How to increase turnout in low-salience elections

Quasi-experimental evidence on the effect of concurrent second-order elections on political participation. *

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Abstract

Voter turnout in second-order elections is on a dramatic decline in many modern democracies. This article investigates how electoral participation can be substantially increased by holding multiple of these less important elections concurrently. Leading to a relative decrease in voting costs, concurrent elections theoretically have economies of scale to the individual voter and thus should see turnout levels larger than those obtained in any stand-alone election. Leveraging as-if-random variation of local election timing in Germany, we estimate the causal effect of concurrent mayoral elections on European Election turnout at around ten percentage points. Exploiting variation in treatment intensity, we show that the magnitude of the concurrency effect is contingent upon district size and the competitiveness of the local race.

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1 Introduction

When faced with low levels of political participation, the legitimacy of political institutions is regularly questioned (Scully, Jones, and Trystan, 2004). Low turnout rates are considered as a 'serious democratic problem' by politicians and political scientists (Lijphart, 1997) alike. Especially second-order elections (Reif and Schmitt, 1980), elections which do not serve the function of electing a head of government, have seen a dramatic decline in turnout in recent decades in many modern democracies. For example, the overall turnout rate for European Parliament elections (EE) decreased from 62% in 1979 to 43% in 2014, with levels as low as 13% in some member states – despite an increase in the formal powers of the institution.

Such low-turnout elections, in which only a minority of voters participate, are also less likely to lead to electoral outcomes that are representative of the political preferences of the entire electorate. While some studies report negligible effects of turnout variation on electoral outcomes (Ferwerda, 2014; Lutz and Marsh, 2007), large shifts have been noted in various contexts (Artés, 2014; Bechtel, Hangartner, and Schmid, 2015; Finseraas and Vernby, 2014). As Lijphart (1997) argued, it is thus important to design institutions in a way that turnout levels are maximized in order to guarantee equal influence of all citizens – he therefore calls for a combination of second-order with first-order elections. Electoral research has consistently found a substantial increase in turnout (see for an overview Geys, 2006), as turnout for the less important election increases to the level of the concurrent first-order elections. But beyond that, there is surprisingly little evidence on the electoral effects of concurrency.

This paper systematically analyzes the turnout effect of concurrent second-order elections (CSOE). We argue that theoretically combining multiple second-order elections should also lead to a substantial increase in turnout, beyond the levels obtained in any counterfactual stand-alone election. Our focus is on a particularly interesting case of concurrency: How is political participation influenced, if the elections for the two most distant levels of government, European Parliamentary elections (EE) and local elections, are held on the same day? We bring a rigorous research design to bear on this question by exploiting partially overlapping electoral cycles as a quasi-experimental treatment condition. In the German state of Lower Saxony we find a closest-to-ideal case of study, where the 2014 European Parliamentary election was held concurrently with local mayoral elections in some municipalities, and not in others.

We find that the concurrency effect of local elections on EE turnout is substantial, on average around 10 percentage points. Furthermore, we show that the turnout effect depends on the nature of the local mayoral election that the EE is combined with. For municipalities that receive a more intense treatment, i.e. by holding a competitive mayoral election in a small village, we find EE turnout to increase by 18 percentage points. Less attractive mayoral elections, such as uncontested races in larger districts, increase EE turnout only marginally. We also provide evidence for the external validity of our causal estimates by analyzing state-level EE turnout in Germany between 1979

and 2014. We find that EE turnout in states that held concurrent state-wide local legislative elections is very consistently over 10 percentage points higher. Finally, we provide indicative evidence that the increase in turnout likely stems from two different electorates that are drawn to the polls, one primarily interested in (singular) local and one interested in (singular) EE elections.

Our findings add to the literature on the relevance of election timing effects. While a positive effect of concurrency has been noted in the past, we are able to address endogeneity concerns that potentially bias results found so far in the literature (e.g. Mattila, 2003; Schakel and Dandoy, 2014) because the timing of concurrent elections is prone to be strategic (Meredith, 2009). In combination with evidence provided by Fauvelle-Aymar and François (2015) on French regional elections and Schmid (2015) on cantonal elections and concurrent referenda in Switzerland, our results indicate that CSOE should 'work' in a wide variety of contexts.

Our contribution does not only inform the narrow field of electoral timing research, but also adds to the broader turnout literature that is concerned with the effect of voting costs (Haspel and Knotts, 2005; Hershey, 2009; Hodler, Luechinger, and Stutzer, 2015; Rallings, Thrasher, and Borisyuk, 2003) and voter pivotality and electoral competitiveness (Cox and Munger, 1989; Endersby, Galatas, and Rackaway, 2002; Kirchgässner and Meyer zu Himmern, 1995; Shachar and Nalebuff, 1999) on turnout. Furthermore, our finding have direct relevance for the ongoing debate on policy measures to increase turnout. Combining multiple "less important" elections is a simple, yet very effective tool to increases turnout substantially in either of them.

2 Why do concurrent elections increase turnout?

2.1 What we know so far

It is a well-established finding of electoral research that turnout in less important, second-order elections increases when these are combined with first-order elections. Evidence stems from a wide range of elections (for an overview see Geys, 2006). In the United States, turnout in US gubernatorial elections increases if they are held together with presidential elections (Boyd, 1989). In European countries, turnout in local or regional election increases if these elections are combined with general national elections (Schakel and Dandoy, 2014; Vetter, 2015). Much less is known about the turnout effect of combining two second-order elections, where turnout is relatively low in both instances. At the regional (Mattila, 2003; Schakel and Dandoy, 2014) and municipal level (Rallings and Thrasher, 2005; Vetter, 2015) a concurrency effect for second-order elections has been noted.

However, much of the literature on the turnout effect of simultaneous elections lacks analytical rigor. First of all, that concurrency increases turnout is all too often treated as a self-evident truth. There is no well-established explicit theoretical model of turnout in multiple elections. Accordingly, the empirical strategy employed by most of the

contributions is limited to multivariate analyses of turnout levels, where concurrency is treated as "just another dummy variable". Confounding factors, such as selection into concurrency, are barely addressed. Reported estimates are therefore prone to omitted variable bias and specification issues, especially in cross-national research.

To the best of our knowledge, there are only three articles that have addressed the turnout effect of concurrency with a causal identification strategy. Fowler (2015) analyzes the effect of concurrent presidential elections on turnout in gubernatorial elections arguing that their overlap is quasi-random. He finds a sizable concurrency effect of 17 percentage points; but as presidential elections are first-order elections such a large effect is to be expected.[1] Most relevant for our research question is the contribution of Fauvelle-Aymar and François (2015), on participation in French regional elections. These take place every six years. Elections in the departments, a tier of government below the region, take place every 3 years in half of the cantons - similar to the electoral calendar of the US House of Representatives. The assignment of cantons to the two groups is random. Conducting difference-in-means tests, Fauvelle-Aymar and Francois document a concurrency effect of departmental elections of around four percentage points. Lastly, a working paper by Schmid (2015) analyzes state-level elections in Switzerland with concurrent federal referendums. Schmid argues that strategic scheduling is unlikely and thus referendum turnout exogenous to cantonal election timing. Using individual level and aggregate data from voting records, he finds a substantial concurrency effect on turnout of around 8.5 percentage points.

2.2 The Calculus of Voting under Concurrency

We extend the canonical Riker and Ordeshook (1968) model to analyze the turnout effect of simultaneous elections. The Riker-Ordeshook model conceptualizes individual turnout decisions in a singular election as a cost-benefit calculus of the form $R = pB + D - C$. R is the individual's expected benefit from turning out, which depends on the benefit derived from the election's result (B), multiplied by the probability of being the decisive voter (p). An individual gains additional satisfaction from fulfilling her civic duty or taste for voting (D). Finally, expected benefit decreases with participation costs (C). If two elections are held on the same day, the model can be extended by separating the terms into election-specific components. This amounts to the idea that voters gain benefits and incur costs that are specific to casting a vote in the European election (subscript e), and specific to casting a vote in the local election (subscript l).

$$R = p_e B_e + D_e + p_l B_l + D_l - C\,;\; C = F + v_e + v_l$$

Costs C can be additionally divided into fixed costs F (costs that are unaffected by the additional election) and variable costs v (costs that increase with the increasing number of elections) (see also Fauvelle-Aymar and François, 2015). F are primarily monetary costs of transportation and opportunity costs of the time spent during trans-

[1] Fowler's main interest is in estimating the political preferences of marginal voters. Towards this end, in a second-step, he uses concurrency as an instrument for turnout to estimate a turnout effect on electoral results finding Democrats to profit from higher turnout.

portation.[2] Variable costs are costs of collecting information about the specific election, and the time costs of filling out election-specific ballots. Since fixed costs are only incurred once for taking part in two elections, participation in concurrent elections has 'economies of scale' to the individual voter (Aldrich, 1993, p. 261).

Having established the notation, we can define the circumstances under which concurrency increases turnout, compared to a singular European election. Turnout in an EE increases if the benefits of the additional local election are larger than its additional variable costs, i.e. if $p_l B_l + D_l > v_l$. Moreover, if voting is not compulsory in any of the elections, there is a mechanism that assures that voters can not be deterred by additional elections, i.e. that $p_l B_l + D_l - v_l \geq 0$. Voters whose additional variable costs are larger than their additional benefit can simply avoid incurring additional costs by not casting a vote in the additional election. This is done by casting a blank ticket in the local election, or invalidating the local ballot.[3] Another, and much more common strategy to deal with high election-specific information costs are informational shortcuts and heuristics, such as party identification or national-level party preferences. This has been discussed in the context of cross-ballot and cross-election contamination or interaction effects (Ferejohn and Calvert, 1984; Ferrara, Herron, and Nishikawa, 2005; Herron and Nishikawa, 2000). No matter whether voters cast invalid or 'contaminated' votes to deal with high information costs, these 'opt-out' options assure that no rational voter can be deterred from participation by the fact that an additional election takes place.[4] In turn, this means that turnout in concurrent elections should never fall below (counterfactual) turnout in singular elections.

Election-specific benefits and costs of course vary greatly between voters. While some voters are primarily motivated to participate in the European election, others see more benefit in participating in the local election. Based on the different sum of benefits and cost perceptions, four representative voter ideal types can be identified, that are relevant for an analysis of turnout in concurrent second-order elections (see Figure 1).[5] For the sake of illustration, consider voters to turn out based on the summary benefits, relative to a constant cost threshold. Voter A will vote in the European elections irrespective of whether there is a concurrent local election but will note vote in a singular local election. Voter B does not turn out, even in concurrent elections, since the sum of benefits does not outweigh costs. Voter C would not participate in any singular EE, but will in concurrent elections, as the benefit derived from voting in the local election pushes her above the participation threshold. Voter D assigns a benefit high enough to vote in local elections, irrespective of European elections, but would not participate in a singular EE.

As outlined above, the conditions under which concurrent local elections do *not* increase turnout are very strict. First of all, if voters follow a rational calculus, the

[2] Postal votes exhibit similar fixed costs for voters in Germany, as only one application is required to obtain both ballot papers, and voters can use the same envelope to sent back both ballots.

[3] The share of blank or invalid votes is shown to increase in concurrent elections (Schmid, 2015).

[4] Absent of irrational, psychological costs of casting a blank or invalid ticket, or casting an informed vote.

[5] There is also a fifth voter ideal type, a habitual voter who will always vote in any of the two elections, and thereby plays no role in the CSOE effect.

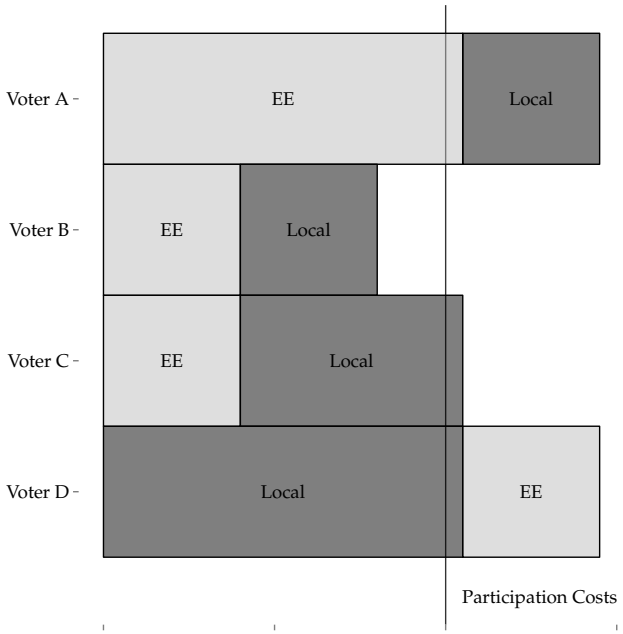

Figure 1: *Illustration of benefits and costs calculus in concurrent elections. Voter A always votes in EE irrespective of CSOE. Voter B never votes irrespective of CSOE. Voter C only votes in case of CSOE. Voter D always votes in local elections but only in case of CSOE does she vote in EE. Not shown here is an additional voter type E that always votes, irrespective of election type.*

likelihood of turnout can not be decreased for any voter by the fact that another election takes place on the same day. Secondly, the electorate needs to be composed only of specific voter types for concurrency to not have a positive turnout effect. Only if the electorate consists exclusively of voters of type A and B will concurrent elections not have an effect on turnout rates in European elections. As these conditions are very unlikely to be fulfilled in any real-world election, we should expect turnout to always increase if additional elections are held on the same day, and if the additional elections are not negligibly important to voters. In our case, since voters assign some notable importance to the office of mayor, we expect a substantial increase in EE participation due to simultaneously held local elections.

2.3 District-level variation

Apart from individual-level variations in the turnout calculus, there is also systematic variation between local units - due to the specific characteristics of the local elections. While all voters vote in the same nation-wide electoral district in the European election, voters from different localities experience different electoral circumstances in the local election. Thus, we expect the specific features of the local election to have a systematic influence on the cost-benefit calculus of voters, and in turn the turnout effect of concurrency. This can best be understood as variation in the "attractiveness", or intensity, of local races.

The Riker-Ordershook model indicates the election-level characteristics that deter-

mine the treatment intensity. First of all, the probability of being the decisive voter in the local election (p_l) is a function of the competitiveness of the local race, and the number of eligible voters in the local district.[6] With increasing competitiveness and decreasing size of the municipality, the benefits of participation in the local election increase, pushing more and more citizens over the participation threshold that would not have voted in a singular EE election (voter types C and D).

Apart from increasing treatment intensity by modulation of the p_l term, we expect municipality size to also have an effect on the non-instrumental benefit, the D_l term. Citizens in smaller municipalities participate more because they have a greater sense of community and political effectiveness than citizens in larger municipalities (Wright, Verba, and Nie, 1975). This sense of community should primarily apply to elections of local offices (D_l), and not at the European level (D_e). Consequently, in small municipalities relatively more voters of type C and D will exist than in larger municipalities. We therefore expect the concurrency effect on EE turnout to decrease in the size of the municipality. This finding should hold irrespective of the competitiveness of the local race – in small municipalities, we expect to find a concurrency effect even for uncontested local races, where the p_l term should practically play no role.

3 Research design

Election timing has been shown to depend on strategic considerations of policy makers such as future economic prospects or anticipated feelings in the electorate (Kayser, 2005; Lupia and Strom, 1995; Smith, 2003). This could well imply that unobserved confounders correlate both with the occurrence of concurrent elections and counterfactual turnout levels. In this section, we discuss our identification strategy to deal with these methodological issues and why we think that our research design provides causal estimates.

We exploit a quasi-experimental situation in the German federal state of Lower Saxony, where term length changes for mayors were likely unrelated to turnout for the European Parliament Election. Additionally, we draw on a Difference-in-Differences design (DiD) to reduce necessary assumptions. We assess the credibility of our design with a number of tests of the identifying assumptions. As dependent variable, we use the difference of EE turnout to turnout in the preceding General Election (GE)[7] – contrary to using the difference to preceding EE turnout, which is the more standard specification of DiD.

We choose to difference EE turnout to the preceding General Election for several reasons. First of all, following second-order theory, the frame of reference for second-

[6] For an overview of economic theories of turnout see Dhillon and Peralta (2002). A positive effect of closeness on turnout has been established empirically in a number of different settings (Cox and Munger, 1989; Endersby, Galatas, and Rackaway, 2002; Shachar and Nalebuff, 1999). Of particular interest for our case is evidence for a small positive closeness effect in German general elections from 1983 to 1994 (Kirchgässner and Meyer zu Himmern, 1995) and a larger effect in mayoral run-off elections in Bavaria Arnold (2015).

[7] Refer to the Online Appendix for a description of all data used and sources.

order elections is the first-order arena: "the campaign and results of each and every type of SOE are more or less heavily influenced by the political constellation of the dominant political arena within the system, the first order political arena" (Reif, 1997, p. 117). Secondly, we also see a number of methodological advantages. GE turnout can be viewed as the 'maximum turnout potential' for second-order elections. GE then are always in a untreated 'control' state as concurrent second-order election do not change GE, i.e. first-order, turnout.[8] We also opt for GE because they are temporally closer to any given EE than the preceding EE (see Fig. 2).[9] Another advantage of using the the preceding GE is that we're able to use the first election in our time series which in a classical DID setting would drop out because there is no first difference for it. We use DiD because it differences out all unobserved *time-constant* confounders (Kodzi, 2010). Our strategy allows us to keep the temporal distance low, which makes it more likely that necessary assumptions are met. Since the electoral cycle for EE is 5 years and that for GE elections is 4 years, there always is a federal election that is temporally closer to a given EE than the preceding EE.

In the case under investigation, the May 2014 EE in the German state of Lower Saxony, the preceding GE was held in September 2013 (highlighted in Fig. 2). We also estimate a standard fixed effects model with turnout level in EE as dependent variable. In the Appendix we provide the results to alternative specifications. Using the differences to the preceding EE as the dependent variable in our models presented in sections 4 (on Lower Saxony) and 5 (on generalizing effects), our results remain substantively unchanged.[10]

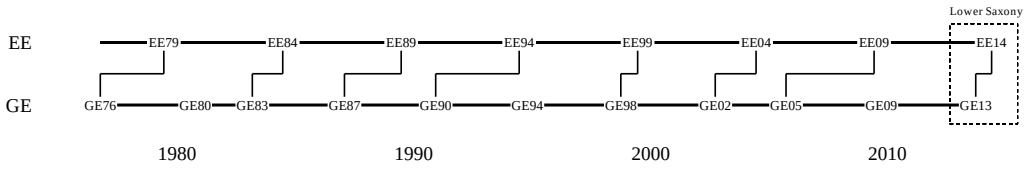

Figure 2: *Timeline of EE elections (EE) and general elections (GE) indicating which GE serves as baseline - 'maximum turnout potential' - for which EE election.*

In a potential outcomes framework following the Neyman-Rubin model (Rubin, 1974), our quantity of interest is the average treatment effect (ATE) for concurrent elections for our sample. ATE is the average difference between the difference to turnout potential under treatment and control condition for each locality i and each time period t.[11] As we do not observe counterfactual outcomes directly, our estimation strategy builds on the core assumption that absent local elections, our 'treated', i.e. concurrent ($D = 1$), localities would experience similar outcomes as 'untreated', i.e. stand-alone

[8] We test this empirically: Some states held state-level elections or state-wide local election concurrently with GE. Concurrency has no effect on the turnout in a GE (see table 1 of the appendix).

[9] The temporal distance between two EE elections is 5 years while the average temporal distance between an EE and the preceding GE is only 2.1, the minimum distance being one year and the maximum distance, because of the shorter legislative periods at the German national leve, four years.

[10] See Tables 6, Lower Saxony, and 8, federal states, in the Appendix.

[11] $\beta = E((Y^1_{it,EE} - Y^1_{it,GE}) - (Y^0_{it,EE} - E(Y^0_{it,GE}))|D_{it} = 1)$

EE ($D = 0$), localities (Kodzi, 2010).

As campaigning for EEs takes place on the national and European level, exceeding state and municipality boundaries where our treatment varies, this assumption is at first sight plausible. Still, we have to ensure that the mechanism that assigns treatment and control locations is unrelated to turnout. For the case of Lower Saxony, the following section provides evidence that this is the case. We can therefore assume high internal validity of our estimates for Lower Saxony. For the case of the federal states, while our strategy has a broader applicability and therefore has a in tendency higher external validity, administrative scrutiny over election timing is higher. We rely on placebo tests that assess whether our treatment has no effect on pre-treatment outcomes. Effectively, we test whether pre-treatment levels – $E(Y_i^0|D_i = 1) = E(Y_i^0|D_i = 0)$ – and trends in our dependent variable – $E(Y_{i,EE}^0 - Y_{i,GE}^0|D_{i,t-1} = 1) = E(Y_{i,EE}^0 - Y_{i,GE-1}^0|D_{i,t-1} = 0)$ – are identical in the control and treatment group. As we show, differences are both insignificant and substantially small. We interpret this as an indication that our research design is likely providing causal estimates (Lechner, 2011). For Lower Saxony we show these placebo tests not only for our main effect, but as well for sub-groups, where we might be worried that these show different turnout levels or follow distinctively different turnout trends for unobserved reasons.[12] Again, we show that this is not the case. Our estimation for Lower Saxony follows the following functional form:

$$(turnout_{14}^{EE} - turnout_{13}^{GE}) = \beta_0 + \beta_1 D_i + \epsilon_i.$$

We additionally report results of level regressions as treatment is, as we argue, exogenous.[13] The results for both models are reported in Table 1.

A final note concerns the Stable Unit Treatment Value Assumption (Basu and Rubin, 1980). SUTVA has two elements (Imbens and Rubin, 2014, pp. 10-13): first, no interference between units and, second, no hidden variations in treatments which lead to different potential outcomes. Both of these are plausible in our case, especially because we deem general equilibrium effects (e.g. changes in overall party campaign behavior) unlikely. In our case, forms of active treatment are labeled CSOE but contain CSOEs with different degree of competitiveness and voter pivotality in municipal elections. Still, the comparison of group averages is a valid estimator of the causal effect if there are no common causes of treatment and treatment version (VanderWeele and Hernán, 2013). As the distributions of covariates in both treatment and control group are very similar it seems plausible to estimate an ATE. [14] Although this exclusion restriction is necessarily a strong assumption which we cannot proof, estimating an ATE is, from a policy perspective, highly desirable: Policy makers would be interested in the average effect of conducting CSOE. In our case, the ATE is defined as CSOE in a municipality with average district size and competitiveness - around 15.000 inhabitants and 2.5 mayoral candidates.

[12] See Section 4.2 in this article and Table 4 in the appendix.
[13] For this model the functional form is: $turnout_{14}^{EE} = \beta_0 + \beta_1 D_i + \epsilon_i$.
[14] See balance tests in Table 3 of the Appendix.

In section 5 we generalize our findings to the federal level. Here, the unit of analysis is an election result at the federal state level. Municipal elections are held state-wide and the date is set by the state government – our case of mayoral elections in Lower Saxony in 2014 was an exception to this rule. This means assuming exogeneity of concurrency is less plausible when the unit of analysis are municipal election results at the state level. Hence, our goal is not to estimate another treatment effect on a different level but to check for observable implications of our findings. If CSOE do indeed exert a causal and positive effect on turnout we should expect to see higher turnout in states which hold municipal elections concurrently with EE than in those that do not.

The functional form for our models estimated on a panel of state-level EE election results is

$$(turnout_{it}^{EE} - turnout_{it}^{GE(preceding)}) = \beta_0 + \beta_1 D_{it} + \beta_2 O_{it} + \epsilon_i \tag{1}$$

$$(turnout_{it}^{EE} - turnout_{it}^{GE(preceding)}) = \beta_0 + \beta_1 D_{it} + \beta_2 O_{it} + \zeta_i + \tau_t + \epsilon_{it} \tag{2}$$

$$turnout_{it}^{EE} = \beta_0 + \beta_1 D_{it} + \beta_2 O_{it} + \zeta_i + \tau_t + \epsilon_{it} \tag{3}$$

The results for these models are reported in Table 2. D_{it} as before, is the treatment indicator, O_{it} is a dummy to indicate other concurrent ballots, state elections or referendums and ζ_i and τ_t are state and year fixed effects respectively.

4 A quasi-experiment in Lower Saxony

The following establishes the effect of concurrent second order elections for the state of Lower Saxony. First of all, we introduce the institutional setting and legislative changes that led to the quasi-experimental setting. We corroborate this by provide tests that help establish that our average treatment effect and our subgroup analysis is likely unbiased. Secondly, we provide evidence that CSOEs led to a turnout increase of about 10 percentage points. We finally show that this effect decreases in the size of the municipality and increases in the competitiveness of the local election.

4.1 The case of Lower Saxony

To test the turnout effect of concurrent second-order elections, we draw on the case of concurrency in the 2014 European election in the German state of Lower Saxony. In some municipalities, mayoral elections were held on the same day. The 2014 European election in Lower Saxony is a closest to ideal case to study because the timing of the mayoral elections can be leveraged as a quasi-random treatment condition, that is plausibly exogenous to EE turnout. Here, we introduce the institutional setting and provide evidence for the quasi-randomness of treatment assignment.

The timing of European elections follows a 5-year election cycle. Member states have scope to set the exact date of the election in terms with national electoral traditions between Thursday and Sunday – in Germany elections are always held on a Sunday.

Therefore, in Germany, the 2014 European election was held on a Sunday, 25 May 2014. All voters in Lower Saxony faced the same party lists and had the same influence on the composition of the European Parliament.[15] But on the same date, part of the municipalities in Lower Saxony also elected their mayor. We refer to these municipalities with European and mayoral elections as treatment municipalities' or 'CSOE municipalities' in the following. The selection into treatment was the result of a complex and partially stochastic process.

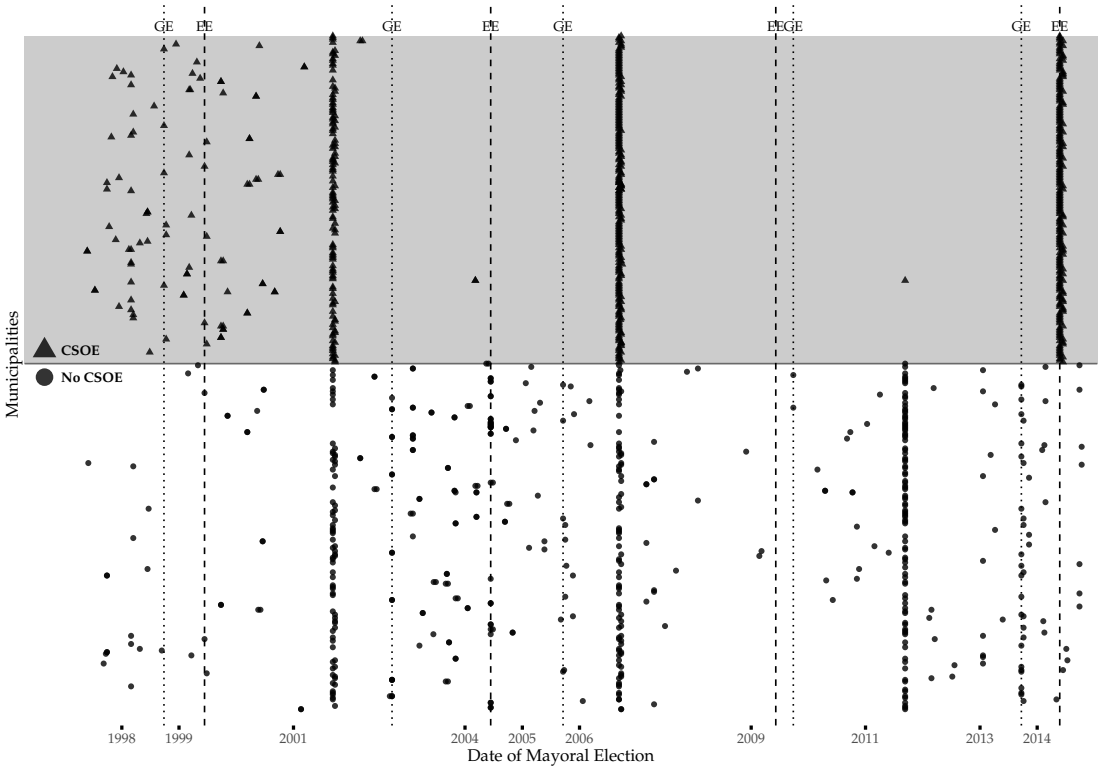

Figure 3: *Timeline of mayoral elections in Lower Saxony. The figure presents one marker for each mayoral election in control (dot) and treatment group (triangle) since 1997. The treatment group (n=201) conducted elections on cycle, i.e. 2001, 2006 and 2014. Selection into this 'normal' electoral cycle occurred when the terms of indirectly elected local executives ended in the late 1990s and if mayors did not step back early. The control group (n=213) conducted its last mayoral elections primarily in 2011 (concurrent with local council elections) and 2013 (concurrent with federal elections). Selection into the control group occurred, first, when the terms of indirectly elected local executives ended after 2001 and elections were held 'off-cycle' since. Second, some municipalities that first held elections 'on cycle' 2001 and 2006 selected into the control group when mayors resigned before their term ended, calling for early elections (n=81). Some control group observations we cannot adequately track over time to determine their type, primarily where changes in administrative boundaries induced changes in the electoral cycle.*

Municipalities were until the 1990s headed by a dual leadership, an honorary mayor and a professional local executive. The latter was indirectly elected by local municipal councils for 12 years. In 1996, the social-democratic SPD introduced the direct election of these local executives with 5 year terms, against the opposition of the centre-right CDU.

[15] Parties in Germany can opt for a country-wide or state-wide closed list of party candidates. Seats are distributed following proportional representation among parties without threshold.

Mayoral elections were to be held concurrently to council elections (Detjen, 2000). Local council and mayoral elections were thus held simultaneously in 2001 and 2006 in most municipalities. In the last regular election 2006, 280 of the 414 municipalities we observe were conducting on-cycle elections. The fact that municipalities were 'off the cycle' in 2006 was the consequence of transitional rules that did not force local executives to face reelection in 1996 and 2001 if their original 12 year term was still running (Armbrust, 2007, 60f.), and of exceptional elections due to death, retirement, resignation or changes in administrative boundaries.[16] In 2005, now under CDU rule and contested by the SPD-led opposition, the term length of mayors was prolonged to eight years (Armbrust, 2007, 60f.). The explicit political aim of the reform was to desynchronize mayoral and local council elections.[17] The legislation became effective for all mayoral elections since 2005.

Accordingly, for the 201 treatment municipalities that held concurrent mayoral elections in 2014, the last mayoral election was regularly held in 2006. Mayoral elections in 2014 could be conducted concurrently wherever the term of the local executive ended within nine months of May 25th (Ipsen, 2011). Whether elections are then actually held concurrently, is under scrutiny of the local administration, but technical rather than political reasons dominate this question: Only 8 out of the 213 municipalities (3.8%) in our control group could by law have voted for their local executive at the European Election day, but did not (for unknown reasons). The municipalities that did not hold mayoral elections concurrently with the 2014 EE were either among the "off-cycle" municipalities in 2006 or municipalities where local executives stepped down or retired between 2006 and 2014.

Altogether, assignment of municipalities to the treatment condition, i.e. holding a concurrent mayoral elections in 2014, depended on remaining time in the term of office of mayors in 1996, when direct elections were introduced, and the individual retirement decisions of in-office mayors in the 1990s and 2000s. These electoral circumstances provide exogenous variation for analyzing the effect of CSOEs on turnout.

4.2 Empirical tests for pre-treatment trends, placebo effects and balance of control and treatment group

While we could think of potential confounders related to both retirement and turnout, such as local competitiveness, tests on covariate balance and pre-treatment trends in our dependent variable indicate very similar distributions in treatment and control group. To substantiate this claim, we first look at descriptive statistics. Figure 4 plots the trend in EE and GE turnout since 1998 for average municipalities with and without CSOE in 2014. As can be seen for general election turnout (upper lines), treatment and control municipalities do not differ in their average turnout. Similarly, the difference

[16] De-selection of local executives is not an issue. There are very high political hurdles, only two cases until 2008 are known where this occurred, see http://www.bpb.de/apuz/144111/politische-verfasstheit-der-kommunalen-ebene?p=all

[17] In 2013, again under SPD rule, this prolongation and desynchronization was reversed under the new government (STK, 2013).

in turnout levels and turnout changes of European Election turnout for treatment and control municipalities is substantially small in the pre-treatment period, though sizable with treatment in 2014. Table 2 in the Appendix reports results of a regression with year and state fixed effects that tests for differences in the pre-treatment trend of CSOE and non-CSOE municipalities - we find substantially small and on the 10%-level insignificant coefficients when testing for different time trends in the 1998-2004 [-0.40(0.49)] and the 2004-2009 [0.72(0.44)] period between both groups.

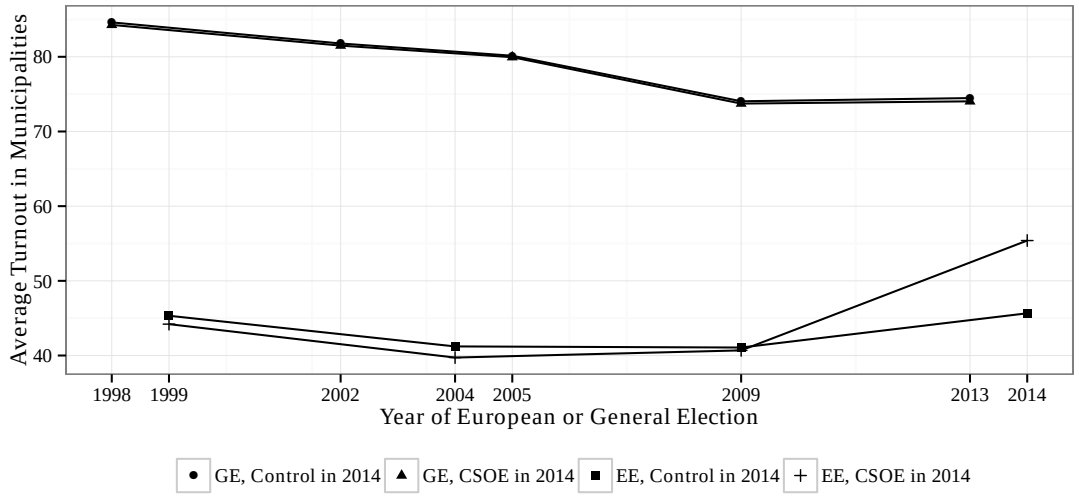

Figure 4: *Trend of EE and GE turnout of an average CSOE and an average non-CSOE municipality in Lower Saxony. Averages are calculated for 201 CSOE and 213 non-CSOE municipalities. Election results are calculated in 2014 administrative boundaries with data from the Statistical Office of Lower Saxony (http://www.statistik.niedersachsen.de.)*

Additionally, we checked for the balance of pre-treatment covariates related to mayoral elections between the treatment and control group in 2014. Specifically, we tested whether the distribution of mayoral party and gender of mayor is similar in both groups, whether treatment and control municipalities are equally distributed in the four regions of Lower Saxony, whether treatment correlates with different administrative types of municipalities (rural municipality, city, joint (rural) municipality), whether mayors had to face a runoff election, whether mayors are in a consecutive term and whether mayors stem from municipalities of different size. Concerning all but one of these variables, we find no significant differences between both groups (based on simple two-sided t-tests). Significant differences are present only for the share of mayors in a consecutive term, which is a consequence of the selection process as in the treatment group municipalities following the regular elections cycle without replacements during the term are over-represented. We also show that pre-treatment trends by consecutive term are similar and that treatment effects controlling for consecutive term are substantially unchanged.[18]

One final concern relates to the selection process. Potentially, the control group could consist of more competitive municipalities, as selection might be driven by strategic res-

[18] See Figure 1 in the appendix.

ignations – and at the same time this competitiveness is a specific treatment condition we want to test for and which determines political participation levels more generally.[19] First, the similar turnout trend and levels in the pre-treatment period for European and General Elections do not point in this direction. Second, to directly compare the competitiveness levels of mayoral elections in both groups, we would need to observe standalone mayoral elections in our treatment and control group at the same point in time. As the regular mayoral election cycle had its last election in 2006, we compare our treatment and control observations with data from the 2006 mayoral elections. Importantly, we only observe 97, and thus less than half, of the control municipalities in 2006 – the sample is therefore potentially biased. Nonetheless, it is comforting that when testing for differences in turnout levels, average number of parties competing and the share of mayors facing runoff elections we find no significant differences between both groups. On the 5% level, the only significant differences lies in the average age of 2006 elected mayors, which is higher in the control group. This indicates that resignations were not driven by strategic considerations, but more likely age-related.[20]

In the Appendix, we additionally report a series of placebo regressions for all our specifications (average CSOE effect and CSOE effect by local competitiveness and by municipality size), drawing on the difference in turnout for the 2009 EE and 2009 GE (held on 27 September 2009) - the coefficients are all substantially (very) small and insignificant.

Overall, both the political process that led to the decoupling of electoral cycles for local executive elections in Lower Saxony and empirical tests on pre-treatment turnout provide evidence of a unique case: 201 out of 414 municipalities in Lower Saxony were quasi-randomly conducting concurrent mayoral elections (our treatment group), while 213 municipalities were not (our control group). In the following, we leverage this quasi-experimental situation to derive especially robust inferences on the turnout effects of concurrent second order elections.

4.3 Average treatment effect of concurrent mayoral elections on EE election turnout

We estimate the average treatment effect of mayoral elections on EE election turnout rates with two models. The first model implements our proposed DiD design, and has the difference in turnout rates between the European and General Election as the dependent variable. The second model has the turnout rate in the EE as the dependent variable. If treatment is assigned as-if-randomly as we have argued in the previous section, and the common linear trend assumption holds, both models yield in expectation the same estimates of the ATE. However, we expect the DiD model effect estimate to be more precise, as between-municipality variation in turnout due to time-constant factors is differenced out.

[19] We thank the anonymous reviewer for raising that issue.
[20] Full results in the Appendix, Table 4.

	Turnout rate	
	DiD (EE2014-GE2013)	EE2014
Constant	−28.8*	45.7*
	(0.3)	(0.4)
Mayoral election	10.2*	9.7*
	(0.4)	(0.6)
Observations	416	416
Adjusted R^2	0.57	0.41
Note:		*p<0.01

Table 1: *ATE of concurrent mayoral election on EE turnout. Results of cross-sectional OLS regressions of 2014 turnout trend between the 2014 European Election and the 2013 Federal Election (Model 1) and 2014 European Election turnout (Model 2) on treatment indicator.*

Table 1 shows that both models yield practically identical estimates of the concurrency effect. Concurrent mayoral elections boost the turnout rate in the EE election on average by 10 (95% CI: [9,11]) percentage points. While turnout in the EE election drops 27 percentage points below the GE turnout rate in untreated municipalities, the decline is only 17 percentage points in municipalities that held concurrent mayoral elections. As expected, the DiD model realizes some noticeable gains in efficiency and model fit, lending support to the outlined estimation strategy.

4.4 Treatment intensity subgroup analysis

Local elections are notoriously diverse. Some take place in very small rural municipalities, others in large cities. Some are highly contested, politicized or both, with multiple viable candidates jockeying for position. In other races there is only one candidate for the job. As we have argued earlier, these different characteristics of the local contests can best be understood as variation in the treatment intensity. Our central premise is that the concurrency effect increases with the intensity, i.e. with the 'importance' of the concurrently held local election. Our theoretical model highlights two central factors that modulate treatment intensity: The size of the local electorate and the competitiveness of the mayoral race.

In the following, we identify twelve treatment intensity subgroups that are defined by district size and competitiveness. We infer the competitiveness of a mayoral election from the candidate set of the local races and the closeness of the electoral results: uncontested, contested and close races. We classify 44 races as uncompetitive because only one candidate stood for election. 134 races were identified as contested races – races in which at least two candidates stood for election, but which were not particularly close. Closeness is operationalized as a difference of less than five percentage points between the vote share of the winning and the second-placed candidate. Judging the electoral

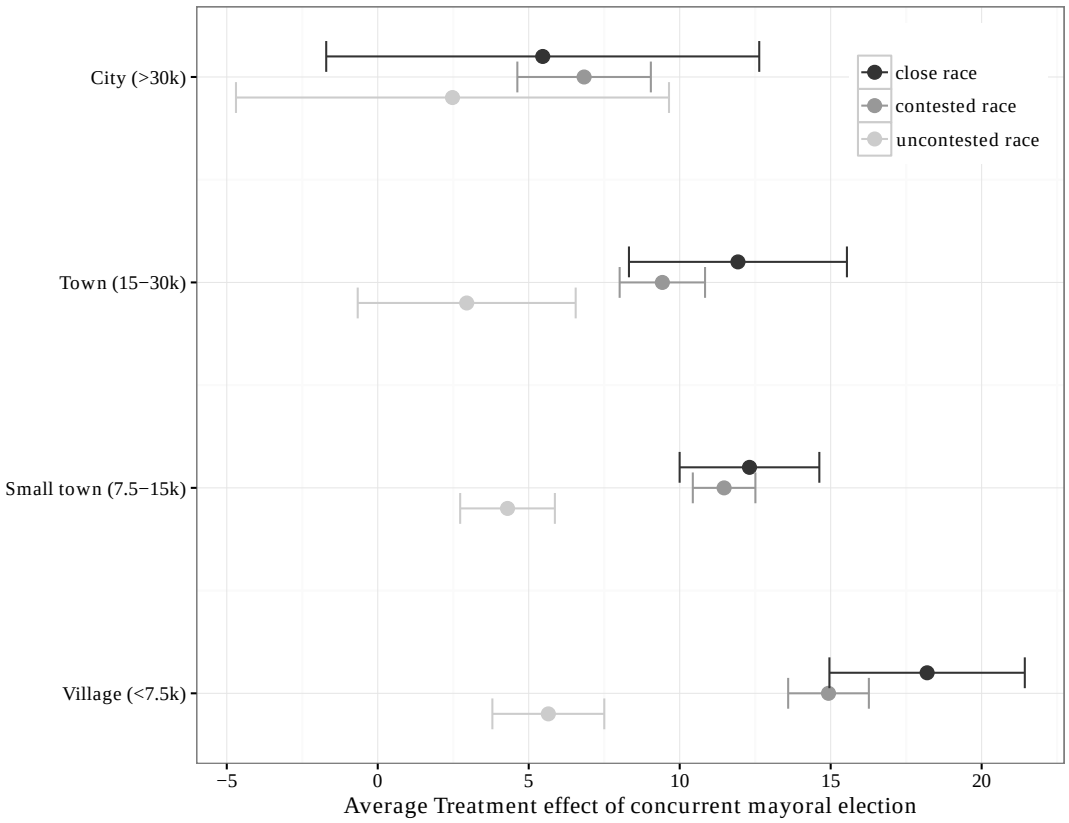

Figure 5: *ATE estimates with 95% confidence intervals for treatment intensity subgroups. Subgroups are defined by the size of the municipality and the competitiveness of the mayoral race. Regression output is reported in Table 7 in the Appendix.*

chances of candidates in local elections is very difficult for voters because in most cases polling data is not available. In this information-scarce environment, five percentage points can be considered well within the 'margin of error' of voters using simple heuristics to determine the viability of candidates. Moreover, the chosen threshold guarantees a group size that is sufficient to make sustainable statistical claims – in our sample there are 20 close races, i.e. contested races with a winning margin of less than five percentage points.

The second criterion for identifying the treatment intensity subgroups is the size of the local unit. We classify treated municipalities according to the size of the electorate, i.e. the number of eligible voters in the mayoral election, into four categories: 54 villages with less than 7,500 eligible voters, 94 small towns with 7,500 to 15,000 voters, 37 towns with 15,000 to 30,000, and 16 cities with more than 30,000 voters.

Our results, presented in Figure 5, show strong support for our theoretical expectations.[21] The concurrency effect, the boost to EE turnout from holding concurrent elections, increases systematically with treatment intensity. Given the size of a municipality, more competitive concurrently-held mayoral elections lead to higher increases in EE turnout due to concurrency. The concurrency effect of uncontested races is much smaller than that in contested races. It ranges from barely noticeable in cities to around

[21] See Appendix, Table 7 for the regression table.

6 percentage points in villages. Our interpretation of this finding is that while there is not much at stake when there is only one candidate for the job, voters in small municipalities, unlike voters in larger, more anonymous municipalities, still feel obliged to show up at the polls to fulfill their sense of duty to vote. As soon as there are two candidates for the job, the concurrency effect is substantial in all size groups. While a contested race raises turnout in cities by 7 percentage points, it is even higher in towns (9 percentage points) and in small towns (12 percentage points). In villages, the treatment effect of a contested mayoral race is the highest - turnout is 15 percentage points higher than in untreated municipalities. For close races, races with a margin of less than five percentage points between winner and runner-up, our results point in the direction of an additional increase in the treatment effect. For villages, small towns and towns we find the treatment effect to be 3, 1 and 2.5 percentage points higher than in contested races. However, confidence to conclude a substantial difference in the treatment effect between contested and close races is not supported by the results. There is simply not enough data, and estimation uncertainty is too large, to statistically distinguish the concurrency effect between contested and close races of the same size.

Nevertheless, the observed pattern is remarkably robust, indicating a systematic relationship between characteristics of the local election that modulate treatment intensity and the magnitude of the concurrency effect. These findings do not only corroborate our thesis that concurrency increases turnout, but provide valuable insights into the concurrency effect. The magnitude of the realized turnout increase ultimately depends on treatment intensity, i.e. how "attractive" the local election is that the EE is combined with. For the purposes of policy evaluation, these insights are of great value, such as for predicting the turnout effect of a synchronization of local and EE cycles in other countries or contexts. Based on our results, we predict that a synchronization would have a larger turnout effect in countries with smaller local-level political entities, and where local elections are generally more competitive. Additionally, we would speculate that the concurrency effect also varies with the formal power that local parliaments and governments have. However, we could not test this preposition since in the cases of our investigation there is no variation between municipalities in that respect.

Another noteworthy implication of our findings concerns a possible over-representation of rural voter preferences in EE elections by introducing concurrency (compared to a status-quo with singular elections). If rural municipalities are on average smaller than urban municipalities, and party preferences of rural and urban voters systematically differ, holding local elections together with EE (or any other state-level election) will favor specific parties. This is because treatment intensity, and in turn the realized turnout increase, is higher in smaller rural municipalities. It follows that more additional rural than urban voters will be drawn to the polls. Parties that have a higher vote share among rural voters should then profit from concurrency.

5 Generalization of effects

To assess the external validity of our results we conduct an analysis of the variation in concurrent EEs and local elections between the 16 German states over the last 35 years. For this, we no longer analyze mayoral elections but local council elections which in all the 16 states are held at one point in time across the whole state, usually every five to six years, depending on state regulations. We report differences between states with and without CSOEs of around ten to thirteen percentage points, very much in line with our findings from Lower Saxony. Because states set CSOEs independently our case for identification is not as strong as for Lower Saxony. Consequently, these results should only be regarded as indicative and we avoid to speak of 'treatment effects'.

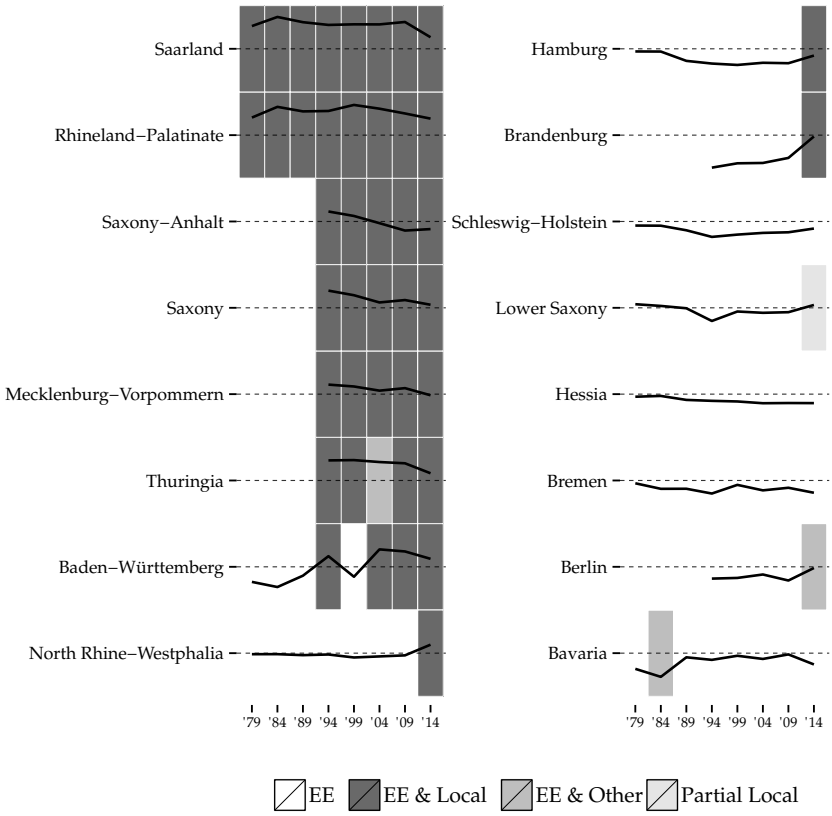

Figure 6: *Concurrency of EE, state-level and local elections. Boxes indicate concurrency of EE elections with local elections, state-level elections (light gray) – in Thuringia – or a state-wide referendum (dark gray) – in Bavaria and Berlin – or partial local elections (lighter gray) – in Lower Saxony. (Dashed) horizontal lines indicate mean turnout in a given EE election.*

We assembled a data set on state-level election returns for all eight European elections held in Germany since 1979 and all general elections in the same period.[22] Concurrency in general depends on the overlap of European and local electoral cycles (Fig. 6).[23]

[22] West Germany (ten states) participated in EE between 1979 to 1989, after reunification this number rose to 16 states.

[23] Term length for elected offices at the local level most often are five years matching the legislative term of the European Parliament which is why once they are held together EP and local elections synchronize, unless election days are explicitly set apart (Fig. 6)

The 'effect' of CSOE can easily be 'seen' in the case of Baden-Württemberg. This state always saw below-average turnout in EE up until 1994 when, for the first time, it held local elections concurrently with European elections. EE turnout dropped below the national average again in 1999, when the European and local election were held on different dates, and returned to and remained at above-average levels when electoral calendars were resynchronized from 2004 onwards.

In the following, we present our results for three different models. First, we estimate a pooled model on the dataset of all eight EE elections regressing the difference between turnout in the EE and the preceding GE on our treatment variable indicating whether a state held local elections in parallel with the EE (Tab. 2, model 1).[24] The difference in turnout between GE and EE elections is always negative reflecting the fact that European elections generally see lower turnout than general elections. In states that did not hold concurrent elections the difference in turnout between national and European elections is on average -32.3 percentage points.[25] The turnout differential between European and general election is less pronounced in states that held local elections: the estimated *average difference between CSOE- and no-CSOE states* is 14.7 percentage points.

	(1)	(2)	(3)
Local	14.7**	10.5**	11.7**
	(1.2)	(2.8)	(3.3)
Intercept	-32.3**	-24.6**	64.4**
	(0.8)	(1.4)	(1.7)
State Fixed-Effects	No	Yes	Yes
Year Fixed-Effects	No	Yes	Yes
R^2	0.477	0.845	0.905
N	110	110	110

Standard errors in parentheses
* $p < .05$, ** $p < .01$

Table 2: *Regression models on a panel of state-level EE results with the difference in turnout in a EE and the preceding GE as dependent variable (1) and concurrent local elections and concurrent other elections or referendums as independent variable, with (2) the same specification but with additional state and year fixed effects, and with (3) only EE turnout as the dependent variable and the aforementioned independent variables and fixed effects – all with clustered standard errors. All models include a dummy variable to indicate concurrent state elections or referendums (only three cases) which is not reported in the table.*

Second, by adding state and time fixed-effects to the specification of model 1 we estimate the change in turnout resulting in the move from a stand-alone EE to concurrent European and local elections (Tab. 2, model 2). The *average turnout increase in states that introduced CSOE* is 11.7 percentage points. Third, we extend the classical

[24] Cameron, Gelbach, and Miller (2008) caution against the use of conventional standard error adjustments in panel data analysis with a small number of clusters advocating the use of bootstrapping as alternative. We present results with clustered standard errors here and results with bootstrapped standard errors in the appendix, Table 7. No substantial change in standard error estimates and corresponding significance levels occurs.

[25] Models 2 and 3 include state fixed effects which are estimated via the within-transformation. The intercept displayed is the average value of the fixed effects and as such does not lend itself to such a straightforward interpretation.

Difference-in-differences set-up to multiple time-periods by regressing EE turnout on our local elections dummy, another elections or referendums dummy as well as state and year fixed effects (Tab. 2, model 3).

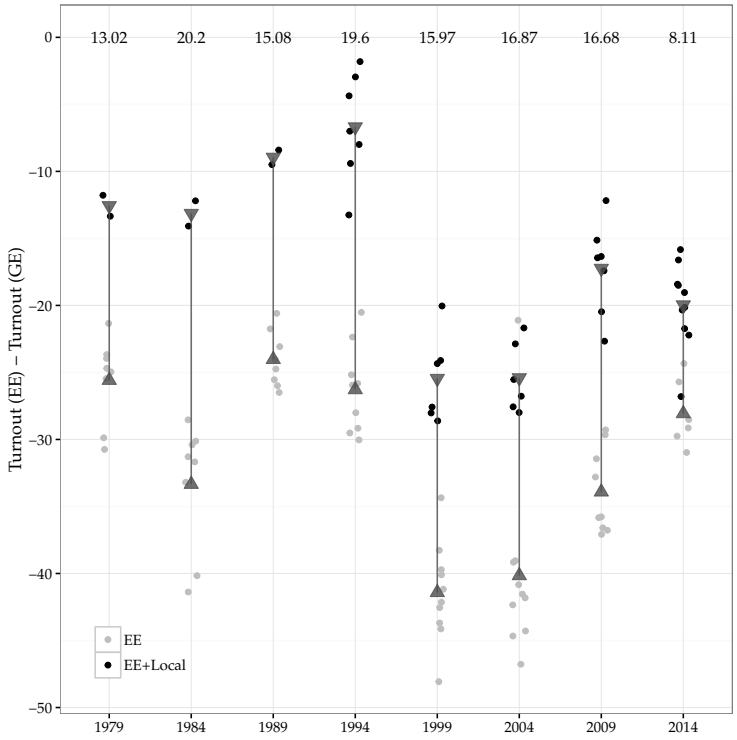

Figure 7: *Difference-in-differences estimates for the turnout effect of concurrent local elections by European Election. Difference between turnout in European election (EE) and preceding general election (GE) on y-axis. Election-specific difference-in-difference estimates – all significant at the .1% level – are printed in the top part of the graph.*

These estimates are consistently higher than our estimates obtained from the data from Lower Saxony described above. Although the argument for exogeneity of treatment is less strong for state-level data we believe it is unlikely that these differences are indicative of strong bias. Note that if there is any systematic relationship between turnout levels and CSOE it is that states with lower turnout should be more likely to opt for concurrency than states with higher turnout. Indeed, it is former East German states that have consistently synced local with European elections and that also have consistently lower turnout levels than former West German states. Note also that in most German states, elections to the municipal council and to the mayoral office, if it is an elected office[26], are held concurrently and therefore see higher turnout than stand-alone mayoral elections which translates into a stronger CSOE effect.

To assess the heterogeneity of our results over time we also estimated separate 'Difference-in-differences' models. We again use the difference in turnout to the preceding GE for each EE and now obtain the *election-specific average difference between CSOE- and no-CSOE states*. For each and every European election, average turnout in states with concurrent local elections is consistently higher than in states without, with

[26] In some states the mayor is elected by the municipal council.

average differences varying from 8 to 20 percentage points (Fig. 7).[27]

Finally, the robustness in our results is supported by placebo tests (presented in the appendix, table 9) that show that CSOE-states are unlikely to be on a differing turnout trend compared to non-CSOE states and that shifting the treatment period forward does not yield substantially or statistically significant results. Last but not least, we get similar CSOE effects when exploiting the geographic discontinuity of concurrency for municipalities at state borders (Table 10 and figure 2 in the appendix), where our identifying assumptions are more likely to be met. Overall, this provides some evidence that the estimates we present here likely approximate treatment effects of concurrent local elections at the state level.

In this section we have tried to generalize our results to the full population of European elections in Germany on the basis of state-level returns. The fact that state governments set the term lengths and dates for municipal elections gives rise to endogeneity concerns. Nevertheless, the results presented here suggest that a CSOE effect is at play, too, which may even be higher when full municipal elections are held concurrently with EE – although we are unable to quantify it exactly.

6 Discussion

6.1 Are CSOE more than any of their parts?

The turnout effect of a concurrent local election is substantial – EE turnout increases by around 10 percentage points. While this seems impressive at first sight, there is an alternative explanation which would undermine the substantive relevance of this finding. If turnout in a singular local election were generally higher than in EE elections, a turnout increase in concurrent EEs would mechanically follow, given that voters rarely cast blank ballots.

The more pertinent question is therefore whether CSOE turnout increases beyond the counter-factual turnout levels obtained in any singular SOE. To answer this question, we would ideally report average turnout levels for counter-factual stand-alone mayoral elections for the same localities at the same point in time. Unfortunately, there are no municipalities that conducted only mayoral, but no EE elections, since EEs were conducted in all municipalities. We therefore cannot estimate turnout for singular mayoral elections at the same point in time.

We use stand-alone mayoral run-off elections in June 2014 and October 2013 as

[27] There are only two exceptions where a single CSOE state experienced turnout lower than any non-CSOE state: Firstly, in 2004 Thuringia did not hold concurrent local elections but concurrent state elections which is why it experienced considerably higher turnout than one would expect from a state without CSOE. Secondly, Hamburg which in 2014 held CSOE for the first time still saw less than average turnout when compared to other states in 2014. Note however that the state did see an increase in turnout vis-à-vis the prior European election 2009 and that Hamburg is one of the three German city states which consistently obtain lower turnout than larger states. One reasons for this is that local elections are less salient as districts are merely administrative units with less autonomy than municipalities.

Election	Average municipality turnout	Number of municipalities	Standard deviation	Min	Max
2013 singular ME run-off	47.14%	9	5.65	38.20%	56.00%
2014 singular ME run-off	46.34%	46	9.57	27.59%	69.38%
2014 singular EE	45.71%	213	4.98	32.68%	62.82%
2014 concurrent EE and ME	55.40%	201	6.58	39.79%	76.95%

Table 3: *Average turnout in singular mayoral (ME) run-off and European elections (EE) in 2013 and 2014 as well as turnout in treatment and control group 2014. Source: Own calculations, data from Election Officer of Lower Saxony (http://www.landeswahlleiter.niedersachsen.de). Mayoral elections in 2013 are all singular run-off elections on 06.10.2013; mayoral elections in 2014 are all singular run-off elections on 15.06.2014; EE in 2014 are all 2014 EE with/without mayoral elections on 25.05.2014 in Lower Saxony*

the arguably best proxy for counter-factual singular mayoral election turnout.[28][29] In the 2013 and 2014 singular mayoral run-off elections, average municipality turnout was 46.3 and 47.1 percent (Table 3). This is slightly higher than turnout in an average municipality that held singular European Elections (45.7 percent). An average CSOE municipality experienced turnout of about 55.4 percent, substantially larger than both singular EE and singular mayoral run-off elections. Keeping in mind that the samples of municipalities and election dates differ, and that we use run-off elections as a proxy for first-round elections, we do not interpret these findings as definitive evidence. Still, we are confident in concluding that turnout levels in CSOEs are indeed 'higher than in any of their parts.' Concurrent second-order elections not only push participation rates to that of the highest counter-factual singular election, they realize a 'net gain' in participation.

6.2 Who are the additional voters?

The natural follow-up question then is who these additional voters are. Are these voters which are primarily interested in the additional election that the SOE is combined with (voter type D in Figure 1), or are these voters who only turn out in concurrent elections (voter type C)? The former would indicate that CSOEs increase turnout by combining different arena-specific sub-electorates, the latter that CSOEs motivate 'completely new' voters otherwise not participating in second-order elections.

[28] Since the vast majority of our control group municipalities held their last mayoral elections concurrently with general elections on September 22nd, 2013 or concurrent local council elections on September 11th, 2011, we cannot use the last mayoral election either.

[29] Whilst runoff elections are advocated as natural experiment in comparison with first-round elections (Indridason, 2008), average turnout in mayoral runoff elections is not directly comparable to first-round turnout. Although runoff elections might be more competitive on average, this must not be the case if the margin between first-round winner and runner-up is relatively large and who wins can be predicted with large certainty by citizens. Given figures from the German federal state Hesse, bordering Lower Saxony, where an average difference of about 3.5 percentage points between mayoral first- and second-round elections is observed for the period 1993-2012 (Garmann, 2014), and the average difference in Bavaria, where average turnout differs by 5 percentage points for the period 1946-2009 (Arnold, 2015), bias of the size of our treatment effect seems unlikely.

This important question can only be answered with individual-level data. Unfortunately, available voter surveys are far from ideal, as they mostly focus on only one electoral arena, and do not address local contests and politics. The best survey data at our disposal is a voter survey on the 2014 EE in Lower Saxony by the Making Electoral Democracy Work project (Blais, 2010). We test an observable implication that might give some insight into the motivations of the additional voters. If the concurrency effect is driven by voters of type D, i.e. voters that would vote in a singular mayoral, but not in a singular EE, we would expect voters that are more interested in local politics to be more likely to turn out in EEs, if these are held concurrently with local elections. For lack of a better measure, we proxy interest in local politics with the degree of local attachment.[30] The treatment is whether the 2014 EE was held concurrently with a mayoral election in the respondent's home municipality.

Column 1 in Table 4 shows that average local attachment scores are balanced between the control and treatment group. Column 2 indicates that the general treatment effect replicates in the survey data. Column 3 shows that treated respondents with high local attachment are eight percentage points more likely to report EE turnout than their untreated counterparts.

	(1) Local Attachment	(2) Turnout	(3) Voted EE & Locally Attached
NO CSOE	7.5	0.68	0.55
CSOE	7.6	0.76	0.62
Differences	0.1 (0.57)	0.08 (0.01)	0.08 (0.04)
N	969	814	790

Table 4: *Voter survey data from Lower Saxony. Comparison of mean local attachment (column 1), turnout between municipalities that held concurrent mayoral elections and those that did not (2), and the share of voters with a local attachment (3). P-Values for difference-in-means (column 1) and χ^2-tests respectively (columns 2 and 3) in parentheses.*

Keeping the limited ability to identify different types of voters based on the available survey data in mind, our tentative conclusion is that there is a substantial amount of voters primarily interested in the local contest which turn out in EE because of the concurrency. It is less clear how we could identify type C voters, voters who only turn out in concurrent elections. Which types of voters are additionally drawn to the polls remains an important, but challenging question for future research endeavors.

7 Conclusion

Second-order elections see markedly lower participation rates than first-order, i.e. general national, elections. In many second-order elections, the costs of voting surpass its benefits for more than half of the electorate, which is worrying for the legitimacy of

[30] Respondents are asked to indicate the strength of their local attachment by answering the question 'How strongly attached to you feel to: your city/municipality?' on an 11-point scale from 0 to 10 with higher values indicating stronger attachment. We use this question as respondents were not asked about their interest or participation in local elections.

the elected. This paper investigates how the combined holding of multiple second-order elections can increase turnout rates.

Theoretically, in concurrent elections voters incur fixed participation costs only once, while they can reap potential benefits multiple times. As in concurrent elections the benefits from participation stem from multiple electoral arenas, singular factors that induce participation such as perceptions of pivotality or electoral closeness can now push voters above their participation threshold for only one election, while leading them to vote in the other electoral arena as well.

We estimate the causal effect of combining two second-order elections on turnout in a quasi-experimental design. In the German State of Lower Saxony, some municipalities held mayoral races concurrently with the 2014 European Parliamentary election (EE). Mayoral election timing was plausibly exogenous to counterfactual turnout levels in the municipalities. We show that concurrent mayoral elections increase turnout by over 10 percentage points (i.e. more than 20%). Leveraging variation in treatment intensity, we show that the effect of concurrent second-order elections (CSOEs) is highest in competitive races in small municipalities (up to 20 percentage points) and close to zero in uncompetitive races in large cities. Analyzing state-level turnout in eight EEs held in Germany, we demonstrate large differences in turnout rates between states with concurrent municipal elections and those that held singular EEs, thereby establishing the external validity of our findings.

Our findings, which are robust to the use of different specifications and subsamples, have direct relevance for the ongoing political debate on policy measures against, and consequences of, low turnout. Our results, in combination with evidence provided by Fauvelle-Aymar and François (2015) on French regional elections and Schmid (2015) on cantonal elections and concurrent referendums in Switzerland indicate that CSOEs should 'work' in a wide variety of contexts. Combining multiple SOEs is a simple, yet very effective policy tool to increase turnout rates. Taking our results literally, more than 80% of the much noted on increase in EE turnout in Germany between 2009 and 2014 (from 43.3 to 48.1 percent) was due to the introduction of concurrency in German states (3.9 percentage points). Without concurrency in any state, counter-factual 2014 EE turnout in Germany would have been at only 39.0 percent instead of the actual 48.1 percent.[31]

Most importantly, CSOEs do not simply push up turnout to the turnout level of the most attractive SOE - they are 'more than any of their parts'. CSOEs increase turnout beyond the level of any of the two elections. Theory and suggestive evidence from survey data leads us to suggest that this net increase in turnout is primarily due to a combination of sub-electorates that only turn out in one of the elections. In our

[31]The counter-factual turnout rate is calculated by subtracting the estimated concurrency effect in Table 2, Model 1 from observed turnout in states with concurrent elections in 2014 and thus recalculating counter-factual EE turnout without CSOEs. Similar calculation (based on Table 2, Model 2) leads to the estimation of additional voters in the German states introducing concurrent local elections (Hamburg, North-Rhine Westphalia, Brandenburg) or a concurrent referendum (Berlin). Additional voters in Lower Saxony were calculated drawing on Table 1, Model 1, and the share of voters in municipalities with concurrent elections (46.9%).

case, this would imply that many of the additional EE voters are not interested in the EE, but only participate because there is a local election on the same day.

This indicates that there is a trade-off involved. While high turnout is desirable as the characteristics of voters resemble the general population more closely when turnout increases (Lijphart, 1997; Singh, 2015), the mixing of different subsections of the population that are not necessarily interested in one of the elections might lower the quality of vote choices. For instance, Börgers (2004) and Krishna and Morgan (2011) argue theoretically that voluntary participation Pareto-dominates compulsory voting. Hodler, Luechinger, and Stutzer (2015) provide evidence that the introduction of postal voting in Switzerland (i.e. lower costs) is associated with on average less knowledgeable voters and lower welfare expenditure because uniformed voters are more likely to be swayed by special interests. This is in line with survey evidence from Switzerland on concurrent referendums, where, while turnout increases, the average level of political knowledge of voters decreases (Schmid, 2015). However, Schmid also reports an increase in information search behavior of these new voters. Although this might not offset the knowledge-effect in the short-term, exposure and engagement with the political system should increase knowledge over time (Wong, 2000).

The question on whether concurrent elections (and lower voting costs in general) decrease the average quality of vote choice has to be further investigated, ideally with panel survey data covering interest and participation in concurrent second-order elections. Future research should also focus on the differences in the preference distributions between the sub-electorates that are drawn to the polls in concurrent elections. This would help us to better understand the implications of holding concurrent elections for electoral outcomes.

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Appendix

June 7, 2016

Appendix to "How to increase turnout in low salience elections. Quasi-experimental evidence on the effect of simultaneous second-order elections on political participation." Additional tables and figures appear in the order they are referenced in the paper. The sectioning of the appendix mirrors that of the article.

3 Research Design (and Data)

The following paragraphs describe our dataset and the construction of our dependent variable.

Citizens in Germany generally vote on four levels. On the European level, elections to the European Parliament (EE) take place every five years. Elections on federal level (GE), for the German parliament (Bundestag) take place every four years. Elections on federal state level for federal state parliaments take, depending on state election laws, place every four to mostly five years. Elections on local level comprise elections for local councils, district councils, mayors and district administrators. Councils are elected every five to six years, depending on state regulations. Mayors and district administrators are directly elected every five to nine years (except for the city states) depending on state and community regulations (Glejdura, 1972).

To analyze the effect of concurrent local elections on EE turnout we assembled two datasets – one dataset of municipal-level election returns for Federal and European Elections in the 2009-2014 period in Lower Saxony and another dataset of state-level returns for all eight European elections and the closest Federal Election held in Germany since 1979. To analyze the effect of concurrent elections on turnout and vote shares, we draw on variation in the timing of European and local elections on the municipal (in the state of Lower Saxony). We generalize our findings with election data on the state level (for all of Germany).

As dependent variable, we primarily use the difference of EE turnout to turnout in the temporally closest GE as variable of interest. By calculating this turnout differential we control for level differences in what we call 'maximum turnout potential'. This strategy cancels out all time-constant factors that affect turnout similarly for European and Federal elections (demographics, socialization etc.). Our estimate of the turnout effect of local elections is then based on the difference in the turnout decline from federal to EP elections. We choose the temporally closest federal election, which does not necessarily

take place before the EP election it serves as a baseline for. We also opt for (temporally close) GEs because they are temporally closer to any given EE than the preceding EE and thereby the parallel trends assumption implied by our DiD design is more likely to be met. The average absolute temporal distance between the EP election and the closest federal election is 0.75 years (distance to preceding federal election: 2.25 years).[1] In the case of the 2014 EP election the temporally closest election is also the preceding federal election, that of 2013.

Depending on the specification, state or time fixed effects are included and the data may be time-series cross-sectional or, in the case of Lower Saxony, cross-sectional – details are provided in the corresponding tables and discussion of our two cases further below.

3.1 Maximum turnout potential

To estimate the effect of CSOE on EP election turnout we use the difference of EP turnout to turnout in the temporally closest federal election as dependent variable. By calculating this turnout differential we control for state level differences in what we call 'maximum turnout potential'. This strategy serves to cancel out all state level factors that similarly affect state and federal elections (demographics, socialization etc.).

As a first placebo test, we note that turnout in federal elections is not substantially or significantly affected by concurrent second-order elections. Results of the estimation are given in table 1. This results supports our argument that turnout in the federal election does indeed capture what we call maximum turnout potential.

	Model 1
Local	0.010
	(0.011)
Land	0.010
	(0.005)
R^2	0.946
Adj. R^2	0.766
Num. obs.	142

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 1: *Estimating the (non-)effect of CSOE on turnout in federal elections. Fixed-effects model with state (within-transformation) and year (dummies, estimates omitted) fixed effects.*

[1] Clearly, temporally close federal elections, even if they are preceded by the EE they serve as base category for, are a much better measure of maximum turnout potential at the time of the EE than GE that may precede the European elections by as much as four years.

4 A quasi-experiment in Lower Saxony

4.1 Empirical tests for pre-treatment trends, placebo effects and balance of control and treatment group

4.1.1 Treatment effects and pre-treatment trends for the 1998-2004 European Elections

Table 2 tests whether trends between municipalities that held concurrent elections in 2014 differ in the 1998-2014 electoral periods. They do not, except of course for the treatment period (2014).

	(1)
	to
year=2004	-4.08***
	(0.40)
year=2009	-4.23***
	(0.28)
year=2014	0.35
	(0.38)
2014csoe=1 × year=2004	-0.40
	(0.49)
2014csoe=1 × year=2009	0.72
	(0.44)
2014csoe=1 × year=2014	10.8***
	(0.57)
Constant	44.8***
	(0.16)
N	1656.00
r2_a	0.70

Table 2: *The table shows results of a fixed effects regression with state and municipality fixed effects on turnout for the Lower Saxony European Elections (1999, 2004, 2009, 2014), with seperate year trends for communities that held concurrent mayoral elections in 2014. Standard errors clustered at community level are in parantheses. ** (*, ***) indicates p<0.05 (0.10, 0.01)*

4.1.2 Balance between treatment and control group characteristics in 2014

We conducted balance tests on pre-treatment covariates of mayoral elections (see Table 3. Specifically, we tested whether the distribution of mayoral party and gender of mayor is similar in both groups, whether treatment and control communities are equally distributed in the four regions of Lower Saxony, whether treatment correlates with different types of municipalities (rural community, city, joint (rural) community), whether mayors

had to face a runoff election, whether mayors are in a consecutive term, and how large the electorate in a municipality is (absolute and split into the subgroups used in the paper). Concerning most of these variables, we find no significant differences between both groups (following a simple two-sided t-test). Significant differences are present, first, only for the share of mayors in a consecutive term, which is a consequence of the selection process as in the treatment group communities following the regular elections cycle without replacements during the term are overrepresented. See below for a test showing that this does not bias our treatment effect. Significant differences are present, second, for the share of very small communities with less than 7500 inhabitants (overrepresented in the control group) - this does not bias our results, however, as the specific treatment effects for these subgroups estimated in the paper show as well as the parallelism of the pre-treatment trend by community size subgroups (see Table 3). Finally, especially the insignificance of differences in the share of mayors facing runoff elections is comforting, given potential concerns about differences in average competitiveness of treatment and control communities. Note however, that this result is based on a small subsample for two reasons: We do not observe the presence of runoff elections for the period mid-2011 to mid-2013 as the CDU government abolished runoff elections in mayoral races during this time. We were able to gather information on runoff elections only for selected timeframes where mayors were elected on a joint date in several communities.[2]

[2]These timeframes were: 9/10/2006 and 9/24/2006; 3/4/2007 and 4/22/2007; 9/22/2013 and 10/6/2013; 5/25/2014 and 6/15/2014 (all treatment observations); 9/28/2014 and 10/12/2014.

	Control mean	Treated mean	Diff-In-2014 Means(se)	N Control	N Treated
cdu	0.33	0.29	0.04 (0.05)	211	201
spd	0.30	0.32	-0.02 (0.05)	211	201
independent	0.37	0.39	-0.02 (0.05)	211	201
female	0.09	0.10	-0.01 (0.03)	211	201
region_braunschweig	0.21	0.16	0.05 (0.04)	211	201
region_hannover	0.24	0.20	0.03 (0.04)	211	201
region_lueneburg	0.25	0.26	-0.01 (0.04)	211	201
region_weser_ems	0.30	0.37	-0.07 (0.05)	211	201
community	0.37	0.40	-0.02 (0.05)	211	201
joint_community	0.31	0.29	0.01 (0.05)	211	201
city	0.32	0.31	0.01 (0.05)	211	201
runoff	0.29	0.22	0.07 (0.06)	58	201
mayor in consecutive term	0.26	0.46	-0.20*** (0.05)	211	201
eligibles	15261.31	14301.07	960.24 (2388.99)	213	201
pop<7500	0.37	0.27	0.10** (0.05)	213	201
7500<pop<15000	0.39	0.47	-0.08 (0.05)	213	201
15000<pop<30000	0.16	0.18	-0.02 (0.04)	213	201
pop>30000	0.08	0.08	0.00 (0.03)	213	201
Observations	414				

Table 3: *The table reports t-tests for differences in means comparing 2014 characteristics of treatment group and control group mayors.*

4.1.3 Trends by reelection-status

Given the imbalance between treatment and control group with respect to municipalities with mayors being in a consecutive term before European Elections 2014, we checked whether our results hold controlling for reelection-status. Figure 1 below shows the trend in EE turnout for communities with and without reelected mayors by 2014 treatment status. But while communities with reelected mayors of the control group seem to exhibit higher EE election turnout [2.09 (1.24)] in 2004, significant on the 10%-level, this is neither the case in 1998 nor 2009. Especially the parallel trend of control group mayors by reelection status between 2009 and 2013 suggests that our results are not biased.

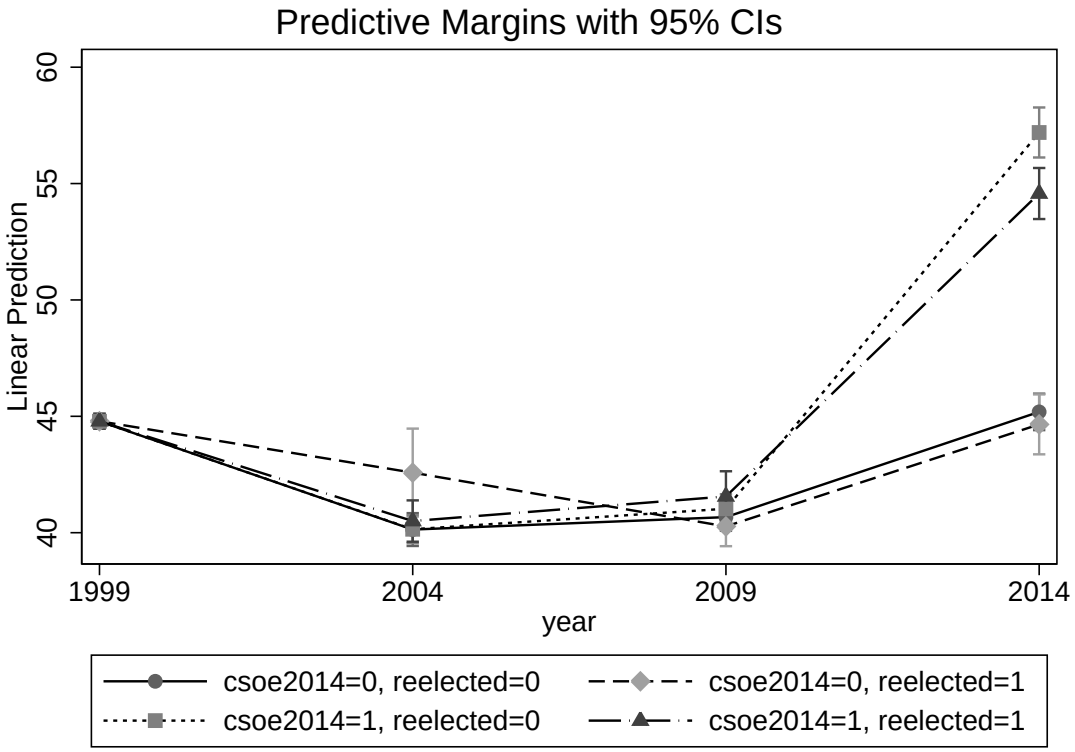

Figure 1: *Predictions for EE and GE turnout of an average community in Lower Saxony by treatment status in 2014 and whether a mayor was a reelected mayor in 2014, with 95% confidence intervals. 1999 is the baseline year. Predictions follow from regressions with municipality and year fixed effects, clustered at the community level. Election results are in 2014 administrative boundaries with data from the Statistical Office of Lower Saxony (`http://www.statistik.niedersachsen.de`.). Reelection status is observed for 406 out of 414 municipalities.*

4.1.4 Balance between treatment and control groups in 2006

To directly compare the competitiveness levels of mayoral elections in both groups, we would need to observe our treatment and control group at a different point in time. Optimally, we would even observe both groups in a situation where treatment and control observations vote at the same point in time, to hold the general political environment constant. As the last regular mayoral election cycle had its last election in 2006, we

compare our treatment and control observations at this point in time (see Table 4). Importantly, we only observe 97, and thus less than half, of our control communities in 2006 – the sample is therefore potentially biased. Nonetheless, it is reassuring that when testing for differences in turnout levels, average number of parties competing and the share of mayors facing runoff elections we find no significant differences between both groups. Additionally, we checked for differences in the average number of eligible, age of elected mayor, community type, party affiliation of the election winner, relative vote share of leading first-round candidate,[3] absolute margin between first-round leader and runner-up (within contested municipalities), and share of municipalities with margin less than 5 percentage points (within contested municipalities). On the 5% level, the only significant differences rests in an elected mayors age, higher in the control group, which makes sense as these communities in the following were more likely to not sustain a full electoral cycle. This indicates that resignations were not driven by strategic considerations, but more likely age-related. Additionally, the community type rural municipality seems to be over- and SPD-led municipalities underrepresented in the treatment group (differences significant on the 10% level), which points towards a potential bias in the 97 control communities observable here as these differences are not observed for 2014 see Table 3).

[3]One note of caution is warranted here: We estimated this comparison and the winning margin variables drawing on the share of mayors from the SPD, CDU, Green Party, FDP and "All Others". We had to combine the vote share of "all other" competing candidates to one variable due to data limitations, and have to assume it is only one "other" candidate running there. However, it is rare that more than one strong independent candidate runs in a municipality.

	(1)				
	Control/mean	Treated/mean	Diff-In-2014-Means/se	N Controls	N Treated
turnout_2006	57.33	56.82	0.51 (0.80)	97	200
number_candidates	2.86	2.67	0.19 (0.15)	97	200
runoff_election	0.32	0.23	0.08 (0.05)	97	200
eligible_voters	18742.85	14795.11	3947.74 (3523.00)	97	200
age	51.43	49.24	2.19** (0.87)	97	200
city municipality	0.33	0.32	0.01 (0.06)	97	200
joint municipality	0.38	0.29	0.09 (0.06)	97	200
rural municipality	0.29	0.39	-0.10* (0.06)	97	200
CDU win	0.28	0.32	-0.04 (0.06)	97	200
other party win	0.28	0.35	-0.08 (0.06)	97	200
SPD win	0.44	0.33	0.11* (0.06)	97	200
relative winner's vote share	63.74	64.61	-0.87 (2.19)	97	200
margin to runner-up	27.61	28.19	-0.58 (2.75)	86	179
share of municipalities with margin less 5 percentage points	0.14	0.15	-0.01 (0.05)	86	179
N	297				

Table 4: *The table reports t-tests for differences in means comparing outcomes and characteristics of the 2006 mayoral elections in Lower Saxony, involving 297 municipalities, of which 200 are in the 2014 treatment group and 97 are in the 2014 control group. The last two rows draw only on municipalities with more than one candidate competing.*

4.1.5 Placebo tests for treatment regressions

In the following, we report placebo regressions, drawing on the turnout for the 2009 European Parliament and 2009 Federal Parliament election (election held on 27 September 2009), both unaffected by CSOE.

Table 5 shows three placebo tests:

Model 1 provides our core placebo test for the full sample with a single dummy for communities that held CSOE in 2014. Our test results indicate that the respective trend coefficient is very small, at 0.08 percentage points, and statistically insignificant. We can therefore plausibly assume that CSOE and non-CSOE-communities in 2014 do not differ in (pre-treatment) turnout trends. In the manuscript, we also report results of a sub-group analysis to learn on heterogeneity of the CSOE effect. To test whether the number of local candidates running and EP turnout might be endogenous, e.g. via local political culture, we assess whether turnout trends in these sub-groups are correlated with treatment assignment. Model 2 tests pre-treatment differences where CSOEs are held contested (more than one candidate running) vs. uncontested in 2014. Model 3 finally assesses whether communities with CSOE of different size (population >5000, >10000, >30000) follow different trends. Again, the respective coefficients in Model 2 and 3 are substantially small and far from conventional levels of significance.

	DiD (EP2009-FE2009)		
	(1)	(2)	(3)
	D.to	D.to	D.to
CSOE	-0.037		0.30
	(0.30)		(0.67)
uncontested CSOE		-0.14	
		(0.73)	
contested CSOE		-0.10	
		(0.32)	
close CSOE		0.19	
		(0.45)	
population=7500			-0.63
			(0.52)
population=15000			-0.36
			(0.52)
population=30000			-0.25
			(0.58)
csoe=1 × population=7500			-0.45
			(0.82)
csoe=1 × population=15000			-0.22
			(0.89)
csoe=1 × population=30000			-0.44
			(0.91)
Constant	-33.0***	-33.0***	-32.7***
	(0.21)	(0.21)	(0.37)
N	414.00	414.00	414.00
r2_a	-0.00	-0.01	-0.00

Table 5: *The table shows results of a regression with on the difference in 2009 turnout between European and Federal Elections on: a dummy indicating communities with occurence of CSOEs in 2014 (Model 1); the csoe-dummy split into subgroups of competitiveness (only one candidate ('uncontested') in concurrent mayoral elections 2014, two or more candidates ('contested') or two or more candidates with winning margin smaller than 10 percentage points ('close') in concurrent mayoral elections 2014) (Model 2); Model 3 reports results with an interaction term between concurrent elections and dummies for communities with population ¿ 7500, ¿10.000 and ¿30.000. Robust standard erros in parentheses. ** (*, ***) indicates p<0.05 (0.10, 0.01)*

4.2 Using the preceding EE instead of the temporally closest GE as baseline period

	Turnout rate
	DiD (EP2014-EE2009)
Constant	4.6**
	(0.3)
Mayoral election	10.1**
	(0.5)
Observations	414
Adjusted R^2	0.5
Note:	*p<0.05; **p<0.01

Table 6: *Lower Saxony ATE analysis. Results of cross-sectional OLS regressions of 2014 turnout trend between the 2014 European Election and the 2009 European Election on treatment indicator of concurrently held mayoral election)*

4.3 Treatment intensity subgroup analysis

The following table (Table 7) shows that regression model underlying Figure 5 in the manuscript.

	Dependent variable:
	DiD (EP2014-GE2013)
Constant	-28.7^{**}
	(0.2)
Uncont./City	2.5
	(3.7)
Cont./City	6.8^{**}
	(1.1)
Close/City	5.5
	(3.7)
Uncont./Town	2.9
	(1.8)
Cont./Town	9.4^{**}
	(0.7)
Close/Town	11.9^{**}
	(1.8)
Uncont./Small Town	4.3^{**}
	(0.8)
Cont./Small Town	11.5^{**}
	(0.5)
Close/Small Town	12.3^{**}
	(1.2)
Uncont./Village	5.6^{**}
	(0.9)
Cont./Village	14.9^{**}
	(0.7)
Close/Village	18.2^{**}
	(1.7)
Observations	416
Adjusted R^2	0.7
Note:	*p<0.05, **p<0.01

Table 7: *ATE estimates for treatment intensity subgroups. Sub- groups are defined by the size of the municipality and the competitiveness of the mayoral race. Results are reported visually by means of a coefficient plot in Figure 5 in the manuscript.*

5 Generalization of effects

5.1 Alternative estimation of state-level results

Cameron, Gelbach, and Miller (2008) caution against the use of conventional standard error adjustments in panel data analysis with a small number of clusters advocating the use of bootstrapping as alternative. Therefore, we present results with clustered standard errors in the text and present results with bootstrapped standard errors here in the appendix. The bootstrapped results are based on 1000 bootstrap replications. No substantial change in standard error estimates and corresponding significance levels occurs.

	(1)	(2)	(3)
Local	14.7**	10.5**	11.7**
	(1.5)	(3.6)	(3.9)
Intercept	-32.3**	-24.6**	64.4**
	(0.9)	(1.8)	(2.2)
State Fixed-Effects	No	Yes	Yes
Year Fixed-Effects	No	Yes	Yes
R^2	0.477	0.845	0.905
N	110	110	110

Standard errors in parentheses
* $p < .05$, ** $p < .01$

Table 8: *Regression models on a panel of state-level EE results with the difference in turnout in a EE and the preceding GE as dependent variable (1) and concurrent local elections and concurrent other elections or referendums as independent variable, with (2) the same specification but with additional state and year fixed effects, and with (3) EE turnout as the dependent variable and the aforementioned independent variables and fixed effects – all with bootstrapped standard errors. All models include a dummy variable to indicate concurrent state elections or referendums (only three cases) which is not reported in the table.*

5.2 Using the closest GE instead of the preceding GE as baseline period

As explained in the paper and above we choose the preceding federal election, which is not necessarily the temporally closest GE to an EE. We difference against turnout in the GE because we want to capture the 'maximum turnout potential' at the time of the election. GE then are always in an untreated 'control' state as concurrent second-order elections do not change GE, i.e. first-order, turnout. Arguably, GE turnout delivers the best approximation of 'maximum turnout potential' when temporal distance between the elections is minimized.

When looking at the temporally closest elections, the preceding GE is the temporally closest GE in three cases while in five cases the temporally closest GE postdates the EE (see Fig. 2). The average absolute temporal distance between a European and the closest general election is 0.75 years while the average absolute distance between EE and preceding GE is 2.25 years.

Here we present results that we obtain when using the difference in turnout to the

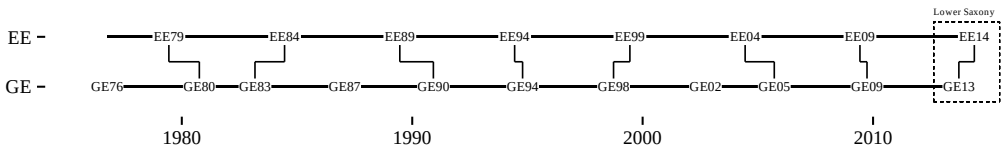

Figure 2: *Timeline of EE elections (EE) and general elections (GE) indicating which GE serves as baseline - 'maximum turnout potential' - for which EE election.*

temporally closest GE rather than the preceding EE. We reproduce models 1 and 2 from Table 2 in the manuscript. While this specification minimizes the temporal difference between 'control' and 'treatment' period it opens up the possibility that the EE influences turnout in the subsequent GE. This is unlikely as not even concurrent elections influence turnout in a GE (see Tab. 1). Nevertheless, we opt for showing results for the difference to the preceding EE in the main text. It evades the problem just outlined and is closer to the classic DiD setup. The results obtained for the two difference operationalizations are substantially the same. No operationalization is strictly better than the other in terms of precision of the estimates or model fit.

	(1)	(2)
Local	13.7**	10.7**
	(1.4)	(2.7)
Intercept	-31.1**	-22.7**
	(0.9)	(1.4)
State Fixed-Effects	No	Yes
Year Fixed-Effects	No	Yes
R^2	0.401	0.860
N	110	110

Standard errors in parentheses

* $p < .05$, ** $p < .01$

Table 9: *Replication of Table 2, models 1 - 2, using the turnout differential between EE and closest GE. (1) Regresses the difference in turnout between EE and preceding GE on dummies for concurrent local elections and concurrent other elections or referendums. (2) adds state and year fixed effects tot specification – all with clustered standard errors. All models include a dummy variable to indicate concurrent state elections or referendums (only three cases) which is not reported in the table.*

5.3 Placebo tests

While the timing of EE is set at the European level, and plausibly exogenous to turnout and electoral preferences on the state level, the timing of local elections is not. State level discretion in election timing is potentially worrying and endogeneity thus might be a potential concern.[4] In our generalization exercise we therefore also analyzed the

[4]Clearly, the level of turnout varies e.g. between states introducing CSOEs, as most East German states, with generally lower turnout levels, conducted CSOEs with their first elections.

difference from land-level turnout potential (i.e. federal election turnout) and trends in our dependent variable to effectively control for all potential time constant confounders. Here, we conduct placebo tests to justify the assumptions of our research design.

We report two placebo tests in Table 10. Here, we on the one hand want to assess whether states that introduce and uphold CSOEs are on a different turnout trend as compared to non-CSOE states. For this, we replace the turnout to European elections with the closest Federal Election Turnout in the said state and estimate the effect of the CSOE placebo on the trend between those general elections. As reported in Model 1, the estimated placebo effect is positive, but small (0.84 percentage points), and insignificant.

On the other hand, in Model 2, we assess specifically the turnout trend for the introduction of CSOEs in several federal states in 2013 and estimate whether these states were on a differentiating trend in the 2004-2009 period. The dependent variable is the double difference of the 2009 and 2004 European elections to the 2005 and 2009 Federal elections ((2009EP-2009FE)-(2004EP-2005FE)). The estimated coefficient for the CSOE placebo is again positive, but small (around 1.2 percentage points) and insignificant (the coefficient size is similar to using a simple difference in difference on 2004-2009 EE turnout).[5] Overall, these placebo tests provide clear justification for our estimation strategy.

	Turnout rate	
	DiD:	DiDiD:
	1980-2013 closest FEs	(EE2009-GE2009)-(EE2004-GE2005)
Local election	0.84	1.233
	(1.10)	(1.47)
Constant	-2.47**	6.94**
	(0.63)	(0.73)
N	94	16
R^2	0.01	0.04

Standard errors in parentheses. * $p < .05$, ** $p < .01$

Table 10: *State level placebo regressions with robust standard errors. Model 1 regresses a dummy of CSOEs for the 1979-2014 EE election on the first differences between the closest Federal Parliament Elections. Model 2 reports estimates for a CSOE dummy indicating federal states introducing CSOEs in 2014 on the first difference of their 2005-2009 turnout trend, with the difference between EE turnout and the closest Federal Election turnout as dependent variable.*

5.4 Geographic Discontinuities

The following section provides additional evidence for the validity of the CSOE effect on turnout as described above. To corroborate our results we use a design based on geographic discontinuities and matching for the last electoral period 2009-2014 (Keele, Titiunik, and Zubizarreta, 2015; Keele and Titiunik, 2015b). Although our set-up is designed to account for time-constant state-level confounders, unobserved time-varying

[5] We additionally added a lead effect to the treatment model (Table 8, Model 1), which is insignificant and substantially small Kodzi (comp. for an overview on this idea of a Granger-test of causality 2010, p. 178).

heterogeneity that could determine turnout and might be related to treatment is a potential confounder (i.e. changes in economic structure). Our design builds on the insight that a comparison of adjacent communities will improve the average balance of observable and unobservable confounders relative to any random pair of communities (Dube, Lester, and Reich, 2010). Placebo analysis give an indication for the plausbility of the design.[6] This analysis draws on the same dependent variables as above, thus taking time-constant confounders directly into account. As we do not expect treatment heterogeneity along the border we restrict ourselves to a comparison of mean turnout differentials in adjacent communities.

We analyse specifically the adjacent communities along the border of Lower Saxony (with mayoral elections about half its communities in 2014) and Hessia (in general no CSOE, with the exceptio of few mayoral elections in 2014) as well as Lower Saxony and North-Rhine Westphalia (local council elections in all communities in 2014). Table 11 reports results of placebo estimates (Model 2) and treatment effect estimates (Model 1) following nearest neighbour matching on longitude and latitude of municipality centroids of all treatment and control communities along the border of Lower Saxony with Hessia and North-Rhine Westphalia, with the difference of turnout to the European Parliament elections to closest Federal Elections as dependent variable. Figure 3 shows the distribution of turnout by treatment status and border for the 2013-2014 (treatment) period and the 2009 (placebo) period.

As can be seen, the turnout differential of treated and control municipalities is remarkably similarly distributed in the pre-treatment period. There is no indication of a large and/or significant pre-treatment difference between communities that conducted CSOEs (placebo estimate of -1.02 percentage points). On the contrary, the estimated ATE of 11.45 percentage points in Model 1 of Table 11 is very much in line with the effect size estimated above. Additionally, the upper panel of Figure 3, especially the comparison of the CSOE effect along the border of Lower Saxony to North-Rhine Westphalia, reveals that concurrent mayoral (in Lower Saxony) and concurrent local council elections (in North-Rhine Westphalia) show a very similar CSOE effect.

[6]Successful placebo tests are especially important as crossing state borders implies 'compound treatment effects', i.e. not only the concurrency of local elections but as well other contextual variables change sharply. Identification of the CSOE effect therefore relies on what Keele and Titiunik (2015a) call the 'Compound Treatment Irrelevance Assumption'. Successful placebo test indicate, that other contextual variables are unlikely to be systematically related to turnout and can therefore be plausibly ignored.

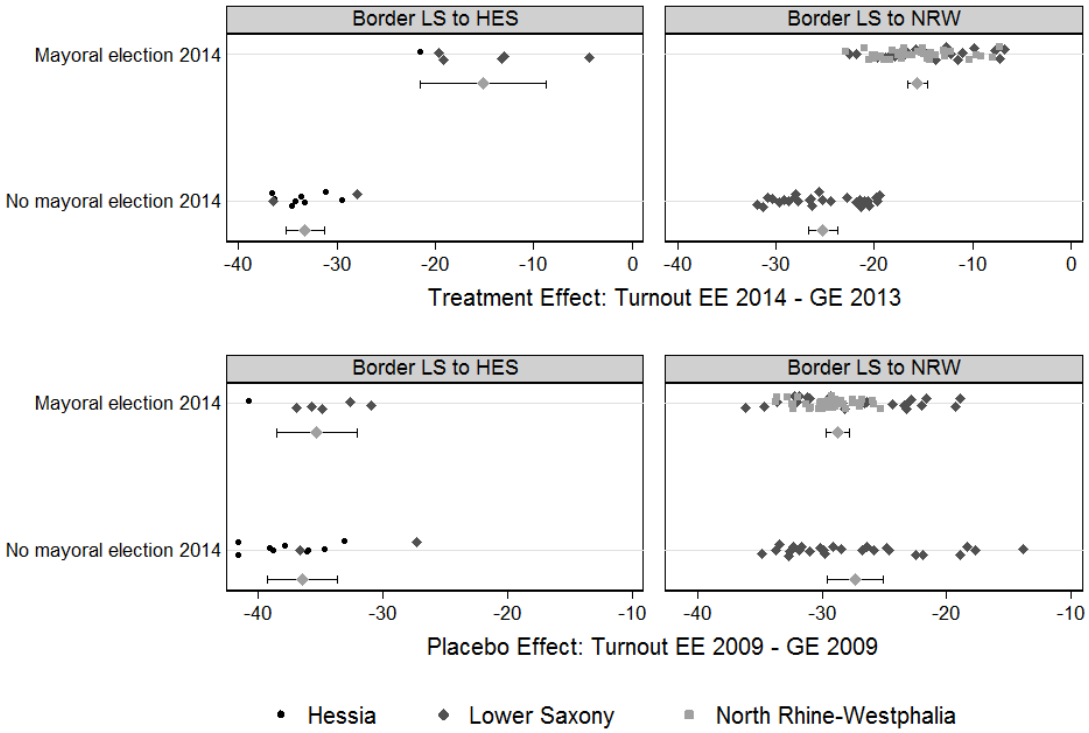

Figure 3: *The table shows a comparison of the turnout differential (European Election to closest Federal Election) for border communities in states primarily with or without CSOEs. The upper panel shows effects for the treatment period, the lower panel the placebo distribution for the pre-treatment period. Left panel compares municipalities in Hessia (HES; no CSOEs with few exceptions) with adjacent communities in Lower Saxony (LS; mostly CSOEs). Right panel compares adjacent communities in North Rhine-Westphalia (NRW; CSOEs) with municipalities in Lower Saxony (LS; partly CSOE). Grey diamonds with bars indicate the respective distribution means with 95% confidence intervals.*

	(1) DiD EE2014 - GE2013	(2) DiD EE2009 - GE2009
Mayoral/local election	11.45**	-1.020
	(0.977)	(0.955)
N	105	104

Standard errors in parentheses. * $p < .05$, ** $p < .01$

Table 11: *The table reports Average Treatment Effects (Model 1) and placebo estimates (Model 2) for communities along the state border of Lower Saxony (partly municipality-level mayoral elections) with North-Rhine Westphalia (municipality level local elections) and Hessia (partly municipality-level mayoral elections) following nearest neighbour matching of treated and control units on community centroid latitude and longitude with one match per observation (robust standard errors in parentheses).*

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