

Please Don't Vote for Me: Voting in a Natural Experiment with Perverse Incentives*

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Abstract

Whether individuals vote strategically is one of the most important questions at the intersection of economics and political science. Exploiting a flaw in the German electoral system by which a party may gain seats by receiving *fewer* votes, this paper documents patterns of preference misrepresentation in a large, real world election. During the 2005 elections to the Bundestag, the sudden death of a right-wing candidate necessitated a by-election in one electoral district. Knowing the results in all other districts and given the paradoxical incentives in place, a substantial fraction of the electorate voted for a party other than their most preferred one, or abstained. As a result, the Christian Democratic Union won an additional mandate, extending its narrow lead over the Social Democrats.

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1. Introduction

Elections are a cornerstone of any modern democracy. Social choice theory, however, has shown almost all voting systems to be susceptible to strategic manipulation (Arrow 1951; Gibbard 1973; Satterthwaite 1975). That is, in theory voters have a systematic incentive to misrepresent their true preferences in order to affect the result of an election. Although tactical voting is individually rational, it may be socially undesirable, as it precludes the proper aggregation of preferences, and can thereby lead to inferior electoral outcomes. Hence, with a significant number of voters behaving strategically, lack of strategy-proofness in social choice mechanisms may have important real-world ramifications.¹

Yet, whether individuals actually cast strategic ballots to avoid “wasting their vote” (Duverger 1954) remains one of the most important open questions at the intersection of economics and political science. While strategic voting is well understood in theory (e.g., Carroll 2011; Cox 1994; Myerson and Weber 1993), the unobservability of preferences has made it very difficult to document tactical behavior empirically—despite substantial anecdotal evidence.² In fact, in an important paper Degan and Merlo (2009) prove that one can always find preference profiles to rationalize *any* cross-section of votes..

Some even argue that if there are any (psychic) costs associated with performing the “calculus of voting” (Riker and Ordeshook 1968), then individuals cannot possibly be expected to behave strategically (e.g., Downs 1957; Green and Shapiro 1994). After all, in large elections the probability of casting the pivotal vote becomes vanishingly small. Of course, how voters actually behave is ultimately an empirical question.

Given the difficulties in identifying strategic voting, it may not be surprising that the existing literature draws mixed conclusions. While Coate et al. (2008) reject the pivotal-voter model, Cox (1997) presents a swath of evidence broadly consistent with strategic behavior in different electoral systems. More recently, Kawai and Watanabe (2013) estimate a structural model of preferences and voting decisions, from which they infer that between 1.4% and 4.2% of voters actually misrepresent their preferences. At the same time Kawai and Watanabe (2013) argue that at least 63.4% of voters would do so if their preferred candidate was not in contention for victory.

Relying on survey responses about preferences, expectations, and votes, a large number of earlier studies argue that the prevalence of instrumentally rational voting is actually very low. Estimates typically range from 3% to 17% (e.g., Blais et al. 2001; Niemi et al. 1993; and

¹Throughout the paper the terms “strategic” and “tactical” are used interchangeably.

²Small-scale laboratory experiments relying on induced preferences are an important exception. These studies typically find convincing evidence of strategic voting, which increases with the availability of coordination devices such as polls or access to voting histories (e.g., Forsythe et al. 1993, 1996). Palfrey (2009) provides a useful review.

Abramson et al. 1992), but may be subject to severe survey biases (see Alvarez and Nagler 2000).³

Instead of taking survey responses at face value or imposing strong assumptions about voters' underlying preferences, this paper contributes to the existing literature by providing evidence from a natural experiment. The empirical strategy relies on a geographically localized, temporary reversal of incentives due to a flaw in the German electoral system by which a party may actually gain seats if it receives *fewer* votes. Using multiple years of election data and comparing the behavior of affected voters with that of unaffected ones (exploiting both geographic variation and variation over time), identification in this paper is not subject to the problems that plague most of the existing literature.⁴

Eleven days before the 2005 federal elections the candidate (*Direktkandidat*) of the far right-wing National Democratic Party (NPD) in Electoral District 160 (Dresden I) passed away unexpectedly. Although she was never expected to win, electoral law required a by-election. But in contrast to prior cases, her death occurred too close to election day for the by-election to be held on the originally scheduled date. Consequently, almost 220,000 eligible voters in Saxony's District 160 were given the opportunity to cast their ballots two weeks after everybody else, and after the Federal Returning Officer (*Bundeswahlleiter*) had announced the preliminary results of the election in all other districts.

Based on these results, the Christian Democratic Union (CDU) and its Bavarian sister party, the Christian Social Union (CSU), won 225 seats in the Bundestag, whereas the rival Social Democratic Party (SPD) received only 222. Under ordinary circumstances, one would hardly expect less than .5% of the electorate to influence the outcome of an election, especially not when the allocation of seats is approximately proportional to the number of votes. Yet, in 2005 a few hundred votes could make the difference between a 2-, 3-, or even 4-seat lead for the CDU/CSU.

Although the German electoral system aims for proportional representation of all parties clearing a 5%-threshold, determining a party's exact number of seats is substantially more complicated. The crucial point is that individuals cast two different votes—a list vote and a candidate vote—and that a party may lose a seat in the legislature by obtaining “too many” list votes in states in which it won the plurality of the candidate vote in sufficiently many districts (see Section 2 for a detailed explanation). In 2005 exactly this situation occurred.

³Kawai and Watanabe (2013) as well as Alvarez et al. (2006) point out that some of the literature mistakes the share of voters who actually voted for someone other than their most preferred candidate as equal to the fraction of voters who would do so if their preferred candidate was believed to be unlikely to win the race. Not surprisingly, both papers estimate the former number to be significantly smaller than the latter.

⁴Throughout most of the paper the identifying assumption is that the one-time reversal in incentives did not coincide with larger deviations from trend for affected voters than for unaffected ones.

Figure 1 illustrates the paradoxical incentives in place during the by-election. If the CDU were to win more than approximately 41,000 list votes in Saxony’s District 160—it had received 49,638 in the 2002 election—then it would lose a seat in parliament. On the other hand, it might even increase its seat total by one if it garnered few list votes, but the plurality of the candidate vote.⁵

Critically, since the by-election was conducted almost two weeks after the preliminary results had been announced, the electorate in District 160 was potentially aware of the peculiar circumstances *before* going to the polls.⁶ In fact, due to the closeness of the national race, the by-election received considerable media coverage with pundits lamenting the perverse incentives. Consequently, if voters reacted to the change in incentives, one would expect the following: (i) The CDU should garner fewer list votes in the by-election than under ordinary circumstances, but (ii) it should receive a larger share of the candidate vote.⁷

In the end, the CDU did win the plurality of the candidate vote, while receiving “only” 38,208 list votes. It was, therefore, able to extend its narrow lead over the SPD. A priori, however, is not clear whether this would have happened even in the absence of the by-election, or whether voters misrepresented their preferences to affect the outcome of the election.

The empirical evidence presented in this paper points clearly toward misrepresentation of preferences. That is, some individuals did not vote for the party or candidate that was ideologically closest to them. For instance, the upper panel in Table 1 shows that between 2002 and 2005 the CDU’s share of list votes declined by 6.1 percentage points in District 160, whereas it decreased only 3.4 percentage points in Saxony’s other districts, or 3.0 percentage points in East Germany as a whole. Yet, between 2005 and 2009 the CDU gained 9.9 percentage points in District 160, relative to 5.3 and 3.4 percentage points in the comparison groups. By contrast, the lower panel in Table 1 indicates that going from the 2002 to the subsequent 2005 elections, the CDU’s share of the candidate vote actually increased in District 160, while it decreased in the remainder of Saxony.⁸ The opposite is true for the difference between 2005 and 2009. Thus, consistent with voters behaving tactically, the raw

⁵Given past election results in District 160 only the CDU had a realistic chance of gaining or losing a seat. While other parties stood to gain a mandate in the state of Saxony, this would have come at the expense of a mandate in some other state.

⁶The inherent uncertainty in a party’s number of votes in each state makes it almost impossible to predict similar situations in advance, although *ex post* one can in most election years find instances in which a party may have benefited by receiving fewer votes in some state.

⁷While winning the plurality of the candidate vote may not necessarily increase a party’s seat total in regular election years (cf. Section 2), in the 2005 by-election it was known that the CDU would gain a seat in the Bundestag by securing the direct mandate in District 160.

⁸Since the political landscape in East Germany continues to be quite different from that in the West, electoral districts in West Germany may be a relatively poor comparison group. Nevertheless, the results in this paper are qualitatively robust to including those districts as well (cf. Table 7).

data suggest that the CDU won an unusually low fraction of list votes, but received a higher than normal share of the by-election’s candidate vote.

Using hitherto unavailable official election data on the sub-district level, the main result of this paper establishes that the disparities documented in Table 1 are quite robust (even to the inclusion of municipality specific trends) and unlikely to be due to chance. Moreover, exploiting the different incentive structure associated with candidate and list votes, the evidence presented below indicates that supporters of the rival SPD voted for the CDU (in an attempt to hurt it), whereas adherents of the latter either abstained or substituted toward the libertarian Free Democratic Party (FDP)—the CDU’s traditional coalition partner.

Although the by-election was a highly unusual event and the incentives to cast misaligned votes might have been especially salient, the point estimates indicate that at least 8.8% of the electorate in District 160 misrepresented their preferences when faced with the new incentive structure. Moreover, the findings in this paper confirm the long-standing concern that voters’ misrepresentation of preferences might distort electoral outcomes.

It is important to note at the outset that the main estimates in this paper refer to voters misrepresenting their preferences, and that this behavior can, but need not be, strategically motivated. For instance, some voters may be naïve or “behavioral” and simply follow the official party line. These voters might still misrepresent their true preferences if party leaders made such a request, but one would not necessarily want to call them “strategic.” These agents do, however, vote “non-ideologically” in the sense of Degan and Merlo (2009). That is, they do not vote for the candidate or party that is ideologically closest to them.

For the issue of strategy-proofness in social choice it is inconsequential why voters misrepresent their preferences. After all, the possibility that misaligned votes may affect electoral outcomes remains regardless of voters’ motivation. It is, therefore, important to empirically document patterns of preference misrepresentation.

Ancillary results suggest that individuals did not merely follow parties’ official recommendations as to how to cast their votes, but at least some agents acted “strategically” in the narrower sense of the word. That is, the estimates presented below speak not only to the question of whether voters do, in fact, misrepresent their true preferences, but also as to why they might do so.

The remainder of the paper proceeds as follows. Section 2 provides background information on the electoral system used in Germany’s elections to the Bundestag, as well as further details on the natural experiment. Section 3 describes the data, and Section 4 contains the empirical results. The last section concludes. A Data Appendix with the precise definitions and sources of all variables is provided on the author’s website.

2. Germany’s Electoral System and the Case of District 160

Elections of representatives to the Federal Diet of Germany (*Bundestag*) are held according to a mixed member system with approximately proportional representation. Except for minor modifications, the same system has been in place since 1957 (see Bawn 1993 for an account of its genesis). This section explains the specifics of the German voting system in which a party may gain seats by receiving fewer votes.⁹ Furthermore, it provides additional information on the by-election in District 160 and the perverse incentives in place. Readers satisfied with the description in the introduction may skip the section without much loss in continuity.

2.1. *Perverse Incentives in Germany’s Federal Elections*

As mentioned before, each voter in Germany casts two votes. The first, or candidate vote (*Erststimme*), is used to elect a constituency representative in each of 299 single-member districts according to plurality rule.¹⁰ The winners of these races hold direct mandates, and are automatically awarded a seat in the legislature. More importantly, the second, or list vote (*Zweitstimme*), is cast for a party list, and the total number of party members in the Bundestag is roughly proportional to a party’s share of the national list vote among parties clearing a 5%-threshold.¹¹ Approximately proportional representation is achieved by deducting direct mandates from the number of mandates to which parties are entitled based on the list vote, i.e. their list mandates.¹²

Deviations from strict proportionality are due to three factors: the 5%-threshold, rounding, and overhang mandates (*Überhangmandate*). The latter are a peculiarity of the German system that arises when in some state a party wins more direct mandates than seats under proportional representation. In such cases, the total number of seats in the Bundestag is raised and said party gets to keep all direct mandates without losing seats in other states.¹³

More formally, let $d_{p,s}$ denote the number of direct mandates accruing to party p in state s . $v_{p,s}$ is the number of list votes that p received in s , with the equivalent number on the national level given by \tilde{v}_p , i.e. $\tilde{v}_p = \sum_s v_{p,s}$. With this notation in hand, party p ’s seat total is calculated in three steps:

⁹In doing so it borrows from Korte (2010).

¹⁰Single-member districts are represented by exactly one delegate in parliament, and plurality rule refers to an electoral system in which whoever receives the most votes is declared the winner of the election.

¹¹A party list is a pre-determined ranking of candidates based on which list mandates are awarded. By law parties must post different lists in each state, and a candidate can appear on only one list (§27 BWG). Proportionality rule means that a party’s vote share equals its share of seats in parliament.

¹²In this context, mandate stands for a parliamentary seat won in an election.

¹³Although it had been known for a long time that a voting system which provides constituencies with the opportunity to elect some (but not all) members of parliament directly might give rise to perverse incentives, until the by-election in District 160 the German Constitutional Court had judged this to be inconsequential.

Step 1: Proportional Allocation of List Mandates to Parties. Absent overhang mandates, there are 598 seats in the Bundestag. These are allocated by proportionality rule to the set of parties clearing the 5%-threshold or winning at least three direct mandates. That is, the number of list mandates of party p equals

$$\tilde{l}_p \cong \begin{cases} 598 \times \tilde{v}_p / \sum_{p' \in P} \tilde{v}_{p'} & \text{if } p \in P \\ 0 & \text{if } p \notin P \end{cases},$$

where $P = \{p | \tilde{v}_p / \sum_{p' \in P} \tilde{v}_{p'} \geq .05 \vee \sum_s d_{p,s} \geq 3\}$ and \cong represents equality after rounding according to the method of Hare-Niemeyer.¹⁴ Also known as the “largest remainder method,” Hare-Niemeyer first assigns each party a number of seats equal to the integer part of $598 \times \tilde{v}_p / \sum_{p' \in P} \tilde{v}_{p'}$. The parties with the largest remainders are then allocated one additional list mandate until all available seats have been distributed. This ensures that $\sum_p \tilde{l}_p = 598$.

Step 2: Proportional Allocation of Mandates to State Lists. By law parties are required to decide on different lists in each state. Hence, \tilde{l}_p needs to be broken down to the state level, $l_{p,s}$, with $\tilde{l}_p = \sum_s l_{p,s}$. Again, this is done in approximately proportional fashion. More precisely, for all s and all p ,

$$(1) \quad l_{p,s} \cong \begin{cases} \tilde{l}_p \times v_{p,s} / \tilde{v}_p & \text{if } p \in P \\ 0 & \text{if } p \notin P \end{cases},$$

where \cong is defined as above.

Step 3: Determination of the Actual Number of Seats. The actual number of seats that party p receives in state s is given by

$$(2) \quad n_{p,s} = \max \{d_{p,s}, l_{p,s}\} \quad \forall p.$$

If $d_{p,s} < l_{p,s}$ then, in addition to the district winners, the first $l_{p,s} - d_{p,s}$ candidates on p 's list in s are elected to the Bundestag as well. Otherwise, only holders of direct mandates receive a seat

Note that unless a party can secure overhang mandates, i.e. unless $d_{p,s} > l_{p,s}$ for some s , its seat total, $\tilde{n}_p = \sum_s n_{p,s}$, equals the number of seats it would be assigned under proportional representation, i.e. \tilde{l}_p . Empirically, overhang mandates are not uncommon—15 occurred after the 2005 elections and 24 after those in 2009.¹⁵

¹⁴In 2009 the Sainte-Laguë method was used instead.

¹⁵Generally they accrue only to the two major factions, i.e. the CDU/CSU or the SPD.

Importantly for the purposes of this paper, there also exist scenarios in which a party *loses* a seat by *gaining* list votes. To see this, consider a small increase in $v_{p,s}$, not large enough to affect \tilde{l}_p . Although the total number of p 's list mandates does not change, even a small gain in $v_{p,s}$ may be enough for $\tilde{l}_p \times v_{p,s}/\tilde{v}_p$ in equation (1) to be rounded upward instead of downward. But for $\tilde{l}_p = \sum_s l_{p,s}$ to continue to hold, an increase in $l_{p,s}$ must result in a corresponding reduction in $l_{p,s'}$ for some $s' \neq s$. If $d_{p,s} > l_{p,s}$ and $d_{p,s'} < l_{p,s'}$, then according to (2) that small increase in votes would actually lower party p 's seat total. In words, a small gain in the number of list votes may lead to the reassignment of a list mandate from a state in which a party won few direct mandates to one in which it secured more direct mandates than seats under proportional representation. But since the actual number of seats it receives in any state equals the maximum of direct and list mandates (cf. Step 3), reallocating a list mandate from one state to another may lead to a lower seat total.

In general, the occurrence of such a situation is very difficult to predict in advance—primarily because the inherent uncertainty in $v_{p,s}$ makes rounding in (1) almost impossible to anticipate. This was not the case, however, leading up the 2005 elections in District 160.

2.2. *The Case of District 160*

On September 5, 2005, Kerstin Lorenz, running as the National Democratic Party's (NPD) direct candidate in Electoral District 160 (Dresden I), suffered a stroke during a campaign event. As a result of the stroke she passed away on September 7—eleven days before the elections to the Bundestag.

Although Lorenz had virtually no chance of winning, German electoral law requires a by-election whenever a direct candidate dies prior to election day (cf. §43 BWG and §82 BWO). In similar instances, by-elections were usually held on election day itself. But in the case of District 160, Lorenz's death occurred too close to the originally scheduled date for the NPD to be given sufficient time to nominate another candidate, and for new ballots to be printed. Thus, the electorate in District 160 was asked to go to the polls on October 2, while elections in all other districts took place on September 18.¹⁶ Yet, by October 2 the Federal Returning Officer (*Bundeswahlleiter*) had already announced the preliminary official results of the election (Bundeswahlleiter 2005c).

According to these results, the Christian Democratic Union (CDU) was entitled to 179 seats in the Bundestag, and its Bavarian sister party, the Christian Social Union (CSU), would be represented by 46 delegates. The rival Social Democratic Party (SPD) received

¹⁶In Germany, elections are almost always held on Sundays. It is important to point out that October 3, 2005 was a national holiday. While the fact that the by-election was held on a three-day-weekend may explain the lower turnout, it cannot account for the CDU receiving a lower share of the list vote while simultaneously garnering a higher fraction of the candidate vote (see Section 4).

only 222 mandates. Hence, prior to the by-election in District 160 the CDU/CSU had a narrow 3-seat lead over the SPD, making it the largest faction.¹⁷

Table 2 displays the preliminary results (excluding District 160), as well as the calculations used to determine the CDU’s seat total. Based on these results and past outcomes in District 160, no party could hope to win an additional list mandate on the national level (cf. Step 1). Focusing on the second step, however, it is straightforward to verify that the CDU found itself in the perverse situation described above.

Should, say, 42,000 voters cast their list votes for the CDU, then it would receive 11 instead of 10 list mandates in Saxony, and 46 instead of 47 in the state of North Rhine-Westphalia. But given the number of direct mandates it had already won in these states, receiving “too many” list votes in the by-election would actually cost the CDU a seat in the Bundestag (cf. Step 3).

However, if it received fewer than 41,000 list votes, then the number of list mandates in the state of Saxony would remain at 10 (and potentially increase from 2 to 3 in the Saarland offset by a corresponding reduction in North Rhine-Westphalia). Thus, by winning the direct mandate in District 160 while not receiving “too many” list votes, the CDU/CSU faction could even gain a seat (cf. Figure 1).

Due to the closeness of the national race, the by-election received substantial attention from the media. Although coverage focused mostly on the competition for the outstanding direct mandate, a number of pundits also commented on the perverse incentives associated with the list vote.

Moreover, some parties adapted their campaign strategies.¹⁸ The CDU posted banners promoting its direct candidate (“Erststimme für Andreas Lämmel”), whereas the SPD contender used an interview with the local newspaper to ask supporters of the Green Party, whose nominee had virtually no chance of being elected, for their support (Sächsische Zeitung 2005a). Only the libertarian FDP, however, came out with an *explicit* recommendation as to how their supporters should behave. Despite the fact that the FDP could not gain an additional seat in the Bundestag, it printed more than 1,000 new posters prompting voters to cast their first vote for the CDU candidate and their second vote for the FDP (see Figure 2).¹⁹ All other parties, including the CDU and the SPD, both of which stood to gain the

¹⁷Traditionally, the president (*Bundespräsident*) charges the leader of the largest faction with forming a new government.

¹⁸Unfortunately, there does not exist geographically disaggregated campaign spending data.

¹⁹Both FDP and CDU officials denied allegations of collusion. In the past the FDP had often campaigned for the list votes of CDU supporters in order to pass the national 5%-threshold. But it hadn’t asked its own supporters to abandon the FDP direct candidate in favor of that of the CDU. In 2005, the FDP gained 9.8% of the national list vote, and was thus far from failing to clear the 5%-threshold. By winning enough list votes in District 160 the FDP could only receive an additional list mandate in Saxony at the expense of one

most from voters’ strategic behavior, were very hesitant to blatantly ask their supporters to cast tactical list votes—though, if prompted, party volunteers would explain the peculiar incentives. When asked about why his party didn’t endorse strategic voting, the CDU candidate, Andreas Lämmel, responded that he didn’t approve of such “games” (Sächsische Zeitung 2005c). And the SPD candidate, Marlies Volkmer, answered that the SPD “wants to be strongest party and doesn’t engage in any tactical considerations” (Sächsische Zeitung 2005c). Presumably sincere voters might penalize the party for attempting to “manipulate” the by-election. For similar reasons the Green Party completely abstained from making any recommendations. Its representative only said Green Party supporters were smart enough to know what the best decision would be (Frankfurter Allgemeine Zeitung 2005). And The Left explicitly asked its followers to vote for them with both votes, just as in any other year.

The columns on the right of Table 2 show the final result of the election. In the end, the CDU received “only” 38,208 list votes, and won the plurality of the candidate vote.²⁰ Therefore, it ended up gaining one seat.

Given the intense media coverage, it seems reasonable to assume that a non-trivial fraction of voters were aware of the reversed incentives. But since a single vote has almost no chance of being decisive, it is not clear whether individual voters would indeed react by misrepresenting their preferences, or whether the CDU would have won the additional seat even under ordinary circumstances.

3. Data Sources and Summary Statistics

In order to answer this question, the present paper relies on data from several sources. The primary data set consists of the official results of the 2002, 2005, and 2009 elections to the Bundestag by polling precinct (*Wahlbezirk*). In Germany, polling precincts are the smallest administrative units at which votes are counted. Each precinct is fully contained in an electoral district, and is associated with one polling station, where a returning officer is to ensure the lawfulness of the election. As a rule, no precinct should consist of more than 2,500 eligible voters. The data include information on the number of list and candidate votes for each party, the number of eligible voters, as well as the number of invalid votes. They have been obtained from the Federal Statistical Office, and were, until recently, not publicly available.

Although the total number of districts remained unchanged between 2002 and 2009, migration led to the redrawing of electoral districts in a small number of instances. In particular, the state of Saxony contained 17 districts in 2002 and 2005, but only 16 in 2009 (see Figure

in North Rhine-Westphalia or Saxony-Anhalt.

²⁰Note that, compared to the preliminary results, the final list vote count in other districts changes slightly.

3 for a map of electoral districts in the state of Saxony as of 2005). In order to ensure comparability over time, this paper constructs a consistent mapping between polling precincts and electoral districts. The mapping relies on municipality identifiers contained in the raw data, as well as the appendices to the Bundeswahlgesetz, which in every election year list all municipalities in any given district. Thus, knowing in which municipality a precinct is located and given information on the district to which this municipality belonged in 2005, it is relatively straightforward to map precincts into geographically constant districts. All results build on this mapping.²¹

Information on demographic as well as socio-economic characteristics of districts is provided by Bundeswahlleiter (2002b, 2005d, 2009b). These publications rely on official numbers from the Federal Statistical Office, aggregated to the level of the electoral district. Unfortunately, there exists no comparable information on polling precincts.

Differentiating between District 160 and Saxony’s other districts, Table 3 presents summary statistics for all variables used throughout the analysis. Not surprisingly, given that District 160 consists of the southern parts of the city of Dresden, there exist important differences. Not only is District 160 substantially more urban, but in contrast to other districts its population is actually growing. Moreover, residents of District 160 are less likely to work in manufacturing, and experience lower rates of unemployment—although unemployment is still a major problem. In terms of election results, the CDU receives on average somewhat lower vote shares in District 160, whereas the SPD and the Green Party fare slightly worse in the remainder of Saxony.

Although differences in political preferences appear to be less stark than those in socio-economic characteristics, District 160 is clearly not perfectly representative. It is, therefore, important to account for district specific idiosyncrasies in determining the electorate’s reaction to the paradoxical incentives it faced during the by-election.

4. Evidence of Preference Misrepresentation

4.1. *Econometric Approach*

The empirical strategy in this paper builds on the classical difference-in-differences (DD) approach, with District 160 being “treated” and all other districts in the state of Saxony serving as the “control group”.²² The most important difference is that there are three instead

²¹See the Data Appendix for additional information on the construction of the mapping. Since individual precincts change considerably between 2002 and 2009 and there do not exist data on the sub-precinct level, it is not possible to link precincts over time. Reassuringly the results in this paper are robust to excluding all electoral districts which did not remain geographically constant over time (cf. Table 7).

²²Districts in the same state are likely subject to similar overall trends and may thus be a better comparison group than districts in East Germany as a whole, or even West Germany. The robustness checks in Table 7 show that the main results are qualitatively robust to the choice of control group.

of the usual two periods. As voters in District 160 faced disparate incentives only during the 2005 elections, it is useful to think of 2005 as the “treatment” period, whereas 2002 and 2009 should be regarded as pre- and post-treatment periods, respectively.

In the standard DD approach the key identifying assumption is fairly restrictive: in the absence of treatment, i.e. without a by-election, District 160 would have followed the same path as districts in the comparison group. Although this assumption is not directly testable, one might be willing to judge its reasonability by comparing outcomes in the pre- and post-treatment periods. To this end, consider the upper panel in Figure 4. The graph on the left plots the CDU’s share of the list vote in 2002 against that in 2009, and the graph on the right does so for its share of the candidate vote. Each dot corresponds to an electoral district in Saxony. Evidently the CDU’s 2009 vote share in District 160 is in both cases very close to what one would predict based on that in 2002. That is, in non-treatment years District 160 appears to conform to the same general pattern as other districts in the same state.

By contrast the middle and bottom panels in Figure 4 plot the CDU’s vote shares in 2005 against those in 2002 and 2009, respectively. In each of these graphs District 160 is a clear outlier. While the CDU received substantially fewer list votes in 2005 than predicted based on the outcome of the previous election, the opposite is true for 2009. Conversely, it received a larger than predicted share of the candidate vote in the year of the by-election, but a smaller one thereafter. The pattern of deviations conforms, therefore, exactly to what one would expect if a substantial fraction of the electorate in District 160 did, indeed, cast misaligned votes.

Although the patterns in Figure 4 are certainly suggestive, quantifying the extent of misaligned voting more precisely, accounting for district specific idiosyncrasies, and testing additional predictions requires some “econometric machinery.” Therefore, consider the following specification:

$$(3) \quad v_{p,i,t} = \tau_{p,t} + \mu_{p,d} + \delta \mathbf{1}[d = 160] \times \mathbf{1}[t = 2005] + \varepsilon_{p,i,t},$$

where $v_{p,i,t}$ denotes party p ’s vote share in polling precinct i during election year t . $\tau_{p,t}$ and $\mu_{p,d}$ mark comprehensive sets of time and district fixed effects, with d indexing electoral districts. $\mathbf{1}[d = 160]$ is an indicator variable equal to one if precinct i is part of District 160, and zero otherwise. Similarly, $\mathbf{1}[t = 2005]$ equals one for the 2005 elections. The parameter of interest is δ . It indicates what effect the incentives described above had on the voting behavior of the constituency in District 160.

4.2. Empirical Evidence

Table 4 presents the main empirical results. The numbers therein correspond to DD estimates of δ , obtained from estimating equation (3) by weighted least squares, with weights corresponding to the number of voters. Results in the top panel are based on district level data, whereas the bottom one uses all available information by relying on individual polling precincts as the unit of observation. To allow for arbitrary forms of autocorrelation in the residuals as well as for correlation across precincts, standard errors are clustered by electoral district. Moving from the left to the right within each group of regressions, the set of included fixed effects steadily grows.

As one would expect, point estimates based on district and precinct level data align quite closely, but the latter are estimated more precisely. Quantitatively, δ is estimated to be fairly large. For instance, in the absence of the by-election the CDU is predicted to have garnered about 4.9% less of the candidate vote, but an additional 3.6% of the list vote.²³ Under most reasonable assumptions on turnout, the latter would have resulted in substantially more than 41,000 list votes, and, therefore, cost the CDU a seat in parliament.²⁴

Of course, it is far from certain whether the key identifying assumption of the DD approach is, in fact, satisfied. For instance, compared to Saxony’s other electoral districts, CDU vote shares in District 160 might have been on a different trajectory, even in the absence of a by-election. If districts do, indeed, follow differential trends, then the estimates in Table 4 might be biased.²⁵

Fortunately, the fact that the current setup contains three instead of the usual two periods allows for econometric models that control quite flexibly for any possible (unobserved) trends. More specifically, the following specification explicitly assigns each village, or municipality m its own linear trend $\gamma_{p,m}$:

$$(4) \quad v_{p,i,t} = \tau_{p,t} + \mu_{p,m} + \delta \mathbf{1}[d = 160] \times \mathbf{1}[t = 2005] + \gamma_{p,m}t + \varepsilon_{p,i,t},$$

Here, the key identifying assumption is substantially weaker than in the standard DD approach. For estimates of δ to be unbiased it needs to be the case that the one-time reversal in

²³Other parties’ gains and losses are estimated as follows: +3.23% for the SPD, +7.02% for the FDP, -1.55% for the Green Party, and -2.81% for The Left, and -2.31% for all remaining parties.

²⁴Note well, the DD estimates indicate the *difference* in the extent of non-ideological voting between the by-election and the “regular” election on September 18. This does not require the assumption that all voters in the “regular” election behaved sincerely. For this reason one may want to regard the DD estimates as lower bound (see also the discussion in Section 5)

²⁵At least in principle, voters could have changed their rankings of parties not because of a change in preferences but due to the arrival of new information between September 18 and the by-election. While this may cause problems for standard DD estimates, the fact that CDU received *more* list votes but *fewer* candidate votes is at odds with such an explanation.

incentives did not coincide with larger *deviations from trend* in affected municipalities than in unaffected ones.

Results obtained from estimating equation (4) by weighted least squares are displayed in the upper two panels of Table 5. Within each group the leftmost point estimate corresponds to the usual DD estimate, i.e. the baseline result from the previous table. The second estimate accounts for district specific trends, whereas the rightmost allows for each municipality in the data to follow its own trajectory. The results in columns (1)–(3) and (7)–(9) are based on the main precinct level data set covering the period from 2002–2009. Additional results based on municipality level data from 1994–2009 appear in columns (4)–(6) as well as (10)–(12). The latter data offer a longer pre-treatment period (making it easier to pick up trends), but they only exist at a higher level of aggregation.

Reassuringly, independent of the level of aggregation and the length of the time period under consideration, estimates of δ which explicitly control for trends are very similar to their DD counterparts in Table 4. This suggests that differential trends cannot explain the patterns in the data.²⁶

Another way to account for the pitfalls of the DD approach in isolating the effect of the by-election and its unusual incentive structure is to pursue a triple-differencing strategy. That is, to use the *difference* in the CDU’s share of the candidate and list vote as dependent variable, and to estimate

$$(5) \quad v_{p,i,t}^C - v_{p,i,t}^L = \tau_{p,t} + \mu_{p,m} + \delta \mathbf{1}[d = 160] \times \mathbf{1}[t = 2005] + \gamma_{p,m}t + \varepsilon_{p,i,t},$$

where $v_{p,i,t}^C$ refers to the CDU’s share of the candidate vote in precinct i during election year t , and $v_{p,i,t}^L$ denotes its share of the list vote. Note well, even if there was an unobserved deviation from trend specific to District 160 and the 2005 election, as long as it did not have a differential impact on voters’ perceptions of the CDU’s direct candidate and the CDU as a whole, the estimates reported in the lower panel of Table 5 would still be consistent.

It may therefore be comforting to know that the triple-difference estimate of δ continues to be large, i.e. about 8 percentage points. Moreover, the precision of the estimates makes it extremely unlikely that the results in Table 5 are due to chance. Instead, it appears that at least some voters in District 160 misrepresented their preferences.

²⁶The municipality level data even allow for quadratic or cubic trends. The point estimates, however, remain almost unaffected.

4.3. *Strategic Voting?*

Table 6 exploits the precinct level nature of the data, and presents additional evidence in favor of this assertion. The evidence therein also indicates that supporters of parties other than the CDU misrepresented their preferences, and that at least some voters did so strategically.

First, however, columns (1) and (2), explore the by-election’s effect on turnout.²⁷ If the electorate was, indeed, aware of the peculiar incentives and reacted rationally, there should have been lower turnout in the by-election—after all the distribution of seats in parliament had, for the most part, been already determined.

In line with this prediction, overall turnout is estimated to be 3.9 percentage points lower. More importantly, the additional interaction term in column (2) indicates a larger effect for CDU partisans. To see this, note that voters who wish the CDU to gain an additional mandate should never cast their candidate vote for the rival SPD. Thus, the share of a precinct’s candidate vote accruing to the SPD can be interpreted as a proxy for the fraction of voters who do not support the CDU. By this measure, turnout is estimated to be higher among those who view the CDU unfavorably. Recall, CDU supporters could potentially cost their most preferred party a seat by voting for it, whereas supporters of other parties were not able to influence their parties’ seat totals. The results in columns (1) and (2) are, therefore, consistent with the view that some voters abstained for strategic reasons.²⁸

Columns (3) and (4) demonstrate that the FDP, the CDU’s traditional coalition partner, benefited from the by-election. Taking the coefficient in column (3) at face value, the FDP received an additional 6.5% of the list vote. By a similar argument as above, column (4) shows that the FDP’s gain was much lower, even negative, in precincts more critical of the CDU. This suggests that CDU partisans who did go to the polls cast their list votes for the FDP.

If supporters of the SPD fully grasped the situation and behaved strategically, then one might even expect them to attempt to hurt the CDU by voting for its list. While in regular election years there is a very strong negative correlation between a precinct’s SPD share of the candidate vote and its CDU share of the list vote, as evidenced by the large negative coefficient in the second row of column (6), this was not the case during the by-election in District 160. The positive interaction term in the third row of column (6) indicates that a significant number of individuals split their tickets between SPD candidate and CDU list.

²⁷The number of observations in columns (1) and (2) of Table 6 is lower than in the remaining columns because turnout cannot be calculated for precincts which handle only absentee ballots. Absentee voters are included in the turnout figure of the precinct in which they reside.

²⁸Of course, there exist alternative explanations for why overall turnout might have been lower during the by-election. For instance, individuals might derive less utility from the act of voting when the results of the election are already known. However, such “behavioral” explanations are incompatible with there being large partisan differences in the by-election’s effect on turnout.

Since neither sincere, nor strategic, CDU supporters would ever vote for the SPD candidate, these must have been agents who viewed the CDU unfavorably.

Along the same lines, the coefficients in column (7) shows a strong negative correlation between CDU list and Green Party candidate votes in regular election years. But this relationship becomes positive and large in 2005—exactly when an additional list vote might cost the CDU a seat in parliament.

The next column shows that a similar phenomenon can not only be observed among adherents of SPD and Green Party, but also among those of The Left. As explained in Section 2.2, The Left’s representatives explicitly instructed its supporters to refrain from casting tactical list votes, asking them to vote for The Left instead—just as in any other election year. Yet, the results in column (8) show that some of The Left’s followers chose to ignore their party’s request, and voted for the CDU instead. The evidence, therefore, suggests that adherents of opposition parties attempted to hurt the CDU by voting for it—despite the fact that none of these rival parties had instructed their followers to do so.

Moreover, the results in columns (9) and (10) show that the SPD received a higher than usual fraction of the candidate vote. Since the Green Party’s own candidate had essentially no chance of winning the direct mandate, and given that the Green Party is much closer in policy space to the SPD than the CDU, strategic Green Party supporters should choose the SPD candidate with their first vote. Consistent with this prediction the interaction term in the seventh row of column (10) indicates that the relationship between the Green Party’s share of the list vote and the SPD’s share of the candidate vote was substantially stronger during the by-election than in regular election years. It therefore appears that supporters of the Green Party did not only attempt to hurt the CDU by casting tactical list votes, but that they also behaved strategically with their candidate vote. Note well, this comes despite the Green Party’s refusal to ask its supporters to cast tactical ballots.

Taken together, these results suggest that agents did not merely follow official party positions as to how to cast their vote. At least some voters behaved strategically in the narrower sense of the word and cast tactical ballots out of their own volition. That is, they strategically misrepresented their preferences.

Additional support in favor of this conclusion comes from absentee voters.²⁹ After the death of the NPD direct candidate, new ballots were mailed to registered absentee voters in District 160 in order to ensure that they would be able to participate in the by-election. Critically, one would expect absentee voters to be less exposed and, therefore, less affected by

²⁹Absentee voters are individuals who were not physically present in District 160 on election day, or who were not physically able to appear at a polling station in person, i.e. elderly or handicapped citizens. Absentee ballots are generally cast by mail and are reported separately in the official data.

parties’ rallies, posters, and other campaign activities, all of which might affect the behavior of “regular” voters. Thus, looking at absentee voters provides another way to disentangle the impact of parties’ activities from individuals themselves being strategic.

Interestingly, estimating equation (4) on the set of absentee voters yields results that are qualitatively similar to those above. For instance, in the by-election absentee voters in District 160 were 3.99 percentage points more likely to vote for the CDU candidate than absentee voters in the control group. Not only is this estimate economically large (about 89% the size of its counterpart in Table 5), but given a standard error of .293 it is also statistically highly significant.

The empirical approach relying on the weakest identifying assumptions is the triple differencing specification in equation (5). The corresponding estimate for absentee voters is 2.19 percentage points (with a standard error of .492). Although this estimate is considerably smaller than that in Table 5, it implies that parties adapting their campaign strategies cannot be the whole story. Instead, the evidence suggests that at least some voters internalized the by-election’s strange incentives and strategically misrepresented their preferences.

4.4. *Sensitivity and Robustness Analysis*

Broadly summarizing, the results presented above indicate that parts of the electorate reacted to the perverse incentives in the by-election by casting misaligned votes. Supporters of the CDU either abstained or cast their list vote for the FDP, whereas adherents of the rival SPD appear to have voted for the CDU. This subsection explores the sensitivity and robustness of these results.

To this end, the upper panel in Table 7 probes the sensitivity of $\hat{\delta}$ with respect to different specifications. For comparison, the first row displays the baseline results, i.e. those from columns (1), (3), (5), (7), and (9) in Table 6. Successive rows vary the weighting scheme, the set of included controls, as well as the control group. The last row estimates the baseline model using vote shares calculated as the percentage of all eligible voters. Since turnout is itself affected by the reversal of incentives and the distribution of seats depends on the *number* of list votes, these estimates may be more informative for answering certain questions than ones based on ordinary vote shares.

Although individual point estimates do, of course, vary, $\hat{\delta}$ is qualitatively quite robust. Neither including different covariates, nor changing the control group, would alter the conclusions drawn above.³⁰

³⁰At first, it may seem preferable to include covariates even in the baseline specifications. Yet, this is not necessarily the case. Since the baseline specifications already account for municipality specific trends, there is very little variation left to identify the coefficients on these controls. In fact, for most of the control variable the estimates are statistically indistinguishable from zero. Moreover, given that very little variation

At first glance, there appear to be two exceptions, however. The CDU’s share of the candidate vote increases by only .8 percentage points when calculated as a fraction of all eligible voters, and the SPD’s gain is estimated to be much smaller than at baseline. Given that turnout is itself endogenous, this is hardly surprising. If supporters of all parties were more likely to abstain than in ordinary years, but adherents of the CDU did so disproportionately, then this alone may explain the differences.

Compositional effects, i.e. endogenous turnout, are an especially important concern as the by-election in District 160 was held on the Sunday of a three-day weekend, whereas the election in all other districts took place on a “regular” Sunday. Yet, compositional effects cannot explain why the CDU’s share of the list vote measured as percentage of *all* eligible voters declined by almost 6 percentage points when turnout itself declined by only 3.9 percentage points, and why the share of agents casting list votes for the FDP actually increased by more than 5 percentage points. Reconciling these estimates requires a theory of why CDU supporters would substitute toward the FDP.³¹

The lower panel in Table 7 performs robustness checks on the additional interaction terms included in columns (2), (4), (6), (7), and (10) of Table 6. Again, the first row shows the baseline results. As was the case for the main effects, the estimates appear to be quite robust.

5. Concluding Remarks

Whether, and if so to what extent, individuals cast strategic ballots is one of the most important questions at the intersection of economics and political science. This paper provides empirical evidence from a natural experiment in which a party benefited by receiving *fewer* votes. Comparing the behavior of constituencies facing different incentives, identification in this paper is not subject to the problems that plague most of the existing literature.

The evidence presented above points clearly toward a non-trivial incidence of preference misrepresentation, despite the fact that the by-election could at most have had a small effect on the overall distribution of seats. While agents might not vote according to their preferences for non-instrumental reasons, the evidence suggest that at least some voters did not merely follow official party recommendations, but misrepresented their preferences strategically.

Taking the estimates at face value, one can derive a lower bound on the fraction of agents who did not vote ideologically in the sense of Degan and Merlo (2009), i.e. who voted for

is used to identify the model’s coefficients, measurement error in the covariates becomes a first order concern. Unfortunately, without imposing strong assumptions it is not possible to sign the bias in δ resulting from measurement error in the covariates.

³¹In Germany, it is also possible to abstain with one vote without invalidating the other. However, there is no evidence that this kind of selective abstention was quantitatively important during the by-election.

a candidate or party other than the ideologically closest one. To do so, add the change in the FDP's share of the list vote and the change in the SPD's share of the candidate vote (both calculated as percentage of all eligible voters, cf. Table 7). While this number includes CDU supporters substituting toward the FDP and adherents of other parties supporting the SPD candidate, it does not capture SPD supporters casting their list votes for the CDU, nor does it encompass FDP adherents voting for the CDU candidate. Therefore, it is likely to understate the true extent of misaligned voting in the by-election.

Nevertheless, according to this measure (at least) 8.8% of the electorate misrepresented their preferences when faced with new, paradoxical incentives. Thus, the lower bound identified under relatively weak assumptions in this paper is among the largest of findings in the literature (most of which come from elections under plurality rule or from run-off elections). Especially compared to Kawai and Watanabe's 1.2%–2.7% estimate of misaligned voting, the lower bound derived for the by-election in District 160 appears to be very large. This may be surprising as electoral systems with (approximately) proportional representation are often believed to be relatively immune to strategic voting. In sum, the evidence in this paper suggests that the share of agents who do not vote ideologically may be much higher than previously thought.³²

Of course, it is difficult to say what the results from this, admittedly, unusual by-election imply for strategic voting under ordinary circumstances, or even in other electoral systems. One may, however, be willing to draw somewhat more general conclusions by focussing on the race for the outstanding direct mandate. As in elections under plurality rule elsewhere, voters in District 160 knew that the winner of the district level race would represent them in parliament, and that only two or three candidates were in contention for victory. Faced with this particular set of incentives, voters chose to misrepresent their preferences. Since this is essentially the same situation as in many countries around the world, it may not be unreasonable to expect similar behavior elsewhere.

Moreover, the results of this paper have potentially important implications for the design of electoral systems. If a significant number of voters do, indeed, misrepresent their true preferences, then the issue of strategy-proofness in social choice becomes more than a theoretical curiosity. With a non-trivial number of non-ideological voters, susceptibility to strategic ma-

³²Kawai and Watanabe (2013) estimate a 63%–85% extent of *strategic* voting. In the context of the by-election in District 160 one can derive an upper bound on the share of *strategic* voters, although it is unlikely to be tight. To do so note that individuals voting for the direct candidates of the FDP, Green Party, The Left, or the NPD cannot have cast strategic ballots, since these candidates were known to have had virtually no chance of winning the district, or being tied for first (see, for instance, the representative poll published nine days before the by-election in the *Sächsische Zeitung* 2005b). By this measure at most 69.2% of voters could have behaved strategically. Interestingly, this upper bound is somewhat smaller than the lower bound in Kawai and Watanabe (2013).

nipulation should be an important criterion by which to judge the performance of different mechanisms.

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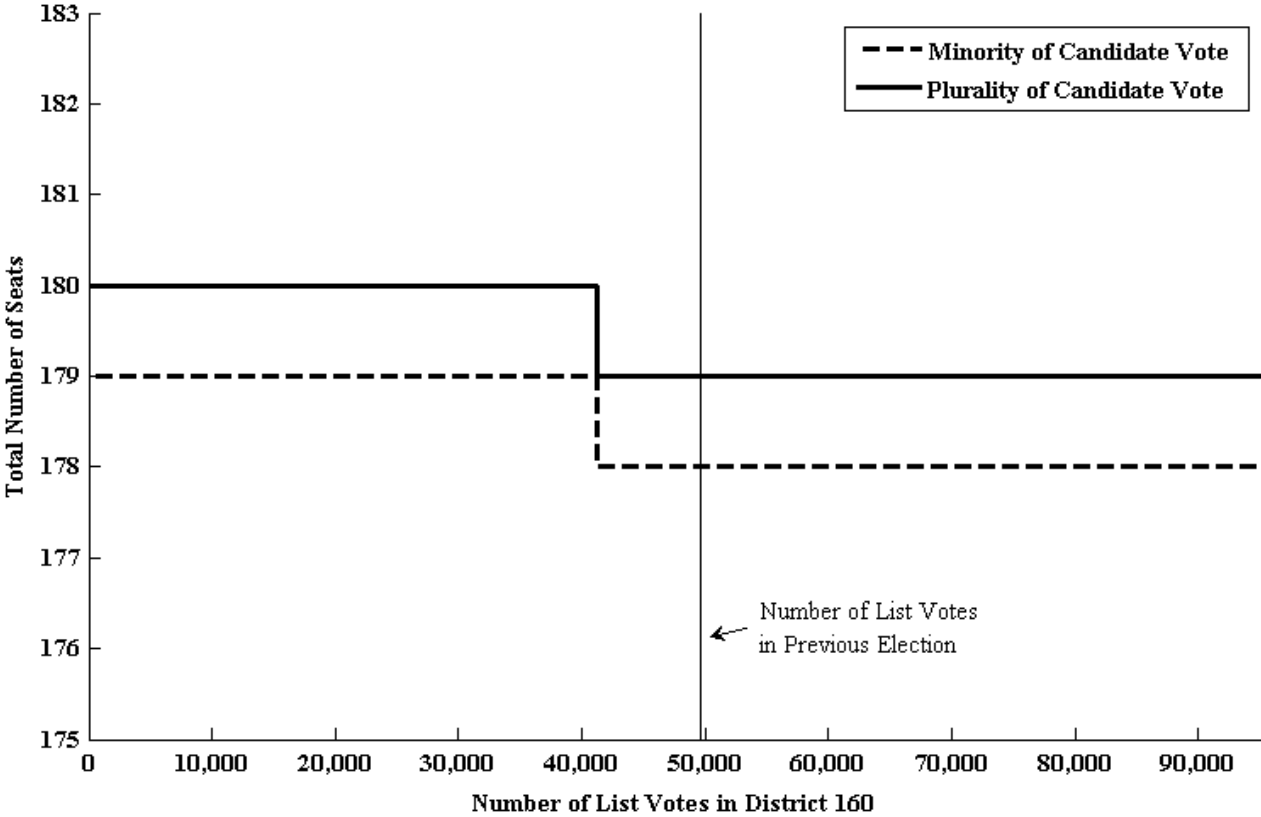
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Figure 1: Seats Accruing to the CDU in the 16th Bundestag as a Function of List Votes in District 160



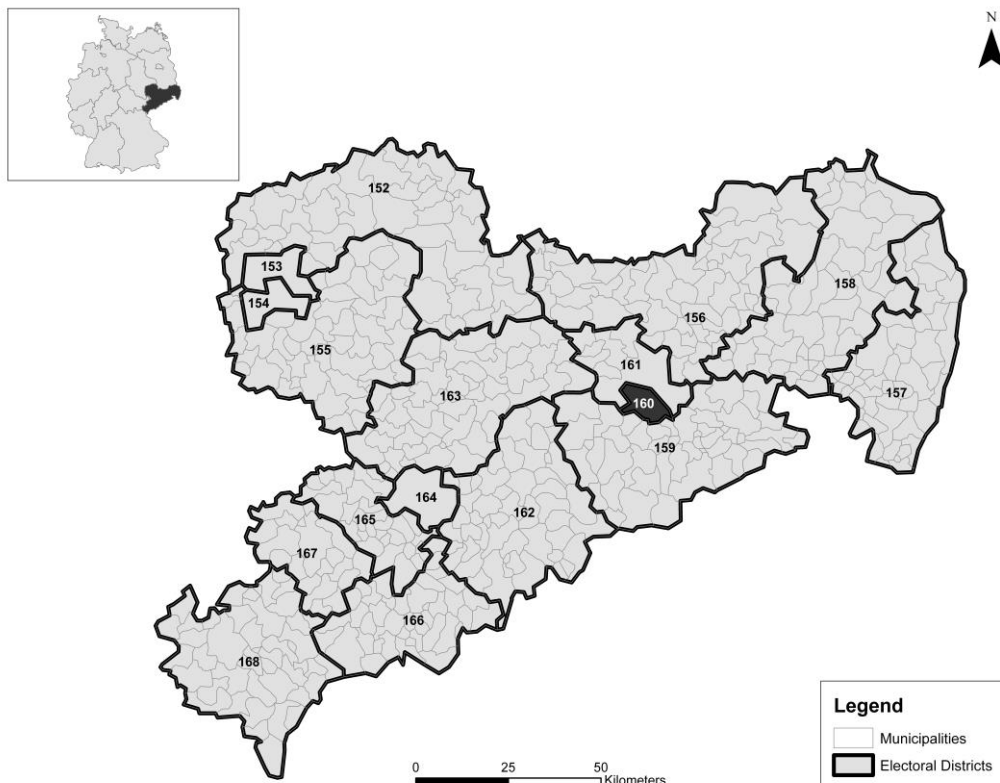
Notes: Figure shows the total number of seats accruing the CDU as a function of its number of list votes in District 160, and whether it wins the outstanding direct mandate. Although not essential for the location of the discontinuity or the general shape of the seat-vote curve, the figure is drawn under the assumption that all other parties receive a number of list votes similar to that in the 2002 election. Since the CSU won 46 seats, the total number of seats of the CDU/CSU faction could range from 224 to 226.

Figure 2: Campaign Poster Used by the Free Democratic Party



Notes: The text translates to "Candidate Vote CDU", "List Vote FDP", and "Typical Dresden ;-) Black & Yellow. Good for Germany!"

Figure 3: Electoral Districts in the State of Saxony as of the 2005 Elections to the Bundestag



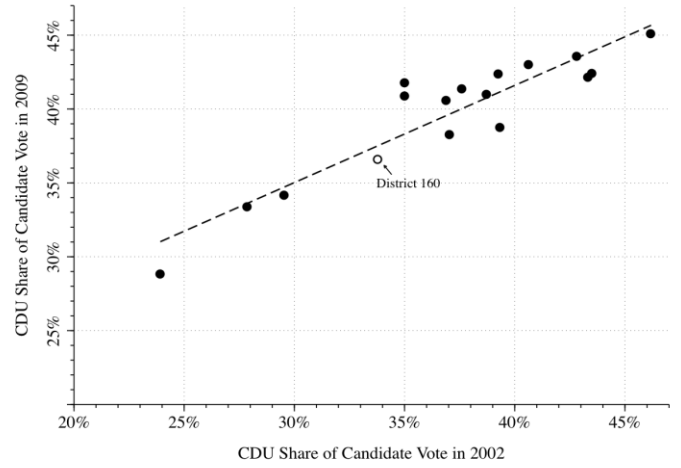
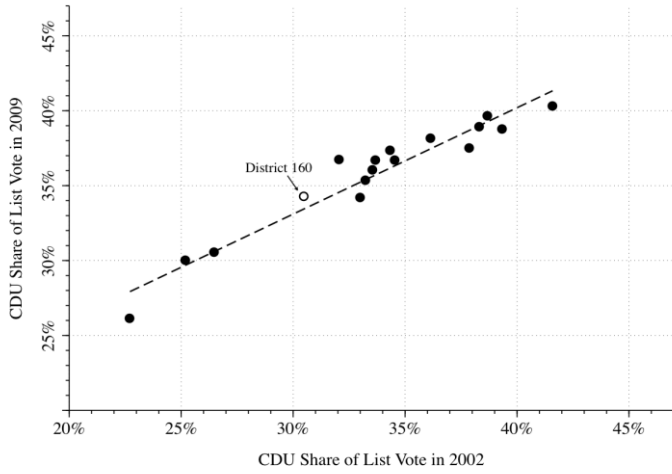
Sources: Based on Bundeswahlleiter (2005b) and Landesvermessungsamt Sachsen (2006).

Figure 4: Share of List and Candidate Votes Accruing to the CDU in the State of Saxony

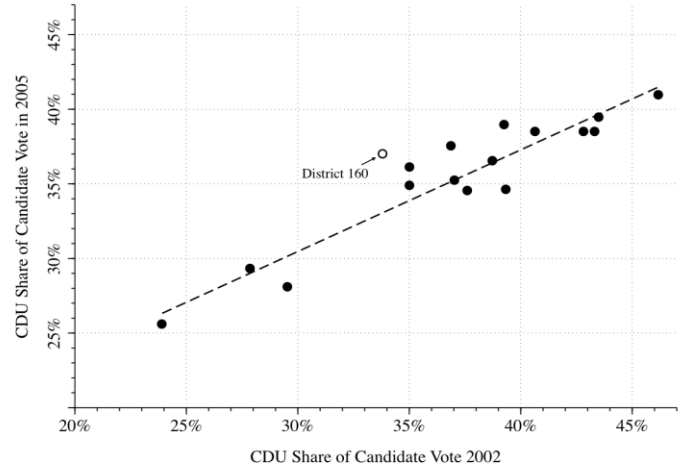
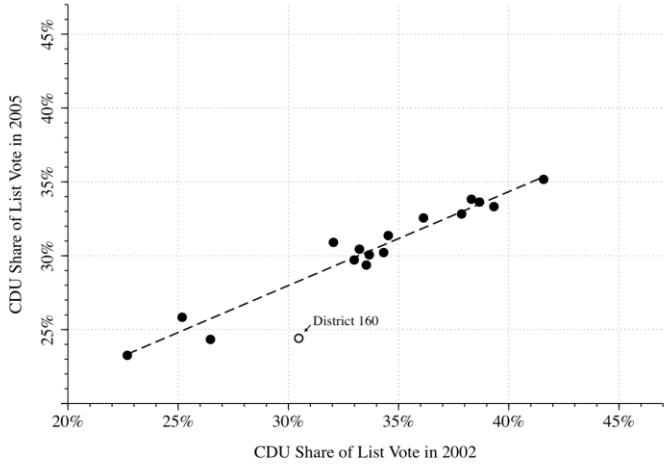
I. List Vote

II. Candidate Vote

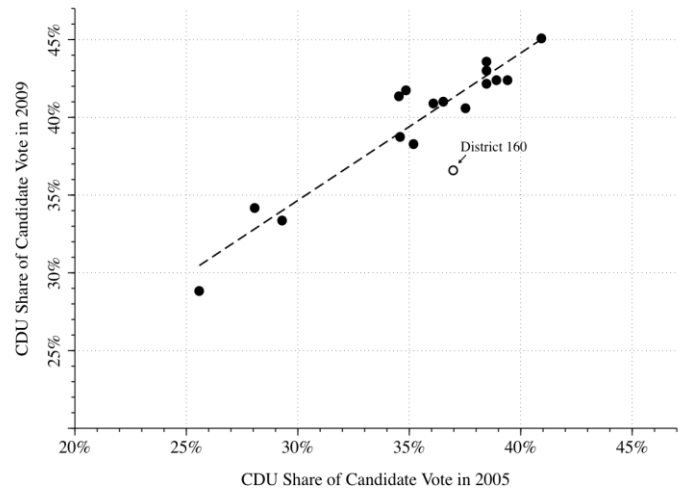
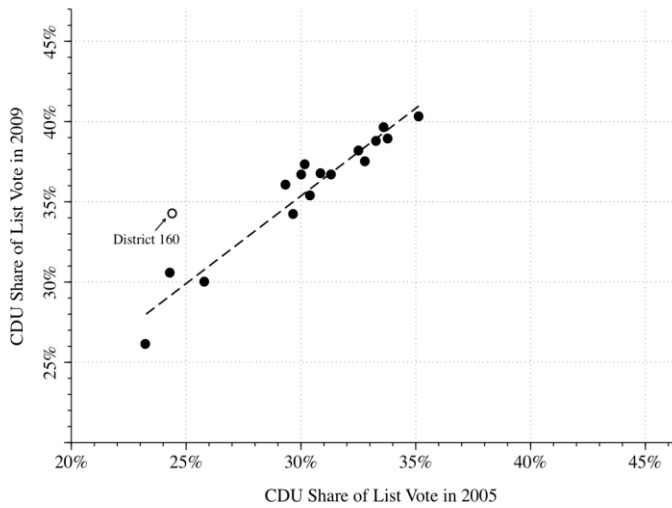
A. 2002 & 2009 Federal Elections



B. 2002 & 2005 Federal Elections



C. 2005 & 2009 Federal Elections



Notes: Graphs on the left plot the CDU's share of the list vote in one election year against its share in a subsequent election. Graphs on the right use the CDU's share of the candidate vote instead. Each dot represents an electoral district in the state of Saxony.

Table 1: List Votes Accruing to the CDU in the 2002, 2005 and 2009 Elections to the Bundestag

A. List Vote

	Share of List Vote			Δ Share of List Vote	
	2002	2005	2009	2005 – 2002	2009 – 2005
Electoral District 160	30.5%	24.4%	34.3%	-6.1%	9.9%
Other Districts in Saxony	33.8%	30.4%	35.7%	-3.4%	5.3%
Other States in East Germany	27.0%	24.0%	27.4%	-3.0%	3.4%

B. Candidate Vote

	Share of Candidate Vote			Δ Share of Candidate Vote	
	2002	2005	2009	2005 – 2002	2009 – 2005
Electoral District 160	33.8%	37.0%	36.6%	3.2%	-0.4%
Other Districts in Saxony	37.3%	35.4%	39.7%	-1.8%	4.3%
Other States in East Germany	28.6%	27.2%	29.6%	-1.5%	2.4%

Sources: Author's calculations based on Bundeswahlleiter (2002a, 2005a, 2009a).

Table 2: Determining the Number of Seats for the CDU in the 16th Bundestag

Step 1: Proportional Allocation of List Mandates to Parties

Party	Preliminary Result (Excluding District 160)			Final Result		
	Number of List Votes	Equivalent Number of Seats	Rounded Number of Seats	Number of List Votes	Equivalent Number of Seats	Rounded Number of Seats
SPD	16,148,240	213.307	213	16,194,665	213.170	213
CDU	13,096,556	172.996	173	13,136,740	172.919	173
CSU	3,494,564	46.161	46	3,494,309	45.996	46
Green Party	3,826,194	50.541	51	3,838,326	50.524	51
FDP	4,619,519	61.021	61	4,648,144	61.184	61
The Left	4,086,134	53.975	54	4,118,194	54.208	54
Total	45,271,207	598.000	598	45,430,378	598.000	598

Step 2: Proportional Allocation of Mandates to State Lists (CDU)

State	Preliminary Result (Excluding District 160)			Final Result		
	Number of List Votes	Equivalent Number of Seats	Seats under Proportionality	Number of List Votes	Equivalent Number of Seats	Seats under Proportionality
Schleswig-Holstein	623,922	8.242	8	624,510	8.224	8
Hamburg	272,798	3.604	4	272,418	3.588	4
Lower Saxony	1,599,867	21.134	21	1,599,947	21.070	21
Bremen	82,411	1.089	1	82,389	1.085	1
North Rhine-Westphalia	3,524,374	46.555	47	3,524,351	46.413	46
Hesse	1,130,099	14.928	15	1,131,496	14.901	15
Rhineland-Palatinate	877,213	11.588	12	877,632	11.558	12
Baden-Württemberg	2,282,729	30.154	30	2,283,085	30.066	30
Saarland	191,065	2.524	2	191,067	2.516	3
Berlin	408,809	5.400	5	408,715	5.382	5
Mecklenburg-Vorpommern	293,278	3.874	4	293,316	3.863	4
Brandenburg	322,394	4.259	4	322,400	4.246	4
Saxony-Anhalt	357,638	4.724	5	357,663	4.710	5
Thuringia	372,593	4.922	5	372,435	4.905	5
Saxony	757,366	10.004	10	795,316	10.474	10
Total	13,096,556	173.000	173	13,136,740	173.000	173

Step 3: Determination of the Actual Number of Seats (CDU)

State	Preliminary Result (Excluding District 160)			Final Result		
	Number of Direct Mandates	Seats under Proportionality	Actual Number of Seats	Number of Direct Mandates	Seats under Proportionality	Actual Number of Seats
Schleswig-Holstein	6	8	8	6	8	8
Hamburg	0	4	4	0	4	4
Lower Saxony	4	21	21	4	21	21
Bremen	0	1	1	0	1	1
North Rhine-Westphalia	24	47	47	24	46	46
Hesse	8	15	15	8	15	15
Rhineland-Palatinate	10	12	12	10	12	12
Baden-Württemberg	33	30	33	33	30	33
Saarland	0	2	2	0	3	3
Berlin	1	5	5	1	5	5
Mecklenburg-Vorpommern	3	4	4	3	4	4
Brandenburg	0	4	4	0	4	4
Saxony-Anhalt	0	5	5	0	5	5
Thuringia	3	5	5	3	5	5
Saxony	13	10	13	14	10	14
Total	105	173	179	106	173	180

Notes: The table shows the calculations in determining the CDU's number of seats in the 16th Bundestag. The columns on the left are based on the preliminary results of the 2005 election, i.e. before the constituency of District 160 voted, whereas the columns on the right use the final vote counts in all districts. As explained in the main text, Step 1 calculates the number of parties' list mandates on the national level. Step 2 assigns these to party lists in individual states, and Step 3 determines the actual number of seats by taking the maximum of list and direct mandates. Rounding in Step 1 and 2 is done according to the method of Hare-Niemeyer.

Sources: Based on Bundeswahlleiter (2005a, 2005c).

Table 3: Summary Statistics of District and Precinct Level Variables for the 2002, 2005 and 2009 Elections to the Bundestag

	Full Sample		Electoral District 160		Other Districts in Saxony	
	Mean	SD	Mean	SD	Mean	SD
District Level Variables:						
Population (in 1,000)	260.7	(22.5)	272.1	(7.3)	260.0	(23.0)
Population Density (residents per km ²)	656.6	(879.8)	3,330	(70.5)	476.5	(549.4)
Population Growth (per 1,000 residents)	-6.642	(7.139)	4.967	(7.836)	-7.424	(6.461)
Number of Cars (per 1,000 residents)	589.4	(76.7)	477.5	(31.5)	596.9	(72.9)
Percent of Labor Force in Manufacturing	33.63	(7.51)	22.52	(1.56)	34.34	(7.16)
Percent of Labor Force in Service Industry	63.66	(8.61)	76.82	(1.78)	62.81	(8.17)
Unemployment Rate (in %)	17.31	(3.21)	14.20	(2.05)	17.53	(3.18)
Precinct Level Variables:						
Share of Candidate Vote (in %):						
CDU	37.25	(8.17)	35.75	(5.02)	37.35	(8.33)
SPD	24.66	(9.19)	26.79	(7.68)	24.52	(9.27)
FDP	7.76	(3.50)	6.57	(2.64)	7.84	(3.54)
The Left	21.28	(5.93)	21.35	(5.23)	21.27	(5.97)
Green Party	4.28	(3.43)	6.07	(3.74)	4.16	(3.38)
Others	4.78	(2.98)	3.47	(1.55)	4.86	(3.03)
Share of List Vote (in %):						
CDU	32.92	(7.41)	29.71	(6.09)	33.14	(7.44)
SPD	24.57	(9.06)	25.62	(7.82)	24.50	(9.13)
FDP	10.10	(3.71)	12.23	(5.40)	9.96	(3.52)
The Left	21.02	(6.05)	19.80	(5.18)	21.10	(6.10)
Green Party	5.30	(4.11)	8.60	(4.46)	5.08	(3.99)
Others	6.09	(2.78)	4.05	(1.67)	6.22	(2.78)
Number of Eligible Voters	1,112	(391)	1,170	(238)	1,109	(398)
Turnout (in %)	72.29	(7.58)	73.12	(7.48)	72.24	(7.59)
Absentee Precinct	.144	(.351)	.233	(.423)	.138	(.345)
Number of Districts	17		1		16	
Number of Observations	13,107		774		12,363	

Notes: Entries are weighted means and standard deviations of district and precinct level variables, pooled across years. Only observations with non-missing information are used in the calculations. See the Data Appendix for the precise definition and source of each variable

Table 4: Evidence of Preference Misrepresentation

<i>A. District Level Regressions</i>	CDU Share of List Vote			CDU Share of Candidate Vote		
	(1)	(2)	(3)	(4)	(5)	(6)
District 160 × 2005	-8.700 (.975)	-5.995 (.855)	-3.560 (.417)	-.261 (1.140)	1.537 (1.088)	4.918 (.430)
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
District Fixed Effects	No	No	Yes	No	No	Yes
R-Squared	.071	.270	.960	.000	.106	.943
Number of Observations	51	51	51	51	51	51
<i>B. Precinct Level Regressions</i>						
	CDU Share of List Vote			CDU Share of Candidate Vote		
	(7)	(8)	(9)	(10)	(11)	(12)
District 160 × 2005	-8.715 (.966)	-6.007 (.829)	-3.568 (.329)	-.266 (1.129)	1.535 (1.055)	4.921 (.340)
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
District Fixed Effects	No	No	Yes	No	No	Yes
R-Squared	.028	.107	.381	.000	.039	.345
Number of Observations	13,107	13,107	13,107	13,107	13,107	13,107

Notes: Entries are coefficients and standard errors on δ , obtained by estimating equation (3) using weighted least squares with weights corresponding to the number of voters. The respective dependent variables are listed at the top of each column. The upper panel uses only district level data, whereas the lower one relies on individual polling precincts as the level of observation. See the Data Appendix for the precise definitions and sources of all variables.

Table 5: Accounting for Trends

A. List Vote

	CDU Share of List Vote					
	(1)	(2)	(3)	(4)	(5)	(6)
District 160 × 2005	-3.568 (.329)	-3.473 (.348)	-3.459 (.288)	-4.623 (.494)	-4.364 (.298)	-4.066 (.373)
Unit of Observation	Precinct	Precinct	Precinct	Municipality	Municipality	Municipality
Time Period	2002–2009	2002–2009	2002–2009	1994–2009	1994–2009	1994–2009
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	Yes	Yes	No
District Specific Trends	No	Yes	No	No	Yes	No
Municipality Fixed Effects	No	No	Yes	No	No	Yes
Municipality Specific Trends	No	No	Yes	No	No	Yes
R-Squared	.381	.389	.616	.737	.758	.948
Number of Observations	13,107	13,107	13,107	3,404	3,404	3,404

B. Candidate Vote

	CDU Share of Candidate Vote					
	(7)	(8)	(9)	(10)	(11)	(12)
District 160 × 2005	4.921 (.340)	4.925 (.363)	4.479 (.430)	3.619 (.432)	4.169 (.306)	4.079 (.311)
Unit of Observation	Precinct	Precinct	Precinct	Municipality	Municipality	Municipality
Time Period	2002–2009	2002–2009	2002–2009	1994–2009	1994–2009	1994–2009
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	Yes	Yes	No
District Specific Trends	No	Yes	No	No	Yes	No
Municipality Fixed Effects	No	No	Yes	No	No	Yes
Municipality Specific Trends	No	No	Yes	No	No	Yes
R-Squared	.345	.360	.621	.672	.689	.924
Number of Observations	13,107	13,107	13,107	3,404	3,404	3,404

C. Difference Between CDU Share of Candidate Vote and List Vote

	Difference Between CDU Share of Candidate Vote and List Vote					
	(13)	(14)	(15)	(16)	(17)	(18)
District 160 × 2005	8.488 (.265)	8.398 (.270)	7.938 (.373)	8.241 (.280)	8.533 (.254)	8.145 (.398)
Unit of Observation	Precinct	Precinct	Precinct	Municipality	Municipality	Municipality
Time Period	2002–2009	2002–2009	2002–2009	1994–2009	1994–2009	1994–2009
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	Yes	Yes	No
District Specific Trends	No	Yes	No	No	Yes	No
Municipality Fixed Effects	No	No	Yes	No	No	Yes
Municipality Specific Trends	No	No	Yes	No	No	Yes
R-Squared	.274	.311	.550	.341	.366	.645
Number of Observations	13,107	13,107	13,107	3,404	3,404	3,404

Notes: Entries are coefficients and standard errors on δ , obtained by estimating equations (3), (4), and (5) using weighted least squares with weights corresponding to the number of voters. The respective dependent variables are listed at the top of each column. See the Data Appendix for the precise definitions and sources of all variables.

Table 6: Strategic Voting?

	Turnout		FDP Share of List Vote		CDU Share of List Vote				SPD Share of Candidate Vote		CDU Share of Candidate Vote		CDU Share of Candidate Vote	Difference in CDU Share of Candidate and List Vote
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
District 160 × 2005	-3.857 (.610)	-16.499 (1.270)	6.500 (.378)	20.918 (.514)	-3.459 (.288)	-14.735 (1.225)	-9.275 (1.244)	-11.155 (1.540)	7.773 (.378)	7.266 (.661)	4.479 (.430)	21.858 (.376)	3.991 (.293)	2.192 (.492)
SPD Share of Candidate Vote		-.133 (.044)		-.224 (.014)		-.726 (.052)								
District 160 × 2005 × SPD Share of Candidate Vote		.430 (.044)		-.395 (.014)		.527 (.052)								
Green Party Share of Candidate Vote														
District 160 × 2005 × Green Party Share of Candidate Vote														
The Left Share of Candidate Vote														
District 160 × 2005 × The Left Share of Candidate Vote														
Green Party Share of List Vote														
District 160 × 2005 × Green Party Share of List Vote														
CDU Share of List Vote														
District 160 × 2005 × CDU Share of List Vote														
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Included Voters	All	All	All	All	All	All	All	All	All	All	All	All	Absentee	Absentee
R-Squared	.507	.510	.634	.680	.616	.717	.641	.737	.872	.876	.621	.940	.663	.582
Number of Observations	11,512	11,512	13,107	13,107	13,107	13,107	13,107	13,107	13,107	13,107	13,107	13,107	1,595	1,595

Notes: Entries are coefficients and standard errors from estimating models analogous to equation (4) using weighted least squares with weights corresponding to the number of voters. The respective dependent variables are listed at the top of each column. Individual polling precincts are the level of observation. Columns (1) – (12) use information on all voters, whereas columns (13) and (14) restrict attention to absentee voters. See the Data Appendix for the precise definitions and sources of all variables.

Table 7: Sensitivity and Robustness Analysis

<i>A. Main Effects</i>					
Sample / Specification	CDU Share of List Vote	CDU Share of Candidate Vote	Turnout	FDP Share of List Vote	SPD Share of Candidate Vote
Baseline	-3.459 (.288)	4.479 (.430)	-3.857 (.610)	6.500 (.378)	7.773 (.378)
Unweighted	-3.982 (.298)	4.384 (.537)	-3.830 (.555)	6.888 (.382)	8.076 (.401)
Controlling for CDU Share of List Votes in Previous Election	-3.584 (.404)	4.873 (.426)	-3.748 (.690)	7.005 (.340)	5.887 (.402)
Controlling for District Level Covariates	-4.335 (.710)	4.702 (.788)	-3.141 (.476)	7.418 (.316)	5.174 (.744)
Geographically Constant Districts	-4.768 (1.097)	4.347 (.970)	-4.199 (.827)	7.581 (.420)	6.108 (.586)
Control Group:					
Other Districts in East Germany	-4.931 (.202)	3.543 (.293)	-2.830 (.221)	7.374 (.090)	5.439 (.433)
All Other Districts in Germany	-6.720 (.101)	1.173 (.140)	-.594 (.081)	7.654 (.060)	4.437 (.126)
As Percentage of All Eligible Voters	-5.801 (.171)	.765 (.200)	--	5.108 (.243)	3.657 (.332)
<i>B. Additional Interaction Terms</i>					
Sample / Specification	Column (2)	Column (4)	Column (6)	Column (7)	Column (10)
Baseline	.430 (.044)	-.395 (.014)	.527 (.052)	1.044 (.098)	.141 (.031)
Unweighted	.426 (.033)	-.419 (.014)	.521 (.038)	1.038 (.087)	.138 (.026)
Controlling for CDU Share of List Votes in Previous Election	.432 (.044)	-.397 (.016)	.574 (.050)	1.044 (.098)	.137 (.031)
Controlling for District Level Covariates	.430 (.047)	-.393 (.015)	.559 (.050)	1.053 (.103)	.135 (.031)
Geographically Constant Districts	.267 (.113)	-.416 (.027)	.479 (.030)	1.001 (.069)	.133 (.039)
Control Group:					
Other Districts in East Germany	.507 (.027)	-.468 (.012)	.442 (.050)	.984 (.103)	.174 (.054)
All Other Districts in Germany	.523 (.012)	-.472 (.007)	.635 (.011)	1.294 (.059)	.079 (.030)
As Percentage of All Eligible Voters	--	-.120 (.011)	.439 (.040)	.390 (.096)	.320 (.021)

Notes: Entries in the upper panel are coefficients and standard errors on δ , obtained by estimating equation (4) using least squares. Entries in the lower panel correspond to the additional interaction terms added in columns (2), (4), (6), (7), and (10) in Table 6. The respective dependent variables are listed at the top of each column, and the relevant sample restriction or change in the set of controls is denoted on the left of each row. All specifications, except for those including districts outside the state of Saxony, contain year fixed effects, municipality fixed effects, municipality specific linear trends, and, if applicable, indicator variables for missing covariates. Regressions including districts outside the state of Saxony contain year fixed effects, district fixed effects and district specific linear trends instead. See the Data Appendix for the precise definitions and sources of all variables.