Natural Disasters and Political Engagement: Evidence from the 2010-11 Pakistani Floods*

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Abstract

How natural disasters affect politics in developing countries is an important question given the fragility of fledgling democratic institutions in some of these countries as well as likely increased exposure to natural disasters over time due to climate change. Research in sociology and psychology suggests traumatic events can inspire pro-social behavior and therefore might increase political engagement. Research in political science argues that economic resources are critical for political engagement and thus the economic dislocation from disasters may dampen participation. We argue that when the government and civil society response effectively blunts a disaster's economic impacts, then political engagement may increase as citizens learn about government capacity. Using diverse data from the massive 2010-11 Pakistan floods, we find that Pakistanis in highly flood-affected areas turned out to vote at substantially higher rates three years later than those less exposed. We also provide speculative evidence on the mechanism. The increase in turnout was higher in areas with lower *ex ante* flood risk, which is consistent with a learning process. These results suggest that natural disasters may not necessarily undermine civil society in emerging developing democracies. (JEL: A12, D72, D74, I28, O17)

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How do natural disasters affect politics in developing countries? Addressing this question is important given the fragility of fledgling democratic institutions in some of these countries as well as likely increased exposure to natural disasters over time due to climate change (Intergovernmental Panel on Climate Change 2013). The existing social science literature makes contradictory predictions. On the one hand, research from sociology and psychology suggests that traumatic events such as natural disasters can inspire pro-social behavior and therefore might increase political engagement (e.g., Bardo, 1978; Bolin and Stanford, 1998; Rodriguez, Trainor and Quarantelli, 2006; Toya and Skidmore, 2014). If this is the case, then disasters might enhance the quality of government by increasing accountability pressures and selecting for a higher-quality political class (e.g., Putnam, Leonardi and Nanetti, 1994; Besley, 2007). On the other hand, political scientists have argued that economic resources are critical ingredients for civic engagement (e.g., Verba, Schlozman and Brady, 1995). Kosec and Mo (2015) find that economic shocks resulting from natural disasters can reduce citizen aspiration levels, which are positively correlated with various forms of civic engagement. Disasters may therefore dampen participation. Moreover, scholars from a range of disciplines have suggested that economic shocks create opportunities for violent non-state actors to appeal to citizens (e.g., Collier and Hoeffler, 2004; Dal Bo and Dal Bo, 2011; Dube and Vargas, 2013), which may also discourage citizens from working within democratic political channels (United States Agency for International Development, 2011; Hendirx and Salehyan, 2012).

We argue that when the government and civil society response effectively blunts a disaster's economic impacts, mass political engagement should increase.[1] Conditional on economic harms being mitigated, at least three theoretical pathways suggest that natural disasters should increase political participation.

First, natural disasters lead to the grassroots creation of self-help organizations in many societies (e.g., Hawkins and Maurer, 2010; Yamamura, 2010). In many places such civic associations help train citizens in basic functions of self-governance as well as reveal the positive outputs from collective action, both features that should be positively correlated with political engagement (e.g.,

[1] Our argument is similar to that of Kosec and Mo (2015), who find that flood relief from the government can mitigate the negative effects of economic shocks on aspiration levels. They also study the political effects of the 2010 Pakistani floods. This study is distinct in that our dependent variable is turnout whereas the dependent variable in Kosec and Mo (2015) is aspiration level. Kosec and Mo (2015) show that aspirations are politically meaningful because they are positively correlated with reports of *past* turnout before the floods. They do not examine how disasters affect *future* turnout (i.e., after the floods).

Putnam, Leonardi and Nanetti, 1994; Banks, 1997). In addition, an extensive psychological literature has argued that natural disasters encourage altruistic and pro-social behavior such as search and rescue or providing food and shelter for victims (e.g., Bardo, 1978; Bolin and Stanford, 1998; Levine and Thompson, 2004; Rodriguez, Trainor and Quarantelli, 2006; Vollhardt, 2009; Toya and Skidmore, 2014).[2] In models of voting, where turnout is driven in part by concerns with the welfare of other citizens, such changes would be expected to increase participation (e.g., Myatt, 2015).

Second, natural disasters appear to be positively correlated with some indicators of social capital (e.g., Yamamura, 2016). Critically, the relationship appears to depend on the efficacy of government response. The correlation between self-reported damage from earthquakes and self-expressed interpersonal trust in Latin America, for example, is strongly negative in places where governments respond poorly to natural disasters (as judged by researchers), but the correlation reverses sign among those who feel the government response was effective (Carlin, Love and Zechmeister, 2014). And even though the impact of social capital broadly defined on political participation is contested (Atkinson and Fowler, 2012, e.g.,), the majority of the literature expects it to be positive.

Third, personal exposure to natural disasters may make salient the importance of government action and policies that ameliorate economic harm. This, in turn, might make citizens more engaged with the voting process given a better understanding of the stakes of democratic politics (e.g., Jackman, 1987; Hajnal and Lewis, 2003; Pacek, Pop-Eleches and Tucker, 2009). Recent models of voting suggest that turnout should be increasing in the extent to which citizens think the choice of government matters for future welfare (Myatt, 2015).

Taken as a whole these three mechanisms suggest that if the government and civil society response to a disaster is sufficiently effective to blunt its economic impacts, and therefore counteract the potential negative effects described above, then natural disasters should (a) increase political participation, and (b) increase engagement by stimulating citizen learning.

We explore these hypotheses in the context of Pakistan, an extremely important country of study for practical and epistemological reasons. On the practical side, Pakistan is of immense geopolitical relevance. Understanding the drivers of its politics is thus important in its own right. On the epistemic side, previous research in this area has focused on advanced economies such as the

[2] Rodriguez, Trainor and Quarantelli (2006), for example, conducted extensive field research in the aftermath of Hurricane Katrina in New Orleans in 2005 and found that instances of pro-social behavior greatly outnumbered instances of behavior.

United States (e.g., Achen and Bartels, 2004; Healy and Malhotra, 2010; Gasper and Reeves, 2011; Sinclair, Hall and Alvarez, 2011) and Germany (Bechtel and Hainmueller, 2011). In the closest U.S.-based study to ours Sinclair, Hall and Alvarez (2011) show that registered voters in New Orleans who experienced large-scale flooding were more likely to participate in the following year's mayoral election due to receiving more political information from politicians and interest groups than less-affected citizens. Few studies have departed from the Organization of Economic Co-operation and Development (OECD) context. They examined relatively well-established democracies (e.g., Cole, Healy and Werker, 2011 on India and Gallego, 2012 on Columbia) and explore vote choice—not participation—as the focal dependent variable.

Specifically, we examine the 2010-11 floods in Pakistan. The 2010 flood affected more than 20 million people, caused between 1,800 and 2,000 deaths, and damaged or destroyed approximately 1.7 million houses, making it the worst flood in Pakistan's modern history.[3] The 2010 floods were driven by an unusual monsoon storm that dropped historically unprecedented levels of moisture on the mountainous northwest regions of the country. Khyber Pakhtunkhwa (KPK) province, for example, received 12 feet of rain from July 28 to August 3, four times its average annual total (Gronewold, 2010). Those exceptionally high rainfall rates triggered flash floods that vastly exceeded anything in historical memory. As the water drained from KPK during the first week of August, a more typical monsoon storm inundated the Indus flood plain, rendering it incapable of absorbing the dramatic inflows from the mountainous regions and overwhelming water management structures. The following year Pakistan was again hit by an unusually strong monsoon, causing another round of devastating floods in the southern plains. In both cases the surging waters hit some places more than others due to the unpredictable combination of human action, prior differences in soil moisture, micro-topographic differences, and complex fluid dynamics.

Leveraging that plausibly exogenous variation along with diverse data sources—multiple measures of *ex ante* flood risk, geospatial flood measures, an original survey of 13,282 households conducted in January-March 2012, and constituency-level voting results from the last three national elections (2002, 2008, and 2013)—we show that Pakistanis living in flood-affected places had substantially higher levels of political participation than their unaffected peers. They turned

[3]The EM-DAT International Disaster Database records approximately 20.4 million people affected and 1,985 killed from the 2010 floods.

out to vote at higher rates in the 2013 general elections and exhibited a greater increase in electoral participation relative to the last election (2008). These effects are substantively large. Our estimates suggest that moving from no flooding to the median level of flooding among affected constituencies (7.9% of the population affected) would lead to a 0.5 percentage point increase in turnout. Moving from the median to the 90^{th} percentile in flooding (42% of the population affected) would lead to a 2.2 percentage point increase in turnout. These effects are in line with those observed in door-to-door get-out-the-vote campaigns in the United States and therefore substantively meaningful.

Because of the limited area affected by the floods, the overall impact of the flooding was small. Once past political competition is accounted for, approximately 4% of the 11 percentage point increase in turnout between the 2008 and 2013 general elections (i.e., from 44% to 55%) can be attributed to the impact of the 2010-11 floods. While the floods were unlikely to have changed the election result, they clearly shifted the behavior of those who were heavily affected. For districts above the 75^{th} percentile of flooding (22% of the population affected), for example, the impact of the floods accounted for a 2.5 percentage point increase in turnout, roughly 17% of the increase in turnout in these areas.

Some speculative evidence supports the hypothesis that flood exposure increased participation via citizen learning. The effect of flood damage on turnout is greatest in areas with the lowest *ex ante* flood risk, which are precisely those places where citizens relatively unfamiliar with floods would learn most about the importance of government action. We also rule out three alternative mechanisms: (1) differential trends in urban areas as part of the democratization process in Pakistan; (2) the floods merely changed the composition of the electorate due to disaster-induced migration; and (3) incumbents simply engaged in turnout buying.

We make several scholarly contributions. First, we investigate the political economy of natural disasters in a country outside the developed world, one with fragile democratic institutions and which is of significant strategic and policy importance. Second, we introduce a new set of mechanisms to the study natural disasters' political consequences. Previous research focuses on two channels. The economic channel, which has primarily been studied in non-democracies, argues that natural disasters create economic shocks which decrease the opportunity cost of rebellion for citizens (and therefore increase the likelihood of rebellion), thereby increasing the responsiveness of the state to citizen demands (e.g., Acemoglu and Robinson, 2001; Besley and Persson, 2010, 2011; Brückner and Ciccone, 2011). The political channel, mainly investigated in established democracies, argues that natural disasters provide a strong signal of a government's type, giving citizens the opportunity to exercise electoral accountability (e.g., Healy and Malhotra, 2010; Bechtel and Hainmueller, 2011; Cole, Healy and Werker, 2011; Gasper and Reeves, 2011). Neither body of literature considers the possibility that the experience of the disaster and associated response may have direct effects on political participation. Third, we provide a clear example of why using natural disasters to instrument for economic shocks can be a problematic empirical strategy for outcomes influenced by politics. Natural disasters are not pure economic shocks; these events change many features of the political environment, and the extent to which they do so can be influenced by government action.

The Pakistani Floods 2010-2011: A Major Natural Disaster with Relatively Good Response

As the flooding in Pakistan began in July 2010 almost all observers expected the disaster to take a massive human and political toll. The scale of the 2010 floods dwarfed any Pakistani natural disaster in recent memory, affecting more than 20 million people (about 11% of the total population), temporarily displacing more than 10 million people, and killing at least 1,879, with the 2011 floods affecting another 5 million, displacing another 660,000 people, and killing at least 505 more (Dartmouth Flood Observatory (DFO), 2013; Center for Research on the Epidemiology of Disasters (CRED), 2013). A Fall-2010 survey of 1,769 households in 29 severely flood-affected districts found that 54.8% of households reported damage to their homes, 77% reported at least one household member with health problems, and 88% reported a significant reduction in household income (Kirsch et al., 2012). Figure 1 shows the combined maximal extent of the 2010 and 2011 floods.

INSERT FIGURE 1 HERE

While the scale of the disaster was unprecedented, it was not nearly as bad as many observers predicted at the time. By mid-August the death toll from the floods was estimated to be about 1,600 people. An editorial in *The Economist* on August 21 expected that number to increase dramatically, arguing that "it is no more than the plain truth that the worst is yet to come—in terms of hunger, disease, looting and violence as victims scramble to save themselves and their families." Journalists worried that the unfolding disaster would be a boon for militant organizations.[4] Typical headlines at the time described a situation in which militants could step in and win loyalty by providing badly needed services:

- "Militant groups have 3000 volunteers working around the country." *Christian Science Monitor*, August 6.
- "Pakistani flood disaster gives opening to militants." *Los Angeles Times*, August 10.
- "'Hardline groups step in to fill Pakistan aid vacuum." *BBC News*, August 10.
- "Race to provide aid emerges between West and extremists." *Der Spiegel*, August 16.
- "Pakistan's floods: a window of opportunity for insurgents?" *ABC News*, September 8.

Yet none of that came to pass because, as described in detail below, there was an extremely effective response by government and civil society. The floods also did not have any substantial impact on support for militancy, which many expected and which would have indicated a negative effect on engagement with politics through traditional channels.[5] Though the death toll increased by 20% from mid-August onward, there were no large-scale disease outbreaks, and there was little looting or violence. When the UN Environmental Program modeled risks from a 1 in 100 year flood in Pakistan using historical worldwide data, they predicted mortality four times greater than was actually observed (Maskrey, 2011, 30).

Background on the Floods

The 2010 disaster was a significant outlier in Pakistan's flood history. Figure 2 shows standardized values for the number affected, displaced, and killed for floods over the past few decades. The upper part of the figure presents data from the International Disaster Database (EM-DAT) hosted

[4]Militant organizations played to this concern, with a Tehrik-i-Taliban Pakistan (TTP) spokesman famously offering to contribute $20 million to the relief effort if the Pakistani government would eschew any Western aid (Associated Press of Pakistan (APP), 2010).

[5]Using an endorsement experiment we find no evidence that the floods led to increased support for militancy. Support for militants in 2012 was actually somewhat lower in heavily flooded areas controlling for a range of geographic and demographic variables, though the results are only modestly statistically significant. Detailed results available from authors.

by the Center for Research on the Epidemiology of Disasters (2013) (data range 1975-2012) and the lower graph draws on data from the Global Active Archive of Large Flood Events of the Dartmouth Flood Observatory (DFO) (2013) (data range 1988-2012). In terms of the number affected and the number temporarily displaced, the 2010 floods were the largest in the modern history of Pakistan by several orders of magnitude, and almost twice as devastating as the next largest flood according to the EM-DAT.[6]

INSERT FIGURE 2 HERE

Commensurate with the devastation, the 2010-11 floods also led to an unprecedented reaction by the central, provincial, and district governments as well as by Pakistani civil society and the international community (Ahmed, 2010). Pakistan's Economic Affairs Division took the overall lead on donor coordination, while Pakistan's National Disaster Management Agency (NDMA) directed and coordinated the various relief efforts. The NDMA maintained close working relationships with relevant federal ministries and departments, Pakistan's armed forces,[7] and donor organizations supporting the relief efforts to ensure that resources were mobilized consistent with local needs. At the provincial level, the chief minister of each province was responsible for making sure that various line ministries and the Provincial Disaster Management Authorities acted in concert with each other and with the international and domestic relief efforts (Office for the Coordination of Humanitarian Affairs (OCHA), 2010; National Disaster Management Agency, 2011).

In response to the Pakistani government's appeal to international donors for help in responding to the disaster, the United Nations launched its relief efforts calling for $460 million to provide immediate assistance such as food, shelter, and clean water. Countries and international organizations from around the world donated money and supplies, sent specialists, and provided equipment to supplement the Pakistani government's relief efforts. According to the United Nations Office for the Coordination of Humanitarian Affairs (UNOCHA) (2010), by November 2010, a total of close to $1.792 billion had been committed in humanitarian support, the largest amount by the United States (30.7%), followed by private individuals and organizations (17.5%) and Saudi Arabia

[6] The next largest was the 1992 flood which affected 12.8 million, temporarily displaced 4.3 million, and killed at least 1,446 people.

[7] The military deployed troops in all affected areas together with 21 helicopters and 150 boats (Khan and Mughal, 2010).

(13.5%).[8]

In addition, spontaneous, localized self-help efforts emerged during the initial phase of the crisis and continued throughout. These included victims' and their kin's own efforts to save their belongings as well as survivor-led repairs of local access roads and bridges after the floods receded. This was in addition to an enormous civil society response that tended to spontaneously coalesce at very local levels (mohallas, union councils, villages, etc.). Such local groups collected and distributed truckloads of relief items. Along with non-governmental organizations they set up collection sites for donations of goods and cash and then distributed the collected resources. Individual philanthropists, professional bodies, and even chambers of commerce donated money and supplies to the victims. Scholars associated with Pakistan's Sustainable Development Policy Institute note the importance of these local forms of assistance, but contend that they are virtually unknown (and thus poorly documented) beyond the local level (Shahbaz et al., 2012). Such volunteerism was not unique to the 2010 flood; rather, it is a common feature in Pakistan's domestic response to major disasters. Halvorson and Hamilton (2010), for example, document extremely high levels of volunteerism following the 2005 Kashmir earthquake.[9]

Together, the government's and civil society's effort and the massive influx of foreign aid was quite effective compared to responses to previous natural disasters. The ratio of people killed to 1,000 people affected from the EM-DAT data, and the ratio of deaths to 1,000 people displaced for each flood between 1975 and 2012 from the DFO data, provide proxies for the effectiveness of the government's response. For the 2010 flood, the ratios are 0.10 and 0.19, respectively, which is the smallest ratio in the DFO series (1988-2012) and the seventh smallest in the EM-DAT series (1975-2012).[10] Strikingly, the 2010 ratio is only 21% of the median ratio of killed to 1,000 displaced in the DFO data, so roughly one fifth as many people died as would have been expected given the median response in the last 37 years. Overall, the government's performance in handling the immediate challenge from the 2010 floods appears to have been quite good, implying that death and temporary migration cannot account for the large changes in political participation reported

[8] By April 2013, this total had increased to more than $2.653 billion with the three largest donor groups being the United States (25.8%), private individuals and organizations (13.4%), and Japan (11.3%) (UNOCHA, 2013).

[9] It is also not unique to Pakistan. Scholars have documented similar behavior elsewhere in South Asia (e.g., Haque, 2004; Rahman, 2006; Ghosh, 2009).

[10] Compared to the 1992 flood, the only flood of comparable magnitude in the last 30 years, the 2010 ratio is 72% smaller in the DFO data and 8% smaller in the EM-DAT data.

here.[11]

As would be expected given the qualitative discussion above, we find no quantitative evidence that flood-affected areas suffered medium-term negative economic impacts relative to other areas. Using the nationally representative survey detailed in Appendix C we found that flood exposure had no impact on self-reported income or expenditures in 2012 and only a small negative effect on household assets, with that effect concentrated in farming households.[12] Moreover, using nightlight satellite imagery and micro-data from two waves of the Punjab Multiple Indicator Cluster Survey (MICS), one before and one after the floods, we find little evidence that the 2010-11 floods led to differential economic changes across the flood gradient.[13]

Data Sources and Measurements

We leverage three data sources in our analysis: (1) geocoded data on the floods; (2) constituency level results from the 2002, 2008, and 2013 National and Provincial Assembly elections; and (3) a range of geo-spatial variables that predict *ex ante* flood risk. Appendix C describes the original survey of Pakistan we conducted in early 2012 which is referenced in the text but not part of the main analyses presented here. Summary statistics of all variables for each data set are provided in Appendix Table A.1.

Data Sources

Geocoded Flood Data

Geo-spatial data on the 2010 and 2011 floods come from the United Nations Institute for Training and Research's (UNITAR) Operational Satellite Applications Program (UNOSAT) (United Nations Institute for Training and Research, 2003). These data combine multiple different sources and are the most precise data we know of on those floods, providing estimates of flood extent at 100m$\times$100m resolution. We overlapped various UNOSAT images to generate a layer of maximal flood extent in 2010, 2011, and 2012.

[11] For comparison, Hurricane Katrina killed 1,833 people in the Gulf Coast in 2005 even though many fewer people were directly affected (approximately 500,000 according to the EM-DAT database).

[12] Results available from authors.

[13] Results available from authors.

Electoral Data

We collected constituency-level voting data published by the Election Commission of Pakistan (ECP) from the 2002, 2008, and 2013 National Assembly (NA) and Provincial Assembly (PA) elections. Both assemblies consist of members elected in single-member first-past-the-post elections at the constituency level (272 for the NA and 577 for the PA in the four main provinces) with a number of seats reserved for women and minorities (70 in the NA) that are allocated among parties according to a proportional representation scheme. Most candidates align with a party during the campaign, and those affiliations are recorded in the voting data, but some run as independents and affiliate with a party for coalition formation purposes after the election is complete. Candidates in the 2013 election campaigns combined appeals to national issues and party platforms with locality-specific appeals and promises of patronage, with the mix varying by candidate. The candidates' 2013 appeals are commonly understood to have been more focused on national policies than in previous elections.

For each constituency we recorded the number of registered voters, total number of votes cast, total number of valid votes cast, and the number of votes received by each candidate on the ballot. In the analysis below we focus on PA constituencies which are substantially smaller than NA constituencies and therefore entail less aggregation of our flood data. All core results are substantively similar at the NA level, though less precisely estimated in some cases.

Treatment Variable: Flood Exposure

We measure flood exposure with objective measures based on geo-spatial data. Figure 1 shows the combined maximum flood extents in 2010 and 2011 overlaid on a map of Pakistan with the 216 tehsils in which we surveyed highlighted in grey.[14]

Using the 2010 Landscan gridded (5km $\times$ 5km resolution) population data (Oak Ridge National Laboratory, 2008), we calculate the percent of the population exposed to the floods for each of the 409 tehsils and each of the 577 single-member PA constituencies.[15] The UNOSAT data underestimate the floods' impacts in steep areas where the flood waters did not spread out enough to

[14]The tehsil is the third level administrative unit in Pakistan, below provinces and districts.

[15]We also calculated the percent of area flooded for each of the geographic units. The two measures are highly correlated ($r = 0.85$) and all reported results are qualitatively similar using the area-based instead of the population-based measure.

be identified with overhead imagery but where contemporaneous accounts clearly show there was major damage at the bottom of river valleys. In Upper Dir district in KPK, for example, the UN-OSAT data show no flooding. Contemporaneous media accounts and survey-based measurements, however, clearly indicate the floods did a great deal of damage to structures that were placed well above the normal high-water mark but still very close to rivers (e.g., Agency for Technical Cooperation and Development, 2010). If the floods had an impact on citizen attitudes and behavior, as we hypothesize, then this kind of measurement error will attenuate our estimate of flood impacts because we are counting places as having low values on the treatment where the floods actually had substantial effects.

Outcome Variables: Political Behavior

Based on the constituency-level electoral returns, we construct two measures of political behavior: turnout and candidate vote shares. Turnout is defined as the proportion of total votes cast out of all registered voters in a constituency. All results are robust to measuring turnout as only based on valid votes cast out of all registered voters in a constituency. Candidate vote shares are calculated by dividing the number of votes for each candidate by the number of valid votes in the constituency.

Control Variables

In addition to the regression-specific controls highlighted in the following section, we include two main groups of control variables in all specifications: a measure of *ex ante* flood risk and geographic controls.

Ex Ante Flood Risk

We use risk data developed by the UN Environmental Program (UNEP) to measure *ex ante* flood risk. These data estimate risk based on hydrological models combined with data on historical disasters, ground cover, rainfall, soil type, and topography. The UNEP data estimate flood risk on a 0 (low) to 5 (extreme) scale for 10km$\times$10km grid cells worldwide.[16] Since these grid cells are too large to nest neatly within PA constituency boundaries we estimate the area-weighted flood risk

[16]For details on the methodology see Herold and Mouton (2011).

for each constituency. If 50% of the area of a constituency was in a 0 cell and 50% was in a 1 cell, then the constituency would receive a value of .5 on for *ex ante* flood risk, and so on.

Geographic Controls

In addition to the *ex ante* flood risk, we include in all our regressions the following four control variables for each geographic unit: distance from the unit centroid to the nearest major river, an indicator for units bordering a major river, the mean elevation, and the standard deviation of a unit's standard deviation. Major rivers include the Indus and its main arms (i.e., Chenab, Jhelum, Kabul, Ravi, and Sutlij). Because there was significant flooding in 2012 in a few areas though not nearly so extensive at in 2010-11, less than 10% of constituencies saw any meaningful flooding in 2012, we also control from flooding in 2012 in all specifications.

Empirical Strategy

Identification

Our identification strategy for assessing flood impacts throughout the paper relies on the observation that whether and how much any individual or region was affected by the floods had a large random component due to topographical factors, levy breaks, and strategic dam destructions which had unpredictable consequences on subsequent flows (e.g., Waraich, 2010). Once we control for observables that citizens could have used to predict flood exposure, and thus may have impacted economic outcomes or settlement patterns—risk estimates based on topography and hydrology, distance to major rivers, elevation, and steepness of terrain—the remaining variance in flooding should be conditionally independent of other factors influencing the outcomes we study.

The correlation between *ex ante* risk and flooding in 2010-11 is modest at best (see Figure 3, which plots average flood risk against observed exposure in 2010-11). Each column reports a different level of geographic aggregation: tehsils, NA constituencies, and PA constituencies. The top row shows exposure measured in terms of proportion of area exposed and the bottom row shows the proportion of the population exposed. Across all six scatter plots it is clear that there is tremendous variance in flood exposure at all but the lowest levels of flood risk. Only 10-12% of the variance in the proportion of the population exposed in 2010-11 could have been predicted with a cubic polynomial model of *ex ante* flood risk.

INSERT FIGURE 3 HERE

Estimation Strategy

Our estimation strategy at the constituency level is inspired by two observations. First, conditional on a combination of regional fixed-effects and constituency-level geographic controls, we can isolate the impact of local variation in flood intensity on electoral turnout. In doing so, we need to control for a range of locality-specific confounders. We might worry, for example, that it is easier for politicians to deliver patronage to constituencies close to rivers (which are most likely to be flooded) through a combination of water management projects and prior flood relief, making them more likely to turn out. To avoid confounding flood exposure with fixed characteristics of constituencies we control for a range of geographic factors. We also show that our results are consistent within subsets that are more similar in proximity to rivers.

Second, controlling for past turnout at the locality level can help estimate the impact of events on voting. In developed democracies individual turnout decisions are highly consistent from election to election (Fowler, 2006; Denny and Doyle, 2008), and past turnout in an area is an excellent predictor of future turnout in that area (Fujiwara, Meng and Vogl, 2013). Given that fact, we follow the logic of Gerber and Green (2000) and Gerber, Green and Larimer (2008), who use lagged dependent variable models to improve precision in their estimates of the marginal impact of exogenous events. In their studies, the exogenous events were experimental treatments. In our case, it is the variance in flooding conditional on *ex ante* flood risk and our geographic controls.

In Pakistan, individual turnout decisions are likely not as consistent as in more developed polities, so there is no obvious right way to execute this strategy. In particular, the 2002 and 2008 elections were held under very different circumstances with different configurations of parties. The 2002 election was held to transition out of a military dictatorship and a number of prominent politicians, including the current prime minister, were bared from running. The 2008 election was the first completely democratic contest, but the results were heavily influenced by the assassination of Benazir Bhutto, the leader of the Pakistan People's Party (PPP), two weeks before the originally planned vote, and by the subsequent delay to allow the PPP to choose another leader. Since the array of parties in the 2002 and 2008 elections were not obviously comparable to those in the 2013 election we show all results three ways: (a) without controls for previous voting; (b) with controls for trends from 2002 to 2008; and (c) with controls for levels in 2002 and 2008. Note that controlling for trends imposes the condition that the relationship between voting in 2008 and voting in 2013 is symmetric with the relationship between voting in 2002 and voting in 2013. It is therefore a less flexible specification than controlling for levels in both elections.

Our preferred specification is therefore:

$$y_{2013} = \alpha + \beta_1 F_i + \beta_2 R_i + \delta_1 y_{2008} + \delta_2 y_{2002} + \gamma_d + \mathbf{B}\mathbf{X_i} + \epsilon_i \tag{1}$$

where y_{2013} is a measure of voting behavior (turnout, vote choice) in the 2013 election, F_i is a measure of flood impact, R_i is the UNEP measure of *ex ante* flood risk, the y_{2008} and y_{2002} variables capture the voting behavior in question in the previous two elections, and $\mathbf{X_i}$ is our vector of geo-spatial controls plus the proportion of population affected in the smaller 2012 floods that occurred before the 2013 general elections. γ_d is a unit fixed effect for the division, a defunct administrative unit that was larger than the district but smaller than the province. We control for the 27 divisions instead of districts because, outside of Punjab, PA constituencies are often aligned with district boundaries or contain multiple districts.[17] The geo-spatial controls plus division fixed-effects account for 66.2% of the variance in the percentage of the population affected in PA constituencies. We cluster standard errors at the district level to account for the high probability that the cross-constituency variance in turnout changes is correlated within districts as campaign activities in Pakistan are generally managed at the district level.

If the impact of the floods on electoral behavior works through the suggested theoretical mechanisms, as opposed to a response to service delivery or an economic shock, we would expect two patterns:

1. There should be no consistent impact on the incumbent or the main opposition party's vote share; and

2. The impact should be strongest in places that where genuinely surprised by the flooding.

[17] 57% of districts have four or fewer PA constituencies. Hence, using district fixed effects would essentially limit our results to being estimated off the large districts. For transparency all main results are shown without any unit fixed-effects, province fixed-effects, and division fixed-effects.

Put more starkly, none of the theoretical mechanisms necessarily predict changes in vote shares, but they do indicate that surprised constituencies will have higher turnout due to the greater informational and psychological impact of the floods. To assess the prediction (1) we re-run Equation 1 on the national and the provincial level incumbent vote shares. To assess prediction (2) we interact our flood exposure measures with the UNEP measure of *ex ante* flood risk, resulting in the following estimation equation:

$$y_{2013} = \alpha + \beta_1 F_i + \beta_2 R_i + \beta_3 (F_i \times R_i) + \delta_1 y_{2008} + \delta_2 y_{2002} + \gamma_d + \mathbf{BX_i} + \mathbf{CP_i} + \epsilon_i. \qquad (2)$$

Here β_3 captures the change in the impact of flooding on different voting outcomes as one moves across levels of *ex ante* risk. For thoroughness we estimate both the continuous interaction between flood exposure and *ex ante* risk as well as that between flood exposure and a dummy variable for whether flood risk is greater than 1 on the 0-5 UNEP scale (roughly the median of the scale).

To be clear, this approach cannot separately identify the effect of response from that of harm, similar to other papers in this literature. We provide suggestive evidence that the result is not a simple gratitude reaction in Appendix B, where we show that controlling for relief spending does little to attenuate the estimated impact of flood exposure on turnout. Those results must be taken as suggestive, however, as the assignment of relief spending was not, obviously, independent of harm and relief spending was recorded at higher levels of spatial aggregation than voting.

Results

In this section we report the main results on turnout and then show they are robust: (1) across sub-samples; (2) to a quadratic functional form; and (3) a placebo test. We also provide evidence on the impact of flooding on vote share. Overall we find that flood exposure significantly and positively increased turnout in the 2013 elections. We then turn to more speculative evidence on the mechanisms, showing that these effects are strongest in constituencies that had the lowest *ex ante* flood risk and therefore experienced the strongest shocks.

Impact on Electoral Behavior

Turnout

Our main results are based on official election data recorded 21-33 months after the floods. Table 1 shows the impact of flood exposure on constituency-level turnout in the 2013 PA elections. All regressions include controls for *ex ante* flood risk and geographic controls. To make clear how controlling for past turnout impacts the conditional correlation between flood exposure and turnout Columns (1)-(3) do not include controls for past turnout, Columns (4)-(6) control for the change in turnout in the constituency from 2002 to 2008, and Columns (7)-(9) control independently for the levels of turnout in 2002 and 2008. Within each block we show the results with no fixed effects, with province fixed effects to partial out any uncontrolled heterogeneity across the first-level administrative sub-unit which in Pakistan is responsible for most service delivery, and division fixed-effects to control for such factors at a finer geographic scale. Estimates are fairly stable and consistent across different specifications. In our preferred specification, Column (9), increasing the share of the population exposed to the flood from 0% to 100% increased turnout by 6.6 percentage points. This is a substantively large effect, given that the mean turnout change between 2008 and 2013 was 10.3 percentage points. Further, the effect size represents over one-half a standard deviation of the dependent variable.

INSERT TABLE 1 HERE

Examining how the coefficients change across specifications is helpful for understanding potential biases. It is possible that places which show flooding would be expected to experience greater changes in turnout than the places that do not, perhaps due to past experiences or issues related to geography. Some of this concern is addressed by controlling for *ex ante* flood risk, but one might also want to control for previous trends in voting behavior. Comparing across specifications with the same fixed effects (e.g., Column 1 vs. Columns 4 and 7) shows that controlling for trends makes the results substantively larger, which is the opposite of what would happen if the main result were identifying either reversion to mean from previous trends or some kind of secular tendency in places likely to be flooded.

In standardized terms a one standard deviation increase in the proportion of a constituency's population flooded (0.146) led to a 0.96 percentage point increase in turnout. This is a modest average increase in electoral mobilization. Compared to recent U.S. presidential elections this increase in absolute terms is roughly 1/6th of the 5.4 percentage point increase in turnout between 2000 and 2004, and greater than half the 1.4 percentage point increase in turnout between 2004 and 2008, typically attributed to an unusually motivated electorate turning out in support of a historic candidate. The fact that we find no significant flood effect on the number of registered voters, but a significant impact on the number of votes cast, further supports our theoretical argument (see Appendix Table B.5, which shows components of turnout for different subsets of the data).[18] The increase in turnout is not due to lower registration but due to an increase in the number of votes cast in flooded constituencies, which is what we would expect if the floods increased civic engagement.[19] Since controlling for past levels is more flexible then controlling for trends and is more conservative (in that it yields smaller estimates) all subsequent results include controls for levels.

Sensitivity to Sub-Samples and Functional Form

One potential concern with the main result is that it may be an artifact of pre-existing differences between places in the flood plain and those outside of it. Table 2 therefore shows results for the full sample (Columns 1-3) as well as for two subsamples: places near major rivers, which we define as constituencies bordering major rivers and ones immediately adjacent to those (Columns 4-6), and only constituencies bordering major rivers (Columns 7-9). This essentially restricts the analysis to areas that were proximate to major water sources in case the effects we observe are driven by systematic differences between such places and those further away that are not accounted for by our controls. Without fixed effects or with province fixed effects the results become substantively smaller but remain statistically significant as the sample is restricted (Columns 1 vs. 4/7 or Columns 2 vs. 5/8), suggesting that some share of the main result in those specifications was driven by underlying differences between places in the flood plain and those outside of it. However, once division fixed

[18]Note that in the appendix table the coefficients on votes cast and registration are sensitive to the fixed effects being used, which is not the case for the overall turnout numbers. Once province or division fixed effects are included votes cast increased using either measure. Registered voters drop insignificantly with province fixed effects and increase insignificantly with division fixed effects).

[19]Put differently this is not a change in turnout driven by changes in the denominator.

effects are included (Columns 3 vs. 6/9) these differences disappear and all estimates are similar in magnitude and statistical significance across subsets. This consistency provides evidence that the effect of flood exposure is identified off variation in flooding across areas with similar propensity to flood and not off differences between constituencies closer to potential flooding sources (those in the flood plain) compared to those further away.

INSERT TABLE 2 HERE

A second potential concern with these results is that there may be some significant non-linearity in the relationship between flood exposure and turnout. To test for that possibility Appendix Table B.1 adds a squared term for flood exposure to all the specifications from Table 2. The squared term is rarely significant in any subset and while the test for joint significance of the linear and squared terms rejects the null of no joint effect ($p < .05$ in all specifications), the actual improvement in r-squared is quite modest. Marginal effects calculations in the appendix show that at high levels of flood exposure the marginal impact of flood exposure is substantively large and strongly statistically significant in almost all specifications. It seems unlikely that our linear specification is leading to erroneous inference.

Placebo Test

If our identifying assumptions about isolating the exogenous component of the 2010-11 floods are valid, then there should be no consistent relationship between flood exposure in 2010-11 and turnout in previous years. Table 3 shows the conditional correlation between flood exposure and turnout in 2002, turnout in 2008, and the trend across samples (Panel A vs. Panel B vs. Panel C), with different types of fixed effects.

INSERT TABLE 3 HERE

The placebo test is fairly clean but there is a statistically significant negative correlation between the change in turnout from 2002 to 2008 and the level of flooding in 2010-11 in the full sample for the fixed effects models. That relationship is statistically insignificant and becomes smaller in magnitude as the sample is restricted (Columns 7-9 in Panel A vs. Panels B/C). In Panel A the coefficients are negative and weakly significant while in Panels B and C they are negative and statistically insignificant. These patterns suggest that the flood treatment is not simply capturing an omitted aspect of the constituencies that is also positively related to turnout. It is particularly informative that in the model with division fixed effects the placebo regression on past changes shows a conditional correlation that gets closer to zero as the sample is restricted, whereas in the same model for 2013 turnout there is no change across subsets (Table 2 Columns 3/6/9). It is therefore unlikely that some long-run relationship between flood risk and voting patterns is responsible for our findings.

Vote Share

We next test whether partisan swings drive the result. Table 4 presents the effect of the floods on major party vote shares in the 2013 PA elections. Panels A-C show the estimates for three different outcomes: the provincial incumbent party's vote share (i.e., the PPP in Balochistan and Sindh, the PML-N in Punjab, and the ethnic Awami National Party (ANP) in Khyber-Pakhtunkhwa (KPK)) (Panel A), the national ruling party (i.e. the PPP) (Panel B), and the main opposition's vote share (i.e., the PML-N) (Panel C). The columns indicate different administrative subsets of constituencies: Column 1 presents the results for all PA constituencies in Pakistan's four regular provinces, Column 2 for the two smaller provinces Balochistan and KPK, Column 3 for Punjab, the largest province, and Column 4 for Sindh province. For succinctness we show the results with and without division fixed effects. All regressions include geographic controls.[20]

INSERT TABLE 4 HERE

We see no consistent evidence that the floods led to an increase in vote share for the ruling provincial parties, the ruling national party, or the main national opposition. The effects are inconsistent across subsets and fail to reach conventional levels of statistical significance once fixed effects are included. It therefore seems highly unlikely that the turnout results completely reflect a partisan response which rewarded those in power for effectively addressing the floods.

The implications of the fact that the incumbent party was neither punished nor rewarded after such a historic event for theories of democratic accountability is a difficult question in this

[20]Results are almost identical if we include political controls: the outcome variable in 2008, the degree of political competition in the 2008 elections, a series of dummy variables indicating which major party represented the constituency between 2008 and 2013, and interaction terms between the party dummies and political competition. The only substantive difference of note is that the coefficient in Panel B (Column 1) is about 10% smaller.

context. The challenge is that the national ruling party at the time of the floods was the Pakistan People's Party (PPP). They were responsible for working with the Army to coordinate relief by national level bodies and relief from international organizations. However, the relief effort within provinces was managed by the provincial governments. In Punjab (roughly half the population) the provincial government in charge of relief was the Pakistan Muslim League Nawaz (PML-N). In Sindh and Balochistant (roughly 30% of the population) and Balochistan it was the PPP. In Khyber Pakhtunkhwa (KP, roughly 20% of the population) it was the Awami National Party—a secular Pashtun nationalist party—in 2010 and a new national party, the Pakistan-Tarek-Insaf, in 2013. Given that complexity it would have been hard for a voter to know which party to reward or punish, akin to the concept of "clarity of responsibility" (Powell Jr. and Whiten, 1993). It is impossible to tell from our data whether the lack of partisan swing reflects the ambiguous responsibility in this context or a general failure to reward good performance.

Evidence on Mechanisms

Turning to mechanisms, we examine whether the impact of flood exposure on turnout varies with *ex ante* flood risk in ways that are consistent with the learning mechanism. As explained above, we would expect the treatment to have the largest effect where the floods "suprised" people. To assess this we re-estimate the models shown in Table 1, but include an interaction term between the UNEP *ex ante* flood risk and our measures of flood exposure. Table 5 presents the full results two different ways. Panel A shows the results across different sets of controls for the continuous interaction between flood exposure and *ex ante* risk. Panel B shows how the slope of the relationship shifts for places above and below a flood risk of 1 on a 0-5 scale, approximately the median of the risk distribution.

INSERT TABLE 5 AND FIGURE 4 HERE

Once controls for the level of turnout in the past two elections are included (Columns 7-9) it is clear that the flood impact on turnout varies by the *ex ante* flood risk of a constituency: the lower the *ex ante* flood risk of a constituency, the greater the impact of the floods on turnout. In very low risk (*ex ante* flood risk = 0) constituencies, moving across the possible range of proportion of population flooded led to a substantively large 11.5 percentage point increase in turnout. In standardized terms a one s.d. increase in flood exposure (.146) predicts a 1.7 percentage point increase in turnout in low risk areas, almost three times the average treatment effect in Table 1 (Column 9). For constituencies at the highest level of risk (*ex ante* flood risk = 5), however, there is no longer a statistically significant relationship between flood exposure and turnout. To illustrate the relationship visually Figure 4 shows the average marginal effects of a unit increase in flood exposure for different levels of *ex ante* flood risk from our preferred specification. We interpret these results as modest evidence that flood exposure had a greater impact where people were surprised by the flooding.

It is important, however, not to overstate the magnitude of the interaction effect. As Figure 4 makes clear the difference between the treatment effect from flood exposure at very high levels of risk vs. very low levels of risk is not statistically significant. To avoid assuming linearity in the interaction term, consider the regression in Panel B where we compare the slope of the coefficient on flood exposure in places below 1 on the risk scale with places above it. The slope shift is only statistically significant in one specification, but it is negative in all but one and not particularly close to zero once we control for levels of past turnout. As shown in Panel B (Columns 7-9) the marginal effect of flood exposure is always larger in low-risk areas, but is not statistically significantly so in our preferred specification ($t = 1.47$).

In Appendix C we provide survey evidence captured 5-17 months post-flood and 14 months pre-election that indicates a behavioral change among flood-affected individuals in so far as they seem to have invested more time and effort in acquiring political knowledge and become more supportive of aggressive political action.[21] Given that the survey data are cross-sectional, we are unable to make strong causal inferences. Nonetheless, we include information about the survey in the appendix to better elucidate the inductive reasoning that generated the predictions in this paper.

Overall, our results suggest that turnout in the 2013 PA elections significantly increased across the flood gradient and did so more strongly in places that had a low *ex ante* flood risk than in

[21]The survey results, in fact, generated the theoretical predictions in this paper. We thought the flood might influence support for aggressive political action/participation and so built the vignette experiment described in Appendix C to test that hypothesis and fielded it in Spring 2012. Shapiro gave a talk on April 6, 2013, presenting results from that survey showing that those exposed to the flooding had more aggressive attitudes about demanding government services. During the talk several Pakistani scholars argued that if the results in the vignette experiment did capture aggressiveness about demanding government services then we should see increased turnout in the 2013 election. As the analysis above makes clear that appears to have been correct.

areas with a higher flood risk. This heterogeneity together with the lack of consistent evidence of partisan response, provides empirical evidence that is inconsistent with predictions from standard political accountability models, but is in line with our learning mechanism.

Robustness

We explore three alternative explanations for our findings to assess the robustness of the conditional correlation between flood exposure and turnout. First, we show that the result is not driven by differential response to flooding in urban areas. Second, we provide evidence that the effect is unlikely to be driven by compositional changes wherein people with a low propensity to vote left flood-affected areas and/or those with a high propensity to do so moved in. Third, we show that controlling for the distribution of food and shelter relief in the aftermath of the flood does not substantively change the results. This suggests that the increased political engagement in flood-affected PA constituencies are not an artefact of citizens rewarding aid spending.

Do the Results Reflect an Urban Effect?

One concern is that rapid changes in voting behavior that result from democratization are driving the result.[22] Voter participation tends to increase dramatically during democratization and the changes may be especially large in urban areas that had historically experienced low turnout. In Pakistan many of these urban areas are concentrated around the Indus River. Hence it is possible that these democratization factors are driving the correlation between flood exposure and turnout. While controlling for past turnout and trends would partially address this concern, a more direct test is to look for differences in the conditional correlation between flooding and turnout in urban areas vs. other areas.

To do so we use Landscan population data to classify constituencies as 'urban' if their population density is above the 75$^{\text{th}}$ percentile of the population density distribution, approximately 921 people per square kilometer.[23] Appendix Table B.2 shows that the conditional correlation between flooding and turnout is in fact weaker in urban areas. In the full sample (Columns 1-3), the conditional

[22]We thank an anonymous reviewer for making this valuable point.

[23]For reference this is approximately the density of Tuscon, Arizona or Reno, Nevada according to the 2010 census. Using the median population density of 403 people per square kilometer, approximately $\frac{2}{3}$ the population density of sprawling Atlanta, Georgia, does not substantially alter the results.

correlation between flooding and turnout is close to zero in urban areas but strongly positive everywhere else. Once the sample is restricted to constituencies near rivers (Columns 4-6) or just neighboring rivers (Columns 7-9), the effect in urban areas is statistically indistinguishable from that in other areas. Importantly, urban areas share common support with rural areas on flood exposure, so the estimate of the interaction term is valid. It is therefore extremely unlikely that our results are driven by the fact that urban areas were more likely to be flooded and saw greater turnout increases due to the natural progress of democratization.

Is This Just a Compositional Effect?

An immediate concern with any analysis of the impact of a natural disaster which is not based on individual-level panel data is that we may simply be picking up a compositional effect. If people who moved out after the floods were systematically less likely to vote than those who stayed put (or moved in), then the changes we are attributing to the flood's impact on individual civic engagement could actually be an artefact of those migration decisions.

This possibility seems unlikely to drive our results for several reasons. First, there is no evidence in surveys designed to study migration that there were significant, permanent population shifts in Pakistan due to the 2010-11 floods, either to or from flood-affected districts (Mueller, Gray and Kosec, 2013). Less than 2% of those reporting their village was hit in the 2010 or 2011 floods in a nationally representative panel study were living in a different village than in 2001.[24] That would be inadequate to cause effects of the size we observe.

Second, the particulars of Pakistan's voting system also make it unlikely that compositional changes are driving the results. The major door-to-door voter registration effort by the Electoral Commission of Pakistan for the 2013 election occurred from August 22 to November 30, 2011 (mostly after the 2011 floods). Voters were registered at the address on their national identity card and anyone not at home during the door-to-door drive could register until March 22, 2013 at their local electoral commission office by providing a national identity card. Because changing the address on one's national identity card is a relatively cumbersome process (it requires visiting an office with either proof of property ownership or a certificate from a local government representative), many people choose to vote where they were registered rather than changing that address. This

[24]Private communication with the authors of Mueller, Gray and Kosec (2013).

registration process means that if those who moved out were disproportionately inclined not to vote, then their registration would likely remain in flood-affected areas (there would be, after all, no reason for them to shift their registration if they do not plan to vote). This would introduce a downward bias, making our main estimates of the effects of the floods on turnout conservative.

We conduct three tests to assess whether compositional effects drive the results. First, we replicate Tables 1 and 2 using (valid votes/registered voters) as an alternative measure of turnout. As Appendix Tables B.3 and B.4 show all results remain substantially the same. This suggests that neither a sudden influx of highly competent voters (i.e., those more likely to cast valid votes) to flooded areas, nor a mass departure of incompetent ones (i.e., those less likely to case valid votes) from such areas is driving the result. Second, we show that the results are unlikely to be driven by changing registration rates (i.e., out migration post-flood lowering the denominator in flooded areas). Appendix Table B.5 shows that registration is inconsistently correlated with flooding across different specifications and is positively correlated with flood exposure in our preferred specification (Column 9).

While we cannot rule out a compositional effect without better information on migration patterns, the aggregate migration figures and nature of the registration process make it unlikely. We provide suggestive evidence from our 2012 survey to generate a proxy measure for migration and include it in our core turnout regressions. We first calculate the number of respondents reported suffering from flood damage who lived in 2012 in places that were not affected by the 2010-11 floods. If we assume that all those reporting any damage who live in unaffected districts migrated because of the flood, then we estimate that 4.6% of the population in unaffected districts are migrants from the flood-affected regions and that a total of 2.05% of Pakistan's population migrated as a result of flood damage. This is surely an overestimate as many of those who report being affected but live in districts with no flooding either moved for other reasons, are referring to damage suffered by kin, or answered based on damage suffered from monsoon rains in the summer of 2010. Still, we can use our estimates of migration to benchmark the difference in electoral behavior attributable to the impact of the flood.

The simplest way to do so is to estimate the migration rates for the 61 districts in our survey (recall the sample was designed to be district representative) and include the estimates of the proportion of migrants in a district in our core regressions. If people who moved out were less likely to vote, then we should see a negative conditional correlation between the number of migrants in unaffected communities and our outcome variables. Panel A of Appendix Table B.6 shows that controlling for migration generally has little impact on the correlation of interest in the full sample (Columns 1-3) compared to the main results, but could account for roughly half of the flood exposure effect in places neighboring rivers (Columns 7-9). Instead of controlling for migration directly—since our measure of migration is restricted to a subset of constituencies and does not capture in-migration by people unaffected very well—we can also estimate our baseline regressions for the PA constituencies that we estimate did not receive any migrants using our imperfect definition (i.e., those in districts we surveyed that were either clearly hit by the floods or that had no one report flood exposure). As Panel B shows the core results remain substantially unchanged within that sub-sample. Hence, it seems very unlikely that we are simply capturing the impact of differential migration.

Does Increased Turnout Reflect a Reward for Relief Efforts?

The final concern is that the turnout effects merely reflect "turnout buying" arising from relief spending (Nichter, 2008). To measure relief spending we used district-level data on food and shelter disbursements provided by the National Disaster Management Authority. We then constructed a standardized additive index of total aid disbursement from eight categories of shelter relief and the standardized amount of food relief.[25] Note that our aid data are at the district level and may therefore mask intra-district variation in disbursements that are correlated with constituency-level flood exposure, so this is not as strong a test as one might like.[26]

We first examine how our baseline results in Table 2 change if we control directly for relief efforts at the district level. If relief effort is the driving factor underlying our results, we should expect the inclusion of a variable for total flood relief to drastically attenuate the coefficient of our flood treatment. We find little evidence of this. Appendix Table B.7 shows that controlling for total flood relief has little impact on the size of our coefficient estimates of interest. Controlling for aid spending does attenuate the coefficient on population exposed due to its correlation with

[25] The eight categories of shelter aid were: tents, tarpaulins, ropes, toolkits, blankets, kitchen kits, bedding, and plastic mats. The data record the count of each item distributed at the district level.

[26] Higher resolution aid data were collected during the recovery effort but unfortunately erased when the NDMA changed its website and computer systems in early 2013. Personal communication with NDMA officials, November 15, 2013.

aid distribution in the full sample (i.e., R-squared = .25 in a regression of total flood relief on population exposed without division fixed effects and .42 including division fixed effects), but the resulting attenuation is generally less than 10 percent of the effect size. It therefore seems unlikely that gratitude for preferential aid spending in the immediate aftermath of the floods is driving the results.

We also added total flood relief to our models of major party vote shares.[27] Again, we find little evidence that voters rewarded the national or provincial ruling party for what was generally considered to be an effective response (particularly compared to previous floods in Pakistan) or disproportionate flood relief. Our estimates of flood exposure remain almost identical in size and inconsistent across specifications. Moreover, the coefficient estimates of total flood relief have no consistent sign and are generally insignificant, providing little evidence for a patronage effect.

Conclusion

We have shown that in the case of the 2010-11 Pakistani floods—the largest floods in the last 20 years in Pakistan—a major natural disaster led to greater political engagement. In early-2012, 5-17 months after the most recent floods, citizens exposed to the disaster knew more about politics, reflecting a greater investment in acquiring political information. In May 2013, 14 months later, citizens in those same areas turned out to vote at substantially higher rates compared to otherwise-similar unaffected constituencies, exactly as one would have expected given the survey results.[28]

Examining underlying mechanisms, three pieces of evidence point toward the previously described psychological and social changes in the aftermath of natural disasters (e.g., Bardo, 1978; Bolin and Stanford, 1998; Toya and Skidmore, 2014; Rodriguez, Trainor and Quarantelli, 2006; Levine and Thompson, 2004; Vollhardt, 2009). First, the survey results are consistent with citizens becoming more politically engaged in hard-hit areas. Second, the effects described above were particularly strong in the subset of places that had a low *ex ante* risk of being flooded (i.e., those places that were genuinely surprised by the flood). Third, we found only modest evidence that these changes in electoral participation reflect citizens rewarding politicians for their relatively effective handling of the disaster. Instead, the data suggest to us that flood exposure can highlight

[27] Results available from authors.

[28] And as our Pakistani colleagues did expect in April 2013, as noted in section 5.

the importance of a responsive government and community, which creates incentives to invest in political knowledge and to become more politically engaged.

Overall, this is good news for policymakers worried that natural disasters in weakly institutionalized countries undermine democratic institutions. Exposure to natural disasters that are well-handled might actually highlight the necessity of governmental services and foster citizens' political engagement. Future research should assess this possibility further in the following three ways. First, it is important to test more directly the underlying psychological and social mechanisms. Second, we need to assess whether the increase in political engagement we identified actually leads to policy changes, such as the provision of local goods and services across the flood boundary. Finally, an open question is how long these effects last. What we know now is that the floods had enduring political effects two years on, but evidence from work in Germany suggests we may expect effects even long after that (Bechtel and Hainmueller, 2011).

From a policy perspective, the increase in political engagement we observed in Pakistan most likely depended on a relatively effective government response, one that was far more effective than outsiders expected it would be. Policymakers can do a great deal to enable such responses.[29] By reallocating modest funds from their current investments in response, donors could support regular exercises in coordinating large-scale aid flows with emergency management authorities in disaster-prone areas. In doing so they would create the social and organizational ties that can enhance cooperation in the wake of a disaster.[30]

Our results also speak to three additional literatures. First, these results raise questions about the interpretation of a broad set of papers that use natural disasters as a source of variation in economic conditions that is plausibly exogenous to political factors (e.g., Miguel, Satyanath and Sergenti, 2004; Burke and Leigh, 2010; Brückner and Ciccone, 2011; Chaney, 2013). The economic impact of disasters can obviously be highly contingent on government response. And even when that response effectively minimizes economic impacts, we can still observe large changes in citizens' political attitudes and behavior. Thus, the exclusion restriction in a number of recent papers on political liberalization and democratic transition is clearly violated in at least one important case

[29] Andrabi and Das (2010) show that international aid helped significantly in the wake of Pakistan's devastating 2005 earthquake, for example.

[30] Establishing those ties is a key reason allied militaries regularly exercise together and there is no reason to think similar dynamics do not apply in the disaster response field.

and may be in others.

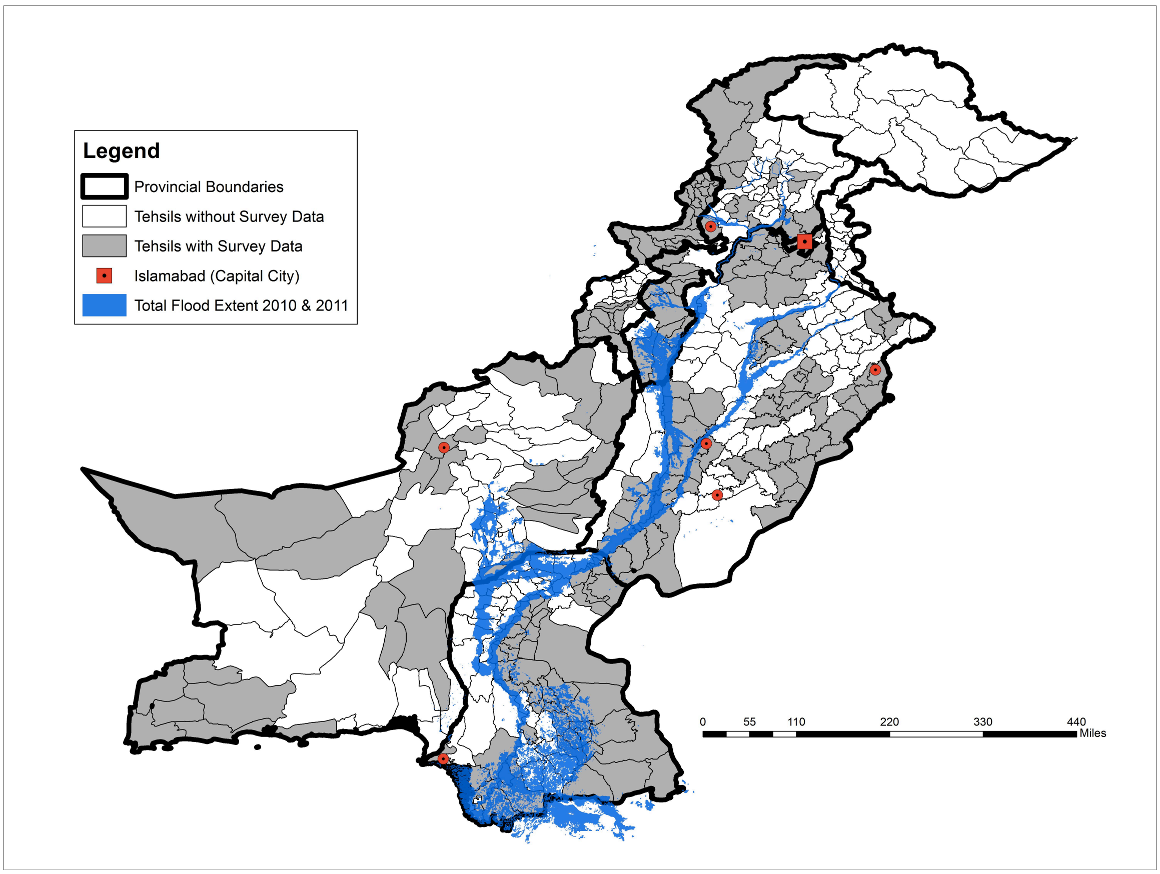
Second, we provide valuable evidence on the question of what drives governments from patron-client systems—which focus on providing targeted benefits to supporters at the cost of services with larger collective benefits—to programmatic systems focused on effective service provision. Most work on the subject has focused on elite bargaining and has left unexamined how changes in citizens' preferences impact elite incentives (e.g., Shefter, 1977; Acemoglu and Robinson, 2012). Yet, as Besley and Burgess (2002) show theoretically and empirically, more informed and politically active electorates create strong incentives for governments to deliver services.[31] The evidence from Pakistan, a country long considered a stronghold of patronage politics, suggests that exogenous events can create just such changes in the electorate.

Finally, our results are relevant to the emerging academic literature on the impact of natural disasters on conflict and to government decision makers planning disaster response. Scholars in this literature typically find a positive relationship between natural disasters and conflict (see e.g., Miguel, Satyanath and Sergenti, 2004; Brancati, 2007; Ghimire and Ferreira, 2013), though there are exceptions (Berghold and Lujala, 2012). These findings worry many as climate change is predicted by most models to lead to a long-run increase in the incidence of severe weather-related disasters (Burke, Hsiang and Miguel, 2013). The evidence from Pakistan suggests that effective response to such disasters can mitigate their negative political consequences. In this case, the international community provided a great deal of post-disaster assistance which the state effectively coordinated. The net result was an increase in legal political engagement by citizens in flood-affected regions compared to non-affected regions. The results thus provide micro-level evidence that aid in the wake of natural disasters can turn them into events which enhance democracy, a possibility consistent with the cross-national pattern identified in Ahlerup (2011), who finds that natural disasters are correlated with democratization in countries that are substantial aid recipients. Overall, our findings suggest enhanced investments in helping poor countries respond well to natural disasters could yield long-run political gains in addition to their obvious economic value.

[31] Pande (2011) provides a review of experimental evidence showing that providing voters with information improves electoral accountability.

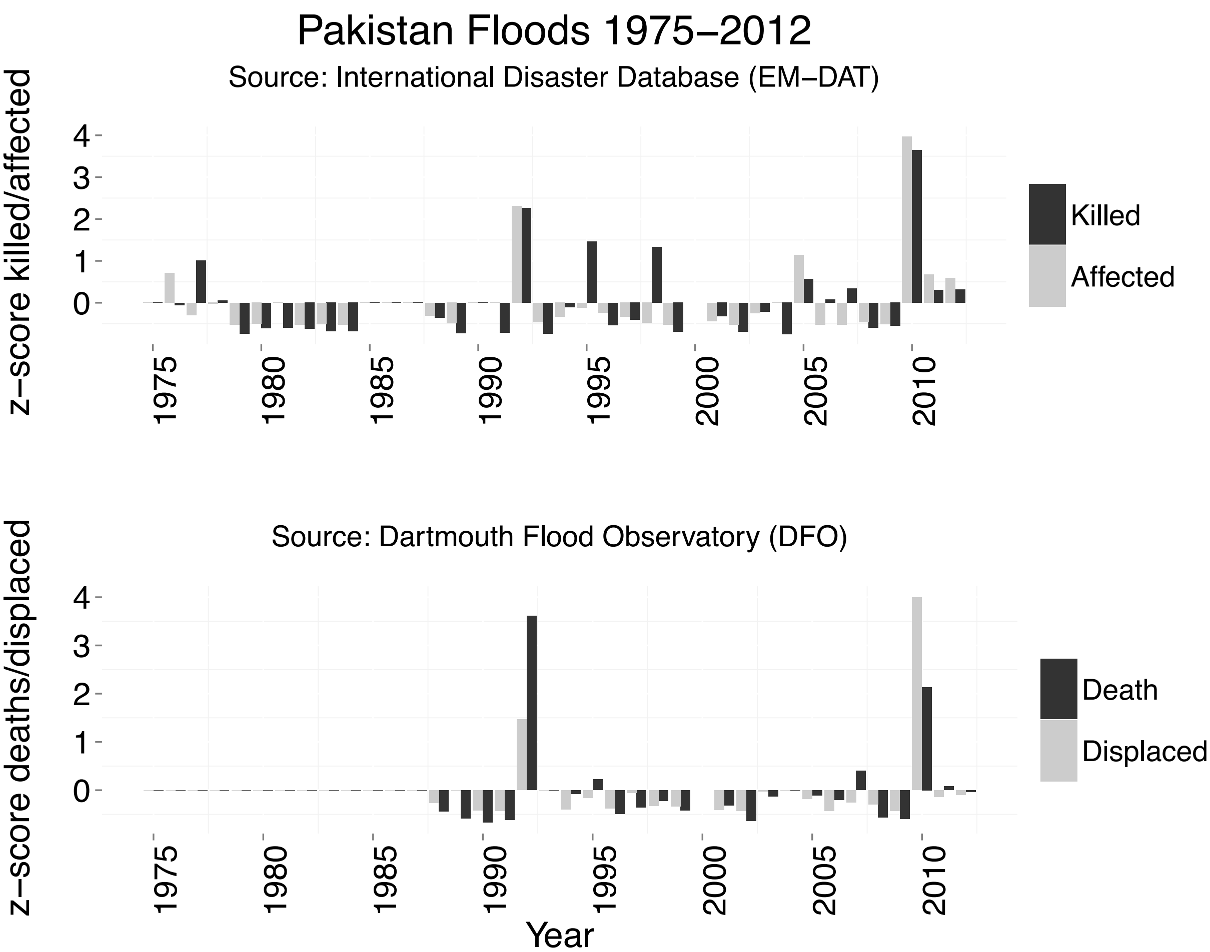
Figures

Figure 1: Maximal Composite Flood Extent in 2010 and 2011 and Surveyed Tehsils



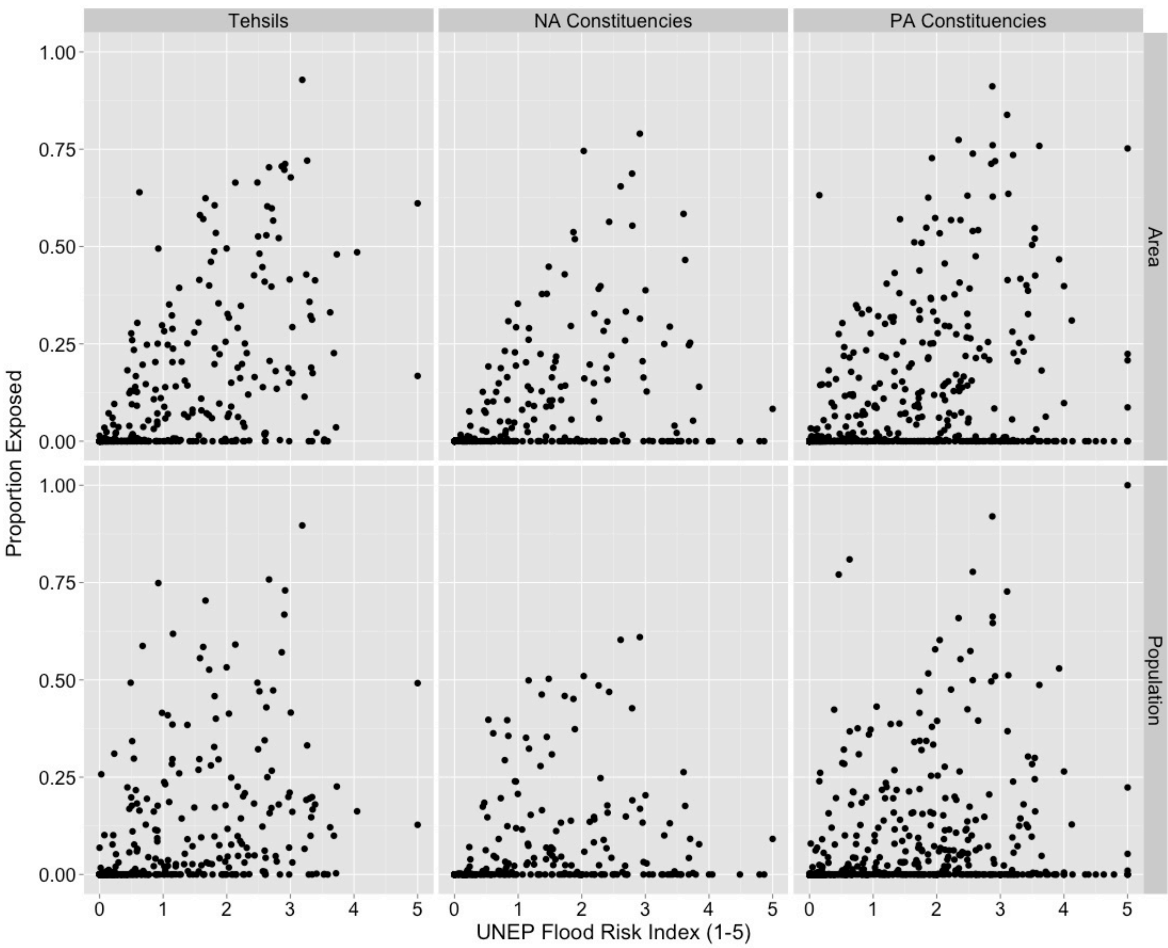
Combined maximal flood extent of the 2010 and 2011 Pakistani floods. Grey colored tehsils indicate locations that were sampled for the 2012 district representative survey. Flood data (area in blue) was taken from UNITAR's Operational Satellite Applications Programme (UNOSAT).

Figure 2: Standardized Impact of Floods in Pakistan 1975 – 2012



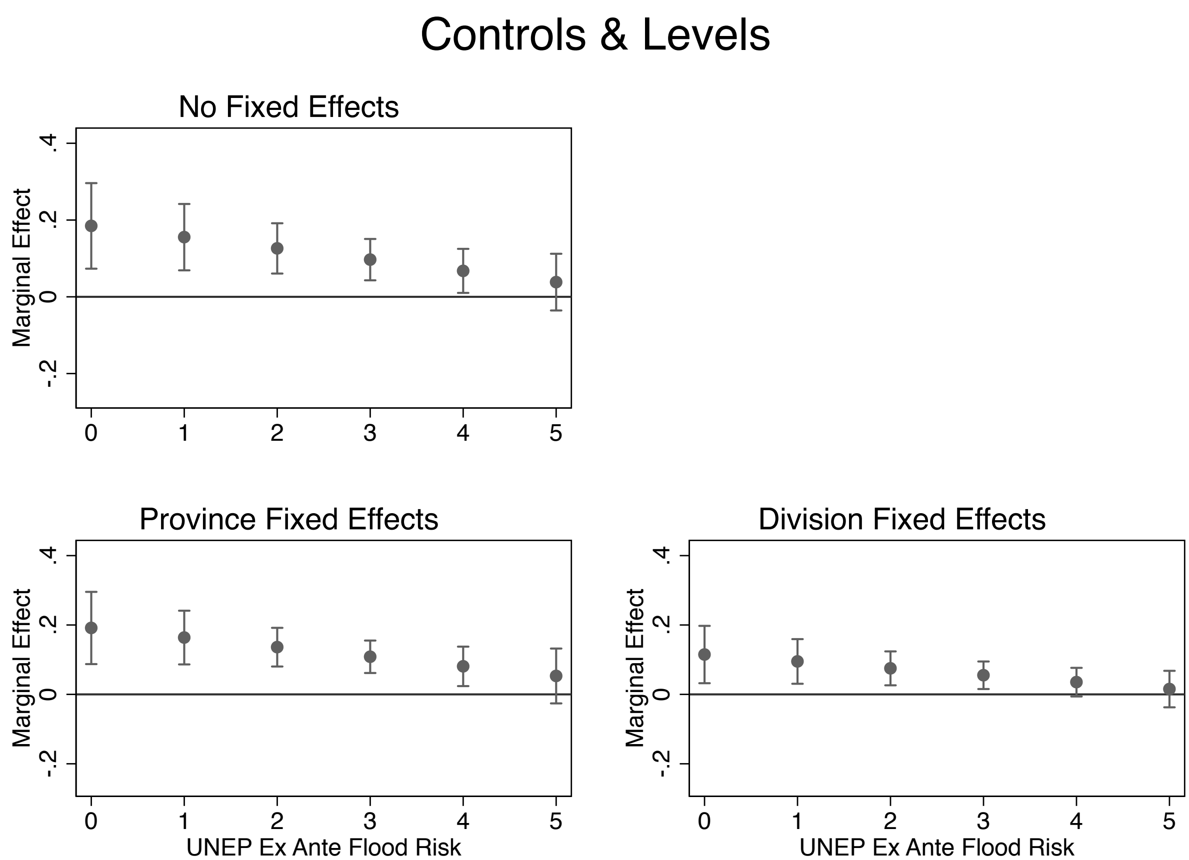
Standardized values for the number affected, displaced, and killed for each flood between 1975 and 2012.

Figure 3: Scatter Plots of UNEP Flood Risk versus Effective Flood Exposure 2010/11



Correlation between *ex ante* flood risk from the UN Environmental Program (UNEP) and exposure measures. Exposure area was calculated from UNITAR's Operational Satellite Appliations Programme (UNOSAT) data. Population *(objective)* exposure was calculated using 2010 Landscan population data along with UNOSAT flooded area data.

Figure 4: Average Marginal Effect of Flood Exposure by Flood Risk in the 2013 PA Elections



Average marginal effects of a one unit change in the proportion of population exposed to the 2010-11 floods in a constituency for different *ex ante* flood risks (calculated from the UN Environmental Program (UNEP) and exposure measures). Flood exposure calucated using objective measures from 2010 Landscan population data along with UNOSAT flooded area data.

Tables

Table 1: Main Result with Different Controls for Past Turnout

	Turnout 2013 (mean=0.541; sd=0.104)								
	Controls			Controls & Turnout Trend			Controls & Turnout Levels		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Pop. Exposed	0.097	0.147	0.066	0.117	0.165	0.084	0.118	0.130	0.066
	(0.033)	(0.028)	(0.026)	(0.039)	(0.033)	(0.026)	(0.034)	(0.030)	(0.022)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.301	0.465	0.613	0.345	0.488	0.636	0.548	0.604	0.706
Observations	556	556	556	556	556	556	556	556	556
Clusters	109	109	109	109	109	109	109	109	109

Notes: Outcome variable is turnout in the 2013 election. Models 4 through 6 control for previous turnout using a trend variable ($trend = \text{turnout}_{08} - \text{turnout}_{02}$). Models 7 through 9 control for previous turnout through 2002 and 2008 turnout level variables (turnout_{02}, turnout_{08}). All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, and mean constituency elevation, as well as the percentage of the population affected by flooding in 2012. Unit of observation is a Provincial Assembly constituency. Standard errors are clustered at the district level and reported in parentheses.

Table 2: Main Result for Different Subsets

	Full Sample			Near Maj. Rivers			Neighboring Maj. River		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Pop. Exposed	0.118	0.130	0.066	0.092	0.110	0.062	0.063	0.081	0.068
	(0.034)	(0.030)	(0.022)	(0.033)	(0.027)	(0.023)	(0.027)	(0.024)	(0.024)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.548	0.604	0.706	0.547	0.612	0.752	0.584	0.624	0.707
Observations	556	556	556	389	389	389	209	209	209
Clusters	109	109	109	77	77	77	63	63	63

Notes: Outcome variable is turnout in the 2013 election. Unit of observation is a Provincial Assembly constituency. All models control for previous turnout through 2002 and 2008 turnout level variables (turnout_{02}, turnout_{08}). All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, and the percentage of the population affected by flooding in 2012. Standard errors are clustered at the district level and reported in parentheses.

Table 3: Placebo Regressions with Different Controls for Past Turnout for Different Subsets

	Turnout 2002			**Turnout 2008**			**Turnout Difference 08-02**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
% Pop. Exposed	-0.046	0.046	0.024	-0.103	-0.037	-0.040	-0.058	-0.082	-0.064
	(0.038)	(0.035)	(0.021)	(0.046)	(0.039)	(0.042)	(0.034)	(0.034)	(0.036)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.247	0.426	0.569	0.247	0.382	0.502	0.063	0.134	0.277
Observations	565	565	565	565	565	565	565	565	565
Clusters	109	109	109	109	109	109	109	109	109
Panel B: Near Major Rivers									
% Pop. Exposed	-0.061	0.019	0.021	-0.105	-0.031	-0.035	-0.044	-0.049	-0.056
	(0.040)	(0.032)	(0.024)	(0.052)	(0.043)	(0.048)	(0.036)	(0.035)	(0.042)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.257	0.425	0.621	0.199	0.339	0.492	0.033	0.060	0.198
Observations	394	394	394	394	394	394	394	394	394
Clusters	77	77	77	77	77	77	77	77	77
Panel C: Neighboring Major Rivers									
% Pop. Exposed	-0.027	0.052	0.017	-0.058	0.025	-0.003	-0.031	-0.026	-0.020
	(0.044)	(0.038)	(0.031)	(0.058)	(0.041)	(0.049)	(0.039)	(0.038)	(0.044)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.353	0.547	0.648	0.224	0.410	0.508	0.046	0.056	0.139
Observations	212	212	212	212	212	212	212	212	212
Clusters	63	63	63	63	63	63	63	63	63

Notes: Unit of observation is a Provincial Assembly constituency. All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, and mean constituency elevation. Standard errors are clustered at the district level and reported in parentheses.

Table 4: Vote Share Regressions

	National		Balochistan & KPK		Punjab		Sindh	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Provincial Incumbent's Vote Share 2013								
% Pop. Exposed	-0.061	0.065	0.051	0.046	-0.021	0.098	0.086	0.015
	(0.087)	(0.062)	(0.067)	(0.153)	(0.139)	(0.163)	(0.098)	(0.074)
Division FE		X		X		X		X
R-Squared	0.405	0.682	0.482	0.570	0.178	0.233	0.551	0.743
Observations	565	565	149	149	288	288	128	128
Clusters	109	109	50	50	36	36	23	23
Panel B: PPP Vote Share 2013								
% Pop. Exposed	0.219	0.048	0.146	0.149	-0.036	-0.037	0.086	0.015
	(0.074)	(0.048)	(0.082)	(0.112)	(0.101)	(0.103)	(0.098)	(0.074)
Division FE		X		X		X		X
R-Squared	0.501	0.716	0.270	0.486	0.218	0.272	0.551	0.743
Observations	565	565	149	149	288	288	128	128
Clusters	109	109	50	50	36	36	23	23
Panel C: PML-N Vote Share 2013								
% Pop. Exposed	-0.102	0.021	-0.102	-0.038	-0.021	0.098	0.091	0.015
	(0.081)	(0.059)	(0.110)	(0.112)	(0.139)	(0.163)	(0.056)	(0.054)
Division FE		X		X		X		X
R-Squared	0.427	0.657	0.309	0.385	0.178	0.233	0.212	0.275
Observations	565	565	149	149	288	288	128	128
Clusters	109	109	50	50	36	36	23	23

Notes: Unit of observation is a Provincial Assembly constituency. All regressions include geographic controls, i.e., ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, and the percentage of the population affected by flooding in 2012. Results are almost identical if we include political controls, i.e., the outcome variable in 2008, the degree of political competitiveness in 2008 elections, a series of dummy variables indicating which major party represented the constituency between 2008 and 2013, and interaction terms between the party dummies and political competition. Electoral data collected at the constituency level from the 2002, 2008, and 2013 National Assembly and Provincial Assembly elections. Standard errors are clustered at the district level and reported in parentheses.

Table 5: Continuous and Dichotomous Interaction of Exposure with Flood Risk

	Controls			Controls & Turnout Trend			Controls & Turnout Levels		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Continuous Interaction									
% Pop. Exposed	0.114	0.158	0.050	0.172	0.198	0.100	0.185	0.191	0.115
	(0.063)	(0.056)	(0.045)	(0.069)	(0.063)	(0.051)	(0.056)	(0.053)	(0.042)
Flood Risk	-0.010	-0.014	-0.010	-0.009	-0.012	-0.008	0.001	-0.003	-0.001
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
% Flooded × Risk	-0.008	-0.005	0.007	-0.024	-0.015	-0.006	-0.029	-0.028	-0.020
	(0.017)	(0.018)	(0.011)	(0.022)	(0.023)	(0.015)	(0.015)	(0.016)	(0.011)
Marginal Effects:									
Flood Risk (25th pctile = .13)	0.113	0.157	0.051	0.169	0.196	0.099	0.181	0.188	0.112
	(0.061)	(0.054)	(0.044)	(0.067)	(0.061)	(0.049)	(0.055)	(0.051)	(0.040)
Flood Risk (Median = .96)	0.107	0.153	0.056	0.149	0.184	0.094	0.157	0.165	0.096
	(0.049)	(0.042)	(0.037)	(0.052)	(0.045)	(0.038)	(0.044)	(0.040)	(0.033)
Flood Risk (75th pctile = 2.22)	0.097	0.147	0.065	0.119	0.165	0.086	0.120	0.130	0.071
	(0.033)	(0.028)	(0.027)	(0.037)	(0.032)	(0.027)	(0.031)	(0.026)	(0.023)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.301	0.465	0.613	0.346	0.489	0.636	0.551	0.606	0.707
Observations	556	556	556	556	556	556	556	556	556
Clusters	109	109	109	109	109	109	109	109	109
Panel B: Dichtomous Interaction with Flood Risk ≥ 1									
% Pop. Exposed	0.153	0.149	0.063	0.192	0.177	0.095	0.202	0.184	0.114
	(0.067)	(0.066)	(0.053)	(0.066)	(0.065)	(0.051)	(0.055)	(0.058)	(0.046)
Flood Risk ≥ 1	0.003	0.012	0.000	0.007	0.014	0.003	-0.008	0.002	0.000
	(0.013)	(0.010)	(0.009)	(0.014)	(0.011)	(0.009)	(0.012)	(0.010)	(0.009)
% Flooded × Risk ≥ 1	-0.072	-0.005	0.004	-0.096	-0.018	-0.013	-0.107	-0.072	-0.061
	(0.063)	(0.064)	(0.047)	(0.065)	(0.063)	(0.046)	(0.053)	(0.056)	(0.045)
Effect Differential by Impact:									
Flood Impact (Median = .07)	0.002	-0.012	-0.001	-0.000	-0.012	-0.002	0.015	0.002	0.004
	(0.013)	(0.010)	(0.008)	(0.014)	(0.011)	(0.009)	(0.011)	(0.009)	(0.008)
Flood Impact (75th pctile = .21)	0.012	-0.011	-0.001	0.014	-0.010	0.000	0.030	0.013	0.013
	(0.017)	(0.015)	(0.012)	(0.016)	(0.015)	(0.011)	(0.013)	(0.012)	(0.010)
Flood Impact (90th pctile = .42)	0.027	-0.010	-0.002	0.033	-0.006	0.003	0.052	0.028	0.025
	(0.027)	(0.026)	(0.020)	(0.026)	(0.025)	(0.018)	(0.021)	(0.021)	(0.017)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.303	0.466	0.613	0.347	0.489	0.636	0.553	0.606	0.707
Observations	556	556	556	556	556	556	556	556	556
Clusters	109	109	109	109	109	109	109	109	109

Notes: Unit of observation is a Provincial Assembly constituency. Models 4 through 6 control for previous turnout using a trend variable ($trend$ = turnout$_{08}$ − turnout$_{02}$). Models 7 through 9 control for previous turnout through 2002 and 2008 turnout level variables (turnout$_{02}$, turnout$_{08}$). All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, and the percentage of the population affected by flooding in 2012. Marginal Effects in Panel A shows the effect on turnout at the indicated level of flood risk. Effect Differentials in Panel B show the difference between the effect of flood impact on high risk areas versus low risk areas (turnout$_{risk<1}$ − turnout$_{risk>1}$) at the indicated level of flood exposure (where the distribution is conditional on flood exposure>0). Standard errors are clustered at the district level and reported in parentheses.

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Online Appendix to "Natural Disasters and Political Engagement: Evidence from the 2010-11 Pakistani Floods"

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The appendix consists of three sections. Section A contains the summary statistics on all the variables in the dataset. Section B shows the results of additional regressions mentioned in the main text. Finally, Section C provides survey evidence captured 5-17 months post-flood and 14 months pre-election that indicates a behavioral change among flood-affected individuals in so far as they seem to have invested more time and effort in acquiring political knowledge and become more supportive of aggressive political action. However, given that the survey data are cross-sectional, we are unable to make strong causal inferences. We include this information to better elucidate the inductive reasoning that generated the predictions in this paper.

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A Appendix: Summary Statistics

Table A.1: Summary Statistics for All Covariates of the Provincial Assembly Constituency Data

Variable	Unit	Median	Mean	Std. Dev.	Min	Max	N
Panel A: Outcomes							
Turnout 2002	percentage points [0,1]	0.408	0.409	0.093	0.096	0.763	565
Turnout 2008	percentage points [0,1]	0.442	0.440	0.120	0.064	0.786	565
Turnout 2013	percentage points [0,1]	0.564	0.541	0.104	0.019	0.738	556
Turnout Change 2008-2002	percentage points [0,1]	0.030	0.031	0.080	-0.316	0.316	565
Total Votes Cast 2013	100,000 voters	0.843	0.802	0.273	0.007	1.564	556
Total Valid Votes 2013	100,000 voters	0.822	0.776	0.264	0.007	1.509	565
Registered Voters 2013	100,000 voters	1.475	1.452	0.388	0.328	3.004	565
Provincial Incumbent Vote Share 2013	percent [0,1]	0.349	0.308	0.213	0	0.898	565
PPP Vote Share 2013	percent [0,1]	0.060	0.144	0.184	0	0.898	565
PML-N Vote Share 2013	percent [0,1]	0.271	0.256	0.206	0	0.714	565
Panel B: Flood Treatment							
% Population Exposed 2010 & 2011	percent [0,1]	0	0.067	0.147	0	1	565
Panel C: Controls							
PPP Won Seat 2008	dummy	0	0.306	0.461	0	1	565
PML-N Won Seat 2008	dummy	0	0.191	0.394	0	1	565
PML-Q Won Seat 2008	dummy	0	0.173	0.379	0	1	565
Electoral Competitiveness 2008	index	0.835	0.771	0.215	0.037	0.999	565
UNEP Flood Risk	average of 6pt index	0.957	1.312	1.266	0	5	565
Flood Risk ≥ 1	dummy	0	0.497	0.500	0	5	565
Distance to Nearest Major River	100 kilometers	0.270	0.582	0.895	0.001	6.353	565
Constituency Neighboring Major River	dummy	0	0.375	0.485	0	1	565
Std. Dev. of Elevation	1,000 meters	0.002	0.026	0.053	0	0.323	565
Mean of Elevation	1,000 meters	0.054	0.118	0.183	0.001	1.293	565
% Population Exposed 2012	percent [0,1]	0	0.013	0.062	0	0.700	565
Total Votes Cast 2008	100,000 voters	0.641	0.606	0.222	0.087	1.270	565
Total Valid Votes 2008	100,000 voters	0.623	0.590	0.216	0.086	1.243	565
Registered Voters 2008	100,000 voters	1.364	1.365	0.357	0.139	2.259	565
Total Votes Cast 2002	100,000 voters	0.519	0.510	0.193	0.070	1.062	565
Total Valid Votes 2002	100,000 voters	0.506	0.496	0.187	0.068	1.027	565
Registered Voters 2002	100,000 voters	1.286	1.217	0.282	0.325	1.945	565
Index of Total Relief	pca index	-0.773	0.011	1.621	-0.773	7.628	555
Migration Estimate	percent [0,1]	0.003	0.021	0.04	0	0.224	351

B Appendix:Further Robustness Checks

Table B.1: Non-Linear Turnout Response to Flood Exposure

	Full Sample			**Near Maj. Rivers**			**Neighboring Maj. River**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Pop. Exposed	0.153	0.182	0.078	0.129	0.151	0.048	0.095	0.108	0.079
	(0.070)	(0.070)	(0.050)	(0.066)	(0.057)	(0.048)	(0.069)	(0.060)	(0.058)
% Pop. Exposed^2	-0.059	-0.085	-0.018	-0.060	-0.065	0.021	-0.051	-0.042	-0.016
	(0.085)	(0.086)	(0.060)	(0.075)	(0.071)	(0.054)	(0.082)	(0.072)	(0.069)
Joint Significance:									
F-stat	6.688	11.524	4.703	4.162	9.663	3.833	3.185	7.750	4.531
p-value	(0.002)	(0.000)	(0.011)	(0.019)	(0.000)	(0.026)	(0.048)	(0.001)	(0.015)
Marginal Effects:									
Flood Impact (Median = .07)	0.010	0.012	0.005	0.009	0.010	0.003	0.006	0.007	0.005
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Flood Impact (75th pctile = .21)	0.030	0.035	0.016	0.025	0.029	0.011	0.018	0.021	0.016
	(0.012)	(0.011)	(0.008)	(0.011)	(0.009)	(0.008)	(0.011)	(0.010)	(0.010)
Flood Impact (90th pctile = .42)	0.053	0.061	0.029	0.043	0.051	0.023	0.031	0.038	0.030
	(0.017)	(0.016)	(0.012)	(0.017)	(0.014)	(0.012)	(0.016)	(0.014)	(0.014)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.549	0.605	0.706	0.547	0.612	0.752	0.584	0.624	0.707
Observations	556	556	556	389	389	389	209	209	209
Clusters	109	109	109	77	77	77	63	63	63

Notes: Unit of observation is a Provincial Assembly constituency. All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, the percentage of the population affected by flooding in 2012, and turnout levels in 2002 and 2008. Joint Significance reports the F-statistic for significance of % Pop. Exposed and its squared term jointly. Marginal Effects show the linear combination of % Pop. Exposed and % Pop. Exposed^2 at the indicated level of flood impact (Note: impact distribution level conditional on flood exposure>0). Standard errors are clustered at the district level and reported in parentheses.

Table B.2: Checking For Difference in Response to Flooding Between Urban and Rural Areas

	Full Sample			**Near Maj. Rivers**			**Neighboring Maj. River**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Pop. Exposed	0.132	0.138	0.081	0.098	0.114	0.066	0.060	0.081	0.064
	(0.036)	(0.030)	(0.028)	(0.037)	(0.026)	(0.028)	(0.032)	(0.025)	(0.031)
Urban	0.000	-0.002	0.010	-0.018	-0.015	-0.008	-0.033	-0.021	-0.022
	(0.010)	(0.010)	(0.013)	(0.013)	(0.010)	(0.008)	(0.012)	(0.010)	(0.009)
% Flooded × Urban	-0.112	-0.077	-0.094	-0.052	-0.029	-0.038	-0.006	-0.016	-0.024
	(0.052)	(0.058)	(0.049)	(0.056)	(0.064)	(0.034)	(0.048)	(0.054)	(0.045)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.551	0.606	0.708	0.554	0.615	0.754	0.598	0.630	0.713
Observations	556	556	556	389	389	389	209	209	209
Clusters	109	109	109	77	77	77	63	63	63

Notes: Outcome is turnout in 2013. Unit of observation is a Provincial Assembly constituency. 'Urban' is defined as constituencies with population density above the 75^{th} percentile of the population density distribution, approximately 921 people per square kilometer. All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, the percentage of the population affected by flooding in 2012, and turnout levels in 2002 and 2008. Standard errors are clustered at the district level and reported in parentheses.

Table B.3: Alternative Turnout Measure (Valid Votes/ Registered Voters)

	Turnout 2013 (mean=0.525; sd=0.100)								
	Controls			**Controls & Turnout Trend**			**Controls & Turnout Levels**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Pop. Exposed	0.088	0.131	0.057	0.094	0.136	0.060	0.094	0.106	0.043
	(0.032)	(0.024)	(0.027)	(0.032)	(0.025)	(0.026)	(0.030)	(0.024)	(0.023)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.304	0.472	0.626	0.311	0.475	0.628	0.485	0.569	0.682
Observations	565	565	565	565	565	565	565	565	565
Clusters	109	109	109	109	109	109	109	109	109

Notes: Outcome variable is turnout in the 2013 election measured as valid votes/registered voters. Models 4 through 6 control for previous turnout using a trend variable ($trend$ = turnout$_{08}$ − turnout$_{02}$). Models 7 through 9 control for previous turnout through 2002 and 2008 turnout level variables (turnout$_{02}$, turnout$_{08}$). All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, and mean constituency elevation, as well as the percentage of the population affected by flooding in 2012. Unit of observation is a Provincial Assembly constituency. Standard errors are clustered at the district level and reported in parentheses.

Table B.4: Alternative Turnout Measure (Valid Votes/ Registered Voters)

	Full Sample			**Near Maj. Rivers**			**Neighboring Maj. River**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Pop. Exposed	0.094	0.106	0.043	0.069	0.092	0.045	0.050	0.070	0.048
	(0.030)	(0.024)	(0.023)	(0.028)	(0.017)	(0.022)	(0.025)	(0.017)	(0.023)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.485	0.569	0.682	0.468	0.564	0.691	0.531	0.586	0.665
Observations	565	565	565	394	394	394	212	212	212
Clusters	109	109	109	77	77	77	63	63	63

Notes: Outcome variable is turnout in the 2013 election measured as valid votes/registered voters. Unit of observation is a Provincial Assembly constituency. All models control for previous turnout through 2002 and 2008 turnout level variables (turnout$_{02}$, turnout$_{08}$). All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, and the percentage of the population affected by flooding in 2012. Standard errors are clustered at the district level and reported in parentheses.

Table B.5: Regressions on Components of Turnout for Different Subsets

	Votes Cast 2013			Valid Votes Cast 2013			Registered Voters 2013		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
% Pop. Exposed	-0.004	0.128	0.142	-0.008	0.115	0.130	-0.234	-0.063	0.104
	(0.084)	(0.048)	(0.050)	(0.086)	(0.049)	(0.048)	(0.135)	(0.110)	(0.064)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.618	0.751	0.798	0.611	0.745	0.799	0.427	0.586	0.702
Observations	556	556	556	565	565	565	565	565	565
Clusters	109	109	109	109	109	109	109	109	109
Panel B: Near Major Rivers									
% Pop. Exposed	0.051	0.193	0.162	0.044	0.177	0.148	-0.117	0.080	0.143
	(0.100)	(0.056)	(0.060)	(0.100)	(0.053)	(0.056)	(0.145)	(0.095)	(0.076)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.474	0.654	0.732	0.460	0.641	0.719	0.223	0.419	0.582
Observations	389	389	389	394	394	394	394	394	394
Clusters	77	77	77	77	77	77	77	77	77
Panel C: Neighboring Major Rivers									
% Pop. Exposed	0.012	0.174	0.186	0.006	0.151	0.150	-0.136	0.098	0.151
	(0.098)	(0.062)	(0.067)	(0.095)	(0.057)	(0.061)	(0.161)	(0.119)	(0.074)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.532	0.651	0.757	0.525	0.638	0.749	0.277	0.412	0.671
Observations	209	209	209	212	212	212	212	212	212
Clusters	63	63	63	63	63	63	63	63	63

Notes: Unit of observation is a Provincial Assembly constituency. All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, the percentage of the population affected by flooding in 2012, and turnout levels in 2002 and 2008. Standard errors are clustered at the district level and reported in parentheses.

Table B.6: Controlling for Migration

	Full Sample			Near Maj. Rivers			Neighboring Maj. River		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Controlling for Migration									
% Pop. Exposed	0.097	0.120	0.058	0.040	0.066	0.037	0.036	0.036	0.040
	(0.042)	(0.036)	(0.027)	(0.030)	(0.029)	(0.029)	(0.018)	(0.021)	(0.039)
Migration	0.089	0.074	-0.081	-0.202	-0.200	-0.117	0.205	0.214	0.238
	(0.189)	(0.174)	(0.149)	(0.179)	(0.163)	(0.144)	(0.104)	(0.107)	(0.159)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.582	0.626	0.741	0.511	0.537	0.729	0.630	0.638	0.680
Observations	345	345	345	218	218	218	113	113	113
Clusters	58	58	58	39	39	39	30	30	30
Panel B: Subset of Constituencies with No Migration									
% Pop. Exposed	0.102	0.102	0.060	0.090	0.097	0.061	0.099	0.104	0.066
	(0.031)	(0.026)	(0.021)	(0.034)	(0.030)	(0.024)	(0.034)	(0.033)	(0.025)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.569	0.644	0.743	0.549	0.606	0.719	0.549	0.602	0.717
Observations	378	378	378	332	332	332	281	281	281
Clusters	87	87	87	77	77	77	71	71	71

Notes: Migration measure was calculated using our national survey data by assuming that all those reporting any damage who live in unaffected districts migrated because of the flood and then estimating the migration rates for the 61 districts in our survey (recall the sample was designed to be district representative). This is an imperfect measure but if people who moved out were less likely to vote, then we should see a negative conditional correlation between the number of migrants in unaffected communities and our outcome variables. Unit of observation is a Provincial Assembly constituency. All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, the percentage of the population affected by flooding in 2012, and turnout levels in 2002 and 2008. Standard errors are clustered at the district level and reported in parentheses.

Table B.7: Controlling for Total Relief Provision

	Full Sample			**Near Maj. Rivers**			**Neighboring Maj. River**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Pop. Exposed	0.126	0.117	0.060	0.099	0.099	0.061	0.084	0.081	0.069
	(0.037)	(0.032)	(0.023)	(0.038)	(0.031)	(0.025)	(0.033)	(0.030)	(0.026)
Total Relief	-0.002	0.002	0.002	-0.002	0.001	0.001	-0.003	0.000	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Province FE		X			X			X	
Division FE			X			X			X
R-Squared	0.549	0.604	0.706	0.544	0.609	0.751	0.587	0.624	0.707
Observations	546	546	546	379	379	379	209	209	209
Clusters	106	106	106	74	74	74	63	63	63

Notes: Unit of observation is a Provincial Assembly constituency. All regressions include controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation, the percentage of the population affected by flooding in 2012, and turnout levels in 2002 and 2008. Standard errors are clustered at the district level and reported in parentheses.

C Appendix: Individual-Level Survey Based Results

This section summarizes results from a nationally representative survey we conducted in early 2012, in between the floods and the 2013 general election in Pakistan. The questions reported on in this section were included in the 2012 survey because we expected that the flood might influence support for aggressive political action/participation. We built in a vignette experiment to test that hypothesis and also included measures of political knowledge.

The research described in this appendix generated the hypothesis in the main paper that flood impact should increase turnout at the aggregate level. We present this work here to better explain our inductive process.

C.1 Data

The National Survey

We created district-representative samples of 155-675 households in 61 districts with a modest over-sample in heavily flood-affected districts as determined by our spatial flood exposure data. We sampled 15 districts in Balochistan, 14 in KPK, 12 in Sindh, and 20 in Punjab to ensure we covered a large proportion of the districts in each of Pakistan's four regular provinces. Within each province we sampled the two largest districts and then chose additional districts using a simple random sample. The main results below should be taken as representative for our sampling strategy which, while diverse in terms of coverage, does overrepresent Pakistanis from the smaller provinces.[1] Our Pakistani partners, SEDCO Associates, fielded the survey between January 7 and March 21, 2012, 17 months after the 2010 flood, 5 months after the summer 2011 floods in Sindh, and 14 months prior to the 2013 general election. Our overall response rate was 71%, with 14.5% of households contacted refusing to complete the survey and 14.5% of the targeted households not interviewed because no one was home who could take the survey. This response rate is similar to the 70% obtained in the General Social Survey in recent years and exceeds the 59.5% achieved by the 2012 American National Election Survey (GSS, 2013; ANES, 2014).

Treatment Measure: Subjective Reports

Our subjective measure of flood exposure comes from a question included in our survey. To get variation in flood impacts at the household level, we also asked respondents how the floods impacted them personally. We use the following question to measure respondents' subjective assessments of flood damage:[2]

> "How badly were you personally harmed by the floods?" (response options: "extremely badly," "very badly," "somewhat badly," "not at all")

We coded the Likert scale to range from 0 to 1.

[1] Weighted results using either sample weights calculated from Landscan gridded population data or those provided by the Pakistan Federal Bureau of Statistics for our survey are substantively and statistically similar.

[2] Responses to this question correlate well with our other self-reported measure. We asked respondents to rate how much money they lost as a result of the floods on an ordinal scale: less than 50k Pakistani Rupee (Rs.), 50k Rs. to 100k Rs., 100k-300k Rs., and more than 300k Rs. The Pearson correlation between that loss and the subjective measure of exposure above is quite high ($r = .73$).

Outcome Measure: Political Knowledge

We construct a measure of political knowledge using a battery of binary questions. To tap awareness of political issues, we asked respondents whether they were aware of four policy debates prominent in early 2012: whether to use the army to reduce conflict in Karachi; how to incorporate the FATA into the rest of Pakistan; what should be done to resolve the disputed border with Afghanistan; and whether the government should open peace talks with India. We also asked six questions about various political offices and scored whether respondents correctly identified the following: who led the ruling coalition in Parliament (the PPP); and the names of the President, Prime Minister, Chief Minister of their Province, Chief of Army Staff, and Chief Justice of the Supreme Court.

Following Kolenikov and Angeles (2009), we conducted a principal components analysis on the polychoric correlation matrix of these items and use the first principal component as our measure of political knowledge. That component accounts for 49.15% of the variance in the index of ten components, suggesting it does a good job of capturing the underlying construct.

The political knowledge index can also be considered a proxy for political engagement. Respondents either know these facts or they do not; they cannot dissemble. Differences between respondents across the flood gradient must either reflect some real investment in acquiring political knowledge or some pre-existing background trait that is correlated with both experiencing the floods and political knowledge. The latter is unlikely subject to our identifying assumptions outlined below. Moreover, since our core estimating equation for individual-level outcomes includes controls for a range of slow-changing factors that one could imagine are correlated with both exposure risk and political engagement—including literacy, numeracy, age, education, gender, and head of household status—we believe the knowledge variable most likely represents investments in political knowledge triggered by the flood.

Outcome Measure: Support for Assertive Political Action

In order to measure support for assertive political action we use a vignette experiment. This approach circumvents three main challenges to measuring political attitudes. First, respondents may face social desirability pressures to not explicitly support particular views (e.g., aggressive civic protests). Second, concepts such as the political efficacy of being assertive are not easily explainable in standard survey questions but can be illustrated with examples. Third, respondent answers to direct questions may not be interpersonally comparable (King et al., 2004). To overcome these challenges, we wrote two vignettes describing concrete (but fictional) examples of two different ways of getting the government's attention: passive petition or assertive protest. Respondents were randomly assigned to receive one of the vignettes before answering the same two survey questions on how effective they think the chosen method is and whether they approve of it.

More specifically, the vignette experiment works as follows. At the primary sampling unit (PSU) level, respondents are randomly assigned to read one of the following two vignettes:

> *Passive Petition*. Junaid lives in a village that lacks clean drinking water. He works with his neighbors to draw attention to the issue by collecting signatures on a petition. He plans to present the petition to each of the candidates before the upcoming local elections.
>
> *Assertive Protest*. Junaid lives in a village that lacks clean drinking water. He works with his neighbors to draw attention to the issue by angrily protesting outside the office of the district coordinating official. As the government workers exit the office, they threaten and shove them.

Following the vignette respondents are asked the following:

- *Effectiveness*. "How effective do you think Junaid will be in getting clean drinking water for his village?" (response options: "extremely effective," "very effective," "moderately effective," "slightly effective," "not effective at all")
- *Approval*. "How much do you approve of Junaid's actions?" (response options: "a great deal," "a lot," "a moderate amount," "a little," "not at all")

Our vignette experiment is intended to convey a clear difference in the aggressiveness with which citizens demand government services. We therefore varied three elements of how the citizens in the vignette engaged with the government: immediacy, target, and method. The next election was expected to happen in 2013 when we did our survey, so the *Passive Petition* vignette clearly conveys a demand that will be delayed, while the *Assertive Protest* one describes something that could happen right now. With respect to target, the district coordinating official (DCO) is the relevant official for drinking water issues, but he/she is an appointed bureaucrat who reports to the Chief Minister of the province. If citizens go to the DCO he/she can do something right away, whereas local politicians have to go to the senior party leadership of the party in power in their province who may then reach back down to the DCO. The *Passive Petition* vignette thus conveys a situation where the response to citizen action will be indirect at best, while the *Assertive Protest* one portrays citizens going right to the official who has to implement any changes. With respect to method, holding a protest that turns violent is clearly more aggressive than signing a petition. While our compound treatment does not allow us to distinguish which of the three elements was critical, it provides a clear difference in how assertive the citizens' approach is understood to be in the Pakistani context.

Our sample is well balanced across conditions in the vignette experiment on a broad range of geographic, demographic, and attitudinal variables. The difference in means between the groups within a region therefore provides an estimate of how effective/acceptable citizens think the use of assertive action is to pressure their government officials.

Demographic Controls

We include the following demographic controls: gender, a head of household indicator, age, a literacy and basic numeracy competency indicator, education, a Sunni indicator, and an index of religious practice.

C.2 Empirical Strategy

Our estimation strategy at the individual level is to use fixed effects and respondent-level controls to isolate the effect of local variance in flood impact that is unrelated to average flood risk. At the individual level, we estimate the model with tehsil fixed effects, the third level administrative subunit in Pakistan, thereby exploiting variation in flood effects at the household level within tehsils. Our choice of this strategy is motivated by discussions with those involved in flood relief who cited great within-village variance. Surveys done to assess post-flood recovery needs also showed there could be huge variation in damages suffered at the household level that were not anticipated (Kurosaki et al., 2011), likely due to minor topographical features that impacted flow rates, how long areas were submerged, and so on.

For the the political knowledge index our estimating equation is therefore a fixed-effect regression

$$Y_i = \alpha + \beta_1 F_i + \beta_2 R_i + \gamma_d + \mathbf{B}\mathbf{X_i} + \epsilon_i, \tag{1}$$

where F_i is one of our flood exposure measures, R_i is the UNEP measure of *ex ante* flood risk, γ_d is a district fixed-effect for objective flood treatments (i.e., the measure of tehsil-level flood exposure derived from the UNOSAT data) and a tehsil fixed-effect for subjective flood exposure measures (i.e., self-reports), and $\mathbf{X_i}$ is a vector of demographic and geographic controls to further isolate the impact of idiosyncratic flood effects by accounting for the linear impact of those variables within tehsils. We cluster standard errors at the primary sampling unit level when analyzing survey data.

For the vignette experiment our measurement approach leverages a difference-in-difference estimator to answer the following question: given that people are generally opposed to the assertive vignette, is the difference in reactions between the two vignettes smaller for people in areas exposed to the flooding? To answer that question we need to control for a range of potential confounding variables. For example, is it possible that the land in districts close to rivers (which are most likely to be flooded) is less desirable and so people living there tend to be poorer and more marginalized, which one could argue would make them more willing to support aggressive protest. In the Pakistani context this is unlikely since land near the rivers is actually more fertile, which may be why population density is substantially higher near major rivers. This observation, however, raises the possibility that people who are more likely to be affected by floods would tend to be wealthier and less marginalized. In either case, we risk confounding flood exposure with a more fixed characteristic of the region and the people who reside there. We cannot completely overcome this challenge because we only have survey data from a single cross section, but by including a broad range of controls and examining subsets of the data we can gain increased confidence in the results.

We therefore estimate the following as our core specification for analyzing the vignette experiment:

$$Y_i = \alpha + \beta_1 A_i + \beta_2 F_i + \beta_3 (A_i \times F_i) + \gamma_d + \mathbf{B}\mathbf{X_i} + \epsilon_i, \tag{2}$$

where i indexes respondents. The outcome, Y_i, represents a response to either the effectiveness or approval question. The key treatment variables are A_i, an indicator for whether an individual received the assertive protest vignette, and F_i, a respondent's flood exposure (either objectively measured or self-reported). To control for locality-specific propensities to express approval or perceived effectiveness, we include γ_d, a district fixed effect when F_i is based on objective tehsil-level measurements and a tehsil fixed-effect when F_i is measured with individually reported flood exposure. $\mathbf{X_i}$ is a vector of demographic and geographic controls to further isolate the impact of idiosyncratic flood effects. We again cluster standard errors at the PSU level since that is the level at which the vignette was randomized.[3]

The estimate of β_3 in these equations isolates the causal impact of the flood to the extent that: (1) which vignette a respondent got was exogenous to their political attitudes; and (2) how exposed one was to the floods depended on factors orthogonal to pre-existing political factors once we condition on district-specific traits and the geographic controls. The first condition is true due to random assignment of the survey treatment. The second condition is likely to be met because the floods had a large random component and because we are controlling for the obvious factors that could have been used *ex ante* to predict which areas were likely to be flooded.

[3]Results are robust to clustering at the district level to account for the possibility that the variance in attitudes is highly correlated within districts as well as within PSUs.

C.3 Results

The floods clear increased both political knowledge acquired 7-14 months after the floods as well as support for assertive politics.

As Table C.1 highlights, we find clear evidence of behavioral change among flood-affected individuals in so far as they seem to have invested more in acquiring political knowledge. Our main index of political knowledge is increasing across both the UNOSAT data on flood exposure and self assessments of flood impact. The effects are statistically quite strong and substantively meaningful. A one standard deviation increase in the proportion of the population affected by the floods in the surveyed teshils (.165) predicts a .022 increase in the additive knowledge index and a .072 increase in the PCA index, both approximately .1 standard deviation treatment effects. For self-assessed flood exposure movement from the bottom of the scale to the top was associated with a .13 standard deviation increase in the additive index and a .16 standard deviation increase in the PCA index.

Table C.1: Impact of Flooding on Political Knowledge

	Additive Index		**PCA Index**	
	(1)	(2)	(3)	(4)
% Pop. Exposed	0.136		0.438	
	(0.041)		(0.138)	
Flood Exposure (4-pt Likert Scale)		0.029		0.120
		(0.012)		(0.040)
District FE	X		X	
Tehsil FE		X		X
R-Squared	0.379	0.407	0.375	0.404
Observations	10925	10925	10925	10925
Clusters	1058	1058	1058	1058

Notes: Unit of observation is an individual. All regressions include individual level controls for gender, head of household, age, reading and mathematical abilities, education, sunni, and intensity of religious practice. Columns 1 and 3 also include geographic controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation calculated at the tehsil level. Standard errors are clustered at the survey's primary sampling unit and reported in parentheses. Subjective flood exposure scaled to be in .25 to 1.

Turning to the vignette experiment, we find that citizens exposed to floods are significantly more supportive of assertive approaches towards demanding public services and believe them to be more effective than their non-exposed counterparts, as Table C.2 shows. The first coefficient in each model, β_1 from Equation 2, measures the difference in the outcome variables between the assertive and passive vignettes for people who scored a zero on the flood exposure measures. Those who received the assertive vignette but did not experience flooding rated the effectiveness of Junaid's actions between .23 and .3 units lower on a 0-1 scale controlling for a broad range of geographic and demographic controls, a movement of more than .5 standard deviations in all models. That effect is consistent across objective (Column 1) and self-reported measures of flooding (Column 2). We find results of similar magnitude for the approval dependent variable (Columns 3 and 4).

Exposure to the flood substantially and significantly decreased this disapproval. The coefficient on the third variable in each model, β_3 from Equation 2, indicates the moderating impact of flood exposure on the effect of the assertive vignette. A one standard deviation movement in the proportion of the population exposed in tehsils with non-zero flood exposure (.17) corresponds to a .16 standard deviation increase in perceived effectiveness of the assertive approach and a .18

standard deviation increase in approval for it. High levels of flood exposure

To benchmark these results we can consider the relationship between the vignette response and gender. Existing research has shown that men are on average more likely to have more assertive attitudes in the context of normal social relations (e.g., Funk et al., 1999) and tend to have more extreme views in some political settings involving violent contestation (e.g., Jaeger et al., 2012). The difference in perceived effectiveness of the assertive vignette between men and women is approximately .08, which equates to a .2 standard deviation movement in effectiveness, and the difference in approval is of similar size (.07). The difference between those affected by the flood and those who were not in terms of approval is thus slightly smaller than the gender difference in the approval of assertive action. The gender difference in perceived effectiveness and approval across the two conditions is even smaller, roughly .06 for effectiveness and .04 for approval, both of which are substantially smaller than all the flood coefficients.

Drawing on prior work we can also compare the flood affects to differences across attitudes towards Islamist militants' political positions. Following Fair, Malhotra and Shapiro (2012), we measured individuals' support for five political positions espoused by militant Islamist groups and combined these in a simple additive scale ranging from 0 to 1. Moving from 0 to 1 on this scale equates to a .21 increase in approval for the assertive vignette and a .11 increase in effectiveness. The impact of a one standard deviation move in flood exposure is similar in terms of approval of the assertive vignette to the difference between people who agree with none of the Islamist policy positions and those who agree with all five (and is much larger on the effectiveness measure), which indicates a substantively significant shift.

Interestingly, the results are not a proxy for satisfaction with flood relief. We asked respondents "In your opinion, did the government do a good or bad job in responding to the floods after they occurred?" on a four-point scale ranging from "very bad" to "very good" with no midpoint so respondents were forced to assign a direction to their views of the government response. Respondents' feelings about the assertive vignette are not consistently correlated with how they believe the government did in responding to the floods. For some measure of flood effects the difference-in-difference is larger among the 5,188 respondents who felt the government did a poor job of responding to the floods (about 50% of the sample after controlling for individual level covariates), while for others it is higher among the 5,171 respondents who felt the government did a good job. Clearly we cannot interpret the lack of a difference as falsifying a causal relationship between the quality of government response and attitudes on the vignette. Individuals who rate the government response poorly may do so because they have some unobservable difference that also makes them more approving of assertive protests to gain political services. Nevertheless, the fact that there is no consistent correlation suggests that the floods affected attitudes through some channel other than satisfaction with government performance.[4]

The finding that flood victims approve of assertive protests and believe they are more effective in getting a government response is quite robust. One might be concerned, for example, that there is unobserved heterogeneity between tehsils which is driving these results. To account for this possibility and to exploit the substantial within-village variation noted by many observers (Kurosaki et al., 2011), we estimated the impact of self-reported flood measures on the vignette experiment including tehsil fixed effects (Columns 2 and 4). The results on self-reported flood effects are about the same when we use fixed effects to account for any tehsil-level variance in flood impacts and other potential tehsil-level confounders.

Overall, it appears clear that citizens hit hard by the floods developed more assertive attitudes about demanding government services and invest more in acquiring political knowledge, but they do

[4]Results available from authors.

not swing to either the national ruling party at the time of the flood or the main opposition party. These changes could none-the-less shift politicians' incentives. It is easy to imagine a situation in which an exogenous increase in citizen attention leads politicians on all sides to exert increased effort, leading to increased turnout but leaving equilibrium vote shares unchanged.

Table C.2: Impact of Flooding on Approval and Perceived Efficacy of Aggressive Protest

	Effectivness		**Approval**	
	(1)	(2)	(3)	(4)
Aggressive Vignette	-0.226	-0.231	-0.243	-0.250
	(0.018)	(0.019)	(0.018)	(0.019)
% Pop. Exposed	0.010		-0.043	
	(0.106)		(0.112)	
Aggressive × % Flooded	0.325		0.366	
	(0.089)		(0.091)	
Flood Exposure (4-pt Likert Scale)		0.023		-0.018
		(0.026)		(0.029)
Aggressive × Exposure		0.140		0.156
		(0.035)		(0.038)
District FE	X		X	
Tehsil FE		X		X
R-Squared	0.258	0.331	0.281	0.352
Observations	10761	10761	10761	10761
Clusters	1055	1055	1055	1055

Notes: Unit of observation is an individual. All regressions include individual level controls for gender, head of household, age, reading and mathematical abilities, education, sunni, and intensity of religious practice. Columns 1 and 3 also include geographic controls for ex ante UNEP flood risk, distance to major river, dummy for constituencies bordering a major river, std. dev. of the constituency's elevation, mean constituency elevation calculated at the tehsil level. Standard errors are clustered at the survey's primary sampling unit and reported in parentheses.

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