the decline in absolute terms is larger. To account for this concern, we estimate the baseline models with the log of council size as dependent variable, which allows for a percentage interpretation. The results are collected in Table 4. They show that the percentage decline at the normalized cutoff, too, is significantly negative. More specifically, council sizes declined by about 10 to 15 percent more to the right of the normalized threshold than to the left after the abolishment of the five percent threshold.

5.5 Effect heterogeneity

An interesting question is whether the treatment effects varies according to the composition of the council. Do councils where the two large mainstream parties (the CDU and SPD) together have a majority, either absolute or two-thirds, decrease council sizes more? Does the difference in the seat share between the two largest parties – whichever they may be – in the council matter? To explore these questions, we interact the treatment dummy

in Equation 4 with various variables that measure the composition of the current council. The basic model we estimate is as follows:

$$
\begin{aligned}
y_{i,t}^M =&f(NLPOP) + \zeta V_{i,t} + D_i(\gamma_{t \geq T}^M + f(NLPOP)) + I_t(\alpha + f(NLPOP)) \\
&+ D_i(I_t(\delta^M + \kappa V_{i,t}) + I_t f(NLPOP)) + \epsilon_{i,t} \text{ if } |NLPOP| < h,
\end{aligned}
\tag{6}
$$

where $V_{i,t}$ indicates various measures for the composition of the council before the election held in t. All other variables are defined as in Equation 4. If the effect varies with the the composition of the council, the estimate for κ should be significant.

As proxies for $V_{i,t}$, we have experimented with various variables, including a dummy for whether the CDU and SPD have an absolute majority, a dummy for whether these two parties together have a two-third majority, and their seat shares. We have also experimented with variables that account for the difference in the seat share of the two largest parties in the council, whichever they may be, and variables that capture whether the largest party has an absolute majority. We found a significant interaction effect only for one specification: when we used a dummy for whether the CDU and SPD together have an absolute majority. Table 5 reports the estimates for interaction effects for this specification. The results suggest that the decline in council size is more pronounced when the two large mainstream parties have an absolute majority.[15]

It is curious that we find significant interaction effects for joint CDU and SPD absolute majorities but insignificant ones for two-third majorities, even though a two-third majority is required to change council size. If there are differences in the size of the treatment effect, we should expect them to be particularly pronounced at the two-third majority threshold. It is possible, however, that an absolute majority is already sufficient for the large parities to push through a change in council size. An absolute majority gives the large parties a

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[15]Results for the other interaction models are omitted, but they are available from the authors.

veto over council decisions. It is hence possible that the remaining parties are willing to trade their support for a reduction in council size against concessions in other areas.

6 Validity of the identification strategy

6.1 Direct election of the mayor

As mentioned in Section 2.1, another important reform occurred at the local level in the early nineties: the introduction of direct elections for the mayoral office. Before 1993, mayors in all municipalities were appointed by the council. After 1993, municipalities switched to direct elections of the mayor. The transition to the new system for mayoral elections was staggered, with some municipalities switching immediately while others retaining their appointed mayors for a few years. It is possible that the switch may have affected municipalities differently along the population thresholds for council size. Hence, it may not be possible to separately identify the effect of the abolishment of the electoral threshold from the effect of the electoral rule for the mayor.

However, there is no good reason why the switch in how to choose the mayor should affect council size, and even less so why any effect should vary along the population thresholds. Second, the mayoral office continued to be relatively unimportant even after the switch to direct elections, and thus any effects of the switch on council size or other political variables should be limited. Finally, all municipalities had switched to direct elections of the mayor by 1998. It is therefore unlikely that the effects of the switch to direct elections, even if they differed between municipalities at the population thresholds, persisted to the period after the implementation of the *Kommunalwahlreform*.

6.2 Other elements of the *Kommunalwahlreform*

6.2.1 Lengthening of the legislative period

We interpret the negative treatment effect as evidence that the mainstream parties reduced council size to offset a perceived increase in the competitiveness of smaller parties. There might be other explanations. One possible alternative explanation is that voters demanded a reduction in council size following the reform because of the lengthening of the legislative period from four to five years. That is, the lengthening of the representatives' terms of office may have led to demands for fewer representatives. Politicians, in turn, may have found it easier to reduce council size if the councils were larger to begin with.

While we cannot explicitly test whether the reductions in council sizes are ultimately due to an attempt by the large parties to limit electoral competition or due to voter demands since we do not observe voters' preferences regarding their preferred council size nor the politicians' true motive to reduce the number of seats in the council, it is more plausible that council size was reduced to limit political competition. It is unlikely that the lengthening of the legislative period, a minor aspect of the reform, would have led to such vociferous calls from voters for council size reductions that it would force representatives to implement these reductions, presumably against their will. Second, a two-third majority in the council is necessary for a change in council size. It seems unlikely that even if the voters demanded a reduction in council size, these demands were so strong to enforce coalitions across party lines. Third, we observe that council size already declines in 2001, i. e. the first election after the reform. Given that the reform law passed the state parliament in 1999, there were at most two years in which council sizes could be changed. Even if voters demanded smaller councils, it is unlikely their demands would be sufficiently pronounced and well articulated to effect concrete actions by the representatives within this relatively short time period.

6.2.2 *Kumulieren and Panaschieren*

The second important element of the *Kommunalwahlreform* was to allow voters to cast as many votes as seats are available in the council. This element may affect municipalities differently at the population thresholds. The more seats a council has, the more votes can be cast by each voter. However, while more votes increases the flexibility of voters to either cumulate or split their votes across different parties and candidates, this increased flexibility at the population thresholds should not have as decisive an effect on the competitiveness of smaller parties as the reduction in implicit thresholds.

In addition, note that we are not interested in the causal effect of abolishing an electoral threshold per se. We are ultimately interested in how the established parties responded to an increase in the competitiveness of the smaller parties. Whether the competitiveness of the smaller parties increases because of the abolishment of the electoral threshold, or because of a larger number of available votes, or both is immaterial for our main conclusions. While co-treatment because of an increase in the number of votes would not allow us to give the estimates a causal quantitative interpretation about how implicit thresholds affect council size, the results show credibly that an increase in the political competitiveness of smaller parties causes mainstream parties to reduce the number of seats in the council.

7 Conclusion

We ask in this paper whether politicians from established parties adopt measures to disadvantage smaller parties once they become a viable political alternative. To study this question, we exploit a reform in the German federal state of Hesse that improved the competitiveness of smaller parties for exogenous reasons, the abolishment of an electoral threshold. As the impact of the reform varied discontinuously between municipalities with different population sizes, we can implement a Difference-in-Discontinuity design to identify

how established politicians responded to the abolishment. One straightforward measure that established politicians can adopt is to decrease the size of the council. In smaller councils, implicit thresholds are lower, and hence smaller parties will find it more difficult to gain their first seat. The results suggest that municipalities which had on average smaller implicit thresholds because they were to the right of a certain population cutoff indeed reduced their council size more.

The substantial effects on council size we find are particularly remarkable because local politicians typically prefer to have large councils as evidenced by the fact that in the pretreatment period, they consistently chose the largest possible council size. But it appears that preventing new formations from entering the council is even more important to them than maximizing their chances of gaining a council seat.

From a policy perspective, our findings imply that the chances of new political formations to graduate into the political mainstream are possibly even lower than commonly believed. Established parties appear to erect additional barriers once new formations improve their electoral prospects. Given the potentially positive implications of political competition, endogenous adjustments of political institutions to disadvantage new formations might be perceived as problematic.[16] On the other hand, of course, it is also possible to make the case that erecting barriers to entry into the mainstream increases the stability of the political system. The measures taken by the established parties in Hesse in the aftermath of the abolishment of the electoral threshold and, more generally, attempts of established parties to limit the electoral prospects of new formations, should ultimately be viewed through the lens of how political competition affects economic and social outcomes. Theoretically, the effect seems ambiguous (Lizzeri and Persico, 2005). To further explore this question is an interesting and important avenue for future research.

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[16]However, we document in a companion paper that the reduction in council sizes did not harm the smaller parties too much. Municipalities where implicit thresholds were lower saw a significant increase in the vote and seat shares of smaller parties.

Acknowledgments

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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 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. The "number of observations" denotes the the total number of observation in a population bracket in the whole sample, which includes six elections between 1989-2011, not the number of municipalities. For example, the six observations in the "500001–1000000" bracket are all from the same municipality.

Table 2: THE ABOLISHMENT OF THE ELECTION THRESHOLD AND THE
NUMBER OF COUNCIL SEATS.

	BW=0.5	BW=0.3	BW=0.2	BW=0.15	BW=0.1	BW=0.05
Linear	-1.823***	-2.229***	-2.607***	-3.227***	-3.937***	-4.309***
	(0.298)	(0.352)	(0.453)	(0.472)	(0.592)	(0.847)
Quadratic	-2.241***	-2.787***	-3.848***	-4.185***	-4.266***	-3.531***
	(0.388)	(0.543)	(0.634)	(0.710)	(0.873)	(1.239)
Cubic	-2.636***	-3.799***	-4.526***	-4.552***	-4.450***	-3.020*
	(0.554)	(0.689)	(0.823)	(0.931)	(1.150)	(1.656)
N	3496	2121	1416	1079	723	369

Notes: This table presents difference-in-discontinuity regressions for the size of the council (number of seats). 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 (0.5, 0.3, 0.2, 0.15, 0.1, 0.05) 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 3: EFFECT OF ABOLISHMENT AT INDIVIDUAL CUTOFFS.

	T=3001	T=5001	T=10001	T=25001
Linear	-0.968***	-2.318***	-1.893***	-2.492***
	(0.316)	(0.532)	(0.505)	(0.856)
Quadratic	-0.116	-3.419***	-2.663***	-4.479***
	(0.487)	(0.765)	(0.657)	(1.121)
Cubic	0.001	-4.472***	-2.724***	-6.179***
	(0.608)	(0.979)	(0.843)	(1.268)

Notes: This table presents difference-in-discontinuity regressions at the following individual population cutoffs: 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 linear to quartic polynomials 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 4: THE ABOLISHMENT OF THE ELECTION THRESHOLD AND THE NUMBER OF COUNCIL SEATS, LOG SPECIFICATION.

	BW=0.5	BW=0.3	BW=0.2	BW=0.15	BW=0.1	BW=0.05
Linear	-0.059***	-0.069***	-0.083***	-0.100***	-0.124***	-0.141***
	(0.012)	(0.014)	(0.017)	(0.018)	(0.022)	(0.031)
Quadratic	-0.068***	-0.085***	-0.119***	-0.138***	-0.146***	-0.105**
	(0.015)	(0.020)	(0.024)	(0.027)	(0.034)	(0.051)
Cubic	-0.080***	-0.120***	-0.153***	-0.160***	-0.148***	-0.079
	(0.021)	(0.026)	(0.031)	(0.036)	(0.046)	(0.065)
N	3496	2121	1416	1079	723	369

Notes: This table presents difference-in-discontinuity regressions for the log of the size of the council (log number of seats). 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 (0.5, 0.3, 0.2, 0.15, 0.1, 0.05) 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 HET-
EROGENOUS EFFECTS IN MUNICIPALITIES WITH JOINT CDU AND SPD
ABSOLUTE MAJORITES.

	BW=0.5	BW=0.3	BW=0.2	BW=0.15	BW=0.1	BW=0.05
Linear	-0.128	-0.889**	-1.532***	-1.720***	-1.768***	-2.175**
	(0.269)	(0.407)	(0.364)	(0.366)	(0.456)	(1.090)
Quadratic	-0.128	-0.814**	-1.393***	-1.581***	-1.772***	-2.234**
	(0.266)	(0.405)	(0.330)	(0.361)	(0.446)	(1.123)
Cubic	-0.071	-0.745*	-1.369***	-1.597***	-1.765***	-2.487**
	(0.266)	(0.384)	(0.324)	(0.353)	(0.453)	(1.207)
N	3496	2121	1416	1079	723	369

Notes: This table presents estimates from difference-in-discontinuity regressions for an interac-
tion variable between the dummy indicating the normalized threshold and a dummy indicating
whether the CDU and SPD have jointly an absolute majority (i. e. whether CDU seat share
+ SPD seat share $\geq$ 50%) in the council. All population cutoffs at which council size is al-
lowed to change are analyzed simultaneously by normalizing population size. Estimates for the
interaction effect are reported for different bandwidths (0.5, 0.3, 0.2, 0.15, 0.1, 0.05) and in-
creasingly 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%(***).
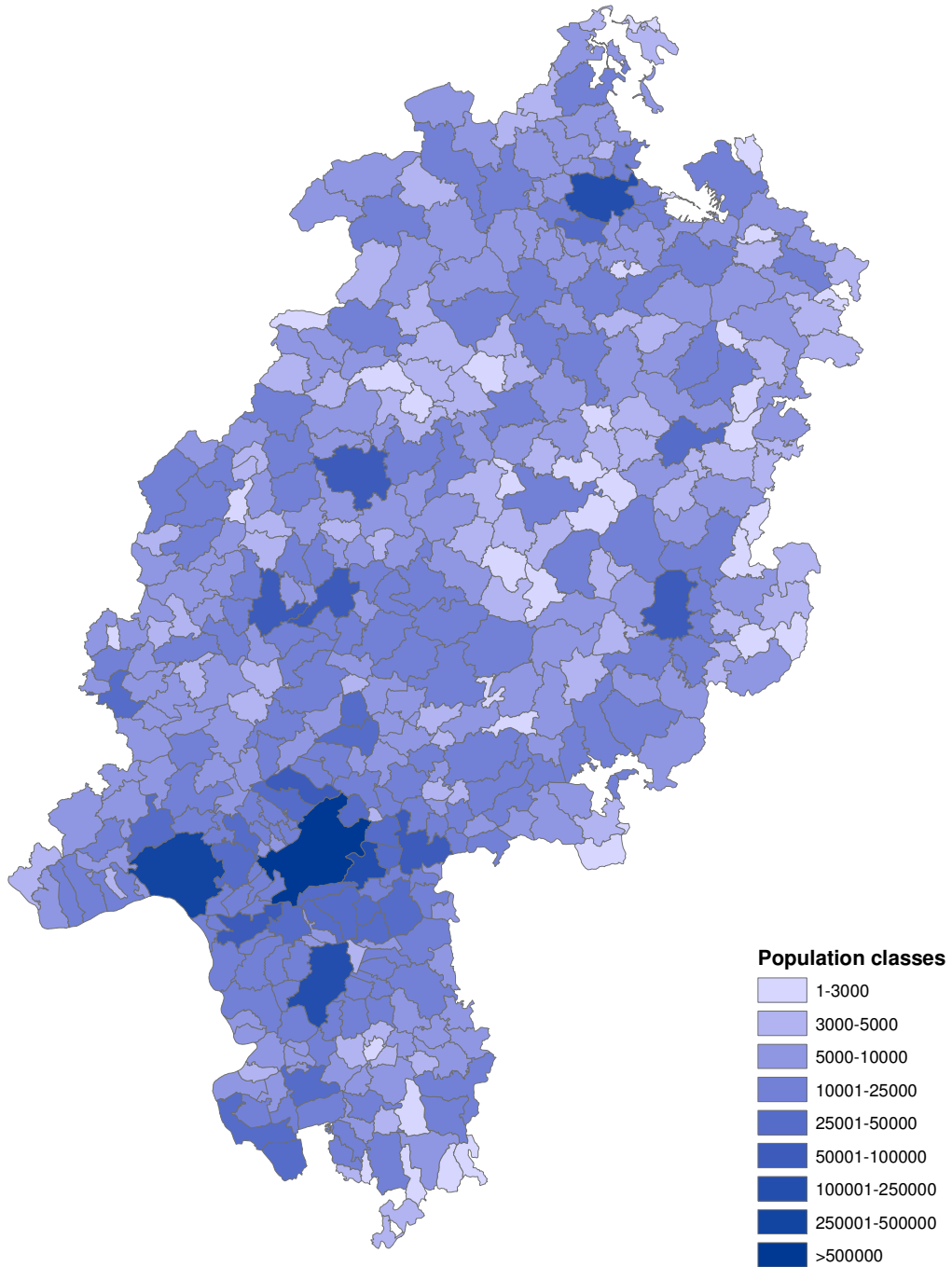
Figure 1: Average population sizes in Hessian municipalities during the sample period.
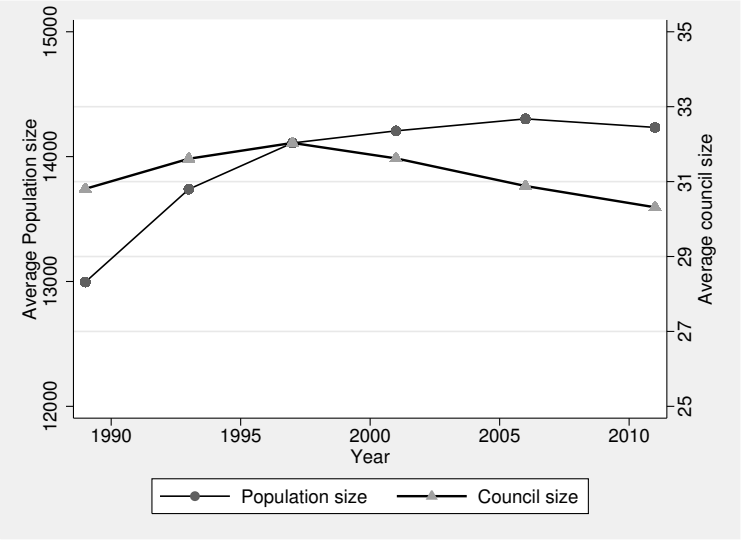
Figure 2: Average council and population size in Hessian municipalities during the sample period.
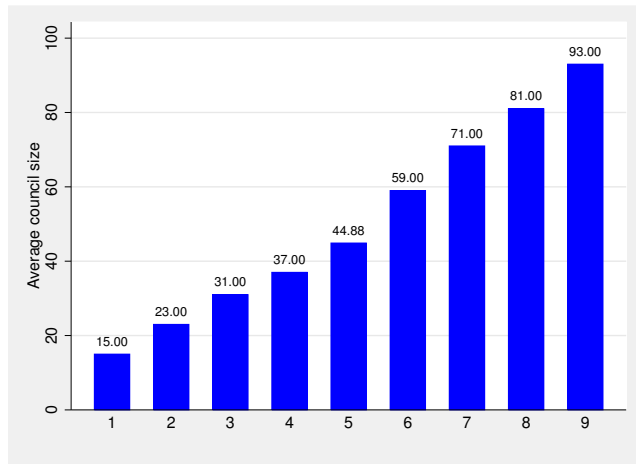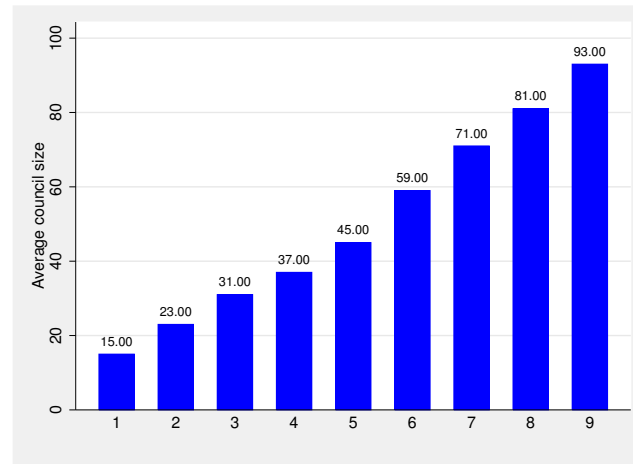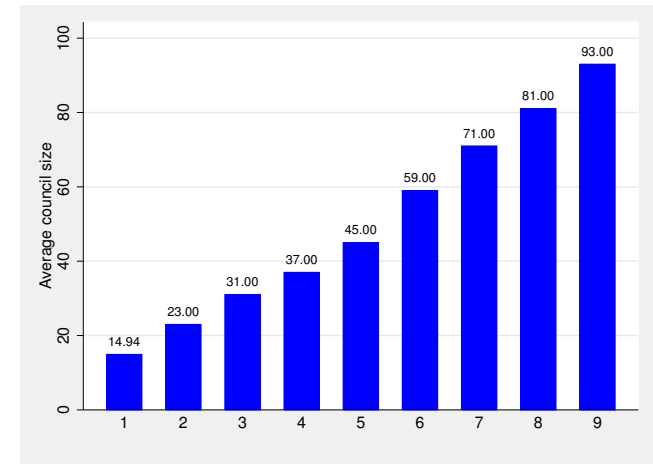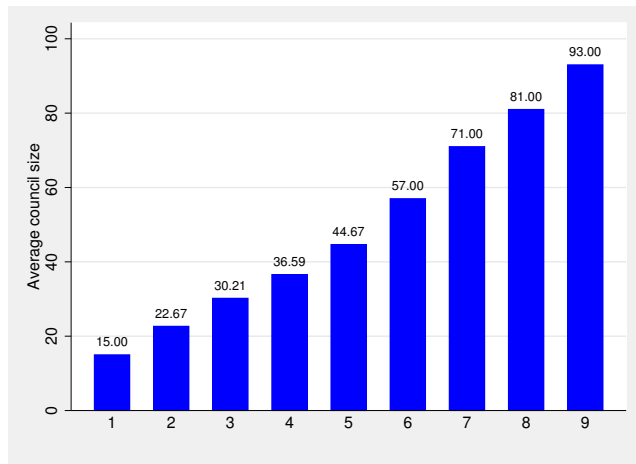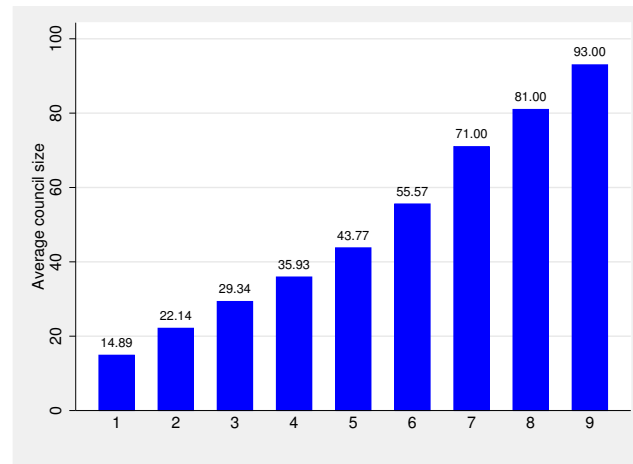
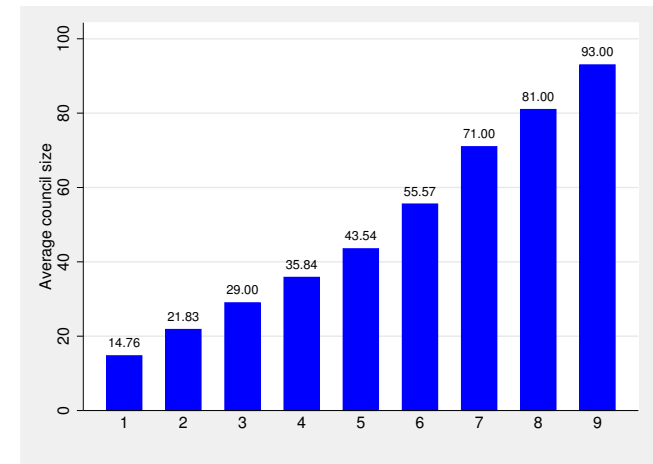Figure 3: Average council size in different population brackets at the beginning of each legislative period between 1989-2011. This figure shows the average council size of municipalities in population brackets 1-3000 (1), 3001-5000 (2), 5001-10000 (3), 10001-25000 (4), 25001-50000 (5), 50001-100000 (6), 100001-250000 (7), 250001-500000 (8), 500001-1000000 (9) after each legislative period.
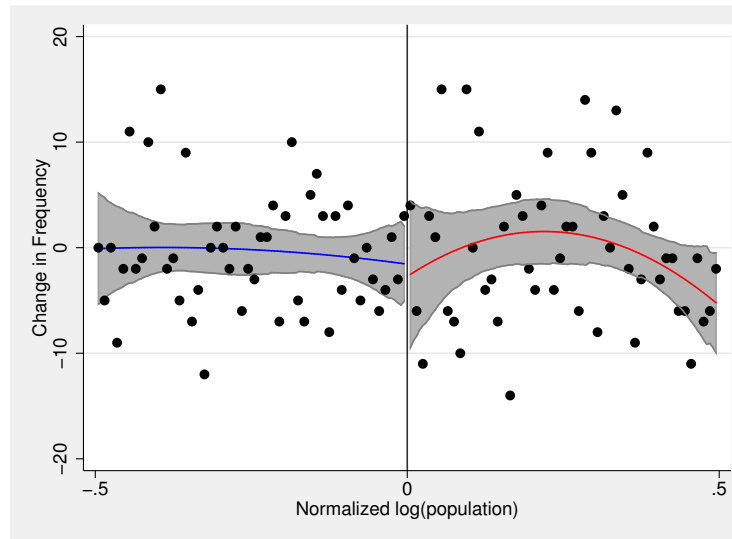
Figure 4: Density plots for change in normalized log population size. This figure presents a density plot for the Diff-in-Disc design in the spirit of McCrary plots (McCrary, 2008). We first divide normalized log population size in bins of width 0.01. Then we calculate the change in the total number 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..
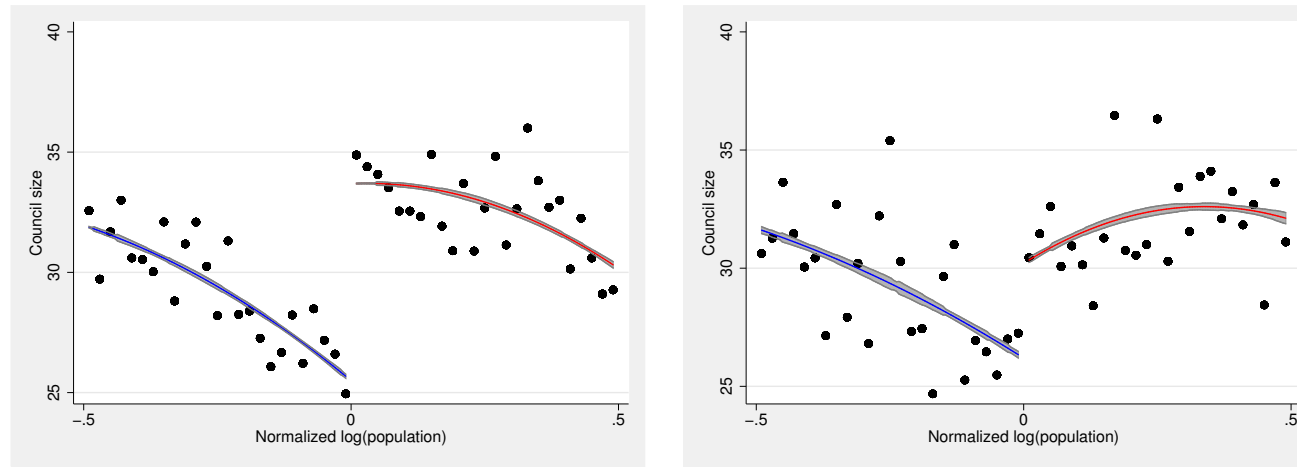
(a) RDD plot for pre-treatment

(b) RDD plot for post-treatment
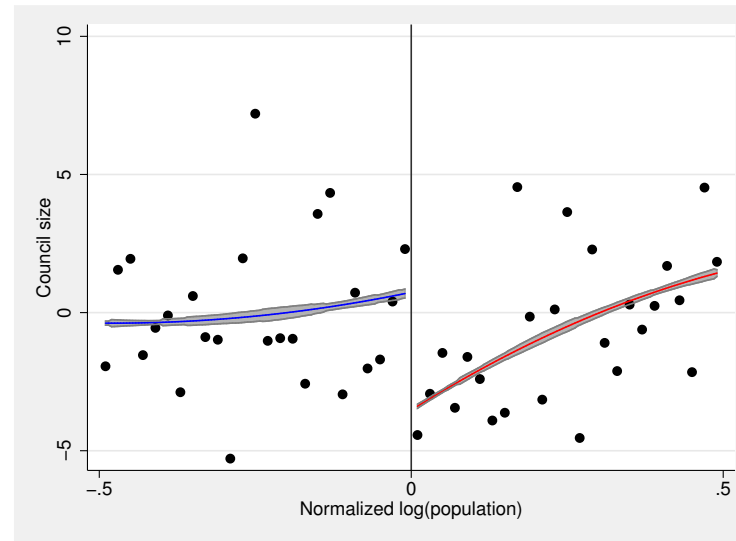
(c) Diff-in-Disc plot

Figure 5: Abolishment of electoral thresholds and council size. This figure shows standard RD plots for the pre- and post-treatment periods (subfigure a and b) and a Diff-in-Disc plot (subfigure c) for council size. 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. 95% confidence intervals are indicated in gray.
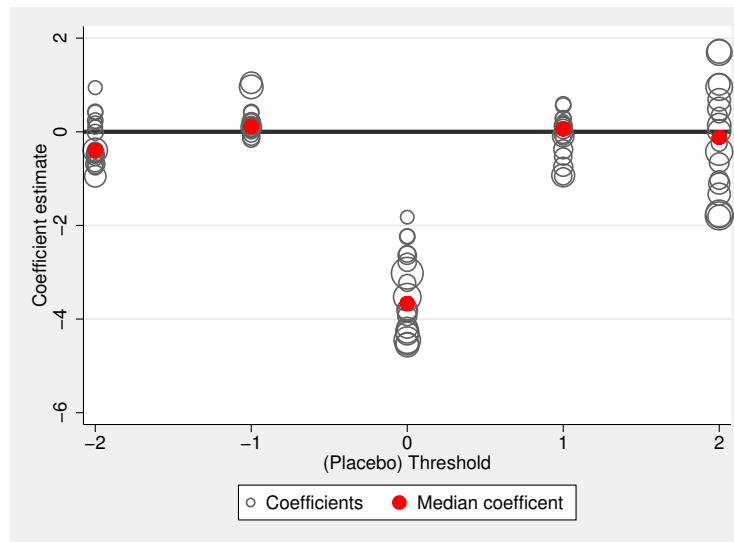
Figure 6: **Placebo treatments with fake thresholds.** This figure shows coefficient estimates of the Diff-in-Disc model for council size with placebo treatments. The size of the dots indicates the standard error of each estimate. The median estimates are indicated with red dots. The thresholds are redefined 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.
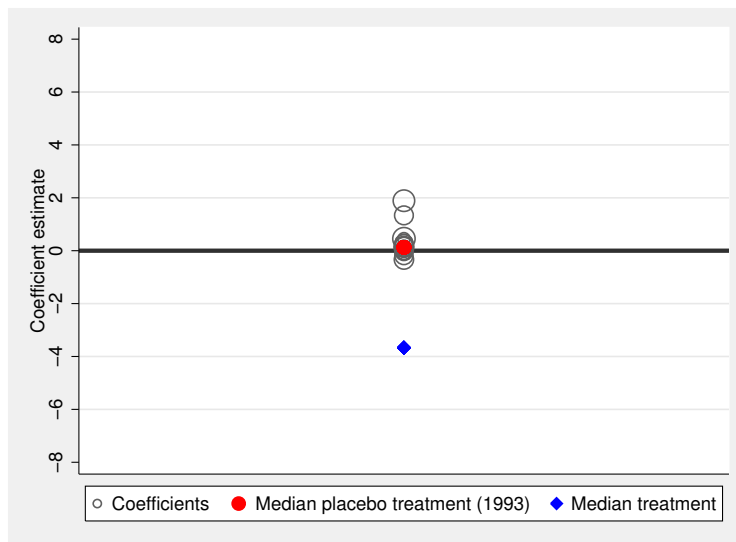
Figure 7: Placebo treatment for placebo year. This figure shows coefficient estimates of the Diff-in-Disc model with a placebo treatment defined to set in 1993. The sample covers the period 1989-1997. The size of the dots indicates the standard error of each estimate. The median estimate at the fake treatment year is indicated with a red dot. The median estimate at the true treatment year is indicated with a blue dot.
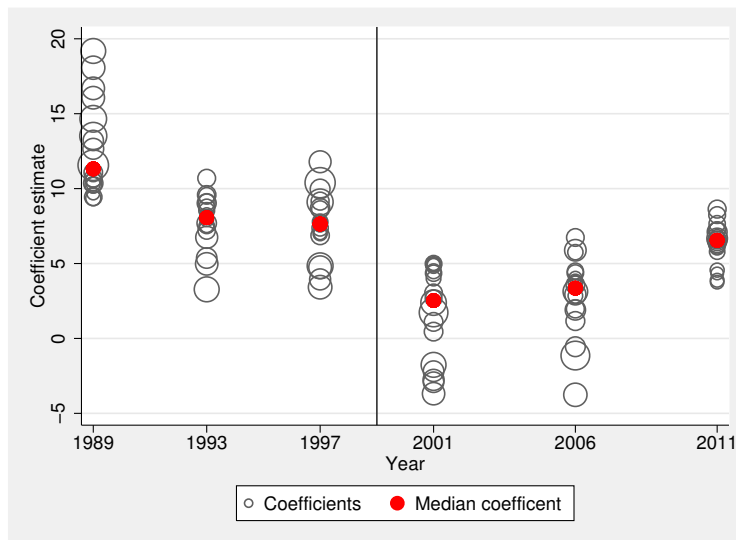
Figure 8: Event study plot. This figure shows coefficient estimates of traditional RDD regressions for all elections from 1989 to 2011. The size of the dots indicates the standard error of each estimate. The median estimate for each election is indicated with a red dot.