Natural Disasters and Political Participation: Evidence from the 2002 and 2013 Floods in Germany*

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Abstract

How do natural disasters affect electoral participation? The existing social science literature offers contradicting predictions. On the one hand a considerable literature in sociology and psychology suggest that traumatic events can inspire pro-social behavior, which might increase turnout. On the other hand, political science has long held that economic resources are crucial ingredients for civic engagement. Consequentially, natural disasters should reduce electoral participation. We show how these distinct views can be jointly analysed within the Riker and Ordeshook model of voting. This paper then reports results on the impact of the 2002 and 2013 floods in Germany on turnout in federal and state elections in Saxonia and Bavaria, conducted few weeks after the floods. Analyzing community level turnout data, and drawing on a difference-in-differences framework, we find that flood exposure has a consistent negative effect on turnout, even in the wake of a quick and effective government response. This indicates that the increase in the costs of voting outweighed any increase in political engagement in our case. This stands in contrast to results from developing contexts, where flood management was convincingly linked to electoral participation, suggesting that the impact of natural disasters might differ substantially between developed and developing countries. We additionally find no support for mechanisms proposed in other contexts, e.g. the salience of the race or partisanship, pointing to the need for additional research.

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INTRODUCTION

What is the impact of natural disasters on politics? With an expected increase in the frequency of weather extremes in the future due to climate change,[1] addressing this question is important. Previous research on this topic has focused predominately on the impact of natural disasters on incumbent vote shares. While one part of the literature considers any effect of natural disasters on candidates' vote shares as blind retrospection[2], an other part sees natural disasters as an opportunity for voters to observe their government's reaction, learn about its type, and condition their vote accordingly.[3] Comparatively few studies have considered explicitly how natural disasters affect political participation.[4]

This paper investigates the impact of two natural disasters in Germany, two floods termed 'one hundred year events', on voter turnout. The existing social science literature offers contradictory predictions for such events. On the one hand, research in sociology and psychology suggests that traumatic events, such as natural disasters, can inspire pro-social behaviour[5] and lead to grass-roots creation of self-help organizations[6], both of which have been shown to be positively correlated with political engagement and thereby turnout.[7] On the other hand, political science has long argued that economic resources are critical ingredients for civic engagement.[8] Political participation is modelled as a regular consumption decision, requiring time and money. Consequentially, the destruction of economic resources and potential dislocation due to natural disasters should therefore dampen turnout in affected communities.

We show how these distinct views can be jointly analysed within the Riker and Ordeshook[9] model of voting. We then investigate the theoretical puzzle empirically in the context of the 2002 Elbe flood on federal election turnout in Saxony and the 2013 Danube floods on participation in federal and state elections in Bavaria. Both floods occurred close to elections and played an important role in shaping the dynamics of the electoral campaigns.[10] The two German states Saxonia and Bavaria are interesting contexts, since they differ in at least two important aspects: experience with democracy and average economic well-being. Paying close attention to causal identification in our research design, we find a consistent, though moderate-sized, negative impact of flood exposure on turnout, especially among the most severely flooded communities (effects range between 0.35 and 0.65 percentage points). Our results suggest that, at least in the German context, the increase in costs of voting outweighed any increase in political engagement in the aftermath of the floods.

LITERATURE REVIEW

Among the studies that have looked at how natural disaster affect turnout, empirical results are mixed. Combining geo-coded flood data, official election returns from national and provincial assembly elections, a large household survey from Punjab province, and a large district-level representative survey to study the impact and mechanism of the 2010-11 Pakistani floods on political engagement, Fair et al.[11] find that Pakistanis in highly flood-affected areas became significantly more politically engaged than those less exposed. They argue that when the government and civil society response effectively blunts a disaster's economic impacts, then political engagement may increase as citizens learn the value of government capacity. Consistent with the proposed learning mechanism, they find evidence that the increase in turnout was higher in areas with lower *ex ante* flood risk.

Sinclair, Hall, and Alvarez,[12] on the other hand, argue that Hurricane Katrina had an average negative impact on turnout, based on combining flood-depth information with voting records data for mayoral elections in New Orleans. On closer inspection, however, they found that the relationship is u-shaped: while light to moderate flood exposure reduced electoral participation, more severely affected areas had an increase in turnout. Referring to two competing mechanisms, they argue that the Katrina floods potentially both increased the costs of voting due to economic hardship and at the same time increased salience among the most severely affected citizens, as flood reconstruction plans were a key issue in the mayoral race. Hence, while the costs of voting outweighed the potential benefits among less affected voters, the potential benefits seemed to have been significantly higher than voting costs for the most severely affected.

Again others, find no significant impact of natural disasters on turnout. Bodet, Thomas, and Tessier,[13] for example, study the 2013 floods in Calgary and found that turnout did not change in flood-affected compared to non-affected polling subdivisions. Similarly, Remmer[14] notes that in her comparative study on incumbent re-election prospects and exogenous shocks in Caribbean countries 1970-2009 natural disasters do not seem to affect electoral turnout.

Finally, in closely related research, Chen[15] looks beyond natural disasters per se to the effects of post-disaster government relief on turnout. Using individual level data on the Federal Emergency Management Agency's (FEMA) hurricane disaster aid awards in 2004 Florida, linked with voter turnout records from the 2002 (pre-hurricane) gubernatorial elections and 2004 (post-hurricane) presidential elections, Chen[16] finds that government delivery of distributive aid increases turnout among incumbent party supporters, but decreases opposition party voter turnout. While not looking at the impact of natural disaster exposure on turnout directly, Chen[17] reports in additional tests that disaster affectedness (conditional on not receiving relief aid before the election) is related negatively (though substantially small and statistically insignificant) to turnout.

The diversity of empirical results suggests that competing mechanisms link the occurrence of natural disasters and political participation. In the following, we first draw on the Riker and Ordeshook[18] framework to contextualize the empirical literature so far, highlighting how natural disaster affect turnout through changing the benefit derived from the election results, the costs of voting and the sense of civic duty. We then provide empirical evidence from the Elbe floods in Saxonia 2002 and the 2013 Danube floods in Bavaria, two economically, socially, and politically very different contexts within Germany, suggesting that the floods had a consistent negative effect on turnout. This paper thereby empirically contributes to the evidence base necessary for future research to systematically investigate the conditioning factors affecting the impact of natural disasters on political participation.

NATURAL DISASTERS AND THE CALCULUS OF VOTING

We use the Riker and Ordeshook[19] model to analyse the turnout effect of natural disasters. Extending the rational choice framework,[20] the Riker-Ordeshook model conceptualizes the turnout decision as a cost-benefit calculus of the form

$$R = pB + D - C,$$

where R is the expected benefit from turning out, which depends on the benefit derived from the election result (B), multiplied by the probability of casting a decisive vote (p) (i.e., either creating or breaking a tie), the benefit derived engaging in the process (i.e., an inherent taste for voting) or fulfilling a civic duty (D), and finally the costs of participation (C) (i.e., the time and resources necessary to make an informed choice and cast a ballot). Hence, the greater the benefit derived from the election result or engaging in the process, the greater the probability of casting a deciding vote, and the lower the costs of voting, the greater the individual turnout propensity should be. Theoretically, natural disasters may affect B, C, and D. Below we discuss their potential impact on each term separately.

The benefits derived from an election result (B) depends largely on the difference in ideological positions of the candidates or parties. Natural disasters might affect issue salience and thereby highlight key differences for voters between contenders. For example, natural disasters could highlight differences in recovery and disaster prevention policy, which will make the outcome of the election more consequential for natural disaster victims, which should increase their turnout. However, because the probability of casting a decisive ballot is virtually zero, any differential changes in B between victims and non-victims should have no measurable effect on turnout.[21]

Because the probability of casting a decisive ballot is virtually zero and therefore any benefit derived from the election result is extremely low, even small increases in costs (C) can lead to considerable drops in turnout.[22] Hence, as natural disasters increase the individual costs associated with learning about candidates and parties and most importantly voting per se, we should expect aggregate turnout in areas affected by natural disasters to be lower, especially in those places that were most severely hit.[23]

Natural disasters might also affect the D term through inspiring pro-social behavior and the formation of social capital. An extensive psychology literature argues that in the aftermath of natural disasters pro-social attitudes and behaviour tend to dominate.[24] Rodriguez, Trainor, and Quarantelli,[25] for example, conducted extensive field research in the aftermath of Hurricane Katrina in New Orleans in 2005 and found that instances of pro-social behaviour greatly outnumbered instances of anti-social behaviour. If voting is driven in part by concerns about the welfare of other citizens[26], such a change in attitudes would be expected to increase turnout. Natural disasters also appear to be positively correlated with some indicators of social capital due to the spontaneous formation of self-help organizations in their aftermath.[27] These civic associations help train citizens in basic functions of self-governance as well as reveal the positive outputs from collective action, both features that, according to the social capital literature, should be positively correlated with political engagement.[28].

Hence, whether a natural disaster increases or decreases turnout will therefore depend on the relative size of the increase in C and D. If the increased sense of duty outweighs the increased cost of voting, then turnout will increase; if the cost of voting increase more than the sense of duty, then turnout will decrease, and if the change is roughly equal in size, then there should be no differential turnout effect. In the remainder of this article, we investigate this theoretical puzzle in the German context for two different floods in two distinct states.

THE 2002 AND 2013 FLOODS IN GERMANY

We analyse the impact of floods on turnout in two different contexts: for the 2002 floods we look at Saxony, a relatively poor area of Germany hit particularly hard and at the time still in a catch-up process after the economic decline in post-Cold-War Eastern Germany; with regard to the 2013 floods we look at the state of Bavaria, a relatively wealthy German state experiencing the most devastating disaster in the state's recent history. Together we believe these two German states and floods cover a broad range of contexts to analyse the effects of natural disasters on turnout in a developed democratic context. Both floods are with respect to timing unique as they hit close to general elections and were of a similar overall magnitude: the Elbe floods in August 2002 (August 06, 2002 to September 12, 2002) were followed by federal elections on September 22, 2002; the Danube floods occurred in June 2013 (May 18, 2013 to July 4, 2013) with state elections in Bavaria on September 15, 2013 and federal elections following on September 22, 2013.[29]

Both times concentrated heavy rainfall caused severe flooding in Central Europe, breaking multiple records such as an all-time high along the Elbe in the city of Dresden (7.40m in 2002 above normal) and along the Danube in the city of Passau (7.72m in 2013 above normal – a 500 year high). The record rainfalls in the upstream catchment areas of Elbe and Danube resulted in simultaneous flood peaks of Elbe, Danube and their tributaries with record water levels and subsequent breakages of dikes even further downstream in the lowlands of Northern Germany (Elbe) and Austria and Hungary (Danube). Both the 2002 and 2013 floods caused casualties (21 in 2002 and 8 in 2013 in Germany alone) and tens of thousands of people needed to be evacuated (30,000 in 2002 and 85,000 in 2013 in Germany alone). Both floods were classified as, depending on the location, 20 year to above 500 year flood events.[30] The floods caused billions of Euros worth of damage (approximately 9 billion in 2002 and approximately 8 billion in 2013) in Germany, but considerable heterogeneity exists between the damage suffered by individual districts both within and between the events. The federal state of Saxony for example experienced damage amounting to about 6.1 billion in 2002 (8 of 13 districts severely affected). In the federal state of Bavaria damage amounted to 1.3 billion in 2013 (19 of 96 districts affected).

Figures 1 and 2 show the maximal flood extents for Saxony in 2002 and Bavaria in 2013 and the communities affected. Blue is the maximal flood extent and highlighted in grey are the communities affected by the floods.

INSERT FIGURES 1 AND 2 HERE

The response of the federal and state governments was swift and massive. Over 200,000 man-service-days of federal forces (270,000 in 2002 and 215,000 in 2013) were employed to stabilize dikes and aid in evacuations[31] and the federal government and the state governments agreed in both cases to each bear 50% of the relief and reconstruction costs.[32] A considerable part of this aid was given very quickly, without much red tape, and directly to all affected households that applied, following federal and state level regulations.[33]

Germany being a federal state, the political management of such events lies jointly in the hand of federal, state and district level politicians and administrators. While financial contributions were decided at the federal and state level (excluding damage to federal property), implementation of the flood loss compensation programs is controlled by district officials. In terms of civil society, anecdotal evidence suggests that the floods sparked a large extent of grass-root mobilization among citizens who volunteered filling sand bags, offering shelter, and providing relief goods. Especially in 2013, qualitative research indicates that additional to government steered flood relief social media networks were used by citizens to self-organize help, with information flows uncoordinated by government agencies.[34]

Finally, both floods disrupted ongoing campaigns for federal and state elections. The 2002 federal election campaign was characterized by a weak incumbent, with polls indicating a clear victory for the opposition candidate, the governor of Bavaria.[35] The surprise win of the SPD with incumbent Chancellor Schroeder, coming out with a plus in PR votes of only 6,000 and a five seat majority for his SPD-Greens coalition, was consequently linked to the exogenous shock of the 2002 floods and the robust response of the then SPD government.[36] In line with this argument, the 2002 flood played a major role in the media coverage of the 2002 electoral campaigns, especially of SPD and Greens.[37] In contrast, the 2013 federal election was dominated by a strong CDU incumbent, Chancellor Merkel, who had lead the German economy through the Euro crisis.[38] Similarly, the 2013 state election in Bavaria was characterized by a strong CSU incumbent, Governor Seehofer, aiming to regain the parliamentary majority for the CSU, which has been dominating Bavarian politics for decades; campaigning was very much dominated by topics of the federal electoral campaign.[39] Crucial for our analysis here, we could not find any specific campaign issues in both Bavaria or Saxonia that correlate with flood exposure and might therefore confound our flood estimates.[40]

DATA AND EMPIRICAL RESEARCH DESIGN

We use aggregate electoral data on community level – the lowest level of analysis at which turnout changes are traceable and thus the most detailed information that is publicly available.

For our main Tables 1-3 on the 2009-2013 Bavarian federal elections, the 2008-2013 Bavarian state elections, and the 1998-2002 Saxony federal elections our outcome measure (turnout (%)) and control variables come from the statistical offices of Bavaria[41] and Saxony[42], respectively. The data section in the Online Appendix lists all additional data sources.

We select a wide range of control variables following economic voting theory. These include logged population, logged brute community income, logged brute tax income (Saxony only, as it is not available for Bavaria 2013), the proportion of elderly (i.e., age>65) and youth citizens (i.e., age<18), and the employment rate (Bavaria only, as not available for Saxony 2002). Summary statistics for these variables are reported in Appendix Table A8.

We can analyse the 2002 Elbe flood in Saxony and 2013 Danube flood in Bavaria, as there are particularly good geo-coded flood layers for these events.[43] For Saxony 2002, we obtained the flood layer from the Saxonian State Agency for Environment, Agriculture and Geology.[44] The layer represents the maximal flood extent along all rivers and is displayed in Figure 1. Roughly 54% of all communities in Saxony experienced some flooding and among flooded communities exposure ranged from 0.008% to 37.65% of a community's area, with median and mean exposure at 1.68% and 4.12%, respectively. For the Bavarian floods in 2013, we obtained a flood layer from Vista Remote Sensing in Geosciences GmbH. Vista aggregated layers from several satellite pictures that captured the flood extent at the time of the flood tide.[45] Overall, 7.9% of all Bavarian communities were affected and among those exposure ranges from 0.0001% to 55% of a community's area, with median and mean flood exposure at 0.22% and 2.10%, respectively.

We estimate effects for different flood indicators, all of which are derived from the percent of a community's area flooded. Our first indicator is based on the dichotomization of the continuous flood exposure measure, which will enable us to estimate the average treatment effect on the treated (ATT) from a comparison of affected and unaffected communities. As this average might conceal differences between severely and less severely affected communities we additionally estimate separate effects for less and more affected communities as indicated by flood extent. For this, we differentiate communities by exposure quartile, i.e. with a flooded area within the first, second, third and fourth quartile of the exposure distribution among the flood affected communities.

Cutoffs for Saxonia are at 0.72% (first quartile), 1.68% (median), and 4.25% (third quartile) of the community area flooded. In the case of Bavaria they are at 0.06% (first quartile), 0.22% (median), and 1.3% of community area flooded (third quartile). Overall, those indicators allow us to compare severely and lightly affected communities to non-flooded communities directly and assess potential non-monotonicities of flood impacts on turnout.

To estimate flood treatment effects we need estimates for the counterfactual outcome of the n_1 flooded ($F = 1$) communities absent the flood (potential outcome $Y_i^0|F = 1$ for affected communities $i = 1, ...n_1$). For this we use a difference-in-differences design, allowing us to control for any time-invariant unobserved confounders in treatment and control observations by relating treatment to changes in outcome variables. Of course, time-variant bias in treatment and control observations can only insofar be controlled for as it is observable.[46] In terms of the Rubin Causal Framework[47], our analysis relies on the central assumption that common trends in the treated ($i = 1, ...n_1$) and control observations ($i = n_1 + 1, ...n$) are present over the electoral period we observe (from $t - 1$ to t). We therefore assume that $E(Y_{i,t}^0 - Y_{i,t-1}^0|F = 1) = E(Y_{i,t}^0 - Y_{i,t-1}^0|F = 0)$.

We assess whether the central assumption of no effect in the pre-treatment population holds.[48] Our placebo estimates indicated that pre-flood trends were not perfectly parallel (see Placebo Test section and Appendix Table A1), so we pre-processed our data via entropy weighting[49] to generate a control group that perfectly matches the distribution of pre-treatment outcome and control variables. Entropy weighting is a data pre-processing technique that directly generates balanced samples given a binary treatment indicator. The aim is to find a set of weights such that the distribution of treatment group characteristics in pre-specified moments is perfectly matched by the re-weighted control group. Compared to matching entropy weighting has several advantages.[50] First, it provides a higher degree of covariate balance, in our case for the first, second, and third moments of the covariate distributions. This is a valuable property, as we no longer need to check for covariate balance, as the pre-treatment outcome and control variables in both groups show identical distributions by construction, resulting in placebo estimates of precise zeros. Second, entropy weighting retains valuable information in the preprocessed data by allowing the unit weights to vary smoothly across units, achieving balance while keeping them as close to one as possible. Finally, the method is computationally attractive since the optimization problem to find the unit weights is well behaved and globally convex. Hence, if no time-varying confounders that both affect treatment status and pre-treatment outcomes have been left out of the calculation of these weights, our comparison between treatment and control groups will produce unbiased estimates of the average treatment effect of the treated (ATT).

Our ATT is therefore equal to the difference in (entropy weighted) trends between flooded and non-flooded communities :

$$ATT = E(Y^1_{i,t} - Y^0_{i,t-1}|F = 1) - E(w_i(Y^0_{i,t} - Y^0_{i,t-1})|F = 0).$$

We estimate effects with an (entropy weighted) fixed-effects estimator including year (α_t) and unit fixed (γ_i) effects. This estimator will give us the ATT (β) we are interested in via the estimation of

$$Y_{it} = \beta F_{it} + \boldsymbol{X_{it}}\delta + \alpha_t + \gamma_i + \epsilon_{it} \quad (1)$$

A vector **X** of time-variant controls is included to take potential differences in time trends into account. For Bavaria, where a large part of the state was not treated, we restrict our control group to the south-eastern areas of the state (see highlighted area in Figure 2), as we expect the distribution of potential unobservable time-varying confounders to be better balanced between treatment and control group the geographically closer treatment and control groups are.[51]

RESULTS

This section presents our findings. We first discuss the average Elbe 2002 and Danube 2013 flood impact on turnout for the treated communities of Bavaria (federal and state elections) and Saxony (federal elections). We report an, on average, moderate negative treatment effect. Turnout in affected communities declined by approximately half a percentage point. Thereafter, we present disaggregated treatment effects to find out about heterogeneity in the treatment effect. Finally, we assess the robustness of our results, especially to violations of SUTVA - we do not find that spill-overs are likely to bias our results.

Main Results

Table 1 presents our ATT estimates under the (weighted) common trend assumption. Column 1 reports results for the 2002 Elbe flood and Saxonian federal election turnout, Column 2 for the Bavarian federal election, and Column 3 for the Bavarian state election turnout after the 2013 Danube flood.

INSERT TABLE 1 HERE

Our estimates suggest that both the 2002 and the 2013 floods have a consistently negative effect on turnout that is statistically significant at the 5% level. The negative effect ranges between 0.36 for the Bavarian federal elections, 0.52 for the Bavarian state elections and 0.65 percentage points for the Saxonian federal elections. While small in absolute term, the flood effect for Saxony amounts to 9% of the average change in turnout among communities. The flood effect for the federal elections in Bavaria amounts to 39% and the flood effect on the Bavarian state election turnout amounts to 8% of the counterfactual year effect in treatment communities. Hence, in relative terms the flood effect on changes in turnout is moderate to large.

Note that the negative turnout effect is slightly (although not statistically significantly) larger for Saxonia (Column 1), where floods and elections were temporally closer to each other. Moreover, note that the negative turnout effects are substantially larger for the state elections in Bavaria (Column 3) compared to the federal elections (Column 2). Both elections were only one week apart, with state elections preceding federal elections (September 15th and 22nd, respectively), three months after the flood. This suggests that although people in flooded communities had a lower propensity to turn out on average in both elections, their lower turnout propensity was especially pronounced in state elections. While the difference in effects is not statistically significant, this makes intuitive sense, as state elections in Germany are generally seen as 'second order' compared to federal elections and the benefits of voting are thus potentially lower.[52] An increase in voting costs should thus push more citizens below their participation threshold. A competing saliency mechanism as referenced by Sinclair, Hall, and Alvarez,[53] as flood prevention measures are upon state authorities to plan and implement, which could have resulted in a counteracting positive turnout effect, is not supported by this data.

Finally, the small absolute average change in turnout in the federal elections in Saxony is in line with Bechtel and Hainmueller's[54] conclusion that the 2002 flood increased SPD vote share primarily through persuasion rather than mobilization.

Treatment Effect Heterogeneity

To assess the heterogeneity of the flood effect, we disaggregate our binary treatment indicator into the quartiles of the distribution of 'severity' of flood affectedness. For each of these treated quartiles, we construct a perfectly balanced control group by entropy weighting and estimate the ATT.

INSERT TABLE 2 HERE

There are two main results. First, as can be seen in Column 4 across all Panels of Table 2, our results indicate that the fourth quartile, those most heavily affected, are consistently less likely to turn out. This effect is most clearly visible for the federal election results in Bavaria: the more affected a community was, the lower its participation in the election. The fourth quartile shows a highly significant negative effect of -0.66 percentage points.[55] For the federal elections in Saxony and the state elections in Bavaria, the fourth quartile shows clear negative effects (Saxonia: -0.69, significant at the 10% level; Bavaria: -0.56, significant at the 5% level), though non-linearities seem to be present.

Second and as indicated above, the average aggregate turnout response among those communities least flooded in part even surpasses the most heavily affected places. Given the prediction of the 'costs of voting'-mechanism, it is a surprise that the first quartile effect for the Saxonian federal elections (-1.08 percentage points, significant at the 5% level) and the second quartile in Bavarian state elections (-0.86 percentage points, significant at the 5% level) show such a strong negative response. Although the estimated coefficients are all not statistically different from each other, this indicates that competing mechanisms might be at work.

There are at least two potential explanations for this finding. Interpreting these results through the lens of[56], they are consistent with a 'surprise' effect. The degree of flood exposure most likely correlates with ex ante risk of being flooded: those communities more severely affected are likely to be those regularly flooded due to their geographic location. Hence, among the least affected the proportion of communities that are hit only in extraordinary floods is likely to be greater. They were least prepared and the floods therefore had the greatest impact on turnout in those communities.

Alternatively, the findings might be an artefact of our measurement, share of the community area flooded, which is a noisy indicator of how traumatic the floods were for a community. The first quantile most likely contains just as many barely affected places as communities that got lucky and just escaped a catastrophe. Satellites would capture almost no flooding in places where the river is channelled, water levels were dangerously high but no dykes broke, but large-scale evacuations might have taken place in many such communities. This certainly has the potential to translate into large political effects as well, although the flood layer would show much flooding.

Robustness

We are especially worried about spill-overs, which would violate the SUTVA assumption and bias our inference. Spill-overs could arise from media-coverage of the floods as well as personal experience or ties with affected regions. We would be especially concerned if spill-overs were positive while our main effect is negative. This would result in an overestimation of the ATT and our results would merely provide an upper bound of the true effect. While we are unable to assess the global effects of spill-overs (e.g. effects of media coverage of the flood etc.), there is good reason to expect regional heterogeneity in these spill-overs. In particular, they should be stronger the geographically closer a community is to affected rivers, which is testable. We therefore coded communities directly bordering our flood layer, but not situated along the affected rivers, separately. Table 3 reports results.

INSERT TABLE 3 HERE

In Columns 1, 4 and 7 we estimate the ATT excluding communities that neighbour flood affected places from our analysis. The estimates remain consistently negative and statistically significant. If anything, our results get slightly stronger, which implies negative spill-overs and an underestimation of the true effect. In fact, the estimates reported in Columns 3, 6 and 9 confirm this conjecture. Estimating a (placebo) effect for adjacent communities and excluding actually flooded communities from the sample, there seems to be a slightly lower turnout trend in communities neighbouring flood affected places than for communities further away. Hence, if there are any spill-overs they are likely to be negative. Finally, Columns 2, 5, and 8 report estimates based on comparing affected to adjacent non-flooded communities. While the estimates are slightly smaller and no longer statistically significant, they are consistently negative, as would be expected if small spill-overs in the direction of the main effect are present. We therefore conclude that spill-overs might be present, though we should not be worried about them econometrically, as they, if anything, lead to an underestimation of our main effect.

To further assess the robustness of our results, we conducted a series of additional checks. We summarize our findings below and report the respective tables in the Online Appendix.

Given the indication of parallel trends through the 1994-1998 placebo, we estimated Models 2 and 3 of Table 1 and Table 2 for Bavaria with unweighted fixed-effects regressions - our main interpretation is unaffected by this modelling strategy (see Table A2). Additionally, as the Bavarian flood layer relies on satellite data that did not perfectly capture flood extent in the southernmost communities of the state, we re-estimated Models 2 and 3 of Table 1 but recoded the treatment dummy to include all communities that border an affected river within districts where disaster alarm was called. Results are unchanged with this definition of treatment (see Table A3 in the Appendix). Finally, for Bavaria, estimating effects with a continuous flood indicator similarly leads to consistent negative effects around 0.3-0.4 percentage points for every 10 percentage points of community area flooded. These effects are estimated with unweighted fixed-effects regressions as entropy weights cannot be calculated for continuous treatments (see Table A4).

Given the community boundary changes in Saxony that required us calculate entropy weights on 1998 levels rather than 1994-1998 trends, we re-estimated the fixed effects regressions on trends (1998-2002) reported in Tables 1 and 2 without entropy weights. Moreover, we also ran a level regression for Saxonia in 2002 controlling for past turnout in federal elections in 1998. The results are reported in Appendix Table A6 and are qualitatively similar to the weighted results reported in the main paper.

An additional concern relates to the voting of displaced persons: If citizens vote outside their voting district (because of flood related dislocations), this mechanically leads to an increase in turnout in unaffected (control group) communities and to a decrease in treatment group communities, which might explain our negative ATT estimates. Appendix Tables A5 and A7 provide evidence that this is unlikely to be the case. If displaced persons indeed voted outside their communities, we would expect this to occur predominately in neighbouring communities, which should result in more urn voting. For Bavaria, where we were able to gather data on community level urn voting and postal voting, urn voting turnout in communities neighbouring flooded communities is lower or equal (Appendix Table A5, column 4 and 8). For Saxonia, Appendix Table A7, column 5, reports effects for an even better measure: we can assess the (cross-sectional) share of voters that voted with a 'voting card' that is required for out of district voting at the ballot box. Comparing Saxonian communities unaffected and those bordering affected communities, there is no evidence for an increase in 'out of district voting'. Overall, this leads us to conclude that flood displacement did not mechanically confound our estimates. Of course, nonetheless physical dislocation might have induced stress and/or increased voting costs to an extent that explains our treatment effect. After all, accessibility is an important determinant of turnout, which we expect to be affected by the flood.[57]

Using the data on postal and urn voting, we can learn even more about how the floods affected participation patterns, lending additional support to our main conclusions. Appendix Table A7 reports the (cross-sectional) difference in the urn and postal voting share between flood affected and unaffected Saxonian communities: urn turnout decreases (by 1.1

percentage points), while postal turnout increases (by 0.4 percentage points). The share of postal voters increases by 0.7 percentage points (Model 4). This is an indication that large amounts of Saxonian voters stayed home, due to the disaster, and that postal voting was used by many flood victims to cast a ballot. For Bavaria, due to the larger temporal distance between flood and election, we would not necessarily expect the same results on urn and postal voting. As indicated by Appendix Table A5, columns 1-3 (federal elections) and 5-7 (state elections), our estimations indicate that both urn voting and postal voting decreased in flood affected communities.

Finally, the role of disruptions of electoral preparations are a potential source of our turnout effect. Were this the case, a reduction in turnout would not be due to the disaster influencing individual citizen behaviour but due to a reduction in state capacity to conduct orderly elections. We consider this unlikely. An extensive search of national and local newspapers found no reports of public discussions of electoral irregularities due to the floods. The last disaster alarms ended both in 2002 and in 2013 before the general elections,[58] election officers had enough time preparing polling stations. Qualitative interviews with election officials support our conclusion.[59] Therefore, we deem it unlikely that the proper execution of the elections was responsible for the effects we observe. The observed negative effect is therefore likely to be behavioral rather than the result of a physical inability of flood victims to cast their ballot.

CONCLUSION

This paper analyzes the linkage between natural disasters and political participation for two large-scale floods in the two German federal states of Saxonia and Bavaria in 2002 and 2013. We find a consistent negative, albeit moderate-sized effect of flooding on electoral participation, ranging between 0.35 (federal elections in Bavaria) and 0.65 (federal elections in Saxonia) percentage points, which accounts for between 8-9 (federal elections in Saxonia and state elections in Bavaria) and 39 percent (federal election in Bavaria) of the average annual change in turnout. Estimating the flood impact for each exposure quartile separately, while it is negative for all quartiles, the effect is particularly strong in those communities affected worst across all cases and years.

The consistency of the negative effect is remarkable for three reasons. First, we are dealing with widely varying socio-economic and political contexts within Germany - Saxonia had been democratizing only for a decade before the 2002 floods and was still in an economic catch-up process; Bavaria is a politically very stable and economically well-developed region in Germany. Second, while the 2002 floods in Saxony were without precedence in the recent history of the state, the 2013 floods in Bavaria were the re-occurrence of a natural disaster termed as 'one hundred year event' within a decade. Third, the standing of the incumbent and electoral campaigns differed widely: while the 2002 campaign was dominated by a weak incumbent, Gerhard Schröder, lagging in the polls until right before the election and motivating swing-voters especially in the East not least through the flood,[60] the 2013 election saw a strong incumbent, Angela Merkel, with issues besides the flood dominating the nation-wide electoral campaigns.[61]

Our results suggest that in this context the increase in the costs of voting outweighed any increase in civic duty, especially in those communities most severely hit. This stands in stark contrast to the findings of Fair et al.[62] in a developing country, suggesting that the extent to which natural disasters affect turnout is context-specific. Future research may aim to uncover the conditional factors determining the circumstances under which the increase in the costs of voting outweigh the gains in civic duty. Having individual-level pre-post disaster panel data will be crucial in performing these analyses.

While we lack the necessary individual-level data to directly assess the underlying mechanism behind the aggregate result, our findings and context-specific factors are inconsistent with two proposed mechanisms. While Sinclair, Hall, and Alvarez[63] find an overall negative impact of flood exposure on turnout, they show a reversal of the effect among those New Orleans residents most severely affected by Hurricane Katrina. They explain this nonmonotonic effect by the increased salience of the mayoral race, which focused on disaster management and future preparedness, issues of particular importance for those residents most severely affected. In our cases we find no evidence for an issue salience effect[64]: those communities most severely flooded had consistently lower turnout rates than non-flooded communities. Moreover, if there was an effect, the most likely place to observe it would have been at the state elections level in Bavaria, where political responsibility for disaster prevention lies. Yet, for the Bavarian state elections, the negative turnout effect is even more pronounced compared to the Bavarian federal elections.

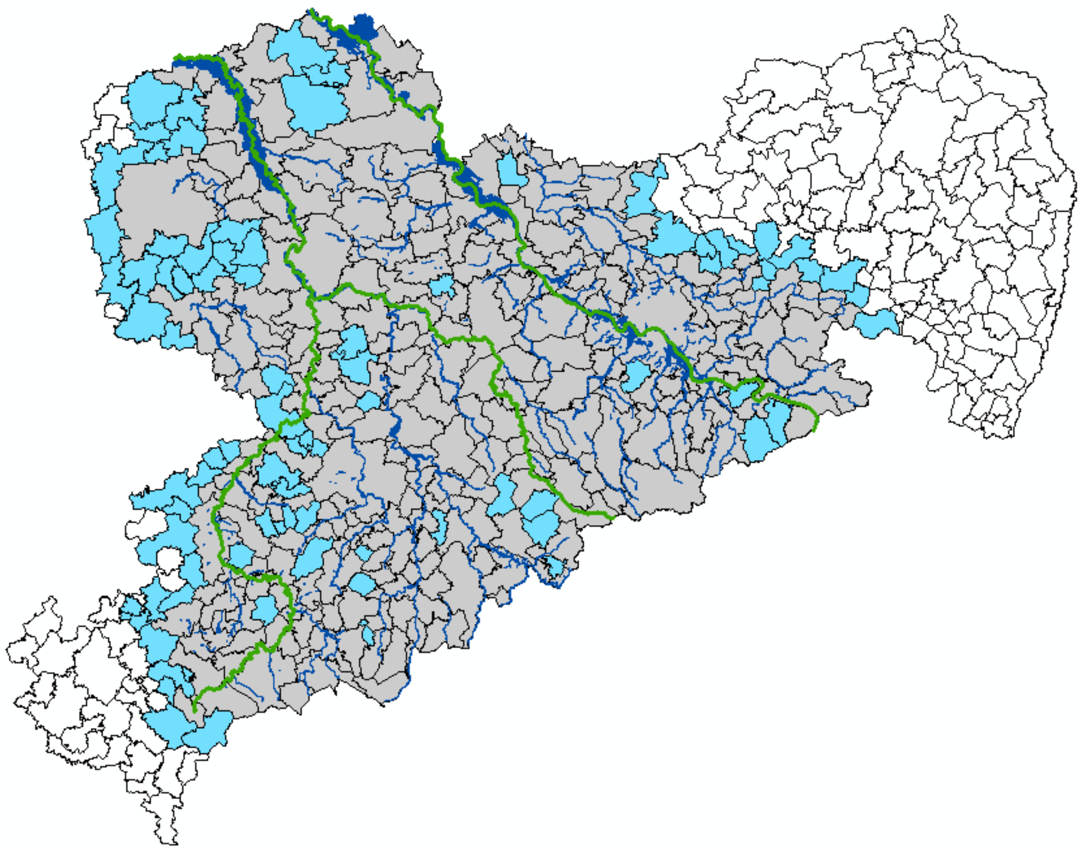
Chen[65] argues for a partisan mechanism that motivates pre-flood incumbent supporters and deters incumbent opponents from turning out. While this could be a potential explanation for the turnout effect in Saxony 2002, where the federal incumbent party received relatively low support levels in pre-flood state elections, this is unlikely to be the case in Bavaria 2013: the state and federal incumbent are of the same party family and incumbent party support is traditionally strong in the flooded areas. Moreover, the argument by Chen[66] is most likely moderated in multi-party systems, where post-electoral coalition dynamics make vote choice decisions more complicated.

With respect to policy implications, both theoretically and empirically our results highlight that it is important to take steps to reduce the costs of voting in a post-disaster environment, e.g. by increased administrative flexibility concerning electoral registration and application processes for postal voting. For Saxonia, where the temporal distance between flood and election was especially short, we find evidence that postal voting was used at a disproportionally higher rate in affected districts, although not to an extent that would have made up for the overall negative turnout effect.

Finally, and more broadly, a negative aggregate turnout effect of natural disasters could have consequential effect on disaster prevention policy, especially in proportional electoral systems. Over time, and in particular if disaster frequencies in a certain region increases, the small turnout effects of any individual disaster can build up to sizeable effects due to habit formation (i.e., voters (especially first-time voters) that do not participate in elections are less likely to participate in future elections).[67] When turnout decreases in regions exposed to natural disasters, the electorate shifts towards constituents for which the saliency of disaster prevention might be lower, with consequences for the electoral platforms offered by politicians.

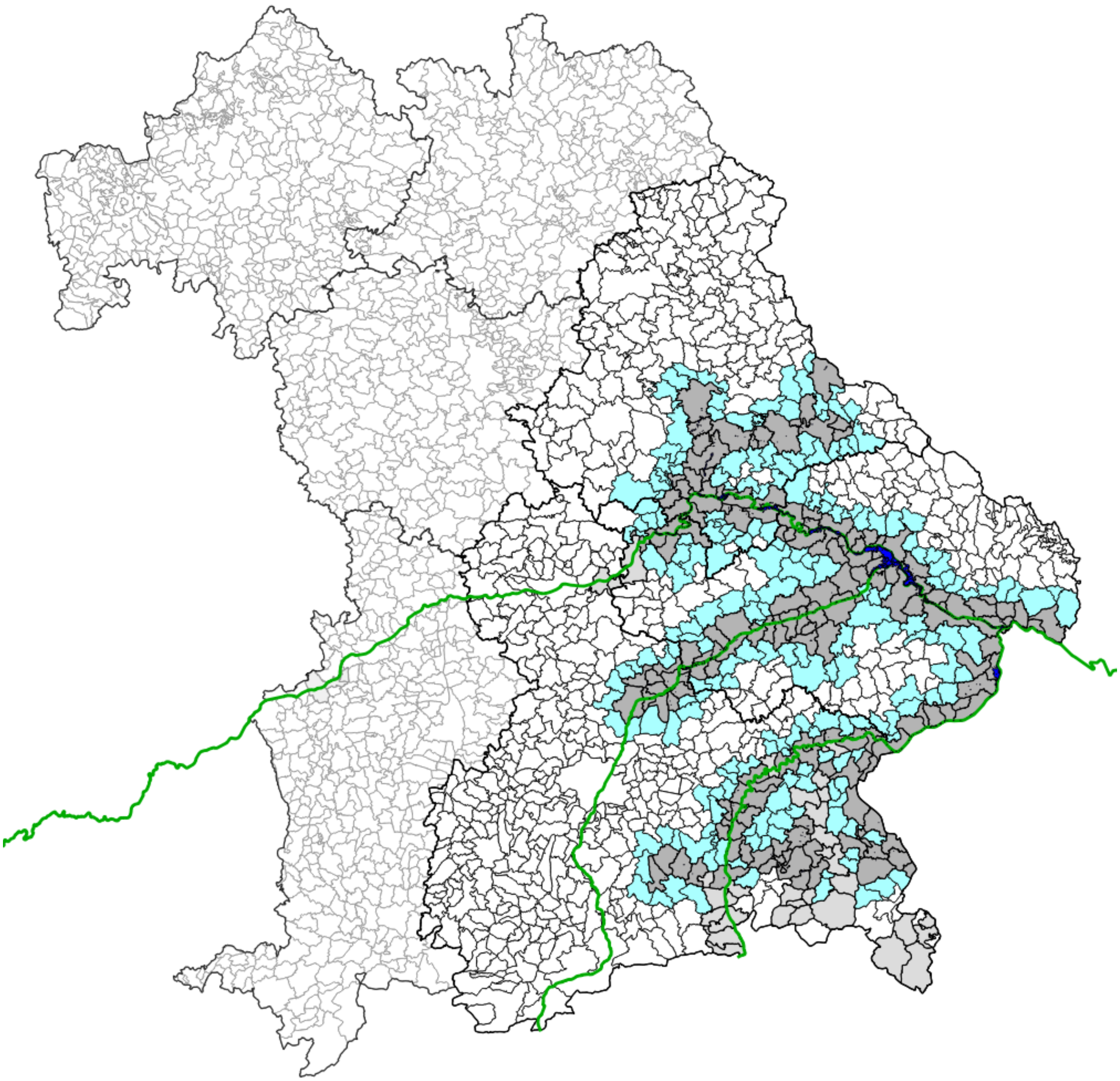
FIGURES

Figure 1: 2002 Flood Extent in Saxonia



Note: The figure shows the 2002 community boundaries and 2002 flood extent in Saxonia. The Elbe and Mulde, the main river systems in Saxonia, are shown in green and the maximal flood extent in the dark blue. Affected communities are highlighted in dark grey. Robustness checks draw on adjacent communities shown in light blue.

Figure 2: 2013 Flood Extent in Bavaria



Note: The figure shows community boundaries and 2013 flood extent in Bavaria. The flood region where treatment effects are estimated is highlighted. The main rivers of the Danube river system in Bavaria are shown in green, flood extent as measured by satellite data (kindly provided by the corporation Vista Remote Sensing in Geosciences GmbH) in the dark blue. Affected communities are highlighted in dark grey. Robustness checks draw on additional potentially affected communities (light grey) and adjacent communities (light blue).

TABLES

Table 1: Regressions on 1998-2002 Federal Election Turnout Trend in Saxony for the 2002 Elbe Flood and the 2009-2013 Federal Election and 2008-2013 State Election Turnout Trend in Bavaria for the 2013 Danube Flood With Entropy Weights

	(1)	(2)	(3)
		Fixed Effects Regressions	
Outcome:	**Saxony FE 1998-2002**	**Bavaria FE 2009-2013**	**Bavaria SE 2008-2013**
Turnout Trend (%)	(mean=-8.47, sd=2.26)	(mean=-0.86, sd=1.96)	(mean=6.11, sd=2.48)
Flood Indicator	-0.65***	-0.36**	-0.52***
	(0.23)	(0.17)	(0.20)
Year 2002	-7.58***		
	(0.43)		
Year 2013		-0.93	6.25***
		(0.74)	(0.73)
Control Variables	included	included	included
R-squared	0.94	0.16	0.87
Observations	860	1968	1968
Clusters	430	984	984

Note: The unit of analysis is a community, the fourth and smallest administrative unit. The fixed-effects regressions are estimated in 2014 community boundaries with entropy weights from 1998 placebo level regressions (Saxonia), 2005-2009 (Bavaria Federal Elections) and 2003-2008 (Bavaria State Elections) placebo fixed effect regressions. Model 1 is estimated for Saxonian communities, models 2 and 3 for communities in the three south-eastern Bavarian regions with floods occurring in 2013. Community level clustered standard errors are shown in brackets. The estimated constant is not shown. Regressions include the following controls: logged population, logged brute income (Saxonia only), logged brute tax income, employment rate (Bavaria only), proportion of elderly citizens (i.e., age>65), and the proportion of youth (i.e. aged<18). Estimates significant at the 0.05 (0.10, 0.01) level are marked with ** (*, ***).

Table 2: Estimation of Turnout Trends per Quartile of Flooded Community Area

	(1)	(2)	(3)	(4)
Panel A: Saxonian Federal Elections 1998-2002				
Area Flooded $\leq$ 25p	-1.08**			
	(0.53)			
25p $<$ Area Flooded $\leq$ 50p		-0.35		
		(0.33)		
50p $<$ Area Flooded $\leq$ 75p			-0.83**	
			(0.35)	
Area Flooded $>$ 75p				-0.69*
				(0.39)
Year 2002	-8.19***	-7.74***	-7.43***	-7.41***
	(1.01)	(0.63)	(0.68)	(0.59)
Observations	512	510	510	510
Panel B: Bavarian Federal Elections 2009-2013				
Area Flooded $\leq$ 25p	0.03			
	(0.27)			
25p $<$ Area Flooded $\leq$ 50p		-0.10		
		(0.27)		
50p $<$ Area Flooded $\leq$ 75p			-0.38	
			(0.44)	
Area Flooded $>$ 75p				-0.66***
				(0.19)
Year 2013	0.24	-2.62*	0.03	-1.10*
	(0.69)	(1.40)	(1.13)	(0.56)
Observations	1728	1728	1728	1728
Panel C: Bavarian State Elections 2008-2013				
Area Flooded $\leq$ 25p	-0.28			
	(0.38)			
25p $<$ Area Flooded $\leq$ 50p		-0.86**		
		(0.34)		
50p $<$ Area Flooded $\leq$ 75p			-0.36	
			(0.41)	
Area Flooded $>$ 75p				-0.56**
				(0.28)
Year 2013	6.33***	5.54***	6.31***	6.45***
	(0.89)	(1.36)	(1.41)	(0.79)
Observations	1728	1728	1728	1728

Note: The unit of analysis is a community, the fourth and smallest administrative unit. The fixed-effects regressions are estimated in 2014 community boundaries with entropy weights from 1998 placebo level regressions (Saxonia), 2005-2009 (Bavaria Federal Elections) and 2003-2008 (Bavaria State Elections) placebo fixed effect regressions. Panel A is estimated for Saxonian communities, panels B and C for communities in the three south-eastern Bavarian regions with floods occurring in 2013. Community level clustered standard errors are shown in brackets. Constant and controls are not shown. Regressions include the following controls: logged population, logged brute income (Saxonia only), logged brute tax income, employment rate (Bavaria only), proportion of elderly (i.e., age>65), proportion of youth (i.e. age<18). Estimates significant at the 0.05 (0.10, 0.01) level are marked with ** (*, ***).

Table 3: Regressions testing for spill-over effects for Federal Elections (FE) in Saxonia and Federal (FE) and State Elections (SE) in Bavaria

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Fixed-Effects Regressions								
	1998-2002 FE Saxony			**2005-2009 FE Bavaria**			**2008-2013 SE Bavaria**		
Outcome: Turnout (%)	Excl. Neighb.	Treated and Neighb.	Excl. Treated	Excl. Neighb.	Treated and Neighb.	Excl. Treated	Excl. Neighb.	Treated and Neighb.	Excl. Treated
Flood Indicator	-0.83***	-0.30		-0.46**	-0.26		-0.69***	-0.13	
	(0.28)	(0.31)		(0.18)	(0.20)		(0.21)	(0.26)	
Neighbours			-0.45			-0.52**			-0.37
			(0.39)			(0.24)			(0.29)
Year 2002	-7.50***	-7.54***	-7.57***						
	(0.51)	(0.48)	(0.70)						
Year 2013				-0.68	-1.59*	0.29	6.10***	6.20***	5.53***
				(0.78)	(0.94)	(1.24)	(0.75)	(1.04)	(0.92)
Control variables	included	included	included	included	included	included	included	included	included
R-Squared	0.94	0.95	0.94	0.14	0.20	0.09	0.87	0.87	0.87
Observations	777	632	627	1562	726	1648	1562	726	1648
Clusters	430	316	430	781	363	824	781	363	824

The unit of analysis is a community, the fourth and smallest administrative unit. The regressions are estimated in the 2014 community boundaries. All models are fixed effects regressions and use entropy weights from placebo difference-in-difference regressions of the preceding electoral period including controls (in case of Saxony: of 1998 turnout and control levels). Models 1-3 are estimated for Saxonian communities, models 4-9 for communities in the three south-eastern Bavarian regions with floods occurring in 2013. Community level clustered standard errors are shown in brackets. The estimated constant is not shown. Regressions include the following controls: logged population, logged brute income (Saxonia only), logged brute tax income, employment rate (Bavaria only), proportion of elderly citizens (i.e., age>65), and the proportion of youth citizens (i.e., age< 18)). Estimates significant at the 0.05 (0.10, 0.01) level are marked with ** (*, ***).

NOTES

[1] *Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergevernmental Panel on Climate Change.* Cambridge, United Kingdom and New York, NY, USA: Cambridge University Press, 2013.

[2] E.g., Gerald H. Kramer. "Short-Term Fluctuations in U.S. Voting Behavior, 1896-1964". In: *American Political Science Review* 65.1 (1971), pp. 131–143; Ray C. Fair. "The Effect of Economic Events on Votes for President". In: *The Review of Economics and Statistics* 60.2 (1978), pp. 159–173; Christopher Achen and Larry Bartels. "Democracy for realists: Holding up a mirror to the electorate". In: *Juncture* 22.4 (2016), pp. 269–275.

[3] E.g., Andrew Healy and Neil Malhotra. "Myopic Voters and Natural Disaster Policy". In: *American Political Science Review* 103.3 (2009), p. 387; Michael M. Bechtel and Jens Hainmueller. "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy". In: *American Journal of Political Science* 55.4 (2011), pp. 851–867; Shawn Cole, Andrew Healy, and Eric Werker. "Do voters demand responsive governments? Evidence from Indian disaster relief". In: *Journal of Development Economics* 97 (2011), pp. 167–181; John T. Gasper and Andrew Reeves. "Make It Rain? Retrospection and the Attentive Electorate in the Context of Natural Disasters". In: *American Journal of Political Science* 55.2 (2011), pp. 340–355.

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[5] E.g., Robert Bolin and Lois Stanford. "The Northridge Earthquake: Community-based Approaches to Unmet Recovery Needs". In: *Disasters* 22.1 (1998), pp. 21–38; Mark Levine and Kirstien Thompson. "Identity, place, and bystander intervention: Social categories and helping after natural disasters". In: *The Journal of social psychology* 144.3 (2004), pp. 229–245; Havidan Rodriguez, Joseph Trainor, and Enrico L Quarantelli. "Rising to the challenges of a catastrophe: The emergent and prosocial behavior following Hurricane Katrina". In: *The annals of the American academy of political and social science* 604.1 (2006), pp. 82–101; Johanna Ray Vollhardt. "Altruism born of suffering and prosocial behavior following adverse life events: A review and conceptualization". In: *Social Justice Research* 22.1 (2009), pp. 53–97.

[6] E.g., Robert L. Hawkins and Katherine Maurer. "Bonding, Bridging and Linking: How Social Capital Operated in New Orleans following Hurricane Katrina". In: *British Journal of Social Work* 40 (2010), pp. 1777–1793; Eiji Yamamura. "Effects of Interations among Social Capital, Income Learning from Experiences of Natural Disasters: A Case Study from Japan". In: *Regional Studies* 8.44 (2010), pp. 1019–1032; Hideki Toya and Mark Skidmore. "Do Natural Disasters Enhance Societal Trust?" In: *Kyklos* 67.2 (2014), pp. 255–279.

[7] E.g., Robert D. Putnam, Robert Leonardi, and Raffaella Y. Nanetti. *Making democracy work: Civic traditions in modern Italy.* Princeton NJ: Princeton University Press, 1994; Eric Banks. "The Social Capital of Self-Help Mutual Aid Groups". In: *Social Policy* 28.1 (1997), pp. 30–38; Anirudh Krishna. "What is the Role of Social Capital?" In: *Comparative Political Studies* 35.4 (2002), pp. 437–460.

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[9] William H. Riker and Peter C. Ordeshook. "A theory of the calculus of voting". In: *American Political Science Review* 62.1 (1968), pp. 25–42.

[10] For 2002, Bechtel and Hainmueller (Bechtel and Hainmueller, "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy") e.g. report an increase around 7 percentage points in the SPD second vote share of affected electoral districts along the Elbe, linked to the flood management. It was generally noted that the government's flood response played an import role in the re-election of Gerhard Schroeder as chancellor.

[11] Fair et al., "Natural Disasters and Political Engagement: Evidence from the 2010-11 Pakistani Floods".

[12] Sinclair, Hall, and Alvarez, "Flooding the Vote: Hurricane Katrina and Voter Participation in New Orleans".

[13] Bodet, Thomas, and Tessier, "Come hell or high water: An investigation of the effect of a natural disaster on a local election".

[14] K. L. Remmer. "Exogenous Shocks and Democratic Accountability: Evidence From the Caribbean". In: *Comparative Political Studies* 47.8 (2014), pp. 1158–1185, p. 1170.

[15] Jowei Chen. "Voter Partisanship and the Effect of Distributive Spending on Political Participation". In: *Journal of Politics* 57.1 (2013), pp. 200–217.

[16] Ibid.

[17] Ibid.

[18] Riker and Ordeshook, "A theory of the calculus of voting".

[19] Ibid.

[20] Anthony Downs. *An Economic theory of democracy.* 1957.

[21] Brian Barry. *Sociologists, Economists and Democracy.* Chicago IL: Chicago University Press, 1978; *To Vote or Not to Vote? the Merits and Limits of Rational Choice Theory*; Andrew Gelman, Jonathan N. Katz, and Joseph Bafumi. "Standard Voting Power Indexes Do Not Work: An Empirical Analysis". In: *British Journal of Political Science* 34 (2004), pp. 657–674.

[22] John Aldrich. "Rational Choice and Turnout". In: *American Political Science Review* 37.1 (1993), pp. 246–278.

[23] For similar arguments regarding cold weather and rain see Ron Shachar and Barry Nalebuff. "Follow the Leader: Theory and Evidence on Political Participation". In: *American Economic Review* 89.3 (1999), pp. 525–547; Thomas Fujiwara, Kyle Meng, and Tom Vogl. "Habit Formation in Voting: Evidence from Rainy Elections". In: *American Economic Journal: Applied Economics* 8.4 (2016), pp. 160–188.

[24] E.g., Levine and Thompson, "Identity, place, and bystander intervention: Social categories and helping after natural disasters"; Vollhardt, "Altruism born of suffering and prosocial behavior following adverse life events: A review and conceptualization".

[25] Rodriguez, Trainor, and Quarantelli, "Rising to the challenges of a catastrophe: The emergent and prosocial behavior following Hurricane Katrina".

[26] E.g., Carole J. Uhlaner. *What the Downsian Voter Weighs: a Reassessment of the Costs and Benefits of Action.* Ann Arbor: University of Michigan Press, 1999; David P. Myatt. *A Theory of Voter Turnout.* Working Paper. London Business School, 2015.

[27] E.g., Hawkins and Maurer, "Bonding, Bridging and Linking: How Social Capital Operated in New Orleans following Hurricane Katrina"; Yamamura, "Effects of Interations among Social Capital, Income Learning from Experiences of Natural Disasters: A Case Study from Japan"; Eiji Yamamura. "Natural Disasters and Social Capital Formation: The Impact of the Great Hanshin-Awaji Earthquake". In: *Papers in Regional Science* (2014).

[28] See e.g., Putnam, Leonardi, and Nanetti, *Making democracy work: Civic traditions in modern Italy*; Banks, "The Social Capital of Self-Help Mutual Aid Groups".

[29] The larger distance between flood and election in 2013 especially allowed for disaster relief to be more effectively distributed and reconstruction to begin to a larger extent in the Bavarian case. Given our theoretical arguments, this could potentially mitigate the effect of the floods on the cost of voting. All else equal, (negative) flood effects in Saxonia 2002 should therefore be larger than in Bavaria 2013. We do not expect large temporal effects, however. First, empirically even a three month period is very short compared to existing research in this area. E.g. Cole, Healy, and Werker (Shawn Cole, Andrew Healy, and Eric Werker. "Do voters demand responsive governments? Evidence from Indian disaster relief". In: *Journal of Development Economics* 97.2 [2012], pp. 167–181) estimate for Indian voters a cut-off of a one-year time period when disasters do no longer impact aggregate electoral outcomes significantly. Similarly, Lazarev et al. (Egor Lazarev et al. "Trial by Fire: A Natural Disaster's Impact on Support for the Authorities in Rural Russia". In: *World Politics* 66.04 [2014], pp. 641–668) note increased government support one year after forest fires in Russia. Bechtel and Hainmueller (Michael M. Bechtel and Jens Hainmueller. "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy". In: *American Journal of Political Science* 55.4 [2011], pp. 852–868) and Eriksson (Lina M. Eriksson. "Winds of Change: Voter Blame and Storm Gudrun in the 2006 Swedish Parliamentary Election". In: *Electoral*

Studies 41 [2016], pp. 129–142) even argue for a persistent influence of natural disasters on vote choice over several electoral cycles. But second, even if we empirically expect an especially high impact of the disaster just before election day (as e.g. argued by Chen, "Voter Partisanship and the Effect of Distributive Spending on Political Participation"), this should make it harder for us to find any effects (especially in the Bavaria case), as we would expect time to off-set both any negative costs and positive duty effects on turnout. We therefore do not consider the potentially differential implementation of flood relieve policy in the following, other than noting that effects in Bavaria might already be dampened to an unknown extent relative to effects in Saxonia.

[30] LUA. *Das Elbehochwasser im Sommer 2002.* Tech. rep. 73. Landesumweltamt Brandenburg, 2002, p. 39; LFU. *Junihochwasser 2013 - Wasserwirtschaftlicher Bericht.* Tech. rep. Bayerisches Landesamt für Umwelt, 2014; BMI. *Bericht zur Flutkatastrophe 2013: Katastrophenhilfe, Entschädigung, Wiederaufbau.* Tech. rep. Stab Fluthilfe im Bundesministerium des Inneren [German Federal Ministry of the Interior], 2013.

[31] CSU. *Hochwasser-Katastrophe 2013: Koalition hilft schnell und unbürokratisch.* Tech. rep. CSU Landesgruppe, Deutscher Bundestag, 2013, pp. 1–4; BMVg. *Hochwasserkatastrophe im August 2002: Einsatz der Bundeswehr.* Tech. rep. August. Bundesministerium der Verteidigung [German Federal Ministry of Defense], 2002.

[32] BMI. *Flutkatastrophe 2013: Katalog der Hilfeleistungen.* Tech. rep. August. Stab Fluthilfe im Bundesministerium des Innern [German Federal Ministry of the Interior], 2013, p. 38.

[33] In Saxony 2002, 500 per affected person, maximum 2,000 per household, were handed out to all households that applied (Restriction: household income $<$ 40,000); in Bavaria 2013, this 'instant flood support' amounted to 1,500 per household without any income restriction (ibid.).

[34] Marc-Andr Kaufhold and Christian Reuter. "Vernetzte Selbsthilfe in Sozialen Medien am Beispiel des Hochwassers 2013". In: *i-com* 13.1 (2014), pp. 20–28.

[35] Patricia Hogwood. "The chancellor-candidates and the campaign". In: *German Politics* 13.2 (2004), pp. 243–267; Franz Urban Pappi, Susumu Shikano, and Evelyn Bytzek. "Der Einfluss politischer Ereignisse auf die Popularität von Parteien und Politikern und auf das Parteiensystem". In: *Kölner Zeitschrift für Soziologie und Sozialpsychologie* 56 (2004), pp. 51–70; Peter Pulzer. "The devil they know: The German federal election of 2002". In: *West European Politics* 26.2 (2003), pp. 153–164.

[36] Bechtel and Hainmueller, "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy".

[37] Christiane Eilders et al. "Surfing the tide : an analysis of party and issue coverage in the national election campaign 2002". In: *German Politics* 13.2 (2004), pp. 218–242.

[38] Benjamin-Immanuel Hoff and Dan Hough. "Not Much Ado About Quite a Lot? the German Election of September 2013". In: *Representation* 50.1 (2014), pp. 129–137.

[39] Rainer-Olaf Schultze. "Die bayerische Landtagswahl vom 15 . September 2013 : Bund und Land Hand in Hand". In: *Zeitschrift für Parlamentsfragen* 45.2 (2014), pp. 326–348.

[40] We looked at the responses of Politbarometer survey respondents to the question 'What is according to your opinion currently the most important problem in Germany?' right before the election. The top 5 topics mentioned (reported in Appendix Figure 1) were mainly economic and/or social security related, with unemployment as most frequently raised topic in both Saxonia (65%) and Bavaria (13%). While unemployment explicitly enters our regressions as control variable, we do not expect that other frequently mentioned topics such as unification or the Euro crisis/Grexit debate have a differential impact within Bavaria/Saxonia, as they are not geographically focused topics. Similarly, important last minute campaign topics mentioned in the literature, as the Iraq war (discussed e.g. in Bechtel and Hainmueller (Bechtel and Hainmueller, "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy")) or a 2013 public scandal in Bavaria (discussed e.g. in Rudolph and Daeubler (Lukas Rudolph and Thomas Daeubler. "Holding Individual Representatives Accountable: The Role of Electoral Systems". In: *Journal of Politics* 78.3 [2016], pp. 746–762)) are not correlated with flood exposure.

[41] `https://www.statistikdaten.bayern.de/`

[42] `http://www.statistik.sachsen.de/`

[43] Saxony currently has no similarly high-quality flood layer for the 2013 floods and Bavaria was not nearly as hard hit in 2002 than it was in 2013.

[44] Publicly available at `http://www.umwelt.sachsen.de/umwelt/wasser/8838.htm`. Many thanks to Kathrin Fischer at the agency for her support.

[45]We are grateful to Heike Bach from Vista for the provision of their data and to Martina Hodrius for her help in preprocessing the GIS files.

[46] Joshua D Angrist and Jörn-Steffen Pischke. *Mostly harmless econometrics: An empiricist's companion.* March. Princeton University Press, 2009; Jeffrey M. Wooldridge. *Econometric Analysis of Cross Section and Panel Data.* Cambridge, London: MIT Press, 2010.

[47] Donald B. Rubin. "Estimating causal effects of treatments in randomized and nonrandomized studies." In: *Journal of Educational Psychology* 66.5 (1974), pp. 688–701.

[48] Michael Lechner. "The Estimation of Causal Effects by Difference-in-Difference Methods". In: *Foundations and Trends in Econometrics* 4.3 (2010), pp. 165–224.

[49] Jens Hainmueller. "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies". In: *Political Analysis* 20.1 (2012), pp. 25–46.

[50] Ibid.

[51]By identifying treatment effects within Saxony and within south-eastern Bavaria, we as well sidestep confounding that might arise from the differential party systems in Bavaria and Eastern Germany - the latter e.g. being referred to as "'different world" in electoral terms' (Thomas Saalfeld. "Party Identification and the Social Bases of Voting Behaviour in the 2002 Bundestag Election". In: *German Politics* 13.2 [2004], pp. 170–200, p. 197).

[52]State elections generally experience lower average turnout rates, although we can expect that electoral results for these elections, especially if so close together temporally, are not independent from each other (Michael M. Bechtel. "Not always second order: Subnational elections, national-level vote intentions, and volatility spillovers in a multi-level electoral system". In: *Electoral Studies* 31.1 [2012], pp. 170–183).

[53] Sinclair, Hall, and Alvarez, "Flooding the Vote: Hurricane Katrina and Voter Participation in New Orleans".

[54] Bechtel and Hainmueller, "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy", pp. 862-863.

[55]For Bavaria, as our placebo tests show that the parallel trend assumption likely holds, we also regressed the continuous indicator and its square on turnout. Although there is, as expected, a negative relationship between the flooded area and turnout, there does not seem to be a strong non-linear relationship. The squared flood indicator showed no significant effect on turnout.

[56] Fair et al., "Natural Disasters and Political Engagement: Evidence from the 2010-11 Pakistani Floods".

[57] J. G. Gimpel and J. E. Schuknecht. "Political participation and the accessibility of the ballot box". In: *Political Geography* 22.5 (2003), pp. 471–488.

[58]This was in 2002 in a small part of the district Sächsische Schweiz on September 21st, 2002 (MDR. *Die Jahrhundertflut - 10 Jahre danach.* 2013) and in 2013 in the district Deggendorf in Bavaria on June 22nd, 2013 (PNP. *Katastrophenfall nach 19 Tagen aufgehoben: Infos zur Hilfe.* 2013).

[59]Personal communication with Thomas Obst, Election Officer in the district Sächsische Schweiz-Osterzgebirge, October 13th, 2015, Berenice König, Head of the Elections Division, Statistical Office of Saxony, October 7th, 2015, with Hartwig Zorn, Department for Emergency Services and Civil Protection, Saxonian Ministry of the Interior, August 24th, 2016 and with Peter Hallermaier, Section I D 4, Bavarian Ministry of the Interior, August 24th, 2016.

[60] Bechtel and Hainmueller, "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy"; Dan Hough. "It's the East Stupid!' Eastern Germany and the outcome of the 2002 Bundestagswahl". In: *Representation* 39.2 (2003), pp. 137–145.

[61] Thorsten Faas. "The German Federal Election of 2013: Merkel's Triumph, the Disappearance of the Liberal Party, and Yet Another Grand Coalition". In: *West European Politics* 38.1 (2015), pp. 238–247; Harald Schoen and Robert Greszki. "A Third Term for a Popular Chancellor: An Analysis of Voting Behaviour in the 2013 German Federal Election". In: *German Politics* 23.4 (2014), pp. 251–267.

[62] Fair et al., "Natural Disasters and Political Engagement: Evidence from the 2010-11 Pakistani Floods".

[63] Sinclair, Hall, and Alvarez, "Flooding the Vote: Hurricane Katrina and Voter Participation in New Orleans".

[64]Note that alternative explanations, especially the spatial distribution of the ethnic composition of communities as a confounder have been suggested as explanation for the u-shape in the results of Sinclair, Hall, and Alvarez (ibid.) as well (James Vanderleeuw, Baodong Liu, and Erica Williams. "The 2006 New

Orleans Mayoral Election: The Political Ramifications of a Large-Scale Natural Disaster". In: *PS: Political Science & Politics* 41.04 [2008], pp. 795–801).

[65] Chen, "Voter Partisanship and the Effect of Distributive Spending on Political Participation".

[66] Ibid.

[67] Fujiwara, Meng, and Vogl, "Habit Formation in Voting: Evidence from Rainy Elections".