The Effect of Disaster Relief on Climate Adaptation: Evidence from Floods in Pakistan*

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Abstract

Extreme weather events are expected to increase with climate change. Government relief programs are designed to ameliorate the negative consequences, but moral hazard models suggest they also reduce adaptation, such as migrating from disaster-prone areas. Using difference-in-differences, we study the long-term effects of cash relief on migration after the 2010 Pakistan floods. Combining survey and population data, we show that cash transfers have two countervailing effects. As expected, they reduce migration through a moral hazard effect and by facilitating in-situ adaptation. However, they also increase migration by providing liquidity. In practice, these effects cancel each other out in flooded areas.

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1 Introduction

Almost 3.4 billion people—about 42 percent of the world's population—live under a significant risk of natural disasters, with a vast majority living in low- and middle-income countries (Dilley, 2005; IPCC, 2023). Climate change is expected to intensify these risks through more erratic rainfall, sea-level rise, and accelerated glacial melt (Rodell and Li, 2023). For centuries, migration has been one of the main ways communities have adapted to climate risk. In 2022 alone, 32.6 million people migrated internally due to climate-induced natural disasters (IDMC, 2023).

In response to disasters, governments and policymakers increasingly rely on cash transfers to provide relief. Cash is often preferred over other methods because it can be disbursed quickly, carries relatively low administrative costs, preserves recipient agency, and supports local market recovery (Jeong and Trako, 2022; Asia Pacific RCWG, 2023). A growing concern, however, is that cash relief may create expectations of future assistance, thereby reducing households' incentives to adapt by relocating to safer areas (Shughart, 2011).

Theoretically, the effect of disaster relief on migration is ambiguous. It may discourage relocation by raising expectations of future aid or by enabling in-situ adaptation, yet also facilitate it by relaxing liquidity and risk constraints—key barriers to mobility in low-income settings (Bryan et al., 2014; Gazeaud et al., 2023). This paper provides new empirical evidence on how post-disaster cash relief shapes migration in the context of floods.

We study this question in the setting of the 2010 floods in Pakistan—one of the worst in the country's history. In response, the government launched the Watan Card program, providing cash relief of up to PKR 60,000 (USD 705; USD 2,742 PPP), equivalent to 3.2 months of average rural household income. All households in a village (mauza/deh/revenue village) were eligible if more than 50 percent of its land area was inundated by the flood. Importantly, relief was delivered through Visa debit cards, which could be reloaded in the event of future disasters. Thus, the program created an institutional infrastructure for disaster relief payments beyond one time relief.

To examine long-term effects of cash relief on migration, we construct a novel village-level panel dataset spanning seven decades (1961-2023). This involves digitizing and linking historical census records to generate population and infrastructure estimates, and digitizing hand-drawn *patwari* (local revenue official) maps to spatially locate villages across Sindh, Pakistan's second most populous province with more than 55 million residents.

Our empirical strategy employs a difference-in-differences (DiD) design. Although eligibility was formally tied to the 50 percent inundation threshold, actual relief allocation exhibited substantial mistargeting due to political clientelism and local need. We estimate 38 percent inclusion error (relief to villages with $< 50\%$ flooding) and 34 percent exclusion error (no relief to villages with $> 50\%$ flooding), consistent with an independent evaluation. This misallocation created two useful comparison groups: flooded villages that did not receive relief, and non-flooded villages that did. We compare population growth in villages that received relief to those that did not, conditional on flood exposure. For villages in each stratum (flooded and non-flooded), we estimate standard DiD with uniform weights and synthetic DiD with optimal weights to construct control groups that closely match pre-treatment trends.

Identification rests on the parallel trends assumption—that in the absence of cash relief, population growth would have evolved similarly across cash and non-cash villages. We assess its plausibility by examining pre-treatment population dynamics. In addition, we compile data on village infrastructure and pre-flood provincial elections to study determinants of cash relief allocation. In flooded areas, relief was more likely in less developed villages, suggesting targeting partly reflected need. In non-flooded areas, however, relief was more common in villages with greater public goods provision, consistent with political favoritism. Overall, villages located in constituencies won by the incumbent Pakistan People's Party (PPP) were more likely to receive cash relief. These correlates could bias estimates only if their relationship with population growth evolves differentially across cash and non-cash villages. We show our findings are robust to controlling for these factors and to stricter specifications, including district by time fixed effects and village-specific linear trends.

We find that floods are associated with a 17 percent decline in population, consistent with existing literature. Cash relief has heterogeneous effects depending on flood exposure. In non-flooded areas, it leads to a 6.8-8.1 percent decline in population, suggesting that cash enables out-migration by magnitudes comparable to those found in other settings. The declines are concentrated among adult males with primary or secondary education, non-Muslims (who are more likely to be landless), and in lower-income villages. In flooded areas, by contrast, cash has a small positive (not statistically significant) effect, with no systematic demographic heterogeneity.

To interpret these patterns, we develop a simple model of households' migration decisions. Households compare the utility of staying versus migrating, weighing perceived flood risk, expected damage from floods, the likelihood of future relief, and migration costs. A flood raises perceived risk of future floods, pushing households to migrate. Cash relief reduces migration cost by easing liquidity and risk constraints, thus encouraging migration. However, it also raises the value of staying by increasing expectations of future relief and by financing in-situ adaptation that reduces expected flood damage. For households receiving cash without a flood, perceived future flood risk is low, so the liquidity effects dominate and, thus, cash encourages out-migration. However, when households receive relief after a flood, perceived flood risk also rises, which amplifies the option value of staying (via increased expectation of future relief and in-situ adaptation), offsetting liquidity effects. Thus, the model can explain why cash leads to out-migration in non-flooded areas and has no effect on population in flooded areas.

Survey evidence supports the model. Using two post-flood household surveys (from 2010 and 2022), we show that cash recipients are 6 pp (17.2%) more likely to believe that the government will provide future cash relief in the event of floods, and are 33 pp (339%) more likely to upgrade their house to be more flood resilient. Moreover, flood-affected households report a 27 pp (64.2%) higher subjective likelihood of future floods.

We contribute to three strands of literature. First, we contribute to the literature on the economics of climate change adaptation, which examines the incentives and constraints households face when responding to climate risks. Prior work has emphasized barriers such as imperfect beliefs, weak safety nets, and housing market frictions (De Mel et al., 2012; Burgess et al., 2017; Macours et al., 2022; Patel, 2023; Kala et al., 2023). While access to credit and insurance can facilitate adaptation (Karlan et al., 2014;

Lane, 2022), some studies have raised concerns that disaster relief may reduce incentives for private adaptation (Henkel et al., 2022; Pang and Sun, 2022), though these focus on high-income settings. To our knowledge, this is the first paper to estimate the effect of disaster relief on migration in a developing-country setting. Conceptually, our work is closest to Hsiao (2023), who document moral hazard from sea wall construction in Indonesia; we study a similar concern in the context of ex-post cash relief.

Second, we contribute to the disaster-relief design literature by examining the effects of ex-post, geographically targeted cash transfers on migration. Prior work debates when to deliver aid (ex-ante vs. ex-post), how to deliver it (cash vs. in-kind), and whom to target (geographic, damage, or community-based) (Basurto et al., 2020; Pople et al., 2021; Tarquinio, 2022; Mahadevan and Shenoy, 2023; Gadenne et al., 2024). Geographic targeting is often argued to be fast and cost-effective and to reach households with a high valuation of consumption smoothing (Gordon et al., 2024). Cash is frequently found to preserve recipient agency and support local market recovery (Jeong and Trako, 2022). We evaluate a potential unintended consequence: that disaster relief generates moral hazard by encouraging households to remain in risk-prone areas. We provide evidence that such moral hazard exists, but for the ex-post geo-targeted cash we study, it is balanced by a liquidity effect: cash relaxes credit constraints and facilitates migration. Although we do not estimate counterfactual designs, our model implies that moral hazard effects are likely to be stronger for damage-based targeting due to the requirement of on-site presence and under in-kind assistance due to lower liquidity effects.

Third, we contribute to future research on Pakistan by building a novel village-level panel dataset. Pakistan has been widely studied for colonial legacies, education and health reforms, and climate adaptation (Bharadwaj et al., 2015; Andrabi et al., 2017; Malik and Mirza, 2022; Asad et al., 2024; Dipoppa and Gulzar, 2024). Yet most empirical work relies on coarse administrative units (districts or *tehsils*), which mask substantial local heterogeneity: the average district contains 300+ villages, and the average tehsil about 80. Conducting analysis at the village level has long been infeasible due to the absence of digital village boundary maps, inconsistent village names across datasets, and a lack of digitized historical data. We address these gaps by digitizing *patwari* maps to create a geospatial dataset of village boundaries, and by digitizing and linking population and infrastructure censuses—using fuzzy matching—to assemble a panel spanning seven decades of population (disaggregated by gender, education, age, and religion) and three decades of infrastructure. Our dataset is analogous to India's village-level SHRUG data by Asher et al. (2021) but focuses on Pakistan and covers a longer historical period. It enables fine-grained research on historical legacies, local development, and climate resilience and—together with SHRUG—creates opportunities for spatial discontinuity designs along the India–Pakistan border.[1]

The remainder of the paper is organized as follows. Section 2 provides background on the 2010 floods and the subsequent relief program. Section 3 describes the data, and Section 4 the empirical strategy. Section 5 reports the main results, Section 6 presents a theory to rationalize the findings along with supporting empirical evidence. Section 7 concludes.

[1] The dataset will be made publicly available with the publication of this paper. An interactive dashboard is available here: `https://muhammad-binkhalid.github.io/pakcensus-panel/`.

2 Context

2.1 Floods

Flooding is a regular and widespread hazard in Pakistan, accounting for nearly half of all natural disasters in the country (Larsen et al., 2014). While some degree of flooding occurs almost every year, severe floods are relatively rare. The 2010 floods were the most severe up to that point, affecting over 20 million people (11 percent of the population) and causing economic losses of around USD 10 billion (6 percent of GDP) through the destruction of homes, crops, and livestock (World Bank, 2010). The floods were triggered by extraordinarily high monsoon rainfall in the upper Indus basin (northwestern Pakistan), which caused the Indus River and its tributaries to overflow. Sindh, located downstream in the Indus River system, was the most severely affected province, accounting for 44 percent of total damages and 37 percent of the affected population (World Bank, 2010).

Migration is a common ex-post adaptation strategy to floods. Most affected households engage in temporary migration, typically lasting weeks or months. Evidence of permanent migration is more limited, though several studies document modest positive effects of floods on permanent relocation (Kirsch et al., 2012; Patel, 2024).

2.2 Cash Relief Program

In response to the 2010 floods, the Government of Pakistan launched the Watan Card program to deliver cash relief in affected areas. The program was implemented in two phases. In the first phase, the government disbursed USD 400 million among 1.62 million households, with each household receiving a transfer of PKR 20,000 (USD 235; USD 914 PPP). All households in villages designated as calamity-affected were eligible. In the second phase, households identified as damaged in a household survey received an additional PKR 40,000 (USD 470; USD 1,828 PPP). In total, the transfer amounted to 56 percent of the damage suffered by an average household in a flooded village.[2]

The combined transfer was equivalent to 3.2 months of average rural household income and could finance about 2.5 months of expenditure for an average urban household (Pakistan Bureau of Statistics, 2011). Compared to other relief programs, the Watan card transfers were relatively generous. After adjusting for inflation, they were about six times larger than the cash relief provided after the 2022 floods and 2.5 times larger than the amount transferred under the Benazir Income Support Program (BISP) in a year.

A key feature of the program was that cash was disbursed through Visa debit cards from which beneficiaries could withdraw the amount at a bank. Rather than one-off transfers, the government issued reusable debit cards that could be topped up in the event of future floods, and politicians made public commitments of future top-ups (Semple, 2011). Over time, an informal market for Watan cards emerged, with some recipients selling them at discounted prices to access cash immediately (DAWN, 2010b). This highlights that while the program provided short-term relief, it also established a lasting payment infrastructure for future relief disbursement for households living in flood-prone areas.

[2] See Appendix A.2.1 for calculation details.

A key weakness, however, was substantial mistargeting. To designate villages as calamity-affected, the government relied on *patwaris* (local revenue officials) to visually assess flood damage. Officially, a village was to be classified as affected if more than 50 percent of its land area was submerged. In practice, implementation was marred by errors, inconsistencies, and political interference. An independent evaluation by Oxford Policy Management concluded that "for every 100 potentially eligible family heads (flood-affected), only 43 received the Watan card" (Hunt et al., 2011). Our own satellite-based estimates suggest that 38 percent of villages receiving relief were not more than 50 percent flooded (inclusion error), while 34 percent of villages that were more than 50 percent flooded were excluded (exclusion error).[3]

To explore patterns in this mistargeting, we compare pre-flood characteristics of villages that did and did not receive cash relief, using the 2008 infrastructure census and electoral results. Appendix Table A.2 shows that among flooded villages, those receiving relief had lower access to public goods and were generally less developed, suggesting that targeting partly aligned with need. In contrast, among non-flooded villages, those receiving relief were more developed, with better public infrastructure, consistent with political favoritism. Panel C of the table, using 2008 electoral data, shows that overall relief was more likely in areas where the Pakistan People's Party (PPP)—the ruling party—had won the provincial assembly seat or had a higher vote share, though these differences are not statistically significant. Overall, the evidence points to a mix of need-based targeting in some flooded areas and politically motivated allocations elsewhere.

3 Data

The cash relief program was implemented at the village level. Since no public shapefile of villages exists for Pakistan, we digitized hand-drawn *patwari* maps for all 5,294 villages in Sindh. Figure 1 shows the village shapefile, Appendix Figure A.2 illustrates how an individual village map was digitized and fitted into the map of Sindh, and Appendix A.1.4 provides more details.

3.1 Floods and Cash Relief

We use UNOSAT's maximal flood extent layer to measure inundation in each village. Following the official rule, a village is classified as flooded if more than 50 percent of its area was inundated; Appendix Table A.4 tests robustness to alternate thresholds and a continuous flood measure.

Cash-relief villages were identified from the official government notification. We matched village names using fuzzy string matching on village, tehsil (sub-district), and district names. We applied four sequential matching criteria: (1) match within tehsil only; (2) allow matches within districts only (accommodating tehsil boundary changes); (3) allow village matches in districts where other matches have occurred (accommodating district boundary changes); and (4) match without any geographic restrictions. These criteria matched 781 (53%), 886 (61%), 1,010 (70%), and 1,027 (70%) villages from the notifi-

[3] There were also widespread news reports about political influence (DAWN, 2010a).

cation, respectively. We use criterion 3 as our preferred specification and test robustness to alternatives in Appendix Table A.5.

Subfigure (a) in Figure 1 shows a map of villages by their flooding and cash relief status, highlighting inclusion errors (not flooded but received cash) and exclusion errors (flooded but no cash).

3.2 Population and Infrastructure

We proxy migration using population changes over time. Village population estimates come from the 1961, 1972, 1981, 1998, 2017, and 2023 population censuses, while infrastructure indicators are drawn from the 1993, 1998, 2003, 2008, and 2020 infrastructure censuses. We used deep learning techniques developed by Shen et al. (2021) to digitize the pre-2000 census waves. We then matched the censuses using fuzzy string matching with manual verification, both across all census waves and to the village map (see Appendix A.1.1 for more details). Out of 5,294 villages in Sindh, we successfully matched 4,404 (83.2%) across all census waves and the village map.

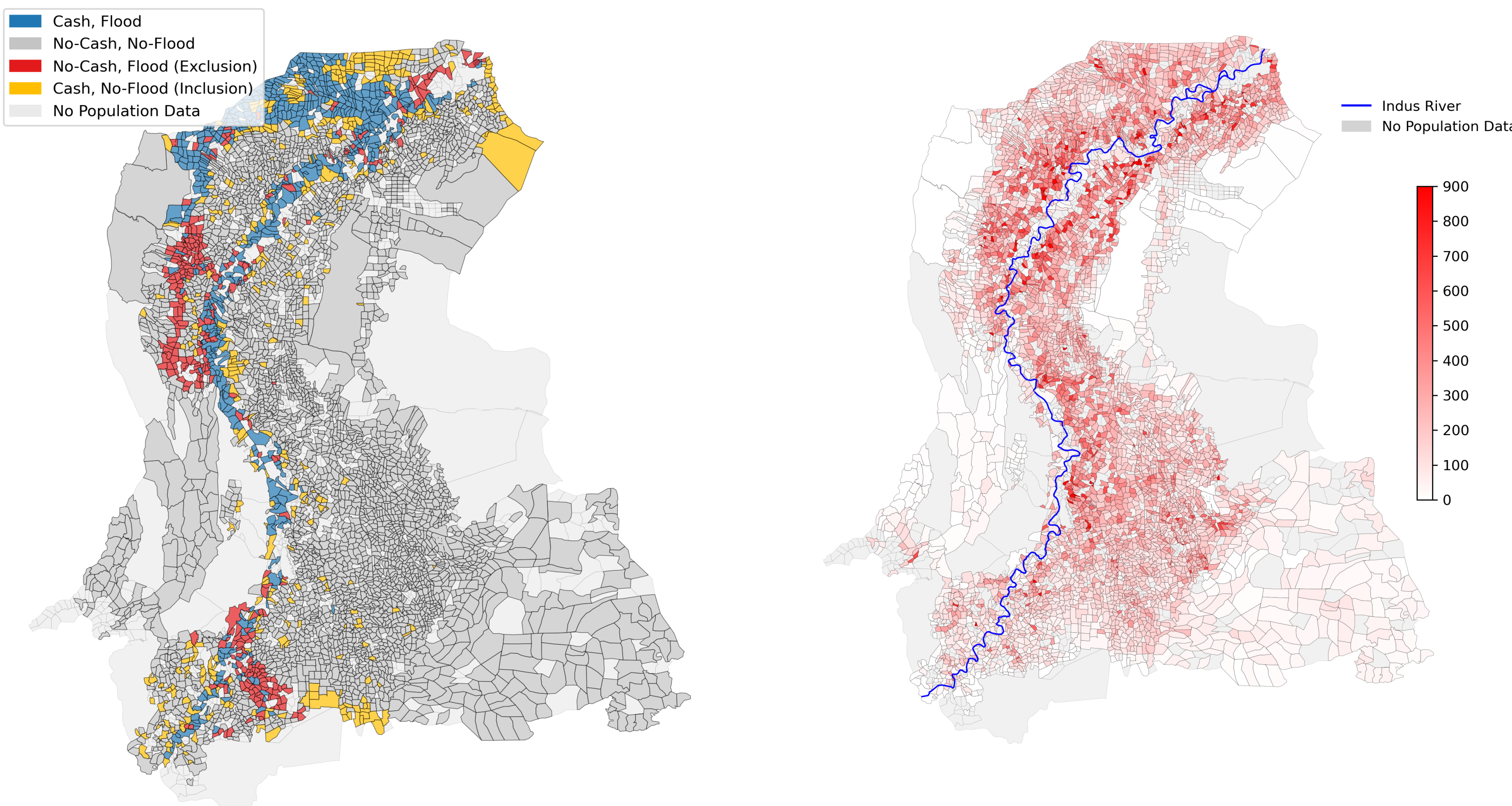
Subfigure (b) in Figure 1 displays the population density data from 1998 as a heat map.

3.3 Post-Flood Household Surveys

We use two household surveys to study links between cash relief, expectations of future support, in-situ adaptation investments, and perceived flood risk. The Pakistan Rural Household Survey (PRHS) covered 2,090 households in 76 randomly selected villages about 1.5 years after the 2010 floods (IFPRI, 2014). It collected data on flood exposure, relief, coping strategies, and beliefs about future support. In addition, we use the World Bank's 2024 survey of 4,000 households from flood-affected areas in Sindh conducted two years after the 2022 floods, which collected data on similar topics.

Figure 1: Maps of Floods, Cash Relief, and Population Density

(a) Floods and Cash Relief **(b)** Population Density (1998)



Notes: This figure displays the digitized village shapefile. Panel (a) categorizes villages by their flood exposure and cash status. Panel (b) presents a heat map of 1998 population density (people per square kilometer), winsorized at the 99th percentile. The path of the Indus River is overlaid in blue. In both panels, villages with missing population data are shaded light gray.

4 Empirical Strategy

Mistargeting in the cash-relief program (inclusion/exclusion errors) creates variation in relief allocation. In particular, it yields two useful comparison groups: (i) flooded villages that did not receive relief (34% of flooded villages), and (ii) non-flooded villages that nevertheless received relief (8% of non-flooded villages). We exploit this variation in a DiD framework to estimate the effect of cash relief on population growth, conditioning on flood exposure.

Specifically, we estimate the following regression,

$$\ln(Pop_{it}) = \beta_0 + \beta_1\, post_t \times cash_i + \beta_2\, post_t \times cash_i \times flood_i + \beta_3\, post_t \times flood_i + \tau_t + \delta_i + \epsilon_{it} \quad (1)$$

where $\ln(\text{Pop}_{it})$ is log population in village i at census year t; post_t equals 1 for post-2010 censuses; flood_i equals 1 if more than 50 percent of the village area was inundated; and cash_i equals 1 if the village received cash relief. We include census-year fixed effects τ_t to capture overall population growth, and village fixed effects δ_i to control for time-invariant characteristics that may affect a village's population, such as climate, proximity to the river, and baseline population. Our coefficients of interest are β_1 and $\beta_1 + \beta_2$, which capture the effect of cash relief in non-flooded and flooded areas, respectively.

To improve precision and pre-treatment balance, we also estimate equation (1) using the Synthetic DiD estimator of Arkhangelsky et al. (2021). Instead of applying uniform weights to all non-cash villages, it applies optimal weights to construct a synthetic control whose pre-treatment population growth closely matches that of the cash villages.

The identifying assumption is that, absent relief, treated villages and their controls (or synthetic controls) would have followed the same population trends (parallel trends). We do not assume that cash relief program was randomly assigned. In fact, we show in Appendix Table A.2 that it is correlated with some pre-flood village characteristics. For an omitted variable to bias our estimates, it would have to be correlated with cash relief and with population growth and, critically, to evolve differentially for cash and non-cash villages.

To assess the plausibility of this assumption, we compare pre-2010 population growth between cash and non-cash villages. Specifically, we estimate the following event-study (dynamic) specification:

$$\ln(Pop_{it}) = \beta_0 + \sum_{t=1961}^{1981} \gamma_t \times cash_i + \sum_{t=2017}^{2023} \beta_t \times cash_i + \tau_t + \delta_i + \epsilon_{it} \quad (2)$$

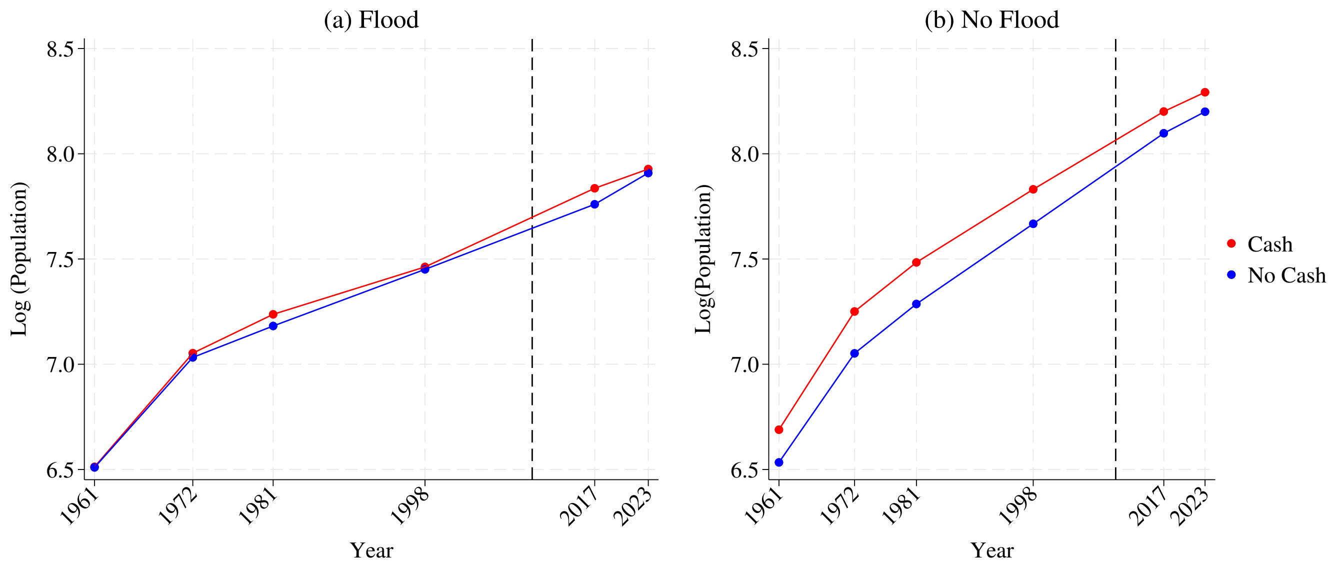
where γ_t captures pre-treatment differences in population growth relative to the omitted year (1998). The coefficients β_t estimate the effect of cash relief in each post-treatment year. We estimate this equation separately for flooded and non-flooded villages, using both standard and synthetic DiD methods.

5 Results

We first present the main effects of cash relief on population change in flooded and non-flooded villages, then show robustness to alternative specifications, and finally explore heterogeneity.

5.1 Main Results

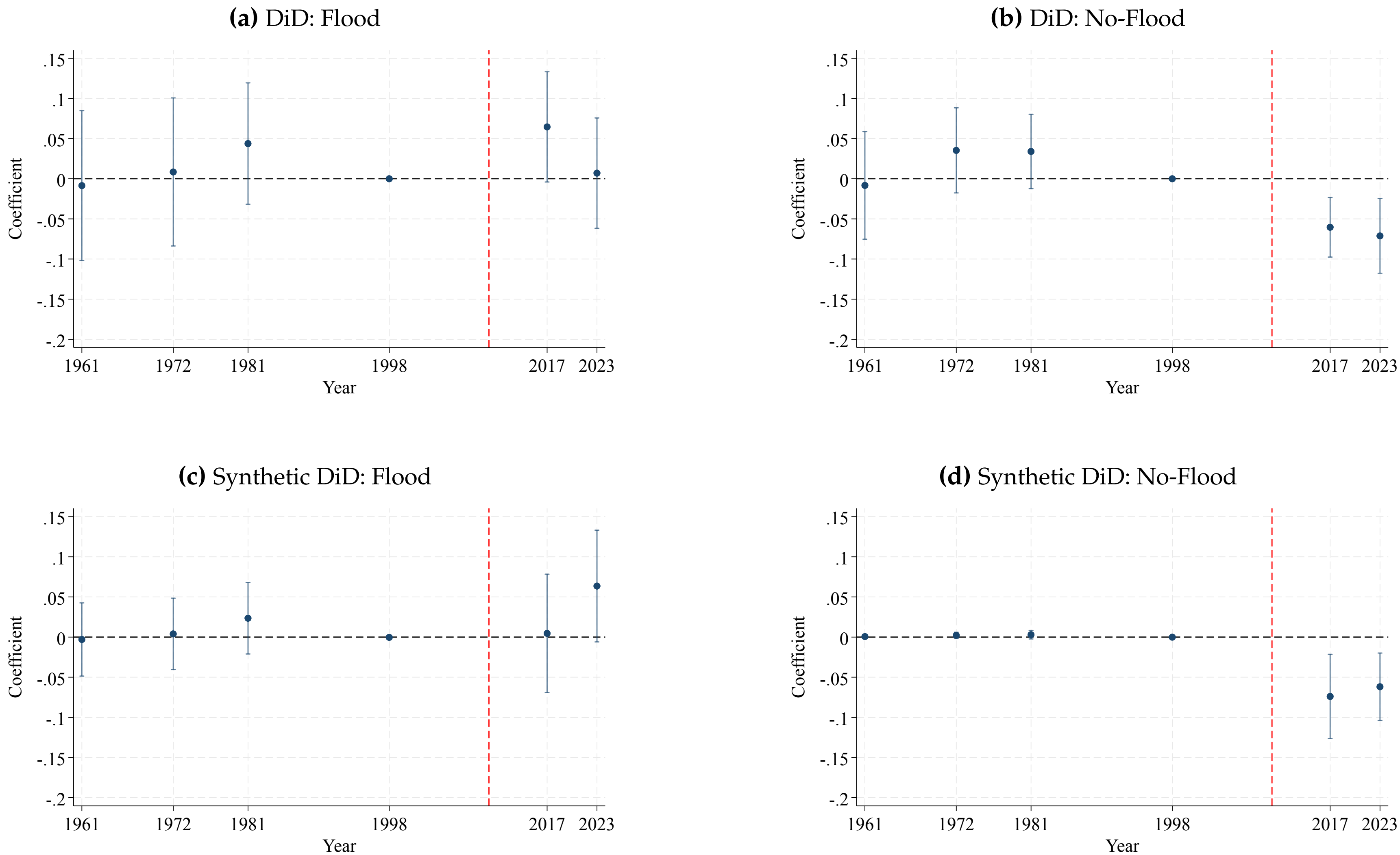
Figure 2: Trend Plot (Population) by Cash Status



Notes: This figure plots population growth (in natural logarithms) in villages that received cash relief and those that did not. Panel A shows trends for flooded villages, while Panel B shows trends for non-flooded villages.

Figure 2 plots population growth (in logs) in cash and non-cash villages, separately by flood status. Between 1961 and 2023, population increased nearly fivefold, from a mean of 930 to 4,594. Growth was faster in non-flooded villages. Cash villages consistently had higher populations than non-cash villages—particularly in non-flooded areas—a gap that predates the floods and is consistent with pre-flood balance patterns. Importantly, we do not observe any visible divergence in pre-flood trends between cash and non-cash villages, which supports the parallel-trends assumption. There is no sharp change in population growth after 2010, suggesting that any relief effects were modest relative to long-run demographic change.

Figure 3: Dynamic DiD Results



Notes: This figure presents dynamic DiD regression results. The outcome variable is log population. Each coefficient represents the interaction between the cash relief indicator and census year dummies. Panels A and C report estimates for flooded villages, while Panels B and D report estimates for non-flooded villages. Panels A and B use standard DiD, whereas Panels C and D use synthetic DiD. All regressions control for census year and village fixed effects. Standard errors are clustered at the village level.

Figure 3 presents dynamic DiD estimates that remove the long-run demographic trend, using both standard DiD (Panels A–B) and synthetic DiD (Panels C–D). In flooded areas (Panels A and C), cash relief has no substantial effect at any point after the program. In non-flooded areas (Panels B and D), cash relief leads to a population decline after 2010. SDiD estimates are based on an optimally weighted control group, which leads to precisely estimated null coefficients in pre-treatment period.[4] Across both estimators, the pre-2010 coefficients are statistically indistinguishable from zero in both flooded and non-flooded areas, supporting the parallel trends assumption.

Table 1: Main Results: Effect of Cash Relief on Population Growth

	Log(Population)			
Panel A: DID Estimates	(1)	(2)	(3)	(4)
Flood × Post	-0.175***	-0.223***		
	(0.024)	(0.035)		
Cash × Post	-0.043**	-0.081***	0.025	-0.081***
	(0.022)	(0.025)	(0.041)	(0.025)
Flood × Cash × Post		0.106**		
		(0.048)		
Panel B: Synthetic DID Estimates				
Cash x Post			0.034	-0.068***
			(0.030)	(0.020)
Sample	All	All	Flood	No Flood
Observations	25,230	25,230	4,584	20,646
Clusters	4,205	4,205	764	3,441
Time FE	✓	✓	✓	✓
Village FE	✓	✓	✓	✓

Notes: This table presents regression results from equation (1), with the log of population as the dependent variable. Panel A reports results using the standard DiD estimation, and Panel B reports results using Synthetic DiD estimation. Column 1 estimates the effect of cash relief on population, controlling for flood exposure. Column 2 adds an interaction between flood exposure and cash relief to assess the heterogeneous impact of relief in flooded versus non-flooded areas. Columns 3 and 4 restrict the sample to flooded and non-flooded villages, respectively, to estimate the effect of cash relief within each group. All regressions include village and census year fixed effects. Standard errors are clustered at the village level $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 1 reports the same results in regression form. Column 1 shows that both floods and cash relief are associated with reductions in population, with the effect of floods about four times larger than that of cash relief. Column 2 shows the heterogeneity revealed in Figure 3: in non-flooded villages, cash

[4]Bootstrap standard errors are used for the SDiD. They are smaller in Panel D because there is a large number of non-cash villages in non-flooded areas, which allow for close matches with the cash villages, reducing variance in placebo estimates. In contrast, the number of non-cash villages in flooded areas is much smaller, which limits match quality, increasing standard errors.

relief leads to an 8.1 percent decline in population, whereas having experienced a flood reverses the effect and makes it positive but small and statistically indistinguishable from zero. Columns 3 and 4 of Panel A confirm that the results are similar when estimating the effects separately on the flooded and non-flooded samples. Columns 3 and 4 of Panel B show that the results are similar when using SDiD estimation.

5.2 Interpretation of Results

Cash transfers can alter population through fertility or mortality, but prior work mostly finds null or positive fertility effects and negative mortality effects—together implying small net increases in population (Stecklov et al., 2007; Cowan and Douds, 2022; Richterman et al., 2023). Those channels therefore do not explain the overall negative effects nor the heterogeneity we observe across flooded and non-flooded villages. We therefore interpret our results as reflecting migration: floods spurred out-migration everywhere, while cash relief appears to have encouraged out-migration in non-flooded areas and had no detectable impact in flooded areas.

Our estimated 7–8 pp increase in migration after one-off cash relief in non-flooded areas is similar in magnitude to estimates from other one-off transfers (+6 pp in Indonesia; +5 pp in Zambia; +3 pp in Comoros) (Tiwari and Winters, 2019; Diop, 2025; Gazeaud et al., 2023). The effect is smaller than estimates for recurring pensions (+25–31 pp) or transfers explicitly tied to mobility (+22–25 pp) (Bryan et al., 2014; Eggleston et al., 2018).

Our data do not reveal destinations, but a 2012 post-flood household survey indicates that about 60 percent of migrants from cash villages moved to urban areas, consistent with prior evidence (Bryan et al., 2014; Diop, 2025).

5.3 Robustness Tests

5.3.1 Threats to Identification

Appendix Table A.2 shows that cash relief is correlated with local development and public goods provision. Less developed villages may have also attracted more NGO aid or government reconstruction programs, which could independently influence population growth. To address this, we add a time-varying index of development and public goods in Columns 2 and 4 of Appendix Table A.3. Since post-flood values of development and public goods may partly capture the treatment effect, we also interact pre-flood values with the post-treatment dummy and show the results in Columns 3 and 5. The results remain unchanged.

Cash relief is also correlated with alignment to the incumbent Pakistan People's Party (PPP). If politically aligned villages also received more reconstruction funds or other public goods, this could confound our estimates. Thus, in Column 6 of Appendix Table A.3, we interact the post-treatment dummy with a PPP victory indicator, but this does not change our results.

Since relief allocation decisions were made at the district level, villages in certain districts may have been more likely to receive aid due to better needs assessments, favorable politics, or larger budgets

(Kousky, 2010). To address this, in Column 7 of Appendix Table A.3, we show that our results are stable to including district-by-post-treatment fixed effects.

Finally, villages may have different underlying population trends. Column 8 of Appendix Table A.3 shows that our results are robust to controlling for village-specific linear time trends. Overall, Table A.3 shows that estimates remain stable across a wide range of controls. Following Altonji et al. (2005), this stability suggests omitted variable bias is unlikely to explain our results.

5.3.2 Alternate Flood Measures

In our main specification, we classify a village as flooded if more than 50 percent of its area was inundated, consistent with the official "calamity-affected" definition. Appendix Table A.4 tests thresholds of 30, 40, 60, and 70 percent (Columns 1–4), as well as a continuous flood measure (Column 5). Results are consistent across all measures.

5.3.3 Alternate Cash Relief Matching Criteria

The official notification for cash relief areas lists villages by district and tehsil. Our preferred specification (Criterion 3) matches villages within districts mentioned in the official notification, accommodating district and tehsil boundary changes. Appendix Table A.5 shows robustness to stricter rules (Criteria 1 and 2: within tehsil or within district only) and a looser rule (Criterion 4: no geographic restriction). Results are stable across all criteria.

5.4 Heterogeneity Analysis

We examine heterogeneity along four demographic dimensions—gender, age, education, and religion—in Appendix Tables A.6 - A.9. In non-flooded areas, cash relief reduces the male population more than the female population, lowering the male share by 0.3 pp and raising the female share correspondingly. It decreases the share of adults (18+) by 1.5 pp and increases the share of children. By education, the largest negative effect is among those with secondary schooling, followed by primary, while the share with no education increases slightly. Moreover, the largest negative effect is among non-Muslims (who are less likely to own land), whose population reduces by 0.3 pp (Minority Rights Group International). Overall, these patterns suggest that population effects in non-flooded areas are driven by migration, concentrated among groups with higher migration propensities in other contexts (Desai and Chatterjee, 2016; Bernard and Bell, 2018).

In flooded areas, effects are smaller and less systematic: we find no significant shifts in gender, primary education, or no-education shares. This muted pattern aligns with our main finding that cash relief did not significantly affect migration in flooded areas.

We also examine heterogeneity by pre-flood economic development and public goods provision (Appendix Table A.10). The negative effect of cash relief on population is larger in villages below the median on these indices, indicating that in non-flooded areas the migration response is stronger in poorer communities—consistent with a liquidity-constraint mechanism.

6 Theory and Mechanisms

The empirical analysis yields three main findings. First, floods are associated with substantial population declines. Second, cash relief has heterogeneous effects: it reduces population in non-flooded villages but has no detectable effect in flooded villages. Third, these effects are stronger for groups who generally have a higher propensity to migrate and are liquidity constrained. In this section, we present a simple theory that rationalizes these patterns and then provide supporting empirical evidence.

6.1 Theory

While there can be multiple channels through which floods and cash relief affect migration, we outline three core mechanisms: liquidity, moral hazard, and in-situ adaptation. While we use the examples of floods, we believe that the channels we highlight are applicable to a wide range of disasters.

6.1.1 Model Setup

Let $R^t \geq 0$ indicate the cash relief received in the current period, and R^{t+1} represent future cash relief. Let $\alpha_f \in [0, 1]$ be the perceived probability of a future flood and $\alpha_{nf} = 1 - \alpha_f$ be the perceived probability of no flood. Let $\pi_f(R^t)$ and $\pi_{nf}(R^t)$ be the perceived probabilities of receiving future relief conditional on flood/no-flood, respectively. Let $m \in [0, 1]$ denote in-situ adaptation (e.g., building a flood-resilient house), with convex cost $k(m)$. Let $D > 0$ be damage due to floods absent adaptation, W_r and W_u be rural and urban earnings.[5] Migration costs are $C(R^t)$ with $C'(R^t) < 0$, as cash relaxes liquidity/risk constraints.

The expected utility from staying is

$$U_S = W_r - k(m) + \alpha_f\big(\pi_f(R^t)\,R^{t+1} - D(1-m)\big) + \alpha_{nf}\big(\pi_{nf}(R^t)\,R^{t+1}\big), \tag{3}$$

and from migrating is

$$U_M = W_u - C(R^t). \tag{4}$$

An individual migrates if $\Delta \equiv U_M - U_S > 0$. We assume expected flood damage net of expected relief is positive: $D(1-m) > \pi_f\,R^{t+1}$.

6.1.2 Effect of the Disaster

Experiencing a flood increases the perceived probability of future floods ($\alpha_f \uparrow$). This reduces the expected utility of staying, and thus encourages migration. It is consistent with the empirical result that floods are associated with population decline.

A flood may also destroy location-specific assets such as a house and reduce economic opportunities in the village, but for simplicity, we do not include these in the model.

[5] We assume urban destinations, but the mechanisms apply equally to moves to higher opportunity rural areas.

6.1.3 Effect of Disaster Relief

Relief can affect migration through three channels:

(i) Liquidity. First, disaster relief, especially in the form of cash, reduces liquidity and risk aversion constraints, reducing migration cost. With $U_M = W_u - C(R^t)$,

$$\frac{\partial U_M}{\partial R^t} = -C'(R^t) > 0,$$

since $C'(R^t) < 0$. Hence, relief makes migration more attractive. One potential mechanism is that migration is a risky investment, and that cash provides liquidity and acts as an insurance against the worst-case scenarios if the migrant fails to find a job.

(ii) In-situ adaptation. Cash relief finances in-situ adaptations (m) like upgrading a house to be flood resilient, which lowers expected flood losses. The stay value shifts by

$$\left.\frac{\partial U_S}{\partial R^t}\right|_{\pi_f, \pi_{nf}} = \big(\alpha_f D - k'(m)\big)\, \frac{\partial m}{\partial R^t} \;\geq\; 0,$$

the term is weakly positive since households are utility maximizing and choose m to maximize U_S, so having access to more cash could not have a negative effect. However, households could be liquidity constrained, and cash may allow them to choose a level of m closer to the optimum. Moreover, for a given $\frac{\partial m}{\partial R^t}$, the marginal benefit is (weakly) increasing in perceived flood risk α_f—i.e., it is larger in high–risk areas.

(iii) Moral hazard. One instance of disaster relief may generate expectations of future relief ($\pi(R^t) \uparrow$). This reduces the incentive to leave by raising the expected value of staying - a classic example of moral hazard (Arrow, 1963; Goodwin and Smith, 1995). In our setting, very few non-flooded villages (8%) receive cash, so the likelihood of relief without a flood is almost negligible ($\pi_{nf}(R^t) \approx 0$). Hence, this channel is also strongest where perceived flood risk α_f is high.

Putting the channels together, the net effect on the migration margin $\Delta \equiv U_M - U_S$ is

$$\frac{\partial \Delta}{\partial R^t} = \underbrace{-C'(R^t)}_{\text{liquidity } >0} - \Big[\underbrace{\alpha_f\, \pi'_f(R^t)\, R^{t+1}}_{\text{moral hazard } \geq 0} + \underbrace{(\alpha_f D - k'(m))\, \frac{\partial m}{\partial R^t}}_{\text{in-situ adaptation } \geq 0}\Big]$$

which is *a priori* ambiguous. The liquidity term does not depend on perceived flood risk, whereas both "stay" forces (moral hazard and in-situ adaptation) increase with perceived future flood risk α_f. Hence, the effect will be larger (or less negative) when the perceived flood risk α_f is low. If α_f is sufficiently low (non-flooded areas), the liquidity effect will dominate, prompting out-migration; if α_f is high (flooded areas), the stay forces are larger, yielding smaller, zero, or even negative effect on migration.

Consistent with the model, we estimate a 7-8 percent population decline in non-flooded areas—indicative of increased out-migration—and a near-zero effect in flooded areas, implying that liquidity gains are offset by expectations of future relief and in-situ adaptation in practice.

6.2 Empirical Evidence

The theory hinges on three conditions: (i) cash relief eases liquidity/risk constraints, enabling migration—a relationship supported by the heterogeneity analysis and well documented in prior work (Bryan et al., 2014; Tiwari and Winters, 2019; Gazeaud et al., 2023; Diop, 2025); (ii) cash raises expectations of future government support and/or finances in-situ adaptation; and (iii) perceived flood risk is higher in areas with past flood exposure. If only (i) holds, relief should raise out-migration everywhere. If (i) and (ii) hold but not (iii), the effect of relief will be directionally ambiguous but similar across flooded and non-flooded areas. When all three are present, we would expect the observed heterogeneity in the effect of cash relief.

Table 2: Mechanisms

	Invest in higher quality house	Govt will help in future shocks	Govt will give cash relief in future flood	Likelihood of flood in the future
	(1)	(2)	(3)	(4)
Cash Relief (2010)	0.332***	0.061**		
	(0.090)	(0.028)		
Flood (2010)		0.014		
		(0.023)		
Cash Relief (2022)			0.066***	
			(0.017)	
Flood (2022)			0.039*	0.267***
			(0.023)	(0.033)
Observations	718	4,238	4,001	4,013
Clusters	28	68	122	122
No-Cash Mean	0.098	0.355	0.398	
No-Flood Mean		0.368	0.407	0.416
Province FE	✓	✓	✓	✓
Data	PRHS 2012	PRHS 2012	WB 2024	WB 2024

Notes: This table examines the effect of cash relief on likelihood of upgrading a house, beliefs about future government support and the effect of floods on perceived future flood risk. In Column 1, the outcome variable is a binary for whether the household invested in upgrading the house quality to pacca/semi-pacca (bricked) or not after 2010 floods. In Column 2, the outcome variable is a categorical variables, rescaled to [0,1], capturing the extent to which the respondent believes that the government will help in case of unexpected future economic shocks. In Column 3, the outcome variable is the extent to which the respondent believes that government will provide cash relief in case of a flood in the future, also rescaled to [0, 1]. In Column 4, the outcome variable is the perceived likelihood of future flood of the same scale as the 2022 floods, also rescaled to [0, 1]. Data for Columns 1 and 2 come from the Pakistan Rural Household Survey conducted two years after the 2010 floods; data for Columns 3 and 4 come from a World Bank survey conducted after the 2022 floods. The sample of households in Column 1 consists of only the flooded households, whereas for all other columns, it is the whole sample. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

Table 2 tests these channels. Columns 1–3, using the PRHS-2012 and World Bank 2024 survey, show cash relief recipients are 33 pp more likely to upgrade housing as a response to the past flood (in-situ adaptation) and 6-7 pp more likely to expect cash relief in case of a future flood, or government help in case of an unexpected shock. Column 4 shows that households reporting having experienced a flood in their immediate surroundings in 2022 are 27 pp more likely to expect a similar event in the future. This aligns with a large body of evidence that perceived disaster probabilities rise after salient events (Shao et al., 2017; Gibson and Mullins, 2020; Bakkensen and Barrage, 2022; Shah et al., 2022).

Appendix Tables A.11–A.12 show these associations are robust to tehsil or village-level geographic fixed effects. Tables A.11 and A.12 also show that government cash relief does not affect expectations of help from non-government sources—pointing to a government-specific belief update rather than generalized optimism.

Taken together, these findings support the empirical results and the theory. Cash relief provides liquidity, raises expectations of future government support, and enables in-situ adaptation; flood exposure increases the subjective likelihood of future floods. Because relief is typically conditional on flooding—and the benefits of in-situ adaptation are likewise tied to flood exposure—the "stay" forces are stronger where perceived risk of future floods is higher. In non-flooded areas, the low perceived risk means the liquidity effect dominates, prompting out-migration. In flooded areas, liquidity and "stay" forces balance out, resulting in no net population change.

7 Conclusion and Policy Implications

In this paper, we study how cash relief—an increasingly common policy tool—affects migration, a key margin of climate adaptation. We show that there are two competing forces: cash relaxes liquidity and risk constraints, encouraging migration, but it also raises the expected value of staying by increasing expectations of future assistance and enabling in-situ adaptation. In a low-income setting where existing liquidity constraints are high, these forces offset, producing no net effect of relief on migration in flooded areas.

There are two key policy implications. First, while we document a moral hazard effect, we show that it is balanced by the liquidity effect. Hence, moral hazard concerns should not be a reason to hold back cash transfers, at least in low-income settings like South Asia. Second, while we do not estimate counterfactuals, our model implies that moral hazard concerns will likely be stronger if relief was provided in-kind due to lower liquidity effects or if relief was provided based on damage-based targeting due to a stronger on-site presence requirement at the time of disaster. Hence, geo-targeted cash relief has the additional advantage of counterbalancing moral hazard effects apart from other well-known advantages such as being faster and more efficient.

Our study focuses on rural South Asia, which accounts for 32 percent of global flood victims (Rentschler et al., 2022). Even if external validity beyond Sindh is imperfect, our findings are indicative of what the effects will be elsewhere in South Asia. A fuller picture requires evidence across geographies, hazards, and relief programs. Our approach—assembling longitudinal datasets and estimating DiD with fully interacting relief and disaster—can be employed for many other regions, disasters, and relief programs.

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8 Appendix

A.1 Data

A.1.1 Population Census

We construct a panel dataset of population estimates at the village (revenue village) level using data from six population censuses conducted between 1961 and 2023. While population data are typically available at the district or tehsil level, this is, to our knowledge, the first effort to harmonize such data at the village level—the lowest administrative unit in Pakistan.

The first step in constructing the panel dataset involved digitizing the pre-1998 census waves. The sections of the census containing village-level population data are available only as scanned images (see Figure A.1 for an example). We used the LayoutParser package developed by Shen et al. (2021) to extract text from these images. This tool employs image recognition and processing techniques to convert historical documents into machine-readable text.

However, due to the historical nature of the documents and the quality of the scanned images, the digitization process introduced several errors—for example, missing values or confusion between letters and numbers. To address these issues, we implemented two validation checks. First, we performed a horizontal check by aggregating disaggregated population data (by gender, age, and education) and verifying that the sum matched the reported total for each village. Second, we carried out a vertical check by aggregating population data across all villages within each Tapedar Circle (the administrative unit above a village) and comparing it with the reported TC-level population totals. We manually corrected any inconsistencies identified through these checks.

Figure A.1: Sample Page with village Population - 1971 Census

TABLE-12 SELECTED POPULATION STATISTICS OF MAUZAS - 1972 133

DADU DISTRICT

HADBAST NUMBER	NAME OF MAUZA AND LOCAL FACILITY	AREA IN ACRES	POPULATION - 1972			LITERATES 10 YEARS & ABOVE	MARRIED		RELIGION		NUMBER OF HOUSE-HOLDS	HOUSE HOLD SIZE
			BOTH SEXES	MALE	FEMALE		MALE	FEMALE	MUSLIMS	OTHERS		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	MAHAL KOHISTAN TALUKA											
	MAHAL KOHISTAN STC											
	BABUR BUND TC											
000	BABUR BUND (PR)	34,989	2,248	1,156	1,092	340	460	466	2,246	2	447	5.0
000	HATHAL BUTH	17,784	1,185	584	601	61	235	239	1,185		231	5.1
000	UTH PALAN (PR)	25,003	437	224	213	19	91	98	437		48	9.1
	TOTAL (T.C.)	77,776	3,870	1,964	1,906	420	786	803	3,868	2	726	5.3
	BHAL TC											
000	BHAL (PR)	84,893	3,554	1,720	1,834	10	777	781	3,553	1	597	6.0
000	DHAMACH (PR)	14,739	1,375	703	672	222	305	314	1,375		201	6.8
000	GAGIARD (MS,PR,PO)	23,115	3,474	1,578	1,896	666	730	630	1,710	1,764	542	6.4
000	KOH TARASH	12,051	475	234	241	2	77	82	475		60	7.9
	TOTAL (T.C.)	134,798	8,878	4,235	4,643	900	1,889	1,807	7,113	1,765	1,400	6.3
	DESVI TC											
000	DESVI (PR)	48,358	2,998	1,569	1,429	209	548	555	2,995	3	459	6.5
000	WAHI ARAB KHAN	17,789	306	164	142		60	61	306		53	5.8
	TOTAL (T.C.)	66,147	3,304	1,733	1,571	209	608	616	3,301	3	512	6.5

Notes: This figure shows a page from the 1972 Census for Dadu District. Column 2 lists village names, followed by area and total population in Columns 3 and 4. Columns 5 to 11 report population disaggregated by gender, educational attainment, religion, and marital status.

We then matched census waves at the village level to track population changes over time. This process involved two main challenges. First, village names may vary slightly across waves due to spelling differences, digitization errors, or actual changes in names over time. Second, administrative units above the village—Tapedar Circles (TC), Sub-Tapedar Circles (STC), tehsils, and districts—may split or merge, causing the same village to appear under different higher administrative units in different waves. While villages themselves can also split or merge, such changes are relatively infrequent, as indicated by the largely stable number of villages shown in Table 2.

To address these challenges, we used a combination of algorithmic and manual methods to match villages across census waves. We first applied fuzzy matching algorithms based on Levenshtein distance—a measure of the minimum number of edits needed to transform one string into another. Specifically, we computed a weighted similarity score using village, TC, and STC names, with weights of 0.5, 0.25, and 0.25, respectively. Including TC and STC names helped avoid incorrect matches between villages from different districts and enabled accurate matching even when district and tehsil boundaries had changed. We then applied a second round of fuzzy matching to the remaining unmatched villages using village, tehsil, and district names, to account for changes in TC and STC structures. Any villages that remained unmatched after these steps were matched manually, keeping track of the changes in higher administrative units.

To reduce errors in the matching process, we implemented three validation checks. First, we manually reviewed all village matches with a similarity score below 70, as these were more likely to be incorrect. Second, we flagged villages whose population growth between consecutive waves was unusually high (above the 95th percentile) or unusually low (below the 5th percentile). Third, we examined cases where the line number difference between two consecutively matched villages exceeded 9, given that villages within the same administrative unit were typically listed in the same order across census waves.

Using the resulting matches, we constructed a panel of census-based population estimates at the village level spanning 1961 to 2023. Anchoring the panel on the 1998 census, we matched villages sequentially—moving forward from 1998 to 2017 and then to 2023, and backward from 1998 to 1981, 1972, and 1961. Table A.1 reports the sequential match rates across all waves. For the four waves adjacent to 1998 (1972, 1981, 2017, and 2023), we were able to match approximately 95 percent of villages.

We then linked the census data to a village-level shapefile in order to geo-locate the villages. The matching process followed a similar approach as above; however, the shapefile did not contain TC and STC information and had greater discrepancies in village names compared to the census waves. Therefore, we had slightly lower match rates. We first matched the 2017 census to the shapefile, then used the previously developed crosswalks to link other waves. As a result, we achieved a maximum match rate of 91 percent between the shapefile and the 2017 census, with match rates ranging from 85 to 90 percent for the other waves.

Overall, of the 5,294 villages recorded in the earliest census wave (1961), we were able to track geo-locate and track population estimates over 7 decades for 4,404 villages (83.2 percent).

Table A.1: villages in Census: Matches with 1998 and with Shapefile

Year	villages in Census	Matches with 1998	Match Rate (%)	Matches with Shapefile	Match Rate (%)
1961	5,294	5,023	85.52	4,443	83.91
1972	5,728	5,553	94.93	4,893	85.42
1981	5,751	5,605	95.47	4,930	85.72
1998	5,872	–	–	5,142	87.58
2017	5,737	5,616	95.63	5,221	90.70
2023	5,697	5,565	94.78	5,100	88.60

Notes: This table reports the number of matches and match rates for village-level matching across the seven census waves and with the village boundary shapefile. villages are first sequentially matched across census waves using fuzzy string matching. Columns 3 and 4 report the number of matches and match rates relative to the 1998 census wave. Subsequently, all census waves are linked to the shapefile by fuzzy matching village names from the 2017 wave to those in the shapefile. Columns 5 and 6 present the resulting matches and match rates with the shapefile.

A.1.2 Public Goods and Economic Development

We use the 2008 village Infrastructure Census to obtain information on public goods provision and indicators of economic development at the village level. We match this data to the village shapefile using a weighted similarity score based on district, tehsil, and village names similar to the census matching. Of the 5,757 villages in the shapefile, we matched 4,955 (86%) with entries from the 2008 census.

A.1.3 Elections 2008

We use provincial assembly election data from the 2008 elections in Sindh, obtained from the Election Commission of Pakistan. The dataset includes information on the vote share of each party, the affiliation of the winning candidate, and the vote margin of victory. We overlayed the constituency boundaries on the village boundaries to identify constituencies for each village.

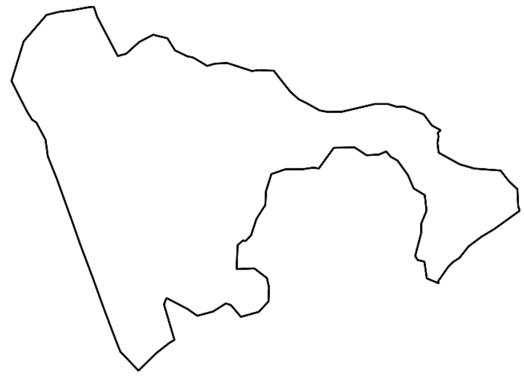
A.1.4 Village Locations

As a first step toward geo-locating villages affected by the floods and those that received relief, we construct a shapefile of all villages in Sindh. While boundary shapefiles for higher administrative units—such as provinces, districts, and tehsils—are readily available, no publicly available shapefiles exist for villages in Pakistan. To our knowledge, we are the first to generate a comprehensive boundary shapefile for all villages in Sindh, the second-largest province in the country.

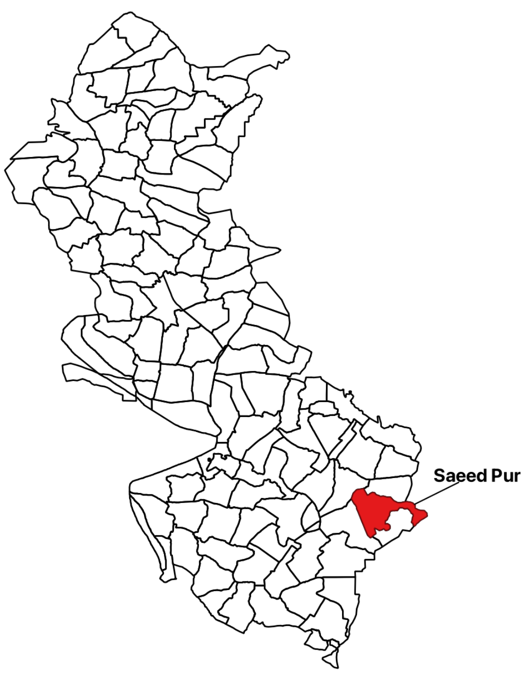
We rely on hand-drawn maps created by local land revenue officials ("patwaris"), which we obtained from the Sindh Board of Revenue. Through a laborious process of geo-referencing and manual digitization, we digitized 5,757 maps covering the entire province. A sample of both the raw and digitized maps is shown below.

Figure A.2: Raw and Digitized Maps for Saeed Pur village

(a) Raw Map



(b) Digitized Map



(c) Digitized Map of District Matiari

(d) Digitized Map of Sindh Province

Note: This figure illustrates the process of digitizing hand-drawn *patwari* (local revenue official) village maps to generate a comprehensive village shapefile for Sindh. Panel (a) shows the original hand-drawn map of Saeed Pur village, while Panel (b) presents its digitized version. Panel (c) displays all digitized villages in District Matiari, with Saeed Pur highlighted in red. Panel (d) shows the full set of villages across Sindh province, again highlighting Saeed Pur in red.

A.1.5 Cash Relief Program

We obtained the notification of calamity-affected areas that received cash relief from the NADRA website, the government agency responsible for disbursing relief payments. The notification listed villages along with their corresponding tehsil and district names. According to the notification, 1,460 villages across 59 tehsils in 19 districts of Sindh received cash relief. We matched these villages to the village-level shapefile in order to geolocate them, measure flood exposure, and link them with population data.

We constructed four batches of matches based on our confidence in match quality, using fuzzy string matching throughout. Since the notification did not include TC or STC codes, we harmonized district and tehsil names across the notification and the shapefile and relied on name similarity scores to guide matching. In the first batch, we matched villages with the same tehsil and district names in both sources, resulting in 952 matches. In the second batch, we relaxed the tehsil constraint and matched villages within the same district only to account for possible changes in tehsil boundaries. For the remaining two batches, we did fuzzy matching of village names alone. In the third batch, we only allowed those villages from the shapefile to be matched whose district was among the 19 districts in the relief notification, and in the fourth batch we dropped this restriction as well. We added 66, 171, and 47 additional matches in the second, third and fourth batches respectively.

In our main analysis, we adopt a slightly conservative approach and define cash villages using Batches 1 and 2 only. Appendix Table A.5, however, demonstrates that our results are robust to alternative matching definitions, including a stricter specification using only Batch 1 and more inclusive specifications that incorporate Batches 3 and 4 as cash villages.

A.2 Estimation Details

A.2.1 Household Damage due to 2010 Floods

We estimate the household damage using the Pakistan Rural Household Survey (2012), conducted around 1.5 years after the 2010 floods. We focus on households residing in villages reported as flooded in the community survey. In the household module, respondents were asked to report monetary values of damages across several dimensions, including income loss due to the death of the main earner, home repair costs, loss of crops, livestock, productive and personal assets, job loss, and displacement-related expenses.

For each household, we sum reported damages across all categories to obtain a total damage estimate. We then compute the village-level average of these totals across all surveyed households. The mean damage per household in flooded villages is PKR 107,032. The median is PKR 63,785, with the 25th percentile at PKR 9,203 and the 75th percentile at PKR 223,143.

A.3 Additional Tables and Figures

Table A.2: Pre-Flood Differences Between Cash and Non-Cash Villages

	Flood			No Flood		
	Cash	No Cash	Diff	Cash	No Cash	Diff
Panel A: Public Goods						
	(1)	(2)	(3)	(4)	(5)	(6)
Schools (0-4)	1.17	1.31	-0.14**	1.47	1.40	0.08
	(0.89)	(0.91)	(0.07)	(0.98)	(0.87)	(0.06)
Health Units (0-3)	0.40	0.60	-0.20**	0.59	0.53	0.05
	(0.96)	(1.12)	(0.08)	(1.11)	(1.08)	(0.07)
Irrigation (0-2)	1.04	1.07	-0.04	1.11	1.06	0.05
	(0.54)	(0.53)	(0.04)	(0.55)	(0.49)	(0.03)
# Water Courses	4.66	4.70	-0.04	6.38	5.34	1.04**
	(6.71)	(11.44)	(0.78)	(7.96)	(6.50)	(0.46)
# Improved Water Courses	1.03	0.93	0.09	1.69	1.37	0.32**
	(1.93)	(1.76)	(0.14)	(2.54)	(2.02)	(0.15)
Electricity (Proportion)	0.38	0.46	-0.08***	0.42	0.44	-0.02
	(0.30)	(0.36)	(0.03)	(0.32)	(0.33)	(0.02)
Post Office	0.11	0.09	0.02	0.16	0.12	0.04*
	(0.32)	(0.29)	(0.02)	(0.37)	(0.33)	(0.02)
Police Station	0.19	0.20	-0.01	0.26	0.15	0.12***
	(0.39)	(0.40)	(0.03)	(0.44)	(0.35)	(0.03)
Government Bank	0.00	0.01	-0.01	0.02	0.03	-0.01
	(0.05)	(0.11)	(0.01)	(0.12)	(0.16)	(0.01)
Govt Graint Procurement Center	0.04	0.07	-0.04**	0.08	0.05	0.03
	(0.19)	(0.26)	(0.02)	(0.27)	(0.22)	(0.02)
Piped Water Supply	0.07	0.10	-0.03	0.08	0.06	0.02
	(0.26)	(0.30)	(0.02)	(0.27)	(0.25)	(0.02)
Sui Gas	0.05	0.07	-0.02	0.08	0.06	0.03
	(0.22)	(0.25)	(0.02)	(0.27)	(0.23)	(0.02)
Metalled Road	0.56	0.63	-0.08**	0.63	0.68	-0.05*
	(0.50)	(0.48)	(0.04)	(0.48)	(0.47)	(0.03)
Public Goods Index	-0.08	0.00	-0.08**	0.07	0.01	0.07**
	(0.40)	(0.49)	(0.04)	(0.47)	(0.45)	(0.03)
Panel B. Economic Indicators						
	(1)	(2)	(3)	(4)	(5)	(6)
# of Irrigation sources (0-5)	1.22	1.21	0.00	1.23	1.18	0.05
	(0.63)	(0.53)	(0.04)	(0.59)	(0.46)	(0.03)
House Material (1-3)	2.38	2.42	-0.04	2.53	2.56	-0.03
	(0.91)	(0.89)	(0.07)	(0.82)	(0.80)	(0.05)
Toilets in Majority houses	0.29	0.44	-0.15***	0.35	0.35	-0.01

Table A.2 – Table (continued)

	Flood			No Flood		
	Cash	No Cash	Diff	Cash	No Cash	Diff
	(0.46)	(0.50)	(0.04)	(0.48)	(0.48)	(0.03)
Bricket Streets (Proportion)	0.08	0.10	-0.01	0.13	0.11	0.03**
	(0.17)	(0.18)	(0.01)	(0.22)	(0.19)	(0.01)
Bricket Drains (Proportion)	0.08	0.08	-0.01	0.10	0.08	0.01
	(0.18)	(0.16)	(0.01)	(0.19)	(0.19)	(0.01)
Sewerage (Proportion)	0.05	0.05	-0.00	0.07	0.06	0.02
	(0.15)	(0.14)	(0.01)	(0.17)	(0.16)	(0.01)
Media Access (0-1)	0.47	0.48	-0.01	0.55	0.53	0.03*
	(0.25)	(0.26)	(0.02)	(0.26)	(0.24)	(0.02)
# Whole Sale Markets	0.08	0.12	-0.04	0.18	0.12	0.06
	(0.49)	(0.55)	(0.04)	(0.76)	(0.60)	(0.04)
Retail Market	0.07	0.07	0.00	0.13	0.10	0.03*
	(0.26)	(0.26)	(0.02)	(0.34)	(0.30)	(0.02)
# Credit Facilities (0-9)	1.73	1.85	-0.12	1.85	1.85	0.00
	(0.80)	(0.99)	(0.07)	(1.00)	(0.99)	(0.06)
# Banks	0.01	0.01	0.01	0.03	0.02	0.01
	(0.13)	(0.12)	(0.01)	(0.22)	(0.34)	(0.01)
Industry (0-4)	0.10	0.15	-0.05	0.22	0.22	0.01
	(0.45)	(0.48)	(0.04)	(0.74)	(0.71)	(0.04)
Development Index	-0.08	-0.02	-0.05	0.05	0.01	0.04
	(0.43)	(0.43)	(0.03)	(0.54)	(0.47)	(0.03)
Observations	493	256	4,195	318	3,128	4,195
Panel C. Elections (2008)						
	(1)	(2)	(3)	(4)	(5)	(6)
Voter Turnout	0.40	0.38	0.01	0.39	0.45	-0.05***
	(0.09)	(0.07)	(0.02)	(0.09)	(0.08)	(0.02)
Vote share PPP	0.60	0.58	0.02	0.61	0.58	0.03
	(0.17)	(0.16)	(0.04)	(0.16)	(0.17)	(0.02)
Won Seat PPP	0.78	0.66	0.13	0.85	0.76	0.09
	(0.41)	(0.48)	(0.12)	(0.36)	(0.43)	(0.06)
Electoral Competitiveness (0-1)	0.60	0.68	-0.08	0.63	0.64	-0.01
	(0.21)	(0.21)	(0.05)	(0.21)	(0.20)	(0.02)
Observations	504	259	4,176	314	3,099	4,176

Note: This table compares pre-flood characteristics between cash and non-cash villages, separately for flooded and non-flooded areas. Columns 1–3 report results for flooded villages; Columns 4–6 for non-flooded villages. Differences between groups are tested using t-tests. Panel A includes public goods variables, and Panel B includes indicators of economic development. Indexes for both sets of variables are constructed using inverse-covariance weighting. All variables in Panels A and B are drawn from the 2008 village Infrastructure Census. Panel C presents election outcomes from the 2008 provincial assembly elections. Standard errors are clustered at the village level in Panels A and B, and at the constituency level in Panel C. $p < 0.01$; $p < 0.05$; $p < 0.1$.

A.3.1 Robustness Tests: Main Results

Table A.3: Robustness: Alternative Specifications

	Log(Population)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Flood × Post	-0.223***	-0.142***	-0.224***	-0.143***	-0.225***	-0.226***	-0.133***	-0.001
	(0.035)	(0.032)	(0.041)	(0.032)	(0.041)	(0.035)	(0.038)	(0.038)
Cash × Post	-0.081***	-0.083***	-0.084***	-0.083***	-0.085***	-0.076***	-0.042*	-0.081**
	(0.025)	(0.025)	(0.027)	(0.025)	(0.027)	(0.025)	(0.025)	(0.033)
Flood × Cash × Post	0.106**	0.085*	0.112**	0.086*	0.115**	0.107**	0.065	0.103*
	(0.048)	(0.047)	(0.054)	(0.047)	(0.055)	(0.048)	(0.049)	(0.059)
Development Index		0.001						
		(0.012)						
Development Index (2008) x Post			0.008					
			(0.017)					
Public Goods Index				-0.010				
				(0.011)				
Public Goods Index (2008) x Post					0.035*			
					(0.018)			
PPP Seat (2008) x Post						-0.046***		
						(0.017)		
Observations	25,230	13,642	20,466	13,642	20,466	25,056	25,230	25,230
Clusters	4,205	3,411	3,411	3,411	3,411	4,176	4,205	4,205
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Village FE	✓	✓	✓	✓	✓	✓	✓	✓
District x Post FE							✓	
Village Time Trend								✓

*Note:*This table presents robustness checks of the main results in Table 1 by controlling for potentially confounding variables. Column 1 replicates Column 3 of Table 1. Columns 2 and 3 control for local development by adding a time-varying control and the interaction of its pre-flood value with the post-treatment dummy. Columns 4 and 5 control for public goods provision in the same way. Column 6 controls for whether the Pakistan Peoples Party (the incumbent provincial ruling party) won the constituency seat, interacted with the post-treatment dummy. Column 7 includes district-by-post dummies for all districts in Sindh. Column 8 includes linear village-specific trends for all villages in the sample. The outcome variable in all columns is the log of population. All regressions include village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

Table A.4: Robustness: Alternate Flood Measures

	Log(Population)				
	(1)	(2)	(3)	(4)	(5)
Cash × Post	-0.062**	-0.065***	-0.087***	-0.073***	-0.049*
	(0.026)	(0.025)	(0.023)	(0.023)	(0.026)
Flood > 30% × Post	-0.216***				
	(0.031)				
Flood > 40% × Post		-0.212***			
		(0.032)			
Flood > 60% × Post			-0.217***		
			(0.037)		
Flood > 70% × Post				-0.229***	
				(0.038)	
Flood × Post (Continuous)					-0.275***
					(0.039)
Flood > 30% × Cash × Post	0.078*				
	(0.045)				
Flood > 40% × Cash × Post		0.075			
		(0.045)			
Flood > 60% × Cash × Post			0.105**		
			(0.049)		
Flood > 70% × Cash × Post				0.081	
				(0.051)	
Flood × Cash × Post (Continuous)					0.091*
					(0.055)
Observations	25,230	25,230	25,230	25,230	25,224
Clusters	4,205	4,205	4,205	4,205	4,204
Time FE	✓	✓	✓	✓	✓
Village FE	✓	✓	✓	✓	✓

Note: This table presents robustness checks of the main results in Table 1 using alternative flood exposure definitions. Columns 1–4 use binary flood indicators based on thresholds of 30% 40%, 60%, and 70% flood exposure, respectively. Column 5 uses a continuous flood exposure measure. All regressions control for village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

Table A.5: Robustness: Alternate Cash Relief Matching Criteria

	Log(Population)			
	(1)	(2)	(3)	(4)
Flood × Post	-0.205***	-0.226***	-0.223***	-0.223***
	(0.032)	(0.033)	(0.035)	(0.035)
Cash 1 × Post	-0.083***			
	(0.031)			
Cash 2 × Post		-0.080***		
		(0.029)		
Cash 3 × Post			-0.081***	
			(0.025)	
Cash 4 × Post				-0.071***
				(0.024)
Flood × Cash 1 × Post	0.084*			
	(0.050)			
Flood × Cash 2 × Post		0.112**		
		(0.049)		
Flood × Cash 3 × Post			0.106**	
			(0.048)	
Flood × Cash 4 × Post				0.096**
				(0.047)
Observations	25,230	25,230	25,230	25,230
Clusters	4,205	4,205	4,205	4,205
Time FE	✓	✓	✓	✓
Village FE	✓	✓	✓	✓

Note: This table presents robustness checks of the main results in Table 1 using alternative matching criteria for cash relief designations. The outcome variable in all Columns is log population. Column 1 uses the most restrictive criteria 1, only keeping matches within the same tehsil. Column 2 uses a slightly more linient criteria of keeping matches within the same district. Column 3 uses criteria 3, also used for main specification, and allows matches in districts in any district mentioned in the cash relief notification. Column 4 uses the most linient criteria 4, which imposes no restriction on the matching and just uses village names to do the matching. All regressions control for village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

A.3.2 Heterogeneity Results

Table A.6: Heterogeneity by Gender

	Male		Female	
	Log	Prop	Log	Prop
	(1)	(2)	(3)	(4)
Flood × Post	-0.190***	0.000	-0.190***	-0.000
	(0.033)	(0.002)	(0.033)	(0.002)
Cash × Post	-0.064***	-0.003***	-0.051**	0.004***
	(0.022)	(0.001)	(0.022)	(0.001)
Flood × Cash × Post	0.106**	0.003	0.091**	-0.004*
	(0.045)	(0.002)	(0.045)	(0.002)
Observations	19,787	19,787	19,787	19,787
Clusters	4,205	4,205	4,205	4,205
Time FE	✓	✓	✓	✓
Village FE	✓	✓	✓	✓

Note: This table presents results on heterogeneity in the effects of cash relief by gender. Columns 1 and 2 report results for males, and Columns 3 and 4 for females. Columns 1 and 3 use log population as the outcome variable, while Columns 2 and 4 use population shares. All regressions include village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

Table A.7: Heterogeneity by Age

	Below 18		Above 18	
	Log	Prop	Log	Prop
	(1)	(2)	(3)	(4)
Flood × Post	-0.075***	0.011***	-0.120***	-0.011***
	(0.028)	(0.004)	(0.025)	(0.004)
Cash × Post	-0.038*	0.015***	-0.099***	-0.015***
	(0.021)	(0.003)	(0.020)	(0.003)
Flood × Cash × Post	0.124***	0.013**	0.069*	-0.013**
	(0.040)	(0.005)	(0.036)	(0.005)
Observations	12,615	12,615	12,615	12,615
Clusters	4,205	4,205	4,205	4,205
Time FE	✓	✓	✓	✓
Village FE	✓	✓	✓	✓

Note: This table presents results on heterogeneity in the effects of cash relief by age group. Columns 1 and 2 report results for Below 18 age group, and Columns 3 and 4 Above 18. Columns 1 and 3 use log population as the outcome variable, while Columns 2 and 4 use population shares. All regressions include village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

Table A.8: Heterogeneity by Education

	Non-Educated		Primary Educated		Secondary Educated	
	Log	Prop	Log	Prop	Log	Prop
	(1)	(2)	(3)	(4)	(5)	(6)
Flood × Post	-0.147***	0.014***	-0.318***	-0.014***	-0.328***	-0.010***
	(0.030)	(0.003)	(0.057)	(0.003)	(0.052)	(0.002)
Cash × Post	-0.050**	0.003	-0.115**	-0.003	-0.214***	-0.005***
	(0.020)	(0.002)	(0.045)	(0.002)	(0.044)	(0.002)
Flood × Cash × Post	0.104**	-0.007*	0.295***	0.007*	0.216***	0.000
	(0.041)	(0.004)	(0.081)	(0.004)	(0.077)	(0.003)
Observations	16,166	16,166	16,166	16,166	16,166	16,166
Clusters	4,205	4,205	4,205	4,205	4,205	4,205
Time FE	✓	✓	✓	✓	✓	✓
Village FE	✓	✓	✓	✓	✓	✓

Note: This table presents results on heterogeneity in the effects of cash relief by education level. Columns 1 and 2 report results for individuals with no formal education; Columns 3 and 4 for those with primary education; and Columns 5 and 6 for those with secondary education. Columns 1, 3, and 5 use log population as the outcome variable, while Columns 2, 4, and 6 use population shares. All regressions include village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

Table A.9: Heterogeneity by Religion

	Muslims		Non-Muslims	
	Log	Prop	Log	Prop
	(1)	(2)	(3)	(4)
Flood × Post	-0.118***	0.040***	-0.720***	-0.041***
	(0.034)	(0.003)	(0.087)	(0.003)
Cash × Post	0.001	0.031***	-0.276***	-0.031***
	(0.022)	(0.003)	(0.079)	(0.003)
Flood × Cash × Post	0.031	-0.035***	0.425***	0.035***
	(0.046)	(0.004)	(0.132)	(0.004)
Observations	19,787	19,787	19,787	19,787
Clusters	4,205	4,205	4,205	4,205
Time FE	✓	✓	✓	✓
Village FE	✓	✓	✓	✓

Note: This table presents results on heterogeneity in the effects of cash relief by religion. Columns 1 and 2 report results for muslims, and Columns 3 and 4 for non-muslims. Columns 1 and 3 use log population as the outcome variable, while Columns 2 and 4 use population shares. All regressions include village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

Table A.10: Heterogeneity by Economic Development and Public Goods Provision (2008)

	Log(Population)	
	(1)	(2)
Flood × Post	-0.224***	-0.229***
	(0.061)	(0.074)
Cash × Post	-0.040	-0.067*
	(0.032)	(0.039)
Flood × Cash × Post	0.078	0.138
	(0.078)	(0.088)
Low Developemnt × Post	0.019	
	(0.015)	
Flood x Low Developemnt x Post	0.001	
	(0.080)	
Cash x Low Developemnt x Post	-0.099*	
	(0.056)	
Flood x Cash x Low Developemnt x Post	0.074	
	(0.110)	
Low Public Goods × Post		-0.021
		(0.016)
Flood x Low Public Goods x Post		0.008
		(0.086)
Cash x Low Public Goods x Post		-0.034
		(0.054)
Flood x Cash x Low Public Goods x Post		-0.032
		(0.111)
Observations	20,466	20,466
Clusters	3,411	3,411
Time FE	✓	✓
Village FE	✓	✓

Note: This table presents results on heterogeneity in the effects of cash relief by village's pre-flood economic development. The outcome variable is log population. In Column 1, cash, flood and post are additionally interacted with a binary for Low Development, defined as a village having development index value below the median using 2008 village infrastructure census. In Column 2, cash, flood, and post are instead interacted with a binary for Low Public Goods, defined as a village having public goods index value below the median in 2008 infrastructure census. All regressions include village and census year fixed effects. Standard errors are clustered at the village level. $p < 0.01$; $p < 0.05$; $p < 0.1$.

A.3.3 Robustness Tests: Mechanisms

Table A.11: Robustness: Will help in case of unexpected economic shock?

	Government				NGOs	Relatives	Community
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash Relief (2010)	0.093***	0.089***	0.061**	0.121***	0.074**	0.009	0.024
	(0.020)	(0.022)	(0.028)	(0.017)	(0.035)	(0.018)	(0.023)
Flood (2010)		0.015	0.014	0.028*	-0.022	0.017	0.042**
		(0.023)	(0.023)	(0.015)	(0.024)	(0.016)	(0.020)
Observations	4,238	4,238	4,238	4,238	3,571	4,244	4,225
Clusters	68	68	68	68	68	68	68
No-Cash Mean	0.355	0.355	0.355	0.355	0.372	0.355	0.357
Province FE			✓	✓	✓	✓	✓
Tehsil FE				✓			

Note: This table examines the effect of cash relief on beliefs about who would provide support in the event of an unexpected economic shock. Columns 1–4 use as the outcome the belief that the government would help in such a situation. Columns 5–7 capture whether respondents believe that NGOs, relatives, or community members, respectively, would provide support. All outcomes are rescaled to the [0,1] interval. All outcomes are from the Pakistan Rural Household Survey conducted two years after the 2010 floods. Standard errors are clustered at the village level.

Table A.12: Robustness: Likelihood of government support in case of a flood?

	Government will give cash relief in case of future floods						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash Relief (2022)	0.068***	0.066***	0.061***	0.055***			
	(0.017)	(0.017)	(0.014)	(0.014)			
NGO Relief					0.048		
					(0.037)		
Relative Help						-0.091***	
						(0.021)	
Village Leader Help							-0.058**
							(0.023)
Flood (2022)		0.039*	0.004	0.007	0.049**	0.045**	0.043*
		(0.023)	(0.024)	(0.025)	(0.024)	(0.022)	(0.024)
Observations	4,001	4,001	4,001	4,001	4,013	4,013	4,013
Clusters	122	122	122	122	122	122	122
No-Cash Mean	0.398	0.398	0.398	0.398	0.398	0.398	0.398
Tehsil FE			✓	✓			
Village FE				✓			

Note: This table examines the effect of cash relief provided by different sources on beliefs that the government would provide cash relief in the event of future floods. The outcome variable across all models is a categorical vairable, rescaled to [0,1], indicating that government will provide cash relief in case of a future flood. Columns 1 - 4 estimates the effect of receiving relief from the government, Column 5 from NGOs, Column 6 from relatives, and Column 7 from village leaders. All outcomes and relief measures come from the World Bank survey conducted after the 2022 floods. Standard errors are clustered at the village level.