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

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We explore whether retrospective voting is related to voters' democratic experience. To this end, we compare the voting behavior in West Germany to the voting behavior in the formerly non-democratic East Germany after a disaster relief program addressing a flood in 2013. Our analysis reveals a 0.9 to 2.5 percentage points increase in the vote share for the incumbent party in the flooded municipalities in the East compared to the West. Analyzing an earlier flood, variation of democratic experience within East Germany, and a panel survey provides further evidence that less democratically experienced voters are easier prey to pre-election policies.

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

JEL-codes: D72, D78, H84, R10;

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This is a revised version. Link to the current version

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.

In this paper, we address the question of whether voters with more or less democratic experience reward a policymaker's performance differently. We exploit two natural experiments.

First, we make use of 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 with nation-wide media coverage. In relation to our analysis, this relief program had particularly appealing features. 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 after the flood in a country with uniform electoral rules after re-unification in 1990.

In particular, our 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).

Eliciting evidence on the effects of democratic experience on governments and voters' behavior is inherently difficult. Cross-country studies face the problem of heterogeneous institutional settings that are compared, see Alt & Rose (2009), and single country studies typically can only draw on variation of democratic experience where democratic experience may co-evolve with a country's institutional setting over time. We go beyond the previous literature and analyze variation in democratic experience on retrospective voting within a country.

Our set-up also enables us to rule out alternative explanations of the effect of democratic experience under plausible assumptions. As we are comparing voters who have different levels of democratic experience within the same country, transparency of policy making (Alt & Lassen, 2006a,b) and media coverage of policy providing reliable information (Shi & Svensson, 2006; Repetto, 2018) appear to be unlikely drivers. The uniformity of the federal program and the broad nation-wide media coverage of the disaster at the time very much speak in favor of that. As, furthermore, the political system does not vary within Germany, we are also able to rule out the level of democracy (Gonzalez, 2002) as opposed to experience with democracy as yet another source of distinct voter behavior to policy.

Our main analysis draws on high-resolution flood data from 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.

We find that the incumbent party launching the disaster relief program received a 0.9 to 2.5 percentage points larger vote share in the flooded municipalities in East Germany as compared to West Germany in the federal elections following the flood.

Our estimates are robust to a range of sensitivity tests. First, we include municipality fixed effects to control for time-invariant confounding drivers such as differences in living conditions or age structure between municipalities. Second, Land (state) times election year fixed effects, for instance, account for changing state governments which might react differently to a federal policy of a government of the same or opposing party. Third, we change the underlying sample in various ways and extend the analysis to previous election years and state elections. Fourth, 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 are aware that democratic experience may be only one cause of the empirical regularity. Other differences between East and West German voters may exist that lead to the observed voting patterns. 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. One may also throw in that the intensity of damage might have been higher in East Germany resulting in more votes in the

East. Furthermore, the incumbent's vote share in past elections was lower in East Germany, which may have made it easier to gain additional votes in the East with the disaster relief program. Our analysis rules out each of these alternative channels.

Various additional pieces of evidence support our interpretation of the empirical regularity. One concern may be that East and West Germans may not be fully comparable. We rule out this concern by studying variation in democratic experience within East Germany. In 1972, the Federal Republic of Germany (FRG) and the German Democratic Republic (GDR) signed a travel agreement, which allowed citizens from West Germany to visit relatives, friends, and tourist places in the GDR close to the boarder. One consequence of this so-called "Kleiner Grenzverkehr" was that citizens in the East living close to the boarder were more exposed to a democratic political system. When we restrict our analysis to East Germany, we find that municipalities more remote to the former border during the Cold War and thus less exposed to a democratic political system vote more in favor of the incumbent party. In addition, we explore the potential role of democratic experience by re-analyzing a previous flood. We find the same distinct voting pattern between East and West Germany for the flood of 2002. 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 expect if retrospective voting is indeed a function of democratic experience because democratic experience in the East should have been lower in 2002 than in 2013. Finally, we use political knowledge as an indicator for democratic experience. We can show that voters who were longer socialized in the GDR exhibit lower levels of political knowledge. In addition, individual level information reveals that voters with less political knowledge have higher odds of voting for the incumbent after being affected by the flood.

We believe that our investigations justify an interpretation of the empirical finding along the lines of Brender & Drazen (2005). Voters with less democratic experience were apparently 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. Less exposure of the East German voters to pre-electoral manipulations in the past made them more responsive to the disaster relief policy.

Our empirical results have more general policy implications. They suggest that retrospective voting may not suffice to steer policymakers' actions in the interest of voters in democratic transition. It could be that less discretion in policymaking, and a more rule-based approach for spending public money, is particularly advantageous in younger democracies.

2. Literature review on voter responsiveness

Empirical work on the accountability of policymakers considers the relationship between retrospective voting and policy. Part of this work is related to the incentives voters impose on policymakers, other contributions look into the voter behavior to policy decisions, and yet another part considers both aspects together (see, for surveys, Ashworth, 2012; Healy & Malhotra, 2013).

Political budget cycles .– We begin with outlining the empirical work on retrospective voting exploring incentives that voters impose on policymakers for pre-electoral policies. Various contributions to the political budget cycle literature found that, while these cycles are not confined to new democracies, they are stronger in countries with less democratic experience (Brender & Drazen, 2005; Streb et al., 2009; Klomp & De Haan, 2011). Moreover, there is evidence for single countries transitioning into democracies that political budget cycles vary with their democratic experience (Block et al., 2003; Akhmedov & Zhuravskaya, 2004; Barberia et al., 2011). The origins of the effect of democratic experience on voter behavior have been attributed to the transparency of policy making (Alt & Lassen, 2006a,b), media coverage providing reliable information (Shi & Svensson, 2006; Repetto, 2018), or voters' past exposure to pre-electoral fiscal manipulations (Brender & Drazen, 2005) which all may be less marked in younger democracies. We conjecture, that, much like there are political budget cycles related to the democratic experience of countries, governments providing disaster relief may be rewarded differently by (in)experienced voters. Thus our connection to this strand of the literature is that we focus on the responsiveness of voters to pre-electoral disaster relief.

Voter responsiveness after natural disasters .– In particular, we are interested in the responsiveness of voters to a disaster relief program, and whether their responsiveness is a function of democratic experience. Earlier work on voter behavior following policy decisions started in the economic domain 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 reverse causality when elections are near, with policymakers diverting resources to boost the economy (see, e.g., Nordhaus, 1975; Hibbs, 1977; Alesina, 1988; Rogoff, 1990).

In order to obviate these issues, scholars have turned to exploiting natural disasters as exogenous events, and have studied voters' responsiveness to disaster relief programs. 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 election of the mayor who was up for a vote. In the following, voter responsiveness in terms of turnout and support for incumbent policymakers was analyzed for various other natural disasters and policies. Achen & Bartels (2004) looked into the effect of droughts and floods, finding that voters punish the incumbent party in national elections for those disasters. 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. 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. 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.

Mechanisms of voter responsiveness .– Another strand of literature tries to not only detect the responsiveness of voters to policies but also to trace potential mechanisms underlying voter behavior. Again, related to disaster relief programs, a series of papers has shown that voters' attribution matters for voting outcomes, i.e. whether they hold the incumbent responsible for natural disaster preparedness or the launching of disaster relief programs (Arceneaux & Stein, 2006; Healy & Malhotra, 2009; Healy et al., 2010; Gasper & Reeves, 2011). Exploiting a quasi-natural experiment, Ferraz & Finan (2008) elicit the role of information for voters' responsiveness. They show that when voters were informed about corrupt mayors (in Brazil) that these voters threw them out of office more likely. In a field experiment conducted in Mexico, Chong et al. (2011) find that information about corrupt behavior of incumbents decreased electoral support for them. Aker et al. (2017) explore voter behavior in Mozambique after a randomized intervention providing information by SMS and the distribution of free newspapers. They find that civic education increased voting for the incumbent. This evidence shows that providing information to voters can enhance political selection. In an experimental study on start-up grants in Uganda, Blattman et al. (2018) find that receivers of a transfer were more likely to vote for the opposition party if compared to the control group. This finding is interpreted as a wealth effect that possibly frees voters from transactional voting and lets them vote their conscience. Avdeenko (2018) explores to which extent negative life cir-

cumstances experienced under the rule of the communist party (SED) in the GDR affected East Germans' voting behavior after reunification. Linking negative experiences by historical, institutional, and contextual features with the inner-German border region, she shows a quantitatively relevant and significant negative effect on the successor party of the SED. It appears that voters who where more exposed to negative events under communist ruling party are holding the successor party accountable even for a considerable time after reunification.

In our contribution, we explore voters' responsiveness to a disaster relief program, and provide evidence for a channel that has so far not been looked into, i.e. whether their responsiveness is a function of democratic experience.

There are a few contributions that examine democratic learning by looking at voter behavior over time in Eastern Europe after the fall of the Iron Curtain. They show 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.

In contrast, we exploit two natural experiments. In our study, variation on democratic experience enters via the re-unification of Germany that was formerly split into a democratic West and a non-democratic East. Furthermore, a major flood in 2013 followed by a federal disaster relief program is used as the exogenous driver to test for voters' responsiveness.

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 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.

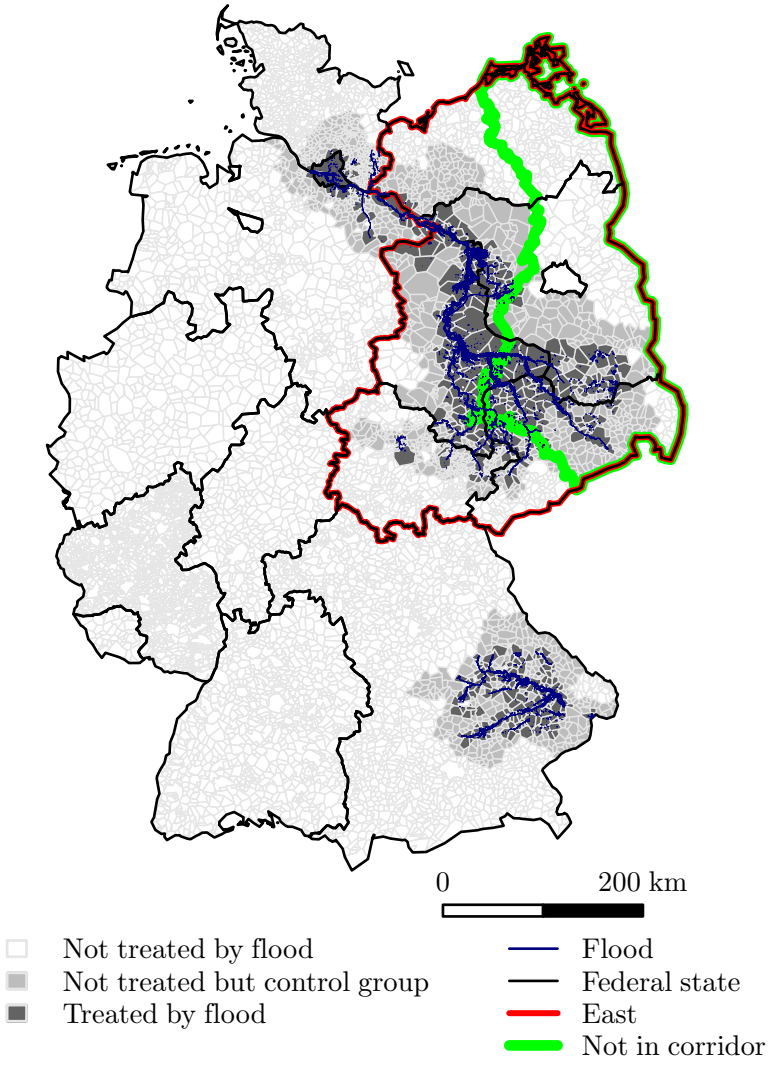

Figure 1: Flooded municipalities, June 2013.

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).

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), Redding & Sturm (2008), Rainer & Siedler (2009), Heineck & Süssmuth (2013), Friehe & Mechtel (2014), or Freier et al. (2016). 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. 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. All data sources are listed in Table C.1 in the Appendix.

The unit of analysis are municipalities. On 1.1.2014, there were 11,136 municipalities in Germany.[2] 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).[3] Angela Merkel was both chancellor and party leader of the CDU. Accordingly, we show the vote shares for the CDU and the coalition government.

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

[2] Due to the constant restructuring of municipalities, we do not have voting data for all municipalities. We are able to draw on 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. Also note that Germany is a federal state with 16 states (NUTS-1: Nomenclature des unités territoriales statistiques) and more than 400 districts (NUTS-3).

[3] CDU 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 because 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 flood and the government transfers after the flood on the vote share in flooded and non-flooded municipalities in East and West Germany we specify the following model:

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

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_i is a municipality fixed effect, $e_{j(i)}$ is a district fixed effect for all municipalities i in district j, $e_{Land \cdot t}$ is a fixed effect of an interaction term of the state (Land) with an indicator variable for the election year t, and $\epsilon_{i,t}$ an error term.

The municipal and district fixed effects should help us to take account of time-invariant differences between the regions which may have an effect on voting decisions. For example, there are large differences in living conditions between municipalities. Moreover, we observe differences in age structure, in particular between rural and urban areas, which may also have an effect on the voting behavior. With the inclusion of the municipal and district fixed effects we are differencing out these potentially confounding drivers. Inclusion of a fixed effect for the interaction of the Land (state) with the election year takes, for instance, into consideration state governments which might react differently to a federal policy of a government of the same or opposing party.

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. For the estimates in column (3), (4), and (5) we merge the data for East and West Germany. The model presented in column (5) follows the most encompassing specification given by Eq. (1). Here, municipality fixed effects and fixed effects for the interaction of the state (Land) and election years are included. 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.[4]

For West Germany, the vote share for the flooded municipalities is 0.7 percentage points lower in 2013 compared to the previous election (column (1)), and for East Germany the vote share for the flooded municipalities is 1.3 percentage points higher in 2013 compared to the previous election, see columns (2). The treatment effect in the combined data is two percentage points (column (3)), and 1.8 percentage points when we include municipality fixed effects in column (4). It drops to 0.9 percentage points with the inclusion of the fixed effects for the state election year interaction (column (5)).

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). In the most encompassing specification, the regression explains 94% of the variation in the data (Adj. R^2).

Common trend assumption and robustness .– 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 in Appendix E. Overall, we are 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.

By including district and municipality fixed effects, we have already addressed differing economic conditions across districts and municipalities as potential confounders. In Appendix F, we also show that our empirical result is robust to a variety of further tests that include various changes to the underlying sample, an extension of the analysis to previous election years and state elections, and an estimation with the synthetic control method to control for time-variant confounding drivers.

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

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)	(5)
Treatment effects:					
$Flooded_i \times Post\ flood_t$	-0.007*	0.013**	-0.007*	-0.007*	-0.004
	(0.003)	(0.005)	(0.003)	(0.003)	(0.003)
$Flooded_i \times Post\ flood_t \times East_i$			0.020**	0.018**	0.009*
			(0.005)	(0.005)	(0.004)
Controls:					
$Flooded_i$	-0.004	-0.020**	-0.004		
	(0.004)	(0.006)	(0.004)		
$Post\ flood_t$	0.083**	0.094**	0.083**	0.083**	
	(0.003)	(0.004)	(0.003)	(0.003)	
$Flooded_i \times East_i$			-0.016*		
			(0.007)		
$Post\ flood_t \times East_i$			0.011*	0.010*	
			(0.005)	(0.005)	
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	No	No	No	Yes	Yes
Land×t Fixed Effects	No	No	No	No	Yes
Adj. R^2	0.72	0.61	0.72	0.93	0.94
F	383.7	251.4	322.7	462.8	2.3
N	1878	2402	4280	4222	4220

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. The inclusion of additional Fixed Effects reduces the number of observations because singleton observations are dropped. We include (but do not show) a constant in all regressions. Clustered *SE* (on district level) in parentheses; $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

4.2. Mechanisms

We are confident to robustly identify differences in the voting behavior following the disaster relief program between East and West Germany. We interpret these findings as evidence for retrospective voting being a function of democratic experience. Other, competing explanations, however, may exist, and we turn to the ones which appear most obvious to us in the following Section 4.2.1.

In a final step, we return to our most favored explanation and present direct evidence in support of the democratic experience channel (Section 4.2.2). We analyze the voting behavior splitting East Germany into two separate regions, one of which being closer to the border of the democratic West. Moreover, we present individual-level evidence on voters with different degrees of democratic experience living in flooded and not flooded electoral districts, and voted in the 2009 and 2013 elections.

4.2.1. Alternative channels

Turnout .– In order to learn more about whether the results presented so far are potentially connected to 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 not different from the flooded areas in 2013 compared to 2009. While East German voters living in flooded areas more likely voted for the incumbent party, they were not more likely to show up at the ballots.[5]

East-West differences in economic living conditions, extent of flood, and past vote share of incumbent .– Rather than a lack of democratic experience in East Germany, other systematic differences between East Germany and West Germany may drive the voting behavior. In an extension of our regression model (see Eq. 1), we take account of differing economic living conditions, lower past vote shares of the incumbent party, and the extent of the flood.

First, while East Germany certainly differs in terms of having 41 years less of democratic experience, 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. Systematic differences in economic conditions between East Germany and West Germany may drive the voting pattern we detect – rather than a lack of democratic experience in East Germany. To investigate this conjecture, we use, as a measure of economic performance, per capita income tax payments at the municipal level (i), which are available from the Statistische Bundesamt for selected years. This data is commonly employed to calculate fiscal transfers within the horizontal tax revenue equalization scheme in Germany, and thus appears appropriate also in our context.

Second, the vote share of the incumbent party was lower in East Germany before the flood. Consequently, the ruling party may have had an easier task to convince additional voters with a generous flood relief programm in East Germany.

Third, one may argue that the intensity of the flood may have been higher in East Germany than in West Germany resulting in a larger voter response in the East. To address the concern we use data provided by the Gesamtverband der Deutschen Versicherungswirtschaft (2015) stemming from insurance companies on the number of insurance cases that arose due to the flooding. As this piece of information is only available for the district level (j) but not for the municipal level (i), we assign to

[5] In 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: Estimates for turnout [column (1)] as dependent variable, interactions with control variables [column (2)] and alternative specification with eastern municipalities closer to the border as an indicator for democratic experience within the east [column (3)].

	$Turnout_{i,t}$	$CDU\ share_{i,t}$	
	(1)	(2)	(3)
Treatment effects:			
$Flooded_i \times Post\ flood_t$	-0.003^{+}	-0.048*	-0.001
	(0.002)	(0.019)	(0.003)
$Flooded_i \times Post\ flood_t \times East_i$	0.006	0.025**	
	(0.004)	(0.009)	
$Post\ flood_t \times Not\ in\ 100km\text{-}border\ corridor_i$			-0.012*
			(0.005)
$Flooded_i \times Post\ flood_t \times Not\ in\ 100km\text{-}border\ corridor_i$			0.015**
			(0.005)
Controls:			
$CDU\ share_{i,t-2}$		-0.058*	
		(0.026)	
$Flooded_i \times CDU\ share_{i,t-2}$		0.092*	
		(0.037)	
$Post\ flood_t \times CDU\ share_{i,t-2}$		-0.027	
		(0.027)	
$Flooded_i \times Post\ flood_t \times CDU\ share_{i,t-2}$		0.080**	
		(0.027)	
$\ln\left(Income\ tax\ payments\ per\ capita_{i,t}\right)$		0.014	
		(0.009)	
$Flooded_i \times \ln\left(Income\ tax\ payments\ per\ capita_{i,t}\right)$		-0.011	
		(0.012)	
$Post\ flood_t \times \ln\left(Income\ tax\ payments\ per\ capita_{i,t}\right)$		-0.002	
		(0.005)	
$Flooded_i \times Post\ flood_t \times \ln\left(Income\ tax\ payments\ per\ capita_{i,t}\right)$		0.007	
		(0.008)	
$Post\ flood_t \times Intensity\ Dummy_j$		0.007	
		(0.005)	
$Flooded_i \times Post\ flood_t \times Intensity\ Dummy_j$		0.005	
		(0.006)	
District Fixed Effects	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes
Land$\times t$ Fixed Effects	Yes	Yes	Yes
Adj. R^2	0.88	0.94	0.91
F	2.0	2.7	3.5
N	4220	3860	2346

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. In column (2), we control for the lagged incumbent vote share $\left(CDU\ share_{i,t-2}\right)$, Income tax payments per capita $\left(\ln\left(Income\ tax\ payments\ per\ capita_{i,t}\right)\right)$, flood intensity $\left(Intensity\ Dummy_j\right)$ and the interactions of the three variables with $Flooded_i$, $Post\ flood_t$ and $Flooded_i \times Post\ flood_t$. The intensity dummy is one if the damage frequency lies over 2.9%. Income tax payments per capita are available for 2010 and 2013. We use the data for 2010 for 2009. In column (3), we have a look at within differences in the East. Since 1972, regions in the east, within a 100km-corridor area to the former inner German border could be visited by West Germans, who lived nearby the inner German border, more easily. $Not\ in\ 100km\text{-}border\ corridor_i$ indicates is one if municipality i is not within 100km distance to the former inner Germany border and zero if the municipality is within 100km distance. We show the corridor area in Figure 1. In column (2), we loose some observations since we do not have information on the lagged imcumbend vote share 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. Clustered *SE* (on district level) in parentheses; $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

districts an indicator variable (Intensity Dummy$_j$) 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.

To address systematic differences between East Germany and West Germany, we augment our regression model with all three measures and interact them with the flood variables. Controlling for the differences in income, past vote share, and intensity of damage, increases the treatment effect to 2.5 percentage points, see column (2) in Table 2. In sum, it appears that none of the potentially systematic differences which we include in an extended regression model challenges our interpretation of the main finding.

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 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 G.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).

4.2.2. More on democratic experience

Intensity of democratic experience within East Germany .– Up to this point we exploited varying degrees of democratic experience between East Germany and West Germany. One may argue that East and West Germans are not fully comparable. We can rule out this concern by focusing on differences in democratic experience within East Germany. One may conjecture that people who live closer to the former West German border were exposed more to the political system of West Germany. In 1972, the FRG and the GDR made a travel agreement ("Kleiner Grenzverkehr") allowing individuals from West Germany 30 daily visits per year of relatives, friends, and tourist places in specified areas (Die Zeit, 1973). These regions were roughly within a corridor of 100 km to the border (c.f. Laudenbach et al., 2018). Thus one my surmise that, in particular citizens being in contact with their West German relatives were receiving information on the democratic political system of West Germany. In this case, we should observe a stronger effect of the flood in municipalities further away from the former West German border.

To test the effect of distance from the border, we split East Germany into two parts: Municipalities within a 100 km corridor to the former West German border, and municipalities further to the East, the ones right to the green line in Figure 1. Column (3) in Table 2 reports the results of the regression in which the indicator variable corresponding to the corridor is interacted with the flood variables. We find that voters in flooded municipalities being less exposed to a democratic political system, i.e. outside of the corridor, voted more likely for the incumbent party. The estimated effect is 1.5 percentage points and significant at the 1% level.

The results of Column (3) in Table 2 remain unaffected from controlling for differences in income, past vote share, and intensity of damage (as in Column (2), Table 2). The estimates are available upon request.

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 (1) of Table 3. 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. The point estimate of the parameter on the interaction term of the treatment variable with the indicator variable on East German electoral districts is positive and significant. 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 because democratic experience in the East should have been lower in 2002 than in 2013. Moreover, as we restrict the analysis to East German districts and apply the 100 km corridor introduced in the previous section there is a positive effect on the vote share of the incumbent for the districts which are more remote to the former border to West Germany, see column (2) in Table 3. Again, this speaks for our interpretation of the main finding as an effect originating from the lack of democratic experience.

Political knowledge .– The lack of democratic experience which we assume for citizens in East Germany could be reflected in voters' political knowledge. If so, one should see systematic differences between East Germany and West Germany in relation to 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 German and West German voters after reunification. Maier (2000) provided evidence on political knowledge of East German 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

Table 3: Diff-in-Diff-in-Diff estimates of incumbent vote share on 2002 flood for federal elections.

Dependent variable	$\Delta SPD\ share_{k,t}$	
Size	All k	All k in East
	(1)	(2)
Treatment effects:		
$Flooded2002_k \times East_k$	0.070**	
	(0.021)	
$Flooded2002_k \times Not\ in\ 100km\text{-}border\ corridor_k$		0.033*
		(0.014)
Controls:		
$Flooded2002_k$	-0.049*	-0.004
	(0.020)	(0.007)
$East_k$	0.073**	
	(0.005)	
$Not\ in\ 100km\text{-}border\ corridor_k$		-0.025$^+$
		(0.013)
District Fixed Effects	–	–
Municipality Fixed Effects	–	–
Land$\times t$ Fixed Effects	–	–
Bechtel/Hainmueller Controls	Yes	Yes
Adj. R^2	0.61	0.33
F	65.2	3.4
N	299	66

Notes: Across columns, we replicate Bechtel & Hainmueller's (2011) results on a flood that happened in 2002 and do not only consider areas flooded by river Elbe but all flooded areas in Germany (according to Bundesministerium der Verteidigung (2013, p. 13)). The dependent variable is – as in Bechtel & Hainmueller's column (4) of Table 1 – the incumbent (in 2002, SPD) vote share in electoral district k (instead of municipality i in the main analyses) at federal election in t (in first differences). Flooded2002_k is one if Bechtel & Hainmueller's Flooded dummy is one or if the electoral district (wkr) is 228, 229, 230, 232, or 234. We focus on the year 2002 (and take into account 1998 for the first difference) and refer to the electoral districts level with 299 of these across Germany. In column (2), we only include electoral districts in the east of Germany. We include (but do not show) a constant in all regressions. Table D.3 in Appendix D contains the descriptive statistics. As Bechtel & Hainmueller (2011), we report robust (Huber-White) SE in parentheses; $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

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 4. According to this source of information, there is still a substantial difference in the share of incorrect 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.

We can expand this analysis and slice through the sample by birth decade. If democratic experience matters, we would expect different answers in relation to political knowledge for older cohorts but not for younger cohorts. As younger cohorts in the East received a substantial part or all of their schooling in the reunified Germany, there should be no difference in democratic experience for them compared to equally old citizens in the West. Quite interestingly, we see large differences in

the share of incorrect answers for the older cohorts. For the younger cohorts 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 the east-west divide in political knowledge speaks very much in favor of our interpretation of the main findings.

Table 4: Political knowledge in West and East Germany.

	Share of respondents with incorrect answer on importance of first and second vote	
	Live in West	Live in East
All	57.7%	63.3%
	Born and live in West	Born and live in East
All	57.7%	64.6%
By birth decade:		
Before 1950	58.9%	69.2%
In 1950s	56.0%	65.4%
In 1960s	54.0%	66.8%
In 1970s	58.5%	65.4%
In 1980s	60.7%	61.4%
In 1990s	61.5%	61.3%

Notes: We show the share of incorrect 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. *[Incorrect]*" – "The second vote. *[Correct]*" – "Both are equally important. *[Incorrect]*" – "I don't know. *[Incorrect]*" There are 16,948 valid answers to this question. 13,753 from people who live in the West and 3,195 who live in the East answered correctly. When focusing on the ones who were born and live in the West (East), we have 12,642 (2,914) incorrect answers: 1,064 (260) of them are born before 1950, 2344 (494) in the 1950s, 3,081 (585) in the 1960s, 2,398 (492) in the 1970s, 2,112 (628) in the 1980s, and 1,6423 (455) 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.

Individual-level analysis .– Further elaborating the role of democratic experience, we take information on the location of birth and political knowledge of voters from yet another panel data set 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. We define the dependent variable to be individual l's voting decision for the CDU in 2013 or 2009, i.e., it is one if individual l voted for CDU in t, otherwise it is zero. Whether an individual is living in a flooded electoral district is captured by the variable $Flooded_{l(k)}$. It is one if individual l lives in one of the flooded electoral districts k in Germany, and otherwise zero. Furthermore, we define a dummy variable *No political knowledge*$_l$

which is one if individual l'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 G.2 we give further details on this micro-level data and also present descriptive statistics.

Table 5 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 Germany and East Germany, respectively. Voters born in the West affected by the flood do not vote differently from not affected voters. Voters born in the East behave differently. 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.

As we turn to the alternative measure for democratic experience, i.e. political knowledge, columns (3) to (6) 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, see column (3). One concern may be that no political knowledge is a proxy for being born in the East. However, as we restrict the sample to voters born in the East of Germany, the estimate increases to 15.6, see column (5). This result confirms our previous finding that no political knowledge increases the odds of voting for the incumbent. These results appear to be robust to the inclusion of a large set of individual controls and electoral district fixed effects. Finally, not shown estimates confirm that we can also control for time-invariant voter-specific characteristics (by adding voter fixed effects). Again, the results remain unaffected from this procedure.

Overall, this micro-level evidence is also 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 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 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

Table 5: Individual level conditional logit estimates of voting for incumbent parties on lack of political knowledge.

	$CDU_{l,t}$					
Born:	West	East	East & West		East	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effects:						
$Flooded_{l(k)} \times Post\ flood_t$	1.67	3.42**	1.11	1.37	0.91	0.83
	(1.10)	(1.61)	(0.52)	(0.73)	(0.51)	(0.48)
$No\ political\ knowledge_l \times Flooded_{l(k)} \times Post\ flood_t$			6.93**	9.21**	15.64**	27.49**
			(3.60)	(6.18)	(11.44)	(26.68)
Controls:						
$Post\ flood_t$	0.68	0.51	0.97	0.88	1.05	0.93
	(0.30)	(0.21)	(0.35)	(0.37)	(0.44)	(0.39)
$No\ political\ knowledge_l$			1.67	1.54	1.22	0.93
			(1.00)	(1.23)	(0.97)	(1.15)
$No\ political\ knowledge_l \times Flooded_{l(k)}$			0.39	0.29	0.39	0.33
			(0.29)	(0.28)	(0.35)	(0.44)
$No\ political\ knowledge_l \times Post\ flood_t$			0.29**	0.28**	0.18**	0.16*
			(0.11)	(0.14)	(0.11)	(0.13)
Further controls	No	No	No	Yes	No	Yes
Electoral district Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	65	242	323	295	240	226

Notes: We show odds ratios (exponentiated coefficients). Across columns, the dependent variable is individual l's voting decision for CDU in 2013 or 2009, i.e., it is one if individual l voted for the incumbent party in t, otherwise zero. No political knowledge$_l$ is one if individual l'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$_{l(k)}$ is one if individual l lives in one of the 33 flooded electoral districts k. The control group consists of the individuals l living in one of the 24 electoral districts which share the border with a flooded electoral district. In columns (4) and (6), 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 for electoral districts. Across columns, there are mainly two observations for every of the remaining respondents, one for 2009 and one for 2013 (e.g., 226 observations for 143 respondents in column (6)). Note, that the conditional logit estimator drops all observations with no variation in the dependent variable (here CDU$_{l,t}$, for every electoral district or for each individual). Clustered *SE* (on electoral districts) in parentheses; $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

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 a large disaster relief program unfolded differently, possibly being a function of voters' democratic experience.

Our main results suggest that the incumbent party received a 0.9 to 2.5 percentage points larger vote share in the flooded municipalities in East Germany compared to the previous federal elections. This empirical regularity is robust to a large set of sensitivity tests.

We do not claim that democratic experience is necessarily the channel for our main finding. However, we may state that numerous results of our investigations are consistent with an interpretation that less democratically experienced voters responded more positively to the relief program and voted for the incumbent. Evidence supporting this interpretation comes from analyzing the 2002 flood in Germany, and from splitting East Germany into more or less democratically experienced municipalities depending on the distance to the former border. An individual-level analysis is

also confirmative: voters with less democratic experience had higher odds of voting for the incumbent after having been affected by the flood.

More generally, besides having a different economic system, the other major difference between East and West Germany was the form of government. 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).

We can also rule out potentially competing explanations. There is neither evidence on systematically differing party identifications nor on systematically differing reciprocal preferences between East Germany and West Germany. Our estimation approach using fixed effects for different jurisdictional levels also makes it unlikely that regional variation in socio-economic factors such as the age structure or economic living conditions were driving the voting behavior. Moreover, systematic regional differences between flooded East German municipalities and flooded West German municipalities in relation to per capita income, flood intensity, or past vote shares of the incumbent party are not driving our main empirical result.

Thus our most favored interpretation for the empirical finding is that democratic experience is indeed linked to retrospective voting. Our set-up which relies on varying degrees of voters' democratic experience within a country also allows us to narrow down some explanations on why democratic experience may matter. It is unlikely that transparent policy making or better media coverage that leads to more informed voters are driving the results. The uniform program of the federal government and nationwide high public awareness of the program appear to speak against these channels. This leaves us with the interpretation of Brender & Drazen (2005) who conjecture that democratic experience matters for fiscal budget cycles because voters with less experience have less exposure to pre-electoral policies. 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.

As a policy implication one may conclude from these findings that retrospective voting does not suffice as a disciplining device for policymakers in democratic transition. Perhaps, it could be advantageous to voters in general to constrain policymakers with a more rule-based approach for public policies that curbs discretionary spending of public money in younger democracies.

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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. Our analysis in Section 4.2.1 takes account of these observations.

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, 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.

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

East	
Berlin-Brandenburg	1600
Königreich Sachsen	1270
Provinz Sachsen	1161
Thüringen	1087
Ave. East	*1279.5*

West	
Westfalen	1045
Rheinprovinz	1171
Hessen	1150
Hannover	1156
Schleswig-Holstein	1192
Hamburg	1746
Bayern	1149
Baden	1117
Württemberg	1348
Ave. West	*1219.3*

Notes: Data source: Petzina (2011).

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 between East and West Germany after the fall of the Iron Curtain may confound our data we consulted the German Socio-Economic Panel (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 % East	West	Total obs.
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, income tax payments and voting outcomes.

Data	Description	Source	Availability
Regional data:			
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)
Income tax payments per capita	Yearly income tax payments and taxpayers on municipality (LAU-2) level for 2010 and 2013	Statistisches Bundesamt, `http://www.regionalstatistik.de/`	Table 73111-01-01-5 downloaded on 20.12.2019
Municipality maps	Administrative areas 1:250,000 as of 31.12.2013	Bundesamt für Kartographie und Geodäse, `http://www.geodatenzentrum.de`	Downloaded on 04.11.2016
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
Survey data:			
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.
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.
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 2 for details on administrative levels.

D. Descriptive statistics

Baseline analysis .– 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.2 contains the descriptive statistics for the federal elections in 1994, 1998, 2002, and 2005.

The 2002 flood revisited .– In Table D.3, we show the descriptive statistics on the electoral district k level for 2002. We take the main part of the data from Bechtel & Hainmueller (2011), who analyze a flood that took place in Germany in 2002.

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$_{i,t}$	2479	3952	10106	78541	2731	4837	10929	37752
Turnout$_{i,t}$	.67	.058	.68	.058	.6	.068	.56	.054
CDU share$_{i,t}$	.41	.088	.43	.096	.34	.073	.32	.055
CDU&FDP share$_{i,t}$	.56	.082	.58	.095	.45	.089	.43	.075
SPD share$_{i,t}$	.2	.067	.18	.069	.18	.057	.18	.054
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$_{i,t}$	2518	4022	10277	80121	2786	4754	10743	37106
Turnout$_{i,t}$	.67	.057	.67	.06	.64	.079	.62	.073
CDU share$_{i,t}$	.49	.084	.51	.097	.43	.063	.42	.049
CDU&FDP share$_{i,t}$	.54	.082	.56	.093	.46	.066	.45	.051
SPD share$_{i,t}$	.23	.075	.2	.077	.16	.052	.17	.043
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, 1994, 2002, and 2005.

	1994							
	West				East			
	Non-flooded in 2013		Flooded in 2013		Non-flooded in 2013		Flooded in 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible voters$_{i,t}$	2309	3778	10270	80768	2679	4998	20998	1.3e+05
Turnout$_{i,t}$	.76	.056	.74	.049	.72	.068	.67	.055
CDU share$_{i,t}$	.51	.088	.53	.09	.44	.1	.41	.071
N	600		238		726		287	
	1998							
	West				East			
	Non-flooded in 2013		Flooded in 2013		Non-flooded in 2013		Flooded in 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible voters$_{i,t}$	2003	3121	9856	81500	2646	4873	10025	22781
Turnout$_{i,t}$	.79	.052	.78	.047	.79	.059	.75	.052
CDU share$_{i,t}$	.45	.098	.49	.1	.31	.075	.28	.046
N	594		223		738		282	
	2002							
	West				East			
	Non-flooded in 2013		Flooded in 2013		Non-flooded in 2013		Flooded in 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible voters$_{i,t}$	2082	3215	9770	80788	2919	5044	9033	20196
Turnout$_{i,t}$	.8	.052	.81	.045	.71	.064	.68	.058
CDU share$_{i,t}$	.54	.16	.61	.16	.33	.075	.31	.058
N	596		230		841		341	
	2005							
	West				East			
	Non-flooded in 2013		Flooded in 2013		Non-flooded in 2013		Flooded in 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible voters$_{i,t}$	2105	3237	9835	81202	2924	5007	8927	20206
Turnout$_{i,t}$	.77	.055	.78	.048	.72	.07	.7	.06
CDU share$_{i,t}$	.47	.11	.51	.12	.29	.067	.27	.046
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.

Table D.3: Descriptive statistics for federal elections on electoral district level in 2002.

	2002							
	West				East			
	Non-flooded 2002		Flooded 2002		Non-flooded 2002		Flooded 2002	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
$\Delta SPD\ share_{k,t}$	-.037	.025	-.091	.052	.028	.039	.047	.014
$SPD\ share_{k,t-1}$	.42	.081	.36	.054	.38	.044	.32	.054
Not in 100km-border corridor$_k$	.	.	.	.	.56	.5	.48	.51
N	226		7		105		27	

Notes: Bechtel & Hainmueller (2011) study the electoral district level k. There were 299 electoral districts in Germany in 2002 and 1998.

E. Common trend assumption

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.[6] 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. In the election years 1998, 2002, 2005, and 2009 the CDU did not have a significantly different vote share from the then flooded municipalities in the East compared to 1994. Overall, we are 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.

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

	$CDU\ share_{i,t}$	$Turnout_{i,t}$
	All	All
	(1)	(2)
Common trend of treated west vs. east:		
$Flooded_i \times 1998 \times East_i$	-0.002	0.009*
	(0.005)	(0.005)
$Flooded_i \times 2002 \times East_i$	0.007	0.008
	(0.009)	(0.006)
$Flooded_i \times 2005 \times East_i$	0.012	0.004
	(0.010)	(0.006)
$Flooded_i \times 2009 \times East_i$	0.013	0.002
	(0.008)	(0.005)
Controls:		
$Flooded_i \times 1998$	0.007*	-0.000
	(0.003)	(0.002)
$Flooded_i \times 2002$	0.006	0.002
	(0.007)	(0.004)
$Flooded_i \times 2005$	0.002	0.006
	(0.007)	(0.004)
$Flooded_i \times 2009$	0.001	0.005
	(0.005)	(0.004)
District Fixed Effects	Yes	Yes
Municipality Fixed Effects	Yes	Yes
Land$\times t$ Fixed Effects	Yes	Yes
Adj. R^2	0.93	0.87
F	2.5	1.7
N	9786	9786

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. Clustered SE (on district level) in parentheses; $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

[6] Table D.2 contains descriptive statistics for the federal elections in the years 1994, 1998, 2002, and 2005.

F. Robustness

By including district and municipality fixed effects, we have already addressed differing economic conditions across districts and municipalities as potential confounders. As we show in the following, our empirical result is robust to a variety of further tests that include various changes to the underlying sample and an alternative methodological approach, i.e. estimation with the synthetic control method.

F.1. Robustness of baseline estimates

Table F.1 summarizes the results of the regression models that we estimate for the robustness checks.

Table F.1: Robustness of Diff-in-Diff-in-Diff estimates of incumbent vote share on flood for federal elections.

	Conley SE	Size i	Control group		More t	Placebo	Other dep. var.
			II	III			
Dependent variable	$CDU\ share_{i,t}$					$SPD\ share_{i,t}$	$CDU\&FDP\ share_{i,t}$
Size	All i	Large i	All i	All i	All i	All i	All i
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment effects:							
$Flooded_i \times Post\ flood_t$	-0.004*	-0.004	-0.004	-0.009	-0.006$^+$	-0.003	-0.000
	(0.002)	(0.003)	(0.003)	(0.006)	(0.003)	(0.003)	(0.004)
$Flooded_i \times Post\ flood_t \times East_i$	0.009**	0.011*	0.008*	0.018*	0.016*	0.002	0.011*
	(0.003)	(0.005)	(0.004)	(0.007)	(0.006)	(0.003)	(0.005)
Controls:							
$Post\ flood_t$	0.114						
	(75.641)						
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Land$\times t$ Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.00	0.97	0.94	0.96	0.93	0.92	0.94
F		2.5	2.0	2.8	3.5	0.7	3.7
N	4280	2066	3958	1866	11985	4220	4220

Notes: In columns (1)-(5), the dependent variable is the incumbent (CDU) vote share in municipality i at federal election in t. In column (6), we show a placebo regression. The dependent variable is the SPD vote share in LAU-2 municipality i at federal election in t. In column (7), we calculate the votes for CDU & FDP (which was in coalition with CDU during the flood) as the incumbent vote share. In columns (1)-(2), (4)-(7), we only include LAU-2 municipalities which are located in a NUTS-3 district with at least one flooded municipality. In column (3), we change the control group and consider all municipalities that do intersect with a 20km buffer of the flooded area. In column (4), we change the control group and consider all municipalities that do not intersect with a 20km buffer of the flooded area. In column (5), we include the years 1994, 1998, 2002, 2005, 2009 and 2013 [in contrast to columns (1)-(3) and (6-7) for which we include the years 2009 and 2013]. We include (but do not show) a constant in all regressions. In column (1), we show *SE* in parentheses that are adjusted for spatialdependence as modelled in Conley (1999) and implemented by Fetzer (2020) (with spatial autocorrelation assumed to linearly decrease in distance upto a cutoff of 100 km). District distances are computed from district centroids. For all remaining columns, Clustered *SE* (on district level) in parentheses; $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Conley standard errors .– In our main specification, we cluster standard errors at the district level j. To test whether the confidence intervals are robust, we apply the procedure by Conley (1999) which takes into account spatial dependence in standard

errors up to 100 km-cutoff-distance. Column (1) of Table F.1 shows standard errors as small as in the baseline specification.

Further in relation to the confidence intervals, we generate a placebo distribution (see Cameron & Miller, 2015) for the estimate on the triple interaction effect ($\text{Flooded}_i \cdot \text{Post flood}_t \cdot \text{East}_i$) by randomly shuffling the assignment of municipalities to East and West Germany. We find that only in 8 out of 1000 cases the placebo estimates are larger than our estimate. Thus it appears to be very unlikely to get an estimate as ours by chance.

Size of municipalities .– 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 F.1 again. Column (2) presents the results for municipalities with a relatively high number of eligible voters. The treatment effect is 1.1 percentage points.

Control group II .– We can move away from restricting our control group following the definition of districts. In column (3) of Table F.1, we include all municipalities, which are within a distance of 20 kilometers to the treated flooded areas. Our results remain unaffected from this procedure.

Control group III .– So far, we included all municipalities which were located in a district with at least one flooded municipality. The rationale 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 column (4) of Table F.1, we challenge this approach and change the underlying sample. We conduct a change in the definition of the control group to address potential spill-overs from treated municipalities to municipalities close to the flooded ones. Such spill-overs may arise if one thinks that family members or friends of 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 (4) take account of this by excluding municipalities as controls that are within a distance of 20 kilometers to the flooded areas. We still include the remaining municipalities, which were located in a district with at least one flooded municipality but within a distance of more than 20 kilometers to the flooded areas. Our results are robust to this change in the specification.

Including earlier elections .– So far, we have only compared the voting behavior at the federal elections in 2013 to the federal elections in 2009. In column (5) of Ta-

ble F.1 we extend the data set and include information on election years from 1994 onwards. This may unduly 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.

Placebo .– Column (6) of Table F.1 reports on the results of a placebo experiment in which we insinuate that the Social Democratic Party (SPD) was the incumbent party launching the disaster relief program. In line with our previous results the treatment effect disappears.

Including coalition party .– 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 explore 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) of Table F.1 confirm the previous findings.

F.2. 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. 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 test whether our empirical results can be replicated as we turn to those elections. We provide descriptive statistics on those two elections, see Table F.2, and a map (Figure F.1) of the flooded and non-flooded municipalities in the two states.

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.

Comparing these two state elections reveals (c.f. Table F.3) 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, and stays at this level as fixed effects for the municipal level and state election year interactions are

Table F.2: 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$_{i,t}$	3155	3080	5120	9901	4920	5687	15368	58597
CDU/CSU share$_{i,t}$	.46	.067	.46	.068	.46	.069	.42	.036
N	280		166		165		106	
	2013/14							
	West				East			
	Non-flooded 2013		Flooded 2013		Non-flooded 2013		Flooded 2013	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Eligible voters$_{i,t}$	3155	3080	5120	9901	4920	5687	15368	58597
CDU/CSU share$_{i,t}$	.54	.068	.52	.075	.45	.063	.44	.042
N	280		166		165		106	

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).

included. These accompanying results on the state elections confirm the findings for the federal elections.

F.3. Synthetic control method

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

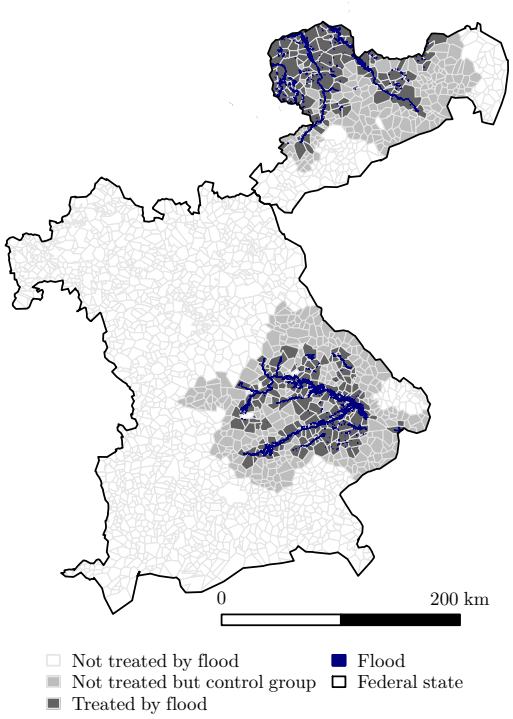


Figure F.1: Flooded municipalities in Bayern (West) and Sachsen (East), June 2013.

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.

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).[7] 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 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.[8]

[7] Doudchenko & 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.

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

Table F.3: Difference-in-Difference estimates of incumbent vote share on flood for state elections in Bayern (West) and Sachsen (East).

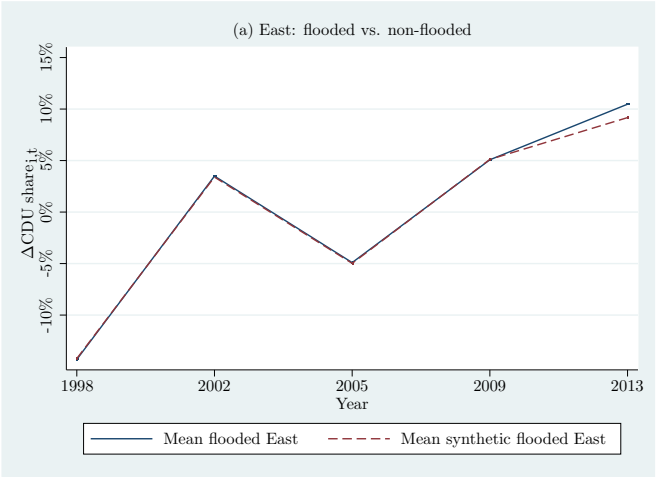
	CDU/CSU share$_{i,t}$		
	All		
	(1)	(2)	(3)
Treatment effects:			
$Flooded_i \times Post\ flood_t$	-0.015	-0.015	-0.015
	(0.009)	(0.009)	(0.009)
$Flooded_i \times Post\ flood_t \times East_i$	0.038*	0.037*	0.037*
	(0.015)	(0.015)	(0.015)
Controls:			
$Flooded_i$	0.002		
	(0.008)		
$Post\ flood_t$	0.076**	0.076**	
	(0.008)	(0.008)	
$Flooded_i \times East_i$	-0.036*		
	(0.014)		
$Post\ flood_t \times East_i$	-0.086**	-0.086**	
	(0.013)	(0.013)	
District Fixed Effects	Yes	Yes	Yes
Municipality Fixed Effects	No	Yes	Yes
Land$\times t$ Fixed Effects	No	No	Yes
Adj. R^2	0.35	0.84	0.84
F	19.2	27.0	2.9
N	1442	1434	1434

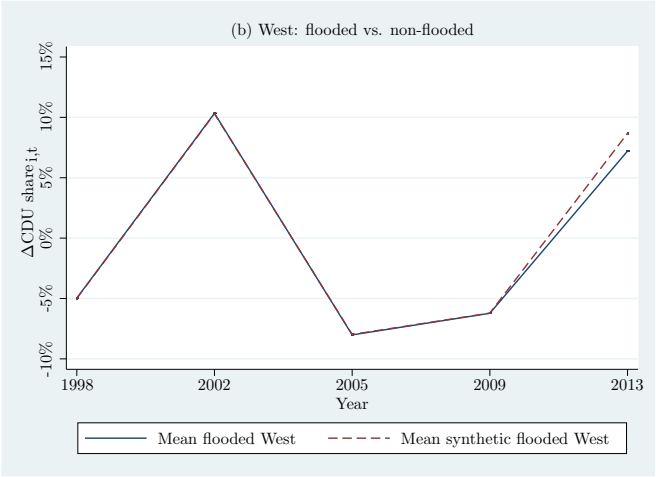
Notes: Across columns, the dependent variable is the incumbent (CDU in Sachsen and CSU in Bayern) vote share in LAU-2 municipality i at state election in t. We set $t = 1$, for the state election in Bayern on 28.9.2008 and the one in Sachsen on 27.9.2009. We set $t = 2$, for the state election in Bayern 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. Clustered *SE* (on district level) in parentheses; $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Figure F.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 can show that voting patterns also differ in a statistical sense.

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 F.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 F.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.





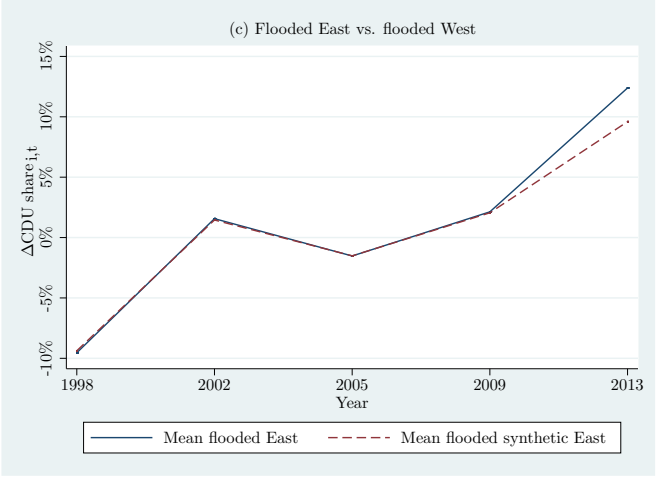


Figure F.2: Synthetic control group figures.

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.

G. Mechanism

G.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 G.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 G.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).

G.2. Individual-level evidence

This analysis draws on data from the German Longitudinal Election Study (Rattinger et al., 2016). Table G.2 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 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 G.2: Descriptive statistics for individual level estimates.

	Mean	SD
$CDU_{l,t}$	.41	.49
Born East$_{l(k)}$	.76	.43
No political knowledge$_l$	.53	.5
Flooded$_{l(k)}$	.64	.48
Controls:		
$Age_{l,t}$	58	15
$\ln\left(Average\ household\ income_{l,t}\right)$	7.5	.56
$Male_l$	.48	.5
$Religion_{l,t}$	2.8	1.4
School degree level$_{l,t}$	3.1	1
N	323	

Notes: $\text{CDU}_{l,t}$ is individual l's voting decision for CDU in 2013 or 2009, i.e., it is one if individual l voted for the incumbent parties in t, otherwise zero. No political knowledge_l is one if individual l'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. $\text{Flooded}_{l(k)}$ is one if individual l lives in one of the 33 flooded electoral districts k in Germany. The control group consists of the individuals l 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.