Durham University Business School

Department of Economics

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Working Paper No. 13, 2026

Durham Business School working papers series ISSN 1749-3641

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https://www.durham.ac.uk/business/about/departments/economics/

Flood Voting

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August 28, 2026

Abstract

This paper examines how a major natural disaster reshapes political preferences using the 1998 Bangladesh flood. Combining district-level flood exposure with individual responses from the 1996 and 2002 World Values Survey, we find that flood exposure increases rejection of both dominant parties, the Awami League and the Bangladesh Nationalist Party, while making the Jatiya Party, the major third political force, relatively more attractive. The political response is strongest in the most severely flooded areas and among economically disadvantaged respondents, while greater government relief is associated with a smaller increase in rejection of the two dominant parties. These results suggest that political dissatisfaction following a disaster need not simply shift support from the government to its main rival. Instead, major shocks can weaken both dominant parties and create space for an alternative political force, a pattern we term "flood voting".

Keywords: flood voting; voting behaviour; party preferences; natural disasters; political accountability; Dominant Parties; third parties

*Corresponding author: habib.rahman@durham.ac.uk. Any errors are our own. We have no material financial interests related to the research described in this paper.

1. Introduction

Natural disasters are commonly studied as economic and humanitarian shocks, but they can also be consequential political events (Besley and Burgess, 2002; Healy and Malhotra, 2009). By imposing sudden and highly visible losses, disasters simultaneously test public infrastructure, administrative capacity, and relief systems. A well-managed response can reinforce perceptions of competence and strengthen support for incumbents, whereas failures in preparedness, relief, or reconstruction can expose state weakness or unequal treatment and provoke political backlash (Bechtel and Hainmueller, 2011; Cole et al., 2012).

Yet the political consequences of such punishment are not obvious. Standard accounts of retrospective accountability predict that citizens withdraw support from incumbents when disaster losses are severe or government responses are inadequate. But a decline in incumbent support does not reveal where that support goes. Citizens may turn to the principal opposition, become more receptive to smaller parties, abstain from electoral participation, or reject the established political class more broadly after a crisis or disaster exposure (Hernández and Kriesi, 2016; Visconti, 2022). These responses have different implications for electoral competition and political stability. Aggregate election results, however, are unable to cleanly distinguish among them because they reflect not only attitudes toward parties but also candidate quality, local competition, campaign activity, turnout, and strategic voting.

This study examines how exposure to the 1998 Bangladesh flood changed citizens' evaluations of the country's major political parties. Combining district-level flood exposure with individual responses from the 1996 and 2002 waves of the World Values Survey (WVS), we examine whether the flood changed citizens' rejection of the incumbent Awami League (AL), the Bangladesh Nationalist Party (BNP), the principal opposition at the time of the flood, and the Jatiya Party, a major third force in national politics. BNP subsequently returned to government following the 2001 parliamentary election, before the 2002 post-flood survey. Accordingly, our post-flood outcomes capture medium-run evaluations of the two dominant parties rather than a contemporaneous incumbent-opposition comparison. We also examine respondents' first-choice party preference as an additional outcome. These survey measures provide comparable individual-level indicators of party evaluations before and after the flood and are less directly shaped by constituency-specific candidates, turnout, and strategic considerations. They nevertheless capture stated attitudes rather than realized electoral behavior. Empirically, we exploit the timing of the flood and spatial variation in exposure using a difference-in-differences (DID) framework that compares changes in political attitudes between 1996 and 2002 across flooded and non-flooded districts. We complement the binary treatment measure with a continuous measure of flood intensity.

Our main finding is that the political response extends beyond conventional incumbent punishment. The benchmark DID estimates show that flood exposure increases the probability of rejecting AL, which governed at the time of the flood, by 23.5 percentage points, and, strikingly, also increases rejection of BNP by 33.2 percentage points. In contrast, rejection of JP declines. The first-choice results reinforce this pattern, as flood-exposed respondents become less likely to name either AL or BNP as their preferred party, while first-choice support for JP increases. Taken together, the results suggest that the differential change associated with flood exposure did not simply translate into greater support for BNP, the party that had been AL's principal rival. Instead, both dominant parties lost ground, while a major third political force became a more attractive alternative. We coin this pattern flood voting: a disaster-induced shift in political preferences away from dominant parties and toward a third-party alternative. This pattern of flood voting constitutes our central contribution by showing that the political consequences of a disaster may extend beyond incumbent accountability to a broader realignment of party competition.

One plausible reason JP benefited is historical. JP was closely associated with the Ershad period in the 1980s, when flood-control infrastructure and embankment-based protection became highly visible after the severe floods of 1987 and 1988. Flood-exposed respondents may therefore have viewed JP not simply as a protest option, but as a party linked in public memory to flood protection. We treat this interpretation as suggestive because the WVS does not ask respondents why they report a particular party preference.

Additional results help clarify this pattern. Flood exposure does not significantly reduce broader confidence in government or political parties, suggesting that the observed changes in party preferences do not reflect a generalized deterioration in political confidence. We also find that greater government relief is associated with a smaller increase in rejection of both AL and BNP. Importantly, this association is evident for both dominant parties, while relief is not significantly associated with the corresponding change in attitudes toward JP. This pattern is consistent with relief cushioning some of the negative political responses associated with disaster-related hardship, rather than simply rewarding the incumbent or driving the movement toward the third-party alternative. This interpretation is consistent with previous evidence that post-disaster assistance can mitigate electoral punishment and generate political rewards for responsive governments (Bechtel and Hainmueller, 2011; Cole et al., 2012).

Our heterogeneity results are consistent with this interpretation. The political effects are strongest in the most heavily flooded areas and among lower-income and lower-social-status respondents, while the responses are weaker among more advantaged groups. This pattern suggests that the political backlash is more pronounced among those likely to have borne greater material costs from the disaster, which may help explain why greater relief provision is associated with a smaller adverse political response. Finally, future flood exposure is not systematically associated with pre-flood political preferences, and the main findings remain robust to alternative measures of flood exposure, changes in the composition of the comparison group, district-level aggregation, and placebo treatment assignments.

Our study contributes to a large literature on political accountability and the consequences of natural disasters. Besley and Burgess (2002) show that political accountability strengthens government responsiveness to calamities, highlighting the role of electoral incentives in disaster response. Subsequent work examines more directly how voters respond to both disaster-related losses and government action. Healy and Malhotra (2009) find that voters reward post-disaster relief more strongly than preparedness, while Gasper and Reeves (2011) show that electoral responses depend on both disaster damage and executive action. Cole et al. (2012) similarly document incumbent punishment following adverse weather shocks in India, with government relief attenuating this effect, whereas Bechtel and Hainmueller (2011) find short- and medium-run electoral rewards following the government response to severe flooding in Germany. Together, these studies show that disasters become politically consequential not only through the losses citizens experience but also through the government response they observe.

Importantly, the magnitude and direction of these political responses depend on the context in which disasters occur. Effective disaster response can generate electoral rewards (Masiero and Santarossa, 2021), while Neugart and Rode (2021) show that electoral rewards following flooding can vary with voters' democratic experience. By contrast, severe losses or inadequate responses can produce strong punishment (Eriksson, 2016; Morvan and Paty, 2024). Chang and Berdiev (2015) similarly show that natural disasters can increase the likelihood of government replacement, while Bodet et al. (2016) show that estimated flood effects can depend importantly on the choice of comparison group.[1] This heterogeneity cautions against viewing natural disasters as mechanically producing either incumbent punishment or gratitude. Instead, political responses depend on the severity of exposure, the quality of the public response, and the context in which voters assign responsibility. Our findings fit this broader pattern, with the strongest political responses occurring in more heavily flooded areas and among lower-income and lower-social-status respondents, who are likely to have been more exposed to the material consequences of the disaster.

Much of this literature, however, is organized around whether the incumbent is rewarded or punished. Incumbent punishment alone does not reveal which political alternative benefits when support for the government declines. This distinction matters because major shocks may weaken established political parties more broadly rather than simply transferring support from government to opposition. Hernández and Kriesi (2016) show that economic crises can weaken incumbents while also destabilizing established patterns of party competition, while Visconti (2022) finds that flood exposure can redirect electoral choices toward left-wing and independent candidates. Related work further suggests that support for protest or alternative parties can represent a structured political response rather than undifferentiated dissatisfaction (Hooghe and Dassonneville, 2018; Birch and Dennison, 2019). Our setting links the disaster-accountability literature to a broader party-system perspective. By identifying flood voting, we show that electoral responses to disasters need not operate solely through punishment of the incumbent. Because we observe evaluations of the incumbent, the principal opposition, and a major third political force simultaneously, we can trace how political support is reallocated across the party system. Flood exposure reduces support for

[1] Recent evidence reinforces the importance of context and heterogeneity. Arin et al. (2025) document heterogeneous and non-monotone flood effects, Livert et al. (2025) find political rewards following disaster response, and Baerlocher et al. (2025), Berlemann et al. (2025), and Gualtieri et al. (2026) show that effects on turnout, preferences, and electoral outcomes vary across local and political settings.

both the incumbent and its main rival, leaving the third political alternative as the primary beneficiary of political dissatisfaction. Flood voting may therefore extend beyond incumbent accountability to a broader reallocation of political support across parties.

Our evidence on government relief also contributes to a related literature on how public assistance shapes political responses to shocks. Post-disaster assistance can reduce material losses, signal government responsiveness, and strengthen political support among affected households. Gallego (2018) shows that disaster assistance can benefit incumbents through aid and clientelism, while Manacorda et al. (2011) find more generally that government transfers increase political support among beneficiaries. At the same time, the distribution of relief reflects not only need but also accessibility and political incentives. Francken et al. (2012) document these forces in the allocation of cyclone relief, while Cooperman (2022) and Duchoslav et al. (2023) show how electoral considerations can influence the timing and targeting of assistance. Our results extend this literature in an important direction. Greater relief provision is associated with a smaller increase in rejection of both the incumbent AL and the main opposition BNP, rather than benefiting only the party in government. At the same time, relief does not significantly affect support for JP. This pattern suggests that post-disaster assistance may soften backlash toward the dominant parties without necessarily accounting for the movement toward the third political alternative.

A related literature examines whether disasters affect political trust, legitimacy, and engagement more broadly. Most closely related to our setting, Rahman et al. (2020) use the 1998 Bangladesh flood and show that flood exposure reduces interpersonal trust. Carlin et al. (2014a) document declines in democratic legitimacy following disaster damage, while Carlin et al. (2014b) show that these responses depend on state capacity and political context. In Pakistan, Fair et al. (2017) find that greater flood exposure increased political engagement, including subsequent voter turnout, while Petrova and Rosvold (2024) show that flood relief can improve institutional trust. In contrast, we find no corresponding decline in general confidence in government or political parties, suggesting that the observed response is concentrated more in party-specific evaluations than in a generalized deterioration in political trust.

The remainder of the paper proceeds as follows. Section 2 describes the political setting and the 1998 Bangladesh flood. Section 3 presents the data and the construction of flood exposure and political preference measures. Section 4 outlines the empirical strategy. Section 5 reports the main results, heterogeneity analyses, political confidence, the role of disaster relief, and robustness exercises. Section 6 concludes.

2. The 1998 Flood in Bangladesh

Bangladesh is exceptionally exposed to riverine flooding. Most of the country lies on the low-lying delta formed by the Ganges, Brahmaputra, and Meghna river systems and their extensive network of tributaries. Seasonal inundation is therefore a regular feature of the monsoon. In a typical year, floodwater covers about 30 percent of the country's land area, generally for only a relatively short period. Extreme floods are much less common, however, and differ sharply from normal flooding in their geographic extent, depth, and duration (del Ninno et al., 2001).

The 1998 flood was exceptional in all three dimensions. Beginning in early July and continuing through late September, it persisted for approximately ten weeks. Over the course of the event, flooding affected 100,250 square kilometres (roughly 68 percent of Bangladesh's land area) and approximately 30 million people. At major river-monitoring stations, water remained above locally defined danger levels for an average of 59 days. The prolonged inundation caused widespread damage to housing, transport infrastructure, livelihoods, and public health. These characteristics led the event to become widely known as Bangladesh's "flood of the century" (del Ninno et al., 2001; Kunii et al., 2002).

The prolonged flood also caused substantial disruption to food production. In particular, it extended into the cultivation period for the main monsoon-season rice crop, contributing to an estimated total rice-production shortfall of around 2 million metric tons (del Ninno et al., 2001). The resulting threat to household food security prompted a substantial public response, including government- and donor-supported distributions of rice and wheat and measures to support food imports and availability (del Ninno et al., 2001, 2003).

A further feature of the Bangladeshi setting is that the principal third party, the Jatiya Party (JP), carried a distinct historical association with flood-control policy. JP was closely tied to Hussain Muhammad Ershad, who governed Bangladesh during the 1980s, and remained associated with his period in power. During this period, and especially after the major floods of 1987 and 1988, flood protection became a highly visible national policy issue. The subsequent Flood Action Plan emphasized embankments, drainage, riverbank protection, and flood protection for major riverine and urban areas. This history matters for interpreting the party-specific results. For voters exposed to the 1998 flood, JP may have represented not only an alternative to the two dominant parties, but also a party associated in public memory with visible flood-protection infrastructure. We therefore interpret the increase in JP support as consistent with a policy-memory channel, while recognizing that the survey data do not allow us to observe respondents' stated reasons for supporting JP.

3. Data and Measurement

3.1. Flood Exposure

We measure local flood exposure using the map produced by the Flood Forecasting and Warning Centre (FFWC) of the Bangladesh Water Development Board.[2] Released on 14 September 1998, the map reports the classification of flooded subdistricts into three severity categories based on water levels relative to a location-specific danger level: normal flooding (more than 50 centimetres below the danger level), moderate flooding (within 50 centimetres below the danger level), and severe flooding (at or above the danger level). Figure 1 reproduces the spatial distribution of these classifications.

To merge the FFWC map with the district-level location information available in the WVS, we translate the published categories into an ordinal score. We assign a value of 0 to subdistricts not classified as flooded on the map and values of 1, 2, and 3 to subdistricts classified by the FFWC as experiencing normal, moderate, and severe flooding, respectively. Because the WVS identifies respondents at the district rather than the subdistrict level, we calculate the unweighted mean of these scores across all subdistricts in district d:

$$\text{Flood severity of district } d = \frac{\sum_{s=1}^{N_d} \text{Flood score}_{sd}}{N_d}. \tag{1}$$

where the flood score for subdistrict s in district d takes one of the four coded values described

[2] Established by the Government of Bangladesh in 1972 as part of the Bangladesh Water Development Board, the Flood Forecasting and Warning Centre monitors river and rainfall conditions and issues flood forecasts and warnings. It also produces subdistrict-level flood maps based on real-time information on water levels and rainfall.

above, and N_d denotes the number of subdistricts in district d. The resulting district-level index ranges from 0 to 3. A value of 0 indicates that no subdistrict in the district was classified as flooded, whereas a value of 3 indicates that every subdistrict was classified as severely flooded. Intermediate values jointly reflect the share of subdistricts exposed and the severity of that exposure; the index does not identify these two dimensions separately. Figure 2 maps the district-level index.

3.2. World Values Survey (WVS) and Political Disaffection

Our individual-level data come from the Bangladesh samples of the WVS. The WVS waves used here are repeated cross-sections of different respondents over time. The third-wave survey was fielded from 1 to 31 August 1996, approximately two years before the flood, and the fourth-wave survey was fielded from 20 August to 15 September 2002, approximately four years afterward. We therefore observe independent samples of adults before and after the disaster.

The post-flood WVS wave was conducted in 2002, roughly four years after the 1998 flood and after the 2001 parliamentary election, when BNP returned to government. Accordingly, our estimates capture medium-run changes in party evaluations associated with flood exposure rather than immediate post-disaster electoral responses. This change in government is also important for interpreting the party-specific estimates. AL governed at the time of the flood, whereas BNP was the principal opposition in 1998 but the governing party by the 2002 survey. We therefore interpret the BNP estimates as changes in evaluations of one of the two dominant parties over this period, rather than as the response to a party that remained in opposition throughout. Throughout the paper, we refer to AL as the incumbent when discussing the 1998 flood because it was the governing party at the time of the disaster.

Following Rahman et al. (2020), we assign each respondent the flood-severity index of the district in which the respondent resides. The merged data contain variation across the pre- and post-flood survey waves and across districts with different levels of flood exposure. The WVS also records respondent characteristics, including age, sex, marital status, number of children, education, income, and self-assessed social class. Controlling for these characteristics helps account for observable compositional differences between survey waves and helps improve the precision of the estimates.

Our primary outcome is an indicator of stated anti-incumbent political disaffection. Respondents were asked, “Is there any party on this list that you would never vote for?” We construct an indicator equal to one if the respondent names AL, the incumbent party at the time of the 1998 flood, and zero if the respondent names another party or “other.” Respondents answering “don’t know” are excluded from the main analysis.

4. Identification and Empirical Strategy

We estimate the effect of the 1998 flood using a difference-in-differences (DID) design that combines geographic variation in flood exposure with the timing of the two Bangladesh waves of the WVS. The 1996 survey was conducted before the flood, whereas the 2002 survey was conducted afterward.

Our baseline specification is:

$$Y_{idrt} = \alpha_d + \gamma_{rt} + \beta \left(Flood_d \times Post_t\right) + \pi' X_{idrt} + \varepsilon_{idrt}. \tag{2}$$

where Y_{idrt} is the political-disaffection outcome for respondent i, residing in district d, located in division r, and surveyed in year t. In the baseline analysis, Y_{idrt} equals one if the respondent reports that they would never vote for the AL, which was the incumbent party at the time of the flood, and zero otherwise.

The term α_d denotes district fixed effects. These absorb all observed and unobserved district characteristics that remain constant over time, including persistent differences in geography, economic conditions, exposure to river systems, and baseline political preferences. Because flood exposure is time invariant, the main effect of $Flood_d$ is also absorbed by the district fixed effects and is therefore not separately included in the equation.

The term γ_{rt} denotes division-by-year fixed effects. These allow each administrative division to experience its own change in political attitudes between 1996 and 2002. They absorb shocks and policy changes common to districts within the same division in each survey wave that may affect attitudes toward political parties and be correlated with the geographic pattern of flood exposure. Because division-by-year fixed effects also absorb the common survey-year effect, a separate year fixed effect or standalone $Post_t$ term is not included.

$Flood_d$ is an indicator equal to one if district d was classified as flood-affected in 1998 and zero if it was not flooded, while $Post_t$ equals one for respondents surveyed in 2002 and zero for respondents surveyed in 1996.[3] Our coefficient of interest is β, which measures the DID effect of flood exposure on anti-incumbent political disaffection.[4]

The vector X_{idrt} contains respondent-level characteristics, including age, gender, marital

[3] Because seasonal flooding is common in Bangladesh, we distinguish meaningful flood exposure from very low levels of exposure by classifying districts with an average flood-severity score below 0.5 as negligible exposure and code them as untreated. In subsequent specifications, we use the continuous district-level severity index directly to preserve variation in the intensity of flood exposure.

[4] For binary outcomes, the DID specifications are estimated as linear probability models.

status, number of children, and an indicator for formal education. Finally, ε_{idrt} denotes the idiosyncratic error term, capturing unobserved individual-level factors that affect political disaffection.

To examine whether the estimated response is concentrated on the party governing at the time of the flood or extends to other major political alternatives, we re-estimate the same specification using indicators for whether respondents report that they would never vote for BNP and JP. BNP was the principal opposition at the time of the 1998 flood but returned to government following the 2001 election, while JP represented the major third political force outside the two dominant parties.

An increase concentrated on AL would be more consistent with punishment of the government in office during the disaster. By contrast, increased rejection of both AL and BNP by 2002 would indicate a broader relative deterioration in evaluations of the two parties that dominated national politics over this period. Because the survey records only one party that each respondent would never support, the party-specific outcomes are mutually exclusive and should be interpreted as changes in the distribution of rejected-party responses rather than as independent outcomes.

In subsequent specifications, we replace the binary $Flood_d$ indicator with the continuous district-level flood-severity index described in Section 3.1. This allows the estimated response to vary with the intensity of flood exposure rather than only distinguishing between affected and unaffected districts. All standard errors are clustered at the district level because flood exposure is assigned at that level.

5. Results

5.1. Pre-Treatment Balance

Before estimating the effects of the 1998 flood, we first examine whether respondents in districts that were subsequently flooded differed systematically from those in non-flooded districts prior to the event. Table 1 compares demographic characteristics and political preferences measured in the 1996 WVS. The two groups are similar across most observable characteristics. As shown in Table 1, there are no statistically significant differences in most observable characteristics.

The balance in political preferences is particularly important for our empirical design. Prior to the flood, future flood exposure is not significantly associated with whether respondents report that they would never vote for any of the three major parties. Similarly, the proportions reporting the AL, BNP, or JP as their first-choice party are statistically indistinguishable across subsequently flooded and non-flooded districts. These patterns provide no evidence that future flood exposure was systematically related to baseline partisan preferences.

Next, Table 2 provides a regression-adjusted version of this comparison. Using the 1996 sample, we regress each pre-flood party-rejection outcome on whether the respondent's district was exposed to the 1998 flood. The specifications include individual-level controls and division fixed effects, with standard errors clustered at the district level. The estimated associations between future flood exposure and rejection of the AL, BNP, and JP are small and statistically insignificant. Taken together, the descriptive and regression-based evidence suggests that flooded and non-flooded districts were broadly comparable before the flood, particularly with respect to the main political outcomes studied. Thus, these balance tests support baseline comparability.

5.2. Flood Exposure and Political Realignment

5.2.1. Benchmark Effects on Party Rejection

Table 3 presents our benchmark estimates, where the results indicate that flood exposure produced different responses toward the three principal political parties. In the binary-treatment specification, column (1) shows that residing in a flooded district after the flood is associated with a 23.5 percentage-point increase in the probability of reporting that one would never vote for AL. Column (2) shows that the estimated effect for BNP is larger with a 33.2 percentage-point increase in rejection. Both estimates are statistically significant at the 1 percent level. By contrast, column (3) indicates that flood exposure reduces rejection of JP by 9.4 percentage points, an estimate that is statistically significant at the 5 percent level.

The continuous flood-intensity specifications displayed in columns (4) to (6) yield a similar pattern. Greater flood intensity is positively associated with rejection of both dominant parties. The coefficients are 0.108 for rejection of AL and 0.091 for rejection of BNP, and both are statistically significant. The estimated coefficient on flood intensity for rejection of JP is negative but small and statistically indistinguishable from zero. Thus, while the binary exposure measure suggests that flooded respondents became less hostile toward JP, the intensity results provide stronger evidence for increased rejection of AL and BNP than for a corresponding decline in JP rejection.

In short, the benchmark estimates show that the political response to the flood was not a uniform decline in support for all political parties. Instead, the effects are concentrated among the AL and BNP, the two parties that dominated national politics. At the same time, attitudes toward the JP either improved or remained comparatively stable. This response motivates our next analysis of whether the flood generated a broader partisan realignment away from the dominant parties.

5.2.2. Flood Voting and Dominant-Party Realignment

Table 4 examines this interpretation by combining AL and BNP into a dominant-party category and by considering respondents' first-choice party. The first column summarizes the party-rejection results reported separately in Table 3. Flood exposure increases the combined rejection measure for the two dominant parties by 0.567.

The first-choice outcomes displayed in Columns (2) and (3) provide more direct evidence of political reallocation. Following the flood, respondents in flooded districts are 23.5 percentage points less likely to identify one of the two dominant parties as their first choice. At the same time, they are 44.8 percentage points more likely to select the JP as their preferred party. Both effects are statistically significant, with the Jatiya Party estimate significant at the 1 percent level and the dominant party estimate significant at the 5 percent level. The continuous treatment estimates shown in Columns (4) to (6) point in the same direction.[5]

These findings are especially notable because they suggest that the political penalty is not confined to the incumbent party. Rather than indicating a simple shift away from the party that governed during the flood toward its principal rival, the estimates are consistent with a broader deterioration in evaluations of both dominant parties by 2002. The JP, as the principal alternative outside this two-party structure, emerged as the main beneficiary of

[5]The first-choice outcomes use a larger sample than the party-rejection outcomes because response rates differ across the two survey questions.

this reallocation in stated political preferences. One interpretation is that JP offered a policy-specific alternative rather than only a generic protest option. In Bangladesh, JP was associated with the Ershad period, when flood-control infrastructure and embankment-based policy became especially salient after the 1987 and 1988 floods. Flood-affected respondents may therefore have viewed JP as a party linked to flood protection, rather than only as a residual protest choice. This interpretation is consistent with the first-choice results, but we cannot test it directly with the WVS data. This pattern is therefore more consistent with flood voting, a broader realignment away from the established party duopoly, than with the conventional retrospective-accountability pattern in which punishment of the government benefits its principal electoral rival.[6]

5.3. General Government and Political Confidence

The preceding results indicate that flood exposure shifted political preferences away from the two dominant parties and toward the JP. One possible explanation is that the flood generated a broader loss of confidence in government and political institutions, rather than a change in evaluations of political parties. Table 5 examines this possibility using respondents' confidence in government and political parties as outcomes. The results provide little evidence of such a generalized decline, where flood exposure has no statistically significant effect on either confidence in government or confidence in political parties, regardless of

[6] Appendix Tables A1 and A2 show that the results are robust to the sequential inclusion of individual-level controls. Adding gender, age, marital status, number of children, and education one at a time leaves the estimated effects of both binary flood exposure and flood intensity qualitatively unchanged.

whether these outcomes are measured as binary indicators or four-point confidence scores.[7]

The lack of statistically significant confidence effects is consistent with the shift being concentrated in party-specific evaluations rather than generalized political disillusionment. The party-specific results therefore suggest a reallocation of support across parties rather than clear evidence of disengagement from the political system altogether.

5.4. Relief and Political Backlash

If the political response to the flood reflected the severity of its consequences or the assistance received afterward, the post-flood change in party evaluations may have varied with the amount of government relief provided. Prior research, including Bechtel and Hainmueller (2011) and Cole et al. (2012), shows that post-disaster assistance can shape the political consequences of natural disasters. To examine this possibility, Table 6 augments the benchmark DID specification by interacting the main treatment term with district-level relief provision.[8] The resulting interaction term shows whether the post-flood increase in party rejection among flood-affected districts varies with the amount of Gratuitous Relief received.[9] We estimate

[7]"Don't know" responses are excluded. The original WVS confidence scale is coded from 1 ("a great deal") to 4 ("not at all"). For ease of interpretation, we reverse-code the four-point score so that higher values indicate greater confidence: 4 = "a great deal," 3 = "quite a lot," 2 = "not very much," and 1 = "not at all." The binary confidence measure equals one for respondents reporting "a great deal" or "quite a lot" of confidence and zero for those reporting "not very much" or "not at all".

[8]The relief data are drawn from the 2000 Household Income and Expenditure Survey and aggregated to the district level.

[9]We focus on Gratuitous Relief because it is intended to provide immediate in-kind assistance to households affected by disasters.

this separately using Gratuitous Relief wheat and rice distributed at the district level.[10] A negative interaction coefficient would therefore indicate that greater relief provision is associated with a smaller increase in party rejection following flood exposure.

Columns (1) – (4) of Table 6 show that the interaction terms between post-flood exposure and both wheat and rice relief are negative and statistically significant for rejection of AL and BNP. Greater relief provision is therefore associated with a smaller increase in rejection of both dominant parties among respondents in flood-affected districts. By contrast, the corresponding interaction terms for JP in Columns (5) and (6) are statistically insignificant, providing little evidence that relief provision is systematically associated with attitudes toward the third-party alternative.

This pattern is particularly informative because relief was provided while AL was in government, yet the association is not confined to subsequent evaluations of AL. Rejection of BNP is also lower in flood-affected districts receiving greater relief. Since BNP returned to government following the 2001 election, before political preferences were measured in 2002, this result should not be interpreted as relief generating a direct political reward for either party. Instead, greater relief provision is associated with a broader easing of the subsequent dissatisfaction directed toward the two parties that dominated national politics over this period. One possible interpretation is that relief reduced the extent to which disaster-related hardship translated into broader rejection of the dominant political alternatives, thereby weakening the political realignment away from AL and BNP.

[10] Wheat and rice are examined separately because they are reported as distinct relief categories in the survey. Relief is measured in kilograms.

5.5. Heterogeneity by Flood Severity and Socioeconomic Status

The relief results are consistent with material hardship playing an important role in linking flood exposure to political rejection. Table 7 therefore examines whether the benchmark effects are stronger in more severely affected areas and among respondents with lower socioeconomic status.[11]

Panel A shows that the political response is concentrated most clearly in districts with high flood intensity. In these districts, flood exposure increases rejection of both AL and BNP and reduces rejection of JP. By contrast, the estimates for low-intensity districts are statistically insignificant across all three outcomes. The moderate-intensity group provides more limited evidence of political response, with a significant increase in BNP rejection but no statistically significant change in rejection of AL or JP. Overall, the broad realignment pattern is therefore most pronounced in the most severely affected districts.

Next, Panels B and C reveal a similar socioeconomic pattern. Flood exposure substantially increases rejection of both dominant parties among low- and middle-income respondents, while the corresponding estimates for high-income respondents are small and statistically insignificant. The social-class estimates show the same broad direction, where rejection of both dominant parties rises among lower/working-class and lower-middle-class respondents, while the corresponding estimates are small and statistically insignificant among upper-

[11] Flood-affected districts are divided into three mutually exclusive groups using the 33rd and 66th percentiles of the district-level flood-intensity distribution among treated districts, with identical intensity values kept in the same category. Income is measured on a ten-point scale and grouped into low income (categories 1–3), middle income (categories 4–5), and high income (categories 6–10) to maintain sufficient observations in each subsample. Social class is grouped as lower/working class, lower-middle class, and upper-middle/upper class based on respondents' self-reported WVS class position.

middle/upper-class respondents. Among lower/working-class respondents, flood exposure is also associated with a significant decline in rejection of JP.

Taken together, these findings indicate that the political consequences of the flood were concentrated among respondents who experienced more serious exposure and had fewer resources with which to absorb the shock. This pattern complements the relief results in Table 6. Greater relief is associated with lower rejection of the dominant parties following flood exposure, while the heterogeneity estimates show stronger political responses among more economically vulnerable groups. Both sets of results are therefore consistent with material hardship playing an important role in translating flood exposure into political realignment.

5.6. Robustness and Falsification Checks

In this section, we conduct several robustness and falsification exercises to assess whether the benchmark findings are driven by the composition of the control group, the use of individual-level data, or spurious correlations in treatment assignment.

First, Table 8 addresses a limitation of our empirical setting: because the flood affected a large share of the country and the WVS does not cover every district, our final sample only consists of three non-flooded control districts. To assess whether the results are driven by any one of these controls, we re-estimate the benchmark specification after excluding each control district in turn. As shown in Table 8, the estimated effects remain similar in sign, magnitude, and statistical significance across all leave-one-control-district-out samples. Flood exposure continues to increase rejection of the AL and BNP and reduce rejection of the JP. This exercise reduces the concern that the benchmark findings are driven by the characteristics or political trajectory of a single control district.

Table 9 provides a complementary check by collapsing the data to the district-year level, which corresponds more closely to the level at which treatment is assigned. Each district-year cell receives equal weight, allowing us to examine whether the directional pattern of the benchmark results is preserved when the analysis is conducted at the treatment-assignment level rather than at the individual level. The estimates retain the same directional pattern as the benchmark results, with flood exposure increasing rejection of the AL and BNP, and decreasing rejection of the JP. The BNP and JP effects remain statistically significant, while the AL estimate remains positive but becomes less precise, which is unsurprising given that the collapsed sample contains only 39 district-year observations. Taken together with Table 8, the results show that the directional pattern of the benchmark estimates is preserved after aggregating the analysis to the district-year level and is not driven by any single comparison district.

As an additional check, Appendix Table A3 reports a within-group falsification exercise based on artificial treatment assignments. We separately restrict the sample to respondents in flood-exposed and non-flooded districts, and randomly assign pseudo-treatment status within each subsample. Because respondents within each subsample share the same actual flood-exposure status, the resulting placebo DID estimates should be centered around zero. Across 1,000 simulations, the mean placebo coefficients for rejection of AL, BNP, and JP are close to zero in both subsamples, with no systematic evidence of effects. Taken together, these checks show that the main directional pattern is preserved across alternative samples and levels of aggregation, and is not reproduced under artificial treatment assignment.

6. Conclusion

This paper examines how exposure to a major natural disaster shapes political preferences using the 1998 Bangladesh flood. Combining district-level flood exposure with individual responses from the 1996 and 2002 waves of the WVS, we find that flood exposure is associated with increased rejection of both AL and BNP, the two parties that dominated national politics over this period, while rejection of JP declines. Consistent with this pattern, respondents in flood-exposed areas become less likely to identify either dominant party as their first choice and more likely to support JP. Because the post-flood survey was conducted several years after the disaster and after a change in national government, these estimates should be interpreted as medium-run changes in party evaluations associated with flood exposure.

These findings suggest that flood voting, as observed in this setting, need not simply transfer political support from the government to its principal opponent. Instead, a major shock can weaken both dominant parties and create political space for an alternative force. We find no corresponding decline in broader political confidence, suggesting that the shift is not simply a generalized withdrawal of trust from the political system. Government relief is associated with a smaller increase in rejection of both dominant parties, while the corresponding association for JP is statistically insignificant. The political response is also strongest in the most severely flooded areas and among lower-income and lower-social-status respondents, consistent with material exposure playing an important role in shaping political backlash.

Taken together, the results broaden the conventional view of disaster accountability. The political consequences of natural disasters may depend not only on whether voters reward or punish the incumbent, but also on how dissatisfaction is redistributed across the available political alternatives. In settings where voters become dissatisfied with both the government and its principal rival, disasters may contribute to a broader realignment of party competition. As climate-related shocks become increasingly salient, understanding these wider political responses and the role that effective post-disaster assistance may play in moderating them will be important for understanding the relationship between government performance, political accountability, and the evolution of party systems.

Future research could examine the mechanisms behind flood voting, including how voters assign responsibility for disaster preparedness and response across political actors. One promising extension would test the policy-memory channel directly, for example by linking local changes in JP support to exposure to Ershad-era flood-control projects, embankments, or local knowledge of party responsibility for flood protection. It would also be useful to investigate the political and institutional conditions under which such dissatisfaction benefits an established third force rather than the principal opposition.

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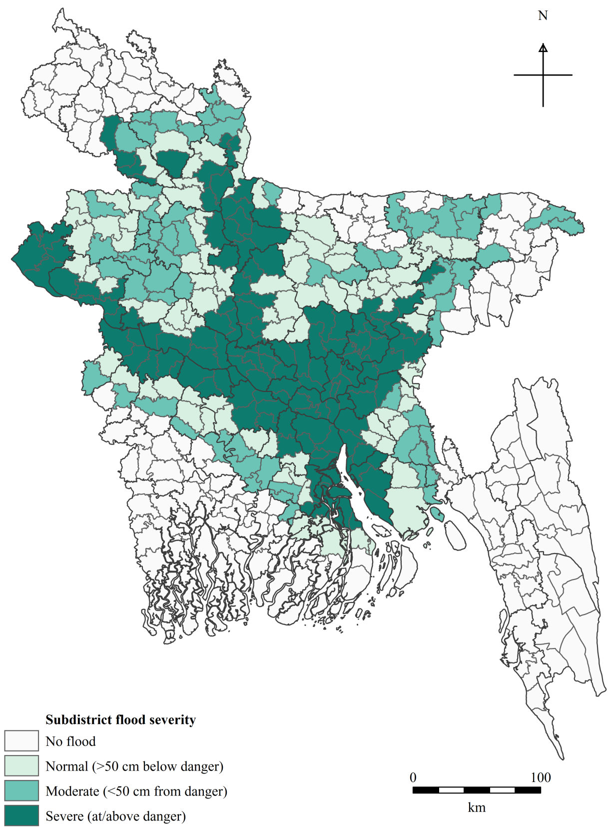

Figure 1: Sub-district level discrete measure of the 1998 Flood Severity of Bangladesh

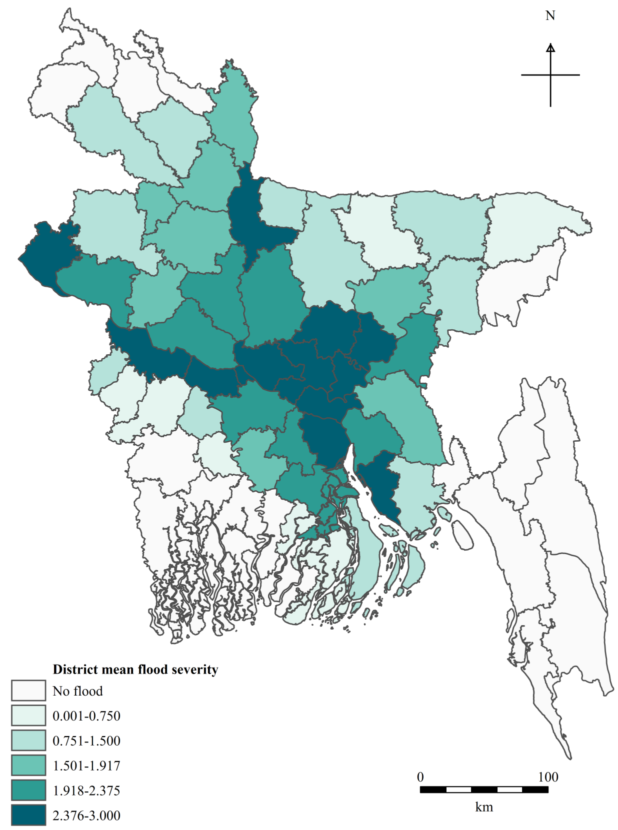

Figure 2: District level continuous measure of the 1998 Flood Severity of Bangladesh

Table 1: Pre-Flood Balance by Flood Exposure, 1996

	Non-flooded mean	Flooded mean	Difference
	(1)	(2)	(3)
Male	0.573	0.581	0.008
Age	36.339	34.307	-2.031**
Married	0.740	0.727	-0.012
Number of children	2.266	2.065	-0.201
Formal Education	1.000	1.000	0.000
Would never vote Awami League	0.062	0.088	0.025
Would never vote BNP	0.104	0.080	-0.024
Would never vote Jatiya Party	0.047	0.048	0.001
Would vote first Awami League	0.625	0.624	-0.001
Would vote first BNP	0.172	0.211	0.039
Would vote first Jatiya Party	0.089	0.099	0.011
Observations: non-flooded	192		
Observations: flooded	774		
Total observations	966		

Note: The sample is restricted to 1996 WVS respondents with non-missing values for all variables shown in the table. Difference is Flooded minus Non-flooded districts. Significance stars refer to tests of equality of group means. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Pre-Flood Outcome Balance OLS Using Flood Exposure

	Reject AL	Reject BNP	Reject JP
	(1)	(2)	(3)
Flooded district	-0.007	0.038	0.001
	(0.014)	(0.036)	(0.011)
Clustered p-value	0.611	0.312	0.949
Individual controls	Yes	Yes	Yes
Division fixed effects	Yes	Yes	Yes
District fixed effects	No	No	No
R^2	0.023	0.019	0.005
Observations	983	983	983

Note: This table tests whether future flood exposure predicts political outcomes before the 1998 flood. The sample is restricted to the 1996 pre-flood survey year and complements Table 1 as a regression-adjusted pre-treatment comparability test. All regressions include division fixed effects and individual controls for age, gender, marital status, number of children, and an indicator for formal education. District fixed effects are not included because flood exposure is defined at the district level and would be absorbed. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Flood Exposure and Party Rejection

	Reject AL	Reject BNP	Reject JP	Reject AL	Reject BNP	Reject JP
	(1)	(2)	(3)	(4)	(5)	(6)
Flooded district x Post	0.235***	0.332***	-0.094**			
	(0.057)	(0.040)	(0.036)			
Flood intensity x Post				0.108***	0.091**	-0.013
				(0.032)	(0.035)	(0.016)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.101	0.081	0.051	0.103	0.076	0.050
Observations	2461	2461	2461	2461	2461	2461

Note: Columns 1–3 use the binary flooded-district treatment; columns 4–6 use continuous flood intensity. All regressions include district fixed effects, division-by-year fixed effects, and individual controls for age, gender, marital status, number of children, and an indicator for formal education. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Flood Exposure and Dominant-Party Realignment

	Reject dominant parties	First choice: dominant party	First choice: Jatiya	Reject dominant parties	First choice: dominant party	First choice: Jatiya
	(1)	(2)	(3)	(4)	(5)	(6)
Flooded district x Post	0.567***	-0.235**	0.448***			
	(0.049)	(0.112)	(0.069)			
Flood intensity x Post				0.199***	-0.081**	0.096*
				(0.051)	(0.034)	(0.052)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.120	0.088	0.124	0.120	0.088	0.114
Observations	2461	2954	2954	2461	2954	2954

Note: Columns 1–3 use the binary flooded-district treatment; columns 4–6 use continuous flood intensity. All regressions include district fixed effects, division-by-year fixed effects, and individual controls for age, gender, marital status, number of children, and an indicator for formal education. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Flood Exposure and General Political Confidence

	Government confidence	Government confidence score	Political party confidence	Political party confidence score
	(1)	(2)	(3)	(4)
Flooded district x Post	-0.048	0.072	-0.182	-0.216
	(0.043)	(0.164)	(0.149)	(0.284)
Individual controls	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes	Yes
R^2	0.114	0.208	0.106	0.117
Observations	2841	2841	2841	2841

Note: Confidence dummy outcomes equal one if the respondent reports confidence in the institution. Confidence score outcomes range from 1 to 4 and are reverse coded so that higher values indicate greater confidence. All regressions include district fixed effects, division-by-year fixed effects, and individual controls for age, gender, marital status, number of children, and an indicator for formal education. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Flood Exposure, Relief, and Party Rejection

	Reject AL	Reject AL	Reject BNP	Reject BNP	Reject JP	Reject JP
	(1)	(2)	(3)	(4)	(5)	(6)
Flooded district x Post	0.240***	0.238***	0.334***	0.333***	-0.094**	-0.094**
	(0.057)	(0.056)	(0.040)	(0.040)	(0.036)	(0.036)
Flooded district x Post x GR wheat	-2.233***		-1.044***		0.013	
	(0.142)		(0.066)		(0.060)	
Flooded district x Post x GR rice		-2.087***		-1.448***		0.444
		(0.725)		(0.362)		(0.411)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.105	0.105	0.083	0.086	0.051	0.051
Observations	2461	2461	2461	2461	2461	2461

Note: GR wheat and GR rice are measured in kilograms. The relief interaction terms capture whether the post-flood change associated with binary flood exposure varies with district-level relief provision. All regressions include district fixed effects, division-by-year fixed effects, and individual controls for age, gender, marital status, number of children, and an indicator for formal education. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneity by Flood Severity and Socioeconomic Status

	Reject AL (1)	Reject BNP (2)	Reject JP (3)
Panel A. Flood intensity			
A1. Low flood intensity			
Flooded district x Post	-0.011	0.106	-0.057
	(0.117)	(0.071)	(0.036)
R^2	0.072	0.038	0.020
Observations	1129	1129	1129
A2. Moderate flood intensity			
Flooded district x Post	-0.070	0.149***	0.041
	(0.051)	(0.042)	(0.028)
R^2	0.140	0.070	0.027
Observations	1054	1054	1054
A3. High flood intensity			
Flooded district x Post	0.220***	0.329***	-0.100*
	(0.061)	(0.040)	(0.046)
R^2	0.094	0.122	0.060
Observations	1080	1080	1080
Panel B. Income group			
B1. Low income			
Flooded district x Post	0.392***	0.502***	-0.112
	(0.129)	(0.136)	(0.174)
R^2	0.194	0.164	0.109
Observations	734	734	734
B2. Middle income			
Flooded district x Post	0.389***	0.300***	-0.075
	(0.050)	(0.028)	(0.113)
R^2	0.149	0.067	0.048
Observations	914	914	914
B3. High income			
Flooded district x Post	0.048	0.082	0.027
	(0.073)	(0.054)	(0.031)
R^2	0.127	0.098	0.063
Observations	751	751	751
Panel C. Social class			
C1. Lower/working class			
Flooded district x Post	0.393***	0.453***	-0.183**
	(0.127)	(0.133)	(0.087)
R^2	0.178	0.229	0.110
Observations	571	571	571
C2. Lower-middle class			
Flooded district x Post	0.418***	0.267***	-0.066
	(0.015)	(0.050)	(0.103)
R^2	0.150	0.077	0.049
Observations	1172	1172	1172
C3. Upper-middle/upper class			
Flooded district x Post	-0.072	0.105	-0.020
	(0.060)	(0.062)	(0.021)
R^2	0.112	0.070	0.044
Observations	682	682	682
Individual controls	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes

Note: Panel A divides flood-affected districts into three mutually exclusive groups using the 33rd and 66th percentiles of the district-level flood-intensity distribution among treated districts. Each flood-intensity group is compared separately with the non-flooded districts. Panel B splits respondents by income group. Panel C groups respondents into lower/working, lower-middle, and upper-middle/upper social classes. All regressions use the binary flood-exposure treatment and include individual controls for age, gender, marital status, number of children, and an indicator for formal education, district fixed effects, and division-by-year fixed effects. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Leave-One-Control-District-Out Robustness

	Reject AL (1)	Reject BNP (2)	Reject JP (3)
Panel A. Drop Control District 1			
Flooded district x Post	0.248***	0.352***	-0.110**
	(0.071)	(0.055)	(0.042)
Clustered p-value	0.002	0.000	0.017
R^2	0.105	0.082	0.055
Observations	2312	2312	2312
Panel B. Drop Control District 2			
Flooded district x Post	0.271***	0.293***	-0.112***
	(0.051)	(0.016)	(0.037)
Clustered p-value	0.000	0.000	0.007
R^2	0.100	0.081	0.052
Observations	2387	2387	2387
Panel C. Drop Control District 3			
Flooded district x Post	0.174***	0.359***	-0.047***
	(0.046)	(0.062)	(0.015)
Clustered p-value	0.001	0.000	0.005
R^2	0.103	0.086	0.052
Observations	2283	2283	2283
Individual controls	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes

Note: Each panel re-estimates the benchmark DID specification from Table 3 after dropping one of the three non-flooded control districts. All regressions include district fixed effects, division-by-year fixed effects, and individual controls for age, gender, marital status, number of children, and an indicator for formal education. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: District-Year Collapsed DID

	Reject AL	Reject BNP	Reject JP
	(1)	(2)	(3)
Flooded district x Post	0.882	0.486**	-0.251**
	(0.518)	(0.221)	(0.115)
Controls	District-year means	District-year means	District-year means
District fixed effects	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes
R^2	0.897	0.941	0.913
District-year observations	39	39	39

Note: The data are collapsed to district-year means before estimation, and each district-year cell receives equal weight. The control variables are district-year means of age, gender, marital status, number of children, and an indicator for formal education. All regressions include district fixed effects and division-by-year fixed effects. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A1: Sequential Addition of Controls: Binary Flood Exposure

	No controls (1)	+ Male (2)	+ Age (3)	+ Married (4)	+ Children (5)	+ Education (6)
Panel A. Reject AL						
Flooded district x Post	0.214***	0.216***	0.228***	0.227***	0.220***	0.235***
	(0.046)	(0.047)	(0.046)	(0.045)	(0.046)	(0.057)
R^2	0.094	0.095	0.099	0.099	0.100	0.101
Observations	2461	2461	2461	2461	2461	2461
Panel B. Reject BNP						
Flooded district x Post	0.336***	0.336***	0.337***	0.341***	0.347***	0.332***
	(0.037)	(0.037)	(0.037)	(0.038)	(0.038)	(0.040)
R^2	0.078	0.078	0.078	0.079	0.080	0.081
Observations	2461	2461	2461	2461	2461	2461
Panel C. Reject JP						
Flooded district x Post	-0.072**	-0.074**	-0.077**	-0.078**	-0.081**	-0.094**
	(0.030)	(0.032)	(0.033)	(0.032)	(0.032)	(0.036)
R^2	0.048	0.049	0.049	0.049	0.050	0.051
Observations	2461	2461	2461	2461	2461	2461
Male control	No	Yes	Yes	Yes	Yes	Yes
Age control	No	No	Yes	Yes	Yes	Yes
Marriage control	No	No	No	Yes	Yes	Yes
Children control	No	No	No	No	Yes	Yes
Education control	No	No	No	No	No	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table adds individual controls cumulatively to the benchmark DID specification. The individual controls are a male indicator, age, marital status, number of children, and an indicator for formal education, as indicated by the rows at the bottom of the table. All columns include district fixed effects and division-by-year fixed effects. The sample is restricted to respondents with non-missing values for all benchmark controls. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Sequential Addition of Controls: Continuous Flood Intensity

	No controls (1)	+ Male (2)	+ Age (3)	+ Married (4)	+ Children (5)	+ Education (6)
Panel A. Reject AL						
Flood intensity x Post	0.100***	0.101***	0.104***	0.103***	0.102***	0.108***
	(0.032)	(0.032)	(0.031)	(0.031)	(0.031)	(0.032)
R^2	0.096	0.097	0.101	0.101	0.102	0.103
Observations	2461	2461	2461	2461	2461	2461
Panel B. Reject BNP						
Flood intensity x Post	0.096**	0.096**	0.096**	0.096**	0.097**	0.091**
	(0.037)	(0.037)	(0.037)	(0.038)	(0.039)	(0.035)
R^2	0.073	0.073	0.073	0.073	0.074	0.076
Observations	2461	2461	2461	2461	2461	2461
Panel C. Reject JP						
Flood intensity x Post	-0.008	-0.009	-0.009	-0.009	-0.010	-0.013
	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.016)
R^2	0.047	0.048	0.049	0.049	0.049	0.050
Observations	2461	2461	2461	2461	2461	2461
Male control	No	Yes	Yes	Yes	Yes	Yes
Age control	No	No	Yes	Yes	Yes	Yes
Marriage control	No	No	No	Yes	Yes	Yes
Children control	No	No	No	No	Yes	Yes
Education control	No	No	No	No	No	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Division-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table adds individual controls cumulatively to the benchmark continuous-intensity DID specification. The individual controls are a male indicator, age, marital status, number of children, and an indicator for formal education, as indicated by the rows at the bottom of the table. All columns include district fixed effects and division-by-year fixed effects. The sample is restricted to respondents with non-missing values for all benchmark controls. Standard errors in parentheses are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Within-Group Falsification Test

	Exposed: Reject AL	Exposed: Reject BNP	Exposed: Reject JP	Unexposed: Reject AL	Unexposed: Reject BNP	Unexposed: Reject JP
	(1)	(2)	(3)	(4)	(5)	(6)
Mean placebo coefficient	-0.001	-0.001	0.001	0.004	0.004	-0.003
Share significant at 5%	0.050	0.045	0.046	0.078	0.040	0.043
Valid simulations	1000	1000	1000	1000	1000	1000

Note: Each column summarizes 1000 simulated regressions. Reported coefficients are mean placebo coefficients across simulations; no statistical significance test is applied to the mean coefficient itself. Pseudo treatment is randomly assigned to respondents using the observed district treatment distribution, and regressions are run separately within actually exposed and actually unexposed districts. Each simulated regression includes district fixed effects, division-by-year fixed effects, and individual controls for age, gender, marital status, number of children, and an indicator for formal education. Standard errors in each simulated regression are clustered at the district level.