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Voting after a major flood: Is there a link between democratic experience and retrospective voting?

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Retrospective voting may be an effective instrument for overcoming moral hazard of politicians if voters evaluate the performance of elected representatives correctly. Whether democratic experience helps them to properly assess a policymaker's performance is less well understood. We analyze whether voters are more likely to vote for an incumbent party which launched a disaster relief program and whether voters' behavior is related to their democratic experience. Our identification rests on two natural experiments: a disastrous flood in Germany in 2013, and the separation of Germany into a democratic West and a non-democratic East after World War II. We find a two percentage points increase for the incumbent party in the flooded municipalities in the East compared to the West in the 2013 elections. Testing for several potential explanations, we deem it to be likely that voters with less democratic experience are easier prey to pre-election policies of incumbent parties.

Keywords: retrospective voting, natural disaster, democratic experience, relief program;

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1. Introduction

In democracies voters delegate power to elected representatives. An endemic problem arising for voters is how to reduce moral hazard on the part of politicians. A key idea has been that through retrospective behavior citizens can sanction poor performance of incumbents and select leaders who act competently (Key et al., 1966; Barro, 1973; Ferejohn, 1986; Fearon, 1999). Elections could thus be an effective means of enhancing the welfare of citizens if voters reward good performance and punish bad performance. While there has been work done on the effect of democratic experience on a range of political outcomes (see, e.g., Inglehart, 1990; Anderson & Mendes, 2006; Tavits & Annus, 2006), we know little about whether voters' evaluation of an incumbent differs with democratic experience.

In this paper we address the question of whether voters with more or less democratic experience reward a policymaker's performance differently. Our identification strategy rests on two natural experiments. First, we recur to the 2013 flood in Germany which affected households and businesses in East and West German municipalities in an unprecedented manner. The affected states and the federal government launched a major disaster relief program. In relation to our analysis, this relief program had one particularly appealing feature. The federal government which was up for election only a few months later on September 22nd, decided to match every euro spent by the federal states to help households, businesses, forestry and farming, and municipalities whose infrastructure was damaged. This particular feature of the policy program implies that we can actually analyze the voters' response to a program of the federal government that uniformly treated voters relative to the damage experienced (whose scale was rated by state level governments). Second, we argue that the separation of Germany into a non-democratic East and a democratic West Germany after World War II allows us to evaluate the effect of democratic experience on voting behavior following the disaster relief program. In particular, this set-up enables us to employ a diff-in-diff-in-diff strategy comparing the vote shares for the incumbent coalition parties of flooded municipalities with non-flooded municipalities (first difference) before and after the flooding (second difference) for East and West Germany (third difference). We can, therefore, elicit the potentially different behavior of less democratically experienced voters in East Germany as a response to a major relief program.

For our analysis we draw on high-resolution flood data that was kindly provided to us by the German Aerospace Center ("Deutsches Zentrum für Luft und Raumfahrt") which documented the natural disaster via overflights and from outer space. We merge this information on flooded and non-flooded areas with data on parties' vote shares for all federal elections from 1994 until 2013 on the municipal level – the smallest administrative unit in Germany.

Our main finding is that the incumbent party received a two percentage point larger vote share in the flooded municipalities in East Germany as compared to West Germany in the federal elections following the flood. This difference is sufficiently large to be decisive in a close election. The result is robust to a range of sensitivity tests. We run treatments that take account of the intensity of damages, change the underlying sample, and analyze state elections that took place in Bayern

(Bavaria, located in the West) and Sachsen (Saxony, located in the East) following the flood. In order to address identification issues arising from potentially time-variant unobserved variables, we also estimate the effect of democratic experience on voting behavior with the synthetic control method.

We find a difference in voting behavior between East and West German voters after the major flood. Our interpretation of the difference is that voters with less democratic experience were more easily convinced by the federal government's disaster program that the incumbent party did, overall, a good job in the legislative period which was about to end. Relating the differences in voting patterns to democratic experience appears to us as a very plausible interpretation. After all, besides having a different economic system, the other major difference between East and West Germany was the form of government.

We are well aware, however, that democratic experience may be only one cause of the heterogeneous voting patterns. Other differences between East and West German voters may exist that lead to the voting decisions that we observe. For example, one can still determine economic differences between East and West Germany in terms of per capita incomes. Moreover, East German citizens may value government intervention more and, consequently, reciprocate to a larger extent, or they may systematically differ in the strength of their party affiliation. We can rule out these potentially other underlying causes. We address regional differences in economic conditions by employing fixed effects for various jurisdictional levels in our regression analysis. Thus, we are differencing out these potentially confounding drivers. We rely on variation between municipalities within a district, or, in an alternative specification, within municipality variation when comparing East and West Germany. Furthermore, we rule out other mechanisms such as systematic differences between East and West German voters in reciprocity or strength of party affiliation with data from the German Socio-Economic Panel. We also provide further evidence in support of our most favored interpretation. We use invalid vote shares as an alternative variable to the East/West separation of Germany for measuring democratic experience. This analysis supports our interpretation of the main result. Finally, an analysis on political knowledge, as yet another indicator for democratic experience, between East and West corroborates these findings. In particular, we show with an individual-level analysis that voters with less political knowledge have higher odds of voting for the incumbent after having been affected by the flood.

We proceed in Section 2 by providing a literature review. Section 3 deals with identification issues. In particular, we describe the natural disaster and the policy response to it. Furthermore, we argue that the separation of Germany can be interpreted and used as a natural experiment to analyze the role of democratic experience. In Section 4 we present our results, and Section 5 concludes.

2. Literature review

Our contribution relates to three strands of literature: retrospective voting, voting after natural disasters, and the role of democratic experience for political outcomes.

Retrospective voting. – For elections to work as a disciplining device for policymakers it has to be the case that voters are actually able to retrospectively evaluate the

performance of policymakers. The literature on voters rewarding good and punishing bad performance of governments started by relating vote shares at the ballots to macroeconomic performance, see, e.g., Powell Jr & Whitten (1993), Markus (1988), or Lewis-Beck & Stegmaier (2007). For various reasons this approach has not been very fruitful. Most importantly macroeconomic outcomes are not randomly assigned, and there could actually be a reverse causality when elections are near, with policymakers diverting resources to boost the economy (see, e.g., Nordhaus, 1975; Hibbs, 1977; Alesina, 1988).

Voting after natural disasters .– In order to obviate these issues scholars have turned to exploiting natural disasters as exogenous events. The underlying idea has been that a rational voter should not hold a government responsible for something that is beyond its control. Among the first, following this line of research Abney & Hill (1966) found that hurricane Betsy, which struck southeastern Louisiana in 1965 did not have an effect on the following election of the mayor. Similarly, Bodet et al. (2016) report that the flood in the city of Calgary in 2013 had neither an effect on the support of the incumbent mayor nor on turnout.

However, there has been a series of research papers that cast doubt on the notion that voters are able to push governments towards welfare-enhancing public policies. Achen & Bartels (2004) report on shark attacks at the beaches of New Jersey in 1916 that depressed incumbent President Wilson’s votes. The same authors also looked into the effect of droughts and floods, finding that voters punish the incumbent party in national elections for those disasters. Sinclair et al. (2011) analyze voter turnout for the mayoral election in New Orleans following hurricane Katrina. They find that, overall, the flood decreased participation but voters who experienced a flood level of more than 6ft were more likely to cast a ballot. Another piece of evidence on voters’ apparent inability to evaluate policymakers’ performance is given by Wolfers (2002). He shows that voters hold regional policymakers accountable for the effects of oil price changes that are obviously not under their control.

Yet, a major difficulty with studies using natural disasters is that policymakers may have handled the consequences of a natural disaster poorly or may have taken insufficient measures to be prepared for a disaster. Consequently, it may be that voters evaluate the policymakers’ measures to prepare for a disaster or the policymaker’s performance managing the consequences of a disaster, rather than erroneously making him responsible for a natural disaster. A number of contributions have addressed this issue. Using survey data, Arceneaux & Stein (2006) find that voters, believing that the city administration in Houston was responsible for the insufficient flood preparation, punished the incumbent mayor after tropical storm Allison had hit the city in 2001. Healy et al. (2010) can show, based on a US data set on tornado damages to counties, that if the president issued a disaster declaration tornado damages increase the vote share. Moreover it is found that voters differentiate between economic losses and fatalities resulting from the tornado. Assuming that governments can only be made accountable for the former but not the latter, this also speaks for voters’ rationality, i.e. they do not blindly punish incumbents for natural disasters.

Looking deeper into the type of policies that steer voter behavior, Healy & Malhotra (2009) show that voters reward the incumbent presidential party for disaster

relief spending, but not for investments targeted at increasing disaster preparedness. Gasper & Reeves (2011) find that the electorate punishes presidents and governors for severe weather damage. But, consistent with rational voter behavior, they can also show that if the president rejects a request of the governor for federal assistance the president is punished and the governor rewarded. Finally, Bechtel & Hainmueller (2011) are interested in dynamic effects of government transfers after a natural disaster on voter behavior. They evaluate the effect of a large-scale targeted transfer program that followed the Elbe flooding in Germany in 2002. Bechtel & Hainmueller (2011) show that the transfers increased the vote share of the incumbent party by seven percentage points in the affected areas in the federal elections that immediately followed. 25% of the electoral reward was still there in the 2005 elections, but could no longer be detected in the elections of 2009.

While our contribution sits well with the existing studies on electoral behavior after disasters, we deviate from them most importantly in terms of the question that we try to answer. We would like to know if voters cast ballots as a function of their democratic experience. In that respect we complement studies on the effect of democratic experience on political outcomes. We now turn to a review of these contributions.

Democratic experience as a voting driver .– There is a literature on democratic learning that looks at vote choice over time in Eastern Europe after the fall of the Iron Curtain. It shows how voters in these countries come to behave more like their West European counterparts. Stegmaier & Lewis-Beck (2009) document that Hungarian voters have moved towards rewarding the government for good times and punishing it for bad times, as suggested by retrospective voting. Similarly, Roberts (2008) finds that voters sanctioned politicians’ poor performance in 10 new democracies in Central and Eastern Europe, indicating that citizens learned quickly to hold governments accountable. As in the earlier studies on retrospective voting, however, vote shares are related to the macroeconomic performance in the respective countries. Moreover, it was shown that political satisfaction is positively correlated with the length of time that democratic institutions have persisted (Inglehart, 1990), and that democratic experience and the prevalence of strategic voting are linked (Tavits & Annus, 2006; Lago & i Coma, 2012). According to Finkel et al. (1989), regime stability is greater with more democratic experience, and political protest by potential minorities is particularly larger in young democracies (Anderson & Mendes, 2006). Finally, it has been argued that political budget cycles are mostly prevalent in new democracies (Brender & Drazen, 2005), once more suggesting that vote choices could be a function of democratic experience.

What has not been addressed in the literature on democratic experience is whether a voter’s response to government transfers is a function of democratic experience. In an in-depth analysis of the democratic values in the unified Germany, Rohrschneider (1999) convincingly argues that citizens were exposed to a learning democracy in East Germany with potential consequences for their vote choice compared to West German citizens. In particular, he asserts that “Unlike in a democratic system, which attaches considerable importance to citizens’ opportunities to scrutinize the political process, citizens were exposed to the notion that the control of those holding political power is secondary in a socialist state” (p. 37). Thus, the comparison of

voting behavior between East and West German municipalities after the disastrous flood in 2013, that was accompanied by a major relief program, may provide us with novel insights into the role of democratic experience with respect to voters' behavior.

It seems plausible to us that our set-up allows us to elicit whether the reaction of voters to the relief program is a function of their democratic experience. For a meaningful identification, however, it also needs to hold that politicians responded to the disaster in the same (non)professional way in East and West Germany. If this is the case, the analysis is not prone to the fallacy of measuring the reaction of voters to different disaster treatments. Furthermore, in order to assess whether voters' behavior can be linked to their democratic experience, it must hold that before the separation of Germany, the East and the West were fairly similar and voters' allocation to those regions was random. We turn to a discussion of these identifying assumptions in the following section.

3. Research design, data, and methodology

We address our research question by using the flood of 2013 and the separation of Germany into a non-democratic East and a democratic West Germany after World War II as natural experiments.

The flood .– As a consequence of heavy rainfalls from May until the beginning of July 2013, large areas in Germany, especially in the states of Bayern, Sachsen and Sachsen-Anhalt, were flooded (Deutscher Bundestag, 2013). Figure 1 provides an overview. The blue-colored areas were under water and the dark grey areas depict the municipalities that were at least partly affected by the flood, i.e. areas in these municipalities were under water. Experts considered the flood of 2013 as even more severe than the so-called “Jahrhundertflut” (centennial flood) in 2002. Damages to federal infrastructure were estimated at 1.3 billion euro. In addition, federal states declared damage of about 6.7 billion euro. The German Insurance Federation (Gesamtverband der Deutschen Versicherungswirtschaft) kept stock of about 180,000 damages in total among their insurance holders with a damage sum of approximately 2 billion euro.

The federal as well as the state governments launched emergency relief programs that targeted households, damages to homes, businesses, farming and forestry, and infrastructure damages in the municipalities. The legal framework for the disaster relief program consisted of a law (“Aufbauhilfefonds-Errichtungsgesetz”) decided upon on July 15th, 2013, and a decree, the so called “Aufbauhilfeverordnung”. The first payments within the disaster relief program already were made at the beginning of August 2013, i.e. well before the federal elections on September 22nd.

Features of the transfer program – together with the separation of Germany into a democratic West and a non-democratic East, to which we turn later – constitute a unique way of identifying the effect of a government program on economic voting. The decree clearly regulated the distribution of the financial resources of the fund. The fund was set up as a matching program in which the federal government matched every euro spent in the emergency relief programs of the federal states with an additional euro. These features make it very likely that the policy treatment was

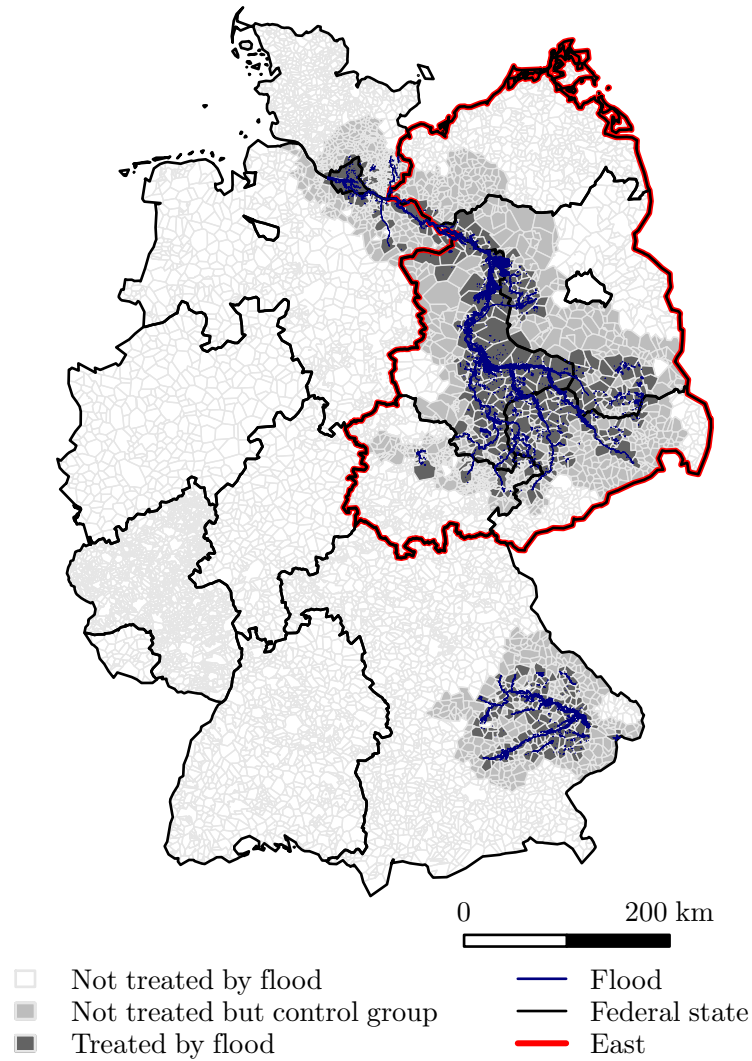


Figure 1: Flooded municipalities, June 2013.

Notes: 614 municipalities (distributed across 71 districts) were affected by the flood. Our control group comprises the 1,554 municipalities which were not affected by the flood but belong to a district in which at least one municipality was affected. Map (and all distance calculations) in Gauss-Krüger zone 3 projection (EPSG: 31467).

uniform across all flooded municipalities for the federal elections. In particular, voters were treated equally by the federal government relative to what the state government that had rated the extent of damages had awarded them. Thus, the matching of the funds by the federal government should also adequately address issues of unequal treatment. Such issues may arise by wealthier places having infrastructure that is more expensive to repair or regions facing different costs of living and, therefore, costs of damages.

The separation of Germany .– The historical events involving the splitting-up of Germany after the Second World War and its reunification in 1990 have previously been used as an identification strategy by Alesina & Fuchs-Schündeln (2007), Rainer & Siedler (2009), Heineck & Süßmuth (2013), and Friehe & Mechtel (2014). We give

an economic and historic account of the German separation in Appendix A. We differ from these previous studies such that we do not have information on where voters lived before re-unification in our baseline analysis. Thus, one may be concerned that identification could be confounded by migration flows after the separation of Germany and also Germany’s reunification. In Appendix B, based on the German Socio-Economic Panel (SOEP, 2015), we show that it is, however, very unlikely that migration distorts our results.¹

The data . – The German Aerospace Center (“Deutsches Zentrum für Luft und Raumfahrt”), in charge of providing information to the emergency units, documented the flood via overflights and from outer space. We gratefully received shape-files for Germany which allowed us to code areas affected and not-affected by the flooding.² We use a PostgreSQL database with PostGIS Add-on to match information on the spatial dimension of the flood with the vote shares of all parties that participated at the federal elections from 1994 through 2013.

The unit of analysis are municipalities. On 1.1.2014, there were 11,136 municipalities in Germany.³ We include municipalities that are either treated by the flood or located in a district, i.e. the next higher level of regional aggregation, with at least one flooded municipality. This sample composition should help us to ensure that the flooded and non-flooded municipalities are – in line with Tobler’s first law of geography – very similar. Tobler’s first law of geography is a well known stylized fact in economic geography and claims that “near things are more related than distant things” (Tobler, 1970, p. 236). Focusing on municipalities that are either flooded or located in a district with at least one flooded municipality results in 2,168 municipalities for 2013 (and 2,112 for 2009) to be included in our baseline sample.

These municipalities are distributed across 71 districts and 9 federal states. In Appendix D, we discuss the descriptive statistics of our data. In the legislative period from 2009 to 2013 there was a coalition government of the Christian Democratic Party (CDU) and the Free Democratic Party (FDP).⁴ Angela Merkel was both chancellor and party leader of the CDU. Accordingly, we show the vote shares for the CDU and the coalition government.

¹The SOEP is a representative longitudinal yearly survey, which includes some 30,000 individuals from 11,000 households in Germany.

²All data sources are listed in Table C.1 in the Appendix.

³Due to the constant restructuring of municipalities, we do not have voting data for all municipalities. We are able to recur to 10,856 (97.5%) municipalities in 2013 (and 10,697 (96.1%) in 2009). Municipalities (LAU-2: Local Administrative Unit) are the smallest administrative unit. Furthermore, note that Germany is a federal state with 16 states (NUTS-1: Nomenclature des unités territoriales statistiques) and more than 400 districts (NUTS-3).

⁴CDU runs for election in all German states but Bavaria. There, its sister party, the Christian Social Union (CSU), runs for election (and CDU does not). We jointly consider both parties under the label CDU since both have always formed a joined parliamentary group, always had a joint candidate for chancellor in federal elections and never (directly) competed in any election.

4. Empirical analysis

For estimating the effect of the federal government transfers paid after the flood on the vote share in flooded and non-flooded municipalities in East and West Germany we recur to the following model:

$$\begin{aligned}
 y_{i,t} = & c + \alpha_1 \cdot Flooded_i + \alpha_2 \cdot Post_flood_t + \alpha_3 \cdot East_i + \alpha_4 \cdot Flooded_i \cdot East_i \\
 & + \alpha_5 \cdot Flooded_i \cdot Post_flood_t + \alpha_6 \cdot Post_flood_t \cdot East_i \\
 & + \alpha_7 \cdot Flooded_i \cdot Post_flood_t \cdot East_i + e_{j(i)} + \epsilon_{i,t}
 \end{aligned} \tag{1}$$

where $y_{i,t}$ is the vote share of the incumbent party in the federal government in municipality i in election years $t = 2009, 2013$. We measure whether a municipality i was flooded in the year 2013 with an indicator variable $Flooded_i$. $Post_flood_t$ is zero for the election year 2009 and one for the election year 2013, and $East_i$ is one if the municipality i is in East Germany and zero otherwise. Finally, c is a constant, $e_{j(i)}$ a district fixed effect for all municipalities i in district j , and $\epsilon_{i,t}$ an error term.

The district fixed effects in all our specifications should help us to take account of differences between East and West German regions which may have an effect on voting decisions. While East Germany certainly differs in terms of having 41 years less of democratic experience as we argued earlier, four decades of a command economy had a transformative effect on the region's economic development. Large parts of East Germany have still not caught up with West Germany in terms of productivity or per-capita incomes. Moreover, we observe differences in age structure, in particular in the rural areas in East Germany, which may also have an effect on the voting behavior. With the inclusion of the district fixed effects ($e_{j(i)}$) we are differencing out these potentially confounding drivers as we rely on variation between municipalities within a district comparing East and West Germany.

We are mostly interested in the sign and significance of parameter α_7 on the triple interaction term. An estimated parameter that is statistically different from zero would indicate that voters in East Germany with less democratic experience vote differently as a response to the relief program following the natural disaster than voters in West Germany.

4.1. Baseline specification

Table 1 shows the results of our baseline specification. In columns (1) and (2), we estimate the effect of the disaster relief program for the incumbent's vote shares in the municipalities for East and West Germany separately. In column (3), we follow the specification given by Eq. (1) and the parameter of interest is on the triple interaction term. Following previous evidence that the party of the chancellor benefits most from economic voting under coalition governments in Germany (Debus et al., 2014), we take the vote share of the CDU as the left hand side variable.⁵

While there is no statistically significant effect for West Germany, the vote share for the flooded municipalities is 1.3 percentage points higher in East Germany in 2013

⁵Later, we run robustness tests with the combined vote share of the two incumbent parties (CDU and FDP).

compared to the previous election. The treatment effect increases to two percentage points (see column (3)) as we move to the full sample. The estimated parameter is significant at the 1% level. In relation to the control variables, we observe an increase in the average vote share for the CDU in the 2013 election of 8.3 percentage points in the West and an even slightly higher increase in the East (9.4 percentage points). Overall, the regression explains more than 70% of the variation in the data.⁶

Table 1: Diff-in-Diff-in-Diff estimates of incumbent vote share on flood for federal elections in 2009 and 2013.

	CDU_share _{i,t}			
	West	East	All	
	(1)	(2)	(3)	(4)
Flooded _i × Post_flood _t	-0.007 (0.005)	0.013** (0.004)	-0.007 (0.005)	-0.007** (0.002)
Flooded _i × Post_flood _t × East _i			0.020** (0.007)	0.018** (0.003)
<i>Controls:</i>				
Flooded _i	-0.004 (0.004)	-0.020** (0.003)	-0.004 (0.004)	
Post_flood _t	0.083** (0.003)	0.094** (0.003)	0.083** (0.003)	0.083** (0.001)
Flooded _i × East _i			-0.016** (0.005)	
Post_flood _t × East _i			0.011** (0.004)	0.010** (0.002)
District Fixed Effects	Yes	Yes	Yes	Yes
Municipality Fixed Effects	No	No	No	Yes
Adj. R ²	0.72	0.61	0.72	0.86
F	384.9	887.0	636.1	3557.7
N	1878	2402	4280	4280

Notes: Across columns, the dependent variable is the incumbent (CDU) vote share in LAU-2 municipality i at federal election in t . Again across columns, we only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality. We include (but do not show) a constant in all regressions. Robust (Huber-White) SE in parentheses; ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Results shown in columns (1) to (3) are based on a specification that includes fixed effects on the district level that take account of common characteristics of municipalities in a given district. As previously argued, these may relate to the age structure of the voters, income, or other characteristics that may have an effect on voting behavior. A similar concern may be voiced with respect to heterogeneity on the municipal level. To address these concerns, we already decided to only compare similar municipalities by restricting the sample to municipalities located in a district with at least one flooded municipality – in keeping with Tobler’s law. In a further effort, we re-estimate our model including not only district fixed effects

⁶The significance levels of our estimates are based on robust standard errors (Huber-White).

Clustering on the district level also provides significant results on the one percent confidence level. In order to address potentially biased standard errors with regional clusters (see Cameron & Miller, 2015), we additionally conduct a permutation test. We generate a placebo distribution for the estimate on the triple interaction effect (Flooded_i × Post_flood_t × East_i) by randomly shuffling the assignment of municipalities to East and West Germany. We find that only in four out of 1000 cases the placebo estimates are larger than our estimate in column (3). Thus, it appears to be very unlikely to get an estimate as ours by chance.

but also municipality fixed effects, see column (4). Our results are not sensitive to controlling for heterogeneity at the municipal level. In other words, restricting the sample to municipalities that are part of a district where at least one municipality was flooded already satisfactorily deals with potentially unobserved heterogeneity at the municipal level.

Common trend assumption .– In order to rule out that the results retrieved so far are driven by an underlying trend in voting behavior that was different for East and West German municipalities we run a regression explaining vote shares of the CDU for federal elections preceding the one in 2013.⁷ We interact the dummy on flooded municipalities and the East dummy with election year dummies preceding the flood. Table E.1 in the Appendix shows the results for these regression models. We are mostly interested in the estimated parameters of the interaction terms of the indicator variable on the flooded areas in 2013 with the indicator on East Germany, and the years of the preceding federal elections. The base is the election year of 1994. With the exception of the elections in 2002, the vote shares of the incumbent in the legislative period 2009 to 2013 do not differ with respect to the base year, see column (1). In the election years 1998, 2005, and 2009 the CDU did not have a significantly different vote share from the then flooded municipalities in the East compared to 1994. There is a deviation from the common trend for 2002 which, however, is not surprising given that in the run up to the federal elections in 2002 there was a large (but smaller compared to 2013) flood with high water at the river Elbe. We will address potential concerns with respect to variables that, in general, may effect the voting outcome and vary with time, applying synthetic control groups in Section 4.2. Overall, we are, however, confident that the treatment effect of the 2013 flooding that we are detecting on the municipalities in the East for the federal elections is not confounded by a violation of the common trend assumption.

4.2. Robustness

We have already addressed different economic conditions still existing in large parts of East Germany as a potentially alternative explanation of the voting behavior by including district or municipality fixed effects. In addition to this we change the underlying sample in various ways (see Appendix E.2) and, in a more elaborate extension, we evaluate not the federal elections but state elections that took place in Bayern (West) and Sachsen (East) following the flood (see Appendix E.3). In all of these robustness checks we arrive at similar results as before.

In the following, we report on two further exercises. First, we allow our treatment variable to vary with the intensity of the damages. Second, we change our estimation approach by applying the synthetic control method.

Intensity of damage .– In the course of the analysis we defined a treated municipality as one for which parts of the area were flooded irrespective of the magnitude of the disaster. As there may be concerns that this is a too broad measure, we extend our analysis using additional information on the intensity with which districts (not municipalities) were affected by the flood provided by the Gesamtverband der

⁷Table D.2 in the Appendix contains descriptive statistics for the federal elections in the years 1994, 1998, 2002, and 2005.

Deutschen Versicherungswirtschaft (2015). This is aggregated data stemming from insurance companies on the number of insurance cases that arose due to the flooding on the district level. As this piece of information is only available for the district but not for the municipal level, we assign to districts an indicator variable that is zero for all entities that were only mildly affected by the flood, and one for all districts for which the share of insurance cases exceeded 2.9% – a threshold above which insurance companies consider districts as heavily affected. Although this additional source does not reveal information on the financial magnitude of a claim, using it may uncover how far the effects evaluated up to now hold. What this alternative approach requires, however, is the assumption that all municipalities within a district were equally strong affected as measured by the share of reported insured events. In order to include this additional piece of information in our analysis, we multiply the indicator variable $Flooded_i$ by the indicator variable on the intensity of the disaster ($Intensity_Dummy_i$). In Table E.2 (Appendix), column (1) shows the results. As in the baseline regression, we get a significant treatment effect of about two percentage points. Moreover, the effect occurs to be homogenous with respect to the intensity of the natural disaster. Voters in municipalities where the reported share of claims in the district is larger than 2.9% do not behave differently than voters living in less affected municipalities.

Synthetic control group .– For comparative studies, researchers are increasingly using the synthetic control method proposed by Abadie et al. (2010). In a nutshell, the synthetic control group is a weighted average of the available control units. The construction of synthetic control groups may better address the issue of having appropriate controls that reproduce the counterfactual outcome trajectory that the municipalities would have experienced in the absence of the governmental transfers. According to Abadie et al. (2010, p.494) “[r]elative to traditional regression methods, transparency and safeguard against extrapolation are two attractive features of the synthetic control method.” In particular, the synthetic control method extends the difference-in-difference framework that we used in our preceding analysis to allow for the effects of unobserved variables on the voting outcome to vary with time.

Applying the synthetic control group, we start by taking the first differences in vote shares for the CDU, and use those as the outcome variable. This allows us to get rid of level effects. These are caused by some of the flooded municipalities in Bayern that have vote shares for the incumbent party at levels for which no comparable flooded municipalities in the East exist. In technical terms, not using first differences would have caused problems in obtaining a weighted combination of untreated units because the treated units would have fallen far from the convex hull (see also Abadie et al., 2015).⁸ Thus, we predict the changes in the CDU vote shares for the election years 1998, 2002, 2005, and 2009. The election year 2013 is our post-treatment year. As prediction variables we use all lagged changes in the

⁸Doudchenko & Imbens (2016) also address these issues arising from the imposition of the restrictions of a zero intercept and positive weights adding up to one in the synthetic control method. They propose an alternative estimation approach based on a “best subset” of controls. This procedure relaxes the assumptions that the intercept between treated and un-treated units is zero and the weights add up to one. Nikolay Doudchenko and Guido Imbens kindly shared their R-code with us. We used this estimation technique (now on vote share *levels*) on our data with qualitatively similar results. These results are available upon request.

CDU vote shares. The donor pool consists of the non-treated municipalities which are of similar size as measured by the number of eligible voters.⁹

Figure 2 summarizes the findings. Panel (a) compares the flooded municipalities in East Germany with an East German donor pool of non-flooded municipalities. For each of the flooded municipalities we construct a synthetic control group. As Panel (a) shows, on average, the flooded municipalities and the synthetic control groups follow each other closely in the pre-treatment election years. For example, in 1998, the CDU lost about 14 percentage points with respect to the election in 1994 in treated and synthetic control municipalities. Following the policy treatment in 2013, the CDU gained more votes in flooded municipalities than in their synthetic counterfactuals. Running the same exercise for West Germany with a donor pool of West German municipalities we can, again, construct synthetic control groups that on average follow closely the flooded municipalities, see Panel (b). Now, however, the post-treatment shows a lower gain for the CDU votes in the treated municipalities when we compare them to the synthetic controls. Finally, we compare the treated East German municipalities with a synthetic control group obtained from the treated West German municipalities. Panel (c) shows that it is possible to construct on average a meaningful comparison group. Again, both trajectories follow each other closely until the policy treatment. Post-treatment, the increase in the CDU vote share is by about two percentage points larger in the flooded municipalities in the East compared to the West. While the graphical inspection already confirms our previous results, we show in Appendix E.4 that voting patterns also differ in a statistical sense.

4.3. Mechanisms

We are confident that we robustly identify differences in the voting behavior following the disaster relief program between East and West Germany. While we interpret these findings as evidence for retrospective voting being a function of democratic experience, we are aware that other differences between East and West Germany may drive the voting pattern we observe. Besides the form of government, economic systems between East and West Germany differed. The consequences of this, e.g. in terms of per capita incomes, are still observable and may actually result in different voting behavior between East and West Germany. However, these differences were already ruled out by us as an alternative explanation before as we included in our regression analysis fixed effects for various jurisdictional levels. Thus, we difference out these potentially confounding drivers and only rely on variation between municipalities within a district (with District Fixed Effects) or variation within a district over time (with Municipality Fixed Effects) comparing East and West Germany. Other, competing explanations, however, still exist, and we turn to the ones which appear most obvious to us next. In a final step, we return to our most favored explanation and present more evidence in support of the democratic experience channel. In particular, we present individual-level evidence on voters with different degrees

⁹For more information on the technical issues of how we implement the synthetic control method, see the Notes to Figure 2.

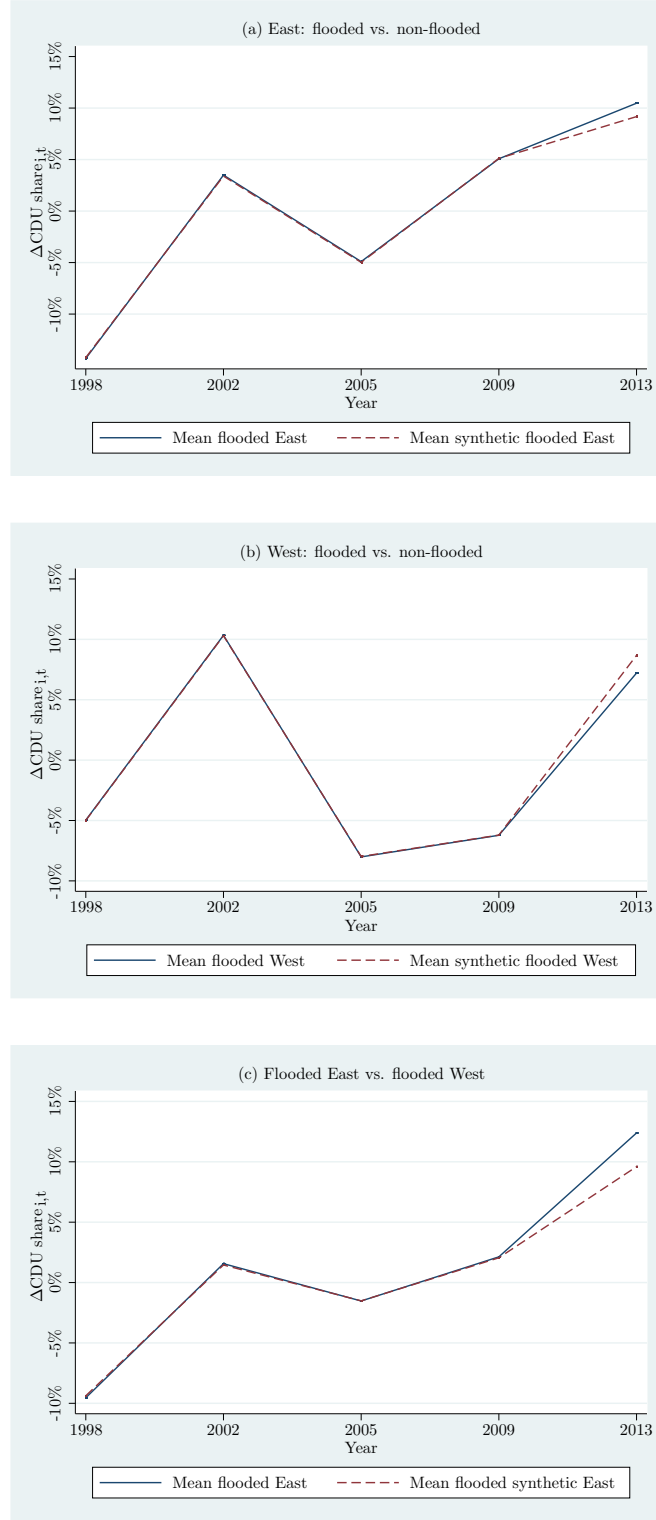


Figure 2: Synthetic control group figures.

Notes: We implement the synthetic control group method using the Stata version of the package described in Abadie et al. (2011). The outcome variable is the first difference of the CDU share: $\Delta \text{CDU share}_{i,t} = \text{CDU share}_{i,t} - \text{CDU share}_{i,t-1}$. We use the nested option and predict with $\Delta \text{CDU share}_{i,1998}$, $\Delta \text{CDU share}_{i,2002}$, $\Delta \text{CDU share}_{i,2005}$, and $\Delta \text{CDU share}_{i,2009}$. We call synth for each treated municipality. The donor pool consists of the non-flooded municipalities that are of similar size measured by the number of eligible voters. In panel (a), we illustrate the mean of flooded municipalities in the East and the mean of the synthetic control group. The mean is constructed for 235 flooded municipalities in the East for which the synth algorithm converged. In panel (b), we compare the mean of the flooded municipalities in the West with the mean of the synthetic control group. The donor pool consists of non-flooded municipalities in the West of similar size in terms of eligible voters. Furthermore, our comparison also conditions on districts for which the pre-treatment fit between flooded and non-flooded has a pre-treatment Root Mean Squared Prediction Error smaller than 0.005. This procedure results in 100 treated municipalities in the West. Finally, in panel (c), we show the mean of flooded municipalities in the East and the mean of the synthetic control group with a donor pool of flooded municipalities in the West. Here, our comparison conditions on municipalities for which the pre-treatment Root Mean Squared Prediction Error is smaller than 0.005. This procedure results in 31 treated municipalities in the East included in the comparison.

of democratic experience living in flooded and not flooded electoral districts, and voted in the 2009 and 2013 elections.

4.3.1. Alternative channels

Turnout .– In order to learn more about whether the results presented so far are potentially connected to changing turnout rates, we replicate the baseline analysis and substitute the dependent variable. Table 2, column (1), presents the results of the effect of the flood on the turnout rates. The estimated parameter on the triple interaction term implies that compared to the West the turnout rates in the municipalities in the East were 2.3 percentage points higher in the flooded areas in 2013 than in 2009. Thus, it is not only the case that East German voters living in flooded areas more likely voted for the incumbent party. They were also more likely to show up at the ballots.¹⁰

We are unable to relate the success of the incumbent party to the higher voter turnout in the respective municipalities given the data at hand. For this we would need voter migration matrices for the municipal levels. This data does not exist. There is, however, national-level survey data provided by infratest dimap (2013) drawing on information from exit polls. From the answers of those voters surveyed right after they went to the ballots matrices for voter migration can be constructed which reveal that the parties disproportionately benefited from the in general higher voter turnout. By far, the incumbent party (CDU) benefited most with in total 1.13 million additional votes in 2013 from voters who did not vote in the preceding federal election. The second largest net gain in terms of absolute votes was received by the SPD with 360.000 votes. Thus, in relation to its total votes in 2013, the CDU gained 7.6% from higher voter turnout, and the SPD 3.8% only. While we are aware that this descriptive evidence does not establish a causal link between higher turnout and voting for the incumbent party, we think that it at least suggests that the relative success of the incumbent party in the flooded areas in East Germany might partly be driven by a higher turnout in these municipalities.

Party identification .– An alternative interpretation of our finding could be that the difference in voting patterns between East and West Germany is related to differing strengths of party identifications or ideological attachment (see, e.g., Lindbeck & Weibull, 1987) on both sides of the former Iron Curtain. If party identification of voters living in East Germany was lower than for those voters living in West Germany, a transfer paid by the incumbent party to those affected by the flood could lead to relatively more East German voters casting their ballot for the incumbent party. That is, voters less attached to parties may more easily switch and vote for the incumbent party as a response to the transfer. A prerequisite for such a mechanism would have to be that there are systematic differences in the stability of party identifications between East and West German voters. In order to evaluate this question further, we analyze data on party identification provided by the German Socio-Economic Panel (SOEP, 2015). There, households are asked whether they identify with a particular party. We compare a voter’s party identification in

¹⁰In column (2) of Table E.1 (Appendix), we test the common trend assumption for turnout as the dependent variable. The estimates do not indicate a violation of the common trend assumption.

Table 2: Difference-in-Difference estimates for turnout as dependent variable [column (1)] and alternative measures for democratic experience [columns (2) and (3)].

	Turnout _{<i>i,t</i>}	CDU_share _{<i>i,t</i>}	
	(1)	(2)	(3)
Flooded _{<i>i</i>} × Post_flood _{<i>t</i>}	-0.003 (0.005)	-0.010 (0.008)	-0.011 (0.008)
Flooded _{<i>i</i>} × Post_flood _{<i>t</i>} × East _{<i>i</i>}	0.023** (0.007)		
Flooded _{<i>i</i>} × Post_flood _{<i>t</i>} × Avg_pre-flood_invalid_share _{<i>i</i>}		0.979* (0.481)	
Flooded _{<i>i</i>} × Post_flood _{<i>t</i>} × Wght_avg_pre-flood_invalid_share _{<i>i</i>}			1.068* (0.477)
<i>Controls:</i>			
Flooded _{<i>i</i>}	-0.002 (0.004)	-0.006 (0.006)	-0.003 (0.006)
Post_flood _{<i>t</i>}	-0.007** (0.002)	0.082** (0.005)	0.085** (0.004)
Flooded _{<i>i</i>} × East _{<i>i</i>}	-0.021** (0.006)		
Post_flood _{<i>t</i>} × East _{<i>i</i>}	0.052** (0.004)		
Avg_pre-flood_invalid_share _{<i>i</i>}		-0.193 (0.239)	
Wght_avg_pre-flood_invalid_share _{<i>i</i>}			0.118 (0.219)
Flooded _{<i>i</i>} × Avg_pre-flood_invalid_share _{<i>i</i>}		-0.495 (0.333)	
Flooded _{<i>i</i>} × Wght_avg_pre-flood_invalid_share _{<i>i</i>}			-0.683* (0.336)
Post_flood _{<i>t</i>} × Avg_pre-flood_invalid_share _{<i>i</i>}		0.371 (0.288)	
Post_flood _{<i>t</i>} × Wght_avg_pre-flood_invalid_share _{<i>i</i>}			0.264 (0.272)
District Fixed Effects	Yes	Yes	Yes
Adj. <i>R</i> ²	0.46	0.72	0.72
F	86.0	492.2	492.1
N	4280	4184	4184

Notes: In column (1), the dependent variable is the turnout in municipality *i* at federal election in *t*. In columns (2) and (3), the dependent variable is the incumbent (CDU) vote share in LAU-2 municipality *i* at federal election in *t*. Avg_pre-flood_invalid_share_{*i*} indicates the average invalid vote share in municipality *i* over the federal elections in 1994, 1998, 2002 and 2005. Wght_avg_pre-flood_invalid_share_{*i*} indicates a simple weighted average invalid vote share in municipality *i* over the federal elections in 1994 (weight 1/10), 1998 (weight 2/10), 2002 (weight 3/10) and 2005 (weight 4/10). In columns (2) and (3), we loose, in comparison to Column (1) of Table 1, 2.2% of the observations since we do not have information on pre-flood invalid votes for all municipalities. Across columns, we only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality. Again across columns, we take into account the elections in 2009 and 2013. We include (but do not show) a constant in all regressions. Robust (Huber-White) *SE* in parentheses; + *p* < 0.1, * *p* < 0.05, ** *p* < 0.01

2009 with her answer in 2013. More specifically, we calculate the share of voters who reported identifying with a particular party in 2009 and still do so in the year of the following federal election in 2013. The shares for all the parties, comparing East and West Germany, are reported in Table F.1 in the Appendix. There does not appear to be a pattern that supports an interpretation of our findings along the lines of differing party identifications.

Reciprocity . – Differing reciprocal behavior of East and West German voters could be another explanation for our findings. Finan & Schechter (2012) provide evidence that politicians target reciprocal individuals for vote buying. According to Finan & Schechter, voters who are offered money or material goods in exchange for their votes

would then reciprocate because they take pleasure in helping the politician who has helped them. If reciprocal behavior explained the differences that we find between East and West German voters, we would have to observe that there are systematic differences in terms of social preferences between East and West Germany. Again, data provided by the SOEP may help to analyze this channel more profoundly. We look into the answers of the panelists to various questions asked in the SOEP on their positive reciprocal behavior, i.e. whether they return favors. If these answers differ at all (results are available upon request), then West Germans have a larger tendency to reciprocate than East German voters. Therefore, reciprocal behavior on both sides of the former border is the reverse of what it would have to be in order to explain the voting pattern. This result is consistent with a more recent finding on the role of reciprocal preferences for voters' behavior in the context of disaster relief programs by Bechtel & Mannino (2017).

4.3.2. More on democratic experience

The 2002 flood revisited.— Earlier on, we mentioned another flood that took place in Germany in the year 2002. Until the flood of 2013 this was considered the most disastrous in the preceding 100 years. This flood was also accompanied by a government transfer program, and Bechtel & Hainmueller (2011), as already reported, find a sizeable effect of the transfer program on the vote share of the Social Democratic Party (SPD) which was the incumbent party then. They base their analysis on electoral districts (not municipalities) along the Elbe river such that only two of the 29 treated electoral districts were in West Germany. We can actually extend their data set by considering areas in Bayern (West Germany) that were flooded along the Donau river, see Bundesministerium der Verteidigung (2013, p. 13). This adds five observations on flooded electoral districts in West Germany to the Bechtel & Hainmueller data which we downloaded from the journal's website. Re-estimating their model (which underlies the results reported in Bechtel & Hainmueller (2011, Table 1)) on the extended data including an interaction term for the East yields what we show in column (8) of Table E.2 in Appendix E.2. We focus on the interaction term of the treatment variable with the indicator variable on East German electoral districts. The estimates confirm the distinct voting pattern between East and West Germany that we find in our analysis of the 2013 flood also for the disaster of 2002. Importantly, the positive effect on the incumbent vote share is much larger in the East for the 2002 flood in comparison to 2013. This is exactly what we would expect if retrospective voting is indeed a function of democratic experience since democratic experience in the East should have been lower in 2002 than in 2013.

Invalid votes.— So far we measured democratic experience with a dummy that divided municipalities into being located in the former GDR and West Germany. One may ask whether other measures of democratic experience could be applied and yield comparable results. Trying to explain the occurrence of invalid votes, scholars have, among other sets of explanations, resorted to democratic experience (Power & Roberts, 1995; Ugglä, 2008). It is argued that voters who have seen less of parliamentary politics may know less how to vote. Thus, an invalid vote may indicate a lack of knowledge about the electoral system or a lack of democratic experience.

For our level of analysis, i.e. municipalities, we have information on invalid votes that we may use as an alternative proxy for democratic experience. Substituting our dummy “East” with the municipal shares of invalid votes in our main specification yields results as shown in Table 2. We present two specifications. For the regression in column (2), we calculate averages of invalid vote shares for the elections in 1994, 1998, 2003, and 2005, i.e. up to the two elections for which we compare the effect of the disaster relief program on the incumbent’s vote share. On average 1.6% of the votes were invalid. In the second specification, we do not calculate simple averages but weigh invalid vote shares in the more distant past less than more recent ones.¹¹ If invalid vote shares were an appropriate proxy for democratic experience and our interpretation of our previous findings was correct, we would expect a positive coefficient on the triple-interaction term. This is the case in both of the specifications. A one percentage point higher share of invalid votes in the past increases the vote share for the incumbent party in the flooded municipalities by about one percentage point.

While we believe that this additional piece of evidence speaks in favor of our interpretation, we are hesitant to overly emphasize it. Invalid votes may be a measure for democratic experience but another set of explanations for invalid votes treats such votes as conscious choices or the result of social structural characteristics (see, e.g., McAllister & Makkai, 1993; Ugglä, 2008). Voters may just use spoiled votes to express their discontent with the political system or may be unable to vote properly because of social marginality and language deficiencies. Thus, our results on using invalid votes as a proxy for democratic experience should be judged in the light of the existence of these alternative interpretations.

Political knowledge .– The lack of democratic experience could be reflected in voters’ political knowledge. Measuring political knowledge is a long-running topic in political science (see, e.g., Converse, 1975) and has also been researched in the German context, comparing East and West German voters after reunification. Maier (2000) provided evidence on political knowledge of East and West German voters in 1998 based on a question that asks which of the two votes, first vote or second vote, is pivotal for the allocation of seats in the federal parliament. While in West Germany 52.4% knew that it is the second vote, only 42.8% gave the correct answer in East Germany. We replicate the analysis on a more recent data set, the Short-term Campaign Panel (Roßteutscher et al., 2018) which is based on a survey of voters mainly conducted in 2017, see Table F.3 (Appendix). According to this source of information, there is still a substantial difference in the share of correct answers of almost five percentage points between East and West Germany. Further taking into account voters’ place of birth does not change the main result. Quite interestingly, as we slice through the sample by birth decade we see large differences in the share of correct answers for the older cohorts. For the younger cohorts that received a substantial part or all of their schooling in the reunified Germany, the difference shrinks or even turns signs. These results are robust to analyzing other questions on political knowledge such as on the meaning of the 5% threshold in parliamentary

¹¹More specifically, invalid vote shares were weighted with 1/10 in 1994, 2/10 in 1998, 3/10 in 2003, and 4/10 in 2005. Table F.2 in the Appendix contains descriptive statistics for the alternative measures for democratic experience.

elections (not shown here). In sum, we believe that these results on an east-west divide in political knowledge speak very much in favor of our interpretation of the main findings.

Individual-level analysis.— In a final step, further elaborating the role of democratic experience, we take information on the location of birth and political knowledge of voters from yet another data set in order to conduct an analysis of voting behavior on the micro-level. The German Longitudinal Election Study (Rattinger et al., 2016) allows us to construct a data set on people’s votes (or intentions to vote) merging our information on flooded areas in Germany with 299 electoral districts. Thus, we have information on whether people voted for the incumbent, whether they lived in a flooded or not flooded electoral district, in combination with their political knowledge, area of birth, and several socio-economic controls. For the analysis, we define the dependent variable to be individual j ’s voting decision for the CDU in 2013 or 2009, i.e., it is one if individual j voted for CDU in t , otherwise it is zero. Whether an individual is living in a flooded electoral district is captured by the variable $Flooded_{j(k)}$. It is one if individual j lives in one of the flooded electoral districts k in Germany, and otherwise zero. Furthermore, we define a dummy variable $No_political_knowledge_j$ which is one if individual j ’s answer to the question on which vote (first or second) is pivotal for the allocation of seats in the federal parliament is not correct. As in our main analysis, our control group consists of respondents living in an electoral district which is a direct neighbor of a flooded electoral district. In Appendix F.3 we give further details on this micro-level data and also present descriptive statistics.

Table 3 summarizes the main results of the regression analysis. In columns (1) and (2), we compare the odds of voting for the incumbent for individuals born in West and East Germany, respectively. As before, when we based our analysis on municipality level data, voters born in the West affected by the flood do not vote differently from not affected voters. Voters born in the East behave differently. In fact, a voter born in the East and affected by the flood has 3.4 times higher odds of voting for the incumbent than a voter living in an electoral district not affected by the flood. We turn to the alternative measure for democratic experience, i.e. political knowledge. Columns (3) to (5) confirm our previous results. Voters with no political knowledge and living in an electoral district that was flooded have 6.9 times higher odds of voting for the incumbent than voters with political knowledge. These results appear to be robust to the inclusion of a large set of individual controls, electoral district fixed effects, and individual fixed effects. Overall, this micro-level evidence is again in line with our interpretation that less democratically experienced voters responded more positively to the relief program and voted for the incumbent.

5. Conclusions

Voters rewarding good performance of the incumbent policymaker and punishing bad performance could be an effective means to overcome the moral hazard problem that comes with delegating power to politicians. We address the issue of retrospective voting with an empirical analysis that draws on two natural experiments: an unprecedented flood that occurred in Germany in May to early July 2013, and the

Table 3: Individual level conditional logit estimates of voting for incumbent parties on lack of political knowledge.

	CDU _{<i>j,t</i>}				
	Born West	Born East	Born East & born West		
	(1)	(2)	(3)	(4)	(5)
<i>Treatment effects:</i>					
Flooded _{<i>j(k)</i>} × Post_flood _{<i>t</i>}	1.67 (1.10)	3.42** (1.61)	1.11 (0.52)	1.37 (0.73)	0.78 (0.49)
No political knowledge _{<i>j</i>} × Flooded _{<i>j(k)</i>} × Post_flood _{<i>t</i>}			6.93** (3.60)	9.21** (6.18)	17.36* (22.33)
<i>Controls:</i>					
Post_flood _{<i>t</i>}	0.68 (0.30)	0.51 (0.21)	0.97 (0.35)	0.88 (0.37)	1.50 (0.63)
No political knowledge _{<i>j</i>}			1.67 ()	1.54 (1.23)	
No political knowledge _{<i>j</i>} × Flooded _{<i>j(k)</i>}			0.39 (0.29)	0.29 (0.28)	
No political knowledge _{<i>j</i>} × Post_flood _{<i>t</i>}			0.29** (0.11)	0.28** (0.14)	0.22 (0.22)
Controls	No	No	No	Yes	No
Electoral district Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	No	No	No	No	Yes
N	65	242	323	295	66

Notes: We show odds ratios (exponentiated coefficients). Across columns, the dependent variable is individual *j*'s voting decision for CDU in 2013 or 2009, i.e., it is one if individual *j* voted for the incumbent parties in *t*, otherwise zero. No political knowledge_{*j*} is one if individual *j*'s answer to the question, which of the two votes in federal elections is pivotal for the allocation of seats in the federal parliament is not correct, otherwise zero. Flooded_{*j(k)*} is one if individual *j* lives in one of the 33 flooded electoral districts *k*. The control group consists of the individuals *j* living in one of the 24 electoral districts which share the border with a flooded electoral district. In column (4)), we include school degree level dummies, religion dummies, the age, the age squared, a male dummy and the natural logarithm of the average household income between 2013 and 2009 (on the individual level). We use the conditional logit estimator with fixed effects: in columns (1)-(4) for electoral districts and in column (5) for each individual respondent. Across columns, there are two observations for every of the remaining 323 respondents, one for 2009 and one for 2013. Note, that the conditional logit estimator drops all observations with no variation in the dependet variable (here CDU_{*j,t*}, for every electoral district or for each individual). Robust SE in parentheses (in column (5), clustered on electoral districts); + *p* < 0.1, * *p* < 0.05, ** *p* < 0.01

separation of Germany into a democratic West and a non-democratic East after World War II. As a consequence of the flood, the federal government, which was up for re-election in September, launched a major disaster relief program matching every euro spent by the state governments. This allows us to look into whether voting behavior after large disaster relief programs unfolded differently, possibly being a function of voters' democratic experience.

Our main results suggest that the incumbent party received a two percentage point larger vote share in the flooded municipalities in East Germany compared to the previous federal elections. This result is robust to a large set of sensitivity tests.

We interpret our findings such that less democratically experienced voters responded more positively to the relief program and voted for the incumbent. Further evidence for this interpretation comes from using invalid vote shares and political knowledge as alternative measures for democratic experience. An individual-level analysis confirms our evidence derived from municipality data: voters with less democratic experience had higher odds of voting for the incumbent after having been affected by the flood. Moreover, we can rule out potentially competing explanations. There is neither evidence on systematically differing party identifications nor on systematically differing reciprocal preferences between East and West Germany. Our estimation approach using fixed effects for different jurisdictional levels

also makes it unlikely that socio-economic factors such as the age structure of municipalities or per capita incomes drive the East West differences in voting behavior. Thus, our most favored explanation for the empirical finding is that democratic experience is indeed linked to retrospective voting. It appears that voters who have often seen programs similar to the disaster relief payments might have experienced that governments used public resources to cushion their actual performance in the past. Therefore, they may be less easy prey to discretionary policy-making.

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Online-Appendix

A. Separation of Germany

A successful identification requires that East and West German citizens were fairly identical before the separation after World War II so that, ideally, they only differ with respect to having 41 years less (1949 to 1990) democratic experience. The states in East and West Germany which constitute what is nowadays known as the unified Germany shared the same experience of the first democratic state (Weimarer Republik) and the totalitarian state that followed and ended with the defeat of Germany in the Second World War.

In addition to having a common political history, also from an economic point of view there were no systematic differences as evidenced by regional data on per-capita incomes, see Table A.1. One may, however, argue that what came after in terms of diverging economic developments caused by the two economic systems in East and West Germany may have an effect on the voting behavior in addition to the differing democratic experience. In fact, one can still observe lower productivity and per-capita incomes in large parts of East Germany compared to the West. To the extent that these economic differences have an effect on voting outcomes, we need to take them into account to measure the effect of democratic experience on voting outcomes appropriately. Therefore, we will introduce fixed effects on various jurisdictional levels in our econometric specification. This helps us to difference out economic differences at a fairly disaggregate level as our identification will rely on variation between municipalities within a district only, comparing East and West Germany.

After Germany had lost World War II, the country was split up by the Allies into so-called occupation zones which had already been decided upon at the conference in Jalta in February 1945. In 1949 the Federal Democratic Republic of Germany, and the German Democratic Republic (GDR), which did not have free elections, were founded. The latter was brought down in a peaceful revolution that culminated in the opening of the Berlin Wall in November 1989, and led to the reunification of West and East Germany on October 3rd, 1990.

B. Migration flows between East and West

As our unit of analysis are municipalities, one may be concerned that identification could be confounded by migration flows after the separation of Germany and also Germany's reunification. Citizens living in a municipality in East Germany nowadays might not have lived there during the time of the GDR. Analogously, citizens living in a municipality in West Germany may have been raised in the GDR. We are, however, confident that given the size of the migration flows, the latter do not seriously challenge our identification strategy. In 1961 the GDR built the Berlin Wall. Until then about 3 million people had emigrated to West Germany (Heidemeyer, 1994; Hubert, 1998). From then onwards until November 1989 borders were closed and no substantial migration took place. In order to check in how far migration be-

Table A.1: Per-capita incomes in the “Deutsche Reich” in 1936.

East	
Berlin-Brandenburg	1600
Königreich Sachsen	1270
Provinz Sachsen	1161
Thüringen	1087
<i>Ave. East</i>	<i>1279.5</i>
West	
Westfalen	1045
Rheinprovinz	1171
Hessen	1150
Hannover	1156
Schleswig-Holstein	1192
Hamburg	1746
Bayern	1149
Baden	1117
Württemberg	1348
<i>Ave. West</i>	<i>1219.3</i>
<i>Notes:</i> Data source: Petzina (2011).	

tween East and West Germany after the fall of the Iron Curtain may confound our data we consulted the German Socio-Economic Panel (SOEP) (SOEP, 2015). As it contains information of the place of residence before re-unification and afterwards, we can calculate ratios of citizens who lived in the GDR before re-unification and in the election years. Similarly, this can be arranged for West Germany. Accordingly, Table B.1 shows that the share of residents who lived on either side of the Iron Wall before and after re-unification does not drop below 95.1% in any of the election years. In consequence, it is very unlikely that migration distorts our results.

Table B.1: Places of birth and residence by East and West Germany

Current residence	Place of residence before reunification in %		Total obs.
	East	West	
All years			
East	98.0	2.0	24,702
West	4.0	96.0	72,783
1994			
East	99.4	0.6	2,279
West	3.0	96.0	5,394
1998			
East	99.0	1.0	2,769
West	4.3	95.7	7,050
2002			
East	98.0	2.0	4,668
West	3.6	96.4	14,738
2005			
East	97.7	2.3	4,374
West	4.2	95.8	13,574
2009			
East	97.2	2.8	3,362
West	4.4	95.6	13,199
2013			
East	96.8	3.2	2,354
West	4.9	95.1	6,684

Notes: Data source: German Socio-Economic Panel (GSOEP). Place of residence before reunification is based on a question in the wave of year 2003. Observations with current residence in Berlin are excluded.

C. Data sources

Table C.1: Data sources on administrative areas, damages, and voting outcomes.

Data	Description	Source	Availability
Damages	Insurance cases on district (NUTS-3) level	Gesamtverband der deutschen Versicherungswirtschaft, http://www.gdv.de/	Data freely available upon personal request at Gesamtverband der deutschen Versicherungswirtschaft
Federal election outcomes 2009, 2013	Votes on municipality (LAU-2) level	Statistisches Bundesamt, http://www.regionalstatistik.de/	Table 252-01-5 downloaded on 4.2.2016
Federal election outcomes 1994, 1998, 2002, 2005	Votes on municipality (LAU-2) level	Statistical Offices of Federal States	Data freely available upon personal request at Statistical Offices of Federal States
Flooded areas 2013	Information on flooded areas from overflights and outer space (TerraSAR-X scenes taken between 03.06.2013 and 18.06.2013, resolution 3.25 meters)	Deutsches Zentrum für Luft und Raumfahrt, http://www.dlr.de	Data available on personal request; in printable form the data is available here https://www.zki.dlr.de/article/2373 (last visit 10.11.2017)
German Longitudinal Election Study	Information on actual votes and vote intentions, and political knowledge	Rattinger et al. (2016)	Data can be accessed via GESIS Data Archive, Cologne. ZA5322 Data file Version 1.1.0.
Municipality maps	Administrative areas 1:250,000 as of 31.12.2013	Bundesamt für Kartographie und Geodäsie, http://www.geodatenzentrum.de	Downloaded on 04.11.2016
Short-term campaign panel	Information on voters' political knowledge	Roßteutscher et al. (2018)	Data can be accessed via GESIS Data Archive, Cologne. ZA6804 Data file Version 5.0.0.
State election outcomes Bayern 2008, 2013	Votes on municipality (LAU-2) level	Statistisches Bundesamt, http://www.regionalstatistik.de/	Table 601-015B09 downloaded on 30.11.2016
State election outcomes Sachsen 2009, 2014	Votes on municipality (LAU-2) level	Statistisches Bundesamt, http://www.regionalstatistik.de/	Table 601-015B14 downloaded on 30.11.2016
Reciprocity and party affiliation	Information on people's reciprocal behavior and party affiliation	German Socio-Economic Panel (GSOEP)	Data available from the GSOEP in Berlin

Notes: See footnote 3 for details on administrative levels.

D. Descriptive statistics

Table D.1 summarizes the main characteristics of our baseline data by municipalities in East and West Germany. 939 municipalities are located in West Germany and 1,229 in East Germany. 258 municipalities were flooded in 2013 in West Germany and 356 in East Germany. Numbers of observations for the year 2009 do not exactly match those for the year 2013 due some restructuring of municipalities. Measuring the size of municipalities by the number of eligible voters reveals that mostly larger municipalities were affected by the flooding in East and West Germany. Variation in the size of the municipalities that enter our analysis is large in the flooded as well as in the non-flooded entities. About two thirds of the voters showed up at the ballots, with turnout rates being slightly lower in the East both in the 2009 and 2013 federal elections. In the legislative period from 2009 to 2013 there was a coalition government of the Christian Democratic Party (CDU) and the Free Democratic Party (FDP). Angela Merkel was both chancellor and party leader of the CDU. Accordingly, we show the vote shares for the CDU and the coalition government. It reveals that in terms of absolute changes in the vote shares from 2009 to 2013, the CDU gained more votes in the flooded than in the non-flooded areas in East Germany but not in West Germany. Due to the great loss in vote shares for the FDP in 2013 compared to their result in 2009, the pattern is slightly different for the coalition government but still underpins the relatively higher success of the incumbents in the East in the flooded municipalities. In particular, the vote share for the flooded as well as the non-flooded municipalities declined by two percentage points in the West, whereas in the East it increased more in the flooded as compared to the non-flooded municipalities.

Table D.1: Descriptive statistics for federal elections in 2009 and 2013.

	2009							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	2479	3952	10106	78541	2731	4837	10929	37752
Turnout _{<i>i,t</i>}	.67	.058	.68	.058	.6	.068	.56	.054
CDU_share _{<i>i,t</i>}	.41	.088	.43	.096	.34	.073	.32	.055
CDU&FDP share _{<i>i,t</i>}	.56	.082	.58	.095	.45	.089	.43	.075
N	681		258		841		332	
	2013							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	2518	4022	10277	80121	2786	4754	10743	37106
Turnout _{<i>i,t</i>}	.67	.057	.67	.06	.64	.079	.62	.073
CDU_share _{<i>i,t</i>}	.49	.084	.51	.097	.43	.063	.42	.049
CDU&FDP share _{<i>i,t</i>}	.54	.082	.56	.093	.46	.066	.45	.051
N	681		258		873		356	

Notes: On 1.1.2014, there were 11,136 LAU-2 municipalities in Germany. Due to the constant restructuring of municipalities, we do not have voting data for all municipalities. We have voting data for 10,856 (97.5%) municipalities in 2013 (and 10,697 (96.1%) in 2009). These municipalities are distributed across 412 NUTS-3 districts and 16 federal states. We only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality into our analysis. This results in 2,168 municipalities for 2013 (and 2,112 for 2009), for 2009 and 2013 distributed across 71 NUTS-3 districts and 9 federal states. Table D.2 in the Appendix shows the descriptive statistics for the federal elections in 1994, 1998, 2002, and 2005.

Table D.2: Descriptive statistics for federal elections in 1994, 1998, 2002, and 2005.

	1994							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	2309	3778	10270	80768	2679	4998	20998	1.3e+05
Turnout _{<i>i,t</i>}	.76	.056	.74	.049	.72	.068	.67	.055
CDU_share _{<i>i,t</i>}	.51	.088	.53	.09	.45	.1	.42	.072
CDU&FDP_share _{<i>i,t</i>}	.58	.08	.59	.079	.49	.1	.45	.074
N	600		238		726		287	
	1998							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	2003	3121	9856	81500	2646	4873	10025	22781
Turnout _{<i>i,t</i>}	.79	.052	.78	.047	.79	.059	.75	.052
CDU_share _{<i>i,t</i>}	.45	.098	.49	.1	.31	.076	.28	.047
CDU&FDP_share _{<i>i,t</i>}	.51	.087	.54	.091	.34	.078	.32	.05
N	594		223		738		282	
	2002							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	2082	3215	9770	80788	2919	5044	9033	20196
Turnout _{<i>i,t</i>}	.8	.052	.81	.045	.71	.064	.68	.058
CDU_share _{<i>i,t</i>}	.54	.16	.61	.16	.33	.076	.31	.06
CDU&FDP_share _{<i>i,t</i>}	.59	.14	.65	.14	.4	.082	.38	.066
N	596		230		841		341	
	2005							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	2105	3237	9835	81202	2924	5007	8927	20206
Turnout _{<i>i,t</i>}	.77	.055	.78	.048	.72	.07	.7	.06
CDU_share _{<i>i,t</i>}	.47	.11	.51	.12	.29	.067	.27	.046
CDU&FDP share _{<i>i,t</i>}	.56	.1	.6	.11	.37	.079	.35	.057
N	599		231		852		345	

Notes: On 1.1.2014, there were 11,136 municipalities in Germany. Due to the constant restructuring of municipalities, we do not have voting data for all municipalities. We have voting data for 10,281 (92.3%) municipalities in 2005, for 10,214 (91.7%) in 2002, for 9,835 (88.3%) in 1998, and for 9,854 (88.5%) in 1994. These municipalities are distributed across 412 NUTS-3 districts and 16 federal states. We only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality into our analysis. This results in 2,027 municipalities for 2005, in 2,008 municipalities for 2002, in 1,837 municipalities for 1998, and in 1,851 municipalities for 1994.

E. Common trend assumption and robustness

E.1. Common trend assumption

Table E.1: Common trend assumption for incumbent vote share and turnout across pre-treatment federal elections [1994, 1998, 2002, 2005 and 2009].

	CDU_share _{<i>i,t</i>}	Turnout _{<i>i,t</i>}
	All	All
	(1)	(2)
<i>Common trend of treated west vs. east:</i>		
Flooded _{<i>i</i>} × East _{<i>i</i>}	-0.001 (0.007)	-0.014** (0.005)
Flooded _{<i>i</i>} × 1998 × East _{<i>i</i>}	-0.011 (0.009)	0.011 (0.007)
Flooded _{<i>i</i>} × 2002 × East _{<i>i</i>}	-0.044** (0.010)	-0.006 (0.007)
Flooded _{<i>i</i>} × 2005 × East _{<i>i</i>}	-0.007 (0.008)	0.008 (0.007)
Flooded _{<i>i</i>} × 2009 × East _{<i>i</i>}	0.003 (0.008)	-0.011 (0.007)
<i>Controls:</i>		
Flooded _{<i>i</i>}	-0.021** (0.005)	-0.014** (0.003)
1998	-0.060** (0.004)	0.029** (0.002)
2002	0.023** (0.005)	0.041** (0.002)
2005	-0.038** (0.004)	0.014** (0.002)
2009	-0.093** (0.004)	-0.084** (0.003)
1998 × East _{<i>i</i>}	-0.075** (0.005)	0.037** (0.004)
2002 × East _{<i>i</i>}	-0.130** (0.006)	-0.053** (0.004)
2005 × East _{<i>i</i>}	-0.113** (0.005)	-0.015** (0.004)
2009 × East _{<i>i</i>}	-0.009+ (0.005)	-0.041** (0.004)
Flooded _{<i>i</i>} × 1998	0.013* (0.007)	0.003 (0.004)
Flooded _{<i>i</i>} × 2002	0.050** (0.008)	0.024** (0.004)
Flooded _{<i>i</i>} × 2005	0.016* (0.007)	0.016** (0.004)
Flooded _{<i>i</i>} × 2009	0.005 (0.006)	0.015** (0.005)
District Fixed Effects	Yes	Yes
Adj. <i>R</i> ²	0.79	0.66
F	311.0	610.5
N	9835	9835

Notes: In column (1), the dependent variable is the incumbent (CDU) vote share in LAU-2 municipality *i* at federal election in *t*. In column (2), the dependent variable is turnout. We only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality. We include (but do not show) a constant in all regressions. Robust (Huber-White) *SE* in parentheses; + *p* < 0.1, * *p* < 0.05, ** *p* < 0.01

E.2. Robustness to changing the underlying sample

Given that municipalities differ in size and that the flooded municipalities are on average larger than the non-flooded areas, we re-analyzed the baseline regression by splitting the sample using the median size of the municipalities by East and West Germany as cut-offs, see Table E.2 again. Columns (2) and (3) present the results for municipalities with a relatively low number and high number of eligible voters, respectively. The treatment effect is 1.7 percentage points for the larger municipalities and 2.2 percentage points of the smaller ones.

So far only those municipalities were included which were located in a district with at least one flooded municipality. The rationale as outlined earlier on has been that this should make the flooded and non-flooded municipalities that we compare similar with respect to other characteristics that may influence the voting outcome. In columns (4) and (5), we challenge this approach and change the underlying sample. First we only consider all municipalities located in a federal state – rather than district – that was affected by the flood. Our results are robust to this more encompassing data set that includes perhaps less well comparable municipalities as non-treated entities. Second, we conduct a change in the definition of the control group to address potential spill-overs from treated municipalities to those municipalities close to the flooded ones. Those spill-overs may arise if one thinks that family members or friends of those voters affected by the flood live nearby and change their voting behavior with the voters to whom they feel close and who suffered from the flood. In that case, we would not apply an appropriate control group. The stable unit value treatment assumption (SUTVA) would be violated. The estimates shown in columns (5) take account of this by excluding municipalities as controls that are within a distance of 20 kilometers to the treated municipality. It transpires that our results are robust to this change in the specification.

So far, we have only compared the voting behavior at the federal elections in 2013 to the federal elections in 2009. In column (6) we extend the data set and include information on election years from 1994 onwards. This may unduely challenge our identification assumption that unobservable controls are constant over time. Nevertheless, the distinction in voting behavior between East and West Germany that we observed in our previous specifications is upheld.

From 2009 until the elections in 2013 the federal government was a coalition government composed of the CDU and the Freie Demokratische Partei (FDP) as the junior partner. Therefore, it may be interesting to look into whether the results hold if the vote share of the parties forming the coalition are considered rather than only looking into the vote share of the CDU, which provided the chancellor. The results presented in column (7) confirm the previous findings.

Finally, Table E.3 reports on a set of robustness checks that evaluate the extent to which the results are driven by the inclusion of certain federal states. The nine columns show the results when we run the baseline regression with the triple interaction and exclude municipalities of a federal state. We may drop municipalities from any of the 9 federal states included in our sample and still get significant estimates in the range of two percentage points. This is comforting as in some states like Bayern and Sachsen the flood was more widespread and more municipalities were therefore affected by the flood. For example, excluding Bayern from the sample drops the

Table E.2: Robustness of Diff-in-Diff estimates of incumbent vote share on flood for federal elections.

Dependent variable	Intensity		Municipality size		Control gr. 2		Control gr. 3		More t		Other dep. var.		Bechtel/Hainmueller			
					CDU_share $_{i,t}$						CDU&FDP_share $_{i,t}$		Δ SPD_share $_{i,t}$			
	All i	(1)	Large i	(2)	Small i	(3)	All i	(4)	All i	(5)	All i	(6)	All i	(7)	—	(8)
Flooded $_i \times \text{Post_flood}_t$	-0.008 (0.005)	-0.005 (0.006)	-0.005 (0.010)	-0.002 (0.005)	-0.002 (0.005)	-0.001 (0.005)	-0.018** (0.005)	0.002 (0.006)								
Flooded $_i \times \text{Post_flood}_t \times \text{East}_i$	0.021** (0.007)	0.017* (0.007)	0.022+ (0.013)	0.015* (0.006)	0.012* (0.006)	0.034** (0.006)	0.014+ (0.007)									
Flooded $_i \times \text{Post_flood}_t \times \text{Intensity_Dummy}_i$	0.004 (0.029)															
Flooded $_i \times \text{Post_flood}_t \times \text{East}_i \times \text{Intensity_Dummy}_i$	0.002 (0.030)															
Flooded2002 $_i \times \text{East}_i$															6.987** (2.066)	
Controls:																
Flooded $_i$	-0.004 (0.004)	-0.007+ (0.004)	0.010 (0.007)	-0.007+ (0.004)	-0.027** (0.010)	-0.002 (0.003)	-0.009* (0.004)									
Post_flood $_t$	0.083** (0.003)	0.081** (0.004)	0.085** (0.004)	0.078** (0.001)	0.078* (0.002)	0.025** (0.003)	-0.024** (0.003)									
Flooded $_i \times \text{Intensity_Dummy}_i$	0.023 (0.023)															
Post_flood $_t \times \text{Intensity_Dummy}_i$	0.017 (0.022)															
Post_flood $_t \times \text{East}_i \times \text{Intensity_Dummy}_i$	-0.032 (0.022)															
Flooded2002 $_i$															-4.900* (1.978)	
East $_i$															7.339** (0.537)	

number of observations from 4,280 to 3,388. Nevertheless, taking these states out of the sample does not change the main findings. Thus, our results seem not to be driven by the inclusion of a particular federal state in our sample.

E.3. State elections in Bayern and Sachsen

Besides the federal elections that we analyze in our baseline specification, there were elections for the state parliaments in Bayern (West) and Sachsen (East) soon after the flood. In fact, the elections in Bayern also took place in September 2013. Elections in Sachsen were about one year later in August 2014. It may be of interest to see whether our results can be replicated as we turn to those elections. We provide descriptive statistics on those two elections, see Table E.4, and a map (Figure E.1) of the flooded and non-flooded municipalities in the two states.

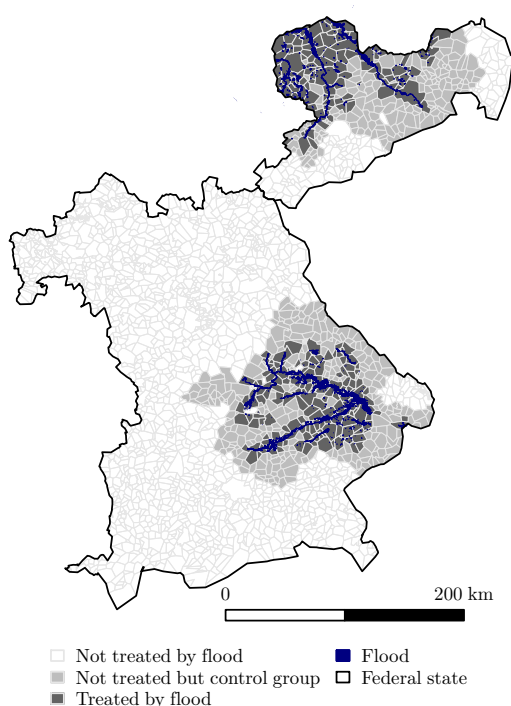


Figure E.1: Flooded municipalities in Bayern (West) and Sachsen (East), June 2013.

Notes: 268 municipalities were affected by the flood. Our control group comprises the 447 municipalities which were not affected by the flood but belong to a NUTS-3 area (district) in which at least one municipality was affected.

We are comparing slightly different treatments for several reasons now. The elections in Sachsen took place one year later. Moreover, one could argue that the state elections do not only differ in terms of timing but also in terms of the office for which candidates are competing (chancellor versus state-level prime minister), campaigns, or policy platforms. Nevertheless, given that the disaster relief program was financed one half each by the federal government and the state governments, we also expect an effect of the disaster relief program on the incumbents' vote shares for the state level elections.

Table E.3: Sensitivity of baseline results to neglecting federal states.

All but	CDU_share _{i,t}							
	Schleswig-Holstein	Hamburg	Niedersachsen	Bayern	Brandenburg	Mecklenburg-Vorpommern	Sachsen-Anhalt	Thüringen
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)
Flooded _i × Post_flood _t	-0.005 (0.006)	-0.006 (0.005)	-0.007 (0.006)	-0.006 (0.009)	-0.007 (0.005)	-0.007 (0.005)	-0.007 (0.005)	-0.007 (0.005)
Flooded _i × Post_flood _t × East _i	0.018* (0.007)	0.019** (0.007)	0.020** (0.007)	0.019+ (0.010)	0.023** (0.007)	0.020** (0.007)	0.023** (0.007)	0.016* (0.007)
<i>Controls:</i>								
Flooded _i	-0.000 (0.004)	-0.004 (0.004)	-0.004 (0.004)	-0.011+ (0.006)	-0.004 (0.004)	-0.004 (0.004)	-0.004 (0.004)	-0.004 (0.004)
Post_flood _t	0.080** (0.004)	0.083** (0.003)	0.082** (0.003)	0.087** (0.004)	0.083** (0.003)	0.083** (0.003)	0.083** (0.003)	0.083** (0.003)
Flooded _i × East _i	-0.020** (0.005)	-0.016** (0.005)	-0.016** (0.005)	-0.009 (0.007)	-0.019** (0.005)	-0.015** (0.005)	-0.016** (0.005)	-0.015** (0.005)
Post_flood _t × East _i	0.013** (0.004)	0.011** (0.004)	0.012** (0.004)	0.007 (0.004)	0.003 (0.004)	0.010* (0.004)	0.008+ (0.004)	0.018** (0.004)
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R ²	0.75	0.72	0.72	0.58	0.68	0.73	0.71	0.76
F	568.3	636.1	587.9	563.8	511.0	595.0	489.5	636.0
N	3618	4278	3958	3388	3770	3985	3908	3610

Notes: Across columns, the dependent variable is the incumbent (CDU) vote share in municipality i at federal election in t . We only include LAU-2 municipalities which are located in a NUTS-3 district with at least one flooded municipality. Across columns, we take into account the elections in 2009 and 2013. We include (but do not show) a constant in all regressions. Robust (Huber-White) SE in parentheses; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table E.4: Descriptive statistics for state elections in Bayern (West) and Sachsen (East).

	2009/10							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	3155	3080	5120	9901	4873	5648	15238	58336
CDU/CSU_share _{<i>i,t</i>}	.46	.067	.46	.068	.46	.068	.42	.035
N	280		166		168		107	
	2013/14							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible_voters _{<i>i,t</i>}	3155	3080	5120	9901	4929	5653	15195	58061
CDU/CSU_share _{<i>i,t</i>}	.54	.068	.52	.075	.45	.063	.44	.042
N	280		166		167		108	

Notes: On 1.1.2014, there were 2,056 LAU-2 municipalities in Bayern and 432 in Sachsen. Due to the constant restructuring of municipalities, we do not have voting data for all municipalities. We have (state election) voting data for 2,055 (99.9%) municipalities in Bayern in 2013 (and 429 (99.3%) in Sachsen in 2013). These municipalities are distributed across 95 (13) NUTS-3 districts in Bayern (Sachsen). We only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality into our analysis. This results in 446 municipalities in Bayern and 275 in Sachsen for 2013/14 (and for 2008/09) distributed across 19 (9) NUTS-3 districts in Bayern (in Sachsen).

Comparing these two state elections reveals, see Table E.5, that the vote share of the incumbent parties was 3.8 percentage points higher in the flooded municipalities in Sachsen as compared to Bayern and the previous state elections. Again, the effect is largely driven by a higher vote share for the incumbent in the East but there is a marginally significant negative effect for Bayern. Overall, however, these accompanying results on the state elections confirm the findings for the federal elections.

Table E.5: Difference-in-Difference estimates of incumbent vote share on flood for state elections in Bayern and Sachsen.

	CDU/CSU_share _{<i>i,t</i>}		
	Bayern (West)	Sachsen (East)	All
	(1)	(2)	(3)
Flooded _{<i>i</i>} × Post_flood _{<i>t</i>}	-0.015 ⁺ (0.009)	0.022** (0.009)	-0.015 ⁺ (0.009)
Flooded _{<i>i</i>} × Post_flood _{<i>t</i>} × East _{<i>i</i>}			0.038** (0.012)
<i>Controls:</i>			
Flooded _{<i>i</i>}	0.002 (0.006)	-0.033** (0.006)	0.002 (0.006)
Post_flood _{<i>t</i>}	0.076** (0.005)	-0.010 (0.007)	0.076** (0.005)
Flooded _{<i>i</i>} × East _{<i>i</i>}			-0.036** (0.009)
Post_flood _{<i>t</i>} × East _{<i>i</i>}			-0.086** (0.009)
District Fixed Effects	Yes	Yes	Yes
Adj. <i>R</i> ²	0.34	0.12	0.35
F	93.4	10.7	52.1
N	892	550	1442

Notes: Across columns, the dependent variable is the incumbent (CDU in Sachsen and CSU in Bayern) vote share in LAU-2 municipality *i* at state election in *t*. We set *t* = 1, for the state election in Bayern on 28.9.2008 and the one in Sachsen on 27.9.2009. We set *t* = 2, for the state election in Bayern on 15.9.2013 and the one in Sachsen on 31.8.2014. Again across columns, we only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality. We include (but do not show) a constant in all regressions. Robust (Huber-White) *SE* in parentheses; ⁺ *p* < 0.1, * *p* < 0.05, ** *p* < 0.01

E.4. Statistical tests for synthetic control method

We follow a procedure proposed by Cavallo et al. (2013). The underlying idea is to construct a distribution of placebo-treatment effects as a counterfactual to which the distribution of treatment effects can be compared. To this end, one takes a non-treated municipality, assumes that it was treated, and compares the outcome for that municipality with its synthetic control of un-treated units. Replicating the procedure for n non-treated municipalities yields a distribution consisting of n placebo effects. Comparing the distributions of the treatment effects for which the means are shown in Panels (a) and (b) of Figure 2 against the distributions of placebo effects for East and West Germany, respectively, shows statistically significant effects for both cases. For constructing a distribution of placebo effects against which we can compare the treatment effects in East Germany (Panel (c)), we put every flooded municipality in West Germany in the donor pool and construct a distribution of placebo treatment effects. Averaging over all treatment effects we have an increase in the vote share when comparing treated East with flooded West German municipalities of 2.79 percentage points. This is the difference shown in Panel (c) of Figure 2. Comparing the two distributions with a Wilcoxon-rank test yields an also statistically significant effect between the treated municipalities in the East and the flooded control municipalities in the West.

F. Mechanism

F.1. Party identification

We analyze data on party identification provided by the German Socio-Economic Panel (SOEP, 2015). Households are asked whether they identify with a particular party. We compare a voter's party identification in 2009 with her answer in 2013. We calculate the share of voters who reported identifying with a particular party in 2009 and still do so in the year of the following federal election in 2013. In Table F.1, we report the shares for all the parties, comparing East and West Germany. There does not appear to be a pattern that supports an interpretation of our findings along the lines of differing party identifications.

Table F.1: Party identification in East and West Germany.

	Share of respondents who identified with same party in 2013 as in 2009	
	East	West
CDU	52.9%	47.6%
SPD	42.0%	41.5%
Bündnis 90/Grüne	32.2%	50.0%
FDP	20.8%	19.0%
Linke	44.8%	20.6%
All other	23.6%	19.7%

Notes: Shown are the shares of respondents who identified themselves in 2013 with the same party as in 2009, respectively. Data source: SOEP (2015).

F.2. Alternative measures for democratic experience

We may use invalid votes as an alternative proxy for democratic experience. Table F.2 contains the corresponding descriptive statistics.

Table F.2: Descriptive statistics for (time-constant) alternative measures for democratic experience for federal elections.

	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Avg_pre-flood_invalid_share _i	.011	.0043	.01	.0044	.022	.0071	.02	.0055
Wght_avg_pre-flood_invalid_share _i	.01	.0054	.0095	.0046	.02	.0072	.019	.0057
N	660		252		855		347	

Notes: On 1.1.2014, there were 11,136 municipalities in Germany. Due to the constant restructuring of municipalities, we do not have voting data for all municipalities. See Notes of Tables D.1 and D.2 for Details. We only include municipalities which are located in a NUTS-3 district with at least one flooded LAU-2 municipality into our analysis. In order to calculate Avg_pre-flood_invalid_share_i and Wght_avg_pre-flood_invalid_share_i, we need information on invalid votes for at least the election in 2005 (but ideally for 2005, 2002, 1998 and 1994). We can therefore come up with both measures for 2,114 municipalities for 2013 and for 2,070 municipalities for 2009. In sum, there are 4,184 observations for the analysis of the federal elections in 2009 and 2013 of Columns (2) and (3) in Table 2.

In another analysis, we check whether differences in political knowledge between East and West German voters still exist. The Short-term Campaign Panel (Roßteutscher et al., 2018), which is based on a survey of voters mainly conducted in

2017, allows us to do so. According to Table F.3, there is still a substantial difference in the share of correct answers of almost five percentage points between East and West Germany. Taking into account voters' place of birth does not change the main result. As we slice through the sample by birth decade, we see large differences in the share of correct answers for the older cohorts. For the younger cohorts that received a substantial part or all of their schooling in the reunified Germany, the difference shrinks or even turns signs. These results are robust to analyzing other questions on political knowledge such as on the meaning of the 5% threshold in parliamentary elections (not shown here). In sum, we believe that these results on an east-west divide in political knowledge speak very much in favor of our interpretation of the main findings.

Table F.3: Political knowledge in West and East Germany.

Share of respondents with correct answer on importance of first and second vote		
	Live in West	Live East
All	48.3%	42.7%
	Born and live in West	Born and live in East
All	48.4%	41.4%
<i>By birth decade:</i>		
Before 1950	44.1%	34.8%
In 1950s	48.9%	39.4%
In 1960s	52.2%	39.3%
In 1970s	48.9%	42.4%
In 1980s	46.1%	44.0%
In 1990s	45.3%	47.1%

Notes: We show the share of correct answers to question kpX_110 from the German Longitudinal Election Study (GLES) 2017 (see Roßteutscher et al. (2018)): "What about federal elections, which of the two votes is pivotal for the allocation of seats in the federal parliament?" – "The first vote." – "The second vote." [Correct] – "Both are equally important." – "I don't know." There are 14,766 valid answers to this question. 5811 from people born in the West and 1171 born in the East answered correctly. When focusing on the ones who were born and live in the West (East), we have 5342 (1033) correct answers: 437 (80) of them are born before 1950, 1031 (171) in the 1950s, 1417 (194) in the 1960s, 995 (170) in the 1970s, 830 (242) in the 1980s, and 632 (176) in the 1990s. Since Berlin was divided before the Reunification of Germany and questions on the origin and the place of living of people in the GLES do not distinguish between East and West Berlin, we neglect people from Berlin.

F.3. Individual-level evidence

This analysis draws on data from the German Longitudinal Election Study (Rattinger et al., 2016). Table F.4 includes the relevant descriptive statistics. The coding of the variable on *No political knowledge_j* is based on the question *What about federal elections, which of the two votes is pivotal for the allocation of seats in the federal parliament? – The first vote. – The second vote. [Correct] – Both are equally important. – I don't know.* 53% of the respondents give the wrong answer. The relatively high share of individuals born in East Germany reflects the fact that more electoral districts were affected by the flood in East Germany compared to West Germany. The share of individuals voting for the CDU is 41%. In order to reach a sufficient number of observations for the analysis, we code votes for the CDU and the other parties using answers on actual votes and also (for 2009 only) vote intentions.

Table F.4: Descriptive statistics for individual level estimates.

	Mean	SD
CDU _{<i>j,t</i>}	.41	.49
Born_East _{<i>j(k)</i>}	.76	.43
No political knowledge _{<i>j</i>}	.53	.5
Flooded _{<i>j(k)</i>}	.64	.48
Age _{<i>j,t</i>}	58	15
School degree level _{<i>j,t</i>}	3.1	1
Male _{<i>j</i>}	.48	.5
ln (Average household income _{<i>j</i>})	7.5	.56
N	323	

Notes: CDU_{*j,t*} is individual *j*'s voting decision for CDU in 2013 or 2009, i.e., it is one if individual *j* voted for the incumbent parties in *t*, otherwise zero. No political knowledge_{*j*} is one if individual *j*'s answer to the question, which of the two votes in federal elections is pivotal for the allocation of seats in the federal parliament is not correct, otherwise zero. Flooded_{*j(k)*} is one if individual *j* lives in one of the 33 flooded electoral districts *k* in Germany. The control group consists of the individuals *j* who live in one of the 24 electoral districts, which share the border with a flooded electoral district in Germany. In total, there are 299 electoral districts in Germany.