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The Importance of Being Early? Anticipatory Cash Transfers for Flood-Affected Households

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Abstract

This study evaluates the effect of a cash transfer on households forecast to experience extreme flooding in Bangladesh five days before the flood peak based on hydrological modeling. Households receiving this "anticipatory" transfer had improved welfare during and three months after the flooding and reduced asset loss. Early cash increased the choice set of actions available to households, thereby altering the flood impacts at a critical time juncture. Benefits accrue in the months before a conventional humanitarian response, highlighting the gains from acting early.

JEL classification D12, O12, Q54

Keywords finance and microfinance, climate change, floods

1. Introduction

Globally, 1.81 billion people are exposed to the risk of extreme flooding, with a disproportionate number located in South Asia and Sub-Saharan Africa where there is the highest overlap in flood risk and poverty (Rentschler, Salhab, and Jafino 2022). Such extreme weather events sometimes escalate to humanitarian crises and can have long-lasting impacts on household welfare (Rosales-Rueda 2018). With climate change, these shocks are becoming more frequent and intense, with disproportionate impacts on low-income households. One question is how best to mitigate the worst effects of these shocks in the immediate term. Cash transfers have been shown to be effective in protecting consumption when

faced with adverse events (de Janvry et al. 2006; Jensen, Barrett, and Mude 2017). However, the billions spent in responding to extreme weather events tend to be mobilized after they occur and some of the negative impacts have already materialized, reaching households late (Clarke and Dercon 2016). What if households were reached earlier and received cash support ahead of a shock?

This paper assesses the impact of a one-off "anticipatory" cash transfer delivered immediately before the height of extreme flooding in Bangladesh. In July 2020, the World Food Programme (WFP) sent BDT 4,500 (USD $137 at 2020 PPP rates, roughly equivalent to two weeks of household food expenditure) using mobile money to more than 23,000 ultra-poor households that were forecast to experience extreme flooding along the Jamuna River. Using forecasts of water flows to trigger the release of cash transfers, WFP delivered cash to affected households 5 days prior to the flood peak and 100 days earlier than its previous interventions in the same context. The floods that followed were some of the most severe and protracted in decades.

To assess impact, households that received an anticipatory cash transfer were compared to otherwise likely comparable households that did not. Digital wallets are a common delivery mechanism for government benefits, and this transfer was also made by digital wallet. However, given the need to deliver transfers quickly, WFP selected one provider—bKash—from 16 possible providers. Those found to have a different digital wallet or inactive bKash account at the time of the verification call were excluded from receiving the cash transfer, and formed the control households.

Both treated and control households were drawn from the same preexisting list of vulnerable households eligible for WFP support in the affected areas, and are balanced on selected demographic and housing characteristics, with the observed difference in primary completion rates less than 3 percent. All but 3 percent of the households surveyed had access to bKash or other digital wallet. There were no differences in the use of mobile phones or digital money between control and treatment households. We undertake additional tests to show that results are robust to the significant difference in education level observed. However, we are limited in the data we have on hand to test differences in observables and cannot look at income, wealth, assets, or skills beyond basic education levels. Differences are controlled for through the inclusion of household characteristics and village fixed effects, and show robustness of results for different subgroups. Nevertheless, it is still possible that there are some unobserved differences between treatment and control households that could affect the estimates of impact; we lack data to test for parallel pretreatment trends in the months leading to the selection of bKash, although the exogenous timing of the shock and intervention is somewhat helpful in this regard.

Welfare is higher three months after the intervention for households that received the anticipatory cash transfer. Children in treated households were 5 percent more likely to consume three meals or more on the day prior to the survey. This result is striking in light of the long-term consequences of temporary child undernutrition (Maccini and Yang 2009). Respondents in treated households also reported 18.7 percent higher life satisfaction on average than those that did not receive the transfer. These welfare effects were measured before the usual humanitarian response reached affected households, highlighting the potential cost of a late response. Moreover, using a second survey with a subset of the sample, treated households recalled that they were 62 percent less likely to go a full day without eating during the floods, suggesting that the immediate relief was also significant.

Analysis shows that some of this difference was likely due to the fact that households receiving the cash transfer took more actions to prepare for the oncoming floods. Compared to control households, households that were sent the anticipatory cash transfer were more likely to evacuate households members and livestock, and more likely to purchase food. This could have been a result of either the greater liquidity available to households in advance of the flood, or the information contained in the transfer, with the transfer serving as a warning of impending flood risk. Consequently, the anticipatory cash transfer appears to have mitigated asset loss and boosted earning potential as an early sign of recovery three months later.

Taken together, the results on increased preemptive action and the subsequent improvements in welfare suggest that the timing of cash transfers relative to shocks matters. The anticipatory cash transfer likely allowed households to take different decisions that altered the flood impacts at a critical juncture in time, compared to households that did not receive cash. The one-off cash transfer was sizable—roughly equivalent to two weeks of food expenditure—but delivered before floods that lasted longer

than a month on average. The results also align with other studies showing that information about climatic shocks can shift beliefs and ex ante behaviors in similar settings (Rosenzweig and Udry 2019; Burlig et al. 2024; Patel 2023).

This research adds to existing evidence in three ways. First, this paper is one of the first rigorous evaluations of the impact of a cash transfer provided to households just in advance of a sudden onset humanitarian crisis. It complements Mogge, Roeckert, and Kraehnert (2025) who evaluate a payment provided to Mongolian herders in advance of severe winter weather. The literature has largely focused on showing the effectiveness of a regular flow of cash transfers in limiting the use of costly coping mechanisms (de Janvry et al. 2006; Aker et al. 2016; Jensen, Barrett, and Mude 2017) and protecting households against low rainfall shocks or drought (Hou 2010; Premand and Stoeffler 2022). However, well-identified evidence on the effectiveness of cash transfers delivered specifically in response to extreme weather events (e.g., Macours, Premand, and Vakis 2022) remains scarce, especially for one-off cash transfers to households. This paper evaluates the impact of a one-off cash transfer targeting at-risk households *predicted* to be affected by a near-term sudden onset disaster.

Second, the timing of cash transfers seems to matter as a design feature. A large body of literature compares the modality of cash versus in-kind interventions and transfer schedules in the form of lump-sum transfers versus monthly installments (Haushofer and Shapiro 2016; Cunha, De Giorgi, and Jayachandran 2019). However, timing as a modality remains underexplored in the context of a shock (Jeong and Trako 2022). Existing evidence has focused on post-shock response to support recovery (De Mel, McKenzie, and Woodruff 2012; Ivaschenko et al. 2020; Macours, Premand, and Vakis 2022), although guaranteed access to credit after a flood can induce ex ante behavioral change (Lane 2024). In contrast, this paper rigorously showcases the potential for an anticipatory cash transfer to limit the initial welfare cost of the shock by enabling preemptive action.[1]

Third, this paper also makes a significant contribution to the literature on humanitarian evaluations, as large-scale evaluations of emergency response are limited. A review of 900 studies on humanitarian programmes found that only 31 could be classified as impact evaluations, of which 8 studies focused on the emergency response phase (Puri et al. 2017). Evaluations of humanitarian interventions have often focused on the impact of regular programming in protracted crises, such as regular cash transfer programmes (e.g., Schwab 2019). Weingärter et al. (2020) specifically review the evidence for anticipatory humanitarian action and find that rigorous evidence of impact is limited.

The paper is structured as follows. After describing the intervention and sample, the empirical strategy and measurement approach are outlined. Results on the effects of cash relative to no cash are then presented, along with evidence on the channels through which welfare improvements occur. The robustness of these results to alternative model specifications is then assessed. A second round of cross-sectional data is used to examine whether effects persist five months after the transfer, before exploring how impacts varied with the timing of the transfer relative to the local flood peak. The paper concludes with a discussion of implications for crisis response and future research.

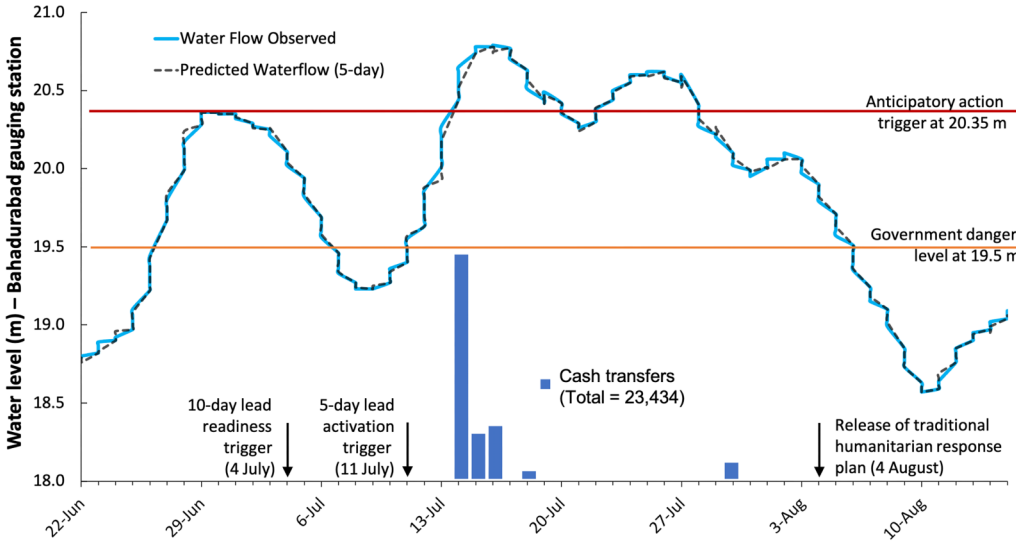
2. Context and intervention

The 2020 monsoon floods in Bangladesh were the second highest since 1989 and the second longest since 1998. According to UN estimates, more than 5.5 million people were directly affected by flooding by the beginning of August 2020. Flood waters halted agricultural production, damaged infrastructure, and disrupted food markets, schools, and health services. The Ministry of Agriculture estimates that 110,000 hectares of crop land were damaged and 257 people lost their lives (United Nations Resident Coordinator Office 2020).

In July 2020, the United Nations piloted anticipatory cash transfers, an earlier response to the humanitarian crisis, which entails employing a data-driven forecast to predict the occurrence of an extreme flood event as the basis for delivering transfers. The pilot took place along the Jamuna River (also

[1] Two earlier papers use propensity score matching to suggest anticipatory cash transfers are beneficial in the context of floods and an extreme winter season in Bangladesh and Mongolia respectively (Gros et al. 2019, 2020). However, these papers suffer from power and identification problems due to much smaller sample sizes and imbalanced control groups.

Figure 1 Timeline of triggers and cash transfers relative to water flow



Source: Figure created by Daniel Pfister (UN Office for the Coordination of Humanitarian Affairs).
Note: The timeline shows the relationship between flood water levels, trigger activation, and the delivery of anticipatory cash transfers. The figure displays water levels at the Bahadurabad gauging station from 22 June to 10 August 2020. The solid blue line shows observed water flow, while the dashed line indicates 5-day predicted water flow. The red horizontal line marks the anticipatory action trigger at 20.35 m, which lies above the government danger level at 19.5 m. Blue bars represent cash transfers totaling 23,434 recipients. Vertical arrows indicate trigger activation dates: 10-day lead readiness (4 July) and 5-day lead activation (11 July).

known as the Brahmaputra River), a particularly flood-prone area of northern Bangladesh. "Anticipatory action" is a relatively novel approach in a largely reactive humanitarian sector. It is a mechanism that enables humanitarian organizations to jointly preagree on who will receive funding for what and based on which rules and triggers, so to effectively "anticipate" or get ahead of a predictable shock. There are three components of anticipatory action designed to maximize speed of interventions, namely, (a) a robust forecast-based trigger; (b) preagreed action plans; and (c) prearranged financing, which came from the UN Central Emergency Response Fund (CERF) in this case. The objective is to mitigate the impact of the shock before it escalates to a crisis, which is typically when the traditional humanitarian response kicks in.

In this pilot, forecasts of upstream water flow measured at a centrally located gauging station were used to trigger the release of preagreed cash transfers prior to the highest flood levels. With support from the Bangladesh Red Crescent Society, the World Food Programme (WFP) sent BDT 4,500—USD $137 at 2020 PPP rates, equivalent to approximately two weeks of household food expenditure (World Bank 2019)—using mobile money to 23,434 vulnerable households. The recipient was very often a woman, as WFP prioritized female-headed households. These households were located across 131 unions, the smallest administrative unit in Bangladesh, which are widely distributed along the Jamuna River. The anticipatory cash transfer was intended to mitigate the worst impact of the flood shock on food consumption and mortality, rather than support post-flood recovery.

The timing of the anticipatory cash transfers was determined by a predefined set of triggers indicating the onset of an extreme flood event, based on forecasts of upstream water flow measured at a centrally located gauge station (the Bahadurabad Station). As illustrated by fig. 1, the forecasts predict actual water flow with a very high degree of accuracy. The first "readiness" trigger was activated on 4 July 2020 and set in motion preparatory activities, including beneficiary verification calls, which will be used to define the sample.[2] The second "activation" trigger was activated on 11 July and initiated the release

[2] The first trigger was activated once water flows forecasted by the Global Flood Awareness System (GloFAS) and/or the Bangladesh Flood Forecast and Warning Centre (FFWC) 15-day probabilistic warning models were predicted to be more than 50 percent likely to cross the one-in-five year return period threshold (100,000 m^3/s) over a period of three

of cash transfers.[3] The forecasts predicted the global flood peak at the Bahadurabad gauging station for 17 July 2020, although satellite imagery indicates large local variation across villages or "mauzas" ex post, with households receiving cash an average of seven days prior to their local flood peak. WFP sent cash to 23,434 households on 14, 15, 16, 18, and 30 July via bKash digital wallets, thus just prior to and immediately after the global flood peak. More than half of households received cash on the earliest date, 14 July. The speed of disbursement was a significant improvement to previous years. For instance, during the last severe flood event in 2019, humanitarian cash transfers funded by CERF only reached households 100 days after the flood peak.

3. Sample and data

This evaluation is not a randomized experiment, as this was not possible during this rapid-onset humanitarian crisis. However, the way in which the anticipatory cash transfers were targeted and rolled out in practice provided an opportunity for a rigorous evaluation of their impact on household welfare. The following sections set out the evaluation strategy and the data used to assess impact.

3.1. Evaluation strategy

When the first "readiness" trigger activated on 4 July 2020, WFP identified target unions based on flood risk and socioeconomic vulnerability. COVID-19 mobility restrictions required using preexisting beneficiary lists from past UN interventions and government safety nets. WFP rapidly contacted households by phone to verify eligibility, intensifying efforts after the second trigger on 11 July 2020 committed them to releasing transfers. Of 40,000 targeted households, WFP successfully contacted 38,000 and ultimately transferred cash to 23,434 households.

Given delivery speed requirements, transfers were made via digital wallets—feasible in Bangladesh's extensive mobile wallet infrastructure with 92.6 million registered users across 16 platforms by July 2020, according to Bangladesh Bank. However, time constraints led WFP to contract only one company—bKash—for delivery. During verification calls, households were asked to verify their name and that they still lived in the union of record. They were asked to provide information on their bKash wallet number, as they may be eligible to receive a transfer in the future. They were not told a flood was coming. Only households with active, correctly reported bKash accounts could receive transfers, while those with inactive, blocked, or incorrect accounts were excluded despite being on identical beneficiary lists and within the same villages.

This exclusion mechanism is plausibly exogenous for three key reasons. First, mobile wallet access is widespread and universal across household types in rural Bangladesh. A 2024 household census of over 80,000 households conducted by GiveDirectly in 15 unions overlapping with our study found 84 percent mobile wallet adoption overall (with the caveat that the census takes place nearly four years after our study). This rate increases to 87 percent among the poorest quintile. Ninety-seven percent of the ultra-poor sample report having access to a digital wallet in the survey.

Second, households have no systematic reason to prefer bKash over alternatives. The three main providers—bKash (40 percent market share), Nagad (25 percent), and Rocket (18 percent)—offer similar functionality, costs, and benefits. Households commonly maintain multiple accounts to access different promotions or government services, which historically used various providers.[4] As a subsidiary of BRAC Bank, a for-profit financial institution (and not directly linked to BRAC itself, the not-for-profit organization), bKash operates as one of many service providers. That said, BRAC Bank and BRAC NGO do cooperate on many activities, so it is important to assess whether households with bKash ownership are more likely to receive assistance. The 2024 GiveDirectly census reveals only weak correlation (0.06)

consecutive days at the Bahadurabad gauging station, with a 10-day lead time. This threshold was set to be 0.85 m above the government danger level of 19.5 m.

[3] The second activation trigger was reached once the water level crossed the government-defined danger level by an additional 0.85 m.

[4] For example, the Primary Education Stipend Program was initially rolled out through SureCash and then Nagad. The government COVID-19 welfare programme starting in April 2020 used SureCash, bKash, Rocket, and Nagad, while the UNDP-supported female empowerment programme SWAPNO uses Nagad and bKash.

between mobile wallet ownership and receiving assistance, with no relationship between active bKash accounts and recent cash or in-kind support. This is supportive of our empirical strategy but does not fully rule out confounders.

Third, account deactivation occurs for routine technical reasons unrelated to household characteristics. Accounts become inactive after SIM card changes, suspicious transactions, incorrect PIN entries, or six months of nonuse. For instance, among bKash wallet owners in the 2024 GiveDirectly census, only 83 percent have active accounts (80 percent among the poorest). Simple reactivation procedures are available through call centers or approaching a mobile wallet agent, requiring wallet holders to follow a simple set of instructions and submit a proof of identity.

Since there are no obvious systematic reasons (access, cost, or usage patterns) why households would have functional bKash accounts over other wallets, using bKash as the delivery platform likely did not systematically exclude certain beneficiary types. Verified households on predefined lists in affected unions formed the treatment group if they had active bKash accounts during verification calls, while those with incorrect, blocked, dormant, or no accounts were excluded from transfers and form the control group. The section on "Balance and robustness" presents balance tests in further support of this evaluation strategy, and discuss other objections and robustness tests.

The sample is constructed by randomly sampling treated and control households within unions that have at least 10 households per group from the WFP lists. For treated households, we sampled 60 households per union (or all available if fewer) transfers sent from 14 to 16 July, and all households receiving transfers on July 30 to exploit variation in timing, given that this group was smaller in size. Control households were similarly sampled proportionally across unions but we oversample treated households to exploit variation in transfer timing and local flood dynamics. The final sample includes 6,803 treated and 2,235 control households across 111 unions (9,038 total).

3.2. Household survey data collection

Due to COVID-19 restrictions and accessibility issues, survey data were collected over the phone. The surveys took place 10 to 12 weeks after the intervention, between 21 September and 8 October 2020. Control households showed marginally lower survey response rates, resulting in a 4.3 percent differential nonresponse conditional on being reached by phone. Phone surveys were conducted in Bangla by enumerators hired and trained through a local survey firm called Data Analysis and Technical Assistance (DATA). The household member whose name was on the WFP records was the one contacted. The phone surveys lasted 30 minutes on average. Respondents were asked a series of questions, including on demographics, behavioral response to the flooding, food consumption, household assets, life satisfaction, work, and use of the cash transfer (if applicable). Respondents received BDT 100 in phone credit for completing the survey.

3.3. Description of the sample and flood experience

The data reflect that the sample of interest is highly vulnerable and badly affected by the 2020 floods. Ninety-seven percent of the sample are female, of whom less than a third have completed primary school. Just over a quarter live in the most fragile of housing structures, which could easily be damaged in the flooding, and there is a high number of dependents relative to adults within households. Eighty-seven percent of control households reported flooding at or above floor level, with 5 percent above roof level.

Using satellite data to measure flood intensity at the mauza level (approximately village level), we estimate that on average, a third of the land area was submerged at peak flooding in mauzas where the sample live. Moreover, the flood extent remained above 50 percent of the local peak for 45 days, on average, an indication that this was one of the most protracted flood events in decades. Although 17 July was the predicted flood peak at the centrally located gauging station used in the triggers, more granular estimates derived from satellite imagery indicate that the most common date of local peak flooding for households in the sample was 22 July. The date of peak flood extent varied across mauzas from mid-July until the beginning of August.

The floods seriously affected lives and livelihoods. Only 60 percent of the control group reported receiving early warning and 53 percent took any action to prepare for the flood. Over half migrated to live elsewhere between the flood and the time of the survey. In terms of asset loss, control households reported losing the same number of small livestock (goats, sheep, and pigs) as they owned at the time of the survey or one animal for every two households. Sixty-one percent of control households reported loss of poultry, another important productive asset. Forty-six percent lost cultivated crops and of these, only half were able to replant by the survey. Food security indicators suggest coping strategies were exhausted. Almost a third of control households reported going at least one day without eating any food during the flood, highlighting the extreme poverty and vulnerability of households in the sample.

3.4. Balance and robustness

While access to digital wallets is close to universal across the sample, the identification strategy depends on the treatment group (those with active bKash accounts) being similar to the control group in every way. In this section, we show that there are few differences between those that received a transfer and those that did not.

Although baseline data are not available, table 1 shows that the treatment and control groups are balanced in most socioeconomic characteristics measured in the follow-up survey that are unlikely to have changed in response to treatment. This is the case when comparing households within mauzas and also within unions. Within mauzas, there is one characteristic on which households are different: respondents in the treatment group are 3 percentage points less likely to have no schooling, 1 percentage point more likely to have completed primary, and 2 percentage points more likely to have completed middle school. These differences are small, but we check robustness by testing whether the results hold for the half of the sample with no schooling (see section on "Robustness"). In addition, to improve power in the analysis, we control for all the individual and household characteristics listed in the table and mauza fixed effects.

We also do not detect any observable differences in use of mobile phones and digital wallets across treated and control households, which supports the identification strategy.

We found that 92.4 percent of treated households reported receiving a cash transfer from World Food Programme (WFP) during the survey, which could reflect compliance issues or survey demand effects. Thus, we estimate and report intent-to-treat effects in the results. There was limited geographic overlap between the cash transfers and other UN interventions received by a small subset of the sample. A higher share of treated households were observed to receive dignity kits from United Nations Population Fund (UNFPA): 14 percent of treated households compared to 7 percent of control households. The analysis shows that results are robust to controlling for these other interventions.

Cash transfers were distributed to treated households over several dates due to a staggered rollout: more than half were sent payments on 14 July 2020, while the remainder were sent theirs on 15, 16, 18, or 30 July. This timing variation may have induced a behavioral response to the treatment, as households receiving later transfers had additional time to reactivate inactive bKash accounts—a process that may have been easier for more educated or tech-savvy individuals. Specifically, some respondents could offer valid but inactive bKash account numbers during the verification calls, and since WFP's final checks were not conducted immediately, households had several days to reactivate these accounts before receiving transfers. While this behavioral response was likely limited in scope as households were never explicitly told they would receive cash transfers during verification, it could still affect the results. To address this concern, the robustness of findings is tested by controlling for transfer date, as discussed in the section on "Robustness".

3.5. Other data used in the analysis

Five months after the floods, additional phone surveys were conducted from 22 to 28 December 2020 as part of a separate analysis of a post-flood cash transfer programme delivered by WFP. Of the 111 unions

Table 1. Balance table of socioeconomic characteristics

	(1) No transfer Mean	(2) Transfer Mean	(1) – (2) Difference (Union FE)	(1) – (2) Difference (Mauza FE)
Individual and household characteristics				
Age	38.320	38.520	−0.200	−0.200*
Female respondent	0.969	0.968	0.001	0.001
Household head	0.187	0.216	−0.029	−0.029
No schooling	0.534	0.506	0.029***	0.029***
Completed primary school	0.305	0.313	−0.008**	−0.008**
Completed middle school	0.098	0.120	−0.023***	−0.023***
Completed high school	0.019	0.025	−0.006	−0.006
Household size	4.643	4.748	−0.105	−0.105
Number of people less than 6 years old	1.537	1.579	−0.041	−0.041
Number of people at least 60 years old	0.442	0.491	−0.049	−0.049
Dependency ratio	0.734	0.759	−0.025	−0.025
Raw material house	0.264	0.269	−0.006**	−0.006
Tin walls and roof house	0.730	0.719	0.012***	0.012
Tin/brick house	0.006	0.012	−0.006	−0.006
Technology use				
Used digital wallet in last six months	0.467	0.476	−0.009	−0.009
Own mobile	0.829	0.797	0.032	0.032
Uses someone else's mobile	0.157	0.186	−0.029	−0.029
Uses mobile at least once a week	0.971	0.963	0.008	0.008
Anticipatory action interventions				
Received World Food Programme transfer (self-report)	0.122	0.924	−0.802***	−0.802***
Received dignity kit from United Nations Population Fund	0.068	0.137	−0.069***	−0.069***
Received feed or storage from Food and Agriculture Organization	0.048	0.071	−0.023	−0.023
Number of observations	2,235	6,803	9,038	9,038

Source: Authors' analysis based on survey data.
Note: The table reports mean values of individual and household characteristics measured in the post-flood survey that are likely to be time invariant, and descriptive statistics on technology use and self-reported receipt of anticipatory action interventions from the World Food Programme, United Nations Population Fund, and Food and Agriculture Organization. The last two columns report the difference in means from ordinary least squares regressions of each variable on the treatment dummy, controlling for union fixed effects in column three, and mauza fixed effects in column four as in the main specification. Robust standard errors were used to correct for heteroskedasticity. The 9,038 households come from 641 mauzas (about 3 km^2 on average) in 131 unions (about 15 km^2 on average). $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

covered in the first round of data collection, 81 unions that were not targeted by these post-flood cash transfers were resurveyed. WFP first selected a subset of unions for the post-flood cash transfer based on observed flood impacts due to a smaller budget, including 30 unions in the original sample. Thus, the 81 unions not targeted by these post-flood cash transfers may have been less affected by the floods. However, within these 81 unions, the allocation to treatment and control for our purposes is unrelated to flood severity.

Within these 81 remaining unions, a sample of treated households was selected using the same principles as in the first survey, whereas the same control households from the first round of data collection were resurveyed.[5] A total of 1,537 surveys were collected within the 81 unions. These surveys provide a further opportunity, albeit with limitations, to test the persistence of results.

[5] Internal data privacy rules on the part of the humanitarian implementation partner meant that we were not given sufficient information to match beneficiaries of the anticipatory cash to the post-cash transfer group.

Using the second round of surveys, supplementary online appendix table S4.1 shows that treatment- and control-group households within the aforementioned 81 unions are balanced on most socioeconomic characteristics—except for education—and their use of mobile phones and digital wallets. Moreover, the follow-up sample is similar to households contacted during the first survey. In this second survey, we also confirm that other sources of cash assistance were very limited during the flood. Only 5 percent of treated households and 6 percent of the control group reported receiving cash from a source other than WFP since the flooding began, mostly from NGOs or government organizations.

The survey data are complemented with satellite-derived estimates of flood timing and severity for each mauza (approximately village level) to allow more granular analysis of the timing of cash transfers. In collaboration with the UN Office for the Coordination of Humanitarian Affairs's Centre for Humanitarian Data and MapAction, we employ the European Space Agency's Sentinel-1 Synthetic Aperture Radar (SAR) imagery with 10-meter spatial resolution. Sentinel-1 SAR data identify water bodies indicated by darker pixels, even in the presence of cloud cover, and have been frequently applied to flood mapping, including in Bangladesh (Uddin, Matin, and Meyer 2019). A change detection and thresholding approach is used to identify flooded areas, based on a methodology developed by the UN-SPIDER Knowledge Portal. This entails comparing pixel intensity and the degree of change relative to a before-flood baseline constructed using the median of all images for the area of interest from December 2019 to January 2020 (a period with no recorded flooding).

To understand the evolution of flooding over time, the change detection process was repeated separately on all available Sentinel-1 data for the area of interest between June 2020 and August 2020. This allows us to estimate the share of area flooded in each mauza for 17 time periods with satellite coverage. As the temporal frequency of the Sentinel-1 imagery can be up to 12 days between images, daily flood extent values are estimated by fitting the Sentinel-1 data points to a Gaussian function. The date of peak flooding (or maximum extent) is identified in each mauza from the modeled time series.[6] These estimates are validated against three independent data sources: (a) river discharge measurements from GloFAS at four measuring stations along the Jamuna River; (b) optical satellite imagery from Sentinel-2; and (c) the perceived extent of flooding from key informants on the ground in 20 unions. Compared to data from the measuring stations, satellite estimates obscure multiple flood peaks occurring in a short period, but overall trends in flooding are very similar. Comparison to optical satellite imagery shows a high degree of overlap with visible surface water. In unions with key informants, perceived flood trends reflect satellite-derived estimates, with similar flood magnitudes in most cases. Further details on the methodology and validation exercise are included in the supplementary online appendix.

3.6. Outcome measures

In constructing the outcome measures, a detailed preanalysis plan, registered in the American Economic Association Registry, is followed.[7] In accordance with our first filing, we examined the follow-up data blinded to treatment assignment and filed a supplement to the preanalysis plan. All data were collected via phone surveys and therefore the outcome measures are limited by this format.

Three measures of household welfare are considered: (a) child food consumption; (b) adult food consumption; and (c) well-being. We focus primarily on food consumption, because the cash was intended to mitigate the effects of the flood shock on food insecurity. Sacrificing food consumption is one of the most frequent coping mechanisms in response to a negative income shock and one that cash is most likely to affect (Aker et al. 2016; Jensen, Barrett, and Mude 2017). A temporary reduction in child food consumption in response to short-term shocks is also likely to have long-run consequences. Given survey data were collected over the phone it was not possible to collect data on consumption, so all of the measures we use to examine food consumption are proxies. Child food consumption is captured by a dummy variable indicating whether children in the household have consumed at least three meals in

[6] When using satellite data, nine mauzas where the Gaussian model fit of flood dynamics was poor were excluded; this accounts for only 0.5 percent of households.

[7] PAP registered here: https://www.socialscienceregistry.org/trials/6576.

the day prior to the survey. This relatively rudimentary measure proxies whether a basic level of food intake is being met and can be measured with a high degree of accuracy over the phone. For ease of exposition this is referred to as "child food consumption" in the results tables and figures. Results on number of meals consumed by children in the day prior to the survey are also reported as a robustness check.

The adult-food-consumption index is a richer measure, albeit still a proxy, capturing quality, rather than quantity, of nutritional intake and dietary diversity. The measure is comprised of two components. The first component captures the number of days during which expensive protein (meat, fish, or eggs) was consumed by any household member in the week prior to the survey. The second component is the food consumption score (FCS) on a scale of 0–112, a measure of general nutritional intake calculated using the frequency in which different food groups were consumed in the week prior to the survey, weighted by nutritional importance (World Food Programme 2009). We exclude rice from this dietary diversity measure: it represents a staple food consumed by over 95 percent of households daily, making it uninformative for identifying variation in food security across our sample, although results remain robust to the inclusion of rice.

Subjective well-being is measured using a 10-scale Cantril ladder of life satisfaction. Flood shocks are likely to be extremely distressing events, and the existing evidence on cash transfers has been shown to have large increases on psychological well-being in the short and long term (Haushofer and Shapiro 2016). Cantril's ladder is widely used in phone surveys through the Gallup World Poll and correlates strongly with other welfare measures, such as income (Deaton 2008). In our preanalysis plan, we prespecified a well-being index that combined Cantril's ladder of life satisfaction with self-reported hours slept the previous night. We reevaluated this decision after reviewing the economics and psychology literature, which revealed that sleep outcomes are extremely challenging to impact through one-off field interventions and may poorly capture psychological well-being when self-reported (Bessone et al. 2021). We excluded this variable from the index in the main results, but discuss robustness to the original index.

To uncover the channels and behaviors through which welfare improvements may have occurred, five additional variables were prespecified: (a) preemptive actions; (b) asset loss; (c) costly borrowing; (d) remittances; and (e) earning potential. The focus on measuring preemptive actions is motivated by the existing evidence showing that actions taken before floods can limit asset loss and damage by up to 50 percent (Kreibich et al. 2005). In the absence of a standard preemptive-actions measure, we employ an index that measures the number of actions taken to prepare for flooding in mid-July before the flood peak, including purchasing food, evacuating, or reinforcing walls. We construct the asset loss index by combining (a) the number of livestock that died in the two months following the flood peak; (b) the number of asset categories that were lost or damaged due to the flood (out of a list of 15); and (c) the area of cultivated crops lost in decimals (1 decimal $\approx$ 40 m^2) due to flooding. The borrowing index is conditional on borrowing and combines both the amount borrowed and the highest monthly interest rate incurred in the two months following the flood peak. We recognize ex post that it is ambiguous in which direction the index should move in order to be welfare-improving for households: being seen to be creditworthy and therefore able to borrow during a crisis could be a useful way of smoothing consumption for credit-constrained households, although growing indebtedness may also be a sign of increasing poverty and vulnerability. We use a dummy variable for whether a household received remittances in the two months after 15 July (just before the flood peak). Lastly, the forward-looking earning-potential index is constructed by combining (a) a dummy variable for whether a household avoided losing crops from flooding or was able to replant, and (b) the number of paid hours of work per adult in the week prior to the survey.[8]

When comparing across measures, all variables are standardized in constructing indices.[9]

[8] As in our PAP amendment, the asset loss index includes a continuous measure of crop area lost (extent of flood damage on the intensive margin), while the earning-potential index includes a binary indicator for whether households retained agricultural capacity by avoiding crop loss or replanting (measured on extensive margin).

[9] We first ensure that all variables are consistently signed within a particular index. When required, we sum individual response items to construct a scale prior to standardizing. We standardize the variables of interest by subtracting the control group mean and dividing by the control standard deviation. If there are multiple subscales within an index, we also sum the standardized subscales, before restandardizing the final index.

4. Empirical strategy

The intent-to-treat effects of the cash transfer on a variety of outcomes is estimated by using the following empirical model:

$$Y_i = \beta_0 + \beta \cdot T_i + \gamma \cdot X_i + \delta_j + \varepsilon_i, \tag{1}$$

where Y_i is the outcome of interest for household i and T_i is an indicator variable for whether household i was identified to receive the anticipatory cash transfer. The variable X_i is a vector of prespecified controls to increase precision in the estimates. These include age, gender, and education level of the respondent; household size; dependency ratio; type of house structure; UNFPA or Food and Agriculture Organization (FAO) recipient status; and land type, derived from satellite imagery at village level as a proxy for flood exposure.[10] In addition, ε_i is a mean zero error term and δ_j is a geographic fixed effect for each mauza (or village) j, each of which is nested within a union. These mauza-level fixed effects represent the finest possible geographic controls, averaging 3 km^2 in size, allowing us to account for local variation in geography and flood impacts.

We also explore whether an earlier anticipatory cash transfer relative to the flood shock matters for household outcomes. Using satellite imagery, we estimate the date of the local flood peak at the mauza level (approximately village level), of which there are 639 in the sample and calculate the number of days prior to the peak that households received their transfer.

The intent-to-treat effect of receiving cash on the date of the local flood peak and the average marginal effect of receiving cash each day earlier relative to the local flood peak, is estimated as follows:

$$Y_i = \beta_0 + \beta_1 \cdot T_i + \beta_2 \cdot T_i \cdot D_i + \gamma \cdot X_i + \delta_j + \varepsilon_i, \tag{2}$$

where T_i is an indicator variable indicating whether WFP identified household i to receive an anticipatory cash transfer. In addition, β_1 captures the intent-to-treat effect of the cash transfer sent on the date of the local flood peak in comparison to the control group; D_i indicates the number of days between the cash transfer and the local flood peak, with negative values indicating that some households received cash after the local flood peak; β_2 captures the average marginal effect of receiving cash each day earlier relative to the local flood peak. For every outcome, the null hypothesis that an earlier cash transfer has no additional impact is tested. Given the flood peak is estimated at the mauza level,we control for union—not mauza—fixed effects in this exploratory analysis.

Robust standard errors are estimated to correct for heteroskedasticity.[11] In addition to presenting p-values from the Wald test, we correct for multiple hypothesis testing across the eight main outcomes of interest. In particular, we present sharpened q-values after correcting for the false discovery rate, following the two-stage procedure developed by Benjamini, Krieger, and Yekutieli (2006).

5. Results

The first question of interest is whether the anticipatory transfer improves welfare. Using equation (1) , we find that children in households receiving the anticipatory cash transfer appear to have significantly higher food consumption than children in control households, even when measured three months after the intervention. Table 2 reports the p-values and sharpened q-values for the standardized treatment effects, as well as the percentage change relative to the control mean.[12] All results reported below remain statistically significant at conventional levels after correcting for the false discovery rate in multiple hypothesis testing.

[10] Variables for land type are defined at village or mauza level. Spatial data are used to categorize each of the 639 mauzas in the sample by the predominant land type. The sample is split into three land types: (a) protected mainland located inside flood embankments; (b) unprotected mainland located outside existing flood embankments; and (c) unprotected char land, which includes low-lying islands along the course of the Jamuna River.

[11] This was the prespecified approach. Robustness of the results to alternative assumptions about the error term, in particular correcting for clustering, will be shown below as well.

[12] The sharpened q-values are sometimes smaller in size than unadjusted p-values, because when most outcomes show low p-values implying many true rejections, more false rejections can be tolerated while maintaining a low false discovery rate.

Table 2. The effect of anticipatory cash transfers on primary outcomes

	Std. treatment effect	Control mean	Δ	% Δ	*p*-value	*q*-value	*N*
Children consumed three meals (0/1)	0.101***	0.80	0.04	+5.0%	0.002	0.007	7,541
Adult-food-consumption index	0.037	–	–	–	0.190	0.105	8,948
Days protein products consumed (0–7)	0.051*	2.66	0.11	+4.1%	0.070	–	8,948
Food consumption score (0–112)	0.019	39.53	0.29	+0.7%	0.514	–	8,948
Life satisfaction (0–10)	0.178***	2.03	0.38	+18.7%	0.000	0.001	8,937
Number of preemptive actions (0–6)	0.075***	0.96	0.09	+9.4%	0.005	0.011	8,944
Asset loss index	−0.074***	–	–	–	0.010	0.014	8,947
Number of livestock died in last two months	−0.060**	0.62	−0.06	−9.7%	0.038	–	8,947
Asset categories lost/damaged (0–15)	−0.034	1.31	−0.04	−3.1%	0.225	–	8,947
Area of cultivated crops lost (decimals)	−0.049*	15.73	−1.65	−10.5%	0.094	–	8,947
Borrowing index	−0.049	–	–	–	0.136	0.100	5,996
Amount borrowed in last two months (in Bangladeshi Taka, BDT)	−0.028	8,595.78	−285.76	−3.3%	0.387	–	5,996
Highest interest rate (percent/month)	−0.047	4.63	−0.26	−5.6%	0.164	–	5,996
Received remittances (0/1)	0.041	0.08	0.01	+12.5%	0.172	0.105	8,947
Earning-potential index	0.051*	–	–	–	0.070	0.060	8,941
No. crops lost/able to replant (0/1)	0.073***	0.64	0.03	+4.7%	0.010	–	8,941
Paid hours of work/adult last week	0.002	9.25	0.03	+0.3%	0.933	–	8,941

Source: Authors' analysis based on survey data.
Note: The first column shows the standardized mean treatment effect for prespecified outcomes (in bold) and subindices. The second column shows the control mean, followed by the nonstandardized treatment effect (Δ) and percentage change in the treatment group relative to the control mean (% Δ). Robust standard errors were used to correct for heteroskedasticity. The *p*-values are reported on all outcomes and subindices. False discovery rate *q*-values for eight hypotheses are calculated over the main outcomes following the sharpened two-stage procedure of Benjamini, Krieger, and Yekutieli (2006). Covariates include age, gender, education level, household size, dependency ratio, house structure, recipient status for United Nations Population Fund and Food and Agriculture Organization-led interventions, and land type. Mauza fixed effects are included. The sample comes from 552 mauzas (about 3 km^2 on average), although the number falls to 535 for child food consumption and 498 for the borrowing outcome. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Children in treated households are 5 percent more likely to have consumed three or more meals in the day prior to the survey, relative to a control mean of 80 percent. While recognizing that this measure is fairly rudimentary, improved child-food-consumption months after the flood shock is salient in light of a large body of literature highlighting the effects of short-term disruptions in nutritional intake for children on long-term educational, earnings, and health outcomes—even decades later, such as in Maccini and Yang (2009). In contrast, we do not observe significantly higher adult food consumption for treated households compared to control households. Note that the adult-food-consumption measure is a richer measure capturing the quality of food consumption by assessing nutritional intake and dietary diversity, whereas the child-food-consumption measure captures consumption at the extensive margin and is thus not directly comparable.

The positive effect on the number of meals consumed by children measured three months after the intervention is likely to be a lower bound compared to the size of the effects at the height of the flood shock. Although we did not collect survey data during the flood, households in the second round of phone surveys were asked to recall the number of days any adult member went a whole day without eating during the flood in mid to late July 2020. Nearly a third (29 percent) of control households reported going a full day without eating. Compared to control households, treated households were 62 percent less likely to go a full day without eating during the flood (see table 5). These results are consistent with the self-reported use of the anticipatory cash transfer, with 91 percent of treated households reporting spending the cash on food or water, among other categories.

Subjective well-being was also higher in treated households: three months after the transfer, average life satisfaction for respondents in treated households was 0.178 standard deviations or 18.7 percent

higher than in the control group. However, even with this increase, treated households continue to report an extremely low level of well-being, given the control group mean of 2 on a 10-item Cantril ladder of life satisfaction, compared to the national average score of 5.

Households receiving the anticipatory cash transfer were also more likely to mitigate asset loss. Households receiving the cash transfer had an asset loss index that was 0.074 standard deviations lower than the control group, with a reduction in loss and damage for livestock, household assets, and crops. Treated households show some evidence of having a higher potential to recover faster, reporting a weakly significant 0.051-standard-deviation increase in the earning-potential index compared to control households. Treated households were 4.7 percent more likely to report avoiding crop loss or being able to replant, but were no more likely to work for a wage or work additional hours at the time of the survey (see table 3 on secondary outcomes). The cash transfer appears to be playing a small but not insignificant role in post-flood recovery, helping to restore livelihoods and the capacity to cope with future shocks.

There is no evidence that households receiving the anticipatory cash transfer borrowed less or were less likely to receive remittances. While treated households are slightly less likely to borrow at all (table 3), they do not borrow significantly less or at cheaper rates conditional on borrowing in the months during and after the floods. This may reflect offsetting effects: the cash transfer could have made households both more creditworthy and less in need of credit to cope with the flood.

There are many channels through which these results could have emerged. Although we cannot definitively point to one channel, we provide evidence in this section showing that the anticipatory cash transfers likely changed behavior before the flood, either through increased liquidity or by signaling information about the likelihood of a forthcoming flood. Together with the improvements observed in the wide range of outcomes discussed above, these results suggest the timing of cash transfers is important, and specifically, the effectiveness of receiving cash early in the trajectory of shocks.

Table 2 shows that treated households took 9.4 percent more actions in preparation before the flood peak, compared to control households. Table 3 reports results on a range of secondary outcomes, including all types of actions from the preemptive action index adopted by more than 5 percent of the sample, again using the specification in equation (1). In the absence of the cash transfer, only half (53 percent) of control households took any action prior to the flood to reduce its impacts, with a control mean of one action per household. In contrast, treated households were 11 percent more likely to take any action to prepare. For instance, treated households were on average 11 percent more likely to evacuate household members, 23 percent more likely to evacuate livestock, and 11 percent more likely to purchase food ahead of the flood peak, which is also consistent with the self-reported use of the anticipatory cash transfer.[13] Not only were treated households more likely to evacuate livestock, but they were also less likely to lose small livestock and poultry.[14] We find no differences in the loss of larger livestock (cows, calves, and buffalo), which are owned by far fewer households.

It is not possible to disentangle the role of information and liquidity in driving these differences in preemptive actions. Although we do not observe impacts on the borrowing index, the sample had high liquidity needs during and following the flood shock. Seventy percent of control households borrowed money between the start of the flood and the survey, borrowing an average amount of BDT 8,595 or USD $261 at 2020 PPP rates. Treated households were marginally less likely to borrow (significant only at 10 percent), but were significantly more likely to buy food before the flood (see table 3). Thus, it is possible that the cash transfer provided much needed liquidity to cash-strapped households.

Yet the anticipatory cash transfer could have also played an important role in conveying information about the severity of the oncoming flood. In the absence of a credible early warning system, the information component of the anticipatory cash transfer could be particularly powerful. Only 60 percent of

[13] Most treated households reported spending the cash on food or water (91 percent of households), followed by medicine or health services (32 percent), agricultural inputs (26 percent), loan repayments (12 percent), clothing (11 percent), and repairs on home or assets (11 percent).

[14] Treated households own 17.2 percent more small livestock (goats, sheep, and pigs) at the time of the survey relative to a control mean of 0.54 animals per household.

Table 3. The effect of anticipatory cash transfers on secondary outcomes

	(1) Any action to prepare (0/1)	(2) Evacuated household (0/1)	(3) Evacuated livestock (0/1)	(4) Purchased food (0/1)	(5) Lost small livestock (0/1)	(6) Lost poultry (0/1)	(7) Lost large livestock (0/1)	(8) Borrowed money (0/1	(9) Worked for wage (0/1)
Intent-to-treat effect	0.057***	0.033**	0.039***	0.041***	−0.027**	−0.022	−0.008	−0.025*	0.020
	(0.014)	(0.013)	(0.010)	(0.014)	(0.013)	(0.014)	(0.007)	(0.014)	(0.014)
Control mean	0.53	0.30	0.17	0.38	0.31	0.61	0.07	0.70	0.68
% Δ	10.7%	11.0%	23.2%	10.8%	−8.5%	−3.7%	−13.0%	−3.6%	3.0%
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mauza fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	8,944	8,944	8,944	8,944	8,949	8,947	8,949	8,943	8,947
R^2	0.17	0.17	0.14	0.19	0.11	0.23	0.08	0.12	0.13

Source: Authors' analysis based on survey data.
Note: The first row shows the mean treatment effect for a range of secondary outcomes, controlling for the same covariates and mauza fixed effects as in the main results. The sample comes from 552 mauzas (about 3 km^2 on average). Robust standard errors were used to correct for heteroskedasticity. The second row shows the control mean, followed by the percentage change in the treatment group relative to the control mean (% Δ). We list all types of actions from the preemptive action index adopted by more than 5 percent of the sample. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

control households reported receiving early warning ahead of the floods and treated households were 5 percent more likely to report receiving early warning ($p = 0.031$). Disentangling the role of information from liquidity warrants future research.

Overall, we find significant effects of the anticipatory cash transfer across a wide range of outcomes, albeit modest in magnitude. The delivery of the intervention meant that both treated and control households are present within the same unions and mauzas. The possibility that negative spillover effects affect the results cannot be ignored. For example, subjective well-being may be affected from seeing others get support but not oneself, or there may be price effects through increased liquidity. We cannot provide any direct evidence on this. However, we note that the protective and preemptive actions taken by recipients were unlikely to have negative externalities on nonbeneficiaries, making it unlikely that the effects are primarily driven by negative spillovers.

6. Robustness

The primary threat to causal identification is that limiting transfers to households with active bKash accounts may have created systematic differences between treatment and control groups beyond the transfer itself. The balance tables suggest that any such differences are likely minimal: treated and control households within the same mauzas are statistically indistinguishable across demographics, asset ownership, digital account ownership, and digital account use, with only a small difference in education levels. While we cannot definitively rule out all differences, we conduct robustness tests to address this concern within the constraints of our data. The evidence does not suggest that the treatment effects are driven by differences in observed or unobserved characteristics.

First, we test whether the welfare results persist when restricting the sample to households with no formal education (see supplementary online appendix table S1.2). Despite halving the sample, the welfare effects remain similar, although significance is lost on the number of preemptive actions and asset loss index.

Second, the robustness of the results to the potential behavioral response to treatment is tested. Some households received transfers on later dates (15, 16, and 30 July) rather than 14 July, as WFP needed more time to complete verification calls and check bKash accounts. This potentially allowed some households to reactivate dormant bKash accounts and become eligible for transfers. We test whether the results hold for households that received the transfer on 14 July, as these households had no time to reactivate their accounts. In table 4, we allow for differential treatment effects by transfer date, where households receiving transfers on the earliest date (14 July) form the base group. Given that the 14 July group includes more than half the sample and the groups with the later transfer dates are each much smaller in size, we test whether the three later groups are jointly statistically different from the 14 July group.[15]

The treatment effects across all outcomes for the 14 July group—arguably the cleanest—almost exactly mirror the main results with the entire sample. Moreover, we find little evidence that there are systematic differences between the earliest and later transfer dates. The main exceptions are the higher life satisfaction for some of the later dates (15 and 16 July) and the lower borrowing index for households receiving cash on 30 July relative to the 14 July group. The latter difference could be driven in part by the start of Eid al-Adha at the end of July, when liquidity needs are typically high.

The main results are also robust to a range of alternative model specifications, reported in supplementary online appendix table S1.1. The main findings remain consistent when using union rather than mauza fixed effects and when clustering standard errors at the mauza level to account for potential treatment effect heterogeneity across mauzas (Abadie et al. 2023). The results are also robust to winsorizing outcomes at the 95th percentile, excluding covariates, and using Lasso-selected controls.

Furthermore, the treatment effect on the number of meals consumed by children remains meaningful and significant when defining the outcome as the number of meals consumed, boosting meals by 0.085

[15] In the preanalysis plan, we prespecified an alternative model comparing households that were identified to receive the cash transfer on 14, 15, and 16 July to households that were identified to receive the transfer on 30 July (see supplementary online appendix table S3.2).

Table 4. Thedifferential effects of an anticipatory cash transfer, by transfer date

	(1) Child food consumption	(2) Adult-food-consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Borrowing index	(7) Remit-tances	(8) Earning-potential index
Intent-to-treat effect	0.099***	0.053*	0.143***	0.076***	−0.075**	−0.014	0.037	0.044
	(0.035)	(0.031)	(0.033)	(0.029)	(0.031)	(0.038)	(0.033)	(0.031)
Transfer∗15July	−0.026	−0.015	0.075**	0.062*	0.081**	−0.036	−0.005	−0.029
	(0.039)	(0.036)	(0.037)	(0.033)	(0.033)	(0.042)	(0.038)	(0.034)
Transfer∗16July	0.064	−0.032	0.075*	−0.043	−0.070**	−0.056	−0.005	0.046
	(0.041)	(0.037)	(0.041)	(0.034)	(0.035)	(0.046)	(0.041)	(0.037)
Transfer∗30July	−0.018	−0.062	0.063	−0.058	−0.039	−0.137***	0.039	0.052
	(0.044)	(0.040)	(0.043)	(0.036)	(0.038)	(0.045)	(0.047)	(0.039)
p-value for joint *F*-test	0.825	0.178	0.012	0.583	0.712	0.018	0.744	0.376
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mauza fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7,541	8,948	8,937	8,944	8,947	5,996	8,947	8,941
R^2	0.10	0.15	0.17	0.18	0.20	0.17	0.10	0.17
Control mean	0.80	0.00	2.03	0.96	0.00	0.00	0.08	0.00

Source: Authors' analysis based on survey data.
Note: The first row shows the standardized mean treatment effects for the prespecified outcomes for households that were identified to receive the cash transfer on the earliest date, 14 July. The subsequent rows show the marginal effect of receiving the cash transfer on 15, 16, and 30 July respectively, relative to the 14 July group. We test whether the three later groups are jointly statistically different from the 14 July group and report the *p*-value from the test. We control for the same covariates and mauza fixed effects used in the main analysis. The sample comes from 552 mauzas (about 3 km^2 on average), although it falls to 535 for child food consumption and 498 for the borrowing outcome. Robust standard errors were used to correct for heteroskedasticity. Please note that the control mean is zero for indices standardized relative to the control group. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

standard deviations relative to the control group ($p < 0.05$). We find a marginally significant effect of 0.53 standard deviations ($p < 0.1$) of the anticipatory cash transfer on the well-being index as originally prespecified, which combines Cantril's life satisfaction with self-reported hours slept the previous night.

The results are also not driven by other support that was offered by FAO and UNFPA to a small subset of the sample with limited geographic overlap at the same time as WFP (see "Sample and data" section for more detail). While estimated treatment effects for those receiving this other support are somewhat different to the overallsample on some measures, supplementary online appendix table S1.2 shows that accounting for these subsamples does not affect any of the main conclusions.

Using Lee bounds (Lee 2009), results on the number of meals consumed by children and asset loss remain robust to correcting for a differential nonresponse of 4.3 percent, conditional on being reached by phone (supplementary online appendix table S4.2). The preemptive action index and life satisfaction measure become only marginally significant for the lower-bound estimates, but remain positive, albeit of a smaller magnitude.

7. Results five months post-intervention

We explore the effects of the anticipatory cash transfer on household welfare five months after the intervention in the subset of unions covered by another round of phone surveys. As noted in the "Sample and data" section, the second round of surveys was conducted in a subset of unions covered by the first round of data collection. We employ the same empirical strategy as outlined in the "Empirical strategy" section.

Table 5 illustrates the standardized treatment effects on the three welfare outcomes for the two rounds of data. For comparability, welfare results are presented from the first survey using only 81 unions (out of 111) covered in both rounds of data collection. Using this smaller set of unions, the treatment effects in the first survey mirror those in the main specification with the full sample. The subset of unions covered by both surveys is thus not dissimilar to the full sample of unions considered in the earlier analysis.

Table 5. The effect of anticipatory cash transfers three and five months later

	Round 1 (10–12 weeks)				Round 2 (20 weeks)			
	Treatment effect	**Control mean**	**% Δ**	***N***	**Treatment effect**	**Control mean**	**% Δ**	***N***
Day without eating in flood (0/1)	–	–	–	–	−0.181***	0.29	−62.2%	1,441
Days without eating in flood	–	–	–	–	−0.462***	0.65	−71.1%	1,441
Children consumed three meals (0/1)	0.092***	0.80	4.5%	6,670	−0.080	0.87	−3.1%	1,177
Adult-food-consumption index	0.040	–	–	7,912	0.361***	–	–	1,441
Days protein products consumed (0–7)	0.054*	2.67	4.2%	7,912	0.317***	3.02	21.5%	1,441
Food consumption score (0–112)	0.022	39.43	0.9%	7,912	0.359***	38.49	14.4%	1,441
Life satisfaction (0–10)	0.185***	2.00	19.8%	7,902	−0.009	2.45	−0.7%	1,440
Asset loss (investment) index	−0.068**	–	–	7,912	−0.037	–	–	1,441
Borrowing index	−0.056	–	–	5,325	−0.024	–	–	680
Received remittances (0/1)	0.045	0.08	16.0%	7,910	0.056	0.11	16.3%	1,440
Earning-potential index	0.049*	–	–	7,904	−0.074	–	–	1,441

Source: Authors' analysis based on survey data.
Note: The table shows the effect of the cash transfer on outcomes measured in the first survey three months after the cash transfer and the follow-up survey five months after the transfer. The sample is restricted to households located within the 81 unions covered by both rounds of data collection. This represents 439 mauzas for round 1 and 210 mauzas for round 2 (exact numbers depend on the outcome measure). A mauza is about 3 km^2 on average. Two outcomes were modified in the five-month follow-up survey to better capture flood recovery. Instead of self-reported asset loss, we report results on an asset investment index, constructed using the net increase in livestock and household assets since early August 2020, i.e., after the flood peak. The earning-potential index was reduced to number of hours worked in the last seven days and no longer included the ability to replant. The first column shows the mean treatment effect on going without food during the flood (first two outcomes) and the standardized mean treatment effect on other prespecified outcomes. The second column shows the control mean, followed by the percentage change in the treatment group relative to the control mean (% Δ). These columns are not defined for composite indices since the control mean is zero by construction. Covariates include age, gender, education level, household size, dependency ratio, house structure, recipient status of United Nations Population Fund and Food and Agriculture Organization-led interventions, and land type. Mauza fixed effects are included. Robust standard errors were used to correct for heteroskedasticity. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Five months after the intervention, we find a strong positive effect of the anticipatory cash transfer on adult food consumption—an increase of nearly 0.3 standard deviations relative to the control group. This effect is driven by a large increase in both adult protein consumption and general nutrient intake, as proxied by the adult-food-consumption score measured in the week prior to the survey. In contrast, there are no treatment effects on child food consumption and life satisfaction five months after the cash transfer, unlike in the earlier survey. As shown in table 5, the control households experience substantial improvements in their child food consumption and life satisfaction between the two survey rounds, thus converging with treated households. Significant treatment effects are not observed on any other primary outcome five months after the intervention.

Taken together, it seems that, after cutbacks in meals eaten during the crisis, control households managed to increase child food consumption back to comparable levels as treated households after five months. The anticipatory cash transfer avoided some of these consumption cuts and had longer-term welfare benefits for treated households through improved adult food consumption.

8. How important is timing?

Given the findings presented thus far, a key question becomes understanding the importance of the timing of transfers.[16] The most relevant question is whether an anticipatory cash transfer is more impactful compared to a cash transfer received after the floods—the usual humanitarian response. The current design does not allow this direct comparison to be made. Nevertheless, the United Nations' Central Emergency Response Fund (CERF), which funded this intervention, reported that the intervention evaluated here reached people with cash approximately 100 days faster than a similar flood response

[16] In our preanalysis plan, we also prespecified that we would explore heterogeneity by land type. There are no clear systematic differences across the land categories (see supplementary online appendix table S3.1) and it is not evident that land type is indeed the best proxy of vulnerability to flooding at a household level.

in Bangladesh in 2019, and reportedly at half the operational cost (UN OCHA 2023). As the first survey took place about two to three months after the floods, this would have been around the time that the usual humanitarian responses would arrive. Therefore, the control group provides an indication of how badly off likely recipients would have been before the transfer, suggesting that with the usual humanitarian response, the transfer group would have experienced three months of significantly worse living conditions compared to those benefiting from an anticipatory action intervention.

Ideally, we would also like to know whether receiving the transfer prior to the flood occurring was better than receiving a transfer quickly after the floods. When providing a transfer in anticipation of a weather event, targeting relies on a prediction of the event at a sufficiently granular level, which may carry some forecasting error. Accepting this error in targeting is only worth it if either the error is small enough or the benefit of acting on a prediction is significantly higher than the benefit of acting after the realization of the event. Again, we cannot directly answer this question with the evidence we have in this pilot, and it is possible that the forecasts at the central gauging station do not uniformly capture flooding at village level. However, we note the strong degree of accuracy in the forecasts in this specific context (fig. 1) and that households that received the transfer undertook more preemptive actions. This suggests that receiving the transfer prior to the flood date did meaningfully affect behavior and could be one driver of the welfare differences observed after the flood. The probable impact of the anticipatory cash transfer in reducing food insecurity during the floods also points to the large benefits of receiving the transfer early. Although this measure is relatively crude and only measured in the second survey five months after the cash transfer, it captures a highly distressing event that we expect would be relatively easy to recall.

The evidence pointing to the benefits of receiving transfers prior to the flood, along with the very low predictive errors in Bangladesh, suggests that the benefit of providing anticipatory transfers in this case is likely to outweigh costs of targeting errors. However, this trade-off would need to be assessed in each case of anticipatory action. Further evidence on the benefits of being early across different settings can help inform what precision in predicting extreme events is needed to make this intervention cost effective. It is worth noting that even if the transfers are distributed when flooding does not ultimately occur, these payments still go to poor households and therefore generate welfare benefits.

To further explore the importance of timing, we exploit variation in the delivery date of cash transfers and local flood dynamics to assess further whether small changes in the timing of the cash relative to flooding matter for household welfare.

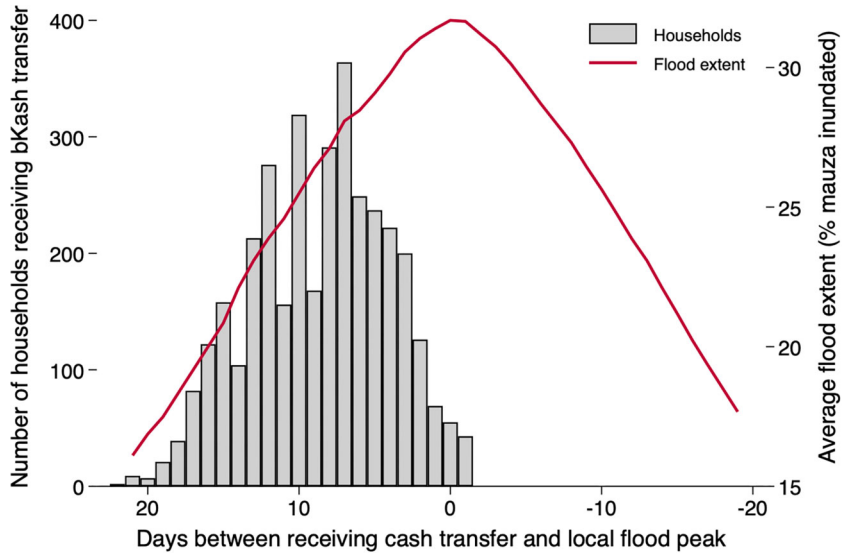
The number of days between 14 July (the date of the first transfer) and the local flood peak in 630 mauzas is calculated based on high-resolution satellite data. For this analysis, we just consider households receiving the cash transfer on 14 July, as per the robustness tests presented in the "Robustness" section. Figure 2 shows the number of households receiving cash by the number of days between the transfer and the local flood peak, where negative days indicate cash sent after the flood peak. Most households were sent the cash before their local flood peak—on average, about seven days prior in the full sample and nine days prior for the 14 July group.

The effect of receiving cash one day earlier is estimated using linear regression, as described in equation (2) in the "Empirical strategy" section. Table 6 reports the average treatment effect of receiving cash on the date of the local flood peak and the marginal effect of receiving the cash each additional day before the flood peak. The average treatment effects on the date of the local flood peak are generally consistent with the results in the "Results" section, except for child food consumption. Furthermore, we find suggestive evidence that receiving cash a day earlier improves outcomes for child food consumption by 0.015 standard deviation and life satisfaction by 0.011 standard deviation. Neither result survives correction for multiple hypothesis testing and should thus be considered exploratory.

9. Conclusion

In the face of increasing climate volatility and stretched aid budgets, a better understanding of how to support households effectively in times of crisis is needed. This paper provides evidence on the impact of an anticipatory cash transfer delivered in advance of predicted floods, rather than in their aftermath.

Figure 2 Number of households receiving cash on 14 July relative to the local flood peak date



Source: Authors' own calculations, using Sentinel-1 SAR data.
Note: The number of households receiving the cash transfer on 14 July is shown on the left-hand axis for each day relative to the date of local flood peak (at zero). The average extent of flooding in mauzas with treated households is shown on the right-hand axis. Local flood peak dates are derived from a Gaussian model of maximum flood extent at mauza level estimated using satellite imagery. Observations are excluded where errors were identified in Gaussian model fit, and outlying dates are excluded by trimming at the 1 percent and 99 percent levels.

Table 6. Treatment effects of an earlier cash transfer on primary outcomes

	(1) Child food consumption	(2) Adult-food-consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Borrowing index	(7) Remit-tances	(8) Earning-potential index
Intent-to-treat (ITT) effect	−0.062	−0.012	0.025	0.116**	−0.117**	−0.014	0.016	0.011
	(0.064)	(0.056)	(0.058)	(0.054)	(0.053)	(0.066)	(0.060)	(0.055)
ITT × days	0.015**	0.007	0.011**	−0.006	0.000	0.000	0.002	0.005
before flood peak	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)
p-value: ITT × days	0.014	0.169	0.044	0.225	0.942	0.957	0.772	0.364
q-value: ITT × days	0.131	0.512	0.182	0.512	0.917	0.917	0.917	0.776
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	4,897	5,765	5,759	5,765	5,764	3,906	5,764	5,761
R^2	0.05	0.09	0.10	0.10	0.13	0.11	0.04	0.12
Control mean	0.80	0.00	2.03	0.96	0.00	0.00	0.08	0.00

Source: Authors' analysis based on survey data.
Note: The first row reports the standardized mean treatment effect when cash is sent on the date of the local flood peak. The second row shows the average marginal treatment effect of receiving the transfer a day earlier relative to the local flood peak. The sample is restricted to households sent cash on 14 July. Robust standard errors were used to correct for heteroskedasticity. The *p*-values test whether the interaction coefficient (the marginal treatment effect) is statistically different from zero. False discovery rate *q*-values are calculated over the marginal treatment effects for the eight outcomes following the sharpened two-stage procedure of Benjamini, Krieger, and Yekutieli (2006). We control for the same household characteristics used in the main analysis and union fixed effects. Please note that the control mean is zero for indices standardized relative to the control group. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

We find that such transfers likely increase the likelihood that households take action to mitigate the impact of the floods, reduce asset losses, and improve welfare three months after the flood. The effect sizes after three months are modest, but surprisingly broad, for a one-off cash transfer amounting to two weeks' of food expenditure but delivered during floods that lasted much longer.

The results suggest important takeaways for crisis response. First, early action in a crisis matters. The welfare impacts measured in this paper occurred before humanitarian assistance would have typically arrived and impact outcomes that are known to have scarring effects, such as on child food consumption. Second, even when acting early, timing matters—in particular, acting before a crisis can allow households to take action that could reduce its impact.

Anticipatory action requires upfront investments in weather prediction, advance preparation, and targeting systems that can scale immediately when triggers activate. At a national level, anticipatory action would require government or humanitarian partners to commit to scaling social protection or humanitarian programs based on predefined triggers, secure prearranged financing, and obtain necessary preapprovals to allow funds to move quickly through the system when needed. Reaching new beneficiaries during crises is more challenging and time consuming than supporting existing beneficiaries (World Bank 2022). Social registries and digital payment systems are thus essential for quick expansion, as this study demonstrates. In the absence of comprehensive social registries, a data-driven targeting method, such as that developed by Aiken et al. (2022), can enable rapid household enrollment during emergency responses.

Anticipatory action also requires that the benefits of early cash transfers exceed the costs of forecasting and targeting errors. Bangladesh's success stems from its highly accurate riverine flood forecasting, built on decades of hydrological investments and favorable geography. Since these are poor households who primarily spent cash on food, providing transfers would likely generate some benefits even in nondisaster years. However, even assuming zero impact when floods do not occur, the program would still deliver a high proportion of observed results given the high accuracy of flood predictions. As forecast accuracy declines, program impacts diminish, raising the question of what accuracy threshold makes anticipatory action worthwhile compared to post-disaster response. Further research is needed to directly compare anticipatory responses based on predicted impacts with rapid post-disaster responses, adjusting estimated benefits according to model accuracy.

This was not a small experimental pilot: about 100,000 people were reached through the intervention we study, approximately 3 percent of the population in the area in which it was implemented. The welfare impacts that we observe may already be inclusive of some general equilibrium effects, such as impacts on food prices, that may result from a broad implementation of this type of intervention. However, we note that this is a setting where price effects are likely minimal, because local markets are well integrated with external markets. However, in other contexts with less market integration, price effects may be a more significant concern. As with nonemergency cash transfers, the optimal anticipatory intervention depends on local market conditions, and policymakers should consider the risk of price spikes that disproportionately harm the poorest, drawing on insights from the extensive literature comparing food versus cash transfers.

More work is needed to test the relative value of anticipatory versus post-shock cash transfers. The relative importance of liquidity versus information in anticipatory cash transfers needs to be disentangled, building on work showcasing the value of information about climatic shocks in similar settings (Rosenzweig and Udry 2019; Burlig et al. 2024; Patel 2023). These are avenues for future research.

Data Availability Statement

The data underlying this article will be shared on reasonable request to the corresponding author.

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