The National Effects of Subnational Representation: Access to Regional Parliaments and National Electoral Performance*

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Abstract

According to scholarly wisdom, party competition at the subnational level plays a negligible role in national elections. We provide theory and evidence that qualifies this view. Subnational elections determine entrance into subnational parliaments, which provides essential organizational resources: members and money. Since in most cases the same political actors compete at all levels of government, they can make use of these resources to improve their status in national party competition. We test our argument exploiting two institutional features of the German multi-level electoral context: the discontinuities generated by the five percent electoral threshold in German state elections, and the occurance of German state elections at different times in the federal election cycle. We find that parties that marginally cross the threshold for state parliamentary representation gain more members, and eventually perform better in national elections, but only if the party has sufficient time to organize between the state and the federal election. Consistent with our organizational explanation, bottom-up effects are more pronounced where state parliamentary parties receive more financial resources. Alternative mechanisms are tested, and receive no empirical support.

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*We would like to thank Alex Coppock, Andy Eggers, Bob Erikson, Edward Fieldhouse, Shigeo Hirano, Camille Strauss-Kahn, Catherine de Vries, two anonymous reviewers and the QJPS editors for their thoughtful comments and suggestions. As a result this paper has been much improved. Moreover we are grateful to Diana Galos and Giordano Neuenschwander for excellent research assistance, and to Heike Merten for background information relating to the development of party financing in Germany.

Over the last decades many countries have delegated previously centrally coordinated competencies to their subnational authorities (Benz 2009; Deschouwer 2003; Rodden 2004). This decentralization process has been accompanied by the formation and strengthening of subnational representative institutions, as well as the growing number and importance of subnational elections fostering party competition at the subnational level (Deschouwer 2003; Detterbeck 2012; Schakel 2013). An important question that arises from this development is whether representation in this lower tier of politics has had any noteworthy effect on national electoral results.

The bulk of the existing literature suggests that this should not be the case. Most studies point to top-down electoral dynamics, whereby subnational politics are mainly reflections of the national electoral momentum and the national election cycle (Campbell 1991; Dinkel 1977; Gabriel 1989; Hainmueller and Kern 2005; Jeffrey and Hough 2002; Jérôme and Lewis-Beck 1999; Lohmann, Brady and Rivers 1997). Subnational elections are characterized as second-order elections (Marsh 1998; Reif and Schmitt 1980, 9), used by voters as mid-term elections, either to punish incumbent parties (McLean, Heath and Taylor 1996; Pallarés and Keating 2003; Tronconi and Roux 2009) or to accommodate their policy demands through electoral balancing (Erikson and Mikhail 2001; Lutz Kern and Hainmueller 2006; Rodden and Wibbels 2010).

The same conclusion is also reached by a different strand of research, pointing to the nationalization of party organization and election campaigns. According to the cartel party thesis (Katz and Mair 1995; 1997; 2009), party competition becomes dominated by the national level at the expense of the local party organization. For the mass party, members were deemed vital for parties' survival. In an electoral context, however, that promotes centrally-orchestrated campaigns in which the party's message is quickly and effectively disseminated to the electorate through the use of the mass media, the question whether a party engages the party membership is said to be less vital to party competition (King 2002; Mughan 1995).

Drawing on Panebianco's theory of party institutionalization (1988), we develop a theoretical account that challenges this view. Although our departure point builds on

the "cartel party thesis" (Katz and Mair 1995), we depart from Katz and Mair (1995) to accommodate multi-level systems of government. The mechanism driving the cartel party thesis is that parties that are part of the political system —so-called "insiders"— gain an electoral edge over their competitors by taking advantage of state resources (Katz and Mair 1994; 1995; 1997). Resources are deemed vital for institutionalization, the process by which political parties ensure their long-term survival (Panebianco 1988). We extend this logic of institutionalization into multi-level government. Outcomes of subnational elections influence national electoral dynamics because they determine whether parties enhance their organizational capacity by gaining access to financial and human resources at lower levels of government. Since the same political parties tend to compete at all levels of government, they can use the money and members gained from entering subnational parliament to improve their standing in a national election.

A empirical test of this idea involves a comparison between parliamentary "insiders" and "outsiders," i.e. parties that do not usually enter parliament. The problem, however, is that these two groups of parties differ on many accounts apart from whether they have access into the resources provided by subnational representative institutions. Since entrance into parliament is not randomly assigned, it is difficult to disentangle the effect of subnational parliamentary representation on national electoral performance from all other potentially confounding differences between the two groups.

We address this problem by carefully selecting our case of study. In particular, we focus on the German federal system. In German state elections ("Landtagswahlen") entrance into the state parliament is determined by the presence of an electoral threshold. Parties need to receive at least 5% of the vote in order to gain access to the state legislature. Parties that receive less than 5% of the vote stay out. We thus construct our comparison groups by using the discontinuities generated by this five percent threshold that applies to all state elections in Germany.[1]

[1] The same threshold applies in federal elections ("Bundestagswahlen"), but we do not use it here, because parties' federal level vote share is only used as the main outcome

Even if this design permits the identification of the effect of parliamentary representation on national electoral performance, it still leaves open one important alternative explanation. Entering state parliament might help parties' future electoral performance not through the provision of material resources but simply because it operates as a signal of electoral viability. In so doing, it may yield spillover effects. Electoral success in one arena might lead to more success in another arena because voters want to be on the winning side or because there is a feeling of ''momentum" (Bartels 1985; Goidel and Shields 1994; Simon 1954).

The German electoral setting is particularly helpful in also addressing this competing explanation. German state elections occur at different times during the federal election cycle. While the spillover mechanism should be more pronounced if elections follow in quick succession, the organizational benefits stemming from entering subnational parliaments should not materialize immediately. Indeed, organizational effects stemming from institutionalization should be the larger, the more time parties have to put these resources to use. The two mechanisms can thus be observationally distinguished by considering the variation in the time elapsed between subnational and national elections. We unpack the mechanism further by examining two observational implications of our organizational theory. First, we show that the electoral effects of entering state parliament are the larger, the larger the available state funding for parliamentary parties in a given state. Second, we further trace the organizational roots of these effects by testing whether subnational representation affects the number of party members in a given state, which we argue is a good measure of organizational capability.

In what follows we first develop the organizational hypothesis and derive expectations about the effect of subnational parliamentary representation on national electoral performance. We then describe the research design and present the results. We discuss the implications from our findings in the concluding section.

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of interest.

The Organizational Returns to Subnational Parliamentary Representation: Money and Members

In a recent study, Bechtel (2012) argues that there are important bottom-up effects from subnational politics to national politics that have previously been ignored. As he acknowledges, "we know virtually nothing about whether and how subnational elections influence vote intentions at the national-level" (Bechtel 2012, 3). We posit that one of the ways in which subnational elections affect national electoral outcomes is by determining which parties gain access to subnational representative institutions. The logic is exemplified in Panebianco's following remark about the way in which different electoral contests are interlinked (Panebianco 1988, 9):

> *Party arenas are interdependent and can be conceived as a network of "relevant" environments. Resources obtained in one arena can be spent in another, and success at one gambling table —the exchange of resources under favorable conditions— often affects the extent of one's success at other tables.*

Multi-level systems of government provide multiple points of access to two types of organizational resources [2] that are key to a party's electoral success: money and party members. In decentralized systems, a larger share of these resources is available at the subnational level of government, and varies conditional on entrance into state parliament: state parliaments provide parties with money to pay for the professionalization of their staff and political activities, which in turn increases the attractiveness of the party to potential activists and supporters. Money and members enable parties to

[2] The assumption that holding elected office comes with resources that are electorally meaningful is mirrored in the literature on US Congressional elections, which shows that direct office holder benefits contribute to explaining the incumbency advantage (Levitt and Wolfram 1997; Serra 1994).

better weather political eventualities and national electoral dynamics. Let us explore these two types of benefits in more detail.

The first organizational resource available to state parliamentary parties is money. State parliaments provide generous budgets to parliamentary parties for their organization and day-to-day business. Parties use this money to pay their party leadership, state representatives, parliamentary researchers, special advisors, and constituency case workers (Katz and Mair 2009). Germany represents a typical case of this practice. Data that we collected from the federal budget and the 16 state budgets show that in 2010 parliamentary parties received a total of around 186 million euros for the upkeep of their parliamentary work, of which around 108 million euros were paid to state parliamentary parties (see Table A.7 in the Appendix for a break-down by state). This compares to a total of 132 million euros at all levels of government that was available through the direct public party financing mechanism, which does not discriminate between parliamentary insiders and outsiders (Bundestag 2011). It is no wonder then that Katz and Mair (Katz and Mair 2009) increasingly see the roots of cartelization in the parliamentary party. Funding provided to parliamentary parties in the German system constitutes a form of indirect, opaque party financing, which is arguably more important than official public subsidies. There is much evidence that parties use funds available to parliamentary parties to support the work of the party on the ground (Pulzer 2001; Scarrow 2006*b*). As Pulzer (2001, 31) writes, "while direct use of their funds for party purposes is illegal, the line between legislative work, academic research, and campaigning is difficult to draw". Moreover, parliamentary parties use part of these funds to raise the salaries of some parliamentarians, chiefly benefitting their leadership, and most oblige office holders to contribute parts of their salary to party work (Scarrow 2006*b*). State funds are also used to equip parliamentary and constituency offices, from where state representatives provide a range of services to constituents. Importantly, all these financial resources stay in the state and support the state party's political operation. If a party loses parliamentary representation in a given state, the money stops flowing and all employees of the state parliamentary

party will be made redundant, meaning the party will not only lose all its members of parliament, but also twice as many professional staff, which vastly outnumber staff in central party offices.

Second, entrance into legislatures improves party organization by increasing incentives for state party membership and grassroots activism (Panebianco 1988). There are at least three types of motivations for joining a party in the parliament. First, winning or losing a seat affects personal efficacy, the prospects of a political career. Whiteley and Seyd (1998), and Fisher and colleagues (2006) find that personal efficacy is a key element explaining why winning a constituency election contest boosts party membership and activism in UK parliamentary seats. Political careers normally begin at the subnational level, especially in the context of multilevel government (Katz and Mair 2009). Second, presence in parliament increases group efficacy, the prospects of influencing policy (Fisher, Denver and Hands 2006). Seyd and Whiteley (1992) and Whiteley, Seyd and Richardson (1994) provide survey evidence that many activists join a party because they feel they can affect policy —a perception that is greatly enhanced once a party enters parliament. If subnational levels of government provide avenues for affecting policy, the same mechanism should hold in multi-level systems. Third, it is also possible that parties reward their members through patronage appointments in ministries, or the public sector more widely (John and Poguntke 2012; Poguntke 1994; Smith 1979). Although parties present in government probably have a larger influence over patronage appointments, in a consensus democracy such as Germany, at least some appointments are thought to be the outcome of negotiations between government and opposition (Smith 1979).[3]

[3] To be sure, partisan appointments might not always reflect clientelistic practices, but the attempt to control decision making. Indicatively, Kopecký, Mair and Spirova (2012) present evidence that patronage within the civil service in advanced democracies is increasingly used to promote civil servants with the right party affiliation to exercise control over policy making, and less to reward faithful party members with jobs in the regional administration. Irrespective of the exact motives driving such appointments, the fact that they are decided on partisan grounds is likely to increase supply of membership.

Increased grassroots membership benefits parties in a multitude of ways. Although in the short run new parties might be able to attract activists without institutionalized structures of support on grounds of shared ideology and collective interests alone, in the long run institutionalization and the ability to affect policy and provide a political career are crucial to the survival and success of the party (Panebianco 1988, 166). In multilevel systems, state party members who joined as a result of a successful subnational election will of course be available to campaign for the party in higher-order, national election contests. Party members and supporters have a positive impact on voter mobilization and persuasion (Cox 2010; Seyd and Whiteley 1992; Whiteley, Seyd and Richardson 1994). They are ready to distribute leaflets, put out electoral advertising and knock on doors. Moreover, party members will spread the party's message in their households, neighborhoods and work places. As Aldrich (1995) has shown at the example of the United States at the beginning of the 19th century, state party organization is key to electoral success at the national level. By entering state parliament, a political party passes an important "organizational threshold" (Panebianco 1988, 193) that helps foster further institutionalization.

A key characteristic of organizational resources is that positive electoral externalities are not felt immediately, but need some time to materialize. Electoral support is expected to rise as the number of voters who benefit from services provided to constituents increases over time. Improvements in the quality of candidates recruited by the state party will need some time to capture media attention. An extended membership base is more likely to affect election results after the party organizes its grassroots support and new members gain campaign experience. Dissemination of party messages also needs time to reach a wider spectrum of constituents. Thus, there are often important long-term effects to improved party organization (Fisher, Denver and Hands 2006).

It is important to note that our line of thinking is driven by general and widely applicable benefits accompanying entrance into subnational legislatures. Our argument does not depend on the direct public subsidies provided to political parties in some

countries. In other words, the benefits discussed here are not contingent upon direct public party financing. They exist even in the absence of direct subsidies to political parties because parliamentary parties have found ways to allocate funds to themselves that are not as easily scrutinized under party financing rules. Although the empirical evidence about the electoral impact of public party financing rules is still ambiguous,[4] the simultaneous presence of public party financing might conflate the observed effects attributed to parliamentary representation. As is explained below, the choice of Germany addresses this concern as official public party financing rules in Germany do not discriminate between parliamentary insiders and outsiders.

An Alternative Explanation: Spillover Effects

Focusing on the importance of regional elections, Bechtel (2012) alludes to their role as information cues transmitted by the national media about state party performance, candidates and electoral success. The bandwagon effects literature on US Presidential primaries goes one step further suggesting that electoral success or failure in one state's primary election produces spillover effects if there is another primary following in quick succession (Bartels 1985; Simon 1954). It has been argued that electoral success leads to more success because people want to be on the winning side (Goidel and Shields 1994). This mechanism has also been associated with increasing media reports and the way individuals recall the information, on which they base their voting decisions (Iyengar 1990; Mutz 1995). Individuals are more likely to recall information to which they have been exposed more recently (Zaller 1992). Extending this line of argument, one could argue that with the exception of major parties, entering into the state parliament might signal success with possibly important side-effects for parties' future electoral

[4] While Katz and Mair (1994) originally located the roots of cartelization in the introduction of direct public financing, in recent years they have raised doubts about its impact on cartelization (2009; see also Pierre, Svasand and Widfeldt (2000); Scarrow (2006*a*)).

performance.[5]

Despite leading to equifinal expectations, the spillover mechanism differs from the organizational hypothesis in one important respect: time. While electoral bandwagon effects can explain electoral momentum from one election to the next if the two elections follow in quick succession, they do not account for the financial and organizational advantages provided to parties that perform well in state elections and manage to enter parliament. The media hype often only lasts a couple of weeks and an electoral bounce after a successful performance is likely to fade away.[6] But substantial benefits of representation, be it service provision in the constituency or organizational and institutional privileges, are there to stay. More importantly, in contrast to spillover effects, they are more likely to yield fruits only after some period has passed since the party entered parliament. In what follows, we provide several tests to assess each of the two mechanisms.

Data and Research Design

To test the effect of state parliamentary representation on federal party success in Germany, we created our own data set, which includes the electoral results of all parties that participated in German state and federal elections from 1946 up to 2013. The data set is based on the official state and federal election results published by the federal election commission (Bundeswahlleiter 2015*a*,*b*). For the period from 1946 up to 1990 it includes the complete results for the 10 states that made up the former

[5] An important critical view to the electoral importance of the spillover mechanism is Mutz (1995). While Mutz agrees that increased media attention leads to an incentive to consider the respective party, she cautions that this need not necessarily lead to a voting decision in favor of the party. People "reassess their own views in light of this new information. From this perspective, momentum appears far less pernicious than typical references to bandwagon phenomena would suggest" (Mutz 1995, 120).

[6] A nice illustration of how spillover effects operate is provided by the German Pirate Party, which after entering the state parliament of Berlin in late 2011, attracted much media attention, skyrocketed in national polls and entered 2 further state parliaments in quick succession scoring 7.4 and 8.4 percent of the vote. However, one year and a half after the party's initial success, it fell back to around 3 percent in national polls and ended up with a poor 2.2% in the 2013 federal election.

Federal Republic before reunification. For the period between 1990 and 2013 the data set includes all electoral results for the 10 old states and the 6 new states (including the city state of Berlin).

Federal elections are held every 4 years and state elections every 4 to 5 years. Each of the old states hence contributes 15-20 state election results and 18 federal election results, while the new states contribute data on 5-6 state elections and 7 federal elections. The resulting dataset gives us 67 unique small parties that have received between 0% and 10% of the vote in German state elections and participated in subsequent federal elections. Moreover, it includes 30 distinct parties that achieved between 1% and 9% of the vote and 10 parties that are located in the immediate neighborhood (4.2% to 5.6%) of the 5% threshold. The four parties that contribute most observations are the FDP, the Greens, the NPD, and the Republikaner (see Table A.10 in the Appendix for a complete list of all parties included in the dataset, and for the list of parties included at different bandwidths). Figure A.9 in the Appendix shows the distribution of cases over time from 1946 to 2013. It is clear that most cases are from the 1990s, 2000s and 2010s when Germany transformed from a two-and-a-half party system into a fully-fledged multi-party system.

Since parliamentary insiders differ from outsiders in various unsobservable ways, we make use of the discontinuities generated by the electoral rules used in German state elections. German parties can only enter the state parliament if they have gathered more than five percent of the total state vote. We use the presence of this electoral threshold, which applies to all states for the whole period under investigation, as a way to identify the effect of state parliamentary representation on federal electoral performance.[7] In particular, we focus on parties with vote shares in the neighborhood

[7] There are two exceptions: Following a constitutional court ruling in 1955, the threshold does not apply to the party of the Danish ethnic minority SSW, which is always guaranteed representation in Schleswig-Holstein. After 1955 the SSW is therefore excluded. Moreover, an exceptional rule has been applied in the state parliament of Bremen, where a party that fails to pass the threshold state-wide, but passes the 5% threshold in the city of Bremerhaven, gets 1 representative. This was the case for the DVU in the 1999 and the 2003 state elections and for the FDP in the 2003

around the electoral threshold, comparing parties above this threshold (treated) with parties below the threshold and hence without state representatives (controls).[8]

As in all regression discontinuity (RD) designs, our identification strategy is based on the assumption that agents—in our case German political parties— cannot precisely manipulate the assignment variable—their vote share in the state election—so that it lie just below or above the cutoff value—the 5% threshold. Under this assumption, potential outcomes (parties vote share with and without prior partliamentary presence) can be credibly assumed to be independent from treatment assignment (state parlaimentary representation) (Lee and Lemieux 2009).[9] For instance, the FDP in 1980 received 4.98 percent of the vote in the state election of Niedersachsen and was thus absent from the state parliament. Imagine a counterfactual in which that party had achieved only 0.02 percent more so as to enter state parliament. Would the party have gained a higher share of the vote in that state in the following federal election of 1983? Fortunately even such close cases are not very rare in German state elections (see Table A.10 in the Appendix for a list of parties' state election results within the narrow 0.7% window around the representation threshold).

Importantly, focusing on the discontinuities generated by the 5% threshold applied in German state elections allows us to estimate the effect of state parliamentary repre-

state election. All analyses shown in the following sections include all observations, irrespective of whether their parliamentary status complies with the five-percent rule or not. Since we do not draw any distinction between crossing the threshold and gaining a seat in the parliament, our estimation recovers the intent-to-treat effects of state parliamentary representation.

[8] Although the findings cannot be extrapolated to major parties or to very small parties, this is not particularly problematic here since for these parties parliamentary representation is almost invariably held constant. Major parties are always guaranteed entrance to the state parliament and very small parties are always left out. Therefore, it cannot be parliamentary representation that accounts for spatial and over-time variation in their electoral performance in federal elections.

[9] This assumption does not mean that there no other determinants of parties' state vote share in the federal election. Rather, it means that the association of all these factors with the outcome is smooth, so there is no other covariate that would cause a discontinuous jump in the conditional distribution of the outcome at that same point.

sentation net of the effect of direct public subsidies. According to the party financing regulations in Germany, parties receive public subsidies once they obtain 1% of the state vote. The amount of these subsidies grows in a linear fashion, i.e. in proportion to party's vote share. Thus, as a result of the direct public party financing mechanism there should be no gap between parliamentary insiders and outsiders.

We estimate the effects with local linear regression, which has been shown to have attractive bias properties in estimating regression functions at the boundary (Fan and Gijbels 1992) and enjoys rate optimality (Porter 2003). Following standard convention (Imbens and Lemieux 2008; Lee and Lemieux 2009), instead of implementing two seperate regressions below and above the threshold, we run a pooled regression on both sides of the cut-off point:

$$Y_{i,s,t+1} = \alpha_l + \tau D_{i,s,t} + \beta_l(X_{i,s,t} - c) + (\beta_r - \beta_l)D_{i,s,t}(X_{i,s,t} - c) + \epsilon_{i,s,t+1},$$

where $Y_{i,s,t+1}$ represents party i's vote share in the federal election $t+1$ at state s, X_{is} denotes party i's vote share in the state election s at t, c denotes the 5% threshold and $c - h \leq X \leq c + h$. D is a dummy that switches on for the parties that acquire representation in the state parliament. Using a triangular kernel, the estimation amounts to a weighted linear regression within a bin h, with higher weights being applied onto observations closer to the threshold. We allow the slope coefficients of each side to differ, thus we denote them here by β_l for the left side of the cuttof point and β_r for the right side respectively. α_l denotes the regression intercept at the left side of the cutoff point, whereas α_r denotes the intercept at the right of the cutoff point, as evaluated at $X = c$. Hence, $\tau = \alpha_r - \alpha_l$, i.e. the difference between the two intercepts as (minimally) extrapolated at the point of the discontinuity. All our main analyses cluster the errors at the state-election level. The results are also robust to various alternative error structures, as discussed in the following sections.

The key question is the choice of the bandwidth, h. We start by using Imbens and Kalyanaraman's (2011) optimal bandwidth algorithm (IK), which performs well in recovering experimental (Green et al. 2009), observational and simulation (Imbens

and Kalyanaraman 2011) benchmarks. However, since this estimator is data driven, it sometimes exceeds the four percent window below the threshold. This means that parties below 1% might be included in some of the analyses. Given that public subsidies start with 1% percent of the vote, there is a possibility for a within-group jump. We address this problem in four ways. First, we examine the sensitivity of the estimates across different bandwidths. Second, we follow Cattaneo, Frandsen and Titiunik (2013) in providing local randomization-inference based estimates, focusing only on the very few observations exactly in the neighborhood of the 5% threshold and well above the 1% threshold. Details regarding the window selection process and the balance tests accompanying our treatment effect estimates are provided in the Appendix.[10] Third, we explicitly test for within-group jumps. Fourth, we also present estimates using a fixed 4% bandwidth ($h = 4$), which results in a comparison of insiders and outsiders, all of whom receive state subsidies made available through the official state financing system of political parties.[11] We also conduct the standard diagnostic checks on the validity of the RD design: a test for sorting in the forcing variable and several balance tests, which we report in a separate section.

As a final point, we need to clarify how we try to disentangle our organization-driven effects from the possibly confounding spill-over effects. We make use of a particular feature of the German federal political system: the occurrence of state elections at different times during the federal election cycle.[12] The dates of the state elections originally differed because the states that later joined the Federal Republic of Germany

[10] Focusing only on these observations makes it also easier to satisfy the exchangeability criterion embedded in the understanding of the RD design as a local randomized experiment (Imbens and Lemieux 2008; Lee and Lemieux 2009).

[11] As a further robustness check, we replicate our main analyses excluding elections between 1959 and 1969, since during this period—the first period in which state subsidies were introduced—access to this source of financing was partially contingent upon parliamentary representation (Duebber 1962, 80). The results, reported in the Appendix (Figure A.2), are very similar to our main results discussed here.

[12] It is relatively uncommon for German state prime ministers to call snap elections. With a few exceptions, elections happen as scheduled at the natural end of each state parliament.

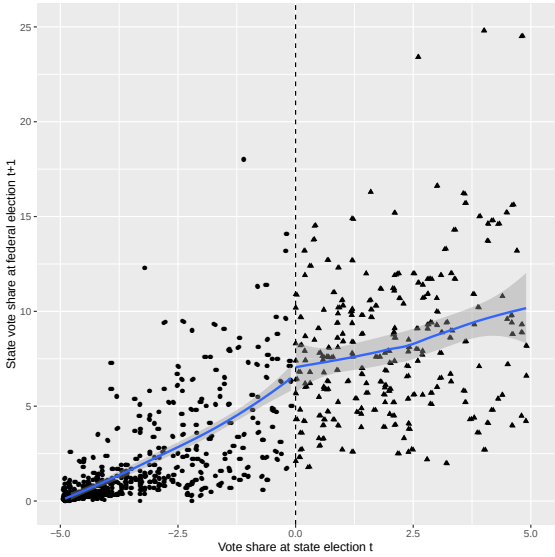
were constituted at different dates prior to the founding of the Federal Republic in 1949, and were part of four different occupation zones. All states occupied by the United States held early elections in 1946, while the majority of British and French occupied states followed only in 1947. Moreover, snap federal elections were called in 1972, 1983 and 2005, which rearranged the order of state elections in the federal election cycle. We assume that spillover effects are more likely to emerge when the period between state and federal elections is small than when the two elections fall far apart. The institutionalization effects hypothesized here need time to materialize, as parties need to organize on the ground. Thus, using the distance between state election and the next federal election, allows an informal assessment of the potential mechanisms driving the effects of parliamentary representation.[13]

Results

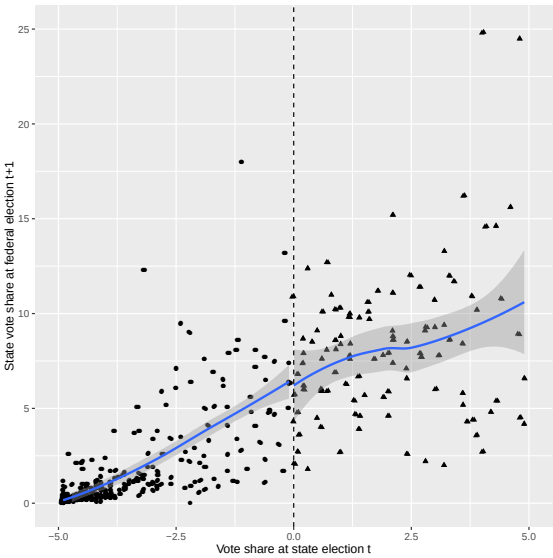
Figure 1 presents three scatterplots. In each of them, the x-axis represents parties' vote share in the state election, centered at the 5% threshold. The y-axis presents the vote share of the same parties in the subsequent federal election in the same state. Parties not having crossed the electoral threshold in the state election appear with circles and parties that have appear with triangles. The local linear regression curve denotes the conditional expectation of parties' vote share in the federal election in the state in question, given their vote share in the prior state election.The vertical line represents the cut-off point of parliamentary representation.

Figure 1a presents all observations within the chosen margin. As expected, a monotone ascending pattern is observed, with high vote shares in the state election predicting also higher vote shares in the coming federal election. The key question is whether there is a jump in parties' federal electoral performance as a result of crossing the threshold of state parliamentary representation. No such jump is observed. Instead, parties' federal vote shares appear to increase relatively smoothly as parties move from just

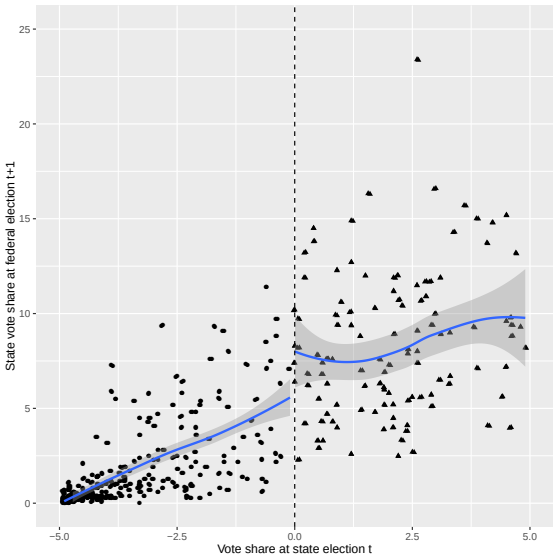
[13] We do not imply that these two mechanisms are exhaustive. We discuss various alternative mechanisms and delve into the organization mechanism in more detail in the sections following the presentation of the main results.



(a) Full Sample



(b) Below Median



(c) Above Median

Figure 1: The effect of crossing the 5% threshold in a state election on a party's subsequent federal election vote share in the same state.

Note: Dots denote parties without state parliamentary representation and pluses denote parties with parliamentary representation. The local regression (black solid) curves trace the mean vote share of $Y_{i,s,t+1}$ for parties below and above the threshold and the dotted curves denote the 95% pointwise confidence intervals.

below to just above the threshold.

Figures 1b and 1c qualify this view, taking into consideration the time frame between the state and the federal election. We split groups with respect to the median number of days intervening between the state and the federal election, i.e. 851 days.

The first group (below the median) represents observations in which state and federal elections took place in close proximity to each other (Figure 1b). The second (above the median) represents the group of observations with considerable distance between the two elections (Figure 1c). Finding a gap in the first case but not in the second would provide evidence for the presence of spillover effects but no support to our organizational hypothesis. Reversely, finding a greater gap in the second than the first case would lend support to the organizational hypothesis. This is what we find here: parties that make it to the state parliament at election t enjoy higher vote shares in that state in the next national election, $t + 1$. However, this effect is confined to those cases, where a relatively long period has passed between the state and the federal election.

We now proceed to a more systematic examination of state parliamentaty effects. Table 1 presents the results from the local linear regression estimation. The first column of the Table uses all observations from our dataset. The last two columns classify observations into the two groups: below and above the median distance between state and federal elections. Thus, whereas the second column tests the spill-over hypothesis, the third column tests our organizational hypothesis. The first part of the table uses the IK bandwidth, whereas the second and the third parts of the table employ half and twice that bandwidth respectively. The fourth section employs a common fixed bandwidth ($h = 4$) to facilitate comparisons between the three columns. The fifth part of the table presents treatment effect estimates using randomization inference, based on observations directly surrounding the 5% threshold.[14]

All parts of the table point to the same conclusion. Looking at the impact of state parliamentary representation when the upcoming federal election approaches, we find

[14] We follow the standard randomization inference procedure to calculate p-values, and confidence intervals outlined at length in Ho and Imai (2006) and in Gerber and Green (2012). The choice of the window in which our local "randomization-type condition" holds is data-driven, based on sequential testing of nested windows of different sizes starting from the smallest and ending at the largest in which the sharp null hypothesis cannot be rejected for any window contained in it (Cattaneo, Frandsen and Titiunik 2013). A detailed explanation of randomization inference, as well as the specifications and results of the window selection procedure are shown in the Appendix in Table A.1).

Table 1: The impact of state-level representation on federal vote share in the same state.

	Full Sample	Below Median	Above Median
		IK Bandwidth	
Treatment Effect	0.649 (0.635)	0.116 (0.769)	1.823 (0.803)**
CIs	[-0.596—1.894]	[-1.391—1.623]	[0.249—3.397]
Bandwidth (h)	4.61	4.74	5.48
n [treated]	213	102	117
n [control]	435	237	328
		Half-IK Bandwidth	
Treatment Effect	0.143 (0.900)	-0.450 (1.077)	1.977 (1.116)*
CIs	[-1.621—1.907]	[-2.561—1.661]	[-0.210—4.164]
Bandwidth (h)	2.31	2.37	2.74
n [treated]	131	64	81
n [control]	123	60	74
		Double-IK Bandwidth	
Treatment Effect	0.861 (0.542)	0.323 (0.703)	1.833 (0.736)**
CIs	[-0.201—1.923]	[-1.055—1.701]	[0.390—3.276]
Bandwidth (h)	9.22	9.48	10.96
n [treated]	226	109	117
n [control]	684	346	328
		Fixed Bandwidth ($h = 4$)	
Treatment Effect	0.613 (0.689)	0.051 (0.837)	1.948 (0.974)**
CIs	[-0.737—1.963]	[-1.589—1.691]	[0.039—3.857]
Bandwidth (h)	4	4	4
n [treated]	194	94	100
n [control]	266	128	136
		Randomization Inference	
Treatment Effect	1.331*	-0.523	2.755***
CIs	[-0.159—2.825]	[-2.833—1.675]	[1.105—4.385]
Window	0.7%	0.6%	1.1%
n [treated]	45	18	34
n [control]	35	13	24
		Placebo Outcome, $V_{s,t-1}$: Above Median	
	IK bandwidth	Half-IK Bandwidth	Double-IK Bandwidth
Treatment Effect	0.884 (1.000)	0.126 (1.330)	1.063 (0.869)
CIs	[-1.069—2.837]	[-2.480—2.731]	[-0.641—2.766]
Bandwidth (h)	4.82	2.41	9.37
n [treated]	83	49	85
n [control]	203	49	268

Note: The entries denote the treatment effect (τ) of entering the state parliament on the party's vote share in the same state in the next federal election. *p<.10; **p<.05; ***p<.01, two-tailed tests.

no gap between outsiders and insiders. On the contrary, looking at those cases in which the federal election comes with considerable delay after the state election, we find a

jump in parties' vote shares, which seems to be around 1.8 percentage points.[15] The randomization inference-based comparison of means across a very small window around the threshold also generates a significant positive effect of parliamentary representation but only for those observations with a considerable time lag between the two elections. The last part of the Table presents a placebo test, using parties' vote share in the previous federal election as outcome. State parliamentary representation should not affect parties' vote share in the previous federal election. As expected, compared to the main treatment effects, the gap is of lower magnitude, non-significant and more sensitive to different bandwidths.[16] Taken as a whole the findings suggest that there is an upward gap associated with state parliamentary representation but only in those cases where the federal election takes place after some period has passed since the last state election. [17]

How sensitive are our findings to the bandwidth choice? Figure 2 illustrates the local average treatment effects across various bandwidths. The first panel includes all observations, whereas the second and third panels use only observations below and above the median distance respectively. The overall pattern reaffirms the results from Table 1. Although more narrow bandwidths around the threshold naturally increase the uncertainty surrounding the point estimates, there is a notable difference in the

[15] In Table A.2 of the Appendix, we replicate the analysis using state, year and party-fixed effects, as well as all their two-way combinations. The results remain robust to the inclusion of all these types of fixed effects.

[16] Since our main interest lies in the subset of observations with more days between state and federal election (column 3), we focus only on those observations when implementing the placebo test. The results are substantively identical when all observations are included in the analysis.

[17] In the Appendix (Table A.3), we extend this evidence by using the number of seats instead of the percentage of votes as the outcome variable. In particular, we use the number of seats (if any) gained by party i in state s at federal election $t+1$ as a result of having crossed the threshold of 5% in the same state in the last state election preceding the federal election. The results from this exercise are substantively identical to those presented here: parties that cross the threshold in the state elections translate their advantage in the next federal election not only into more votes in the same state but also into more seats. This effect is moderated by the distance between state and federal election (see Table A.3).

magnitude of the effects between the second and third panel. This difference remains robust and stable throughout the bandwidth range.[18]

As a next step, we engage in a more detailed exploration of the relationship between state parliamentary representation and the timing of the federal election. We present three sets of analysis. First, we try to assess the extent to which the difference between the two groups (below and above median distance) is a result of sampling variability. We address this question by applying a parametric model across the whole range of observations.[19] In particular, we employ the same equation as in the local linear model, augmenting it by adding a binary term that denotes observations above the median. This term is also interacted with all other terms of the model. To allow for non-linearities in the relationship between state and federal vote share, we further add up to three polynomials of the forcing variable. The results appear in Table 2. The parametric models provide a very similar picture. They suggest that the difference between the two sets of observations is substantive in terms of magnitude and, although imprecisely estimated, it seems unlikely to be an artefact of sampling variability.

Second, we use a different rule to distinguish observations according to the distance between state and federal election. In particular, instead of using the median distance, we simply use the midpoint of the possible time range between the two elections. The federal election can take place up to five years after the state election. We thus divide the two groups according to whether the federal election took place within two and a half years since the state election. We replicate the analysis using both the local linear regression estimator and the global polynomial estimator. The results appear in

[18] It also remains robust to different patterns of clustering, as shown in the Appendix (Figure A.1), where different error structures are used. We also replicate this exercise excluding the years 1959-1969, in which the threshold for participating in the official public financing system for political parties coincided with the representation threshold. The results, shown in the Appendix (Figure A.2), remain substantively intact.

[19] Even here, however, we retain symmetry by considering only observations with up to 10 percentage points in the state election. This helps to avoid extrapolations beyond the support of the data.

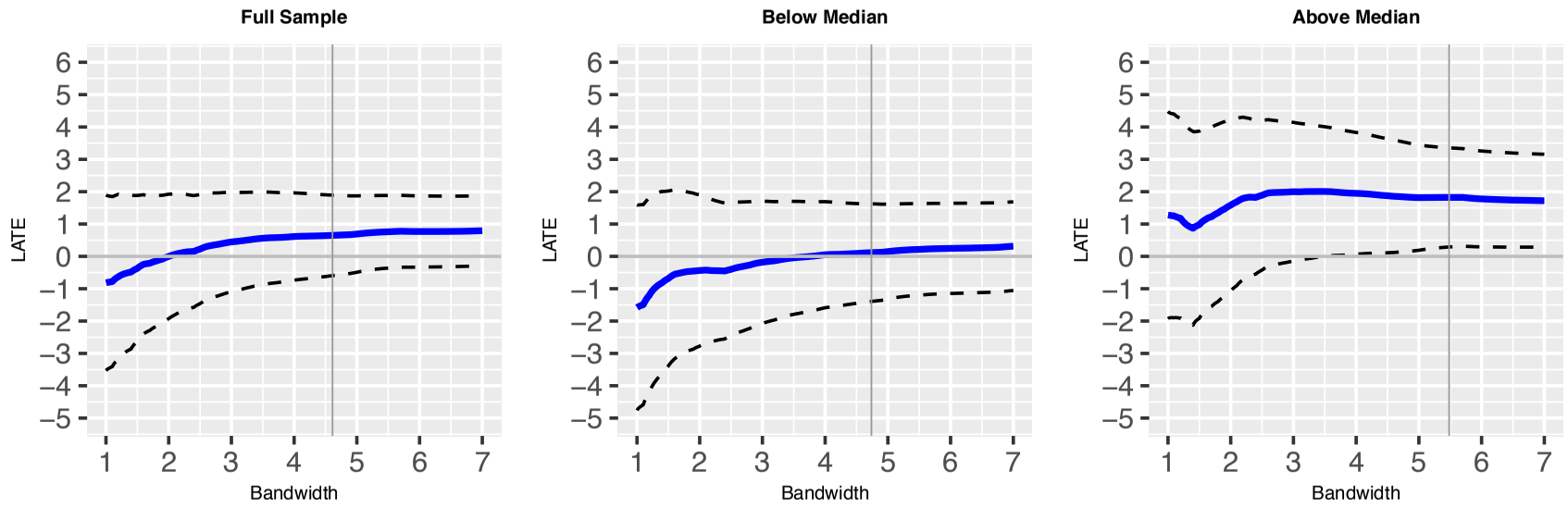


Figure 2: Sensitivity of the Local Linear Regression Estimates to Varying Bandwidth.

Note: The solid black line in each graph denotes the local average treatment effect across different bandwidths, as denoted in the horizontal axis. The dashed curves denote the 95% CIs and the vertical dashed line indicates the IK bandwidth. The first graph includes all observations. The second graph includes only those observations in which the distance between state and federal election is below the median (851 days). The third graph looks at observations in which the distance between state and federal election is above the median.

Table 2: Parametric Analysis of the impact of State Parliamentary Representation on Federal Vote Share.

	One Polynomial	Two Polynomials	Three Polynomials
$X_{i,s.t}$	1.332 (0.110)**	2.140 (0.498)**	2.113 (1.441)
$D_{i,s,t}$	0.112 (0.731)	-0.933 (1.042)	-1.214 (1.418)
Above Median	-1.045 (0.666)	-1.819 (1.142)	-1.519 (1.620)
$X_{i,s.t} \times D_{i,s,t}$	-0.549 (0.303)*	-1.051 (0.946)	-0.162 (2.535)
$X_{i,s.t} \times$ Above Median	-0.254 (0.149)*	-1.011 (0.717)	-0.471 (2.066)
$D_{i,s,t} \times$ Above Median	1.727 (1.006)*	2.715 (1.560)*	3.881 (2.062)*
$X_{i,s.t} \times D_{i,s,t} \times$ Above Median	-0.045 (0.389)	0.404 (1.352)	-4.359 (3.723)
$X^2_{i,s.t}$		0.138 (0.074)*	0.126 (0.538)
$X^2_{i,s.t} \times D_{i,s,t}$		-0.203 (0.196)	-0.658 (1.308)
$X^2_{i,s.t} \times$ Above Median		-0.129 (0.106)	0.097 (0.752)
$X^2_{i,s.t} \times D_{i,s,t} \times$ Above Median		0.195 (0.278)	2.232 (1.798)
$X^3_{i,s.t}$			-0.001 (0.059)
$X^3_{i,s.t} \times D_{i,s,t}$			0.066 (0.180)
$X^3_{i,s.t} \times$ Above Median			0.026 (0.081)
$X^3_{i,s.t} \times D_{i,s,t} \times$ Above Median			-0.336 (0.251)
Intercept	6.450 (0.491)**	7.331 (0.803)**	7.318 (1.082)**
n (clusters)	901 (191)	901 (191)	901 (191)
Average Treatment Effects			
τ: Below Median ($D_{i,s,t}$)	0.112 (0.731)	-0.933 (1.043)	-1.214 (1.418)
τ: Above Median ($D_{i,s,t} + D_{i,s,t} \times$ Above Median)	1.838 (0.718)*	1.783 (1.160)	2.667 (1.478)*

Note: The entries denote OLS estimates, with $X_{i,s,t}$ being the forcing variable (state vote share of party i, in state s and election t) and $D_{i,s,t}$ a binary indicator switching on for observations above the threshold. Errors are clustered at the state-election level. Analytical standard errors are shown in the last two rows of the table. *p<.10; **p<.05, two-tailed tests.

the Appendix (Table A.4 and Figure A.3) and they point to the same direction as the analysis using the median distance between state and federal election.

Third, we try being more agnostic about the exact functional form in the relationship between state parliamentary representation and distance between the two types of elections. Instead of imposing any structure into the data, we try to infer this relationship in a indirect fashion. We estimate the effect of crossing the state threshold on the federal vote share for each additional day passing since the state election. The results are shown in Figure 3. The x-axis depicts the minimum number of days between state and federal election. For instance, zero means that the federal election might have taken place at any point in time starting from the same date as the state

election; fifty means that the federal election has taken place at least 50 days after the state election; and so on up until 1460 days (end of fourth year) since the state election.[20] Each dot presents the local average effect of parliamentary representation, given the number of days that sets the minimum barrier of distance between the two elections. The higher the number the more days have passed since the previous state election, hence the smaller the pool of observations. In other words, each analysis is nested within the previous one. The black solid curve traces the local mean response in the resulting scatterplot. In so doing, it provides a summary of the magnitude of the treatment effects according the number of days that have passed since the state election. The overall pattern is clearly ascending and for most part monotone, denoting that the effect of parliamentary representation increases as the distance between the two elections also increases.

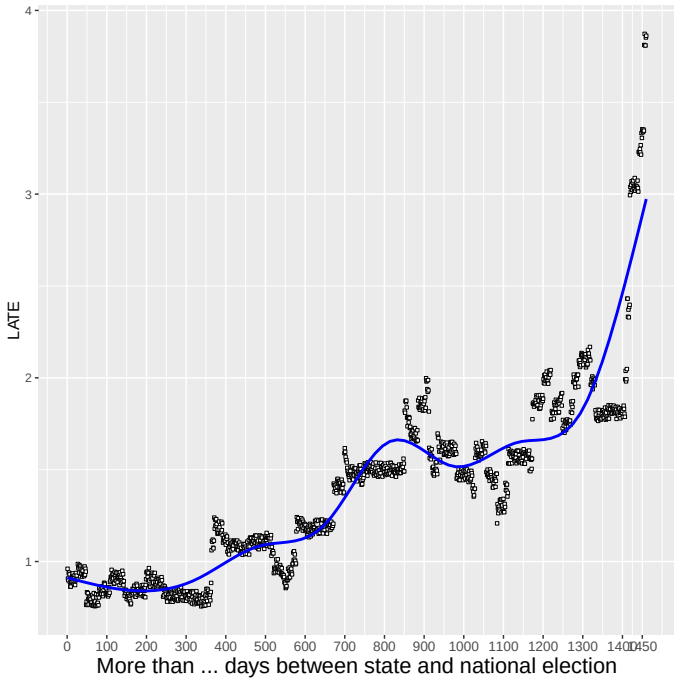


Figure 3: A closer look at the distance between state and federal election and the impact of state representation on federal vote share.

Note: Each dot represents a local average treatment effect, given that the federal election took place at least as many days as denoted in the x-axis since the state election. The local regression curve summarizes the overall pattern.

[20] All observations with state-federal election distance of more than four years are included within the same analysis.

Robustness Checks and Diagnostics

We conduct three tests to examine the sensitivity of the results. First, we need to check for sorting in the running variable. Identification in the regression discontinuity design is based on the assumption that agents have imprecise control over the assignment variable around the threshold value. This means that we assume that parties cannot fully manipulate their vote share so as to be just below or (more likely) above the threshold. Although in free and fair elections it is hard to see how this assumption can be violated, previous studies on incumbency advantage have suggested possible sorting mechanisms (Caughy and Sekhon 2011; Eggers et al. 2015).[21] We test for sorting in the forcing variable using the McCrary (2008) density test. The test refutes this possibility. The p-value for the null of no-sorting is 0.23.[22] Figure A.2 in the Appendix illustrates the absence of sorting and further discusses why sorting cannot be driving our results.[23]

Second, we check for covariate balance at the cutoff point. It might be that some parties, years or states are over- or under-represented on each side of the cut-off point, confounding our estimates. To see whether this is the case, we examine whether a significant gap at the cutoff point is observed in various seemingly irrelevant outcomes. As outcome variables we use a set of dummy variables —for each of the 16 states, Eastern region, FDP party, Green party, and extreme right party— as well as year, both as a linear and quadratic trend and as a pre/post-1989 binary outcome. The results are

[21] That said, it is clear that our design is not based on the marginality of the election. Any type of election could provide us with parties near the 5 percent threshold. Therefore, the criticisms related to studies using the margin of victory as the forcing variable do not apply in our design.

[22] The test has been implemented only for the group of observations above the median distance, since it is the results from this analysis we need to scrutinize.

[23] One reason that might have provided strong incentives for manipulation is the simultaneous access to public subventions. This was the case only between 1959 and 1969. Indeed, when excluding these observations, the p-value of no sorting becomes 0.49 (Figure A.4). As shown in the Appendix (Figure A.2), the results remain robust to the exclusion of these observations. More details about the sorting analyses can be found in the Appendix.

shown in Table 3. In total we implement 21 balance tests and only two of them prove marginally significant at the p<.1 level (Sachsen and Saarland).[24] Importantly, neither the FDP, nor the Green party dummies are statistically significant. This result helps us discount the possibility that the two larger parties, CDU and SPD, strategically "lend" votes to potential coalition partners in order to aid them in crossing the 5%-threshold. While this type of strategic voting might occur occasionally, there is no evidence that the FDP in particular sorts on the right side of the representation threshold. We return to this issue in a following section.

Third, we check whether there are within-group jumps. If the reason for the observed gap between insiders and outsiders is parliamentary representation, we should not find significant discontinuities in the conditional expecetation of the outcome variable among observations that do not differ in terms of their treatment value. To detect such discontinuities, we focus separately on parties below and on parties above the 5% electoral threshold. We then split each group into two subgroups, treating parties with higher vote shares as treated and parties with lower vote shares as controls. To maximize statistical power, the benchmark for this within-group division is the median (we also use the within-group mean, as shown in Table A.6). The results from this analysis appear in the Appendix (Table A.6). We find no instance of within-group jumps. When treatment status does not change, no upward gap is found as a result of a difference in parties' vote shares.

Unpacking the Organizational Mechanism

The results presented thus far indicate that representation in the state parliament offers a comparative advantage in the subsequent federal election. The fact that this effect needs time to materialize points to its organizational nature, but does direct evidence support that inference? In addressing that question we first consider the financial resources available to parliamentary parties in each state. Some German states

[24] In the Appendix (Figure A.5) we present the main results without these two states. The findings remain practically identical. For reasons of completeness, Table A.5 presents the balance tests for the full sample of observations.

Table 3: Balance Checks

	RD Estimates	95% Lower Bound	95% Upper Bound	IK Bandwidth
Extreme right	-0.071 (0.124)	-0.314	0.172	2.15
East Germany	-0.075 (0.133)	-0.336	0.187	2.35
Year	0.606 (2.913)	-5.103	6.315	6.47
Year^2	4415 (10796)	-16745	25575	10.35
Post-1989	0.047 (0.138)	-0.223	0.317	2.48
FDP	-0.105 (0.180)	-0.457	0.248	1.61
Greens	0.182 (0.148)	-0.108	0.472	1.83
Baden-Wuttemberg	0.015 (0.056)	-0.095	0.125	2.22
Bayern	-0.092 (0.072)	-0.233	0.049	1.96
Berlin	-0.022 (0.080)	-0.179	0.135	2.04
Brandenburg	0.035 (0.030)	-0.024	0.094	1.65
Bremen	0.024 (0.053)	-0.080	0.128	1.82
Hamburg	-0.110 (0.124)	-0.353	0.133	1.87
Hessen	-0.031 (0.059)	-0.147	0.084	1.95
Mecklenburg-Vorpommern	-0.067 (0.090)	-0.243	0.109	2.09
Niedersachsen	-0.014 (0.082)	-0.147	0.175	1.97
Nordrhein-Westfalen	0.067 (0.061)	-0.053	0.187	2.28
Rheinland-Pfalz	-0.018 (0.075)	-0.165	0.130	1.99
Saarland	0.135 (0.082)*	-0.026	0.296	2.02
Sachsen	0.093 (0.065)	-0.034	0.220	1.82
Sachsen-Anhalt	-0.054 (0.112)	-0.273	0.165	1.94
Schleswig-Holstein	0.080 (0.085)	-0.086	0.247	2.12
Thueringen	-0.083 (0.050)*	-0.181	0.015	1.87

Note: The entries in the first column denote the treatment effect (τ) of entering the state parliament on various placebo outcomes. Standard errors, clustered at the state-election level, in parentheses. *p<.10; **p<.05, two-tailed tests.

have been more generous when it comes to the appropriation of public money to parliamentary parties than other states. Does subnational parliamentary representation lead to higher national vote shares in states with higher per capita parliamentary party funding?

The absolute amount of state funding made available to parties present in state parliament can be found in the yearly state budgets. We divide the absolute amount of state funding by the number of parties present in the legislature and by the number

of inhabitants in each state to get an idea about how many additional Euro a parliamentary party has at its disposal to spend on organization, service delivery, and on promoting its political goals.[25] The amount of state funding a parliamentary party receives per inhabitant ranges from 10 cents in Baden-Wuerttemberg to 1.7 Euros in Bremen.

We divide our sample into two groups based on whether states fall above or below the median in parliamentary party funding per inhabitant and repeat the analysis on these two subsamples. This step will show whether the consequences of state-level parliamentary election vary depending upon the general level of a state's financial support for political parties. The results displayed in Table 4 strongly support the organizational mechanism, showing that insiders benefit more in electoral terms in those states where parliamentary funding as a rule is more generous.

Table 4: The Role of Parliamentary Benefits

	States with low levels of parliamentary party funding	States with high levels of parliamentary party funding
	IK Bandwidth	
Treatment Effect	0.775 (1.153)	4.176 (1.510)**
95% CIs	[-1.484—3.035]	[1.216—7.136]
Bandwidth	4.84	4.04
n [treated]	80	28
n [control]	154	62
	Fixed Bandwidth ($h = 4$)	
Treatment Effect	0.645 (1.296)	4.208 (1.515)**
95% CIs	[-1.895—3.185]	[1.175—5.502]
n [treated]	72	28
n [control]	77	59

Note: The analysis distinguishes between the eight states with lower (left column) and the eight states with higher (right column) parliamentary funding. We focus only on those cases where the distance between the state and the federal election is at least 851 days, because it is for these cases that we find an effect of parliamentary representation. Using half or double of the IK bandwidth produces substantively identical results. *p<.10; **p<.05, two tailed tests.

[25] We use data from the 2010 fiscal year because this is the earliest year all German states have made their budgets available online. Where data is available from earlier years, we check if the observed rankings of states hold, and it looks like there is a more general pattern of some states providing more generous parliamentary party funding than others. The 2010 data used to rank states according to generosity of parliamentary party funding can be found in Table A.7 in the Appendix.

Second, we explore the effects of state parliamentary representation on future state party membership. If the organizational mechanism holds, parties that enter state parliament should attract more state party activists than parties that fail to pass the 5%-threshold. A growth in the membership base after entering state parliament would thus be evidence for the existence of increasing organizational capabilities in the state.

In order to test if a party attracts more members in those states, where it enters state parliament, we collect yearly data on FDP, Green Party and Left Party membership in the period between 1990 and 2013.[26] Those 23 years were a turbulent period for both parties, in which they lost and gained representation in several state parliaments. Often only a few 1000 votes decided the parties' fates in a state election.

We analyze these data employing again the discontinuities stemming from the 5% threshold. We thus compare the membership rates of the FDP, the Greens and the Left Party ("die Linke") in cases where they (marginally) enter the state parliament to cases in which they are (marginally) left out. Since our membership data vary by year, we can examine the impact of entering the state parliament at year t on parties' membership rates at every year until the next state election. Our outcome of interest thus becomes state party membership (the number of members per 1 million inhabitants) in years $t+1$, $t+2$, $t+3$ and $t+4$. We use the IK bandwidth, half and twice this bandwidth, a common fixed bandwidth of $h = 4$, and local randomization inference to overcome potential small-sample problems arising from the fact that there are only few observations exactly below and above the cutoff point for the shorter period between 1990 and 2013.[27]

The results, reported in Table 5, illustrate the way parliamentary representation yields significant organizational benefits. In cases where the two parties marginally cross the threshold, they have on average around 600 more members per million in-

[26] Our use of the specific parties and time period is only due to data availability reasons. These were the only reliable data we could find on party membership in Germany.

[27] We again engage in sequential testing of windows from the smallest to the largest to find that the largest window within which the local randomization assumption holds is within 1.6 percentage points on either side of the representation threshold.

habitants than in cases in which they marginally fail to cross the threshold. Despite the uncertainty surrounding this estimate, the effect of parliamentary representation on future state party membership is robust to different bandwidths. Using the $\pm 1.6\%$ window around the cutoff point, in which the local randomization assumption seems to hold, randomization-based inference provides a very similar picture. In the last section of the Table, we further explore the difference in membership rates before and after a state election, conditional on parliamentary status. Parties that make it into the state parliament gain approximately 130 more members two years after entering parliament, compared to when they fail to cross the 5% threshold. In line with our hypothesized mechanism, this number increases over time to 162 additional members in the 4th year. In absolute terms, this means that if the FDP enters the state parliament of Northrhine-Westphalia (the largest state in Germany of around 18 million inhabitants), it gains around 2,900 members in the course of the parliament compared to the year before the election. This amounts to around 17% of its average overall membership in this state.

The sixth part of Table 5 tests whether the effect attributed to state parliamentary presence is due to state-specific unobserved heterogeneity. If we simply capitalize on differential membership rates of both parties across states, we should observe the same gap in the years preceding the state election. We test this possibility and find no significant difference in the membership rates of parties at time $t-1$, $t-2$, $t-3$, and $t-4$ according to whether they entered parliament in election t. Apart from not attaining statistical significance at any conventional level, the estimates are of much lower magnitude, and are also changing sign at $t-4$.

When combined, these sets of findings suggest that the membership effects identified here are due to parties' entrance into the state parliament. Gaining more members, parties have more resources to organize on the ground. More members means more people to distribute leaflets, put up signs and talk to their friends come Election Day. It seems highly plausible that these figures have an impact on the electoral fortunes of the party in national elections.

Table 5: The impact of state-level representation on party membership, FDP, Greens, and Left Party 1990-2013

	$Members_{t+1}$	$Members_{t+2}$	$Members_{t+3}$	$Members_{t+4}$
IK Bandwidth				
Treatment Effect	720.0**	651.8**	569.6*	575.5*
	(361.5)	(329.6)	(305.6)	(306.1)
Bandwidth (h)	3.449	3.567	4.254	4.266
n [treated]	62	60	61	60
n [control]	48	48	49	49
Half IK Bandwidth				
Treatment Effect	641.3	577.4	553.3	537.9
	(419.4)	(382.3)	(362.0)	(357.9)
Bandwidth (h)	1.725	1.784	2.127	2.133
n [treated]	36	35	40	39
n [control]	23	23	26	26
Double IK Bandwidth				
Treatment Effect	651.9**	636.7**	609.4**	604.2**
	(303.4)	(269.8)	(246.0)	(245.6)
Bandwidth (h)	6.898	7.134	8.508	8.531
n [treated]	84	84	84	83
n [control]	50	50	49	49
Fixed Bandwidth ($h = 4$)				
Treatment Effect	709.5**	642.9**	583.4*	590.7*
	(350.6)	(321.2)	(312.2)	(312.8)
n [treated]	66	64	60	59
n [control]	50	50	49	49
Randomization Inference (*Window* = 1.6%)				
Treatment Effect	490.6*	467.6*	433.1*	448.9*
	[-62.9—1012.9]	[-31.0—921.9]	[-45.6—847.6]	[-39.5—857.7]
n [treated]	33	32	32	31
n [control]	22	22	21	21
Placebo Tests:	$Members_{t-1}$	$Members_{t-2}$	$Members_{t-3}$	$Members_{t-4}$
Fixed Bandwidth ($h = 4$)				
Treatment Effect	275.0	161.1	51.9	-157.0
	(344.9)	(383.6)	(457.7)	(566.6)
n [treated]	55	49	46	46
n [control]	47	46	46	45
	Change in Members from $t-1$ *to*:			
($h = 4$)	$t+1$	$t+2$	$t+3$	$t+4$
Treatment Effect	81.7	130.1*	150.6*	161.9
	(54.05)	(70.19)	(90.91)	(109.4)
n [treated]	54	52	48	47
n [control]	47	47	46	46
	Randomization Inference : Change in Members from $t-1$ *to*:			
(*Window* = 1.6%)	$t+1$	$t+2$	$t+3$	$t+4$
Treatment Effect	118.6**	165.5***	188**	207.2**
CIs	[20.2—217.9]	[35.1—300.6]	[26.9—352]	[9.5—402.9]
n [treated]	23	22	22	21
n [control]	21	21	20	20

The entries denote differences in members per 100,000 inhabitants according to whether the party achieved state parliamentary representation. *p<.10; ** p<.05; *** p<.0.01, two tailed tests. State party membership data source: Niedermayer, Oskar (2015). Parteimitglieder in Deutschland: Version 2015. Arbeitshefte aus dem Otto-Stammer-Zentrum, Nr. 25.

Alternative Mechanisms

After presenting two types of direct evidence in support of the organizational mechanism, effects on party membership and effects conditional on financial resources provided to parliamentary parties, we now turn to alternative mechanisms. The three most plausible alternative mechanisms that could explain our results are media attention; learning effects related to political experience; and participation in coalition governments. We examine all three mechanisms.

First, we collected media data at the state level for the period between 2000 and 2015, the period for which data was available from online media archives. Our dependent variable is the absolute number of regional newspaper articles that mention the party following a state election in which the party either marginally entered state parliament or marginally failed to do so. The data was collected by quarter using the media search engines Nexis (www.nexis.com), supplemented by Genios (www.genios.de/dosearch) when Nexis data was unavailable for a specific state, for the following parties: FDP, the Greens, NPD, Alternative für Deutschland, and Die Linke. Where there was more than one newspaper per state, for consistency reasons, we always chose the regional newspaper with the longest over-time availability. The results appear in Figure 4.

If media attention could mediate our results, then first, we would expect newspaper articles about parties that enter parliament to be significantly more frequent than articles about parties that fail to enter parliament. Second, we would expect this media effect to increase over time. In fact, we find the exact opposite to be the case. Parties that just marginally entered parliament do not receive significantly more media attention than parties that just marginally failed to do so. Moreover, there is more attention to insiders just after the state election and this attention decreases during the election cycle, in a monotone fashion, relative to the media attention received by parties that just marginally failed to enter parliament.[28] While we do not have any

[28] The figure uses the IK bandwidth. In the Appendix (Figure A.6) we provide the results from the same analysis, using half- and double-IK bandwidths. In both graphs,

information about whether the tone of the newspaper articles was positive or negative, assuming that parties that enter parliament receive an extra amount of scrutiny, these results might help to explain why we fail to find smaller, but positive effects at the beginning of the circle. Media scrutiny might mitigate the effects of the organizational resources that become available right after an election victory.

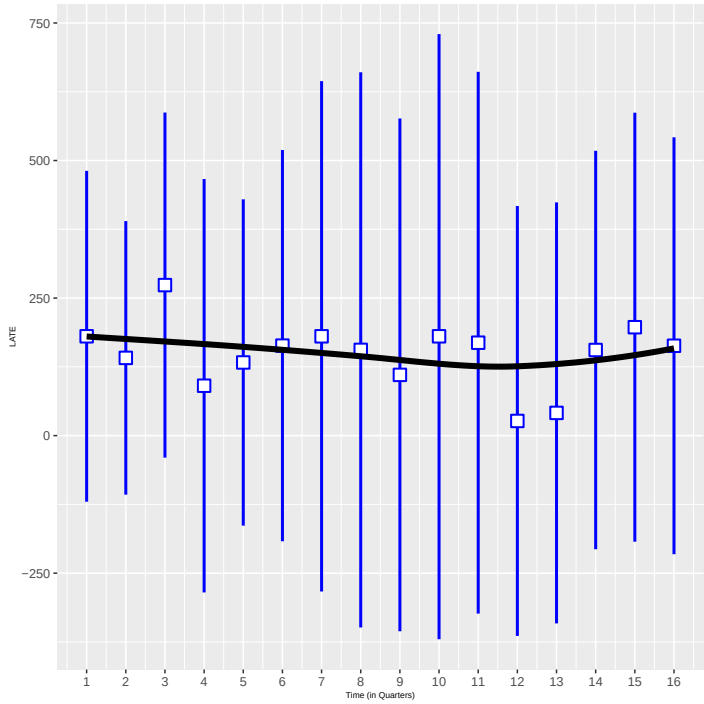


Figure 4: The Impact of State Parliamentary Representation Media Coverage throughout the State Election Cycle.

Note: Each spike denotes the average treatment effect of entering state parliament on media mentions on the party, evaluated at the cutoff point, i.e. the 5% threshold. The empty squares represent estimates stemming from local linear regression, using the IK bandwidth. Each spike presents a quarter of the year. Vertical axes denote the 95% confidence intervals.

Moreover, we test if learning from political experience could explain that parties benefit when more time passes between the state and the federal election. One could imagine that parties gain valuable parliamentary experience and that they can use this experience to gain votes. Following this logic, learning effects should be steeper for parties that have not previously been in parliament than for parties that already have prior parliamentary experience. We try to test this expectation by comparing parties that were present in the previous state parliament versus those that were absent. Full

the overall patterns point to the same conclusion as the pattern shown in Figure 4.

details about the model specifications and the full results are shown in the Appendix (see Table A.8). Table 6 displays the key findings, all of which stem from a global polynomial model in which experience (a dummy switching on for parties being in the previous state legislature) is interacted with all other predictors of federal vote share. Up to three polynomials have been used.

The table is divided into two parts, according to the distance between state and federal election (below or above the median). In each part of the table, the entries of the first two rows denote the local average treatment of state parliamentary presence, evaluated at the cutoff point that corresponds to the electoral threshold, both for parties with and without experience. The third row of each part of the table denotes the difference between the first two rows and thus provides the moderating effect of experience. We do not find consistent evidence in favour of the experience hypothesis. As shown in the third row of both parts of the table, the moderating effect of experience changes sign across the different specifications and is constantly below standard levels of statistical significance. It seems that the only effect that can be detected is in making parliamentary representation from having a negative effect into making no difference for future federal vote share only in cases with small distance between state and federal elections. No effect is found whatsoever for the observations with above-median distance between state and federal elections. Parties that were not present in parliament at time t-1 and enter parliament at time t do not seem to be more likely to do well in federal elections than parties that already gained political experience in the previous parliament.

Finally, coalition dynamics in Germany open one further possibility that needs addressing: Although participation in coalition government is necessarily a consequence of parliamentary representation, we concede that our theorized mechanism does not necessarily rest on participation in state government. To be sure, parties in state government should be given even more privileged access than opposition parties to the organizational benefits alluded to above. However, we believe that being part of the state executive is not a necessary condition for having access into organizational

Table 6: State Parliamentary Representation, Experience and National Electoral Performance.

	One Polynomial	Two Polynomials	Three Polynomials
(1) No Experience × Below	0.342 (1.046)	-2.344 (1.363)*	-2.920 (1.631)**
(2) Experience × Below	0.125 (1.023)	2.961 (1.315)	-0.088 (2.107)
(2) - (1)	-0.217 (1.541)	1.934 (2.090)	2.832 (2.600)
(3) No Experience × Above	2.130 (1.200)	-0.406 (1.536)**	1.957 (1.488)
(4) Experience × Above	1.709 (0.882)**	1.745 (1.434)	3.148 (1.914)*
(4)-(3)	0.421 (1.509)	-1.215 (1.901)	1.911 (2.417)

Note: In each part of the table, the entries of the first two rows denote the local average treatment of state parliamentary presence, conditional on presence or absence of experience. The third row denotes the moderating effect of experience. All estimates stem from OLS models. The standard errors accompanying all point estimates are clustered at the state-election level. Each entry presents a linear combination of coefficients, with analytical standard errors in parentheses. Full details about these models are provided in the Appendix (Table A.8). *p<.10; **p<.05, two-tailed tests.

resources. Consequently, we expect that our results are not driven only by parties that by entering into the legislature become also coalition partners. To test whether this is the case we replicate the analysis in Table 1 excluding the liberal party FDP from the analysis. The FDP has been the main party of government entering frequent coalitions with both CDU and SPD at state and federal level. If results were merely driven by participation in governments, excluding all FDP cases should lower the estimated effect of parliamentary representation. However, as Figure A.7 in the Appendix shows, estimates are very similar when excluding the German liberal party.

Without ruling out all possible alternative mechanisms, these results lend further support to the organizational mechanism underlying the over time effects that we observe in this study.

Discussion

Subnational parliamentary representation can provide a political advantage for parties that contest nationwide elections if the national election does not follow immediately after the successful subnational election. Our results indicate that after 3 or 4 years of crossing the representation threshold in German state parliaments, the electoral advantage in federal elections amounts to around 1.8 percentage points.

What explains this effect? Evidently, it is quite difficult to reconcile this lagged

effect of state parliamentary representation with the idea of bandwagon effects or positive media coverage in the immediate aftermath of a successful election. In contrast, the organizational mechanism seems more likely to fit these results. Parties that are present in state parliaments can offer the material and status benefits of a political career, hire full time staff and use generous funds to build their party organization. The party is therefore better able to attract activists and appeal to the electorate. Activists who set their eyes on a political career find opportunities to get ahead and can in turn be employed by the party for mobilization and persuasion efforts. Voters can address their concerns to state representatives, whose offices provide a range of services to them. However, addressing grievances within the electorate takes time, as does the use of parliamentary funds to build an effective organization. This finding has four important implications that merit some elaboration.

The first implication alludes to the role of subnational institutions as moderators of top-down effects. The more institutionalized a party, the less disruptive the impact of environmental uncertainty (Panebianco 1988, 109). If national trends work against a party, electoral losses will be moderated in those states in which the party has been present in state legislatures. The same holds for gains parties make thanks to national factors. Future research could possibly explore such instances to test some observational implications of the organizational theory. One such implication would be the gradual geographical sorting of small parties' electoral visibility.

Second, the results presented here directly challenge the consensus, according to which politics in decentralized political systems can be described as a one-way street from the higher to the lower level. Such a characterization is overly simplistic. As subnational entities gain more powers, and subnational parliamentary parties gain more resources, political parties gain an electoral advantage from having access to these resources. Since it is easier for smaller political parties to access indirect funding provided to parliamentary parties at the subnational than at the national level, future research can assess whether the multi-level structure of the state might contribute to explaining the varying success of smaller and new parties across established democracies. Al-

though most of the literature on the emergence of new parties has so far concentrated on the proportionality of the electoral system, the degree of decentralization might be another factor that might warrant consideration from researchers.[29]

Third, this paper offers strong support for a crucial assumption of cartelization theory, namely that political parties benefit from being inside the political system, and inside subnational assemblies. However, the mechanism driving this advantage is not necessarily dependent on the systems of direct public party financing. Parties benefit because the state pays for their parliamentary work, because they use state resources to organize, and because access to the state provides parties with high-status and materially lucrative positions to distribute amongst its leadership. In light of these results and previous work, it seems that the mechanism of how established parties benefit in Western democracies is more complex than the debate about official public party financing suggests.

Fourth, the paper calls for a qualification of the dominant view that subnational elections are simply imperfect reflections of party competition at the national level. Previous studies have alluded to such a top-down process either when examining elections at lower (Schakel and Jeffery 2012) or higher (van der Eijk and Franklin 1996) level than the national level. The resulting theory of second-order elections stated that voters often transfer their national voting decision rules into subnational elections. This study reveals that even if this is the case, the final outcome of subnational elections may have indirect effects. By getting access to state funds, parliamentary parties have the opportunity to become more visible and useful to their electorate at different levels of government. Previous evidence also from Germany suggests that voters' gratitude to local services (Bechtel and Hainmueller 2011) lasts for at least one election term. Thus, when there is time for this organizational advantage to materialize, parties that

[29] Given that the Federal Republic of Germany has been a decentralized state right from its founding after WW2, we should not observe significant time trends in this case. While most of the observations in our data set come from more recent decades, preliminary analyses conducted here and shown in detail in the Appendix (Table A.9 and Figure A.8) suggest that there are no robust, significant time trends in our treatment effects, which speaks to their generalizability across different time periods.

have gained access to the decentralized state eventually transfer this benefit from the subnational to the national domain.

Although our empirical results are based on data from German state and federal elections, our theoretical predictions should also apply to other decentralized political systems. The conditions are the existence of an integrated party system across multiple levels of government, and subnational representative bodies that provide access to substantial state resources. Therefore, as subnational entities continue to gain more competencies over the center, one can deduce that apart from the well-documented process of the nationalization of subnational politics, a parallel process of decentralization of national politics is also taking place. As decentralization advances in most established democracies, the political machinery created through representation in subnational institutions gradually transforms national politics by bringing subnational dynamics back into the national political arena.

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Online Appendix

The National Effects of Subnational Representation: Access to Regional Parliaments and National Electoral Performance*

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Abstract

According to scholarly wisdom, party competition at the subnational level plays a negligible role in national elections. We provide theory and evidence that qualifies this view. Subnational elections determine entrance into subnational parliaments, which provides essential organizational resources: members and money. Since in most cases the same political actors compete at all levels of government, they can make use of these resources to improve their status in national party competition. We test our argument exploiting two institutional features of the German multi-level electoral context: the discontinuities generated by the five percent electoral threshold in German state elections, and the occurance of German state elections at different times in the federal election cycle. We find that parties that marginally cross the threshold for state parliamentary representation gain more members, and eventually perform better in national elections, but only if the party has sufficient time to organize between the state and the federal election. Consistent with our organizational explanation, bottom-up effects are more pronounced where state parliamentary parties receive more financial resources. Alternative mechanisms are tested, and receive no empirical support.

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*We would like to thank Alex Coppock, Andy Eggers, Bob Erikson, Edward Fieldhouse, Shigeo Hirano, Camille Strauss-Kahn, Catherine de Vries, two anonymous reviewers and the QJPS editors for their thoughtful comments and suggestions. As a result this paper has been much improved. Moreover we are grateful to Diana Galos and Giordano Neuenschwander for excellent research assistance, and to Heike Merten for background information relating to the development of party financing in Germany.

Appendix

Randomization Inference

To determine the window in which the localized random assignment assumption holds, we test for sequentially nested windows whether any covariate imbalances between parties that marginally cross the 5%-threshold (the treated observations) and parties that marginally do not enter parliament (the untreated observations) are greater than those we would expect to occur from sampling variability alone (Cattaneo, Frandsen and Titiunik 2013). For the general analysis, we first extract the f-statistic regressing parliamentary presence on the following pre-treatment covariates: state vote share at time t-1, state election year, FDP, Green Party, and state dummies. For party membership the following covariates are available: party membership at time t-1, year, FDP, Green Party, and state dummies. We then compare the resulting f-statistic to the mean of the f-statistics we obtain after simulating local random assignment around the 5%-threshold within the chosen window 5.000 times. The sampling distribution of any test statistic (in our case the f-statistic) can be approximated by a large random sample of all possible local random assignments. The collection of f-statistics hence "represents the exact sampling distribution under the null hypothesis that no covariates have any effect on the assigned treatment" (Gerber and Green 2012, 107). The resulting p-value indicates whether we can reject the sharp null hypothesis that the covariates taken together have no effect on localized treatment assignment within the chosen window. A low p-value would therefore indicate that we can reject that imbalances could have occurred due to sampling variability. We choose a conservative significance level of $\alpha = 0.15$ to test whether the local randomization assumption is rejected in each window. We start at a 0.5 percentage points window on each side of the threshold, and raise it sequentially by .1 percentage points on either side of the threshold testing symmetric windows of 0.5, 0.6, 0.7 percentage points and so forth until the sharp null hypothesis is rejected with a p-value lower than 0.15. The detailed results of this exercise are shown in (Table A.1).

For the analysis we follow the procedure outlined in Ho and Imai (2006) and in Gerber and Green (2012). We first calculate the differences-in-means between treated and untreated observations (within the chosen window for which the local randomization assumption holds) using the observations in our existing data set. In order to see if estimated differences in the outcome variable could simply be due to sampling variability, we simulate 5.000 hypothetical local randomization outcomes around the 5%-threshold under the assumption of the "sharp null hypothesis" that the LATE is zero for all units.

Under the sharp-null missing outcomes are equal to the observed outcomes (Gerber and Green 2012, 62). We then go on to compare the differences-in-means estimate that we received from our data to the mean of all differences-in-means estimates over 5.000 hypothetical local randomization outcomes under the assumption of no treatment effect for no observation. To construct 95% confidence intervals we approximate the full schedule of potential outcomes assuming a localized version of SUTVA and that the treatment effect is the same for all observations directly surrounding the 5% threshold (Cattaneo, Frandsen and Titiunik 2013). Adapting the method of forming confidence intervals outlined in Gerber and Green to local quasi random assignment, for observations within the window below the threshold, missing "treated" values are imputed by adding the estimated LATE to the observed untreated values (2012, 66f). For treated observations, missing "untreated" values are imputed by substracting the estimated LATE from the observed treated values. We then list the estimated LATE from each local randomization in ascending order, where the 2.5th percentile marks the bottom of the 95% confidence interval, and the estimate at the 97.5th percentile marks the top (ibid). The interpretation of this confidence interval is that if we imagine a series of hypothetical reruns of our state elections under identical conditions, 95 out of 100 local randomized outcomes in the window directly surrounding the threshold will generate intervals that bracket the true LATE.

Table A.1: Randomization Inference Window Test
(Cut-Off p $\leq$ .15; chosen window in bold)

% on each side	0.5%	0.6%	0.7%	0.8%	0.9%	1.0%	1.1%	1.2%	1.3%	1.4%	1.5%	1.6%	1.7%
Balance test: Full sample													
P-values	0.385	0.297	**0.191**	0.078	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
N [treated]	41	50	50	58	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
N [control]	32	32	44	44	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Balance test: # days $\leq$ 870													
P-values	0.216	**0.163**	0.143	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	
N [treated]	27	35	35	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
N [control]	19	19	26	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Balance test: # days $>$ 870													
P-values	0.758	0.666	0.746	0.562	0.698	0.328	**0.328**	0.146	n/a	n/a	n/a	n/a	n/a
N [treated]	13	14	14	17	17	23	23	23	n/a	n/a	n/a	n/a	n/a
N [control]	12	12	17	17	21	23	23	28	n/a	n/a	n/a	n/a	n/a
Balance test: Party membership													
P-values	0.477	0.252	0.190	0.289	0.403	0.264	0.415	0.455	0.351	0.351	0.289	**0.157**	0.145
N [treated]	12	16	19	22	22	23	27	28	32	32	32	34	36
N [control]	6	7	7	7	11	13	13	13	15	15	20	22	22

The entries in the top of every section denote the p-value associated with the sharp-null stemming from balance tests across sequential windows around the 5% threshold.

Table A.2: Main Analysis with Fixed Effects

	Year	Party	State	Year × Party	Year × State	State × Party
			IK Bandwidth ($h = 5.48$)			
Treatment	1.809**	1.408**	1.705**	1.809**	1.417**	1.814**
Effect	(0.701)	(0.680)	(0.857)	(0.701)	(0.652)	(0.789)
n	445	445	445	445	445	445
			Half-IK Bandwidth ($h = 2.74$)			
Treatment	1.311	1.442	0.923	1.195	1.311	2.111*
Effect	(0.963)	(0.947)	(1.317)	(1.235)	(0.963)	(1.123)
n	155	155	155	155	155	155
			Double-IK Bandwidth ($h = 10.96$)			
Treatment	1.885**	1.519**	1.891**	1.504**	1.885**	1.842**
Effect	(0.660)	(0.630)	(0.770)	(0.587)	(0.660)	(0.726)
n	445	445	445	445	445	445

Note: The entries denote the treatment effect (τ) of entering the state parliament on the party's vote share in the same state in the next federal election. The heading of each column denotes the type of fixed effects included in the analysis. In all models, standard errors are clustered at the state-election level. *p<.10; **p<.05, two-tailed tests.

Table A.3: State Parliamentary Representation and Seats Gained in the Next Federal Election

	Full Sample	Below Median	Above Median
		IK Bandwidth	
Treatment Effect	2.943 (1.261)**	0.132 (1.632)	4.758 (2.579)*
95% CIs	[0.471—5.415]	[-3.067—3.331]	[-0.297—9.813]
Bandwidth (h)	4.82	2.90	5.02
n [treated]	455	202	193
n [control]	1741	285	1144
		Half-IK Bandwidth	
Treatment Effect	2.157 (1.627)	0.843 (1.828)	5.325 (3.520)
CIs	[-1.032—5.346]	[-2.740–4.426]	[-1.574—12.224]
Bandwidth (h)	2.41	1.45	2.51
n [treated]	288	106	124
n [control]	332	104	148
		Double-IK Bandwidth	
Treatment Effect	3.400 (1.136)**	2.382 (1.311)**	4.332 (2.270)*
CIs	[-1.173—5.627]	[-0.188–4.952]	[-0.117—8.781]
Bandwidth (h)	9.64	5.80	10.04
n [treated]	468	253	193
n [control]	2696	1316	1144
		Fixed Bandwidth ($h = 4$)	
Treatment Effect	2.640 (1.359)*	1.024 (1.498)	5.130 (2.875)*
CIs	[-0.024—5.304]	[-1.912–3.961]	[-0.505—10.765]
Bandwidth (h)	4	4	4
n [treated]	404	218	166
n [control]	717	364	294
		Placebo Outcome, $V_{s,t-1}$: # days > 870	
	IK bandwidth	Half-IK Bandwidth	Double-IK Bandwidth
Treatment Effect	1.662 (1.719)	0.836 (2.093)	2.217 (1.623)
CIs	[-1.707—5.031]	[-3.266—4.938]	[-0.964—5.398]
Bandwidth (h)	5.06	2.53	10.12
n [treated]	192	124	192
n [control]	1124	143	1124

Note: The table replicates Table 1 of the main text, using number of seats obtained in the same state in the next federal election as the dependent variable. Estimation and interpretation follow the logic explicated in the main text. *p<.10; **p<.05, two-tailed tests.

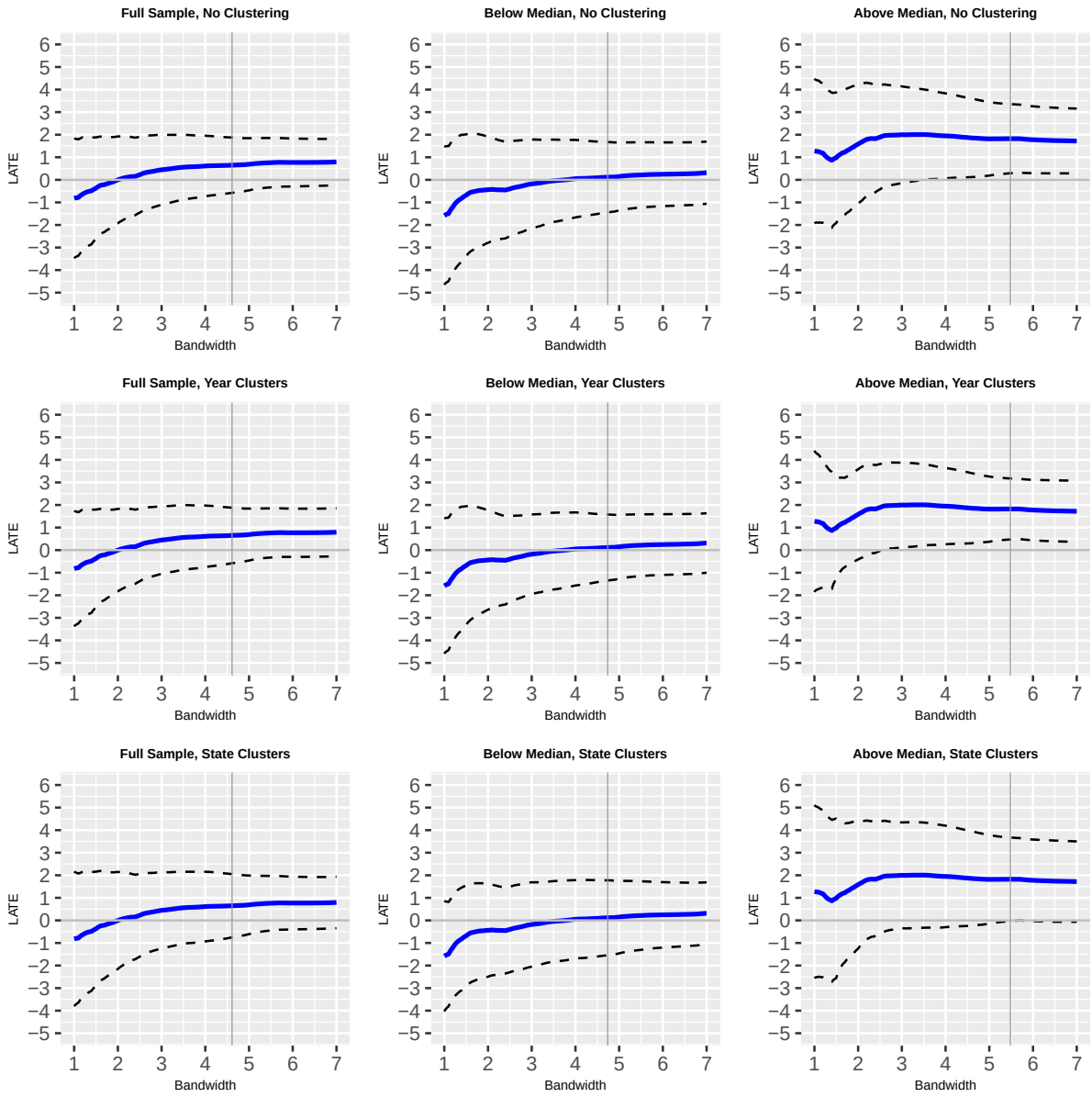

Figure A.1: Replicating the Main Analysis using Alternative Error Structures: No clustering, Year- and State-clustering.

Note: The solid black line in each graph denotes the local average treatment effect across different bandwidths, as denoted in the horizontal axis. The dashed curves denote the 95% CIs and the vertical dashed line indicates the IK bandwidth. The first row uses no clustering; the second clusters errors at the state election year level whereas the third column uses state-level clusters. The first column includes all observations. The second column includes only those observations in which the distance between state and federal election is below the median (851 days). The third column looks at observations in which the distance between state and federal election is above the median.

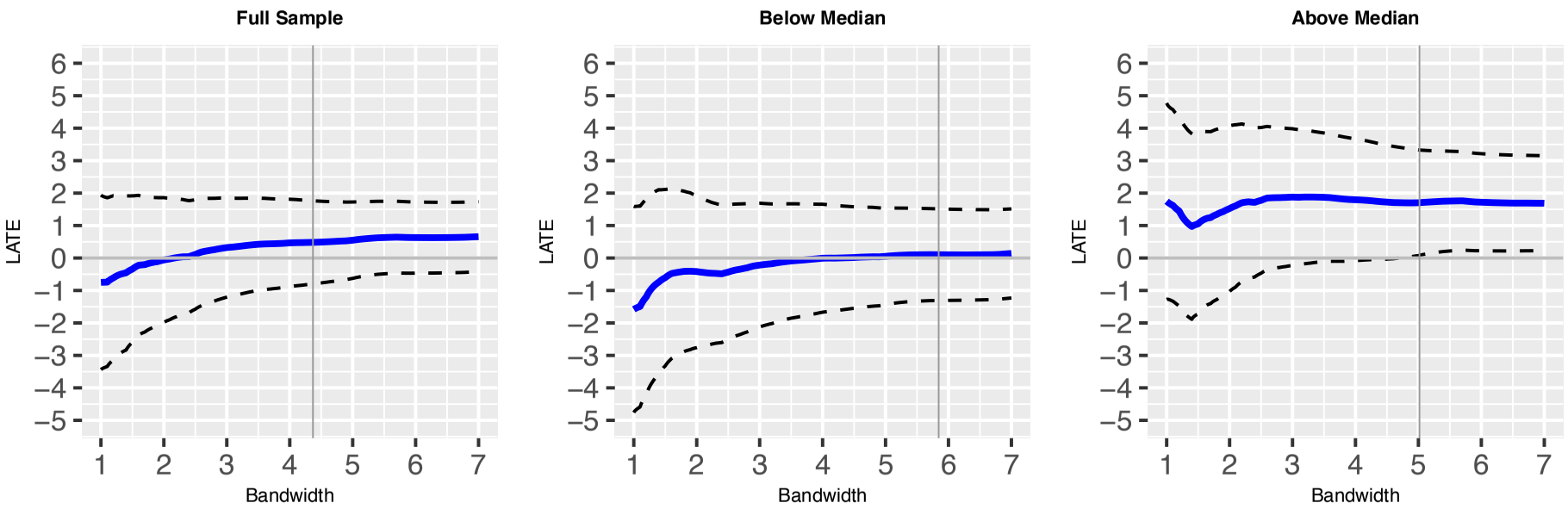

Figure A.2: Replicating the Main Analysis, Excluding Observations between 1959 and 1969

Note: The solid black line in each graph denotes the local average treatment effect across different bandwidths, as denoted in the horizontal axis. The dashed curves denote the 95% CIs and the vertical dashed line indicates the IK bandwidth. The first graph includes all observations. The second graph includes only those observations in which the distance between state and federal election is below the median (851 days). The third graph looks at observations in which the distance between state and federal election is above the median.

Splitting Groups According to the 2.5-Years Rule

Table A.4 and Figure A.3 replicate Table 2 and Figure 2 of the main text, using a different decision rule for the grouping of observations. In particular, instead of dividing between observations below and above the median distance between state and federal elections, we distinguish between observations in which the federal election took place up until 2.5 years since the state election and observations in which the federal election took place after at least 2.5 years had passed since the last state election.

Table A.4: Parametric Analysis of the impact of State Parliamentary Representation on Federal Vote Share.

	One Polynomial	Two Polynomials	Three Polynomials
$X_{i,s.t}$	1.277 (0.101)**	1.961 (0.468)**	2.877 (1.301)**
$D_{i,s,t}$	0.413 (0.682)	-0.503 (1.011)	-0.824 (1.413)
Above 2.5 Years	-0.616 (0.668)	-0.976 (1.107)	-2.276 (1.510)
$X_{i,s.t} \times D_{i,s,t}$	-0.505 (0.281)*	-0.932 (0.676)	-2.169 (1.897)
$X_{i,s.t}\times$ Above 2.5 Years	-0.175 (0.151)	-0.697 (0.708)	-2.557 (1.815)
$D_{i,s,t}\times$ Above 2.5 Years	1.352 (1.000)	2.147 (1.619)	3.831 (2.140)*
$X_{i,s.t} \times D_{i,s,t}\times$ Above 2.5 Years	-0.135 (0.324)	0.075 (0.390)	0.001 (0.423)
$X^2_{i,s.t}$		0.120 (0.070)*	0.472 (0.484)
$X^2_{i,s.t} \times D_{i,s,t}$		-0.166 (0.171)	-0.437 (1.204)
$X^2_{i,s.t}\times$ Above 2.5 Years		-0.099 (0.107)	-0.768 (0.675)
$X^2_{i,s.t} \times D_{i,s,t}\times$ Above 2.5 Years		0.139 (0.252)	2.038 (1.715)
$X^3_{i,s.t}$			0.039 (0.053)
$X^3_{i,s.t} \times D_{i,s,t}$			-0.042 (0.142)
$X^3_{i,s.t}\times$ Above 2.5 Years			-0.070 (0.075)
$X^3_{i,s.t} \times D_{i,s,t}\times$ Above 2.5 Years			-0.113 (0.119)
Intercept	6.230 (0.446)**	6.871 (0.739)**	7.429 (1.001)**
n (clusters)	901 (191)	901 (191)	901 (191)
Average Treatment Effects			
τ: Below Median ($D_{i,s,t}$)	0.413 (0.682)	-0.503 (1.011)	-0.824 (1.413)
τ: Above Median ($D_{i,s,t} + D_{i,s,t}\times$ Above Median)	1.765 (0.766)**	1.645 (1.268)	3.007 (1.549)*

Note: The entries denote OLS estimates, with $X_{i,s,t}$ being the forcing variable (state vote share of party i, in state s and election t) and $D_{i,s,t}$ a binary indicator switching on for observations above the threshold. Errors are clustered at the state-election level. Analytical standard errors are shown in the last two rows of the table. *p<.10; **p<.05, two-tailed tests.

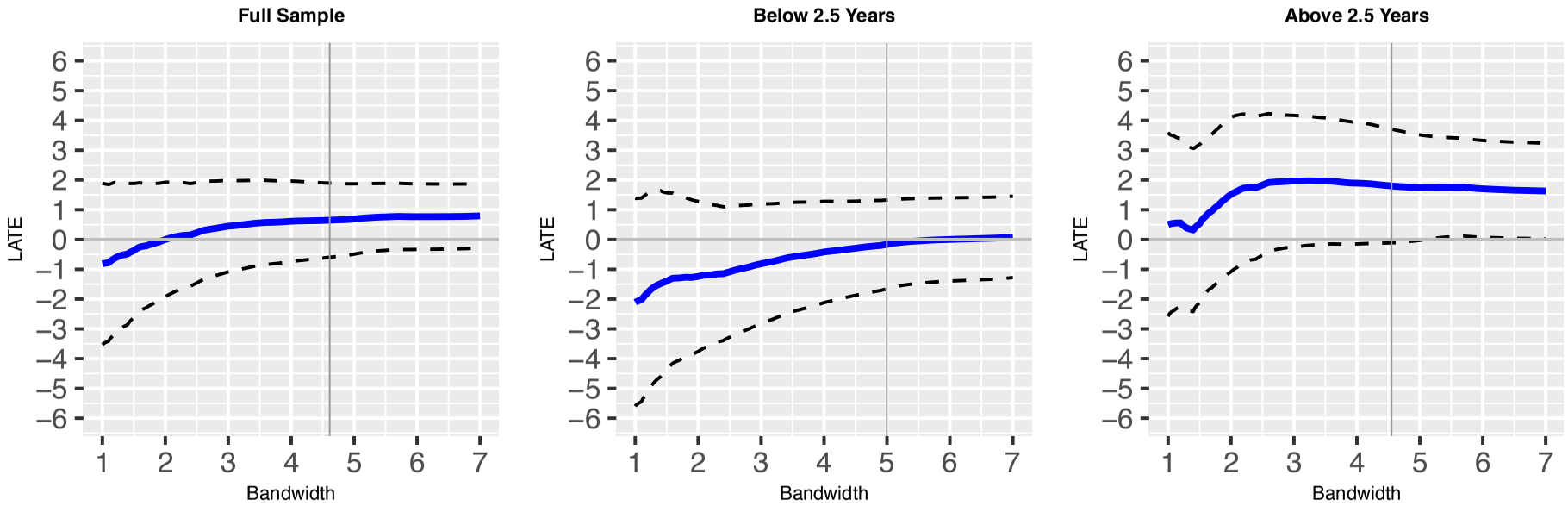

Figure A.3: Local Linear Regression Estimates Across Different Bandwidths, Below and Above 2.5 Years.
Note: The first graph presents the estimates from the full sample, whereas the last two graphs split observations according to whether the federal election has taken place below (second column) or above (third column) 2.5 years. The black solid line denotes the LATE across the range of bandwidths, whereas the dashed curves capture the 95% confidence intervals.

Sorting

We implement the McCrary test, which examines the presence of a significant gap in the density of the assignment variable at the cutoff point. The test is shown in Figure A.4. We focus on the first row of the graph. The first column includes all observations, whereas the second and third columns use only those below and above median distance between state and federal election respectively. We find a significant jump in the density of the state vote share at election t at the cutoff point. The θ statistic of the test is 1.15, which, with a with standard error 0.41, returns a p-value$<$0.01, thus leading us to reject the null of no sorting. The same conclusions is derived from the second column, which includes only observations with close distance between state and federal election. For these observations, $\theta = 1.02$ (std. error 0.45), rejecting the null of no-sorting at p$<$ .05. We find no evidence for sorting in the last column of the first row, however, which looks at observations with considerable distance between state and federal election. θ is 0.54, with a standard error of 0.38, generating a p-value of 0.16, which does not allow us to reject the null of no sorting at conventional levels of statistical significance.

Although the pattern of sorting found is unlikely to drive our results, it still leaves open potential room for manipulation in the assignment variable, even in the last group of observations. We try to further address this possibility by considering one potential reason for such manipulation. Between 1959 and 1969, even if the total amount of subventions was contingent upon the party's vote share, it was only available to parliamentary insiders. This rule could have generated higher incentives among parties to even marginally try to cross the threshold.[1] We thus replicate the sorting analysis excluding all observations with state elections during this period. We thus include only observations before 1959 and after 1969. The results are shown in the second row of Figure A.4. Sorting has been reduced in all three cases, but mainly so in the last column, which includes the set of observations with longer distance between state and federal elections. θ is now 0.42 (std. error 0.61) with p-value very close to 0.5. Clearly, there is no evidence of sorting among this group of observations.[2]. Looking back at Figure A.2, reminds us that that the results remain robust to the exclusion of these observations from the analysis.

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[1] The simultaneous presence of direct subsidies and parliamentary benefits arising from entering parliament also creates a problem of compound treatment effect for this particular period. Excluding these observations thus helps to also address this alternative mechanism for the treatment effects.

[2] The equivalent figures are $\theta = 0.88$ (std. error 0.39, p$<$.05) and $\theta = 0.83$ (std. error 0.41, p$<$.05) for the first and second column of the second row respectively.

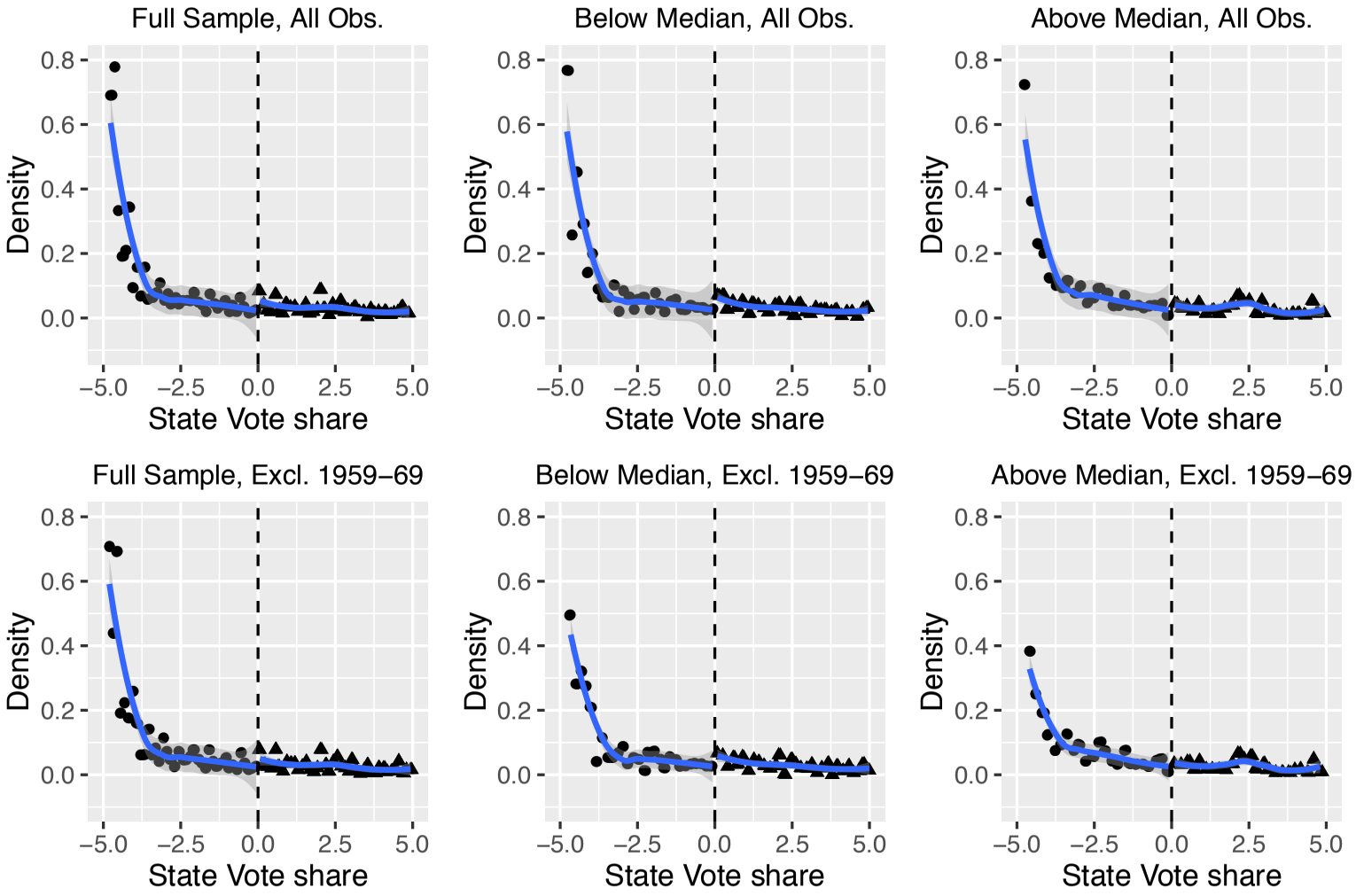


Figure A.4: A Density Inspection for Manipulation in the Forcing Variable

Note: Each dot represents a local average treatment effect, given that the federal election took place at least as many days as denoted in the x-axis since the state election. The blue local regression curve summarizes the overall pattern.

Table A.5: Balance Checks, Full Sample

	RD Estimates	95% Lower Bound	95% Upper Bound	IK Bandwidth
Extreme right	-0.057 (0.074)	-0.203	0.088	2.15
East Germany	-0.015 (0.097)	-0.205	0.174	2.35
Year	3.05574 (3.000)	-2.824	8.936	6.47
Year^2	12064.3 (11881)	-11222	35351	10.35
Post-1989	0.122 (0.090)	-0.054	0.298	2.48
FDP	-0.175 (0.109)	-0.388	0.038	1.61
Greens	0.179 (0.081)**	0.020	0.338	1.83
Baden-Wuttemberg	0.017 (0.034)	-0.050	0.083	2.22
Bayern	-0.039 (0.053)	-0.143	0.065	1.96
Berlin	0.003 (0.058)	-0.112	0.118	2.04
Brandenburg	0.040 (0.021)**	-0.000	0.081	1.65
Bremen	0.047 (0.046)	-0.043	0.137	1.82
Hamburg	-0.217 (0.102)**	-0.418	-.0174	1.87
Hessen	0.060 (0.067)	-0.074	0.192	1.95
Mecklenburg-Vorpommern	-0.023 (0.034)	-0.089	0.043	2.09
Niedersachsen	-0.068 (0.059)	-0.184	(0.048)	1.97
Nordrhein-Westfalen	0.050 (0.057)	-0.062	0.162	2.28
Rheinland-Pfalz	-0.006 (0.042)	-0.088	0.076	1.99
Saarland	0.154 (0.051)**	0.055	0.254	2.02
Sachsen	-0.043 (0.062)	-0.165	0.079	1.82
Sachsen-Anhalt	0.065 (0.044)	-0.021	0.151	1.94
Schleswig-Holstein	-0.004 (0.065)	-0.133	0.124	2.12
Thueringen	-0.058 (0.033)*	-0.123	0.007	1.87

Note: The entries in the first column denote the treatment effect (τ) of entering the state parliament on various placebo outcomes. Standard errors, clustered at the state-election level, in parentheses. *p<.10; **p<.05, two-tailed tests.

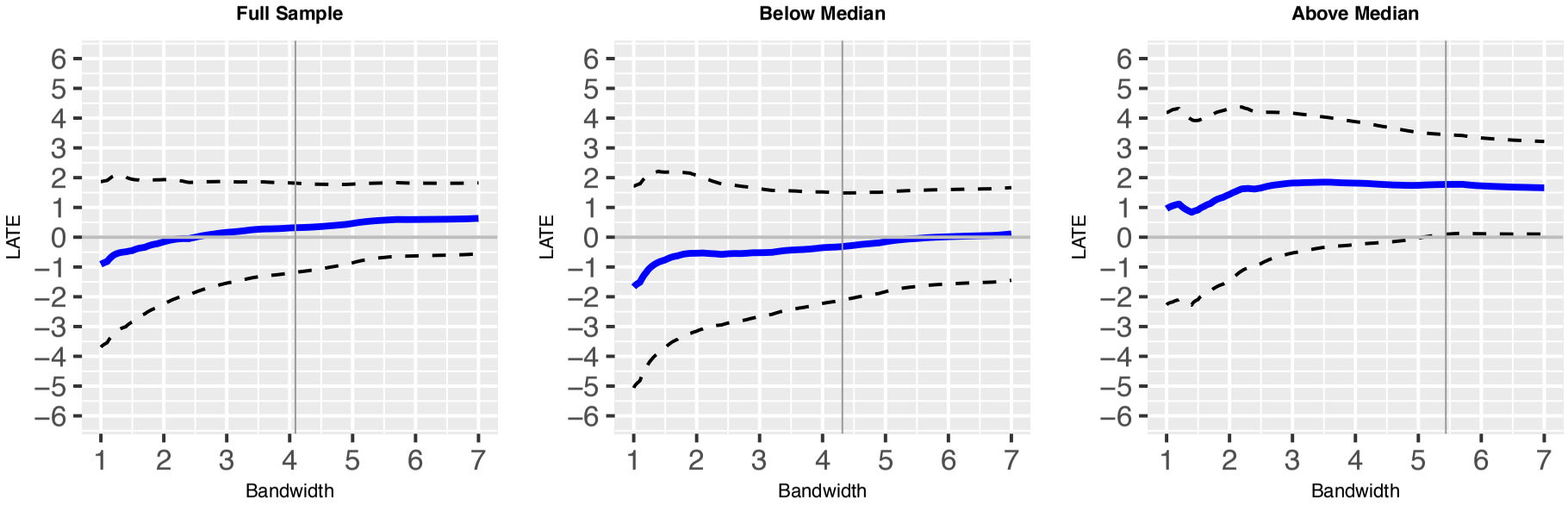


Figure A.5: Replicating the Main Analysis, Excluding the states of Saarland and Thuringen.

Note: The solid black line in each graph denotes the local average treatment effect across different bandwidths, as denoted in the horizontal axis. The dashed curves denote the 95% CIs and the vertical dashed line indicates the IK bandwidth. The first graph includes all observations. The second graph includes only those observations in which the distance between state and federal election is below the median (851 days). The third graph looks at observations in which the distance between state and federal election is above the median.

Within-Group Jumps

We use both the within-group median and the within-group mean for each set of analysis. Among parties with $0 < X_{i,s,t} < 5$, none of whom achieves parliamentary representation, those below the median are compared to those above the median. The same comparison takes place separately for parties with $5 < X_{i,s,t} < 10$, all of whom enter the state parliament. The results appear in Table A.6.

Table A.6: Robustness Check: Within-Group Jumps

	Within-Group Jumps		Within-Group Jumps	
	$c = 0.7$	$c = 1.23$	$c = 7$	$c = 7.11$
	Median	Mean	Median	Mean
	IK Bandwidth			
Tr Effect	0.167 (0.236)	-0.766 (0.501)	1.028 (1.215)	-0.313 (1.668)
95% CIs	[-0.295—0.630]	[-1.628—0.973]	[-1.353—3.409]	[-3.582—2.956]
Bandwidth (h)	1.405	2.479	1.404	1.809
n [treated]	86	52	38	35
n [control]	164	171	26	44
	Bandwidth: $h = 2.5$			
Tr. Effect	0.225 0.215	-0.204 (0.410)	-0.416 (1.089)	-0.119 (1.455)
95% CIs	[-0.197—0.647]	[-1.619—0.967]	[-2.550—1.718]	[-2.970—2.733]
n [treated]	123	97	50	49
n [control]	164	206	54	62

Note: The first two columns compare parties with $c \leq X_{i,s,t} < 5\%$ and parties with $0\% \leq X_{i,s,t} < c$. None of these parties has achieved parliamentary representation. The last two columns compare parties with $5\% \leq X_{i,s,t} < c$ and parties with $c \leq X_{i,s,t} \leq 10\%$. All parties enter the parliament in the last two colums. We use the median vote share of parties with $0\% < X_{i,s,t} < 5\%$ and with $5\% < X_{i,s,t} < 10\%$ to create the comparison groups in columns one and three respectively. We limit our observations to the 0-10% interval. We also use the mean cutoff point, i.e. $c = 1.23$ for parties with $X_{i,s,t} < 5\%$ and $c = 7.11$ for parties with$X_{i,s,t} \leq 5\%$.

Table A.7: Parliamentary Funding per state, adjusted by number of parties in the parliament and by state population size, 2010 data

State	Parliamentary Funding per capita & per party	Total Parliamentary Expenditure (in million euros)
Baden-Wuerttemberg	0.102	5.488
Nordrhein-Westfalen	0.110	9.802
Niedersachsen	0.117	6.837
Bayern	0.234	14.673
Schleswig-Holstein	0.248	4.211
Hessen	0.276	8.383
Rheinland-Pfalz	0.331	3.973
Sachsen	0.410	10.221
Berlin	0.415	7.172
Hamburg	0.447	3.972
Brandenburg	0.460	5.762
Sachsen-Anhalt	0.559	5.192
Saarland	0.629	3.200
Thueringen	0.671	7.494
Mecklenburg-Vorpommern	0.685	5.629
Bremen	1.670	5.516

Note: The dashed horizontal line denotes the distinction between states with low and states . with high levels of parliamentary funding. Data Sources: "Landeshaushalte" as published online: http://www.haushaltssteuerung.de/landeshaushalte.html

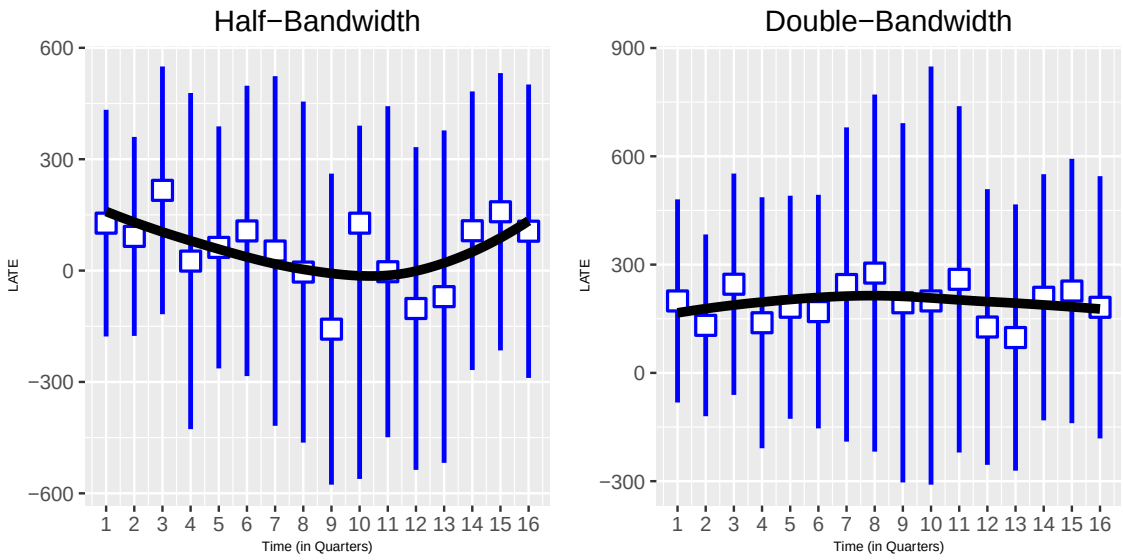


Figure A.6: Testing the Media Hypothesis by Using Half and Double the IK bandwidth.

Note: Both graphs replicate Figure 4. The first panel uses half the IK bandwidth for each quarter, whereas the second panel uses the double of the IK bandwidth. Each spike denotes the average treatment effect of entering state parliament on media mentions on the party, evaluated at the cutoff point, i.e. the 5% threshold. The empty squares represent estimates stemming from local linear regression, using the IK bandwidth. Each spike presents a quarter of the year. Vertical axes denote the 95% confidence intervals.

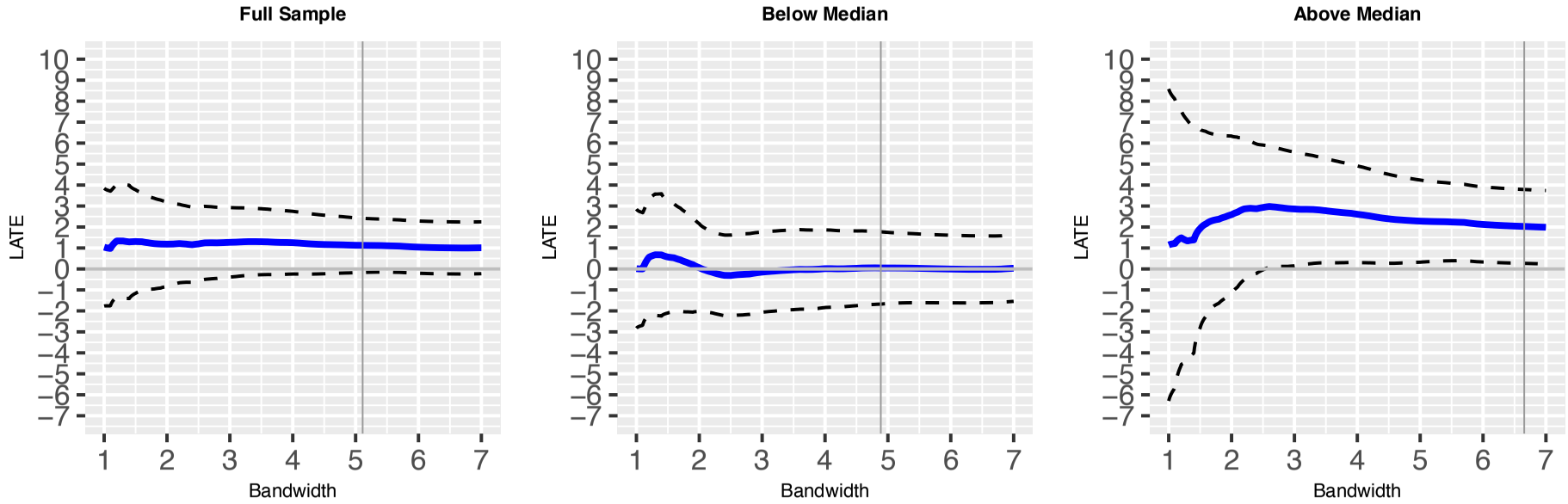


Figure A.7: Replicating the Main Analysis, Excluding the FDP.

Note: The solid black line in each graph denotes the local average treatment effect across different bandwidths, as denoted in the horizontal axis. The dashed curves denote the 95% CIs and the vertical dashed line indicates the IK bandwidth. The first graph includes all observations. The second graph includes only those observations in which the distance between state and federal election is below the median (851 days). The third graph looks at observations in which the distance between state and federal election is above the median.

The (non-)Moderating Role of Experience

To examine whether experience qualifies the effect of parliamentary representation, we estimate the following model:

$$
\begin{aligned}
Y_{i,s,t+1} = \beta_0 &+ \beta_1 X^*_{i,s,t} + \beta_2 D_{i,s,t} + \beta_3 Exp_{i,s,t-1} + \beta_4 AM_{s,t} \\
&+ \beta_5 X^*_{i,s,t} D_{i,s,t} + \beta_6 X^*_{i,s,t} Exp_{i,s,t-1} + \beta_7 X^*_{i,s,t} AM_{s,t} \\
&+ \beta_8 D_{i,s,t} Exp_{i,s,t} + \beta_9 D_{i,s,t} AM_{s,t} + \beta_{10} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{11} X^*_{i,s,t} D_{i,s,t} Exp_{i,s,t} + \beta_{12} X^*_{i,s,t} D_{i,s,t} AM_{s,t} \\
&+ \beta_{13} X^*_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + \beta_{14} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{15} X^*_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + u_{ist}
\end{aligned}
$$

where $Y_{i,s,t+1}$ denotes the vote share of party i, at state s in the federal election that follows the state election t; $D_{i,s,t}$ is a binary indicator of parties that crossed the threshold in the state election; $Exp_{i,s,t}$ is also a dummy variable that switches on for parties that crossed the threshold in the state election $t-1$ and thus can be considered as having already parliamentary experience. $AM_{s,t}$ (*Above Median*) denotes observations with more than median distance between the state election t and the federal election $t+1$. Finally, let $X_{i,s,t}$ represent the assignment variable, i.e. parties' vote share at state election t. If c is the electoral threshold, then: $X^*_{i,s,t} = X_{i,s,t} - c$.

Our interest lies in the moderating effect of experience. To capture this effect, we estimate four different quantities of interest. All of them constitute local average treatment effect, as evaluated at the cutoff point, i.e. the 5% electoral threshold. Thus, for all quantities of interest, $X^*_{i,s,t} = 0$.

- The average effect of parliamentary representation on federal vote share for a party *without experience* in a federal election that comes *near* the previous state election: β_2 (Part 1, Row 1 of Table 6).

- The average effect of parliamentary representation on federal vote share for a party *with experience* in a federal election that takes place *near* the previous state election: $\beta_2 + \beta_8$ (Part1, Row 2 of Table 6).

- The average effect of parliamentary representation on federal vote share for a party *without experience* in a federal election that comes *far* from the previous state election: $\beta_2 + \beta_9$ (Part 2, Row 1 of Table 6).

- The average effect of parliamentary representation on federal vote share for a

party *with experience* in a federal election that comes *far* from the previous state election: $\beta_2 + \beta_8 + \beta_9 + \beta_{14}$ (Part 2, Row 2 of Table 6).

Therefore, the moderating role of *Experience* in the impact of state parliamentary representation on federal vote share is given by:

- β_8 for elections below the median distance between state and federal election (Part 1, Row 3 of Table 6).
- $\beta_8 + \beta_{14}$ for elections above the median distance between state and federal election (Part 2, Row 3 of Table 6).

Analytical standard errors are estimated for each of these quantities. Bootstrapped standard errors provide substantively identical results.

This equation is then augmented to incorporate non-linearities in the relationship between the assignment variable and the outcome. The parameters providing the quantities of interest (shown in Table 6) remain the same in all three models (as described above, in the bullet points). However, the equation is expanded to incorporate the higher polynomials of the assignment variable. In particular, the second polynomial model is estimated as follows:

$$\begin{aligned}
Y_{i,s,t+1} = \beta_0 &+ \beta_1 X^*_{i,s,t} + \beta_2 D_{i,s,t} + \beta_3 Exp_{i,s,t-1} + \beta_4 AM_{s,t} \\
&+ \beta_5 X^*_{i,s,t} D_{i,s,t} + \beta_6 X^*_{i,s,t} Exp_{i,s,t-1} + \beta_7 X^*_{i,s,t} AM_{s,t} \\
&+ \beta_8 D_{i,s,t} Exp_{i,s,t-1} + \beta_9 D_{i,s,t} AM_{s,t} + \beta_{10} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{11} X^*_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} + \beta_{12} X^*_{i,s,t} D_{i,s,t} AM_{s,t} \\
&+ \beta_{13} X^*_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + \beta_{14} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{15} X^*_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + \\
&+ \beta_{16} X^{2*}_{i,s,t} + \beta_{17} D_{i,s,t} X^{2*}_{i,s,t} + \beta_{18} Exp_{i,s,t-1} X^{2*}_{i,s,t} \\
&+ \beta_{19} AM_{s,t} X^{2*}_{i,s,t} + \beta_{20} X^{2*}_{i,s,t} D_{i,s,t} AM_{s,t} + \beta_{21} X^{2*}_{i,s,t} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{22} X^{2*}_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + u_{ist}
\end{aligned}$$

where all terms are defined as above. Including three polynomials, the equation is qualified as follows:

$$
\begin{aligned}
Y_{i,s,t+1} = \beta_0 &+ \beta_1 X^*_{i,s,t} + \beta_2 D_{i,s,t} + \beta_3 Exp_{i,s,t-1} + \beta_4 AM_{s,t} \\
&+ \beta_5 X^*_{i,s,t} D_{i,s,t} + \beta_6 X^*_{i,s,t} Exp_{i,s,t-1} + \beta_7 X^*_{i,s,t} AM_{s,t} \\
&+ \beta_8 D_{i,s,t} Exp_{i,s,t-1} + \beta_9 D_{i,s,t} AM_{s,t} + \beta_{10} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{11} X^*_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} + \beta_{12} X^*_{i,s,t} D_{i,s,t} AM_{s,t} \\
&+ \beta_{13} X^*_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + \beta_{14} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{15} X^*_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + \\
&+ \beta_{16} X^{2*}_{i,s,t} + \beta_{17} D_{i,s,t} X^{2*}_{i,s,t} + \beta_{18} Exp_{i,s,t-1} X^{2*}_{i,s,t} \\
&+ \beta_{19} AM_{s,t} X^{2*}_{i,s,t} + \beta_{20} X^{2*}_{i,s,t} D_{i,s,t} AM_{s,t} + \beta_{21} X^{2*}_{i,s,t} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{22} X^{2*}_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} + \beta_{23} X^{2*}_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{24} X^{3*}_{i,s,t} + \beta_{25} D_{i,s,t} X^{3*}_{i,s,t} + \beta_{26} Exp_{i,s,t-1} X^{3*}_{i,s,t} \\
&+ \beta_{27} AM_{s,t} X^{3*}_{i,s,t} + \beta_{28} X^{3*}_{i,s,t} D_{i,s,t} AM_{s,t} + \beta_{29} X^{3*}_{i,s,t} Exp_{i,s,t-1} AM_{s,t} \\
&+ \beta_{30} X^{3*}_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} \\
&+ \beta_{31} X^{3*}_{i,s,t} D_{i,s,t} Exp_{i,s,t-1} AM_{s,t} + u_{ist}
\end{aligned}
$$

As already pointed out, the key parameters that provide the quantities of interest remain the same in all three models. Table A.8 diplays the full estimates from all three models.

Are there Time Trends in the Magnitude of the Effects?

We try to examine the presence of a time trend in the effect of state parliamentary presence on federal vote share by incorporating a linear time trend in the polynomial specification. Similar to the exercise regarding previous parliamentary experience, we include up to a third polynomial of the assignment variable (party's vote share at state election t). The three model specifications are identical to the ones employed when looking at the moderating effect of parliamentary experience, except from the fact that instead of $Exp_{i,s,t-1}$, we know use $Year_t$, which is a simple counter of the federal election year and ranges from 1947 to 2013. The first polynomial equation, then, takes the following form:

Table A.8: Does Experience Moderate the Effect of Parliamentary Representation on Federal Vote share?

	One Polynomial	Two Polynomials	Three Polynomials
$X_{i,s.t}$	1.347 (0.142)**	2.612 (0.590)**	2.791 (1.762)
$D_{i,s,t}$	0.226 (1.078)	-2.345 (1.364)*	-2.920 (1.631)*
$Exp_{i,s,t-1}$	0.088 (0.977)	-0.621 (1.577)	-0.785 (2.191)
$AM_{s,t}$	-1.627 (0.889)*	-1.892 (1.301)	-1.113 (1.520)
$X_{i,s.t} \times D_{i,s,t}$	-1.226 (0.418)**	1.034 (1.022)	3.720 (3.163)
$X_{i,s.t} \times Exp_{i,s,t-1}$	-0.030 (0.219)	-0.829 (0.960)	-1.176 (2.872)
$X_{i,s.t} \times AM_{s,t}$	-0.397 (0.196)**	-0.948 (0.757)	0.411 (2.271)
$D_{i,s,t} \times Exp_{i,s,t-1}$	-0.077 (1.561)	1.939 (2.090)	2.832 (2.600)
$D_{i,s,t} \times AM_{s,t}$	1.472 (1.731)	5.306 (1.844)**	4.878 (2.185)**
$Exp_{i,s,t} \times AM_{s,t}$	0.760 (1.285)	0.129 (2.057)	-0.568 (2.840)
$X_{i,s.t} \times D_{i,s,t} \times Exp_{i,s,t-1}$	0.738 (0.665)	-1.919 (1.662)	-5.010 (4.515)
$X_{i,s.t} \times D_{i,s,t} \times AM_{s,t}$	0.686 (0.584)	-5.534 (1.570)**	-9.455 (5.298)*
$X_{i,s.t} \times Exp_{i,s,t-1} \times AM_{s,t}$	0.193 (0.286)	-0.093 (1.267)**	-1.297 (3.797)
$D_{i,s,t} \times Exp_{i,s,t-1} \times AM_{s,t}$	-0.234 (2.262)	-3.155 (2.752)	-1.641 (3.517)
$X_{i,s.t} \times D_{i,s,t} \times Exp_{i,s,t-1} \times AM_{s,t}$	-0.849 (0.845)	6.784 (2.521)**	7.291 (7.293)
$X^2_{i,s.t}$		0.221 (0.085)**	0.301 (0.672)
$X^2_{i,s.t} \times D_{i,s,t}$		-1.178 (0.213)**	-3.197 (1.599)**
$X^2_{i,s.t} \times Exp_{i,s,t-1}$		-0.142 (0.139)	-0.294 (1.068)
$X^2_{i,s.t} \times AM_{s,t}$		-0.109 (0.110)	0.451 (0.888)
$X^2_{i,s.t} \times D_{i,s,t} \times Exp_{i,s,t-1}$		1.086 (0.330)**	3.473 (2.030)*
$X^2_{i,s.t} \times D_{i,s,t} \times AM_{s,t}$		1.706 (0.326)**	3.130 (3.097)
$X^2_{i,s.t} \times Exp_{i,s,t-1} \times AM_{s,t}$		-0.031 (0.186)**	-0.526 (1.412)
$X^2_{i,s.t} \times D_{i,s,t} \times Exp_{i,s,t-1} \times AM_{s,t}$		-1.687 (0.495)**	-1.496 (3.735)
$X^3_{i,s.t}$			0.010 (0.074)
$X^3_{i,s.t} \times D_{i,s,t}$			0.309 (0.253)
$X^3_{i,s.t} \times Exp_{i,s,t}$			-0.018 (0.116)
$X^3_{i,s.t} \times AM_{s,t}$			0.065 (0.099)
$X^3_{i,s.t} \times D_{i,s,t} \times Exp_{i,s,t}$			-0.340 (0.299)
$X^3_{i,s.t} \times D_{i,s,t} \times AM_{s,t}$			-0.404 (0.431)
$X^3_{i,s.t} \times Exp_{i,s,t} \times AM_{s,t}$			-0.057 (0.153)
$X^3_{i,s.t} \times D_{i,s,t} \times Exp_{i,s,t} \times AM_{s,t}$			0.169 (0.508)
Intercept	6.442 (0.629)**	7.651 (0.972)**	7.725 (1.284)**
n (clusters)	901 (191)	901 (191)	901 (191)

Note: The entries denote OLS estimates, with standard errors clustered at the state-election level.*p<.10; **p<.05, two-tailed tests.

$$
\begin{aligned}
Y_{i,s,t+1} = \beta_0 &+ \beta_1 X^*_{i,s,t} + \beta_2 D_{i,s,t} + \beta_3 Year_t + \beta_4 AM_{s,t} \\
&+ \beta_5 X^*_{i,s,t} D_{i,s,t} + \beta_6 X^*_{i,s,t} Year_t + \beta_7 X^*_{i,s,t} AM_{s,t} \\
&+ \beta_8 D_{i,s,t} Year_t + \beta_9 D_{i,s,t} AM_{s,t} + \beta_{10} Year_t AM_{s,t} \\
&+ \beta_{11} X^*_{i,s,t} D_{i,s,t} Year_t + \beta_{12} X^*_{i,s,t} D_{i,s,t} AM_{s,t} \\
&+ \beta_{13} X^*_{i,s,t} Year_t AM_{s,t} + \beta_{14} D_{i,s,t} Year_t AM_{s,t} \\
&+ \beta_{15} X^*_{i,s,t} D_{i,s,t} Year_t AM_{s,t} + u_{ist}
\end{aligned}
$$

The second and third polynomial equations are simply expanded to add the higher order terms. The full results are shown in Table A.9. Figure A.8 visualizes the overall pattern. It shows how the predicted difference in the federal vote share of parties which just entered parliament versus those which were left out (thus, evaluating the treatment effect at $X_{i,s,t} = c$) has varied over time.[3] As shown in the first column of the Figure, among the set of observations with above-median distance between state and federal election, there seems to be a modest but positive time trend, with the parliamentary effects being more evident among recent elections. This conclusion, however, is qualified when looking at the higher polynomial specifications. Both the magnitude and the direction of the time-specific effects seem to be susceptible to the exact model specification. Taken as a whole, these findings do not allow out-of-sample generalizations. The evidence for a significant time trend in the effects seems to gain little support from this analysis.

[3] In particular, for observations with below-median distance between state and federal election, the treatment effects are given by: $\beta_2 + \beta_8 Year_t$; for observations above median distance, average conditional treatment effects, given the year of the election, are given by: $\beta_2 + \beta_8 Year_t + \beta_9 + \beta_{14} Year_t$. To be sure, the pattern is only a linear approximation of the underlying time-specific variation in the effect of parliamentary representation. Allowing quadratic trends might well qualify this picture. We have avoided higher-order time-trends because we do not have a strong theory as to why such non-linear effects might have emerged. We thus opted for a simpler specification, which is already very data demanding. Adding higher-order time trends would necessitate more information than is currently available in this data and would thus render the estimation uninformative.

Table A.9: Examining the Variation in the Magnitude of the Treatment Effects Over Time Using a Linear Time Trend.

	One Polynomial	Two Polynomials	Three Polynomials
$X_{i,s.t}$	7.795 (19.31)	59.00 (58.22)	-58.55 (196.8)
$D_{i,s,t}$	-15.98 (117.3)	-138.2 (146.1)	134.8 (173.1)
$Year_t$	-0.008 (0.043)	-0.024 (0.057)	0.000 (0.052)
$AM_{s,t}$	-64.01 (96.03)	-49.52 (140.7)	38.09 (168.6)
$X_{i,s.t} \times D_{i,s,t}$	-2.925 (39.31)	44.68 (119.8)	-330.3 (335.7)
$X_{i,s.t} \times Year_t$	-0.003 (0.010)	-0.028 (0.029)	0.030 (0.099)
$X_{i,s.t} \times AM_{s,t}$	-15.25 (21.79)	-21.24 (79.33)	169.7 (257.2)
$D_{i,s,t} \times Year_t$	0.008 (0.059)	0.069 (0.073)	-0.068 (0.087)
$D_{i,s,t} \times AM_{s,t}$	-33.91 (145.6)	223.2 (213.2)	-198.1 (281.5)
$Year_t \times AM_{s,t}$	0.032 (0.048)	0.024 (0.071)	-0.020 (0.085)
$X_{i,s.t} \times D_{i,s,t} \times Year_t$	0.001 (.020)	-0.023 (0.060)	0.166 (0.168)
$X_{i,s.t} \times D_{i,s,t} \times AM_{s,t}$	20.25 (51.39)	-296.9 (166.3)*	264.2 (530.8)
$D_{i,s,t} \times Year_t \times AM_{s,t}$	0.018 (0.073)	-0.110 (0.107)	0.101 (0.141)
$X_{i,s.t} \times Year_t \times AM_{s,t}$	0.007 (0.011)	0.010 (0.040)	-0.085 (0.129)
$X_{i,s.t} \times D_{i,s,t} \times Year_t \times AM_{s,t}$	-0.010 (0.026)	0.150 (0.083)*	-0.135 (0.266)
$X^2_{i,s.t}$		9.576 (8.516)	-44.32 (88.98)
$X^2_{i,s.t} \times D_{i,s,t}$		-28.54 (21.67)	264.4 (170.3)
$X^2_{i,s.t} \times Year_t$		-0.005 (0.004)	0.022 (0.045)
$X^2_{i,s.t} \times AM_{s,t}$		-1.810 (11.67)	83.83 (107.5)
$X^2_{i,s.t} \times D_{i,s,t} \times Year_t$		0.014 (0.011)	-0.133 (0.085)
$X^2_{i,s.t} \times D_{i,s,t} \times AM_{s,t}$		67.20 (33.04)**	-389.4 (254.8)
$X^2_{i,s.t} \times Year_t \times AM_{s,t}$		0.001 (0.006)	-0.042 (0.054)
$X^2_{i,s.t} \times D_{i,s,t} \times Year_t \times AM_{s,t}$		-0.034 (0.017)**	0.197 (0.128)
$X^3_{i,s.t}$			-6.626 (10.57)
$X^3_{i,s.t} \times D_{i,s,t}$			-24.72 (21.30)
$X^3_{i,s.t} \times Year_t$			0.003 (0.005)
$X^3_{i,s.t} \times AM_{s,t}$			10.42 (12.46)
$X^3_{i,s.t} \times D_{i,s,t} \times Year_t$			0.012 (0.011)
$X^3_{i,s.t} \times D_{i,s,t} \times AM_{s,t}$			38.34 (34.12)
$X^3_{i,s.t} \times Year_t \times AM_{s,t}$			-0.005 (0.006)
$X^3_{i,s.t} \times D_{i,s,t} \times Year_t \times AM_{s,t}$			-0.019 (0.017)
Intercept	22.51 (85.70)	56.06 (113.5)	6.869 (103.3)
n (clusters)	901 (191)	901 (191)	901 (191)

Note: The entries denote OLS estimates, with standard errors are clustered at the state-election level.*p<.10; **p<.05, two-tailed tests.

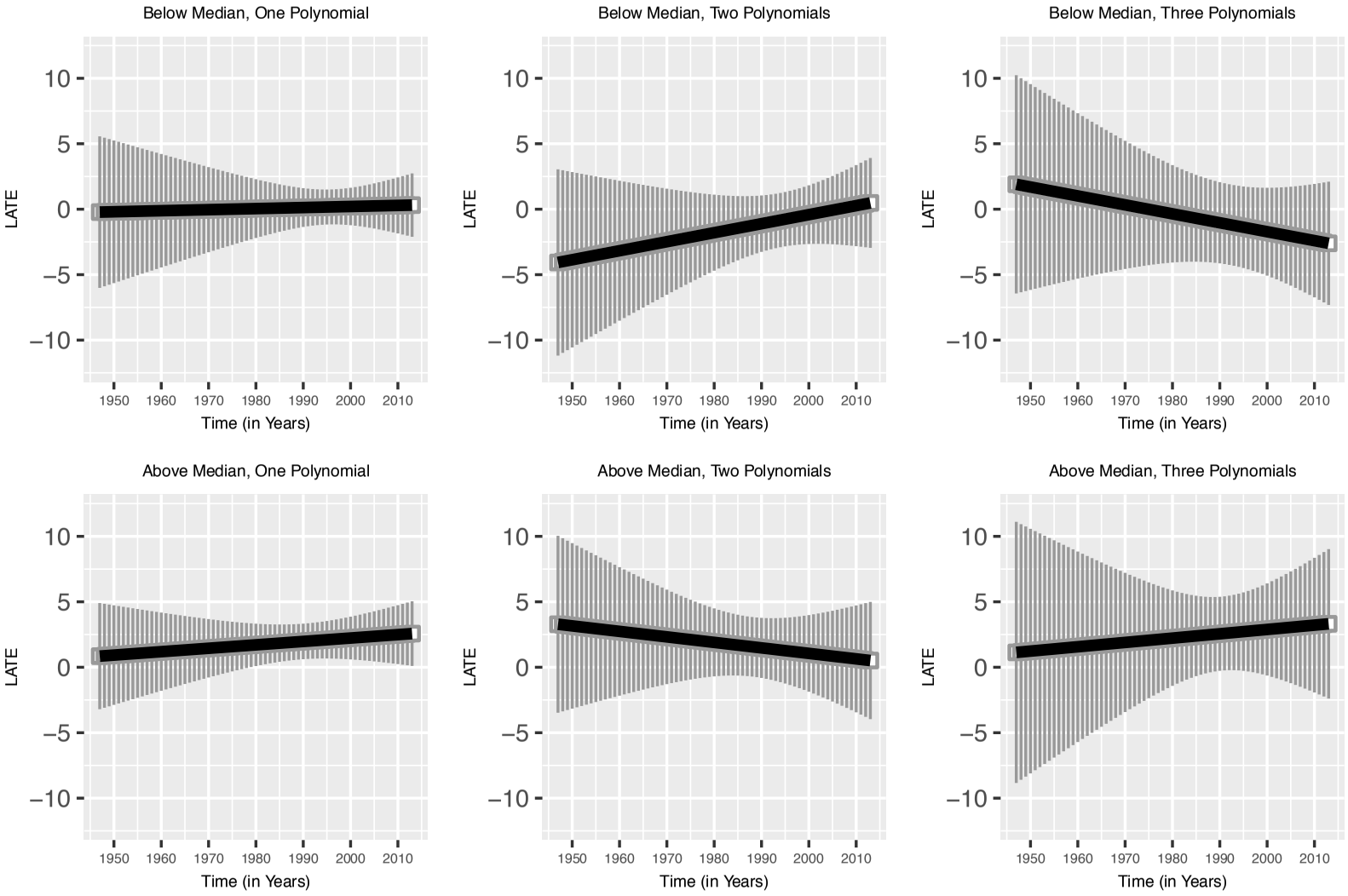
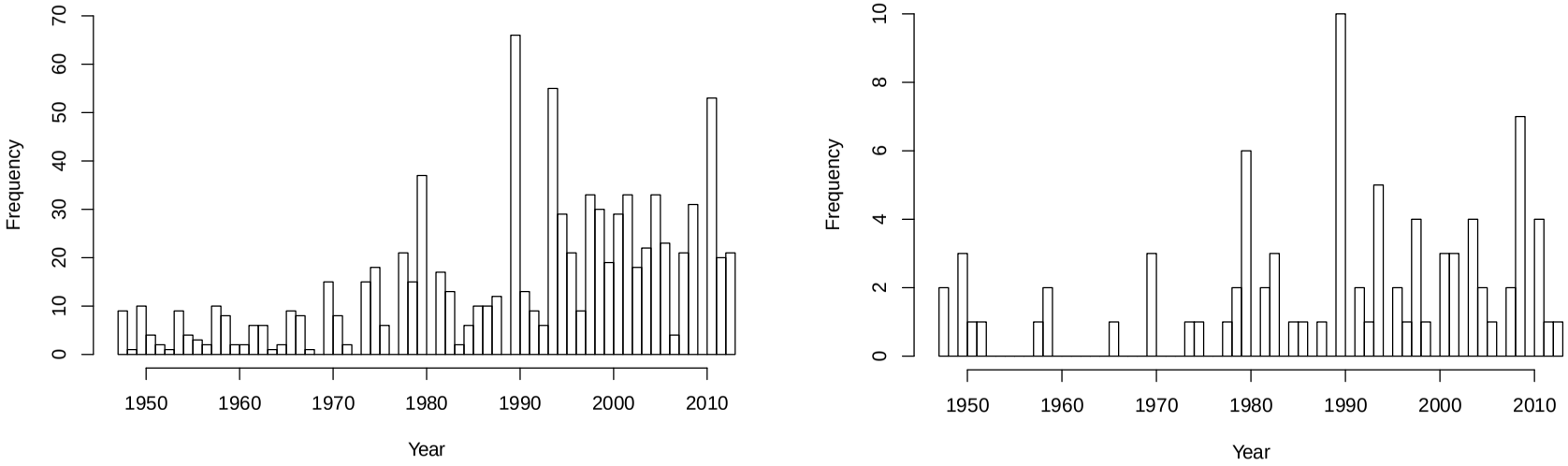


Figure A.8: Over-time Change in the Effect of Parliamentary Representation on Federal Vote Share.

Note: The solid black line denotes the difference in the vote share of parties that enter state parliament compared to parties without regional parliamentary representation, across the time period 1947-2013. The vertical grey lines map the 95% confidence intervals. Estimation draws on the procedure outlined in the Appendix.

Table A.10: Parties included in analysis

	5.0% window		4.0% window		0.7% window	
	Partyname	N	Partyname	N	Partyname	N
1	FDP	169	FDP	158	FDP	27
2	Grune	106	Grune	99	Grune	24
3	NPD	102	REP	52	NPD	5
4	REP	77	NPD	42	KPD	5
5	DRP/DKP	53	DRP/DKP	16	Die Linke	4
6	ODP	52	Die Linke	15	REP	4
7	Graue	34	Piraten	13	DVU	2
8	PBC	29	ODP	9	DRP/DKP	1
9	BUSO	21	GB/BHE	8	GB/BHE	1
10	EAP	16	KPD	8	SSW	1
11	Piraten	16	DFU	7		
12	Die Linke	15	Graue	6		
13	Die Tierschutzpartei	15	DVU	5		
14	KPD	13	Freie Wahler	5		
15	Naturgesetz	12	DSU	4		
16	KBW	12	Familie	4		
17	Familie	11	PDS	4		
18	Freie Waehler	11	BP	3		
19	PDS	9	DP	3		
20	BP	8	SSW	3		
21	CM	8	AfD	1		
22	DFU	8	ASD	1		
23	Zentrum	8	BdD	1		
24	GB/BHE	7	BU90/Gr/UFV	1		
25	BdD	6	Forum	1		
26	DVU	6	Pro	1		
27	DG	5	Pro DM	1		
28	DSU	5	Schill	1		
29	FSU	5	Zentrum	1		
30	Die Frauen	4	50 Plus	1		
31	Die Partei	4				
32	SSW	4				
33	BIG	3				
34	MLDP	3				
35	Partioten	3				
36	UAP	3				
37	C.B.V.	2				
38	HP	2				
39	LIGA	2				
40	Pro DM	2				
41	PSG	2				
42	RRP	2				
43	AfD	1				
44	APD	1				
45	APPD	1				
46	ASD	1				
47	AUD	1				
48	BFB-Die Offensive	1				
49	BU90/Gr/UFV	1				
50	BWK	1				
51	CDU	1				
52	DPD	1				
53	Deutschland	1				
54	Die Violetten	1				
55	Forum	1				
56	Frauenliste	1				
57	Mundige Burger	1				
58	Oko	1				
59	Partei der Vernunft	1				
60	PASS	1				
61	PRO	1				
62	Rentner	1				
63	SPD	1				
64	STATT Partei	1				
65	Schill	1				
66	Volksabstimmung	1				
67	50 Plus	1				
N		905		474		80



(a) 5% window

(b) 0.7% window

Figure A.9: Number of cases over time

References

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