

# The Political Repercussions of Cutting Place-Based Compensation Policies

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## Abstract

*Across economies in the OECD, regional disparities within countries are growing. Governments frequently seek to address these disparities with place-based compensation policies. Yet, we know little about the long-term political and economic consequences of cutting back economic subsidies that are often part of place-based policies. This is surprising, as the last decades have to a large extent been shaped by a decline in the funds governments provides to less developed regions. In this paper we study the political repercussions of cutting a large-scale place-based policy in Germany intended to support municipalities at the Iron Curtain. Using difference-in-differences and regression discontinuity designs, we find that cutting place-based policies decreases turnout in affected regions by about one percentage point. This demobilization effect is not accompanied by decreasing voting for mainstream parties but might in the long-run result in increasing votes for parties at the periphery. Our findings thus show that prematurely cutting regional assistance to deprived regions has long-term detrimental political effects. This has implications for our understanding of the electoral effects of place-based policies as well as the extent to which incumbents can manage the political consequences of lasting structural change in the economy.*

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## 1 Introduction

The deepening of regional economic disparities constitutes one of the fundamental trends in the political economy of many developed countries in recent years (Moretti 2012; Diamond 2016; Odendahl et al. 2019). Today, variation of economic conditions across regions, cities and neighborhoods within in the same country often even exceed differences across national borders (OECD 2016). These disparities result in significant regional differences in income, unemployment, productivity and the provision of local public goods (Feler and Senses 2017; Autor, Dorn, and Hanson 2016). Moreover, regional economic disparities are likely to result in severe longterm consequences far beyond personal job losses such as property value decline, emigration waves and the erosion of social structures (e.g., Broz, Frieden, and Weymouth 2019: 3). Citizens living in regions suffering from long-term economic distress are, in addition, more likely to develop (cultural) grievances and negative judgments about their community’s political representation (Carreras, Irepoglu Carreras, and Bowler 2019; McKay 2019). Accordingly, conditions in regions with declining economic fortunes are often considered to be an important breeding ground for political resentment, dealignment and the recent rise of populist right-wing parties (Broz, Frieden, and Weymouth 2019; Colantone and Stanig 2018a,b).

The question how to address the decline and target economic development in left-behind regions via so called *place-based* policies, therefore, ranks high on the political agenda. For instance, from 2007 to 2013 the European Union (EU) redistributed 277 billion Euros through its European Regional Development Fund to regions whose GDP per capita was less than 75% of the EU average (Barone, David, and de Blasio 2016: 31); the Regional Selective Assistance program in the United Kingdom subsidizes firms in regions characterized by lower GDP per capita and high unemployment (Criscuolo et al. 2019: 51-52); and similar policies are either in place or heavily debated across Europe and the United States (Rodríguez-Pose 2018). However, we know surprisingly little about the economic and in particular the political consequences of such policies (Kline 2010; Busso, Gregory, and Kline 2013; Criscuolo et al. 2019). What are the short, medium, and long-term political consequences of place-based policies for electoral outcomes?

## 1 Introduction

We study the most extensive place-based policy program—the *Zonenrandgebiet* (ZRG) in Germany—to answer this question. After the end of World War II and following Germany’s division into East and West Germany, the federal government of West-Germany implemented a large-scale funding program to economically support districts close to the Iron Curtain. The program was supported by a broad coalition of all mainstream parties and meant to address the structural disadvantage resulting from the inner German boarder (Seidel and Von Ehrlich 2018). With the relatively sudden reunification of Germany in 1990 (Redding and Sturm 2008), the German government put the focus of its regional policy on the newly joined states in the East and ended its funding for several parts of the former ZRG while other regions continued to receive funding. We exploit this change in funding eligibility in a difference-in-differences and regression discontinuity design. Comparing economically struggling municipalities that were continuously supported with those that were not.

Reviewing different theoretical perspectives, cutting place-based funding may impact election outcomes both through its effect on vote choice and on political participation. Yet, clear theoretical predictions about which scenario is more likely to materialize are surprisingly blunt (Margalit 2019: 280-281) — also because both effects may occur simultaneously. Taking into account the context of our study, we conclude that based on available theoretical work on retrospective voting we would expect all mainstream parties to lose support in elections. Likewise, while there are reasons to expect that periphery parties at the left or right profit electorally from this anti-mainstream effect, it is equally plausible that (some) voter retreat politics and abstain which then reduces local turnout.

Empirically, we find that municipalities losing eligibility experience a one percentage point drop in turnout at federal elections. This decrease in mobilization remains persistent throughout the entire study period of 23 years. We do not find that demobilization was differential but rather uniform across parties. Only in the most recent election in 2017, we observe an increase of voters in municipalities that previously lost funding eligibility beginning to vote for smaller, challenger parties such as the radical right party, Alternative für Deutschland (AfD) and THE LINKE, at the expense of the remaining

mainstream parties – such as Angela Merkel’s Christian Democratic Union (CDU) or the Social Democrats (SPD). This effect is, however, not significant on a conventional level of significance. Using information about the number of jobs secured by the funding provided allows us to estimate the ratio between people losing their job and the decrease of turnout. We find that for every unemployed voter another voter does not turn out on election day; which essentially comes down to one household not turning out. This speaks for a rather *direct* effect of job loss on political de-mobilization with people who lost jobs being de-mobilized.

Our findings contribute to existing research in a number of ways. A first empirical contribution relates to a small but growing literature on the political consequences of regional development funds. Much of this literature so far focuses on the role of the EU’s Regional Development Fund and finds that support for Eurosceptic parties tends to be lower in regions which receive economic support from the EU (Schraff 2019; Patana 2018; Borin, Macchi, and Mancini 2018; Bachtrögl and Oberhofer 2018). Ropers (2018) finds similar effects on the national level studying the relationship between regional assistance funds in Germany and voting for the radical right party AfD. We add to this literature by studying the longterm political effects of place-based policies and by highlighting that even 23 years after the loss of funding, turnout is still depreciated. The suggestive effect for the increase in periphery-party support is consistent with other studies showing that globalization shocks over the long-run drove people to vote for Eurosceptic parties (Colantone and Stanig 2018b) and to support Brexit in the United Kingdom (Colantone and Stanig 2018a).

Second, we are not aware of any study exploring the consequences of governments *ceasing* to fund economically struggling regions. Whereas much of the current debate focuses on ways to increase resources for regional development, empirically, the last decades have to a large degree been shaped by a decline in the funds governments provided to less developed regions (OECD 2018). Ultimately, this development raises the question on the political and economic consequences of funding withdrawal. Thus, our study adds an important piece to the politics of resentment that we witness in many left-behind regions.

In addition, our findings are also instructive for policy-makers. Given that regional investment funds are costly endeavors, financially constraint policymakers have to trade-off the benefits of using money for other purposes against the political and economic repercussions of exiting existing programs. Currently, this question, for example, arises most prominently in the context of Brexit. What happens once the UK is no longer member of the European Union and the left behind regions in the UK lose their eligibility to access the EU regional funding? Given our findings such a immediate cut from funding might foster support for right-wing challenger parties while also leading to decreasing turnout in elections.

Third, our study also adds additional insights for the debate about the economic and political drivers of political dealignment (Colantone and Stanig 2018a,b; Broz, Frieden, and Weymouth 2019; Cramer 2012). We show that a loss of government support and its economic consequences depress turnout in economically deprived regions and that this appears to be an immediate effect of job loss. This finding is consistent with a re-emerging strand of studies that emphasizes the importance of the local rather than the national economic context for voting (e.g., Healy and Lenz 2017; Larsen et al. 2019). Yet, our result suggest that observing economic decline — at least in the absence of a credible challenger party — was not sufficient to make voters abstain in the German case we study. Instead voters needed to be directly effect by funding cuts to be de-mobilized. x

## 2 The Case of the ZRG

Even though place-based policies may differ in form and scope, they typically are designed to encourage economic development in lagging and disadvantaged areas to address structural and persistent regional differences in economic activity (Kline and Moretti 2013). Well-known examples of place-based assistance programs include enterprise zones in the U.S. (Busso, Gregory, and Kline 2013), the Structural Funds in the European Union (Becker, Egger, and Von Ehrlich 2013), and national state aid programs like the Regional Selective Assistance in the United Kingdom (cf. Neumark and Simpson 2015).

One of the longest supported and most consistently defined assisted areas in Western

advanced democracies was the so-called *Zonenrandgebiet (ZRG) (Border Zone)* in former West Germany (Wild and Jones 1993). After World War II, the geopolitical tensions between the Western Allies that occupied West Germany and the Soviet Union in East Germany, led to the division of Germany into two countries in 1949, and the fortification of the border three years later in 1952. The closing made exchange of goods and people between the border regions impossible resulting in structural problems for the affected communities. Until then, border regions in the West had shared common regional markets with their neighboring regions in the East and exported up to 50% of their goods to the East (Kiemstedt 1970: 3871). As both an economic and political response to this transformation into the periphery of the Western world, counties along a well-defined strip along the Western side of the border were to continuously receive government assistance for almost 40 years. Only the relative unexpected reunification of West and East Germany in 1989/90 re-shifted the focus of regional policy in Germany to regions in the former German Democratic Republic that became part of the reunified country.

Assistance for the strip of counties that formed the *ZRG* started as early as 1953 through various administrative directives and tax incentives. Already then, the *ZRG* was defined to compose of counties that either lay with more than 50% of their area or accommodated more than 50% of their population within a 40 km distance to the border (Kiemstedt 1970: 3871).<sup>1</sup> In 1971, the special status and the composition of the region was codified in the Border Zone Assistance Act (*Zonenrandförderungsgesetz*) providing the subsequent legal basis for the assistance of the *ZRG*. In total, the *ZRG* consisted of at the time 79 rural counties (*Landkreise*) and 25 urban districts (*Stadtkreise*) in the four states of Schleswig-Holstein, Lower-Saxony, Hesse, and Bavaria covering approximately 19% of the West-German territory and affecting 12% of the total population.

In order to compensate for the structural problems in the border regions, the *Border Zone Assistance Act* stipulated various forms of federal government aid to improve economic activity. The main instruments were investment subsidies and tax incentives to firms as well as support for public investments in infrastructure for municipalities and

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<sup>1</sup>Earlier into the project we also exploit this discontinuity using a (geographic) regression discontinuity but we found that there is a large imbalance between municipalities to the left and right of the cutoff.

counties. Throughout the existence of the *ZRG*, between 1.3-2.5 billion DM (2010 prices) of subsidies were paid out until 1990 (Seidel and Von Ehrlich 2018: 349).

The fall of the Berlin Wall in November 1989 and the formal unification less than a year later led to a rapid reshaping of regional policy in Germany. On June 10, 1991, about half a year after unification, the new regional policy was issued. As East Germany suffered from much more severe structural economic problems than most regions in West Germany, the entire territory of East Germany became eligible for funding. Yet, given limited resources, some regions in West Germany that used to be eligible for funding were no longer supported.

To determine eligibility for funding of West Germany's regions, the government used a ranking of the 167 West German labor market regions according to their economic performance in previous years. Importantly, the ranking was based on the structural development of each labor market and not whether or not it located inside or outside the *ZRG*.<sup>2</sup>

The ranking led to the loss of eligibility for funding in 13 labor market regions in the *ZRG* from October, 1 1991 onwards and it reaffirmed the eligibility of the other 23 labor market regions in the *ZRG*.<sup>3</sup> Figure 1 provides a map of the *ZRG* which shows the eligibility status on the municipality level according to the ranking after October 1, 1991.

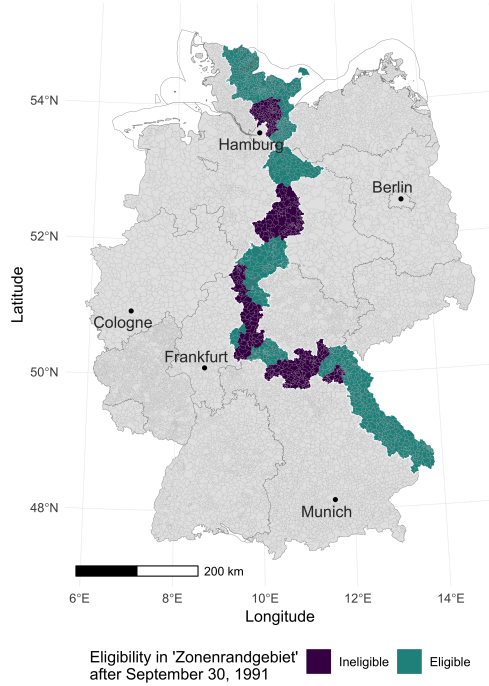
The loss of assistance for areas in the *ZRG* provides an opportunity to examine how the removal of funding shapes electoral outcomes. Three features of our case facilitate our empirical strategy that we discuss in more detail below. First, the relative sudden withdrawal of funding eligibility due to the unification gave local politicians, firms and the voters little time to adapt to the loss of funding and, thus, mitigates concerns regarding anticipatory actions. Second, the availability of election outcomes for four pre-treatment elections allows us to account for pre-existing difference between places that eventually lost funding eligibility and those that remained eligible. Third, the loss in eligibility resulted

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<sup>2</sup>The index was constructed by the Institute for Employment (IAB) and published in January 1991 (Blien, Koller, and Schiebel 1991). The index consists of the weighted product of annual yearly gross income per employee in 1988 (40%), the average unemployment rate in 1987 and 1990 (40%), an infrastructural indicator (10%), and an employment forecast (10%).

<sup>3</sup>The full removal of the "assisted status" was only completed at the end of 1994. Before that, tax deductions for firms located in the *ZRG* remained in place until the end of 1994 (Bundestag 1991).

### 3 Theoretical Perspectives



**Figure 1:** Eligibility in the *Zonenrandgebiet* after September 30, 1991. After the unification of West and East Germany, 488 municipalities in the *Zonenrandgebiet* lost eligibility for federal subsidies.

from a ranking designed in a bureaucratic process with little influence by politicians. Fourth, with the sudden loss in funding eligibility in 1991, we can draw on a long time series of electoral outcomes to evaluate the long-term consequences of withdrawing funding instead of only studying the short-term effects.

### 3 Theoretical Perspectives

How do funding cuts for place-based policies such as the ZRG affect voters' electoral behavior? We follow the literature on political effects of economic shocks (cf. Margalit 2019) and emphasize the negative economic effects of funding cuts to provide sense to this question.<sup>4</sup> Specifically, standard models in economics suggest two separate economic effects of funding cuts at the local level (cf. Kline and Moretti 2014). On the one hand, we would expect a decrease in real wages to the extent that investments and tax incentives

<sup>4</sup>Another possible theoretical mechanism builds on the literature in distributive politics and emphasizes the direct decrease of spending on public goods and services from funding cuts such as, for example, infrastructure investments. Golden and Min (2013) conclude that the vast majority of studies in different institutional and cultural contexts find that incumbents are rewarded by voters for distributing goods and services. Of course, this logic requires a relatively high degree of sophistication among voters as they need to know about the funding cuts. That being said, if voters learn about the cuts, then it also possible that incumbents will suffer electorally — independent of the economic effects.



### 3 Theoretical Perspectives

for firms are functional equivalents to wage subsidies. On the other hand, with labor-market imperfections, there are also good reasons to expect that cuts result in an increase of unemployment. Theories of economic voting postulate that these changes in local economic outcomes, local decline in wages and increases in unemployment, can affect whether (Shah and Wichowsky 2018) and how voters cast their vote at the ballot box (Wright 2012; Hansford and Gomez 2015; Healy and Lenz 2017; Larsen et al. 2019). Yet, somewhat surprisingly we lack models of voting that help us to predict when and which voters will react one way instead of the other.

With respect to voting, we can differentiate a direct mechanism and an indirect mechanism for the effects of cutting place-based policy. First, theoretical models of pocketbook voting would lead us to expect that voters directly affected by the negative economic consequences resent their support for the incumbent (Lewis-Beck and Paldam 2000; Duch and Stevenson 2008; Healy, Persson, and Snowberg 2017). Second, arguments about sociotropic voting suggest that voters not only consider their own economic situation but also that of their local social network (Wright 2012; Hansford and Gomez 2015; Healy and Lenz 2017; Larsen et al. 2019). If so, funding cuts affect a much broader part of the local electorate and not only those voters that were directly affected.

In line with models of retrospective voting, both preceding arguments rest on the assumption that voters blame the incumbent for bad economic performance (Key 1966; Fiorina 1981; Ferejohn 1986; Healy and Malhotra 2013). In the context of our study, however, the cuts were supported by all mainstream parties as a necessary policy to complete Germany's unification including the largest opposition party, the Social Democrats (SPD 1990: 20) — even though responsibility clearly lay with the Christian Democrats and their coalition partner the Free Democrats, legally. Furthermore, in times of economic downturn and little financial leeway of governments a recent body of empirical research suggests that dissatisfied voters may generally be more likely to vote for anti-establishment, radical right parties with their strong nationalist appeal (Colantone and Stanig 2018a,b). Together, we expect that voters not only blame the incumbent for these cuts but all mainstream parties. Therefore, voters become more likely to vote for periph-

ery parties in locations which lose funding.

Beyond the effect on voting behavior, cutting regional funding may also affect electoral participation at the local level. Resource models of political participation suggest that voters who are directly affected by the cuts will have little resources to actively engage in and being informed about politics (Schlozman and Verba 1979; Rosenstone 1982; Radcliff 1992). Struggling economically, these voters might be more pre-occupied with securing their economic livelihood than participating in politics. For these voters, saving the cost of voting seems a sensible choice and we thus would expect a decrease in turnout among them. Moreover, direct individual effects may spill-over to local communities, for example, by lowering social capital and political efficacy in social networks (Shah and Wichowsky 2018). From this line of thinking, we expect that in localities cut-off from funding voters demobilize and overall turnout decreases as a result.

The previous discussion highlights, that while there is ample reason to expect that funding cuts will affect election outcomes, theoretical predictions about who loses and who profits in elections are surprisingly blunt (Margalit 2019: 280-281). One plausible hypothesis is that both scenarios materialize at the same time but for different types of voters — making it a question of empirical testing rather than of *ex ante* theoretical plausibility. That said, we use the preceding theoretical discussion to structure our empirical evaluation of the short, medium and long-term effects of these funding cuts and we discuss the implications of these results for the development of theory in the last section of this paper.

## 4 Data

For our empirical analysis we need information about which municipalities belonged to the ZRG. Therefore, we identified all 104 counties that are listed in the 1971 law that created the ZRG in a map of counties from the same year provided by the Mosaic project (MPIDR 2011; BKG 2011). We over-layed this map with a map of municipalities that existed in December 31, 2014 (BKG 2014) and identified the 1528 municipalities that used to belong to the ZRG. Afterwards, we coded which of these ZRG municipalities

## 4 Data

**Table 1:** Eligibility in municipalities in the 'Zonenrandgebiet' after September 30, 1991 as stipulated by a federal ranking of structural development of West German labor market regions.

	Lost eligibility (treated)	Remained eligible (control)
Schleswig-Holstein	150	551
Lower Saxony	113	114
Hesse	77	23
Bavaria	148	352
Sum	488	1040

lost funding in 1991 using the original ranking of the 167 West German labor markets (Bundestag 1991). In Table 1 we report the number of ZRG municipalities that lost/did not lose funding eligibility in 1991 by state.

In a second step, we collected election results for all federal elections between 1980 to 2017 (11 elections) for all ZRG municipalities. Unfortunately, municipality-level data before 1980 remains unavailable. In German federal elections, voters have two votes: a constituency and a list vote (*Erst- und Zweitstimme*). The constituency vote elects a constituency candidate by plurality vote while the list vote goes to a state party list. For the main analysis we focus on the list vote as only for this vote we have a full panel with all elections but we replicate the analysis with the constituency vote on the abbreviated panel in the SM.

Finally, we collected data on actual funding pay-out. This data comes from the *framework plans* (*Rahmenpläne*) of the *Joint Task Improvement of the Regional Economic Structure*. Until 2007, a government commission published these reports annually to report on the implementation of the government regional policy. The reports provide three pieces of information for each county (*Kreis*) in Germany: the number of retained and newly created jobs through funding, the amount of funding and the number of supported projects. Unfortunately, the amounts are not reported on an annual basis but usually cover periods of 2-3 years. In the analysis below, we merge this information with the federal election data using the midpoint of each period. Table A.1 in the appendix displays how reported periods correspond to election years.

## 5 Empirical Strategy

We use a difference-in-differences strategy to compare the average change in municipality vote shares (the outcome) in municipalities that lost eligibility (treatment group) to the change in vote shares in municipalities that have not lost eligibility (control group). The difference between these two changes identifies the average treatment effect on the treated (ATT). The identifying assumption of this design is that the outcomes in municipalities in the treatment group had, on average, followed the same trend as municipalities in the control group in the absence of losing eligibility for funding. While the parallel-trends assumption is not directly testable – we observe each municipality either as having lost funding or remaining eligible for funding – we can test if treated and untreated municipalities follow similar trends *before* the treatment occurs. The presence of parallel trends in the pre-treatment period tends to lend credibility to the identifying assumption that in the absence of treatment treated and untreated counties would have followed similar patterns.

Our estimation sample is a balanced panel of the 1528 municipalities located in the ZRG minus the 231 municipalities located in 12 Bavarian counties at the border to the Czech Republic. We found that the municipalities in these counties have very different pre-treatment trends as compared to all other ZRG municipalities. As none of these municipalities did lose eligibility for funding (see also Figure 1) we excluded them from the analysis.

We implement the difference-in-differences strategy using a fixed-effect estimator controlling for municipality and state-election fixed effects. We use state-specific election fixed effects (instead of election fixed effects) as this leads to better parallel trends in the pre-treatment period. We substantiate our identifying assumption by constructing a specification that includes a saturated set of lags and leads for the treatment indicator. This event-study design allows us to assess to what extent municipalities in treatment and control followed parallel trends in years before the actual loss and assess the dynamics of the treatment effect following the loss (e.g., Angrist and Pischke 2009: 237). Our specification takes the following form:

## 6 Results

$$Y_{it} = \alpha + \gamma_i + \gamma_{t,s} + \sum_{m=-3}^7 \beta_m D_{i,t-m} + \varepsilon_{it},$$

where  $Y_{it}$  is the vote share for some party and  $D_{i,t}$  is the treatment indicator that is coded as one for municipalities in election  $t$  after the municipality lost eligibility in 1992 and zero otherwise. The specification includes 3 lags and 7 leads of the treatment indicator such that the coefficients  $\beta_m$  measure the effects of losing funding eligibility in election one to seven after the loss (1994 to 2017) as well as placebo checks to test whether there are non-parallel pre-treatment trends in the three elections leading up to the loss (1984 to 1990).  $\gamma_i$  and  $\gamma_t$  are municipality and state-specific election year fixed effects. Throughout our analysis, we use cluster-robust standard errors on the level of labor market regions, the level of the treatment variation.<sup>5</sup>

## 6 Results

### 6.1 Economic effects

Before discussing the electoral effects of losing funding eligibility, we assess whether the loss of funding eligibility had any effect on the actual funding received and on the economic situation within communities. If the loss in funding eligibility would have not materialized in a loss of funding and/or had no effect on economic fundamentals, there is little reasons to expect any effects on political behavior. To that end, we examine three outcomes: The aggregated amount of subsidies to firms and infrastructure projects, jobs retained by the ZRG and unemployment.

Subsidies to firms are investment subsidies for projects concerned with the establishment, expansion or adaption of their business while subsidies to infrastructure projects

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<sup>5</sup>Our baseline design identifies the effect of loosing funding eligibility. Below we also assess how loosing eligibility for funding affects the actual funding received. To the extent that we are willing to assume that the only reason for why the loss of funding eligibility affects political outcomes is because of its effect on the funding received (exclusion restriction), we could identify and estimate the effect of loosing actual funding received via an instrumental variable estimation. To that end, we would rescale the estimated ATT using the first stage regression that measures how loosing eligibility for funding affects the actual funding received. We refrain from doing this as we find the exclusion restriction not necessarily credible.

## 6 Results

**Table 2:** First stage results: DID regression of subsidies received (binary)

	Subsidies received (Firms + infrastructure)	Subsidies received (Firms)	Subsidies received (Infrastructure)
1988-1992 (pre)	0.03 (0.03)	0.04 (0.11)	−0.00 (0.05)
1992-1995 (post)	−0.76*** (0.12)	−0.78*** (0.15)	−0.79*** (0.11)
1996-1998 (post)	−0.54** (0.17)	−0.42* (0.19)	−0.62*** (0.16)
1999-2001 (post)	−0.49** (0.17)	−0.47** (0.16)	−0.49*** (0.14)
2002-2004 (post)	−0.49** (0.18)	−0.35* (0.15)	−0.35* (0.14)
2005-2008 (post)	−0.50* (0.19)	−0.49** (0.16)	−0.35* (0.15)
Num. obs.	357	357	357

\*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ .

The dependent variable is coded as 1 if a county received any subsidies to firms and/or infrastructure projects. Includes county and period-state fixed effects. Standard errors clustered at labor market regions. Reference period: 1985-1989.

went to municipality and county governments. Our unit of analysis are counties since information on actual funding received is only available at this level. Table 2 shows the results when coding the outcome variables as 1 if a county received any subsidies. Before the cuts instituted, we observe no significant difference between counties that lost funding eligibility and counties which retained their eligibility eventually. The point estimates for total subsidies received, subsidies to firms and subsidies into infrastructure in the period 1988-1992 are all estimated to be zero. This adds credibility to our identification assumption that treated and control units followed a similar trend prior to losing funding eligibility.

Turning to the actual effects after the cuts had occurred, we find an immediate and statistically significant decrease in funding during the 1992-1995 period. The probability to receive any funding in the three years after 1991 was about 76 to 79 percentage points lower in counties that lost funding eligibility as compared to counties that retain their eligibility status. In other words, counties in the *ZRG* that lost funding eligibility – as a consequence of the restructuring of regional policy – also received less subsidies. Why did not all counties which lost eligibility also lose funding? One reason is that eligibility

was largely determined by the ranking (see above), but states were allowed to reinstate eligibility status for municipalities that faced particular structural problems such as mining areas (Bundesregierung 1991). The estimates also demonstrate that the decrease in funding is not only a short-term effect but that the loss of subsidies was permanent. The decay of the effect over time is due to the fact that some counties that lost (retained) eligibility in 1991 regained (lost) eligibility afterwards.

Second, we look into the amount of jobs retained by ZRG funding. To do so, we look at differences at the county level in the number of jobs retained and newly created from the provision of regional policy between those counties that lost funding eligibility after 1991 and those which remained eligible. Our data is not reported on an annual basis but for periods covering multiple years (see Table A.1 in the appendix). This data comes from firms that receive funding that are obliged by law to report how they use the funding including information about how many permanent jobs they create or retain due to receipt of funding. It is important to note that if they fail to create or retain these jobs, the administrative regulations require the government to reclaim the funding from the recipient firms. This means that firms have a strong incentive to report the jobs saved from funding as they would otherwise face funding cut-backs. Nevertheless, as these numbers are still reported by firms aiming to increase the likelihood to receive funding, they are likely to constitute an upper bound estimate of the direct labor market effect of regional funding.<sup>6</sup>

Table 3 shows period based effects on the number of jobs retained/created. Municipalities that lost funding after 1991 experienced on average both a 96 percent decrease in jobs that would have been secured (model 1) and a 97 percent decrease in jobs that would have been newly created through regional funding relative to the control municipalities which continued to receive funding after the reunification during the period 1992 to 1995 (model 2). In the third column, we also report the effect on the sum of retained and created jobs which is about 98 percent lower.<sup>7</sup> At the county level, this amounts to an

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<sup>6</sup>Moreover, the following estimates rest on the assumption that without the provision of regional funding, the reported retained and newly created permanent jobs would not have been saved or created, respectively.

<sup>7</sup>Since we here report the results of an OLS regression using a logged transformed dependent variable,

## 6 Results

**Table 3:** Difference-in-difference estimates with cluster-robust standard errors at the labor market regions. The dependent variable are the (log-scaled) number of secured long-term jobs, the (log-scaled) number of newly created long-term jobs, as well as their (log-scaled) sum. The treatment is the loss of eligibility for subsidies in September 1991 following revisions in the regional policy after unification. Reference period: 1985-1992.

	Retained jobs	Created jobs	Sum	Retained jobs	Created jobs	Sum
1988-1992 (pre)	0.33 (0.36)	0.45 (0.27)	0.48 (0.35)			
1992-1995 (post)	-3.26** (1.16)	-3.65*** (0.81)	-4.04*** (1.08)			
1996-1998 (post)	-2.24 (1.47)	-1.87 (1.08)	-2.47 (1.47)			
1999-2001 (post)	-2.59 (1.37)	-2.35* (0.96)	-2.92* (1.37)			
2002-2004 (post)	-1.46 (1.10)	-1.83* (0.83)	-1.98 (1.08)			
2005-2008 (post)	-3.86*** (1.12)	-2.97*** (0.84)	-3.70*** (1.10)			
1992-2008 (pooled)				-3.29 (1.73)	-3.38* (1.37)	-3.84* (1.73)
Num. obs.	357	357	357	102	102	102

\*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ .

average labor market loss of about 536 jobs. Interestingly, we find the strongest drop in retained jobs in the last period we analyze with our data (2005-2008) with a decrease of 98 % fewer retained jobs and 95 % fewer created jobs. This suggests that the economic consequences of funding loss are not only substantial but also persistent if not amplifying across time. In the last three columns we pool the effects for the entire pre- and post-treatment periods (model 4, 5, and 6).<sup>8</sup> In total, we find an overall decrease of 98% retained and newly created jobs compared to the control group.

The results on subsidies and jobs retained by the ZRG suggest that the restructuring of eligibility to regional policy that was a consequence of the German reunification implied not only a short-term economic decline for places in the *ZRG*, but that these effects remain persistent throughout time. Furthermore, we also estimate differences in the

the actual effect amounts to:  $(\exp(\beta) - 1) * 100$ .

<sup>8</sup>For this analysis, we first summarize the outcome values for the pre- and post-treatment period, before we run the pooled regression.



overall unemployment rate (Figure A.1 in the appendix)<sup>9</sup> They suggest that overall levels of unemployment increase shortly after regions are cut-off from funding.

## 6.2 Effects on Electoral Outcomes

Next, we discuss the electoral effects of revoking funding by looking at municipality level election outcomes. As explained we focus on vote shares from the list vote, but report the results based on the constituency vote in the SM (Table A.5). For our main analysis, we focus on the main outcomes shown as coefficient plots in Figure 2.<sup>10</sup> Our first outcome of interest is turnout measured as share of all votes relative to all eligible voters per municipality. Second, we look at the aggregated vote share of all mainstream parties. We include all parties that have been part of a federal government position during our period of study into this variable.<sup>11</sup> Third, we also look at the vote share of all other periphery parties including the socialist party, DIE LINKE, and the radical right-wing challenger party, AfD.<sup>12</sup>

Regarding the validity of our identification strategy, we observe statistically insignificant point estimates which are essentially centered around zero for all our outcomes in the pre-treatment period as shown in Figure 2. This lends credibility to our identifying assumption of parallel trends in our outcome measures between treatment and control group before the revoking of funding eligibility in 1991.

In contrast, immediately after the revoking of funding voters become less likely to turn out at the ballot box in the treated municipalities. Already in the first election after losing funding – 1994 – voters are about 1 percentage point less likely to take part in

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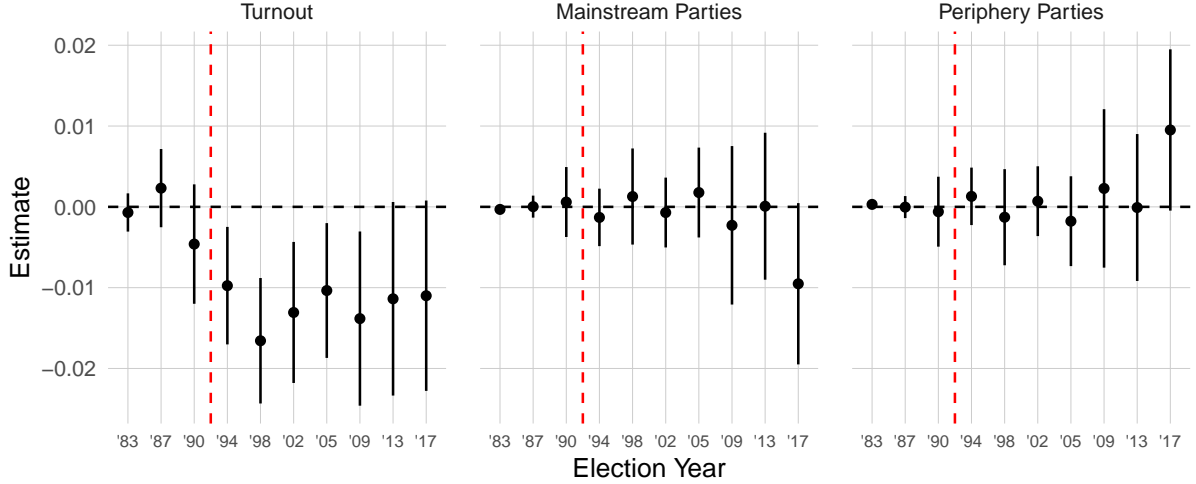
<sup>9</sup>Data come from Ronneberger and Schnapp (2008) for the period 1984-2004 and Bundesinstitut für Bau-, Stadt- und Raumforschung (2018) for 2005-2015. In our baseline DID model, the pre-treatment estimates suggest that counties followed a parallel trend in the pre-treatment period. Although not statistically significant, the post-treatment point estimates indicate an increase in the overall unemployment rate in the first three to five years after the loss of eligibility which, however, disappears in the long run. It provides further, suggestive evidence that unemployment rose stronger in counties which lost eligibility for funding compared to those which remained eligible after 1991.

<sup>10</sup>For the effect on all parties, see Table A.2 and Figure A.2 in the appendix.

<sup>11</sup>This includes the Christian-democratic party, CDU/CSU, the social-democratic party, SPD, the liberal party, FDP, and the Green Party.

<sup>12</sup>Both parties came into existence only after the reunification (1990 and 2013, respectively). Therefore, we do not observe election results for these parties prior to our treatment which is why we cannot analyze these parties separately.

## 6 Results



**Figure 2:** Difference-in-difference estimates with cluster-robust standard errors at the labor market regions. The dependent variable are vote share for all federal elections since 1980. The treatment is the loss of eligibility for subsidies in September 1991 (red dashed line) following revisions in the regional policy after unification. Reference period: 1980. The vote shares for mainstream parties include the votes for CDU, SPD, FDP, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.

federal elections in the treatment group. This effect becomes almost doubles in the 1998 election (1.7 percentage points). What is interesting to note is that this demobilization effect remains persistent across time. Thus, we find support for the idea that economic hardship due to cuts in funding decreases political mobilization; less people turn out to vote. This is plausibly so because they are either directly affected by economic decline or observe the decline within their community.

Turning to the effects on mainstream and periphery party voting, we do not find any significant difference after the cut from funding. The point estimates for mainstream and periphery parties remain consistently close to zero, with the last election in 2017 being the only exception from this pattern. If anything we observe a decrease in voting for mainstream parties in the last federal election, which also brought about the entry of the radical right AfD into the German Bundestag. We find that the vote share for periphery parties was about 0.95 percentage points higher in treated regions in comparison to those municipalities which continued to receive funding (95% CI:-0.01; 1.95).

To address the question whether direct effects or observation of decline explain our findings we would need a individual panel data in the best case. Unfortunately such fine-

grained and long running panel data is not available.<sup>13</sup> In the absence of such data we use the economic results discussed above on job retainment through the ZRG and we put these numbers in relation to the de-mobilization effect – one percentage point decrease in turnout – from the main analysis reported above. We find that for every employee that lost his or her job due to revoking funding (and therefore did not vote), no more than one additional voter did not turn out to vote. Aggregated over the entire post-treatment period (1992-2008), it similarly amounts to a job loss vs. demobilization ratio of about 2.<sup>14</sup>

These findings speak to a key debate in the current scholarly literature on job loss and political behavior, namely whether being directly affected by job loss explains demobilization patterns or whether observing economic decline in the local community drive economic voting patterns (Ansolabehere, Meredith, and Snowberg 2014; Larsen et al. 2019). Our calculation above suggests that the effect from losing ZRG funding appears to be mainly a direct effect as for each lost job two persons do not turn out at the election. Presumably this could be the one person who lost the job along another person within the same household. Of course we cannot test this assumption given that such fine-grained individual level panel data is not available. But the sheer numbers of the effect leave little room for an indirect effect driven by people who observe local economic decline but are not themselves affected by it. With respect to the (suggestive) increase in periphery party vote shares in 2017, conclusions regarding the underlying mechanism are even more speculative. Yet, as we simultaneously still observe the negative turnout effect also documented in the years before, we are more inclined to interpret this effect as an indirect one. Put differently, it seems more likely to conclude that it is not the voters who were directly affected by the funding cuts and who abstained in previous elections but voters that are effected more indirectly by the negative economic effects of funding

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<sup>13</sup>The best possibility in the German case would be to use the Socio-Economic Panel (GSOEP). However, given that SOEP is only representative on the regional level and due to panel mortality using this data would result in an underpowered analyses at best.

<sup>14</sup>Recall that the demobilization effect on turnout amounts to a 1 percentage point drop in turnout. This is equivalent to an average decrease of 870 voters per county. Our calculations in Table 3 suggest an average county labor market loss of about 536 positions during the first post-treatment period. This means that for each job lost no more than two individuals do not turn out to vote.

withdrawal who are mobilized to vote for periphery parties in 2017.

## Robustness

We conduct several robustness tests to rule out alternative explanations for our findings as well as threats to our causal identification strategy. First, the demobilization effect we report above are likely to be subject to some regional heterogeneity. Thus, we drop each state from the analysis and re-estimate the difference-in-differences estimator (Figure A.3 in the appendix). Here we find that the effects are most pronounced in Schleswig-Holstein.<sup>15</sup>

Second, one potential threat to our identification strategy is that there are differential trends in the labor market in the treated and control regions. For example, unemployment might decrease to a larger extent in regions that lose funding (relative to the decrease in unemployment in regions that keep funding), which means that the parallel trends assumption might be violated. To address this concern we take advantage of the deterministic assignment rule through the ranking used by the government to determine eligibility for funding. As described above, the government ranked the 167 West German labor market regions based on an index calculated as the weighted product of annual gross income per employee in 1988, the average unemployment rate in 1987 and 1990, an infrastructural indicator, and an employment forecast. All labor market regions greater or equal to an index value of about 93 lost funding. For the *ZRG*, this meant 13 labor market regions with 488 municipalities lost funding while 23 labor market regions with 1'040 municipalities did not. The specification below estimates the treatment effect at the cutoff akin to a local linear regression in a regression discontinuity design but additionally leverages pre-treatment observations to remove any time-constant differences between regions that lost funding and those that do not. The specification takes the following form:

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<sup>15</sup>Dropping Schleswig-Holstein from the analysis results in an increase of the confidence intervals. Yet, this effect is driven by issues of statistical power since 46 % of all municipalities in our sample are located in Schleswig-Holstein.

$$Y_{it} = \alpha + \gamma_i + \gamma_{t,s} + \sum_{m=-3}^7 \left( \beta_m D_{i,t-m} + \gamma_m D_{i,t-m} \cdot \text{runvar}_i \right) + \psi \text{runvar}_i + \varepsilon_{it}$$

where  $\text{runvar}_i$  is the ranking rescaled such that the cutoff is at zero (running variable). The estimates are shown in Figure A.4 and Table A.3 in the appendix. In this specification the identification strategy rests upon the idea that municipalities just below and just above the cutoff – the assignment rule outlined above – are quasi-randomly assigned to the treatment (losing funding). The point estimates from this regression tend to be similar compared to our main specification albeit standard errors tend to be larger.

Third, in our main DiD specification we rely on state-specific election fixed effects (instead of election fixed effects) as this tends to improve the parallel trends in the pre-treatment period. An alternative approach is to account for unit-specific time trends which allows treatment and control units to follow different trends (Angrist and Pischke 2009: p. 238). We do so by de-trending all outcomes for each municipality (including the previously excluded municipalities located at the Czech border) using the linear time trend we estimated for each municipality in the pre-treatment period (cf. Goodman-Bacon 2016).<sup>16</sup> We then estimate our main specification on the residualized outcomes. The advantage of this specification is that it allows for differential—albeit linear—pre-treatment trends. On the flip-side the standard errors are too small as we do not take into account the variation from the detrending. The results of this specification are shown in Figure A.5 and Table A.4 in the appendix. Again, our findings remain robust. Notice, however, that these models also suggest a negative effect for the CDU/CSU and a positive one for the SPD.

A fourth and final concern is that the results may largely be driven by sorting, for example, emigration of politically active citizens in the post-treatment period. To shed some suggestive light on this question, we replicate our main model specifications with (log-scaled) population size as dependent variable. Results can be found in Figure A.6 in

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<sup>16</sup>We do not include these time trends directly into the model as their slope would largely be informed by the post-treatment period (as we have many more post- and pre-treatment observations) and thus absorb time-varying treatment effects.

the appendix. According to our baseline model, there is no decrease in population size in the immediate years following the loss of funding. Instead, we observe an increasing trend in population size in the long run. Yet, the increasing trend disappears both statistically and in size once we try to account for differential trends with the DID-RDD specification using the labor market ranking as a running variable or the DID specification with unit-specific pre-treatment trends. Together, the results do not provide empirical support that the negative turnout effect may first and foremost be driven by sorting in the form of emigration of politically active citizens (or immigration of politically inactive citizens).

## 7 Conclusion

We studied the political repercussions of cutting a large-scale place-based policy in Germany. Using difference-in-difference and regression discontinuity designs, we find that cutting place-based policies decreases turnout during elections in affected regions by about one percentage point. Interestingly, this demobilization effect is not accompanied by decreasing voting for mainstream parties but might in the long-run result in increasing votes for parties at the periphery. Our findings thus show that prematurely cutting regional assistance to deprived regions has long-term detrimental political effects.

There are few studies seeking to understand the political effects of cutting place-based policies. This is surprising given the increased interest politicians recently have in establishing or extending such policies across Western democracies. Eventually politicians seek to cut place-based policies once economic grievances are addressed and, thus, it seems crucial to understand the political feedback mechanisms resulting from cutting funding from entire regions.

Furthermore, looking into the long term effects of place-based policies both economically as well as politically has rarely been done before (Bachtrögler and Oberhofer 2018). To our knowledge we are amongst the first to do so and we show that the longterm economical and political effects are severe; in affected regions, voters de-mobilize and tend to turn to periphery parties in the long run. In contrast to previous studies on the effects of place-based policies we study the entire set of political outcomes (Margalit 2019). We

show that by doing so interesting patterns emerge which frequently remained undiscovered in previous research which tends to look only into periphery party voting only. In our cases the de-mobilization effect appears to be the key take-away when studying the political effects of cutting place-based policies. In addition, while we find some suggestive evidence that challenger parties benefit electorally in places where funding is cut, these effects seem to materialize only in the very long run and could be driven by voters who are not directly affected from economic decline. This interpretation would be in line with recent studies that emphasize that voters who are only indirectly affected by economic hardship are especially likely to turn to challenger parties (e.g. Kurer 2019; Ansell 2017).

Our findings, thus, contribute to the literature in several ways. First, we add further evidence to the idea that local economic conditions drive political behavior in communities. Second, to our knowledge our study is the first one to research the economic and political effects of cutting place-based policies. Third and finally, we add to a growing body of research investigating the causes of political dealignment (Colantone and Stanig 2018a,b; Broz, Frieden, and Weymouth 2019; Cramer 2012). In fact, we show that governments need to think carefully when and how to cut place-based policies.

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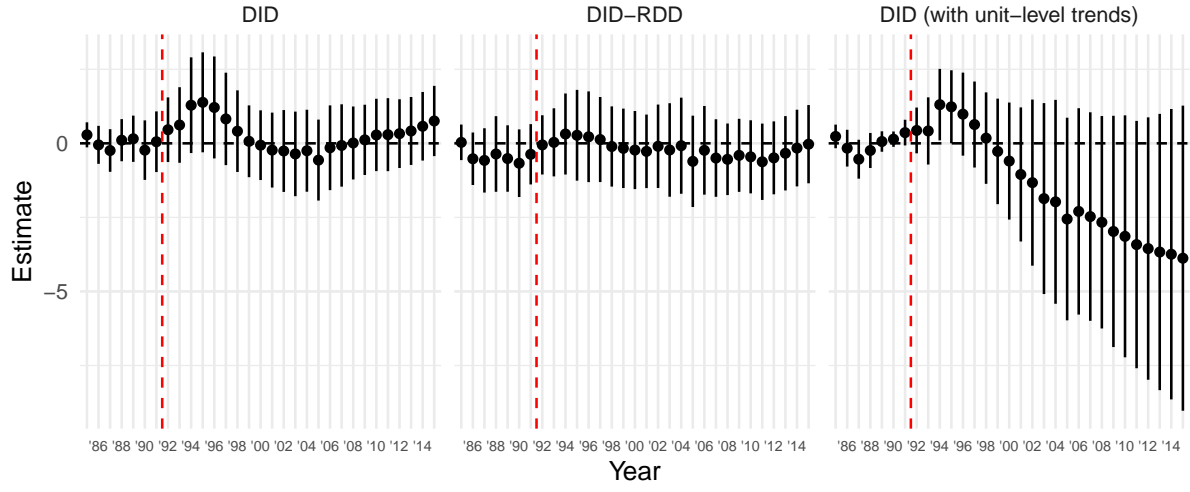
## A Appendix

## Data

**Table A.1:** Data sources and periods covered on actual funding received and retained or newly created jobs

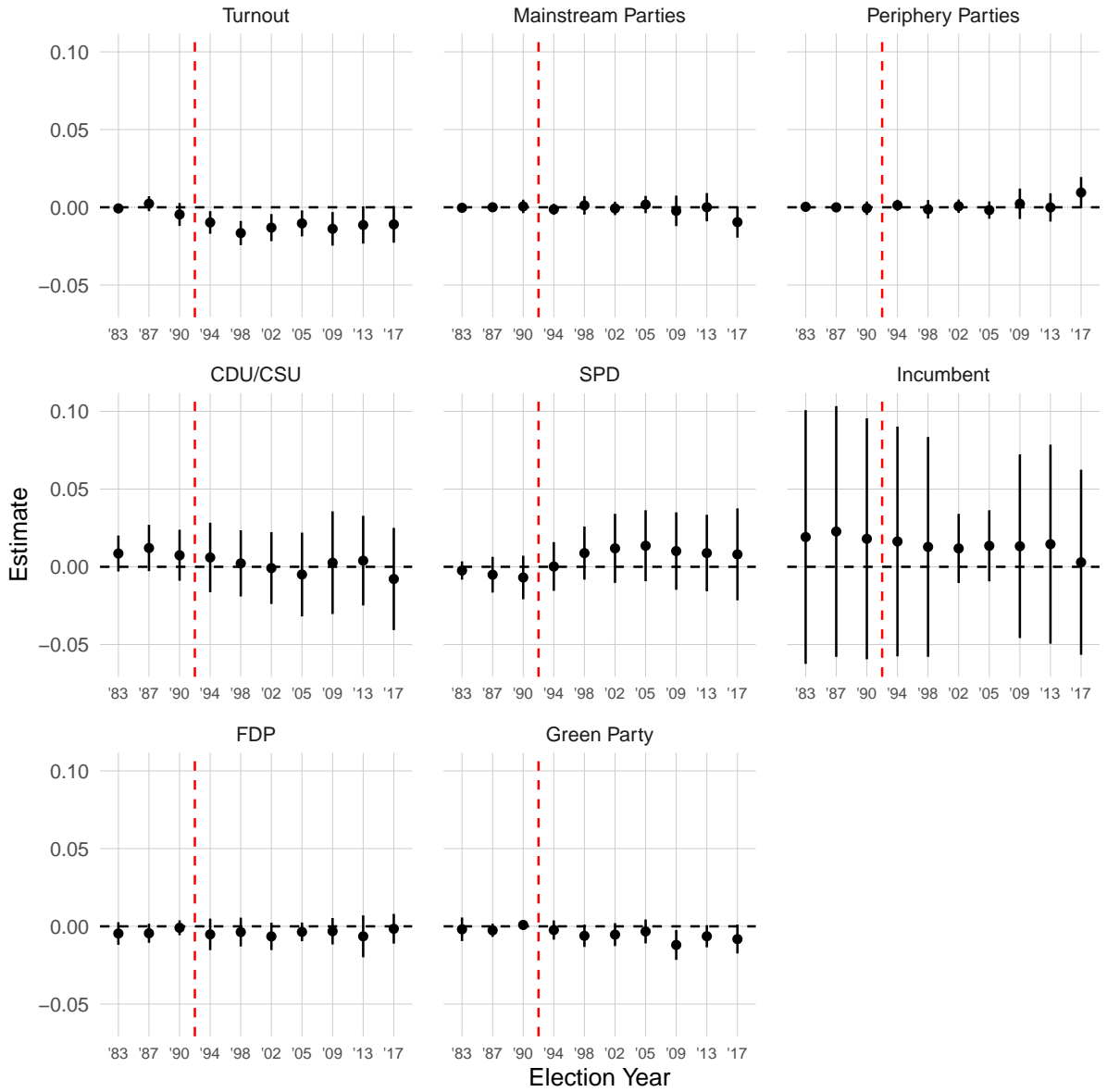
Period start	Period end	Closest election year	Source
1985	1989	1987	Joint Task's Rahmenplan No. 19
1988	1992	1990	Joint Task's Rahmenplan No. 23
1992	1995	1994	Joint Task's Rahmenplan No. 25
1996	1998	1998	Joint Task's Rahmenplan No. 28
1999	2001	2002	Joint Task's Rahmenplan No. 31
2002	2004	2005	Joint Task's Rahmenplan No. 34
2005	2008	2009	Joint Task's Koordinierungsrahmen 2009

## Economic effects



**Figure A.1:** Estimates with cluster-robust standard errors at the labor market regions for different model specifications at the county level. The dependent variable is unemployment rate. The treatment is the loss of eligibility for subsidies in September 1991 (red dashed line) following revisions in the regional policy after unification. Reference period: 1984.

## Main Results



**Figure A.2:** Difference-in-difference estimates with cluster-robust standard errors at the labor market regions. The dependent variable are vote share for all federal elections since 1980. The treatment is the loss of eligibility for subsidies in September 1991 (red dashed line) following revisions in the regional policy after unification. Reference period: 1980. The vote shares for mainstream parties include the votes for CDU, SPD, FDP, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.

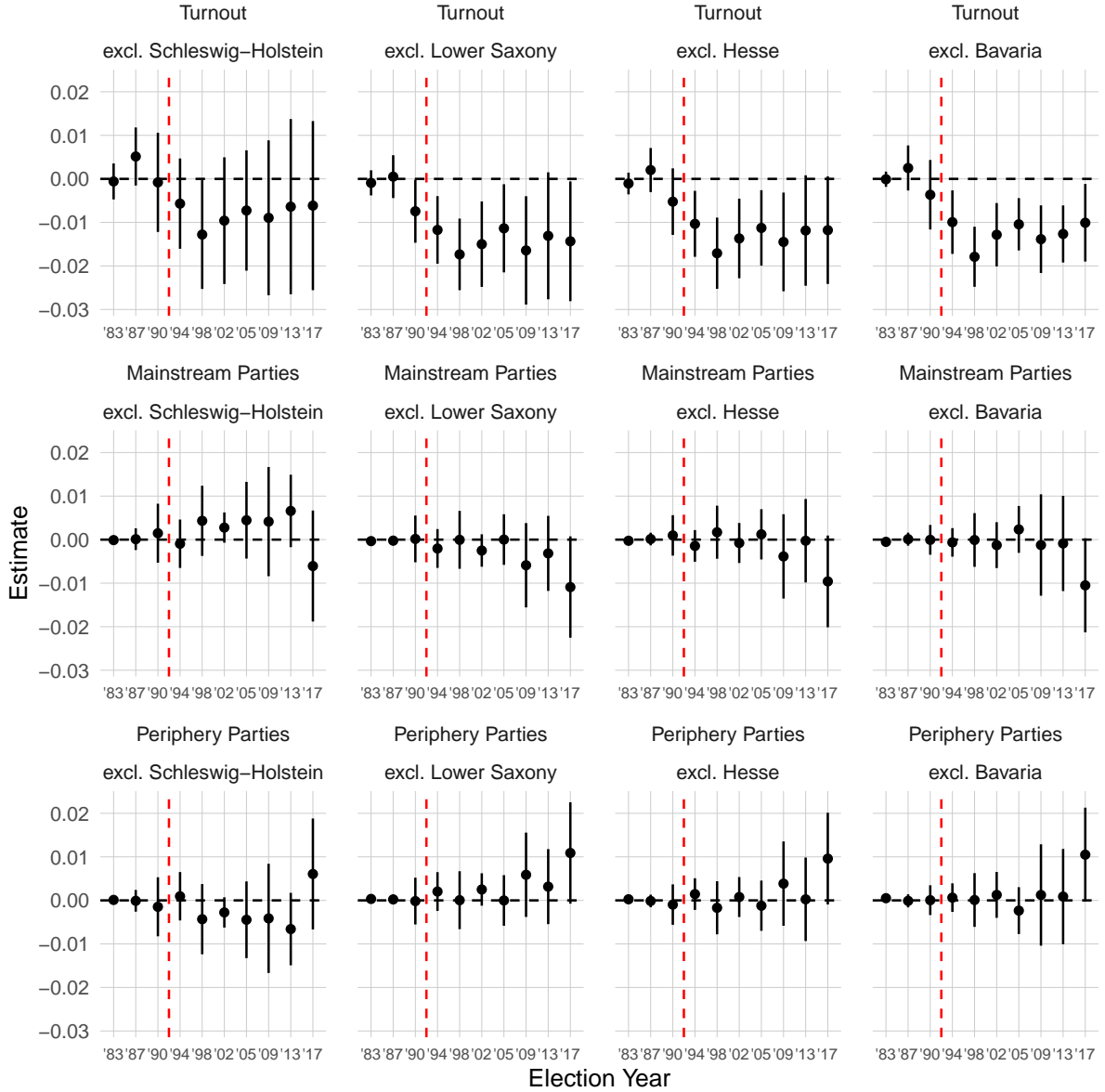
**Table A.2:** Difference-in-difference estimates of municipality level federal election outcomes

	Turnout	Mainstream Parties	Periphery Parties	CDU/CSU	SPD	Incumbent	FDP	Green Party
1983 (pre)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	0.01 (0.01)	-0.00 (0.00)	0.02 (0.04)	-0.00 (0.00)	-0.00 (0.00)
1987 (pre)	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	0.01 (0.01)	-0.01 (0.01)	0.02 (0.04)	-0.00 (0.00)	-0.00 (0.00)
1990 (pre)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	0.01 (0.01)	-0.01 (0.01)	0.02 (0.04)	-0.00 (0.00)	0.00 (0.00)
1994 (post)	-0.01** (0.00)	-0.00 (0.00)	0.00 (0.00)	0.01 (0.01)	0.00 (0.01)	0.02 (0.04)	-0.01 (0.01)	-0.00 (0.00)
1998 (post)	-0.02*** (0.00)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.01)	0.01 (0.01)	0.01 (0.04)	-0.00 (0.00)	-0.01 (0.00)
2002 (post)	-0.01** (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	-0.01 (0.00)	-0.01 (0.00)
2005 (post)	-0.01* (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	-0.00 (0.00)	-0.00 (0.00)
2009 (post)	-0.01* (0.01)	-0.00 (0.01)	0.00 (0.01)	0.00 (0.02)	0.01 (0.01)	0.01 (0.03)	-0.00 (0.00)	-0.01* (0.00)
2013 (post)	-0.01 (0.01)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.01)	0.01 (0.01)	0.01 (0.03)	-0.01 (0.01)	-0.01 (0.00)
2017 (post)	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.01)	-0.01 (0.02)	0.01 (0.02)	0.00 (0.03)	-0.00 (0.00)	-0.01 (0.00)
Num. obs.	14260	14260	14260	14260	14260	14260	14260	14260

\*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ .

Includes municipality and election-state fixed effects. Standard errors clustered at labor market regions. Reference election is 1980. First treatment year is 1992. The vote shares for mainstream parties include the votes for CDU, SPD, FDP, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.

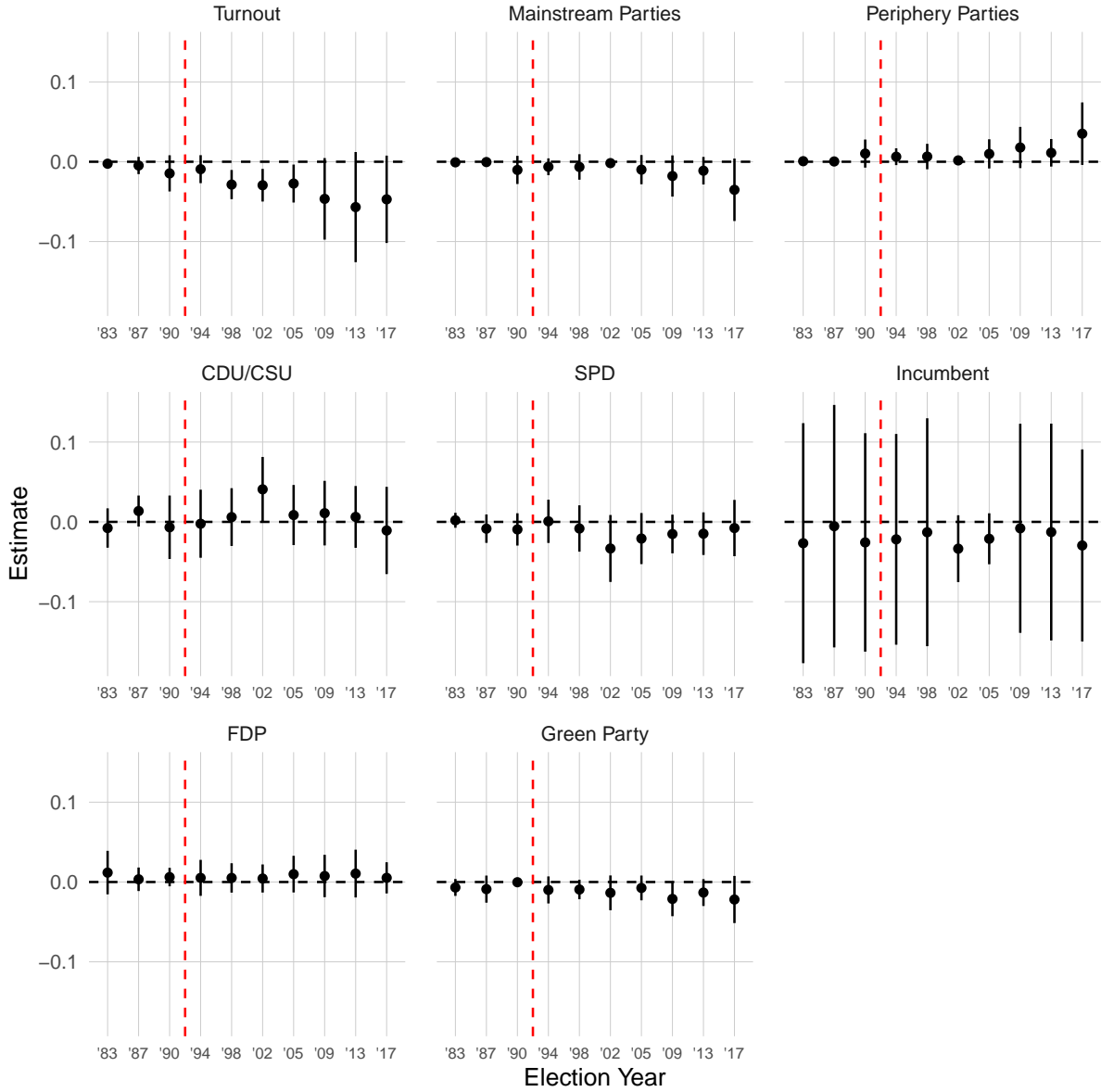
## Heterogeneity



**Figure A.3:** Difference-in-difference estimates with cluster-robust standard errors at the labor market regions when separately excluding one state from the analysis. The dependent variable are vote share for all federal elections since 1980. The treatment is the loss of eligibility for subsidies in September 1991 (red dashed line) following revisions in the regional policy after unification. Reference period: 1980. The vote shares for mainstream parties include the votes for CDU, SPD, FDP, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.



## Robustness



**Figure A.4:** Difference-in-difference estimates with cluster-robust standard errors at the labor market regions from a local linear regression with the eligibility (labor market region) index as running variable. The dependent variable are vote share for all federal elections since 1980. The treatment is the loss of eligibility for subsidies in September 1991 (red dashed line) following revisions in the regional policy after unification. Reference period: 1980. Includes municipality and election fixed effects. Interaction coefficient with running variable (eligibility index) omitted. The vote shares for mainstream parties include the votes for CDU, SPD, FDP, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.

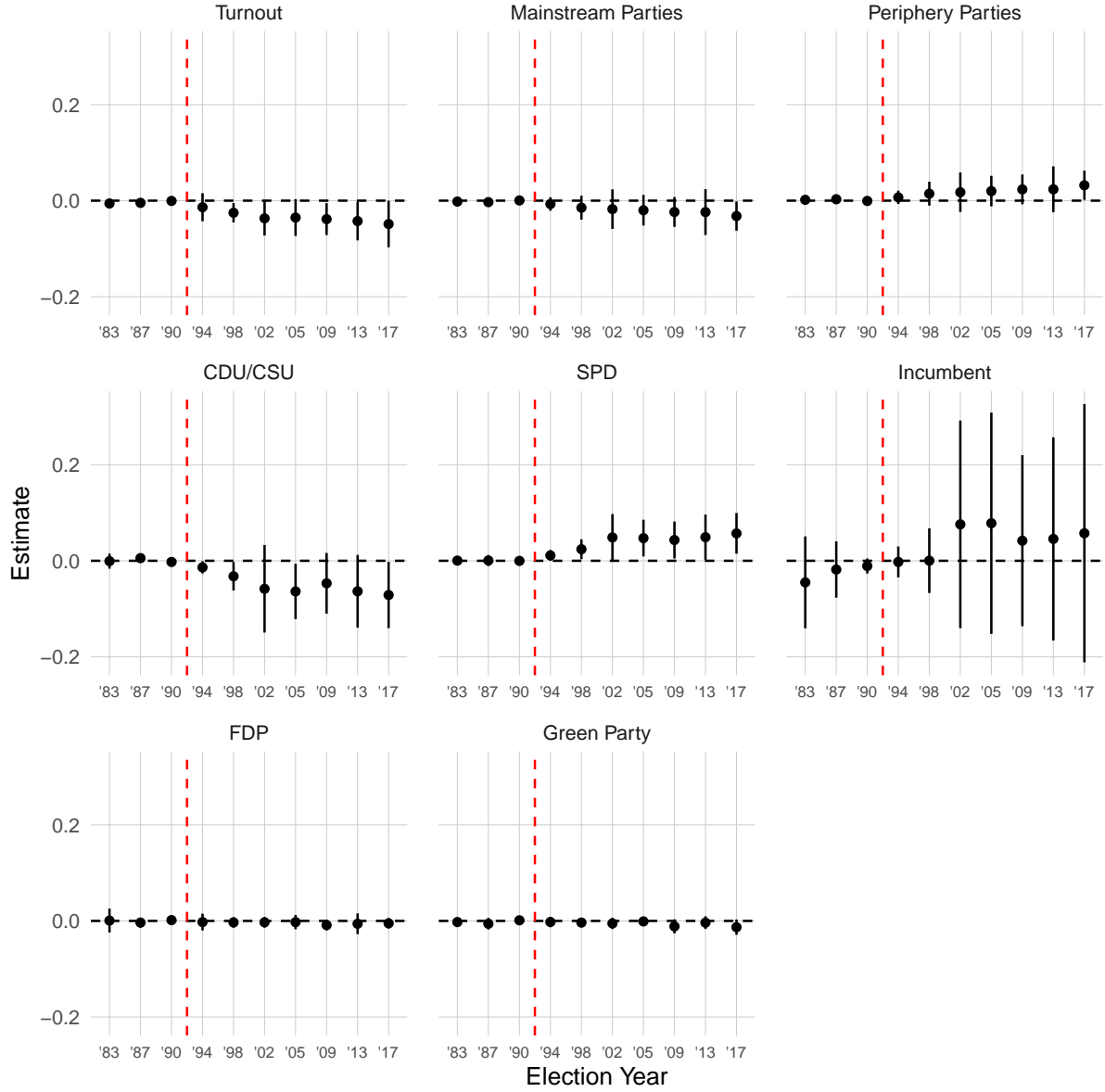
**Table A.3:** Robustness: DID-RDD of municipality level federal election outcomes

	Turnout	Mainstream Parties	Periphery Parties	CDU/CSU	SPD	Incumbent	FDP	Green Party
1983 (pre)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.01 (0.01)	0.00 (0.00)	-0.03 (0.08)	0.01 (0.01)	-0.01 (0.01)
1987 (pre)	-0.00 (0.01)	-0.00 (0.00)	0.00 (0.00)	0.01 (0.01)	-0.01 (0.01)	-0.01 (0.08)	0.00 (0.01)	-0.01 (0.01)
1990 (pre)	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.01)	-0.01 (0.02)	-0.01 (0.01)	-0.03 (0.07)	0.01 (0.01)	-0.00 (0.00)
1994 (post)	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.01)	-0.00 (0.02)	0.00 (0.01)	-0.02 (0.07)	0.01 (0.01)	-0.01 (0.01)
1998 (post)	-0.03** (0.01)	-0.01 (0.01)	0.01 (0.01)	0.01 (0.02)	-0.01 (0.01)	-0.01 (0.07)	0.01 (0.01)	-0.01 (0.01)
2002 (post)	-0.03** (0.01)	-0.00 (0.00)	0.00 (0.00)	0.04* (0.02)	-0.03 (0.02)	-0.03 (0.02)	0.00 (0.01)	-0.01 (0.01)
2005 (post)	-0.03* (0.01)	-0.01 (0.01)	0.01 (0.01)	0.01 (0.02)	-0.02 (0.02)	-0.02 (0.02)	0.01 (0.01)	-0.01 (0.01)
2009 (post)	-0.05 (0.03)	-0.02 (0.01)	0.02 (0.01)	0.01 (0.02)	-0.02 (0.01)	-0.01 (0.07)	0.01 (0.01)	-0.02 (0.01)
2013 (post)	-0.06 (0.04)	-0.01 (0.01)	0.01 (0.01)	0.01 (0.02)	-0.01 (0.01)	-0.01 (0.07)	0.01 (0.02)	-0.01 (0.01)
2017 (post)	-0.05 (0.03)	-0.04 (0.02)	0.04 (0.02)	-0.01 (0.03)	-0.01 (0.02)	-0.03 (0.06)	0.01 (0.01)	-0.02 (0.02)
Num. obs.	14260	14260	14260	14260	14260	14260	14260	14260

\*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ .

Difference-in-difference estimates with cluster-robust standard errors at the labor market regions from a local linear regression with the eligibility (labor market) index as running variable. The dependent variable are vote share for all federal elections since 1980. The treatment is the loss of eligibility for subsidies in September 1991 following revisions in the regional policy after unification. Reference period: 1980. Includes municipality and election fixed effects. Interaction coefficient with running variable (eligibility index) omitted. The vote shares for mainstream parties include the votes for CDU, SPD, FDP, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.

## A Appendix



**Figure A.5:** Difference-in-difference estimates with unit-level trends and cluster-robust standard errors at the labor market regions. The dependent variable are vote share for all federal elections since 1980. The treatment is the loss of eligibility for subsidies in September 1991 (red dashed line) following revisions in the regional policy after unification. Includes municipality and election fixed effects. Reference period: 1980. The vote shares for mainstream parties include the votes for CDU, SPD, FDP, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.

**Table A.4:** Robustness: Difference-in-difference estimates with unit-level trends of municipality level federal election outcomes

	Turnout	Mainstream Parties	Periphery Parties	CDU/CSU	SPD	Incumbent	FDP	Green Party
1983 (pre)	-0.01 (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.01)	0.00 (0.00)	-0.04 (0.05)	0.00 (0.01)	-0.00 (0.00)
1987 (pre)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	0.01 (0.00)	0.00 (0.01)	-0.02 (0.03)	-0.00 (0.01)	-0.01 (0.01)
1990 (pre)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.01 (0.01)	0.00 (0.00)	0.00 (0.00)
1994 (post)	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.01)	-0.01* (0.01)	0.01* (0.01)	-0.00 (0.02)	-0.00 (0.01)	-0.00 (0.01)
1998 (post)	-0.03* (0.01)	-0.01 (0.01)	0.01 (0.01)	-0.03* (0.02)	0.02* (0.01)	0.00 (0.03)	-0.00 (0.01)	-0.00 (0.00)
2002 (post)	-0.04* (0.02)	-0.02 (0.02)	0.02 (0.02)	-0.06 (0.05)	0.05* (0.02)	0.08 (0.11)	-0.00 (0.01)	-0.01 (0.01)
2005 (post)	-0.04 (0.02)	-0.02 (0.02)	0.02 (0.02)	-0.06* (0.03)	0.05* (0.02)	0.08 (0.12)	-0.00 (0.01)	-0.00 (0.00)
2009 (post)	-0.04* (0.02)	-0.02 (0.02)	0.02 (0.02)	-0.05 (0.03)	0.04* (0.02)	0.04 (0.09)	-0.01 (0.01)	-0.01 (0.01)
2013 (post)	-0.04* (0.02)	-0.02 (0.02)	0.02 (0.02)	-0.06 (0.04)	0.05* (0.02)	0.05 (0.11)	-0.01 (0.01)	-0.00 (0.01)
2017 (post)	-0.05* (0.02)	-0.03* (0.02)	0.03* (0.02)	-0.07* (0.04)	0.06** (0.02)	0.06 (0.14)	-0.00 (0.01)	-0.01 (0.01)
Num. obs.	16796	16796	16796	16796	16796	16796	16796	16796

\*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ .

Includes municipality and election fixed effects. Standard errors clustered at labor market regions. Reference election is 1980. First treatment year is 1992. The vote shares for mainstream parties include the votes for CDU, SPD, Greens and the vote shares for periphery parties the votes for AfD, LINKE and all other parties running in the election.

## A Appendix

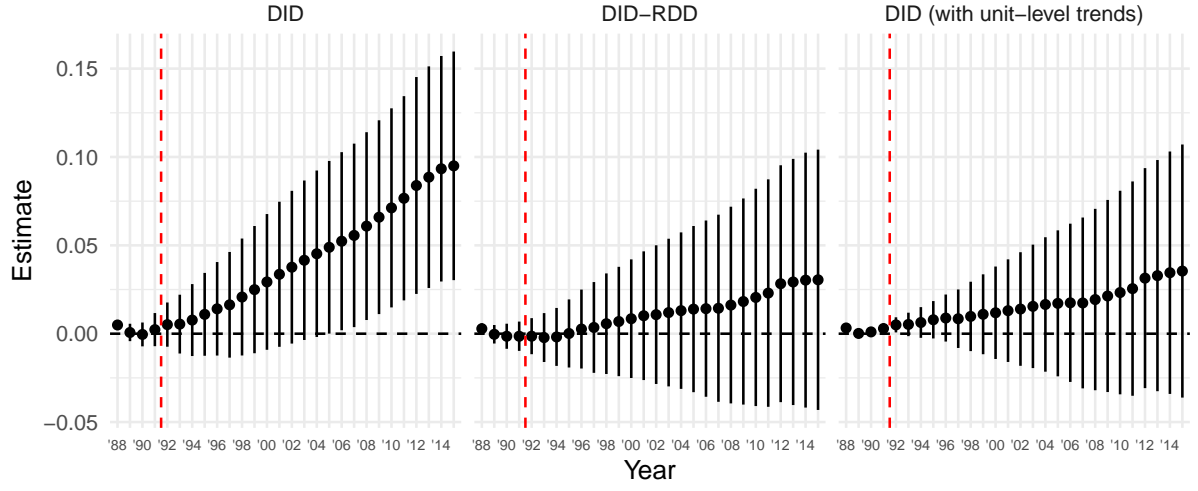
**Table A.5:** Difference-in-difference estimates of municipality level federal election outcomes for the Erststimme (constituency vote)

	Turnout	Mainstream Parties	Periphery Parties
1983 (pre)	−0.00 (0.00)	−0.00 (0.00)	0.00 (0.00)
1990 (pre)	−0.01* (0.00)	−0.00 (0.00)	0.00 (0.00)
1994 (post)	−0.01 (0.01)	−0.00 (0.00)	0.00 (0.00)
1998 (post)	−0.01*** (0.00)	0.00 (0.00)	−0.00 (0.00)
2002 (post)	−0.01** (0.00)	0.00 (0.00)	−0.00 (0.00)
2005 (post)	−0.01* (0.00)	−0.01 (0.01)	0.01 (0.01)
2009 (post)	−0.01* (0.01)	−0.00 (0.00)	0.00 (0.00)
2013 (post)	−0.01 (0.01)	−0.00 (0.01)	0.00 (0.01)
2017 (post)	−0.01 (0.01)	−0.01* (0.00)	0.01* (0.00)
Num. obs.	12968	12968	12968

\*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ .

Includes municipality and election-state fixed effects. Standard errors clustered at labor market regions. Reference election is 1980. First treatment year is 1992.

## Population movement



**Figure A.6:** Estimates with cluster-robust standard errors at the labor market regions for different model specifications at the county level. The dependent variable is population size (log-scaled). The treatment is the loss of eligibility for subsidies in September 1991 (red dashed line) following revisions in the regional policy after unification. Reference period: 1987.