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

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We evaluate the effect of a cash transfer to households forecast to experience extreme flooding in Bangladesh five days before the flood peak based on hydrological modelling. We find that this 'anticipatory' transfer improved welfare during and three months after the flooding, reduced asset loss and supported early recovery. Early cash also 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. Acting one day earlier near the flood peak is welfare improving.

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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 *et al.*, 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 (Birkmann *et al.*, 2022; Rohde, 2023; Rodell and Li, 2023). 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 *et al.*, 2017; Asfaw *et al.*, 2017; Adhvaryu *et al.*, 2022; Macours, Schady, *et al.*, 2012). However, the billions spent in responding to extreme weather events tend to be mobilised after they occur and some of the negative impacts have already materialised, 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 (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 five days prior to the flood peak and one hundred 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, we compare households that were sent an anticipatory cash transfer to otherwise comparable households that did not. Both treated and control households were sampled from the same pre-existing lists, but treated households had access to a specific active digital wallet - a bKash account - at the time of a verification call, which was used to deliver the cash transfer. As the common delivery mechanism for government benefits, digital wallets are widespread in this area. bKash is only one of 16 possible digital wallets, all with similar functionality, costs and benefits. Organisational constraints meant that the humanitarian implementation partner could only contract bKash to deliver the cash transfer, as the largest digital wallet with about 40% market share (Tahasin and Hoque, 2022). Those found to have a different digital wallet or inactive bKash account at the time of the verification call were therefore excluded from receiving the cash transfer, and formed the control households. Ten to twelve weeks after the cash intervention, we survey over 9,000 households via the phone. We found no differences in the use of mobile phones or digital money between control and treatment households, providing further support to our evaluation strategy.

We find that the anticipatory cash transfer improves welfare three months after the intervention and floods had dissipated. Children in treated households were 3.8% 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 under-nutrition (Dercon and Porter, 2014; Dinkelman, 2017). The cash transfer also mitigated the flood's impact on subjective wellbeing, with respondents in treated households reporting 18.7% higher life satisfaction on average. These welfare effects were measured before the usual humanitarian response reached affected households, highlighting the cost of a late response. Moreover, using a second survey with a subset of the sample, treated households recalled that they were 52% less likely to go a full day without eating during the floods, suggesting that the immediate relief was also significant.

We provide evidence to show that the anticipatory cash transfer increased the choice set of actions available to households before the flood peak. Compared to control households, households that were sent the anticipatory cash transfer took more action in preparation for the oncoming floods, including evacuating households members and livestock, and purchasing food. The anticipatory cash transfer mitigated asset loss and boosted earning potential as an early sign of recovery three months later.

Taken together, the results on increased pre-emptive action and the subsequent improvements in welfare suggest that the timing of cash transfers relative to shocks matter. The anticipatory cash transfer enabled 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. Although some of the effect sizes are small, it is nonetheless striking that we measure positive effects on a wide range of outcomes. In the absence of credible early warning systems, the anticipatory cash transfer likely provided much needed liquidity and information about the severity of the incoming flood. This aligns with other studies showing that information about climatic shocks can shift beliefs and ex-ante behaviours in similar settings (Rosenzweig and Udry, 2019; Burlig *et al.*, 2024; Patel, 2023).

We conduct exploratory analysis where we exploit variation in the delivery date of the cash transfers and local flood dynamics using satellite data to provide further evidence that an earlier cash transfer is more effective. For every day earlier that the cash transfer was delivered relative to the local flood peak, the stronger the treatment effects on adult food consumption and the borrowing index. Using non-parametric methods, we also illustrate that a faster response relative to the evolution of the flood is welfare improving. A phone survey two months later than the first one further revealed a strong positive treatment effect on adult food consumption, but no effects on child food consumption and life satisfaction. Control households experience significant improvements in the latter two outcomes, thus converging with treated households.

This research adds to existing evidence in three ways. First, this paper is the first rigorous evaluation of the impact of a cash transfer provided to households just in advance of a sudden onset humanitarian crisis. As such, it adds unique evidence to the large literature on the role of cash transfers in cushioning the negative income effects of shocks. 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 *et al.*, 2017; Adhvaryu *et al.*, 2022; Bottan *et al.*, 2021). Several experimental papers combine survey and satellite data to show that monthly cash transfers can help protect households against low rainfall shocks or drought, albeit sometimes only in combination with other interventions (Hou, 2010; Macours, Schady, *et al.*, 2012; Premand and Stoeffler, 2022). However, well-identified evidence on the effectiveness of cash transfers delivered specifically in response to extreme weather events remains scarce, especially for one-off cash transfers to households. A randomised control trial implemented in Sri Lanka several months after the 2004 tsunami found that a one-off cash grant to firms facilitated business recovery and investment (De Mel *et al.*, 2012). In the aftermath of a slow-onset disaster (a drought) in Nicaragua, a bi-monthly conditional cash transfer was shown to have positive effects on education and nutrition, and when combined with vocational training or a productive investment grant, income diversification (Macours, Schady, *et al.*, 2012; Macours and Vakis, 2014; Macours, Premand, *et al.*, 2022). Our 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, we show that the timing of cash transfers matters 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 (Aker, 2017; Skoufias *et al.*, 2013; Cunha, 2014; Hidrobo *et al.*, 2014; Haushofer and Shapiro, 2016; Cunha *et al.*, 2019). Kansikas *et al.* (2023) elicits preferences over design features, such as tranching and timing relative to the agricultural season. 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 *et al.*, 2012; Ivaschenko *et al.*, 2020; Macours, Schady, *et al.*, 2012; Macours, Premand, *et al.*, 2022), although guaranteed access to credit provided after a flood can induce ex-ante behavioural change (Lane, 2024). In contrast, our paper rigorously showcases the potential for an anticipatory cash transfer to limit the initial welfare cost of the shock by enabling pre-emptive action.[1]

Third, our paper also makes a significant contribution to the literature on humanitarian evaluations, where rigour has been too rare. A review of 900 studies on humanitarian programmes found that only 31 could be classified as impact evaluations, of which eight 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 (for example: Schwab, 2019). Weingärter et al. (2020) specifically review the evidence for anticipatory humanitarian action and find that rigorous evidence of impact is limited. The reasons few experimental studies exist are varied. For instance, it is difficult to justify the ethics of a randomised control trial in life-or-death situations. The need for speed and the lack of transparency in implementation often obfuscates the identification of a valid counterfactual. It is challenging to conduct a baseline when it is unknown *a priori* where a disaster will strike. Disasters ex-post disrupt the supply of basic services and infrastructure, including those needed for data collection. We address these challenges present in our own context by exploiting variation created by incomplete targeting due to the use of a single mobile money provider and collecting a large post-intervention survey over the phone (also a necessity during the COVID-19 pandemic).

Our paper is structured as follows. The next two sections describe the intervention and sample. Section 4 outlines our empirical strategy and measurement. In Section 5, we present our results on the effects of cash relative to no cash, and the channels through which these welfare improvements might occur. Section 6 demonstrates the robustness of our results to alternative model specifications. We explore how the impacts varied with the timing of the cash transfer in Section 7. Section 8 uses a second round of cross sectional data to present effects of the cash transfer five months later, and Section 9 offers conclusions.

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 (United Nations Resident Coordinator Office, 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

[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, Bailey, *et al.*, 2019; Gros, Easton-Calabria, *et al.*, 2020). However, these papers suffer from power and identification problems due to much smaller sample sizes and imbalanced control groups.

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 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 pre-agree 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) pre-agreed 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 was used to trigger the release of pre-agreed cash transfers prior to the highest flood levels. With support from IFRC and the Bangladesh Red Crescent Society, the World Food Programme (WFP) sent BDT 4,500 – USD $137 at 2020 PPP rates[2], equivalent to approximately two weeks of household food expenditure (World Bank, 2019) – using mobile money to 23,434 vulnerable households.[3] These households were located across 131 unions, the smallest administrative unit in Bangladesh. 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. Figure 1 illustrates the wide geographic spread of cash distribution along the Jamuna River.[4]

The timing of the anticipatory cash transfers was determined by a pre-defined 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 in Figure 1). As illustrated by Figure 2, the forecasts predict actual water flow with a high degree of accuracy. The first "readiness" trigger was activated on 4 July 2020 and set in motion

[2]We use the 2020 PPP conversion factor for private consumption from the World Bank's World Development Indicators database.

[3]For comparison, the four major social protection programmes that target the most socially excluded individuals, rather than households, provided monthly cash transfers ranging from 500 to 750 BDT in 2020.

[4]During the pilot, the Food and Agriculture Organization (FAO) and United Nations Population Fund (UNFPA) delivered their own anticipatory action interventions, also funded by CERF. FAO released watertight storage containers and animal feed to 7,000 and 11,760 households respectively. UNFPA delivered hygiene, dignity, and health kits to 15,000 women, girls, and transgender people. There was little geographic overlap between the cash and non-cash interventions, as the UN agencies mostly operated in different districts. This paper focuses on the cash transfers made by WFP only; we account for the limited overlap in Section 6.

preparatory activities, including beneficiary verification calls, which will be used to define our sample.[5] The second "activation" trigger was activated on 11 July and initiated the release of cash transfers.[6] 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 ex-post, with households receiving cash an average of seven days prior to their local flood peak. WFP sent cash to households on 14, 15, 16, 18 and 30 July via bKash digital wallets, thus just prior to and immediately after the global flood peak. 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. In the same year as the pilot, the traditional humanitarian response was initiated on 4 August after the completion of a needs assessment - a full month after the first anticipatory action trigger.

3 Sample and data

3.1 Evaluation strategy

This evaluation is not an experimental study, as this was not possible during this rapid-onset humanitarian crisis. Randomly assigning households to treatment and control groups, especially in the form of denying support during a serious crisis to some vulnerable people when treating all may be possible, is seen as conflicting with humanitarian principles to which humanitarian relief organisations subscribe.[7] However, the way in which the anticipatory cash transfers were targeted and rolled out in practice within organisational constraints provided an opportunity for a rigorous evaluation of their impact on household welfare.

[5]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 was predicted to be more than 50% likely to cross the one-in-five year return period threshold (100,000 m^3/s) over a period of three consecutive days at the Bahadurabad gauging station, with a 10-day lead time. This threshold was set to be 0.85 metres above the government danger level of 19.5 metres. The GloFAS is a global hydrological forecast and monitoring system that couples weather forecast with a hydrological model calibrated for the Jamuna River in Bangladesh. The FFWC is a government agency responsible for flood forecasts.

[6]The second activation trigger was reached once the water level forecasted by the FFWC's five-day lead time model crossed the government-defined danger level by an additional 0.85 metres at the Bahadurabad gauging station.

[7]The first common principle is that of humanity, stating that "human suffering must be addressed wherever it is found, with particular attention to the most vulnerable" (Council of Europe, 2008), backed by the 1991 United Nations General Assembly Resolution 46/182. Intentionally withholding support during a humanitarian crisis is thus ethically questionable. Nonetheless, there are grounds for varying modalities in which support is received, such as timing, which are promising avenues for future research. Developing ethical protocols consistent with humanitarian principles was not possible in this particular case, given the scale and unexpectedly early onset of the disaster that was beginning to unfold.

The activation of the first "readiness" trigger on 4 July 2020 indicated that an extreme flood event may occur in the near future, as described in the previous section. The trigger activation launched a set of preparatory actions to ensure the rapid delivery of the cash transfers. The Red Cross Red Crescent Climate Centre supported our implementation partner, WFP, to identify target geographic regions – unions – based on a joint assessment of their flood risk and overall socio-economic vulnerability. Next, WFP was required to use pre-existing lists to identify potential households within the chosen unions owing to restrictions on movement arising from the COVID-19 pandemic.[8] The households on these lists had received assistance through UN interventions or government safety nets in previous years and were deemed vulnerable. Again due to the COVID-19 pandemic and speed of delivery required, it was decided that the only reasonable way of providing support would be via digital means, using digital wallets. An agreement was reached with bKash, the largest digital wallet provider in the country and part of the BRAC group of social enterprises, to be on stand-by for delivering the cash transfers. Due to time constraints, no other companies were contracted.

We do not expect that delivering cash transfers via a digital wallet will have unduly restricted the target population of vulnerable households. By 2020, Bangladesh had more registered sim cards than its population of 167 million. Its central bank reported 92.6 million registered users across 16 digital wallets, with 42.7 million active accounts and 10 million daily transactions in July 2020 (Bangladesh Bank, 2023). bKash is the market leader, but there is little difference in functionality, cost or benefit across the digital wallets: bKash holds about 40% of all digital wallet accounts in Bangladesh, followed by Nagad (25%) and Rocket (18%) (Tahasin and Hoque, 2022). bKash is a subsidiary of BRAC Bank, a for-profit financial institution and not directly linked to BRAC itself, the not-for-profit organisation. Thus, bKash is seen as one of many wallet service providers and owning a bKash account does not give preferential access to other BRAC services.

These use figures also suggest that many people hold multiple sim cards and multiple digital wallets. These wallets are interoperable, allowing for seamless transfers between them. Households frequently own more than one account, often to take advantage of sales promotions by mobile phone providers or banks, or gain access to government services historically rolled out through multiple providers.[9]

[8]This deviates from their usual practice during a crisis, which typically entails identifying and verifying household needs on the ground. While this procedure was used due to time constraints and COVID-19 restrictions on movement, it also accelerated the delivery of the cash transfers.

[9]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.

Not all accounts are always functional. Opening a new digital wallet is not arduous: a sim card and proof of identification is all that is required. However, for security reasons, many accounts will become inactive, such as after changing a sim card, potentially suspicious transactions, using a wrong PIN three times, or not using the account for 6 months. At the time of writing, re-activating an account required contacting a call centre or an agent of the particular digital wallet and following simple instructions, including submitting a proof of identity.

The following next steps in targeting households enabled the design of our evaluation strategy. To verify their eligibility, WFP contacted as many households on these pre-existing lists as they could reach via the phone prior to the flood peak. This process intensified when the second trigger activated on 11 July 2020, which committed WFP to releasing the cash transfers. In total, approximately 38,000 households out of 40,000 were successfully contacted. As expected from the widespread access to digital wallets in Bangladesh by 2020, the vast majority of those successfully contacted appeared to have had one or more digital wallets: while WFP did not record exact numbers, our follow-up survey, drawn from these about 38,000 households, documented that 97% of our sample had access to a digital wallet.

As only bKash was contracted to deliver the transfer, beyond basic verification of identity and location, households were asked whether they, a trusted relative or friend had a bKash account and to report their account number. Only those households whose bKash account numbers were found to be correct and active could be retained for the intervention. As there is no obvious systematic reason, such as access, use or cost, why they may have had a functional bKash account instead of any other account, this offered the opportunity for our evaluation within this population with widespread access to digital wallets: verified households were part of the treatment group if they were on the pre-defined lists living in affected unions and had access to an active bKash digital wallet at the time of the verification call; verified households on the same lists with the incorrect digital wallet account or with blocked or dormant bKash accounts were excluded from receiving the cash transfer and form our control group. As they were called and reached during verification, the latter had access to a functioning mobile phone account. Virtually all in the control group also had digital wallets, inactive or of the wrong brand: during our follow-up survey of a sample of this group showed that 94% reported to have a digital wallet account.

Treatment assignment was independent of their experience of the floods. During verification, WFP also did not reveal that a flood was incoming or cash transfers were to be provided, let alone that a valid bKash accounts was required. Early warnings had not been developed in time for the pilot, as the triggers were activated much earlier than anticipated. More than half of treated households received the cash transfers on 14 July 2020. The remaining half were sent the cash transfer on 15, 16, 18 or 30 July due to the staggered roll out. As this means that this last group may have had the time to reactivate a dormant bKash account, we will show the robustness of our results to controlling for the transfer date. For example, during verification, some respondents could offer valid bKash account numbers that were inactive. WFP effectively allowed for a few days for respondents to re-activate such accounts as final checks were not done immediately. As a result, there may have been some behavioural response to the intervention, affecting our design. For example, more agile households moved from the control to treatment group. Its impact is not likely to have been substantial, as households were never told that they would receive a cash transfer the verification call. Nevertheless, we test the robustness of our results to an alternative treatment of this group in Section 6.

From this population of treated and control households as specified in WFP records, we randomly sampled treated and control households within the same unions where there were at least 10 households in each group. For treated households, we randomly sampled 60 or all households (whichever was smaller) from each union for those who were sent an anticipatory cash transfer from 14 to 16 July, and all households receiving cash on 30 July.[10] For the control households, we similarly sampled all or a random sub-sample of households that were recorded as excluded by WFP in equal proportions across unions.

Of 23,434 households who were sent the anticipatory cash transfer, we survey 9,130 households. 6,803 treated households and 2,235 control households across 111 unions meet our sampling criteria and form our sample, with a total of 9,038 households.[11] We oversample treated households to exploit variation in the dates that the cash transfers were delivered, thus allowing us to study the impact of the timing of the transfer relative to the local flood peaks. Appendix Table A1 summarises the number of households first targeted by the WFP intervention and then surveyed, by transfer date.

3.2 Household survey data collection

Due to COVID-19 restrictions and accessibility issues, we collected survey data over the phone. The surveys took place 10 to 12 weeks after the intervention between 21 September and 8 October 2020. Phone surveys were conducted in Bangla by enumerators hired and trained through a local survey firm called Data Analysis and Technical Assistance (DATA). We contacted the household member whose name was on the WFP records. The phone

[10]A relatively small number of households received cash on 30 July, so we sampled the entire group to exploit variation in timing. Moreover, a small number of households were sent cash on 18 July and we used this group to pretest the questionnaire. They do not appear in our sample.

[11]Control households were slightly less likely to respond to the survey, resulting in a differential non-response of 4.3%, conditional on being reached by phone. We also initially collected data from some households that were not reached during the verification process, but we exclude these 86 households from our final sample, as we believe they may be systematically different from those reached by phone.

surveys lasted 30 minutes on average. Respondents were asked a series of questions, including on demographics, behavioural 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, flood experience and balance

Our data reflects that our sample of interest is highly vulnerable and badly affected by the 2020 floods. Appendix Table A2 provides the mean value of individual and household characteristics that are likely to be time invariant across treatment and control groups. The table also reports the difference in means with a pairwise t-tests based on our main regression specification where we control for union fixed effects and village-level land type. These measures were collected during the follow-up survey and pre-specified in our pre-analysis plan. 97% of our 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. Appendix Table A3 presents descriptive statistics on technology use and anticipatory action measures, since the intervention and the definition of our control group was based on access to a digital wallet. Around half our sample had used a digital wallet in the last six months, excluding the transfer from WFP. Approximately 80% own a mobile phone and almost all respondents had used a mobile in the last week at the time of the survey.

Our population of interest was heavily affected by the floods. 87% of control households reported flooding at or above floor level, with 5% above roof level. Using satellite data to measure flood intensity at the mauza level (approximately village level), we estimate that a third of the land area was submerged at peak flooding, on average, in mauzas where our sample live. Moreover, the flood extent remained above 50% 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 our sample was 22 July. The date of peak flood extent varied across mauzas from mid-July until the beginning of August. For all three satellite measures – flood extent, duration and peak date – we observe similar distributions between treatment and control groups.

The floods seriously affected lives and livelihoods. Only 60% of the control group reported receiving early warning and 53% 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. 61% of control households reported loss of poultry, another important productive asset. 46% 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 our sample.

Although we do not have baseline data, Appendix Table A2 shows that the treatment and control groups are balanced on most socio-economic characteristics measured in the follow-up survey that are unlikely to have changed in response to the treatment. Where small differences exist in socio-economic characteristics, it appears that treated households are slightly more vulnerable, with the treatment group more likely to live in more fragile housing compared to the control group. Respondents in the treatment group are also marginally more likely to have completed primary school. These differences are small, however. To improve power in our analysis, we will control for all the individual and household characteristics listed in the table.

We do not detect any observable differences in access and use of mobile phones and digital wallets across treated and control households, providing further support to our identification strategy. If the concern is that those with an active bKash accounts are better off as they are more involved in the cash economy, then these observable characteristics offer no evidence for this (or any other) direction of bias.

92.4% of treated households reported receiving a cash transfer from WFP during our survey, which could reflect compliance issues or survey demand effects. Thus, we estimate and report intent-to-treat effects in our results. There was limited geographic overlap between the cash transfers and other UN interventions received by a small subset of our sample. We observe that a higher share of treated households received dignity kits from UNFPA: 14% of treated households compared to 7% of control households. We will show that our results are robust to controlling for these other interventions.

3.4 Other data used in our analysis

3.4.1 Second round of phone surveys

Further data allow us to test the robustness of our results. 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 covered in the first round of data collection, 81 unions that were not targeted by these post-flood cash transfers were resurveyed.[12] 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 our 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, allowing us to make statements on impact, even though we must be careful in comparing them to the evaluation results from the first phone survey as the underlying population is likely to have been less affected by the floods on average.

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.[13] A total of 1537 surveys were collected within the 81 unions. We treat each survey round as containing random samples of treated and control households from their respective populations. These surveys provide a further opportunity, albeit with limitations, to test the robustness and persistence of our results.

Using the second round of surveys, Appendix Table A4 shows that treatment and control group households within the aforementioned 81 unions are balanced on most socio-economic characteristics and their use of mobile phones and digital wallets. Moreover, the follow-up sample is similar to households contacted during the first survey, as described in Appendix Table A2. In this second survey, we also confirm that other sources of cash assistance were very limited during the flood. Only 5% of treated households and 6% of the control group reported receiving cash from a source other than WFP since the flooding began, mostly from NGOs or government organisations.

3.4.2 Satellite data

We complement our survey data 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-metre spatial resolution. Sentinel-1 SAR data identifies water bodies indicated by darker pixels, even in the presence of cloud cover, and has been frequently applied to flood mapping, including in Bangladesh (Uddin *et al.*, 2019; Singha *et al.*, 2020). We use a change detection and

[12] These surveys were originally designed to compare the impact of anticipatory cash transfers to post-flood cash transfers, but the study had to be abandoned once it became evident that WFP had used a different targeting regime for households receiving post-flood cash transfers within these 30 unions.

[13] 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.

thresholding approach 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, we repeated the change detection process 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, we estimate daily flood extent values by fitting the Sentinel-1 data points to a Gaussian function. We identify the date of peak flooding (or maximum extent) in each mauza from the modelled time series.[14] We validate these estimates against three independent data sources: (1) river discharge measurements from GloFAS at four measuring stations along the Jamuna River; (2) optical satellite imagery from Sentinel-2; and (3) 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 Online Appendix Section 1.

3.5 Outcome measures

In constructing our outcome measures, we follow a detailed pre-analysis plan, registered in the American Economic Association Registry.[15] In accordance with our first filing, we examined the follow-up data blinded to treatment assignment and filed a supplement to the pre-analysis plan. We collect all data via phone surveys and therefore our measures are limited by this format. Online Appendix Tables S4 and S5 provide more detail on the precise construction of each variable.

We consider three measures of household welfare: (1) child food consumption; (2) adult food consumption; and (3) wellbeing. 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 *et al.*,

[14]When using satellite data, we exclude nine mauzas where the Gaussian model fit of flood dynamics was poor; this accounts for only 0.5% of households.

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

2017; Asfaw *et al.*, 2017). A temporary reduction in child food consumption in response to short-term shocks is also likely to have long-run consequences (Dercon and Porter, 2014; Dinkelman, 2017). Child food consumption is captured by a dummy variable indicating whether children in the household have consumed at least three meals in the day prior to the survey. This relatively rudimentary measure reflects whether a basic level of food intake is being met and can be measured with a high degree of accuracy over the phone. We also report results on number of meals consumed by children in the day prior to the survey as a robustness check.

The adult food consumption index is a richer measure, 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). Several studies have shown high correlations between the FCS and calorie consumption (Weismann *et al.*, 2006; Coates *et al.*, 2007). We exclude rice from the calculation of this measure, as more than 95% of households reported eating rice every day in the past week.

We measure subjective wellbeing using a 10-scale Cantril's 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 wellbeing in the short- and long-term (Haushofer and Shapiro, 2016). The 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).

To uncover the channels and behaviours through which welfare improvements may have occurred, we pre-specified five additional variables: (1) pre-emptive actions; (2) asset loss; (3) costly borrowing; (4) remittances; and (5) earning potential.[16] Our focus on measuring pre-emptive actions is motivated by the existing evidence showing that actions taken before floods can limit asset loss and damage by up to 50% (Carsell *et al.*, 2004; Kreibich *et al.*, 2005; Thieken *et al.*, 2007). In the absence of a standard pre-emptive 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 our asset loss index by combining (i) the number of livestock that died in the two

[16] Although initially pre-specified as secondary outcomes, we treat these variables as primary in our paper, given their importance for the theory of change posited by our implementation partner, WFP. We correct for multiple hypothesis testing across all eight variables.

months following the flood peak; (ii) the number of asset categories that were lost or damaged due to the flood (out of a list of 15); and (iii) the area of cultivated crops lost in decimals (1 decimal $\approx$ 40 sq metres) 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 recognise 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 earning potential index is constructed by combining (i) a dummy variable for whether a household avoided losing crops from flooding or was able to replant, and (ii) the number of paid hours of work per adult in the week prior to the survey.

When comparing across measures, we standardise all variables following Kling et al. (2007) in constructing indices.[17]

4 Empirical strategy

4.1 The intent-to-treat effect of the anticipatory cash transfer

We estimate the intent-to-treat effects of the cash transfer on a variety of outcomes 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 a household i was identified to receive the anticipatory cash transfer. X_i is a vector of pre-specified controls to increase precision in our estimates. These include age, gender and education level of the respondent; household size; dependency ratio[18]; type of house structure; UNFPA/FAO recipient status; and land type, derived from satellite imagery at village

[17]We first ensure that all variables are consistently signed within a particular index, such that a higher value indicates a more positive outcome. 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. Additionally, if there are multiple sub-scales within an index, we also sum the standardised sub-scales, before re-standardising the final index using the control mean and standard deviation.

[18]The dependency ratio is defined as: number of children under 15 years old and adults over 60 years old as the numerator, divided by total household size less number of dependents as the denominator.

level as a proxy for flood exposure.[19] ε_i is a mean zero error term and δ_j is a geographic fixed effect for each union j.

4.2 Heterogeneity analysis by timing

Our research design allows us to quantify the impact of an anticipatory cash transfer relative to a control group that did not receive the transfer. A second question of interest is the relative impact of an anticipatory cash transfer compared to a post-flood cash transfer. As previous humanitarian responses to floods by CERF came only 100 days after the flood-peak (see Section 2), our quantification from the first survey three months after the peak is informative about the benefits from early transfers in the period up to business-as-usual post-flood responses. We cannot speak to this question directly within our research design. However, we can exploit variation in the timing of the cash transfer relative to local flood dynamics to suggest that speed of delivery matters.

Our second specification assesses whether an earlier anticipatory cash transfer relative to the flood shock matters for household outcomes. We exploit two sources of variation. First, 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 our sample. Second, even within mauzas, WFP sent cash to households on different dates in July 2020, owing to the short time window for implementation and the staggered roll out in verifying households as a result. Approximately 50% of our sample were sent the cash on 14 July, with the rest sent cash on 15, 16 and 30 July. By comparing the date of cash transfers to the local flood peak, we estimate the number of days that the cash transfer was delivered ahead of the local flood peak for each household.

We estimate 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, 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. β_1 captures the intent-to-treat effect of the cash transfer sent

[19] Variables for land type (char land, unprotected mainland or protected mainland) are defined at village or mauza level. We use spatial data to categorise each of the 639 mauzas in our sample by the predominant land type based on their location relative to the braided shape of the Jamuna River and about 800 km of existing flood embankments. We split the sample into three land types due to their unique geographic features, with increasing exposure to flooding: (1) protected mainland located inside flood embankments; (2) unprotected mainland located outside existing flood embankments; and (3) unprotected char land, which includes low-lying islands along the course of the Jamuna River.

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, we test the null hypothesis that an earlier cash transfer has no additional impact. As in our main specification, we control for union fixed effects.

We also use non-parametric methods to check that our timing results are not being driven by our choice of functional form. We estimate local linear regressions of the same form as our main specification to trace out the treatment effect across the flood timeline. The running variable is defined as the number of days between the cash transfer and the local flood peak date. We choose the bandwidth for these local regressions to optimise the Integrated Mean Squared Error (IMSE) following Calonico et al. (2018) and use an Epanechnikov kernel to weight observations.

4.3 Approach to inference

We estimate robust standard errors to correct for heteroskedasticity.[20] In addition to presenting p-values from the Wald test, we correct for multiple hypothesis testing across our 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 *et al.*, 2006 and implemented with code Anderson, 2008.

5 Results

5.1 The anticipatory cash transfer improves welfare

The first question of interest is whether a small, one-off cash transfer delivered just before an extreme weather event succeeds in improving welfare. We find that the anticipatory cash transfer of BDT 4,500 (USD $137 at 2020 PPP rates[21], equivalent to two weeks of food expenditure for an average household) significantly improves child food consumption and subjective well-being for treated households compared to control households – even when measured three months after the intervention. Figure 3 shows the intent-to-treat effects of the cash transfer on pre-specified, standardised outcomes with 95% confidence intervals using p-values. Table 1 reports the p-values and sharpened q-values for the standardised

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

[21]The 2020 PPP conversion factor for private consumption was drawn from the World Bank's World Development Indicators database.

treatment effects, as well as the percentage change relative to the control mean.[22] All results reported below remain statistically significant at conventional levels after correcting for the false discovery rate in multiple hypothesis testing.

Children in treated households are 3.8% more likely to have consumed three or more meals in the day prior to the survey, relative to a control mean of 80%. While recognising 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 (Maccini and Yang, 2009; Dercon and Porter, 2014; Dinkelman, 2017). 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 thus not directly comparable.

The positive effect on child food consumption 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, we ask households in a subset of unions during the second round of phone surveys 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%) of control households reported going a full day without eating. Compared to control households, treated households were 52% less likely to go a full day without eating during the flood (see Figure 6). These results are consistent with the self-reported use of the anticipatory cash transfer, with 91% of treated households reporting spending the cash on food or water, among other categories.

The cash transfer mitigated the flood's impact on subjective wellbeing: three months after the transfer, average life satisfaction for respondents in treated households was 0.178 standard deviations or 18.7% higher than in the control group. However, even with this increase, treated households continue to report an extremely low level of wellbeing, given the control group mean of 2 on a 10-item Cantril's ladder of life satisfaction.[23]

The anticipatory cash transfer also mitigated asset loss and boosted earning potential, an early sign of recovery. Extreme flooding is highly destructive to private assets and disruptive to income generating activities, especially for low-income farmers. The cash transfer

[22]The sharpened q-values are sometimes smaller in size than the unadjusted p-values. This is due to the fact that when there are low p-values for the majority of outcomes implying many true rejections, several false rejections can be tolerated while still maintaining a low false discovery rate (Anderson, 2008).

[23]For comparison, and according to the World Happiness Report of 2021, the average score for Bangladesh as a whole for 2020 was 5.2, with the United States at 7.0, the United Kingdom at 6.9 and the global highest, Finland, at 7.8 (Helliwell *et al.*, 2021).

decreased the asset loss index by 0.099 standard deviations relative to the control group, with a reduction in loss and damage for livestock, household assets and crops. Households receiving the cash transfer have a higher potential to recover faster, with treated households reporting a 0.083 standard deviation increase in the earning potential index relative to control households. Treated households were 7.8% more likely to report avoiding crop loss or being able to replant. Treated households were also 5% more likely to work for a wage at the time of the survey (see Table 2 on secondary outcomes), but do not report working significantly more hours on average. 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 the anticipatory cash transfer impacted the borrowing index or the likelihood of receiving remittances on average. Treated households are not borrowing significantly less or at a cheaper rate conditional on borrowing in the months during and after the floods, although treated households are slightly less likely to borrow at all on the extensive margin (see Table 2). There could have been offsetting effects in play, as the cash transfer may have made the treated households both more creditworthy so they could borrow more, but also less in need of credit to cope with the flood.

5.2 The anticipatory cash increased the choice set of pre-emptive actions

The one-off cash transfer amounted to approximately two weeks of household food expenditure, which is sizable, but delivered in response to floods that lasted longer than a month on average. Therefore, why do we observe significant, albeit small, effects on household welfare three months after the cash transfer and the dissipation of flood waters? 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 to show that the anticipatory cash transfers increased the choice set of actions available to households before the flood. Thus, we argue that households were able to alter the impacts of the floods at a critical juncture in time. Together with the improvements observed in the wide range of outcomes discussed above, these results highlight the importance of the timing of cash transfers and specifically, the effectiveness of receiving cash early in the trajectory of shocks.

Table 1 shows that treated households took 8.3% more actions in preparation for flooding before the flood peak, compared to control households. Table 2 below reports results on a range of secondary outcomes, including all types of actions from the pre-emptive action index adopted by more than 5% of the sample. In the absence of the cash transfer, only half (53%) 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 10% more likely to take any action to prepare. For instance, treated households were on average 12.5% more likely to evacuate household members, 24.3% more likely to evacuate livestock and 8% more likely to purchase food ahead of the flood peak, which is also consistent with the self-reported use of the anticipatory cash transfer.[24] Not only were treated households more likely to evacuate livestock, but they were also less likely to lose small livestock and poultry.[25] We find no differences in the loss of larger livestock (cows, calves and buffalo), which are owned by far fewer households. Our findings are consistent with other literature showing that evacuation in advance of flooding substantially reduces damages by enabling households to move possessions, agricultural equipment and seeds, evacuate poultry and farm animals, and partially harvest crops (Carsell *et al.*, 2004; Subbiah *et al.*, 2008).

Although we do not observe impacts on our borrowing index, we learn descriptively from the data that our sample had high liquidity needs during and in the months following the flood shock. 70% of control households borrowed money between the start of the flood and the survey, borrowing an average amount of BDT 8,595 – almost double the amount of the cash transfer, or USD $261 at 2020 PPP rates.[26] Treated households were 4.1% less likely to borrow, but were more likely to buy food before the flood (see Table 2). Thus, it is highly likely 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, even if WFP had not explicitly labelled the cash transfer. Early warnings have been deemed critical features in ensuring that affected populations adopt preventative actions before the onset of floods (Carsell *et al.*, 2004; Kreibich *et al.*, 2005; Thieken *et al.*, 2007). Long-range seasonal forecasts have been shown to alter agricultural production decisions prior to the onset of the monsoons in India (Rosenzweig and Udry, 2019; Burlig *et al.*, 2024). In the absence of a credible early warning system, the information component of the anticipatory cash transfer could be particularly powerful. Indeed, only 60% of control households reported receiving early warning ahead of the floods and treated households were 5% 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 small in magnitude. The size of these effects is unsurprising, considering

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

[25] Treated households own 17.2% more small livestock (goats, sheep and pigs) at the time of the survey relative to a control mean of 0.54 animals per household, controlling for covariates and union fixed effects.

[26] The WFP Monitoring and Evaluation report found that loans were largely obtained to meet food needs during the crisis period (WFP, 2020).

the one-off cash transfer amounted to approximately two weeks' worth of food expenditure during floods lasting more than a month. Nevertheless, it remains striking that we measure welfare benefits three months after the cash transfer, but before we observe the arrival of more traditional humanitarian cash transfers. Without cash, comparable households experienced lower food consumption, lower wellbeing, higher asset loss and a slower recovery in the period before a traditional response.[27] Some of these deficits, even when temporary, are known to have permanent scarring effects – for example, child undernutrition (Victora *et al.*, 2021) – suggesting that there are significant advantages from acting early. Moreover, we find that treated households take more actions before the flood peak. The timely cash transfer increased the choice set available to households at a critical juncture in time, thereby altering the negative welfare impacts of the flood. Taken together, these findings make a strong case that the timing of the cash transfer matters.

6 Robustness

Our results are robust to a range of alternative model specifications. Appendix Figure A2 illustrates standardised treatment effects with 95% confidence intervals using p-values across four alternative model specifications as compared to our main results reported in Table 1. Further details are provided in Appendix Table A5. Our results are robust to making stronger assumptions on the nature of the error term, specifically using more conservative village (mauza) fixed effects and clustering standard errors at the union level to account for any potential heterogeneity in treatment effects across unions (Abadie *et al.*, 2023). However, with the loss of statistical power in the latter specification, we can no longer reject that the effect on the pre-emptive actions index is significantly different from zero. Our results are also fully robust to winsorising outcomes at the 95th percentile, excluding covariates, and using Lasso-selected controls (see Appendix Table A6).

Furthermore, we find that the treatment effect on child food consumption remains highly significant when defining the outcome as the number of meals consumed (see Appendix Table A7). In the same table, we show that we do not find an effect of the anticipatory cash transfer on a wellbeing index as originally pre-specified, where we combine Cantril's life satisfaction

[27]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 our results cannot be ignored. For example, subjective wellbeing 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. These spillover effects would be common with standard post-flood humanitarian interventions too. Nevertheless, it remains the case that we observe protective and pre-emptive actions by recipients that would be unlikely to have negative externalities on non-beneficiaries, making it unlikely that the effects are primarily driven by negative spillovers.

with self-reported hours slept the previous night.[28]

The results are also not driven by other support that was offered by FAO and UNFPA to a small subset of our sample with limited geographic overlap at the same time as WFP (see Section 3.3 for more detail). While estimated treatment effects for those receiving this other support are somewhat different to the overall sample on some measures, Appendix Table A8 shows that accounting for this sub-sample does not affect any of our main conclusions.

We also find no evidence that our results are driven by behavioural change in response to the way the verification by WFP had to take place that could have contaminated the evaluation design. We account for the possibility that some households identified to receive cash transfers on later dates (15, 16 and 30 July) may have received cash transfers after being more agile in reactivating dormant bKash accounts. Some households with dormant or blocked bKash account could have re-activated them during the brief verification period before the release of cash transfers, making them eligible for the transfer. This was possible given the time needed by WFP for completing verification calls, checking bKash accounts and finalizing the beneficiaries' lists. As a result, some of the later transfers may have gone to more agile households responding to the calls by seeking to re-activate their accounts, and who should have been in the control group but now were in the treatment group. Unlike in the usual ex-post humanitarian operations, the operational delays in delivering pre-flood payments were largely due to the short time window in which to identify eligible households, rather than logistical challenges on the ground related to floods (e.g. infrastructure disruptions) or other household and union characteristics. Thus, we might expect that the impacts observed are driven by those receiving transfers later.

To explore whether this is indeed the case, we present results in Table 3 where we allow for differential treatment effects by transfer date, where the base group include households that were identified to receive transfers on the earliest date, 14 July. 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.[29]

[28] In our pre-analysis plan, we pre-specified a wellbeing index that combined Cantril's ladder of life satisfaction with self-reported hours slept the previous night, as a shorthand proxy for psychological distress. We re-evaluated this decision after a review of the economics and psychology literature revealed that sleep outcomes are extremely challenging to impact through one-off interventions in field settings and may be a poor indicator of psychological wellbeing, especially if captured through self-reported measures (Bessone *et al.*, 2021). Hence, given that it is highly unlikely that our cash treatment - a small indirect intervention - will impact self-reported hours of sleep, we exclude this variable from the index.

[29] In the pre-analysis plan, we pre-specified 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. Results using this model are reported in Online Appendix Table S2. Note that this model is identical to Table 3 but with imposed restrictions on the equality of coefficients for July 14-16 groups and estimated for each sub-sample separately.

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 the 16 and most clearly 30th of July relative to the 14 July group.[30] Those in the 30 July transfer group also do not retain a significant effect of the transfer on pre-emptive actions: getting a transfer late made pre-emptive actions less of an option, alluding to the importance of timing discussed in the next section. We conclude that our results are unlikely to be affected by behavioural change in response to the verification process, which could have affected the composition of treatment and control groups.

Using Lee Bounds (Lee, 2009), we find that our results on child food consumption and asset loss remain robust to correcting for a differential non-response of 4.3%, conditional on being reached by phone (Appendix Table A9). We lose statistical significance on our pre-emptive action index and life satisfaction measure for the lower bound estimates, but they remains positive, albeit of a smaller magnitude.

7 Heterogeneity analysis by timing

The second set of questions relate to the importance of the timing of transfers.[31] The most relevant one is whether an anticipatory cash transfer is more impactful compared to a cash transfer received after the floods - the usual humanitarian response. Our current design does not allow us to make this direct comparison. Nevertheless, United Nations' Central Emergency Response Fund (CERF), who funded this intervention, reported that the intervention evaluated here reached people with cash approximately 2 to 4 months faster than similar flood responses in Bangladesh in 2017 and 2019, and reportedly at half the operational cost (UN OCHA, 2023). As our first survey took place about 2-3 months after

[30] The difference in the borrowing index between the earliest (14 July) and latest (30 July) groups could be in part driven by the timing of the cash transfer relative to an important religious festival. The 30th July transfer was received the day before the start of Eid al-Adha when purchasing gifts and sacrificing livestock are a norm and liquidity needs are high, but maybe too late to arrange loans to pay for these, suggesting that the transfer had a positive impact on creditworthiness.

[31] In our pre-analysis plan, we also pre-specified that we would explore heterogeneity by land type. We proxied vulnerability to flooding by land type defined as three categories at the mauza (village) level using spatial data that are increasingly prone to flooding: (i) protected or embanked mainland; (ii) unprotected mainland located outside the embankment; and (iii) char lands, which include low-lying islands along the course of the Jamuna River. As land type is easily observed, we hoped that it would serve as a useful tool for targeting the intervention in the future. We find no clear systematic differences across the groups (see Online Appendix Table S1). However, it is not evident that land type is indeed the best proxy of vulnerability to flooding, which may be better captured by structural features and vulnerability of livelihoods to flooding.

the floods, this would have been around the time usual humanitarian responses would arrive. Therefore, the control group provides an indication how badly off likely recipients would have been, suggesting that any 'post-flood' transfer group would have experienced three months of significantly worse living conditions compared to those benefiting from an anticipatory action intervention.[32]

We can nevertheless offer more direct evidence about the importance of the timing of transfers within the intervention studied. We exploit variation in the delivery date of cash transfers and local flood dynamics to explore further whether small changes in the timing of the cash relative to flooding matter for household welfare. The Jamuna River Basin is a broad alluvial floodplain with a relatively flat topography the river spreading widely. Thus, floodwaters are relatively slow moving as they progress downstream, rising and receding slowly over a period of days or even weeks. At the same time, due to its high sediment load, the river is dynamic and morphs quickly over time. Thus, there is variation in the timing of floods both across and within unions, which is not entirely predictable at a granular level.

We calculate the number of days between receiving a transfer and the local flood peak in 630 mauzas (approximately village level) based on high resolution satellite data. Figure 4 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. We overlay the average flood extent corresponding to this timing. Most households were sent the cash before their local flood peak – on average, seven days prior. However, dangerous flood levels persisted for several weeks (Figure 1). Thus, the flood peak date should not be considered a discrete event, but rather a useful reference for measuring the timing of the cash transfer relative to flood dynamics over a large geographic region.

We then estimate the effect of receiving cash one day earlier using linear regression and non-parametric methods. We find evidence to suggest the importance of speed in implementation: an earlier cash transfer is welfare improving. Table 4 reports the average effect of receiving cash on the date of the local flood peak and the average marginal effect of receiving the cash a day earlier. First, we observe that the average treatment effects when cash was sent on the date of the local flood peak are consistent with our main results in Section 5. Second, although we do not observe an average effect on adult food consumption overall, receiving cash a day earlier has a stronger effect on the adult food consumption index (measured three months later) by 0.005 standard deviation (p-value=0.029; q-value =0.111). Treated households receiving an earlier cash transfer also report a higher borrowing index of

[32]Of course, we cannot reach any conclusions about what would subsequently happen then for those beneficiaries receiving a post-flood transfer using the usual time frames, relative to those receiving the cash transfer earlier. For example, given their additional liquidity means, they may then be able to recover faster than those receiving the pre-flood transfer. Future research ought to help clarify this.

0.006 standard deviation (p-value=0.006; q-value=0.048).[33] Note that all these results are robust to including controls for survey date and flood intensity (see Online Appendix Table S3).

We check that our main results on timing hold irrespective of our choice of functional form using non-parametric methods. Specifically, we estimate the effect of the cash transfer across the flood timeline using local linear regressions. The results show further evidence that a faster response relative to the evolution of the flood is welfare improving. As illustrated by Appendix Figure A3, the non-parametric analysis suggests that the anticipatory cash transfer is most effective at improving child and adult food consumption when sent at least seven days prior to the local flood peak. Welfare effects tend to dissipate for each day that transfers are delayed in the period before peak flooding, although this relationship appears nonlinear in the case of adult food consumption. A transfer received closer to the local flood peak also resulted in a greater number of pre-emptive actions, but we observe no variation in effects by speed of the cash transfer for other outcomes using non-parametric methods. Results are robust to restricting the sample to transfers made only on 14 July, such that satellite derived flood dynamics are the only source of variation in timing (Appendix Figure A4). We now also observe an earlier transfer is associated with higher subjective well-being with this restriction. Overall, our results on timing are indicative that a faster anticipatory cash transfer improves welfare, but properly understanding these dynamics merits careful investigation in future research.

8 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 Section 3.4 on data, 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 outlined in Section 4.

Table 5 illustrates the standardised treatment effects on our three welfare outcomes for the two rounds of data. For comparability, we present results from the first survey using only 81 unions (out of 111) covered in both rounds of data collection. Using this smaller

[33] When we unpack the borrowing index, we find that treated households borrowed approximately BDT 695 less (p=0.035) – USD $21 at 2020 PPP rates – than the control groups on the date of the flood peak, but households that were sent an earlier cash transfer borrowed slightly more relative to households receiving a later cash transfer (approximately BDT 72 or USD $2 at 2020 PPP rates for each day earlier relative to the local flood peak; p=0.003). This result suggests that receiving an early cash transfer may have boosted credit worthiness helping them to overcome credit constraints during a time when liquidity needs were high, consistent with effects in Table 3 and their discussion.

set of unions, we find that 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 are thus not dissimilar to the full sample of unions considered in our earlier analysis.

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 in-take, as proxied by the adult food consumption score measured in the week prior to the survey.[34] In contrast, we find no effects on child food consumption and life satisfaction five months after the cash transfer, unlike in our earlier survey. As shown in Figure 5, the control households experience significant improvements in their child food consumption and life satisfaction between the two survey rounds, thus converging with treated households. For instance, 86% of control households reported that children had consumed at least three meals in the day prior to the survey five months after the intervention compared to only 80% three months after the intervention. Cutting back on children food intake is one of the most extreme coping strategies, given the long-run consequences for child development. It is thus more likely to bounce back in the medium term, especially as measured by our relatively crude indicator. The decline in adult food consumption highlights the persistent negative effects of the floods on a less extreme coping strategy. However, as noted before, this measure is not directly comparable to the child food consumption measure. We do not observe significant treatment effects on any other primary outcome five months after the intervention (see Appendix Table A10).[35]

We use the second survey to show that the anticipatory cash transfer has a strong effect in reducing food insecurity during the floods, as noted in Section 5.1. Relative to 29% of control households, treated households were 52% less likely to go a full day without eating during the flood (see Figure 6). 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.

Taken together, it seems that, after cutbacks in meals eaten during the crisis, control households managed to increase child consumption back to comparable levels as the 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

[34]The Food Consumption Score for adults in treated households increased by 12.3% (p=0.002) relative to a control mean of 38.5. The number of days in which protein products were consumed by treated households increased by 15.3% (p=0.018) relative to a control mean of 3.

[35]Given that the second survey was conducted five months after the floods, we measured two primary outcomes differently to better capture post-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 – after the flood peak. The earnings potential index was reduced to number of hours worked in the last seven days and no longer included the ability to replant.

food consumption.

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 examines the impact of a one-off anticipatory cash transfer delivered in advance of extreme floods in Bangladesh, approximately five days before the flood peak. We find that the cash transfers increased the choice set of actions available to households, which altered the trajectory of the flood impacts. Cash-receiving households were able to evacuate household members and livestock and stock up on food, with an immediate and large impact on food consumption during the floods.

The effects of the anticipatory cash transfer were still present three months later, impacting both current and future welfare. Children in households that were identified to receive the cash transfers were 3.8% more likely to have consumed three meals in the day prior to the survey. Stated wellbeing was 18.7% higher. The anticipatory cash transfer also mitigated asset loss and boosted earning potential – an early sign of recovery. 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.

Our results suggest three important takeaways for crisis response. First, early action in a crisis matters. Failing to act early has real welfare costs. The welfare impacts measured in this paper occurred before humanitarian assistance would have typically arrived, although we do not negate the value of post-shock support for recovery. Our results also show that anticipatory cash transfers can alleviate losses that are known to have scarring effects, such as on child food consumption. Second, even when acting early, speed of delivery matters. Households that were identified to receive the cash transfer one day earlier relative to the local flood peak experienced a small improvement in adult food consumption three months later. This highlights the importance of upfront investments in preparing and targeting well in advance of the predicted extreme weather event, such that the intervention can scale immediately with the activation of triggers. Aiken et al. (2022) showcase a data-driven targeting method to rapidly enrol households, which could serve well in this context. Third, this unusually large-scale evaluation of a humanitarian intervention presents lessons for measuring the impact of other humanitarian programmes, in a context where there are real challenges to conducting rigorous evaluations and the evidence base is thus slim.

Much more learning on crisis response needed, but there are real challenges to conducting rigorous impact evaluations in these settings. The absence of baseline data requires large ex-post surveys. The impacts of one-off interventions are immediate, which in turn necessitates speed in data collection. This may mean using data collected via phone surveys. However, perhaps most significant is the challenge of identifying a valid control group. This requires a stronger commitment from humanitarian partners to learning and to using rules-based processes to target beneficiaries. When these rules are set out clearly in advance of a crisis and adhered to during implementation, it is possible to conduct rigorous evaluations of impact without withholding time-sensitive support to vulnerable households.

More work is needed to test the relative value of anticipatory versus post-shock cash transfers. In a context where credible early warning systems are often absent, the relative importance of liquidity versus information in anticipatory cash transfers need 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.

10 Figures

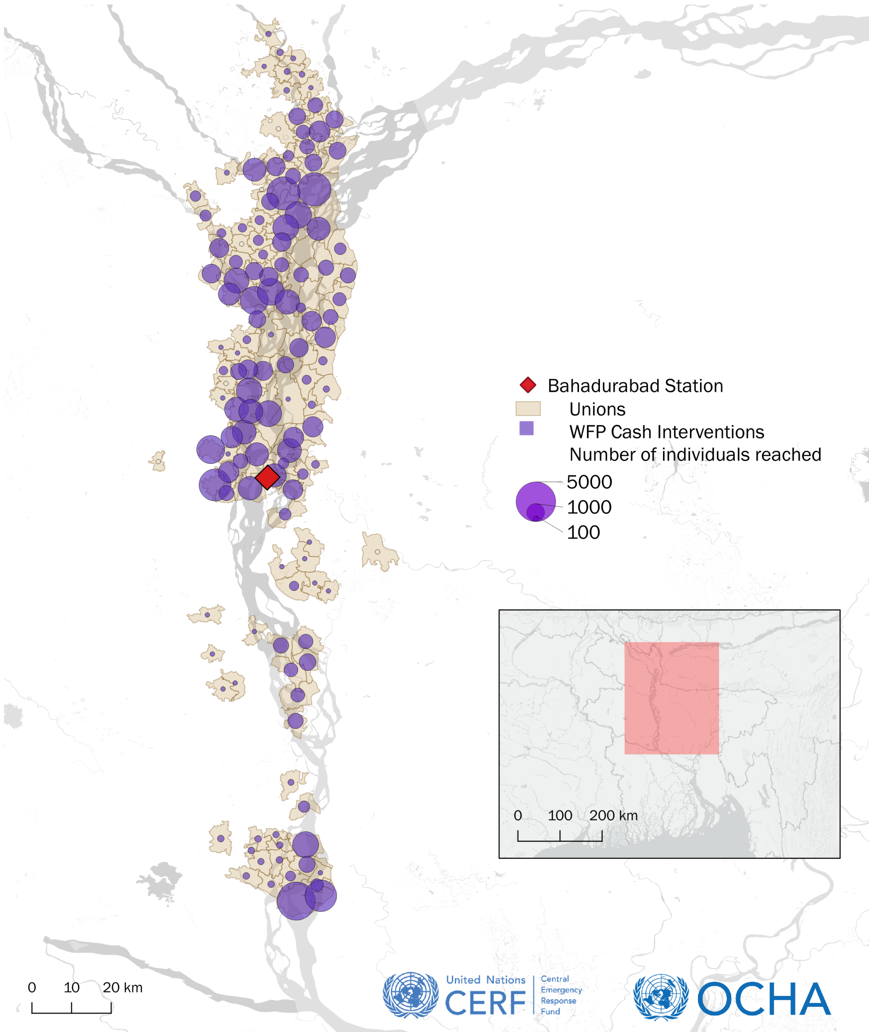


Figure 1: Map of the WFP cash intervention along the Jamuna River

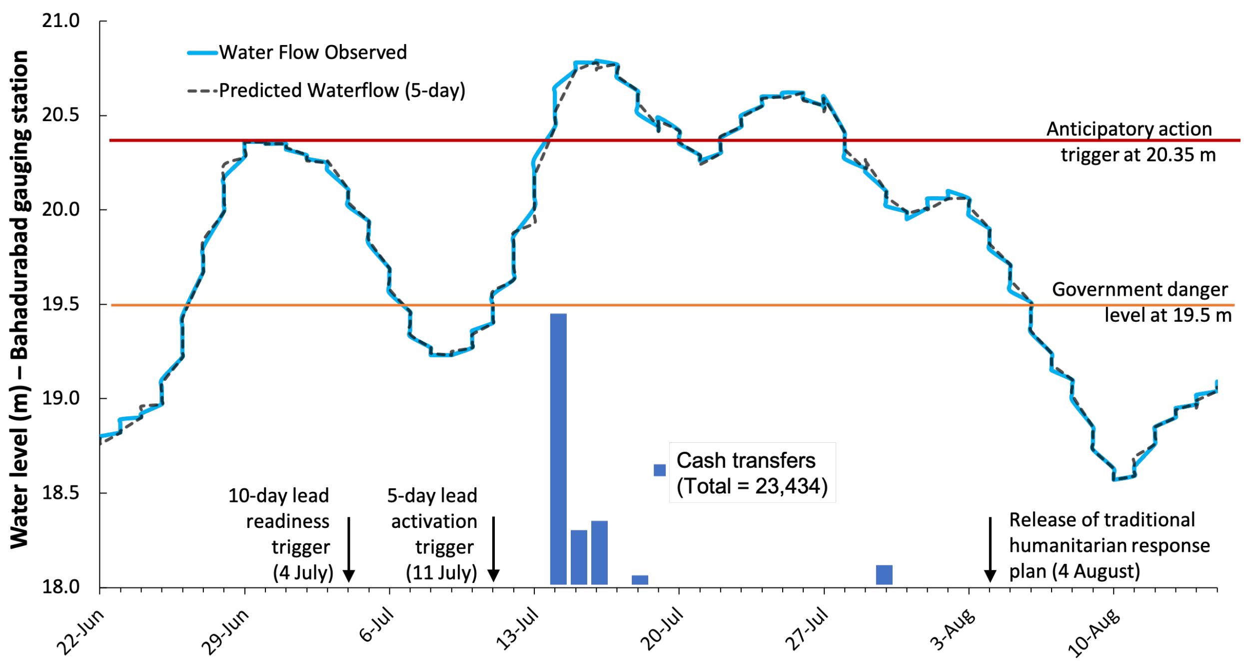


Figure 2: Timeline of triggers and cash transfers relative to water flow

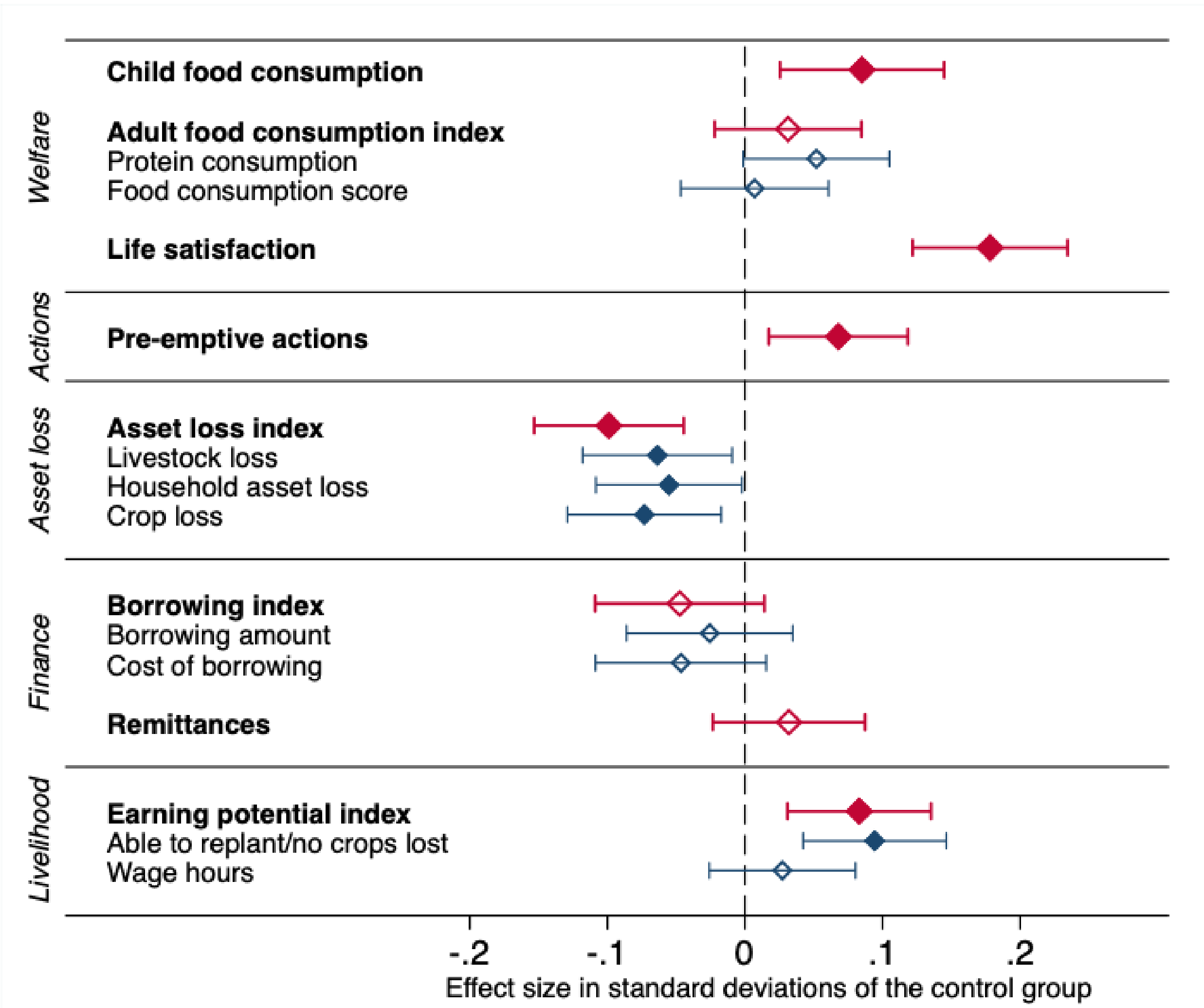


Figure 3: Intent-to-treat effect of receiving an anticipatory cash transfer

Notes: Markers indicate the standardised mean treatment effect of receiving the anticipatory cash transfer on pre-specified outcomes (red) and sub-indices (blue), with 95% confidence intervals shown. Solid markers indicate statistical significance at the 5% level. Non-solid markers indicate that we fail to reject the hypothesis that the effects are significantly different from zero at the 5% level of significance. Covariates include age, gender and education level of the respondent; household size; dependency ratio; type of house structure; UNFPA/FAO recipient status; and land type. Union fixed effects are included.

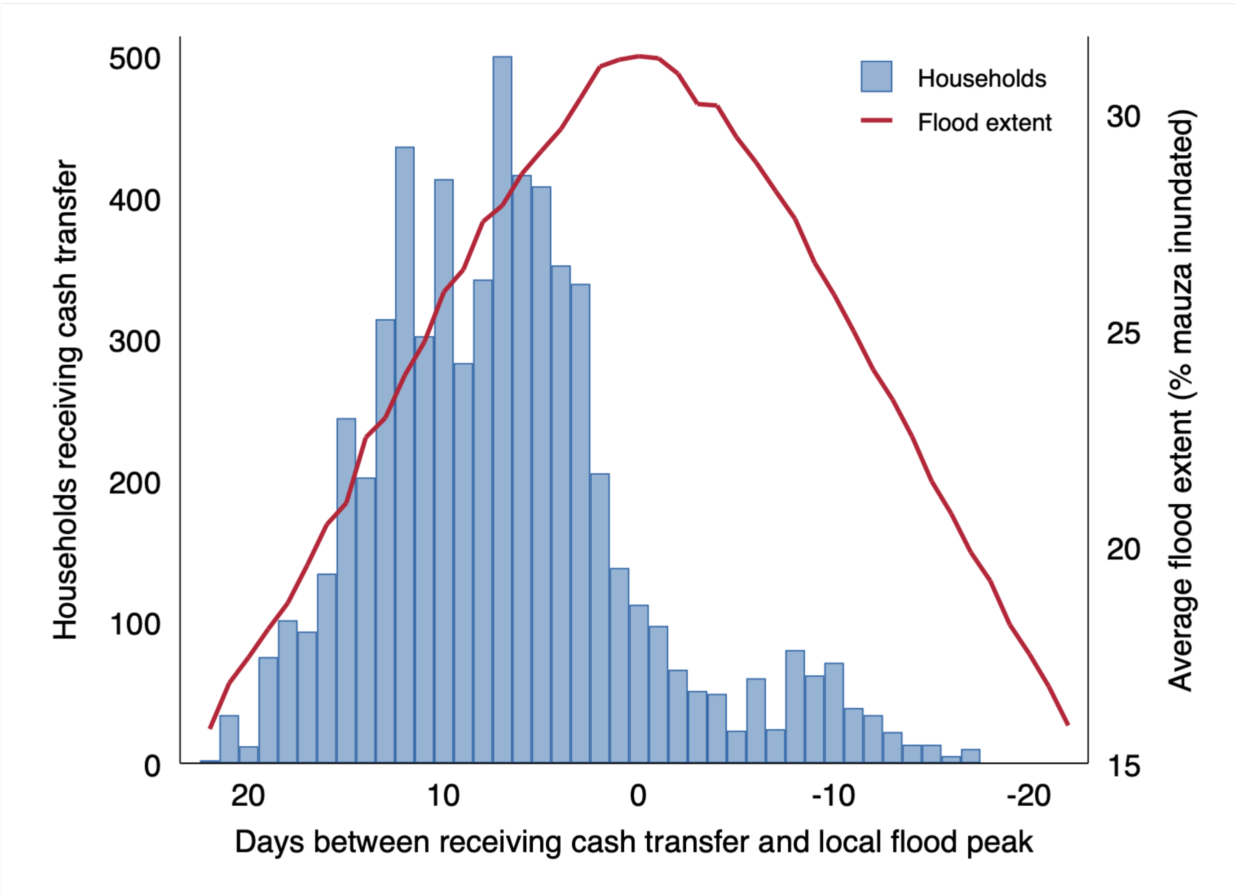
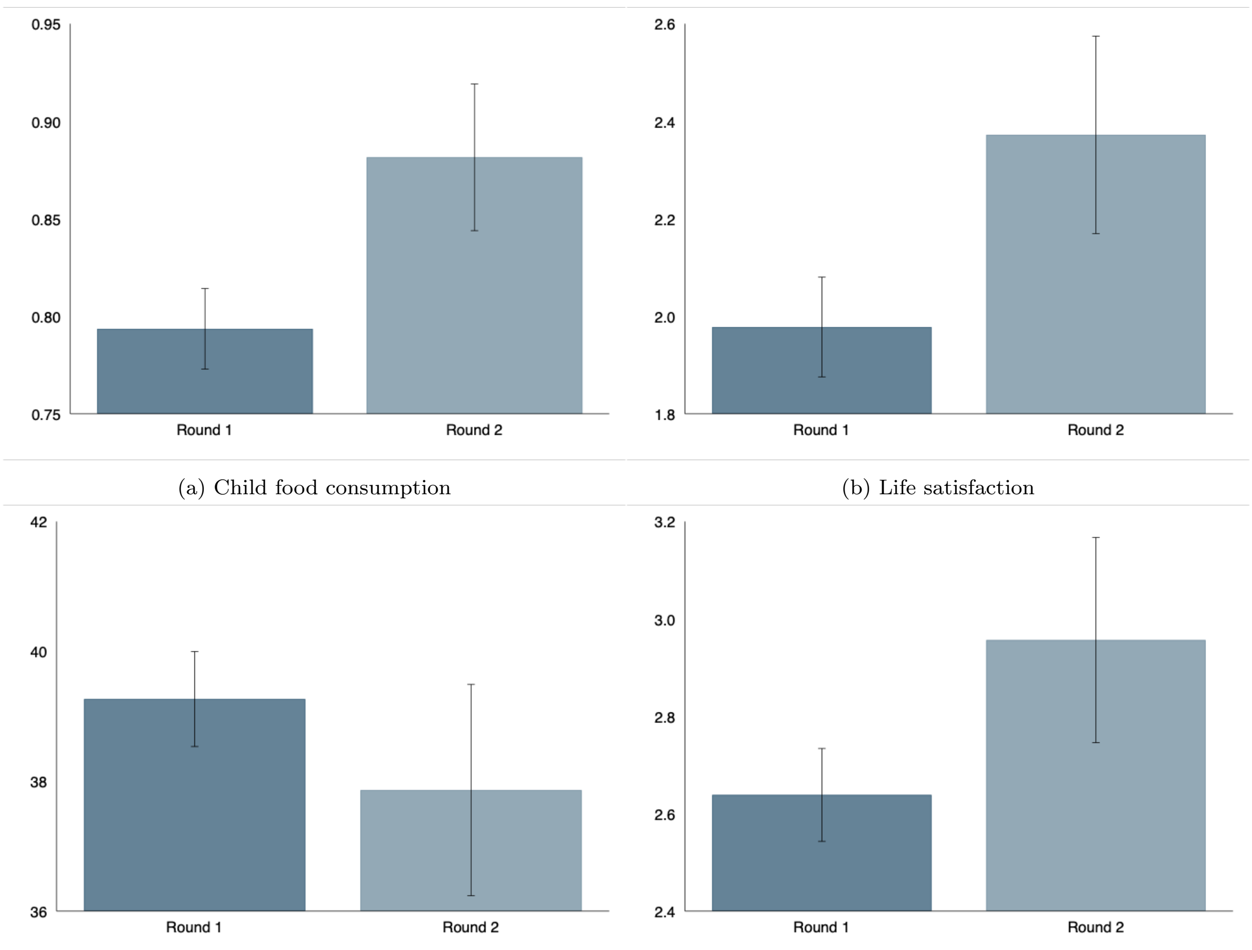


Figure 4: Number of households receiving cash relative to the local flood peak date

Notes: The number of households receiving the cash transfer 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% and 99% level, which reduces the size of the treatment group with a defined transfer date from 6,285 to 6,176 households.



(a) Child food consumption

(b) Life satisfaction

(c) Food Consumption Score (Adult)

(d) Days in which meat is consumed (Adult)

Figure 5: Welfare outcomes for the control group after three months (Round 1) and five months (Round 2)

Notes: We plot the predicted probability for each outcome using only the control group located within the 81 unions covered by both rounds of data collection.

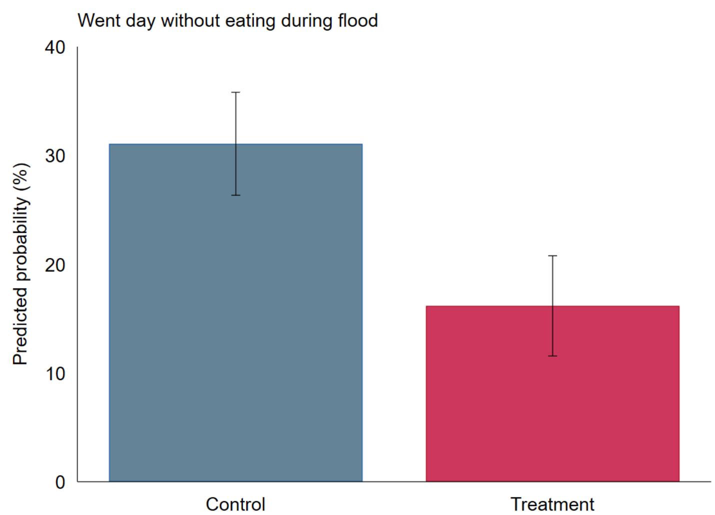


Figure 6: Probability of going a day without eating during the flood

Notes: 95% confidence intervals are shown.

11 Tables

Table 1: 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)	0.085***	0.80	0.03	+3.8%	0.005	0.007	7631
Adult food consumption index	0.031				0.249	0.108	9034
Days protein products consumed (0–7)	0.052*	2.66	0.11	+4.1%	0.055		9034
Food consumption score (0–112)	0.007	39.53	0.11	+0.3%	0.799		9034
Life satisfaction (0–10)	0.178***	2.03	0.38	+18.7%	0.000	0.001	9023
Number of pre-emptive actions (0–6)	0.068***	0.96	0.08	+8.3%	0.008	0.009	9030
Asset loss index	-0.099***				0.000	0.002	9033
Number of livestock died in last two months	-0.064**	0.62	-0.07	-11.3%	0.022		9033
Asset categories lost/damaged (0–15)	-0.055**	1.31	-0.07	-5.3%	0.041		9033
Area of cultivated crops lost (decimals)	-0.073**	15.73	-2.46	-15.6%	0.010		9033
Borrowing index	-0.047				0.131	0.070	6104
Amount borrowed in last two months (BDT)	-0.026	8595.78	-258.72	-3.0%	0.406		6104
Highest interest rate (%/month)	-0.046	4.63	-0.26	-5.6%	0.142		6104
Received remittances (0/1)	0.032	0.08	0.01	+12.5%	0.258	0.108	9033
Earning potential index	0.083***				0.002	0.004	9027
No crops lost/able to replant (0/1)	0.094***	0.64	0.05	+7.8%	0.000		9027
Paid hours of work/adult last week	0.027	9.25	0.30	+3.2%	0.316		9027

Notes: The first column shows the standardised mean treatment effect for pre-specified outcomes (in bold) and sub-indices. The second column shows the control mean, followed by the non-standardised treatment effect (Δ) and percentage change in the treatment group relative to the control mean (% Δ). *p*-values are reported on all outcomes and sub-indices. False discovery rate *q*-values for eight hypotheses are calculated over the main outcomes following the sharpened two-stage procedure of Benjamini *et al.*, 2006. Covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. Union fixed effects are included.
* p<0.10, ** p<0.05, *** p<0.01

Table 2: 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)
ITT	0.053***	0.037***	0.041***	0.030**	-0.029**	-0.036***	-0.005	-0.028**	0.035***
	(0.014)	(0.012)	(0.010)	(0.013)	(0.012)	(0.013)	(0.007)	(0.013)	(0.013)
Control mean	0.53	0.30	0.17	0.38	0.31	0.61	0.07	0.70	0.68
% Δ	9.9%	12.5%	24.3%	7.9%	-9.1%	-5.9%	-8.4%	-4.1%	5.1%
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	9030	9030	9030	9030	9035	9033	9035	9029	9033
R^2	0.10	0.10	0.07	0.13	0.07	0.17	0.03	0.06	0.07

Notes: The first row shows the mean treatment effect for a range of secondary outcomes, controlling for the same covariates and union fixed effects as in the main results. The second row shows the control mean, followed by the percentage change in the treatment group relative to the control mean (% Δ). p-values are reported on all outcomes. We list all types of actions from the pre-emptive action index adopted by more than 5% of the sample. * p<0.10, ** p<0.05, *** p<0.01

Table 3: The differential 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
ITT	0.082**	0.044	0.141***	0.071**	-0.095***	-0.004	0.034	0.074**
	(0.033)	(0.030)	(0.031)	(0.028)	(0.030)	(0.035)	(0.031)	(0.029)
Transfer*15July	-0.024	-0.005	0.082**	0.034	0.052*	-0.063	-0.019	-0.015
	(0.037)	(0.034)	(0.035)	(0.030)	(0.031)	(0.039)	(0.036)	(0.032)
Transfer*16July	0.062	-0.014	0.082**	-0.005	-0.054	-0.072*	0.004	0.043
	(0.039)	(0.034)	(0.039)	(0.032)	(0.033)	(0.042)	(0.039)	(0.035)
Transfer*30July	-0.009	-0.075*	0.064	-0.075**	-0.050	-0.149***	0.012	0.047
	(0.043)	(0.039)	(0.042)	(0.035)	(0.036)	(0.042)	(0.045)	(0.037)
P-value for joint F-test	.731	.217	.005	.504	.47	.001	.98	.302
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11

Notes: The first row shows the standardised mean treatment effects for the pre-specified outcomes for households that were the 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 union fixed effects used in the main analysis. * p<0.10, ** p<0.05, *** p<0.01

Table 4: 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
ITT	0.074**	0.003	0.180***	0.060**	-0.113***	-0.082**	0.037	0.097***
	(0.034)	(0.030)	(0.032)	(0.028)	(0.030)	(0.034)	(0.033)	(0.029)
Transfer × days	0.002	0.005**	-0.001	0.001	0.002	0.006***	-0.001	-0.003
before flood peak (ITT)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
p-value: Transfer × days	0.507	0.029	0.816	0.426	0.335	0.006	0.748	0.205
q-value: Transfer × days	1.000	0.111	1.000	1.000	1.000	0.048	1.000	0.695
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7522	8915	8904	8911	8914	6023	8914	8908
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11

Notes: The first row reports the standardised 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. False discovery rate q-values are calculated over the marginal treatment effects for the eight outcomes following the sharpened two-stage procedure of Benjamini *et al.*, 2006. We control for the same household characteristics and union fixed effects as in the main analysis. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Standardised mean treatment effect on welfare outcomes three and five months after the anticipatory cash transfer

	(1) Days without food in flood	(2) Child food consumption	(3) Adult food consumption index	(4) Life satisfaction
Round 1 (10-12 weeks)				
ITT		0.083***	0.033	0.195***
		(0.032)	(0.028)	(0.030)
N		6730	7965	7955
R^2		0.04	0.09	0.10
Round 2 (20 weeks)				
ITT	-0.328***	-0.053	0.284***	0.031
	(0.095)	(0.103)	(0.097)	(0.094)
N	1537	1273	1537	1536
R^2	0.15	0.09	0.15	0.15
Controls	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓

Notes: The first panel reports the standardised mean treatment effect for welfare outcomes measured in the first survey three months after the cash transfer, but only for households located within the 81 unions covered by both rounds of data collection. The second panel reports the standardised mean treatment effects for welfare outcomes measured in the second survey five months after the cash transfer. Covariates include age, gender, education level, household size, dependency ratio, house structure and land type. Union fixed effects are included. * p<0.10, ** p<0.05, *** p<0.01

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Appendix

Table A1: Number of households, by transfer date and treatment status

	Number of households sent cash (by transfer date)	Number of households surveyed (by transfer date)
Treatment	**23,434**	**6,803**
14 July	14,345	3,605
15 July	2,903	1,293
16 July	3,384	1,143
30 July	1,036	762
Control	0	**2,235**

Notes: The second column shows the number of treated households targeted by WFP across the four transfer dates, according to their administrative records. The third column shows the number of households that were successfully surveyed, by treatment status and transfer date.

Table A2: Summary statistics for Round 1

Variable	(1) No transfer Mean/(SE)	(2) Transfer Mean/(SE)	(1)-(2) Pairwise t-test Mean difference
Age	38.320	38.520	-0.200
	(0.275)	(0.166)	
Female respondent	0.969	0.968	0.001
	(0.004)	(0.002)	
Household head	0.187	0.216	-0.029
	(0.008)	(0.005)	
Completed primary school	0.305	0.313	-0.008**
	(0.010)	(0.006)	
Household size	4.643	4.748	-0.105
	(0.035)	(0.021)	
Dependency ratio	0.734	0.759	-0.025
	(0.010)	(0.006)	
Raw material house	0.264	0.269	-0.006**
	(0.009)	(0.005)	
Number of observations	2235	6803	9038

Notes: The table reports mean values of individual and household characteristics measured in the follow-up survey that are likely to be time invariant. The last column reports the difference in means with stars indicating statistical significance from a pairwise t-test testing equivalence of means. The t-test is based on an ordinary least squares regressions of each variable on the treatment dummy, controlling for union fixed effects and land type at village level as in our main specification. * p<0.10, ** p<0.05, *** p<0.01

Table A3: Descriptive statistics for Round 1

Variable	(1) No transfer Mean/(SE)	(2) Transfer Mean/(SE)	(1)-(2) Pairwise t-test Mean difference
Technology use			
Used digital wallet in last six months	0.467	0.476	-0.009
	(0.011)	(0.006)	
Own mobile	0.829	0.797	0.032
	(0.008)	(0.005)	
Uses someone else's mobile	0.157	0.186	-0.029
	(0.008)	(0.005)	
Uses mobile at least once a week	0.971	0.963	0.008
	(0.004)	(0.002)	
Anticipatory action interventions			
Received WFP transfer (self-report)	0.122	0.924	-0.802***
	(0.007)	(0.003)	
Received dignity kit from UNFPA	0.068	0.137	-0.069***
	(0.005)	(0.004)	
Received feed or storage from FAO	0.048	0.071	-0.023
	(0.005)	(0.003)	
Number of observations	2235	6803	9038

Notes: The table reports descriptive statistics on technology use and self-reported receipt of anticipatory action interventions from WFP, UNFPA and FAO. The last column reports the difference in means with stars indicating statistical significance from a pairwise t-test testing equivalence of means. The t-test is based on an ordinary least squares regressions of each variable on the treatment dummy, controlling for union fixed effects and land type at village level as in our main specification. * p<0.10, ** p<0.05, *** p<0.01

Table A4: Summary statistics for Round 2

Variable	(1) No transfer Mean/(SE)	(2) Transfer Mean/(SE)	(1)-(2) Pairwise t-test Mean difference
Individual and household characteristics			
Age	37.237	37.027	0.210
	(0.413)	(0.462)	
Female respondent	0.977	0.989	-0.012
	(0.005)	(0.004)	
Household head	0.265	0.234	0.031*
	(0.016)	(0.015)	
Completed primary school	0.341	0.421	-0.080
	(0.017)	(0.018)	
Household size	4.508	4.575	-0.067
	(0.059)	(0.057)	
Dependency ratio	0.785	0.744	0.041
	(0.021)	(0.019)	
Raw material house	0.280	0.199	0.081
	(0.016)	(0.015)	
Household asset categories (pre-flood)	6.009	6.241	-0.232
	(0.071)	(0.071)	
Technology use			
Used digital wallet in last six months	0.597	0.540	0.057
	(0.017)	(0.018)	
Own mobile	0.864	0.874	-0.010
	(0.012)	(0.012)	
Uses someone else's mobile	0.127	0.122	0.005
	(0.012)	(0.012)	
Uses mobile at least once a week	0.982	0.991	-0.008
	(0.005)	(0.004)	
Number of observations	789	748	1537

Notes: The first panel of the table reports mean values of individual and household characteristics measured in the follow-up survey that are likely to be time invariant. The second panel reports descriptive statistics on technology use. The last column reports the difference in means with stars indicating statistical significance from a pairwise t-test testing equivalence of means. The t-test is based on an ordinary least squares regressions of each variable on the treatment dummy, controlling for union fixed effects and land type at village level as in our main specification. * p<0.10, ** p<0.05, *** p<0.01

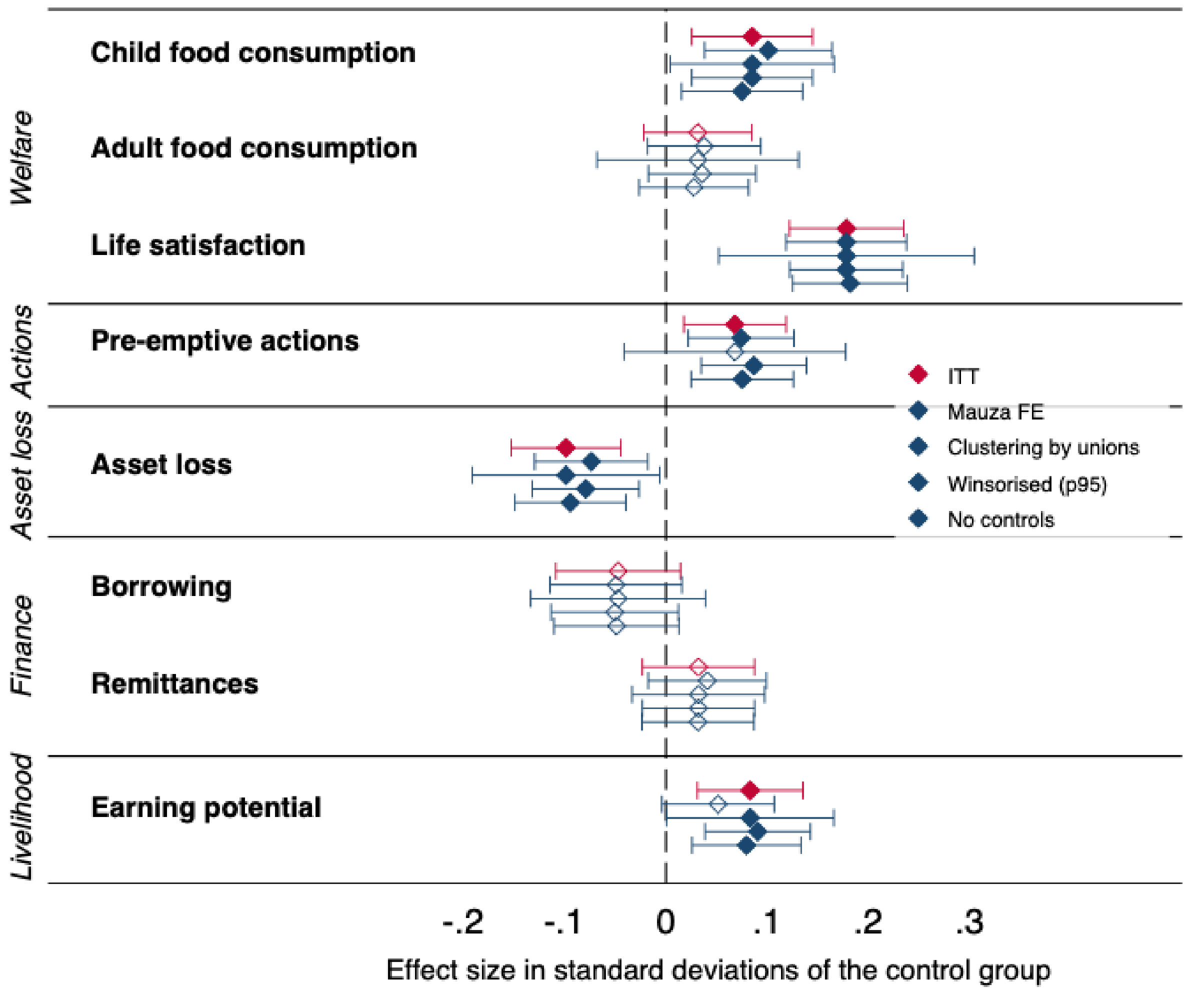


Figure A1: Robustness checks to alternative model specifications

Notes: Markers indicate the standardised mean treatment effect of receiving the cash transfer on pre-specified outcomes for the main specification (red) and alternative model specifications (blue), with 95% confidence intervals shown. Solid markers indicate statistical significance at the 5% level. Non-solid markers indicate that we fail to reject the hypothesis that the effects are significantly different from zero at the 5% level of significance. When used, covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. Union fixed effects are included, except when mauza fixed effects are indicated.

Table A5: Robustness to alternate model specifications

	(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
Main								
ITT	0.085***	0.031	0.178***	0.068***	-0.099***	-0.047	0.032	0.083***
	(0.030)	(0.027)	(0.029)	(0.026)	(0.028)	(0.031)	(0.028)	(0.027)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11
Mauza FE								
ITT	0.101***	0.038	0.178***	0.074***	-0.074***	-0.049	0.041	0.051*
	(0.032)	(0.029)	(0.030)	(0.027)	(0.028)	(0.033)	(0.030)	(0.028)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mauza fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7539	8946	8935	8942	8945	5995	8945	8939
R^2	0.10	0.15	0.17	0.18	0.20	0.17	0.10	0.17
Clustering by unions								
ITT	0.085**	0.031	0.178***	0.068	-0.099**	-0.047	0.032	0.083**
	(0.041)	(0.050)	(0.064)	(0.055)	(0.047)	(0.043)	(0.033)	(0.042)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11
Winsorised (p95)								
ITT	0.085***	0.036	0.178***	0.087***	-0.079***	-0.051	0.032	0.091***
	(0.030)	(0.027)	(0.029)	(0.026)	(0.027)	(0.032)	(0.028)	(0.027)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11
No controls								
ITT	0.075**	0.027	0.182***	0.075***	-0.095***	-0.049	0.032	0.079***
	(0.030)	(0.028)	(0.029)	(0.026)	(0.028)	(0.031)	(0.028)	(0.027)
Controls								
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7633	9036	9025	9032	9035	6105	9035	9029

Notes: The table reports standardised mean treatment effects for alternate model specifications, as shown in Figure A2. When used, covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. Union fixed effects are included, except when mauza fixed effects are indicated. * p<0.10, ** p<0.05, *** p<0.01

Table A6: Robustness to Lasso-selected controls

	(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
Main								
ITT	0.085***	0.031	0.178***	0.068***	-0.099***	-0.047	0.032	0.083***
	(0.030)	(0.027)	(0.029)	(0.026)	(0.028)	(0.031)	(0.028)	(0.027)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11
PDS-Lasso								
ITT	0.182***	0.072*	0.207***	0.098***	-0.082**	0.004	0.053	0.086**
	(0.043)	(0.037)	(0.039)	(0.035)	(0.036)	(0.042)	(0.038)	(0.037)
Lasso selected controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7379	8749	8738	8745	8748	5908	8748	8742
CHS-Lasso								
ITT	0.179***	0.084**	0.231***	0.102***	-0.089**	-0.004	0.052	0.091**
	(0.042)	(0.037)	(0.039)	(0.035)	(0.036)	(0.042)	(0.038)	(0.037)
Orthogonalized	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7379	8749	8738	8745	8748	5908	8748	8742

Notes: The first panel reports the standardised mean treatment effect for each outcome estimated using the main specification. The second panel reports estimates using the post-double-selection methodology (PDS) of Belloni *et al.*, 2014. The PDS method uses the lasso estimator to select the controls. The third panel reports estimates using the post-regularization (or double-orthogonalization) methodology (CHS) of Chernozhukov *et al.*, 2015. The CHS method uses lasso-selected variables to construct orthogonalized versions of the dependent variable. * p<0.10, ** p<0.05, *** p<0.01

Table A7: Estimated treatment effect for the pre-specified wellbeing index and the number of children meals

	(1) Wellbeing index	(2) Number of children meals
ITT	0.037	0.078**
	(0.029)	(0.032)
Controls	✓	✓
Union fixed effects	✓	✓
N	9023	7631
R^2	0.07	0.05

Notes: The table reports standardised mean treatment effects for child food consumption and wellbeing using alternative measurement. We pre-specified a wellbeing index in our pre-analysis plan that combined the Cantril's ladder of life satisfaction with self-reported hours slept the previous night, as a shorthand proxy for psychological distress. We also present results on child food consumption measured as the number of meals consumed by children in the household in the day prior to the survey, rather than a dummy variable indicating whether children in the household have consumed at least three meals (the original pre-specified measure). Covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. Union fixed effects are included.
* p<0.10, ** p<0.05, *** p<0.01

Table A8: The differential effects of an anticipatory cash transfer, by receipt of other UN assistance

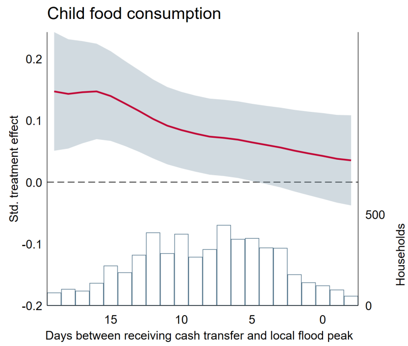
	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis- faction	(4) Pre- emptive actions	(5) Asset loss index	(6) Borrowing index	(7) Remit- tances	(8) Earning potential index
ITT								
ITT	0.085***	0.031	0.178***	0.068***	-0.099***	-0.047	0.032	0.083***
	(0.030)	(0.027)	(0.029)	(0.026)	(0.028)	(0.031)	(0.028)	(0.027)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11
By UN support								
ITT	0.083***	0.036	0.179***	0.064**	-0.101***	-0.023	0.054*	0.078***
	(0.032)	(0.029)	(0.031)	(0.027)	(0.029)	(0.033)	(0.029)	(0.028)
ITT × UNFPA assistance	-0.091**	0.097**	0.023	0.062*	0.016	-0.051	0.035	0.049
	(0.045)	(0.041)	(0.042)	(0.035)	(0.037)	(0.043)	(0.047)	(0.040)
ITT × FAO assistance	-0.051	0.079	0.025	0.104**	0.089**	-0.198***	0.006	-0.036
	(0.054)	(0.051)	(0.049)	(0.046)	(0.045)	(0.051)	(0.056)	(0.048)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11

Notes: The first panel shows the standardised mean treatment effects using our main specification. The second panel shows effects of the cash transfer for households not receiving any support from other sources, and the marginal effects for households receiving assistance from UNFPA and FAO respectively. We control for the same covariates and union fixed effects used in the main analysis. * p<0.10, ** p<0.05, *** p<0.01

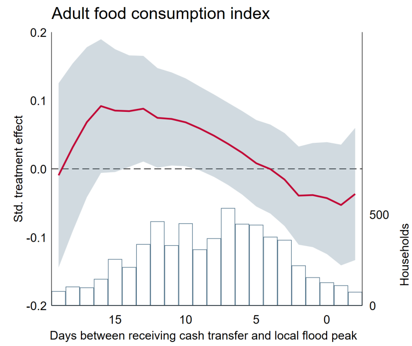
Table A9: Lee bounds to correct for differential attrition

	Standardised mean treatment effect [N]	Call received Δ n-r = 4.3% [N]
Child food consumption	0.085***	0.085***/0.085***
	[7631]	[7631/7631]
Adult food consumption index	0.031	-0.068**/0.092***
	[9034]	[8746/8743]
Life satisfaction	0.178***	0.042/0.178***
	[9023]	[8778/9023]
Pre-emptive actions	0.068***	0.027/0.068***
	[9030]	[8919/9030]
Asset loss index	-0.099***	-0.205***/-0.099***
	[9033]	[8756/9033]
Costly borrowing index	-0.047	-0.151***/-0.032
	[6104]	[5914/6049]
Remittances	0.032	0.032/0.032
	[9033]	[9033/9033]
Earning potential index	0.083***	-0.022/0.083***
	[9027]	[8745/9027]

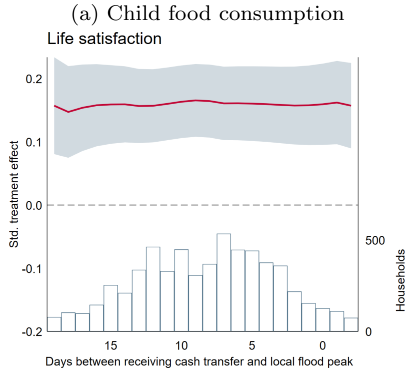
Notes: Lee (2009) bounds (lower/upper) for estimated treatment effects are shown for differential non-response rate of 4.3%, conditional on a call being received. Higher non-response was recorded in the control group. Covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. Union fixed effects are included. Standard errors are clustered at union level for inference. The number of observations included when calculating high and low bounds are shown in square brackets. * $p<0.10$, ** $p<0.05$, *** $p<0.01$



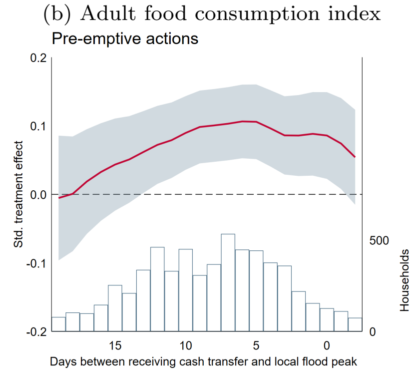
(a) Child food consumption



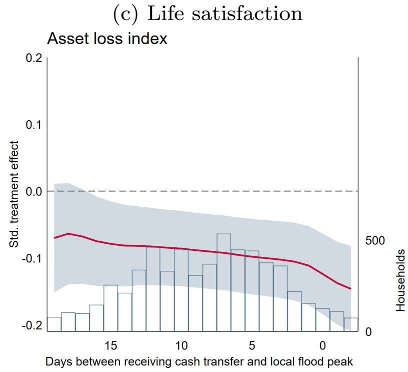
(b) Adult food consumption index



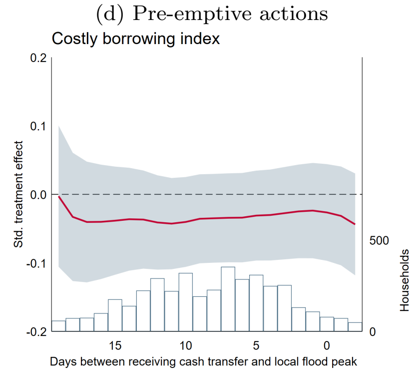
(c) Life satisfaction



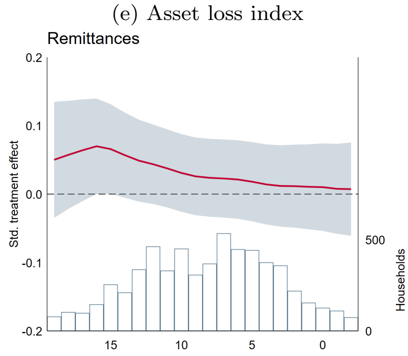
(d) Pre-emptive actions



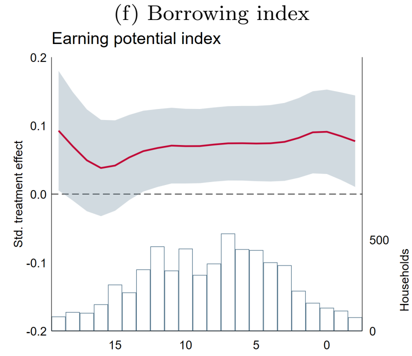
(e) Asset loss index



(f) Borrowing index



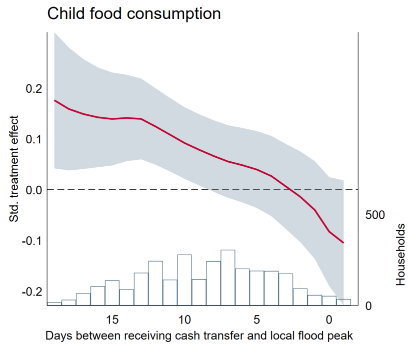
(g) Remittances



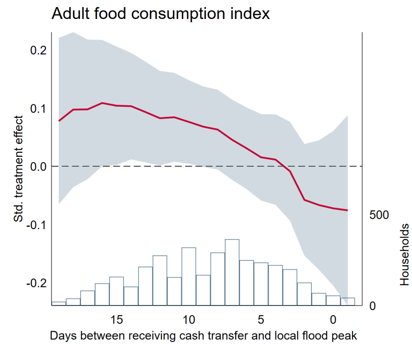
(h) Earning potential index

Figure A2: Non-parametric analysis for all outcomes

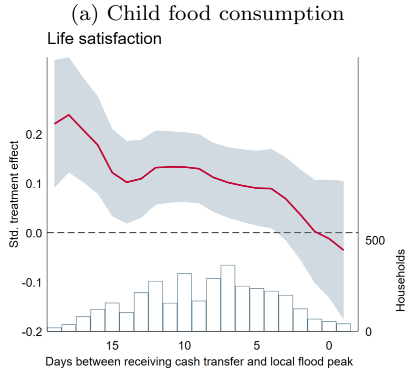
Notes: Estimates are from local linear regressions using Integrated Mean Squared Error (IMSE) optimal bandwidth and Epanechnikov kernel weighting, evaluated on days when transfers were made to at least 1% of treated households. The number of treated households on each day is shown for reference. 95% confidence intervals are shown.



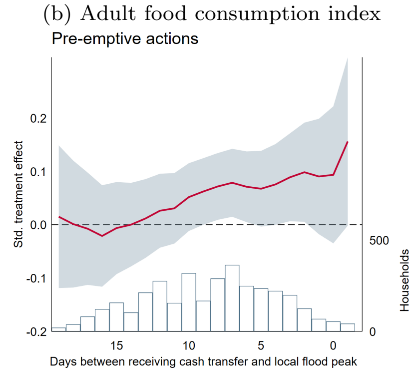
(a) Child food consumption



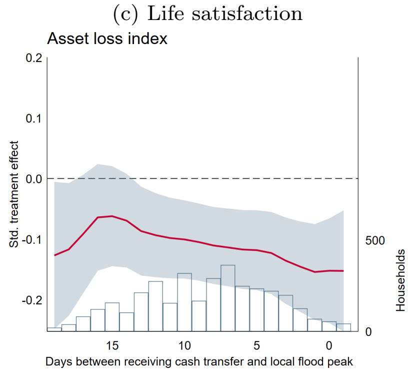
(b) Adult food consumption index



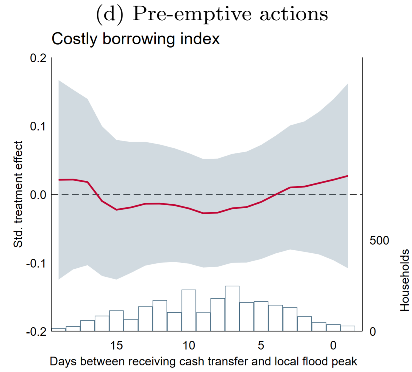
(c) Life satisfaction



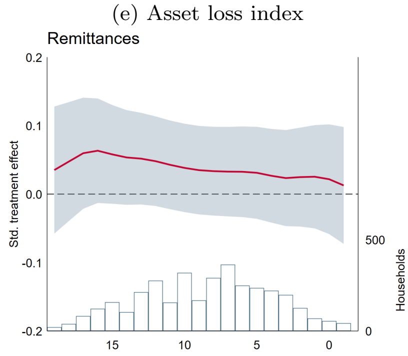
(d) Pre-emptive actions



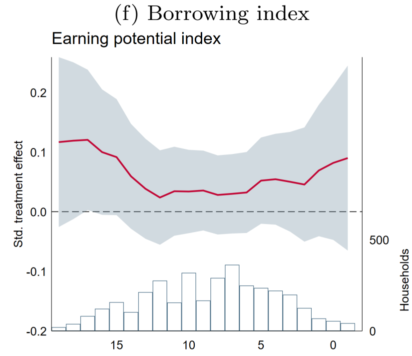
(e) Asset loss index



(f) Borrowing index



(g) Remittances



(h) Earning potential index

Figure A3: Non-parametric analysis for all outcomes - 14 July Transfers

Notes: Estimates are from local linear regressions using Integrated Mean Squared Error (IMSE) optimal bandwidth and Epanechnikov kernel weighting, evaluated on days when transfers were made to at least 1% of treated households. The number of treated households on each day is shown for reference. 95% confidence intervals are shown.

Table A10: Estimated treatment effect for other outcomes in the second survey

	(1) Asset investment index	(2) Borrowing index	(3) Remit-tances	(4) Earning potential index
ITT	-0.038	-0.124	0.074	-0.074
	(0.116)	(0.133)	(0.095)	(0.088)
Controls	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓
N	1537	774	1536	1537
R^2	0.07	0.13	0.09	0.09

Notes: The table reports the standardised mean treatment effects for four primary outcomes measured in the second survey five months after the cash transfer. Please refer to the main text for the other primary outcome variables. Given that the second survey was conducted five months after the floods, two primary outcomes were measured differently to better capture recovery from the floods, relative to the first survey. 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 – after the flood peak. The earnings potential index was reduced to number of hours worked in the last seven days and no longer included the ability to replant. Covariates include age, gender, education level, household size, dependency ratio, house structure and land type. Union fixed effects are included. * p<0.10, ** p<0.05, *** p<0.01

For Online Publication

The following document contains details on satellite flood data construction, variable construction and additional analysis used in the paper "Anticipatory Cash Transfers Improve Outcomes for Flood-Affected Households".

Section 1: Processing and validation of satellite flood data*

Methods

Flood mapping with Sentinel-1 SAR

To estimate the extent of flooding over space and time, we first take freely available 10m Sentinel-1 Synthetic Aperture Radar (SAR) imagery from the European Space Agency.[36] Sentinel-1 SAR data has been frequently applied to map flooding in recent literature, including in Bangladesh (Uddin *et al.*, 2019). SAR imagery is particularly useful for flood mapping as it can be captured even in the presence of cloud cover, unlike satellite imagery from optical sensors such as Landsat and MODIS. This is relevant for the area of interest in Bangladesh which has significant cloud cover during monsoon seasons. Water bodies can be identified from SAR imagery due to their dark appearance.

The methodology used is adapted from the UN-SPIDER Knowledge portal and applies a change detection and thresholding (CDAT) approach to identify flooded areas.[37] The CDAT methodology for identifying flooded areas from Sentinel-1 data has been applied in contexts such as Bangladesh, Namibia and the UK (**long2014flood**; Singha *et al.*, 2020; Clement *et al.*, 2018). The analysis was performed in Google Earth Engine. The image processing methodology described below is largely summarized from the UN-SPIDER guidance.

Image filtering and preprocessing

Available Sentinel-1 imagery for the time period of interest is filtered according to the instrument mode, polarization, and pass direction. This filtering is necessary to ensure that

*We thank Leonardo Milano and Hannah Ker from the UN Office for the Coordination of Humanitarian Affairs's Centre for Humanitarian Data and MapAction for processing and analysis of the satellite flood data. See also `https://ocha-dap.github.io/pa-anticipatory-action/analyses/bgd/docs/summary_flooding.html`

[36]https://sentinel.esa.int/web/sentinel/user-guides/sentinel-1-sar

[37]https://www.un-spider.org/advisory-support/recommended-practices/recommended-practice-google-earth-engine-flood-mapping/in-detail

mosaicked images share the same characteristics. The selected imagery has already undergone pre-processing steps to convert pixel values to their backscatter coefficient. These steps include thermal noise removal, radiometric calibration, and terrain correction, as well as applying a smoothing filter to reduce the speckle effect of radar imagery.[38]

Change detection and thresholding (CDAT) to identify flooding

This methodology identifies flood extent by comparing imagery captured during flooding to baseline imagery without flooding for the area of interest. We took the median of all images from December 2019 to January 2020 from the area of interest to generate the baseline mosaic. We also checked the EM-DAT database to ensure that no floods were recorded during this period. The flood period mosaic is divided by the baseline mosaic, with pixel intensity in the resulting image indicating the degree of change between the two images. A threshold of 1.25 is applied to generate a binary layer indicating the full estimated extent of flooding. This threshold level is taken directly from the UN-SPIDER guidance, where it was selected "through trial and error". The appropriateness of this threshold level was also manually checked by comparing the derived flood extents with the satellite imagery for selected dates.

The flood extent output is further refined to mask the main water bodies and also remove regions where the average slope is greater than 10%. Main water bodies are identified using the JRC Global Surface Water dataset, using a threshold of areas covered by water for at least 10 months in a year. Slope is calculated from the WWF HydroSHEDS DEM, based on SRTM data.

To understand the evolution of flooding over time, we repeated this change detection process separately on all available Sentinel-1 data for the area of interest between June - August 2020. In this case, 17 mosaicked images were available throughout this time period for our area of interest, generating a total of 17 output Shapefiles that delineate flood extent for dates between June 2020 and August 2020, as shown in Figure S1.

The estimates of flood extent were then aggregated to a given admin unit (5831 mauzas) by calculating the total flooded fraction within each unit for each point in time. Note that the area of permanent water bodies was removed from the area of each admin unit. The flooded fraction values thus represent the fraction of flooded area that is not normally covered by water.

While SAR imagery has commonly been used to map flooding, it is not without its limitations. As is well-acknowledged within the literature, classification errors may arise in cases where water surfaces are roughened by wind or rain, and where other flat land surfaces

[38] https://developers.google.com/earth-engine/guides/sentinel1

(such as roads) are misclassified as water. Flood detection from SAR imagery is also poorer in urban areas and areas with dense, protruding vegetation (Schumann and Moller, 2015). Results should also not be mistaken for any indication of flood depth, as only information about surface water coverage is captured by the satellite imagery.

Interpolating and smoothing flooding estimates over time

As the temporal frequency of Sentinel-1 imagery can be up to 12 days between images, we cannot solely rely on the results of analysing this imagery to accurately identify peak flooding dates. We therefore estimate the flooded fraction by admin unit at daily intervals by fitting the Sentinel-1 data points to a Gaussian function. The peak of the Gaussian curve for each admin unit was then used to identify the estimated peak flooding date for that unit. This method simplifies the shape of the flooding time series and reduces the potential impacts from noise introduced by the limitations of the flooding estimates derived directly from the Sentinel-1 imagery.

It should be noted that a Gaussian function significantly simplifies the dynamics of flooding. By fitting to this function, we are making the assumption that flooding extent within a mauza increased and decreased at the same rate, and the flooding had a single, distinct peak. These results should be considered as a best-estimate of the flooding dynamics, based on the information available. A notable limitation of this current approach is that it does not capture multiple flooding peaks, as is known to have occurred in some regions of our study area. Mauzas where the Gaussian function fit were poor are flagged so they can be excluded from the analysis.

Validation

Comparison against the dynamics of GloFAS water discharge measurements

Figure S2 offers a comparison between GloFAS water discharge measurements at stations along the Jamuna River and the satellite-derived flooded fraction for the mauzas that contain those stations. While we should be careful directly comparing measurements of two different variables (flood extent and river water level), this visual comparison allows us to validate that the satellite-derived flooding fraction peaked at a similar time to when the nearby river water level peaked.

Validating the flood extend against optical imagery

In the absence of ground-truth data, it is standard practice within the literature to validate Sentinel-1 derived flood extents against alternative sources of optical satellite imagery. However, obtaining cloud-free imagery to cover a large region, particularly during the rainy season in Bangladesh can be challenging. Nevertheless, we obtained Sentinel-2 imagery from July 27th which provides a cloud-free look at some of the regions within our study area. We can visually compare this imagery against our output flood extent areas from Sentinel-1 imagery from a similar date to check for agreement with areas that appear to be flooded, as demonstrated in Figure S3. While these images are only for a single date and for subsets of our study area, we see a clear agreement between flooded areas in the optical imagery (underlying layer and sole layer on the right) and the red overlaid flood extents from our Sentinel-1 analysis.

Comparison against key informant interviews from selected unions

We received survey data from key informants in 20 unions indicating their perceived flooding extent in their surrounding union. Each of these unions has data from 1-3 interview respondents. This data offers a useful comparison against our satellite-derived flooding estimates. Across these unions we see a general agreement in the flooding trend over time, and in many cases quite similar estimated magnitude, as shown in Figure S4.

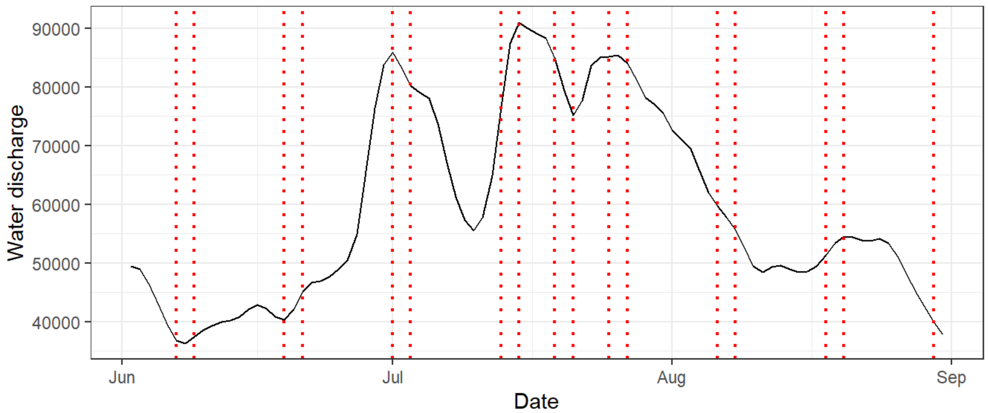


Figure S1: Dates of available Sentinel-1 satellite imagery against GloFAS water discharge measurements at Bahadurabad station

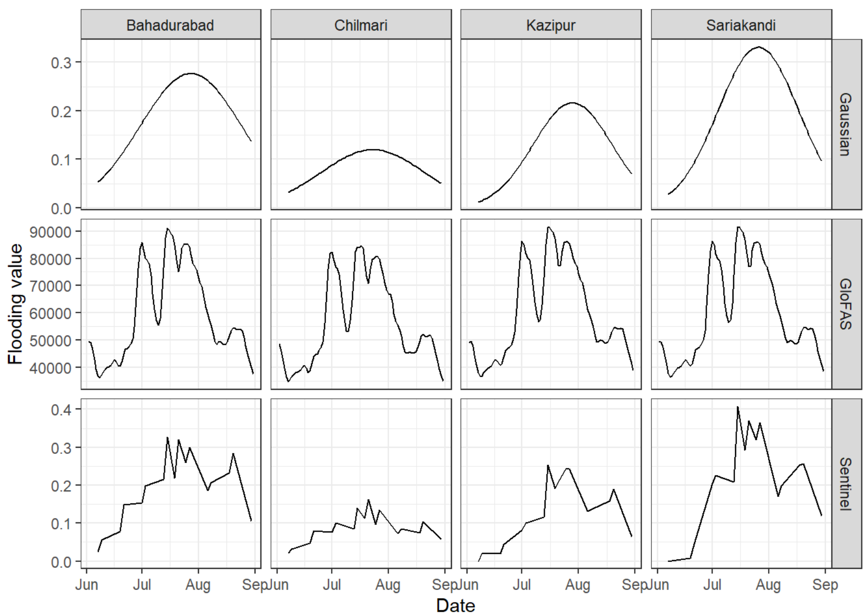


Figure S2: Comparison between flooding estimates against GloFAS water discharge measurements

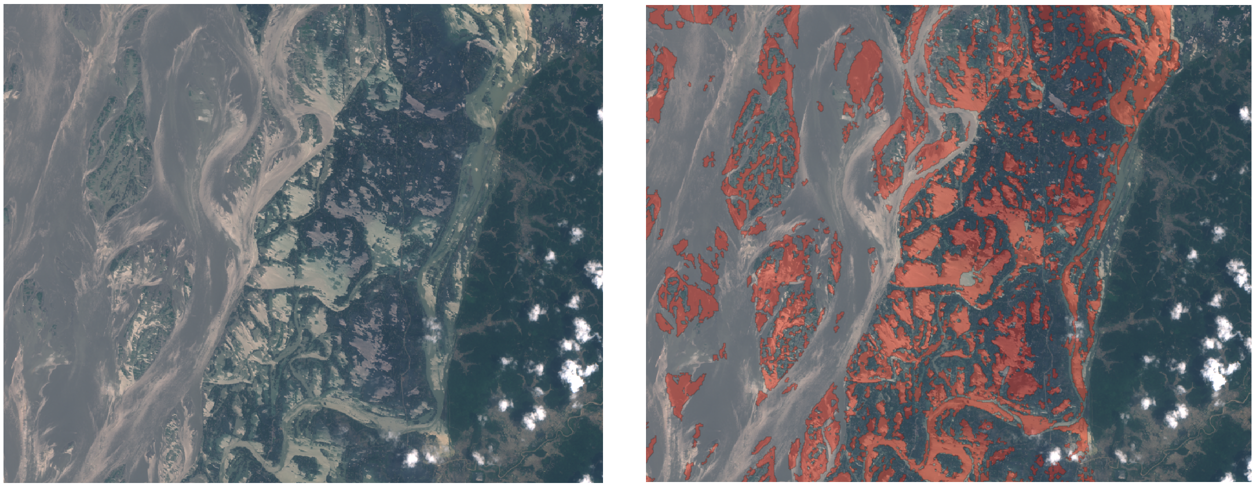


Figure S3: Sentinel-2 optical imagery overlaid with flood extent areas derived from Sentinel-1 imagery

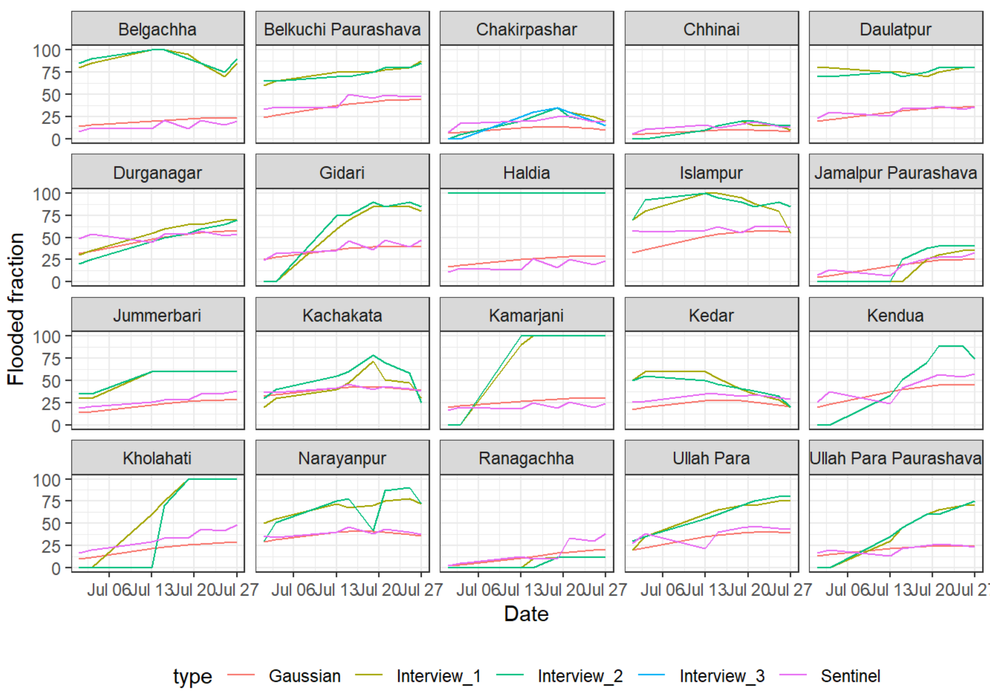


Figure S4: Comparison between satellite derived flooding estimates and those from key informants

Section 2: Additional Analysis

Heterogeneity by land type

In our pre-analysis plan, we pre-specified that we would explore heterogeneity by land type. We proxied vulnerability to flooding by land type defined as three categories that are increasingly prone to flooding: (i) protected or embanked mainland; (ii) unprotected mainland located outside the embankment; and (iii) char lands, which include low-lying islands formed by silt deposits along the course of the Jamuna River. Land type is easily observed and thus could serve as a helpful tool for targeting. Variables for land type are defined at mauza level. We use spatial data to categorise each of the 639 mauzas in our sample by the predominant land type based on their location relative to the braided shape of the Jamuna River and about 800 km of existing flood embankments. Roughly a third of the sample is located on each land type.

Table S1: Heterogeneity by land type

	(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
ITT	0.141**	0.052	0.252***	0.058	-0.134***	-0.038	0.072	0.004
	(0.055)	(0.053)	(0.046)	(0.047)	(0.049)	(0.058)	(0.052)	(0.050)
Char	0.015	-0.171**	0.130**	-0.062	-0.025	-0.025	-0.016	-0.137**
	(0.075)	(0.068)	(0.064)	(0.067)	(0.068)	(0.075)	(0.072)	(0.066)
ITT × char	-0.053	-0.025	-0.200***	0.002	0.016	0.024	-0.026	0.134**
	(0.072)	(0.066)	(0.064)	(0.062)	(0.067)	(0.074)	(0.068)	(0.063)
Protected	0.082	-0.075	0.042	-0.052	-0.169**	0.074	0.117*	-0.006
	(0.078)	(0.072)	(0.068)	(0.070)	(0.068)	(0.081)	(0.071)	(0.070)
ITT × protected	-0.112	-0.036	0.020	0.028	0.095	-0.061	-0.097	0.081
	(0.075)	(0.071)	(0.070)	(0.066)	(0.064)	(0.081)	(0.071)	(0.069)
Treat effect: Char	0.087*	0.027	0.052	0.061	-0.118**	-0.014	0.046	0.139***
Treat effect: Protected	0.028	0.017	0.272***	0.087*	-0.040	-0.099*	-0.025	0.086*
p-value: Char=Prot.	0.394	0.866	0.001	0.674	0.198	0.243	0.275	0.385
F-test p-value:								
Control $\Delta = 0$	0.514	0.037	0.123	0.617	0.024	0.422	0.105	0.050
Treatment $\Delta = 0$	0.321	0.878	0.001	0.889	0.257	0.500	0.352	0.106
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11

Notes: The table reports the standardised mean treatment effect for each outcome estimated using the main specification. The treatment variable is interacted with each category of land type where the household is located, where the baseline is unprotected mainland. Covariates include age, gender, education level, household size, dependency ratio and house structure. Union fixed effects are included. * p<0.10, ** p<0.05, *** p<0.01

Additional robustness checks

The following tables include additional robustness checks for our results on heterogeneity by timing of the cash transfer.

Table S2: Treatment effects by timing of transfer, 14-16 July vs. 30 July

	(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
Transfer on 14–16 July	0.087***	0.040	0.174***	0.078***	-0.093***	-0.031	0.030	0.078***
	(0.031)	(0.028)	(0.029)	(0.026)	(0.028)	(0.032)	(0.029)	(0.027)
Transfer on 30 July	0.072	-0.032	0.207***	-0.004	-0.144***	-0.154***	0.046	0.121***
	(0.048)	(0.042)	(0.045)	(0.039)	(0.041)	(0.046)	(0.048)	(0.041)
q-value: 14-16 July	0.009	0.133	0.001	0.008	0.004	0.273	0.261	0.008
q-value: 30 July	0.129	0.295	0.001	0.512	0.004	0.004	0.273	0.008
p-value: 14-16 July = 30 July	0.728	0.052	0.416	0.014	0.133	0.002	0.716	0.226
q-value: 14-16 July = 30 July	0.399	0.060	0.295	0.019	0.129	0.006	0.399	0.204
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7631	9034	9023	9030	9033	6104	9033	9027
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11

Notes: The table reports the standardised mean treatment effects for transfers made on 14-16 July and for transfers made on 30 July. We report the p-value from a test that treatment effects are equal, and false discovery rate q-values across the eight outcomes for each timing of the transfer respectively. We control for the same covariates and union fixed effects used in the main analysis. * p<0.10, ** p<0.05, *** p<0.01

Table S3: Treatment effects of an earlier cash transfer, controlling for flood intensity and survey 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
ITT	0.118***	0.038	0.160***	0.082***	-0.079**	-0.055	0.026	0.081***
	(0.035)	(0.032)	(0.033)	(0.031)	(0.032)	(0.036)	(0.034)	(0.031)
Transfer × days before flood peak (ITT)	0.002	0.005**	-0.000	0.002	0.002	0.006***	-0.002	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
p-value: Transfer × days	0.429	0.013	0.824	0.422	0.372	0.006	0.522	0.163
q-value: Transfer × days	0.751	0.050	1.000	0.751	0.751	0.047	0.767	0.483
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7492	8878	8867	8874	8877	5999	8877	8871
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11

Notes: The first panel reports the standardised mean treatment effect across outcomes for the main specification. The second panel shows the average marginal treatment effect of receiving the transfer a day earlier relative to the local flood peak. False discovery rate q-values are calculated over the marginal treatment effects for the eight outcomes following the sharpened two-stage procedure of Benjamini *et al.*, 2006. In addition to the same household characteristics and union fixed effects used in the main analysis, we also control for flood extent and duration and survey date. * p<0.10, ** p<0.05, *** p<0.01

Table S4: Variable construction for welfare measures

Index	Sub-scale	Question(s)
1. Children's food consumption	Dummy variable for whether children consumed three or more meals in the previous day.	How many meals did children (younger than 15 years old) eat yesterday? [Number of meals]
2. Adult food consumption	Number of days meat products were consumed over the last seven days (the selection of meat will be confirmed by looking at the variation in frequency of consumption of all food items listed).	How many days over the last seven days, did adult members (15 years or older than 15 years) of your household eat meat, fish, eggs (goat, beef, chicken, buffalo, fish, including tuna, dry fish, and/or other seafood, eggs)?
	Food consumption score (FCS)	The following question will be used to construct the FCS. The FCS will then be calculated according to the standard formula: $FCS = (starches * 2) + (pulses * 3) + vegetables + fruit + (meat * 4) + (dairy * 4) + (fats * .5) + (sugar * .5)$ How many days over the last seven days, did adult members (15 years or older than 15 years) of your household eat the following food items, prepared and/or consumed at your home? 1. Cereals, excluding rice (pasta, bread, sorghum, millet, maize, fonio, potato, yam, cassava, white sweet potato, parched rice (muri), chira) 2. Legumes/nuts (beans, peas, peanuts, lentils, mascalai, mung beans, khesari, ankar, arahar pulses, nut, soy, and / or other nuts) 3. Milk and other dairy products (fresh milk/sour, yogurt, cheese, other dairy products) (exclude margarine/butter or small amounts of milk if used in tea/coffee) 4. Meat, fish, eggs (goat, beef, chicken, buffalo, fish, including tuna, dry fish, and/or other seafood, eggs) 5. Vegetables and leaves (various spinach, onion, tomatoes, carrots, peppers, green beans, lettuce, etc.) 6. Fruits (banana, apple, lemon, mango, papaya, peach, etc.) 7. Oil, fat, butter (vegetable oil, palm oil, shea butter, margarine, other fats/oil) 8. Sugar or sweet (sugar, honey, jam, cakes, candy, cookies, pastries, cakes and other sweets including sugary drinks)
3. Wellbeing	Life satisfaction	Please imagine a ladder with steps numbered from 0 at the bottom to 10 at the top. The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you. Which step of the ladder best represents the way you personally feel you stand these days? [0–10]

Note that for questions on food consumption, respondents were encouraged to pass the phone to someone in the household who could respond the questions about food consumption with sufficiently good recall.

Table S5: Variable construction for other outcome measures

Outcome	**Construction**	**Question(s)**
1. Actions taken to reduce the impact of the flood	Number of preventative actions taken	Which actions did you take to prepare for the flooding? (count of the following actions taken): 1. Protect valuable assets 2. Evacuate household members/moved 3. Purchase food 4. Evacuate livestock 5. Protect roof/walls 6. Warn others
2. Household asset loss or damage	A standardised index of the following variables:	
	Number of livestock that died over the past two months	How many cows, calves and buffalo that you owned died during the past two months (from July 15 to September 15)? How many goats, sheep and pigs that you owned died in the past two months(from July 15 to September 15)?
	Number of categories of household assets that were lost or damaged	Other than damage to your house and animals; were any assets damaged or lost due to the flooding? What assets were damaged/lost? Enter all that apply. 1. Poultry 2. Crop (stock in home) 3. Irrigation pump 4. Fruit plantation 5. Fish 6. Equipment for fishing i.e., fishing net 7. Vehicle by any animal 8. Boat 9. Rickshaw, van, or cycle etc. 10. Shop 11. Sewing machine 12. Furniture 13. Clothes 14. Household appliances like home utensils, mobile phone, television etc. 15. Ornaments (gold and silver) 16. Others: (specify)

Variable construction for outcome measures (cont.)

Outcome	**Construction**	**Question(s)**
	Area of cultivated plots lost [in decimal]	Have you lost cultivated crops in the past two months (from July 15 to September 15) due to the flooding? [Yes/No] If yes: How much have you lost in cultivated plots in decimal?
3. Costly borrowing	A standardised index of the following two variables: 1. How much was borrowed in the last two months (in BDT)? 2. The highest interest rate charged (percent per month)	In the past two months (from July 15 to September 15), has your household borrowed any money from friends/family/credit institutions or groups – both formal and informal – to cover for basic needs? [Yes/No] If yes: How much did you borrow in the past two months (from July 15 to September 15)? [BDT] What is the highest interest rate you were charged on the loan(s) you received in the past two months (from July 15 to September 15)? [%] Was this interest rate per month or per year? [monthly/yearly]
4. Remittances	Dummy variable for whether household received remittances in the last two months.	Did you receive any remittances in the past two months (from July 15 to September 15)? [Yes/No] If yes: How much did you receive in the past two months (from July 15 to September 15)? [BDT]
5. Earnings potential	A standardised index of the following two variables: 1. Able to replant (dummy variable taking the value of 1 if the household reported replanting) 2. Number of hours worked for an income in the last seven days (hours)	Have you lost cultivated crops in the past two months (from July 15 to September 15) due to the flooding? [Yes/No] If yes: Have you been able to replant? [Yes/No] How many hours did you or someone in your household work towards an income in the past seven days?