Anticipatory Cash Transfers in Climate Disaster Response*

Ashley Pople† ⓡ Ruth Hill‡ ⓡ Stefan Dercon§ ⓡ Ben Brunckhorst¶ ⓡ

December 13, 2022

In the face of increasing climate volatility and stretched aid budgets, more effective ways to support households in times of crisis are needed. This paper examines the welfare impact of an anticipatory cash transfer provided to ultra-poor households forecast to experience extreme floods in Bangladesh. Evidence on the impact of one-off cash transfers in disaster settings is limited despite their widespread use, and the welfare cost of delaying assistance is rarely considered. To assess impact, we exploit administrative constraints caused by the rapid onset of the flood and Covid-19 restrictions that excluded otherwise comparable and eligible households. We find that the anticipatory cash transfer was mostly spent on food and water, and beneficiary households were 36% less likely to go a day without eating during the flood. Three months after the flood, beneficiary households had significantly higher child and adult food consumption and subjective well-being. We find that they experienced lower asset loss, engaged in less costly borrowing after the flood, and reported higher earning potential. These benefits accrue in the months before a conventional humanitarian response would arrive, highlighting the welfare gains from acting early. We show further evidence that small changes in timing matter: receiving cash a day earlier resulted in a small increase in food consumption months later.

Keywords: finance and microfinance, climate change
JEL Codes: D12, O12, Q54

---

*We are grateful to Daniel Pfister, Dirk-Jan Omtzigt, Leonardo Milano, Hannah Ker, Ahmadul Hassan and our World Food Programme partners for their ongoing support and provision of data; without them, this project would not have been possible. We thank Abi Adams-Prassl, Christopher Woodruff, Michelle Rao, Paris Kazis, Niger Dilnahar, Johannes Abeler, Hamish Low, Simon Quinn, Ferdinand Rauch and seminar participants at Oxford University, Centre for the Study of African Economies (CSAE), Office for the Coordination of Humanitarian Affairs (OCHA), World Bank and the Bangladesh anticipatory action learning group for their helpful comments. Lastly, we acknowledge Data Analysis and Technical Assistance (DATA) for their support in conducting our large surveys. This randomised control trial was registered in the American Economic Association RCT Registry under trial number 6576 (https://doi.org/10.1257/rct.6576-1.1). It received ethics approval by the Oxford Central University Research Ethics Committee (Protocol No. ECONCIA20-21-24).

†University of Oxford: ashley.pople@economics.ox.ac.uk

‡Centre for Disaster Protection: rhill@disasterprotection.org

§University of Oxford: stefan.dercon@economics.ox.ac.uk

¶Centre for Disaster Protection: ben.brunckhorst@ouce.ox.ac.uk

1 Introduction

When disasters strike, governments or humanitarian organisations often step in to provide time-limited support to those worst affected. Increasingly, support is provided in the form of one-off cash transfers. This trend was emphasised by the unprecedented scale of temporary cash transfers introduced by governments in response to Covid-19. Gentilini *et al.* (2020) record 1,179 social protection measures in 212 countries during the first six months of the pandemic, with half taking the form of cash transfers. The impact evaluation literature on regular transfers indicates that they help households cope with shocks (Janvry *et al.*, 2006; N. D. Jensen *et al.*, 2017; Asfaw *et al.*, 2017; Knippenberg and Hoddinott, 2017; Adhvaryu *et al.*, 2018; Premand and Stoeffler, 2020). Despite their popularity, evidence on the impact of one-off transfers in a crisis is limited, reflecting more broadly a dearth of evaluations in the humanitarian sector (Puri *et al.*, 2017). As a result, little is known about whether one-off transfers have impacts beyond providing immediate support to consumption, and how their timing, size, and targeting affects welfare. Recently in the humanitarian sector, forecast-based 'anticipatory' transfers are becoming more prevalent. These transfers target vulnerable households in advance of a shock to help them mitigate impacts, protect livelihoods and cope in the aftermath, but little is known about the impacts of these either. In a world of increasing climate volatility and stretched aid budgets, a better understanding of how to support households effectively in times of crisis is needed.

Our paper addresses this gap by examining the welfare impact of a one-off anticipatory cash transfer provided to households facing extreme floods. In July 2020, the World Food Programme (WFP) sent BDT 4,500 (approximately US$53, equivalent to two weeks of household food expenditure) using mobile money to 23,434 ultra-poor households along the Jamuna River that were forecast to experience severe flooding. The floods that followed were some of the most severe and protracted in decades. This novel 'anticipatory action' approach to humanitarian relief uses impact-based forecasts to provide support before the peak of the disaster. It contrasts with a more traditional response that reacts ex post to a materialised need. In 2020, the anticipatory cash transfer reached affected households 100 days earlier than previous WFP interventions in the same context.

To assess impact, we compare households that received the cash transfer to otherwise comparable households that did not. Our identification strategy relies on administrative constraints that caused plausibly exogenous variation in treatment. Specifically, pre-existing lists of households were used for targeting due to the Covid-19 pandemic, but not all listed households were reached by phone due to time constraints, nor targeted due to the use of a single mobile banking platform. As a result, some households on the pre-identified list could not be reached, independently of their experience of the flood, and even though they used mobile phones and mobile cash as much as the treated households. 10 to 12 weeks after the cash intervention, we randomly sample 9,130 households, using phone surveys, of which 8,954 households met our criteria for inclusion. As far as we are aware, this is the first large-scale evaluation that rigorously measures the welfare impact of humanitarian cash transfers provided either in anticipation or ex post response to a sudden onset climate disaster (Doocy and Tappis, 2017). Gros, Bailey, *et al.* (2019) and Gros, Easton-Calabria, *et al.* (2020), the papers most similar to our own, show that anticipatory cash transfers in Bangladesh and Mongolia, respectively, are promising, but these studies face dual limitations of small sample sizes and imbalanced comparison groups.

We find that the cash transfer significantly improves child and adult food consumption, and subjective wellbeing, even when measured three months after the intervention. Children in treated households were three percentage points more likely to consume three or more meals on the day prior to the survey (relative to 80% of control households). This result is salient in light of a large body of literature highlighting the long-term consequences of temporary child undernutrition on educational, income and health outcomes (Chen and Zhou, 2007; Maccini and Yang, 2009; Dercon and Porter, 2014; Dinkelman, 2017; Victora *et al.*, 2021).

Our analysis points to a number of behavioural changes that could have contributed to these welfare effects three months after receiving the modest cash transfer. Cash was mostly spent on food and water; treated households were 10 percentage points less likely to go a day without eating during the flood shock itself (relative to 28% of control households). Treated households were more likely to evacuate household members and livestock relative to control households. The cash transfer significantly decreased asset loss (especially for productive assets) and costly borrowing. We also find that treated households reported higher earning potential three months later in terms of wage hours and crop cultivation. Using a second survey conducted five months after the transfers, we observe a persistent positive effect on food consumption for adults, but not for children who catch up among control households. Although the cash transfers likely conveyed information about the severity of the flood, we believe that the cash transfer played a primary role in alleviating liquidity constraints at a critical juncture in the flood. Our results are robust to alternative model specifications, different criteria for the comparison groups, and bounding treatment effects to account for differential non-response.

Although we are unable to directly compare anticipatory action to a traditional humanitarian response, these welfare benefits were all recorded before a traditional response arrived. Without cash, comparable ultra-poor households experienced lower food consumption for both adults and children, lower wellbeing, higher asset loss, higher borrowing, and less employment in the period before the arrival of a traditional response. Some of these deficits, even when temporary, are known to have permanent scarring effects - for example, child undernutrition (Victora *et al.*, 2021) and costly borrowing - suggesting significant gains from acting early, even if later support helps recovery Dercon2004, Alderman2006, Maccini2009. By arriving earlier, the small cash transfer may have set households on a different trajectory by easing liquidity constraints at a critical juncture.

There are three theoretical reasons why receiving the cash earlier before the shock may be more beneficial than receiving it later during recovery. Firstly, an earlier cash transfer could widen the choice set for coping strategies, such as evacuating, protecting productive assets or stocking up on food supplies. Secondly, there are typically market disruptions for the duration of a climate shock, resulting in a limited ability to trade and the inflation of prices, such as increased boat prices during floods. Lastly, sudden onset disasters are traumatic, limiting mental bandwidth, and decisions made at a critical juncture could have long run consequences. Supporting food security and easing liquidity constraints at the onset of a disaster could improve decision making so to mitigate impacts and cope in the aftermath.

To investigate timing further, we explore whether small changes in when transfers were received relative to flood dynamics matter. We use exogenous variation in the number of days between receiving a transfer and peak local flooding in 630 mauzas (approximately village level) based on high resolution satellite data. We then investigate the relationship between the timing of the cash transfer and welfare effects using linear regression and non-parametric methods to estimate the marginal effect of receiving cash one day earlier. We find evidence to suggest speed matters: an earlier anticipatory transfer is welfare improving. Cash received earlier relative to the flood event had stronger effects on food consumption three months later. Welfare effects tend to dissipate for each day transfers are delayed.

Our paper contributes to a large literature on the role of cash transfers in alleviating poverty. There is strong evidence that a regular flow of cash transfers can cushion the negative income effects of shocks and thus partially limit the use of costly coping mechanisms (Janvry *et al.*, 2006; Del Carpio and Macours, 2010; Hou, 2010; Macours *et al.*, 2012; Aker *et al.*, 2016; N. D. Jensen *et al.*, 2017; Asfaw *et al.*, 2017; Knippenberg and Hoddinott, 2017; Adhvaryu *et al.*, 2018; Premand and Stoeffler, 2020). However, rigorous evidence on the effectiveness of a one-off cash transfer remains scarce, especially in response to shocks or disasters, whereby existing evidence focuses on recovery. For instance, Haushofer and Shapiro (2016) show that receiving a GiveDirectly cash transfer in Kenya in a lump-sum or in monthly payments result in similar welfare improvements, albeit different expenditure patterns, but these cash transfers were not shock-responsive. Closer to our work, two papers consider the impact of a top-up payment delivered to households from regular cash transfer programmes after a sudden onset climate disaster (tropical cyclone Winston in Fiji). Using a regression discontinuity design, both papers found that households that received cash transfers in the months after the shock recovered more quickly than those who did not (Mansur *et al.*, 2017; Ivaschenko *et al.*, 2020). In the aftermath of a slow-onset climate disaster (a drought), Del Carpio and Macours (2010) and Macours *et al.* (2012) use a cluster randomised control trial to show that a bi-monthly conditional cash transfer implemented by the Nicaraguan government had positive persistent impacts on child health, development, and labour. In a different domain, 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). Our paper evaluates the impact of a one-off ex ante cash transfer on ultra-poor households' ability to cope with a sudden onset climate disaster.

Our paper also makes a significant contribution to the literature on humanitarian evaluations. A review of 900 studies of humanitarian programmes found that only 31 could be classified as impact evaluations, of which only eight studies focused on the emergency relief phase (Puri *et al.*, 2017). Weingärtner *et al.* (2020) specifically review the evidence for anticipatory humanitarian action and find that little rigorous evidence of impact is available. Evaluations of humanitarian interventions have often focused on the impact of regular programming in protracted crises, for example the impact of regular cash transfer programmes (for example: Lehmann and Masterson, 2014; Battistin, 2016; Aker, 2013; Schwab, 2019).[1] The reasons are varied. For instance, it is often 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 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, and disasters ex-post disrupt the supply of basic services and infrastructure, including those needed for data collection. We address these challenges that are present in our own context by collecting a large post-intervention survey and exploiting the plausibly exogenous variation created by the incomplete targeting of households due to administrative constraints. Our identification strategy enables causal impact analysis of a humanitarian intervention in a fast-onset emergency, with broader policy relevance for development programmes.

An extensive body of literature compares the modality of cash versus in-kind interventions (Leroy *et al.*, 2010; Aker, 2013; Skoufias *et al.*, 2013; Cunha, 2014; Hidrobo *et al.*,

[1]As another example, there are several papers that examine the impact of nutrition interventions (for example: Aguayo *et al.*, 2015; Tranchant *et al.*, 2019), but reviews highlight that there are few papers that have rigorously measured the impact of nutrition interventions in emergencies (P. Webb *et al.*, 2014; P. Webb, 2015).

2014; Cunha *et al.*, 2019), but timing as a modality remains underexplored Aker2016, Asfaw2017, Clavijo2020. Our paper showcases that a faster transfer offers the opportunity to limit the welfare cost of the shock, with policy implications that go beyond the humanitarian sector. With recent advances in data and forecasting technologies, the onset of many disasters is more predictable. As a result, there has been growing interest in the role of 'shock-responsive' safety nets that can temporarily expand both in transfer size and coverage in response to shocks both within the humanitarian and development sector, without excessively burdening budgets (Andrews *et al.*, 2018; World Bank, 2018). The expansion in social protection during the Covid-19 pandemic provides many opportunities for assessing the impact of one-off transfers, and a research base on this is starting to emerge (Abay *et al.*, 2020; Arndt *et al.*, 2020; Bottan *et al.*, 2021). In addition to providing evidence on the impact of humanitarian interventions in sudden climate disasters, our analysis speaks to the potential welfare benefits from quickly and pro-actively adapting safety nets in response to shocks.

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. We explore how the impacts varied with the timing of the transfer in Section 6.1 and by land type in Section 6.2. Section 7 demonstrates the robustness of our results to mobile money use, alternative model specifications, and bounding exercises. 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 Intervention

The 2020 monsoon floods in Bangladesh were the second highest since 1989 and the second longest since 1998. More than one million households were inundated and 5.5 million people were directly affected by flooding at 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 was damaged, while 257 people lost their lives due to the floods (United Nations Resident Coordinator Office, 2020).

In July 2020, the United Nations piloted a novel approach to humanitarian action by employing a data-driven forecast to predict the impact of excess flooding along the Jamuna River[2], a particularly flood-prone area of northern Bangladesh. This forecast was used to

[2]Jamuna River is the local name for the Brahmaputra River.

trigger the release of anticipatory cash transfers prior to peak flooding. With support from the International Federation of Red Cross and Red Crescent Societies and the Bangladesh Red Crescent Society (BDRCS), the WFP sent BDT 4,500 (approximately \$53) using mobile money accounts to 23,434 vulnerable households in 131 unions (the smallest administrative unit in Bangladesh). The cash transfer is equivalent to approximately two weeks' food expenditure (World Bank, 2019). The cash was intended to mitigate the worst effects of the flood shock on household food consumption and mortality during the flood itself, as opposed to post-flood recovery. Water-tight storage containers and animal feed were also provided to 7,000 and 11,760 families, respectively, by the Food and Agriculture Organization of the United Nations (FAO). The United Nations Population Fund (UNFPA) also delivered hygiene, dignity, and health kits to 15,000 women, girls, and transgender people. There was little overlap between the cash and non-cash interventions, as the UN agencies mostly operated in different districts. This analysis focuses on the cash transfers made by WFP only; we account for any overlap in Section 4.

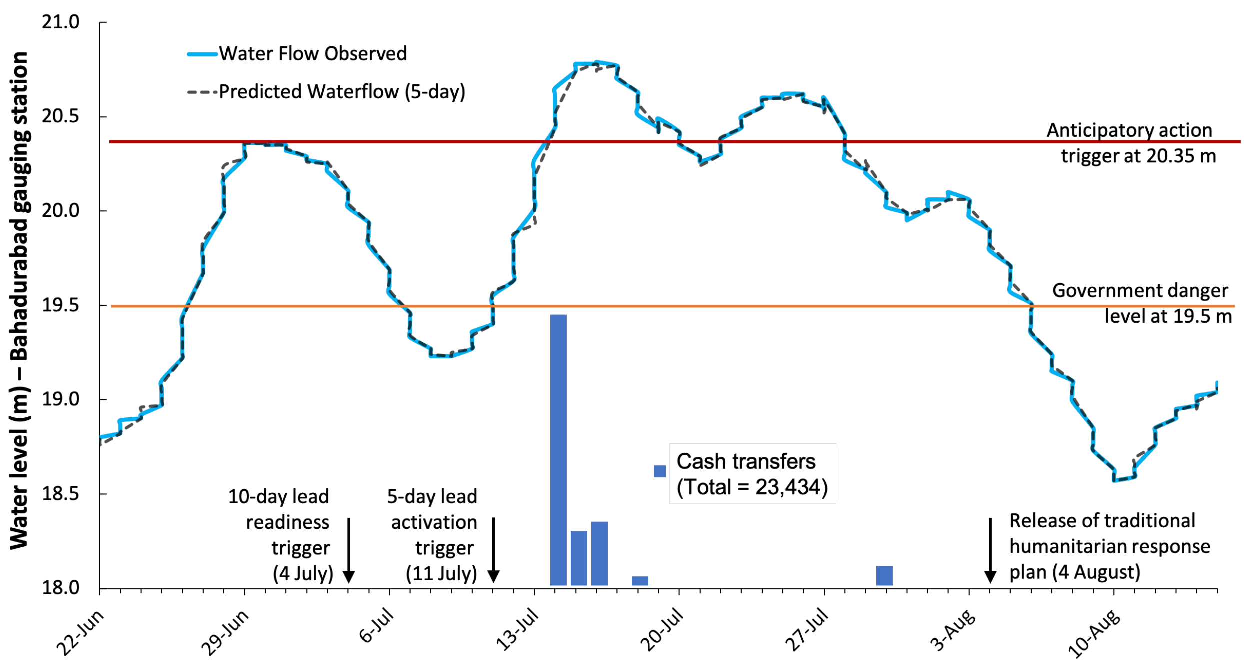
This anticipatory action approach is novel for the humanitarian sector. The approach uses predictive analytics, pre-arranged finance and pre-agreed actions to respond to foreseeable humanitarian needs. The earlier response contrasts to a traditional humanitarian response, which reacts to a materialised need after a severe shock occurs. For instance, during the last severe floods in 2019, beneficiaries received cash only about 100 days after the flood peak, despite a very rapid approval for funding made by the Central Emergency Response Fund. In this anticipatory action pilot, most beneficiaries received cash several days prior to peak flooding.[3] Not only is an anticipatory approach faster, but it can also be operationally cheaper to plan for and deliver an intervention before a climate disaster.

The timing of the anticipatory cash transfers was determined by a pre-defined set of triggers, based on forecasts of water flow from upstream at a centrally located gauge station, as illustrated by Figure 1. Given the accuracy with which forecasted water flow predicts actual water flow in Bangladesh, these forecasts were used in the design of two triggers indicating the onset of an extreme flood event. The first 'readiness' trigger was activated on 4 July 2020 and set in motion preparatory activities, including beneficiary verification

[3]Funding for the cash intervention was approved by the UN Central Emergency Response Fund 35 days after the flood peak in 2019, whereas advance plans and protocols enable the rapid disbursement of funds 14 days before the flood peak at Bahadurabad gauging station, the basis for forecasts, in 2020.

calls.[4] The second 'activation' trigger - activated on 11 July - initiated the cash transfers.[5] The forecasts predicted the flood peak at the Bahadurabad gauging station for 17 July 2020. Due to the unanticipated early arrival of the flood and to time constraints, WFP sent cash to households over a period of five days, on 14, 15, 16, 18 and 30 July, both just prior to and immediately after the flood peak. In contrast to this novel anticipatory approach, the traditional humanitarian response was initiated only after a needs assessment had been conducted on 4 August - a month after the first trigger. Figure 2 illustrates the geographic spread along the Jamuna River.

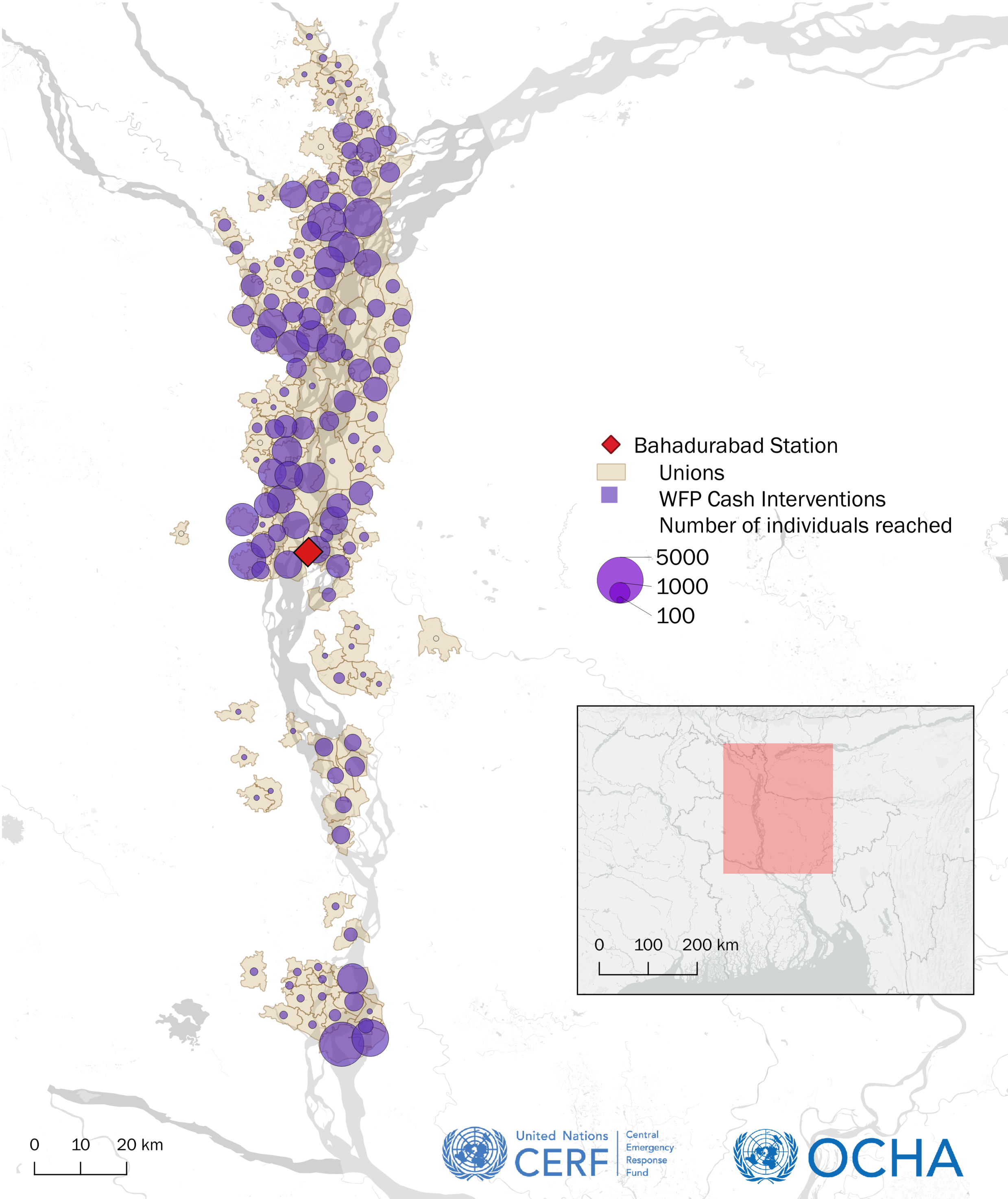
Figure 1: Timeline of triggers and cash transfers



[4]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.

[5]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.

Figure 2: Map of the WFP intervention along the Jamuna River



It is important to note that cash transfers not only provide liquidity to some of the most vulnerable and poorest households, but also convey information about the severity of the oncoming flood. WFP did not inform households that they would receive the cash transfer when they conducted the beneficiary verification phone calls, so households were unaware of the incoming cash transfer until its receipt, often only just before the flood peak itself. Nevertheless, households may have interpreted the receipt of a cash transfer by WFP during rising waters as indicating the severity of the oncoming shock or as a call to action. Just under two thirds of households across the treatment and control groups in our sample reported receiving early warning from formal or informal sources. In the absence of a credible early warning system, the information component of the transfer could be particularly powerful. In earlier studies, early warnings were critical features in ensuring that affected populations adopted preventative actions before floods (Carsell *et al.*, 2004; Kreibich *et al.*, 2005; Thieken *et al.*, 2007).

3 Sample and data

3.1 Sampling strategy

We leverage plausibly exogenous variation induced by administrative constraints experienced during programme roll-out as a natural experiment to assess whether a one-off anticipatory cash transfer reduces the effects of a flood shock on household welfare. We randomly sampled beneficiary households and control households; the latter were otherwise comparable but did not receive the cash transfer due to administrative constraints.

The process of household selection proceeded as follows. After activation of the readiness trigger in early July, the WFP and the International Federation of Red Cross and Red Crescent Societies Climate Centre first selected districts and unions in which to target beneficiaries based on a joint assessment of their flood risk and socio-economic vulnerability. Due to constraints imposed by the Covid-19 pandemic, pre-existing lists of vulnerable households were used to identify households in each union. The households on these lists had received assistance through UN interventions or government safety nets in previous years. After the readiness trigger and union selection, WFP contacted as many households as they could reach on these pre-existing lists via phone over a period of five days. These households were putatively drawn at random across the prioritised unions. During the call, the household's identity and location were verified and they were asked whether they had access to an active bKash account. Time constraints meant that WFP had only been able to establish a contract with one mobile money provider (bKash) for cash distribution, despite the prevalence of several mobile money services in the area.[6] Households deemed not to have an active bKash account were excluded from receiving the cash unless they were able to reactivate dormant accounts or set up a new one by the deadline provided (usually one to two days after the call). Cash was delivered to beneficiaries from 14-18 July owing to the sheer volume of cash transfers that needed to be distributed and the staggered roll-out of beneficiary verification due to time constraints. Households that were contacted later or able to activate a bKash account after 18 July were transferred the cash in a single lump sum on 30 July.

In sum, beneficiary households were selected if the following three criteria were met: (1) they were on the pre-existing lists of vulnerable households; (2) they could be reached by phone during the initial compilation of the list of beneficiaries and subsequent beneficiary verification; and (3) they had access to an active bKash mobile money account by the transfer date. Our control group is comprised of households that also met the first criteria but were deemed not to have met either (2) or (3) during the beneficiary verification.[7] These administrative constraints resulted in households not receiving the cash transfer, independently of their experience of the flooding.

We randomly sampled households that met these criteria within targeted unions where there were at least 10 beneficiaries and relevant control households. For treated households, we randomly sampled all or 60 beneficiaries (whichever was smaller) from each union for those who received a cash transfer from 14 to 16 July, and all households receiving cash on 30 July.[8] For the control households, we similarly sampled all or a random sub-sample of households that were recorded as excluded by WFP for one of the aforementioned reasons in equal proportions across unions.

3.2 Description of the sample

3.2.1 Sample size

Phone surveys were conducted in Bangla by trained phone survey enumerators with 9,130 households, 10 to 12 weeks after the intervention between 21 September and 8 October 2020. We targeted the household member whose names were on the WFP list of potential beneficiaries. Respondents were asked a series of questions, including on demographics, behavioural

[6]bKash currently holds approximately 75% of the market share, with Nagad and Rocket the main competitors holding; together, Nagad and Rocket hold approximately 20% of the market (Corporation, 2016).

[7]During the beneficiary verification, these households were recorded as unreachable, having access to a mobile money account with a different provider (e.g., Nagad or Rocket), a frozen bKash account, or no bKash account at all.

[8]A small number of households received cash on 18 July and we used this group to pretest the questionnaire. They do not appear in our sample.

response to the flooding, food consumption, household assets, life satisfaction, work, and use of the cash transfer (if applicable). Respondents received BDT 100 (approximately $1.18) in phone credit for completing the survey.

Of the 9,130 households surveyed, 8,954 households across 111 unions met our criteria for treated and control households based on their survey responses; 6,566 treated households reported receiving a cash transfer via their bKash mobile money from WFP in July, whereas 2,388 control households reported no transfer, but had access to a mobile money account at the time of the survey. Nearly all control households (97.4%) reported having an active bKash account. This suggests that they did not receive the transfer, because they may not have been reached during beneficiary identification. Table 1 summarises the number of households targeted by WFP and surveyed, by treatment status and transfer date.

Table 1: Sample size, by transfer date and treatment status

	No. of households receiving cash (by transfer date)	No. of households surveyed (by transfer date)
Treatment	**23,434**	**6,566**
14 July	14,345	3,312
15 July	2,903	1,218
16 July	3,384	1,085
30 July	1,036	670
Date not confirmed	0	281
Control	0	**2,388**

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. Treated households reported receiving the cash transfer and having access to a bKash mobile money account by the transfer date, whereas control households had access only to a mobile money account at the time of the survey. A small sample of households (281 households) were unable to confirm the date on which they received the cash transfer.

3.2.2 Sample characteristics and flood experience

Table 2 describes our sample and compares the mean value of time-invariant individual and household characteristics across treatment and control groups. The target population is vulnerable: 97% of intended beneficiaries were female, of whom only 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. We also collected variables measuring the use of technology, since the intervention and the definition of our control group was based on access to mobile money. Around half our sample had used mobile money in the last six months (excluding the transfer from WFP); 80% own a mobile phone and almost all respondents had used a mobile in the last week at the time of the survey.

Table 2: Balance and summary statistics

	Control mean	Treatment mean	Δ	Norm. Diff.	p-value
Individual characteristics					
Age	37.47	38.79	1.32	0.10	0.108
Female respondent	0.97	0.97	0.00	0.00	0.426
Household head	0.18	0.22	0.03	0.08	0.705
Completed primary school	0.34	0.30	-0.04	-0.08	0.576
Household characteristics					
Household size	4.68	4.73	0.05	0.03	0.872
Dependency ratio	0.75	0.76	0.01	0.02	0.282
Raw material house	0.26	0.27	0.01	0.02	0.183
Distance to large water body (m)	1332.73	1249.13	-83.60	-0.06	0.394
Protected mainland	0.43	0.33	-0.09	-0.19	0.470
Unprotected mainland	0.27	0.27	0.00	0.00	0.100
Char land	0.30	0.40	0.09	0.20	0.018
Anticipatory action					
Received WFP cash transfer	0.00	1.00	1.00	.	.
Received dignity kit from UNFPA	0.06	0.14	0.09	0.29	0.000
Received feed or storage from FAO	0.04	0.07	0.03	0.15	0.011
Technology					
Used digital wallet in last six months	0.50	0.47	-0.03	-0.05	0.417
Own mobile	0.82	0.80	-0.02	-0.06	0.633
Uses someone else's mobile	0.16	0.18	0.03	0.07	0.482
Uses mobile at least once a week	0.97	0.96	-0.01	-0.03	0.609
Observations	2388	6566			

Notes: Δ reports the treatment mean minus the control mean. Norm. Diff. reports the normalised difference between the treatment and control group means, following Imbens and Rubin (2015). The last column reports the p-value from ordinary least squares regressions of each variable on the treatment dummy to test equivalence of means, controlling for union fixed effects and clustering standard errors at union level as in our main specification.

Our sample was also heavily affected by the floods, as demonstrated in survey responses. 85% of control households reported flooding at or above floor level, with 5% above roof level. Based on satellite imagery, 30% of the land in local areas where our sample is located was covered in water at the peak of flooding on average.[9] Moreover, the flooding persisted at

[9]Satellite data was used to measure flood intensity at mauza level, the administrative unit below the

above half the local maximum flood extent for an average of 45 days, 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 described in Section 2, the most common date of peak flooding was 22 July for households in our sample. The date of peak flood extent varied across mauzas from mid-July until the beginning of August, according to estimates derived from satellite imagery.

Only 61% 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 lost the same number of small livestock (goats, sheep, and pigs) as they owned at the time of the survey (one animal for every two households); 60% of control households reported loss of poultry, another important productive asset; 46% lost cultivated crops and of these, only half have been able to replant since the flood. Coping strategies were exhausted. Almost a third of control households reported going at least one day without eating any food during the flood, which highlights the extreme poverty and vulnerability of households in our sample.

3.2.3 Balance across treatment and control groups

Households appear well balanced across treatment and control groups.[10] The normalised difference column shows that the largest discrepancy in means is no more than 0.1 times the standard deviation in almost all cases.[11] On average, socio-economic variables and mobile phone use are very similar across groups. Exceptions include the land type beneficiaries live on, and whether they were a recipient of other anticipatory interventions from UN organisations.

We report the *p*-values from ordinary least squares regressions of time-invariant and technology use variables on the treatment dummy to test equivalence of means.[12] We fail to reject at the 10% level in all cases except for the proportion of households on unprotected char land, and those receiving other interventions from the UN. 40% of treatment households are located on unprotected char land, relative to 30% of the control group, and are therefore potentially more exposed to flooding. There was a limited amount of geographic overlap

union or approximately village level.

[10] Appendix Table A1 shows that households receiving cash across the four transfer dates are similar in terms of time-invariant variables.

[11] The normalised difference is strictly defined as the difference in means, divided by the square root of half the sum of treatment and control group variances, following Imbens and Rubin (2015). They use propensity score matching to show that normalised differences of 0.25 or less are well balanced in terms of experimental results but covariate adjustment is unreliable for values of 1 or more.

[12] We control for union fixed effects and cluster for standard errors at union level, as in our main specification.

between the cash transfers and other UN interventions received by a small subset of our sample, but by a higher share of treated households relative to control households; 14% of beneficiaries in our sample received a dignity kit from UNFPA, while 7% received animal feed or food storage from FAO, relative to 6% and 4% of the control group, respectively.

Although treatment and control households are balanced on observable characteristics and their use of mobile money and phones, our sample was not randomised across the two groups. Hence, it is possible that there may be important unobservable differences that limit our ability to attribute the estimated impacts to the cash transfer provided. In order to address this concern, we test the robustness of our results against a number of different ways of defining the treatment and control groups, as discussed in Section 4.3 below.

3.3 Other data used in the analysis

3.3.1 Second round of phone surveys

Between 22 and 28 December 2020, additional phone surveys were conducted as part of a separate evaluation of WFP's post-flood cash transfers. Some of the surveys were conducted in unions that had been excluded from the post-flood cash transfer, because these were deemed to have experienced less flooding. In these unions, 1,291 surveys were conducted with households consistent with our definitions of treatment and control groups. These households are located in only 64 out of the 111 unions covered in the first round of data collection. Given that only a small number of treated households were surveyed in both rounds of data collection, we treat these two rounds as cross-sectional data.[13] This survey provides additional, although limited, data for our analysis.

Appendix Table A2 compares household characteristics across the treatment and control groups in the smaller second round of data collection. Households appear well balanced and we fail to reject equivalent means at the 10% level in all cases except digital wallet use, which is relatively more frequent in the control group.[14]. Moreover, the follow-up sample is broadly representative of households contacted during the first survey. Of 1,291 households in this sample, more than half were included in the initial evaluation.[15] We also confirm that other sources of cash assistance were very limited. In the second survey, only 5% of treated

[13]Of the 1,291 relevant households in the second round of data collection, 93% of 659 control households were contacted in the first survey, but only 32% of the 632 treated households.

[14]61% of control group respondents have used mobile money in the last six months, compared to 49% of the treatment group (p=0.030)

[15]As in the first survey, the average respondent is female, 37–38 years old, and lives in a household of five with two or three dependents. Just over one third completed primary school, one quarter live in the most vulnerable housing structure, and almost all have used a mobile phone in the last week.

households and 6% of the control group reported receiving cash from a source other than WFP since the flooding began.[16]

3.3.2 Satellite data

We complement survey data with satellite imagery to estimate objective measures of flood timing and severity, given that self-reported flood exposure is a weak proxy for true flood exposure (Guiteras *et al.*, 2015). 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 SAR imagery, providing images with 10-metre spatial resolution. In light of its ability to capture information even in the presence of cloud cover, Sentinel-1 SAR data has been frequently applied to flood mapping, including in Bangladesh (Uddin *et al.*, 2019; Singha *et al.*, 2020). Water bodies are identified from their dark appearance. We use a change detection and thresholding approach to identify flooded areas, following a methodology developed by the UN-SPIDER Knowledge Portal. This methodology entails comparing pixel intensity and the degree of change between before-flooding and after-flooding images for the area of interest, with the baseline constructed by using an average of all images from the previous four years.

Using this approach, we estimate the flooding extent by area covered at the mauza level. Furthermore, as the temporal frequency of the Sentinel-1 imagery can be up to 12 days between images, we estimate flooding extent at daily intervals by fitting the Sentinel-1 data points to a Gaussian function, thereby enabling us to identify the estimated date of flood peak (or maximum extent) for each mauza.[17] We validate these estimates against four different external data sources: (1) river discharge measurements from GloFAS and water level measurements from the Flood Forecast and Warning Centre at four points along the Jamuna River; (2) perceived flooding extent from key informants on the ground in 20 unions; (3) optical satellite imagery from Sentinel-2; and (4) satellite flood extent data from UNOSAT for 21 July. Compared to gauging station data, satellite estimates obscure multiple flood peaks occurring in a short period, but overall trends in flooding are very similar. In unions with key informants, perceived flood trends show general agreement, and in many cases flood magnitudes are also similar. Comparison to optical satellite imagery also shows a high degree of overlap with visible surface water.

[16] Almost all these other cash transfers were almost all received from NGOs and government organisations; 80% of the other cash transfers reported were received within 12 weeks of the flood, i.e., the time of the first survey.

[17] 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.

3.4 Outcome measures

In constructing our outcome measures, we follow a detailed pre-analysis plan, registered in the American Economic Association Registry.[18] We collect all data via phone surveys and therefore our measures are limited by this format. Appendix Tables A3 and A4 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, given that 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; Haushofer and Shapiro, 2016; N. D. Jensen *et al.*, 2017; Asfaw *et al.*, 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. The adult food consumption index is a measure of quality, rather than quantity, of nutritional intake. The first component captures the number of days during which expensive protein (meat, fish, or eggs) were consumed by any household member in the seven days prior to the phone survey. The second component is the food consumption score (FCS), a measure of general nutrient intake calculated using the frequency of consumption of different food groups in the week prior to the survey, on a scale of 0–112. We exclude rice from the calculation of this measure, as more than 95% of households reported eating rice every day in the past week.

As the last welfare outcome, we measure wellbeing using a 10-scale Cantril's ladder of life satisfaction.[19] Flood shocks are likely to be extremely distressing events and the existing

[18] `https://www.socialscienceregistry.org/trials/6576`

[19] We pre-specified that the wellbeing index would combine with Cantril's ladder of life satisfaction and self-reported hours slept the previous night, as a shorthand proxy for psychological distress given the phone survey medium for data collection. We re-evaluated this after a review of the relevant economics and psychology literature revealed that sleep outcomes are extremely challenging to impact through one-off interventions and may be a poor measure of psychological wellbeing. While lab experiments in high-income countries have shown benefits from temporarily increased sleep duration on mental, psychological, and physical outcomes (Lim and Dinges, 2010; Banks and Dinges, 2007), sleep interventions in field settings have produced limited results at best, especially in low-income contexts. For instance, a targeted three-week sleep intervention providing information, encouragement, and tools to enhance the sleep environment increased nightly sleep by only 27 minutes in urban India, as measured using state-of-the-art technology (Bessone *et al.*, 2020). Moreover, none of the expected effects on economic outcomes materialised as a result of increased sleep duration, including cognition, productivity, decision making, and even wellbeing. Bessone *et al.* (2020) also highlight the importance of measuring sleep using objective measures, as self-reported measures of sleep often fail to capture true sleep efficiency (Castro *et al.*, 2013; Schokman *et al.*, 2018). Hence, it is highly unlikely that our cash treatment – a small indirect intervention – will impact self-reported hours of sleep, so we exclude this measure from the index. Nonetheless, our results remain robust to correcting for multiple hypothesis testing when we include an additional outcome for self-reported hours of sleep as another independent hypothesis.

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 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. Our focus on measuring pre-emptive actions is motivated by the evidence showing that actions taken before floods can limit asset loss and damage by up to 50%, largely due to effective warning systems (Carsell *et al.*, 2004; Kreibich *et al.*, 2005; Thieken *et al.*, 2007). Like early warning systems, cash was delivered in anticipation of the flood shock to a large proportion of our sample. In the absence of a standard pre-emptive actions measures, we employ a relatively crude index that measure the number of actions taken to prepare for flooding in mid-July before the flood peak, including purchasing food, evacuating, or reinforcing walls, among others.

We construct our asset loss index by combining the number of livestock that died in the two months following the flood peak, the number of asset categories that were lost or damaged due to the flood (out of a list of 15), and the area of cultivated crops lost in decimals (1 decimal $\approx$ 40 sq metres) due to flooding. The costly 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 use a dummy variable for whether a household received remittances to construct the standardised remittance outcome, noting that only 9% of households reported receiving any remittances when we reviewed preparatory data blind to treatment status and amended the pre-analysis plan accordingly. Lastly, the earning potential index is constructed by combining a dummy variable for whether a household did not lose crops from flooding or was able to replant, and the number of paid hours of work per adult in the week prior to the survey.

We standardise all variables for comparability, following Kling *et al.* (2007) in constructing indices. In particular, we first ensure that all variables are consistently signed within a particular index. We then sum individual response items within each scale, before standardising the summed scale 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.

Variables for land type (char land, unprotected mainland or protected mainland) 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.

4 Empirical strategy

4.1 Three empirical specifications

4.1.1 The average treatment effect of cash

We first estimate the average treatment effect of the cash on a variety of outcomes by using the following empirical model:

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

where Y_i is the outcome of interest for household i and T_i is a dummy variable indicating whether a household received an anticipatory cash transfer from WFP, pooling across all transfer dates. X_i is a vector of pre-specified controls to increase precision in our estimates and control for observable imbalances across treatment groups. These include gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type (flood exposure). We control for the union, as our geographic fixed effect (η_i). ε_i is a mean zero error term. We estimate robust standard errors to correct for heteroskedasticity and cluster at the union level, our sampling frame.

4.1.2 Heterogeneity analysis by timing

In addition to exploring the impact of an anticipatory cash transfer relative to the control group, we examine whether the impact varies by timing and land type. Our second specification assesses whether an earlier timing of the cash transfer relative to the flood shock matter for household outcomes. Within mauzas, households received cash on different dates in July 2020. This variation was owing to the large number of transfers that needed to be conducted in a short period of time and the fact that some households were contacted later and/or given the opportunity to activate bKash accounts. Approximately 50% of our sample received cash on 14 July, with the rest receiving cash on 15, 16 and 30 July. We compare the date of cash transfers to the the local flood peak estimated at the mauza level using satellite data. We estimate the average treatment effect of receiving cash centred on the flood peak date and the marginal welfare effect of receiving the cash one day earlier, as follows:

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

where T_i is still a dummy variable indicating whether a household i received a cash transfer from WFP and β_1 captures the average treatment effect of the cash transfer centred on the local flood peak date in comparison to the control group. D_i indicates the number of days between the cash transfer date and the local flood peak date, with negative values indicating when households received cash after the flood peak. β_2 captures the marginal effect of receiving cash one day earlier. For every outcome, we test the null hypothesis that an earlier cash transfer has no impact. We control for mauza fixed effects in this specification to reduce any confounds between flood timing and other spatial characteristics. We also control for the survey date, given that control households were interviewed a few days later than treated households on average. We check that our results are robust to instrumenting the number of days between cash transfer and local flood peak with transfer dates and excluding households which received cash on 30 July.

4.1.3 Heterogeneity analysis by land type

Lastly, we explore whether the impacts of the cash transfers vary for households living on different types of land.[20] Land type captures at least three important dimensions of initial vulnerability to the flood shock: (1) exposure to flooding; (2) socio-economic status; and (3) market isolation. Moreover, land type is easily observed and hence a useful tool for targeting the intervention.

We split the sample into three land types due to their unique features: (1) unprotected char land, which includes low-lying islands along the course of the Jamuna River; (2) unprotected mainland located outside existing flood embankments; and (3) protected mainland. Char lands are formed by silt deposits and the flat, fertile soil makes them attractive for farming. However, they are also extremely vulnerable to flooding and erosion, and they are more isolated from markets and services. More vulnerable transient households typically settle on char lands ('char households'). Unprotected mainlands are located outside flood embankments and are also highly exposed to flooding as a result ('unprotected households'). In contrast, households located on protected mainland – the third land type – are within flood embankments and are less exposed to floods ('protected households'). Roughly a third of the sample is located on each land type, but as noted in Section 3.2, there is a greater proportion of treated households relative to control households on char land.

To conduct the heterogeneity analysis, we estimate a fully interacted model, using unprotected households as the base category. Building on specification (1), we also include dummies for char households (L_{1i}) and protected households (L_{2i}), and their interaction

[20]We specified land type as the relevant dimension of heterogeneity in our pre-analysis plan.

with treatment status ($T_i * L_{1i}$ and $T_i * L_{2i}$):

$$Y_i = \beta_0 + \beta_1 \cdot T_i + \beta_2 \cdot L_{1i} + \beta_3 \cdot T_i * L_{1i} + \beta_4 \cdot L_{2i} + \beta_5 \cdot T_i * L_{2i} + \gamma \cdot X_i + \delta \cdot \eta_i + \varepsilon_i \quad (3)$$

where β_3 captures the additional average treatment effect for char households, and where β_5 captures the additional average treatment effect for protected households, relative to unprotected households.

4.2 Approach to inference

In addition to presenting p-values from the Wald test, we correct for multiple hypothesis testing across our 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 by Anderson (2008).

In the Appendix, we also present p-values from wild bootstrap inference with clustering at the union level to account for differences in cluster size, which can bias standard cluster inference even with many clusters (Mackinnon and M. D. Webb, 2017). There is an uneven distribution of households across the 111 unions, because the WFP was restricted in its targeting efforts by the limited availability of households on the pre-existing beneficiary lists.

4.3 Robustness checks

We check that our results are robust to alternative definitions of the control group and model specifications in Section 7. First, we show that our results are robust to mobile money use by splitting the sample into households that have previously used a mobile money account and households with a new account (38% of the sample). Treated households were required to have access to an active bKash mobile money account to receive the intervention. All control households reported having access to an active mobile money account at the time of survey, and 97.4% reported having access to an active bKash account.[21] Although no differences in average mobile money usage are observed between control and treatment households, it is important to ensure that our results are robust to any unobserved or nonlinear differences in mobile money usage that may be present.

For similar reasons, we also check that our results are robust to the exclusion of house-

[21]In the pre-analysis plan, we proposed testing the robustness of our results by excluding control households that did not have access to an active bKash account. However, this turned out to be only 63 households at the time of the survey.

holds that received the cash transfer on 30 July. These households were targeted at a later date, in part because they were given the opportunity to reactivate or set up new bKash accounts, although some were also contacted later. We observe that households that received cash on 30 July were significantly less likely to have used a mobile money account in the six months prior to the survey (38% vs 48% of households receiving cash on 14–16 July; p=0.000).

In addition, we check for robustness to the use of more conservative geographic fixed effects and the exclusion of outliers and households that also received assistance from other UN bodies (UNFPA and FAO). Furthermore, we exclude covariates and control for various measures of flood intensity (estimated at the mauza level from satellite imagery).

Lastly, given the severity of the flooding and the associated asset loss and displacement, we expected low response rates. We sampled a large number of treated and control households as a result, with replacement. However, given that control households were less likely to be contacted by phone, we expected a higher replacement rate for the control group relative to treated households. In total, we contacted 7,819 treated households and reached 6,670, whereas we contacted 4,459 control households and reached 2,460. In Section 7.3, we provide several reasons to believe that the patterns of non-response suggest that our treatment effects are likely to be lower bounds, and we show that our main results are robust to the use of Lee bounds (Lee, 2009).

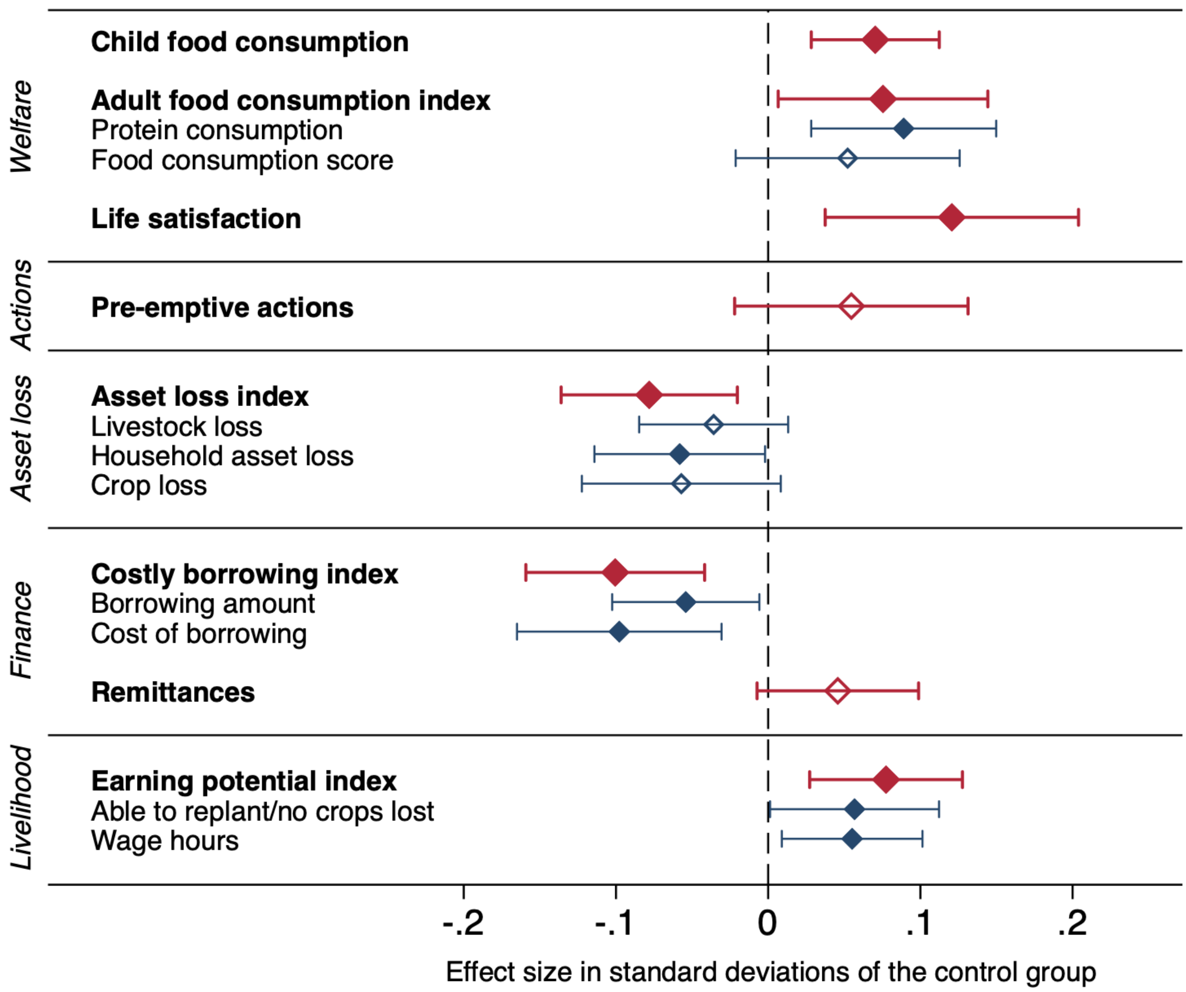
5 Results

5.1 The anticipatory cash transfer improves welfare

The first question of interest is whether a small, one-off cash transfer succeeds in improving welfare. We find that the anticipatory cash transfer significantly improves welfare: child and adult food consumption and well-being are significantly higher for treated households, even when measured three months after the intervention. Figure 3 shows the average effect of the cash transfer on pre-specified, standardised outcomes with 90% confidence intervals. Table 3 reports the p-values and sharpened q-values for the standardised treatment effects and 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 and wild bootstrap inference (see Appendix Table A5).

[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.

Figure 3: Effect of receiving a cash transfer



Notes: Markers indicate the standardised mean treatment effect of receiving the cash transfer on pre-specified outcomes (red) and sub-indices (blue), with 90% confidence intervals shown. Solid markers indicate significance at the 10% level. 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.

Table 3: Treatment effect of receiving anticipatory cash transfer

	Std. treatment effect	Control mean	Δ	% Δ	*p*-value	*q*-value	N
Children consumed three meals (0/1)	0.070***	0.80	0.03	+3.8%	0.006	0.027	7563
Adult food consumption index	0.075*				0.072	0.046	8951
Days protein products consumed (0–7)	0.089**	2.64	0.18	+6.8%	0.017		8951
Food consumption score (0–112)	0.052	39.21	0.82	+2.1%	0.242		8951
Life satisfaction (0–10)	0.121**	2.08	0.26	+12.5%	0.018	0.028	8941
Number of pre-emptive actions (0–6)	0.055	0.95	0.06	+6.3%	0.237	0.099	8947
Asset loss index	-0.078**				0.027	0.033	8950
Number of livestock died in last two months	-0.036	0.59	-0.04	-6.8%	0.228		8950
Asset categories lost/damaged (0–15)	-0.058*	1.30	-0.07	-5.4%	0.090		8950
Area of cultivated crops lost (decimals)	-0.057	15.58	-1.87	-12.0%	0.150		8950
Costly borrowing index	-0.100***				0.005	0.027	6061
Amount borrowed in last two months (BDT)	-0.054*	8927.52	-564.79	-6.3%	0.066		6061
Highest interest rate (%/month)	-0.098**	4.83	-0.56	-11.6%	0.017		6061
Received remittances (0/1)	0.046	0.08	0.01	+12.5%	0.153	0.072	8950
Earning potential index	0.077**				0.012	0.027	8945
No crops lost/able to replant (0/1)	0.057*	0.65	0.03	+4.6%	0.092		8945
Paid hours of work/adult last week	0.055*	9.08	0.60	+6.6%	0.050		8945

Notes: The first column shows the standardised mean treatment effect for pre-specified outcomes (bold) and sub-indices. The second column shows the control mean, followed by the non-standardised treatment effect (Δ) and percentage change relative to the control mean (% Δ). p-values are reported on all outcomes and sub-indices, with standard errors clustered at union level. 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. Includes union fixed effects. * p<0.10, ** p<0.05, *** p<0.01

Children in treated households are 0.07 standard deviations or three percentage points more likely (p=0.006) to have consumed three or more meals in the day prior to the survey, relative to a control mean of 80%. The effect remains highly significant across multiple robustness checks (see Section 7) and to defining the outcome as the number of meals consumed. The importance of improved child food consumption months after the flood shock is salient, in light of a large body of literature highlighting the negative effects of short-term disruptions in nutritional intake for children during droughts on long-term educational, earnings and health outcomes – even decades later (Chen and Zhou, 2007; Maccini and Yang, 2009; Dercon and Porter, 2014; Dinkelman, 2017; Victora *et al.*, 2021).

Similarly, adult food consumption is 0.075 standard deviations higher (p=0.072) for treated households relative to control households. The treatment effect on the adult food consumption index is largely driven by a 6.8% increase (p=0.017) in the number of days during which more expensive forms of protein (meat, fish, or eggs) were consumed in the week prior to the survey, relative to a control mean of 2.6 days. The persistent effects in food consumption three months after the intervention is consistent with other studies that show longer-term improvements in food security from (albeit much larger) cash transfers (Haushofer and Shapiro, 2016; Haushofer and Shapiro, 2018).

These findings are consistent with the recalled number of days without eating during the flood shock, in data collected for a subset of unions during the second round of phone surveys. Results show that treated households were 10 percentage points less likely to go a day without eating during the flood (p=0.027), relative to 28% of control households. On average, they endured half the number of days without eating (p=0.011), compared to a control mean of 0.62 days per household (see Section 8 and Figure A1). These results are consistent with the reported use of the cash transfer: treated households mostly reported spending the cash on food or water (91% of households), followed by medicine or health services (33%), agricultural inputs (26%), loan repayments (12%), clothing (11%), and repairs on home or assets (11%).[23] However, since the modest transfer is approximately equivalent to only 2 weeks of food expenditure, finding effects after 10-12 weeks points to other possible mechanisms discussed in Section 5.2. Results strongly suggest that the treatment effects that we estimate on food consumption three months after the intervention are a lower bound compared to the size of the treatment effects during the shock itself.

Control households report an extraordinarily low level of wellbeing at the time of the survey, with a mean of 2 on a 10-item Cantril's ladder of life satisfaction, evidencing the distressing nature of flood shocks. We find that the cash transfer helps mitigate the flood's impact on general wellbeing: at the time of the survey, average life satisfaction for the treated households was 0.12 standard deviations or 12.5% higher (p=0.018) than in the control group. However, we note that even with this increase, this is an extremely low level of well-being.

5.2 Mechanisms through which welfare is improved

The improvements to welfare suggest that the cash transfer served to smooth a negative income shock created by disruptions to income generation activities and the destruction to private assets. In this section, we show that the cash transfer also significantly mitigated asset loss and costly borrowing, even three months after the flood shock. These effects could arise due to actions taken either before, during or after the flood shock. While collecting

[23] Consistent with these results, the WFP Monitoring and Evaluation report found that treated households spent approximately 67% of the cash (approximately BDT 2000) on meeting food needs, followed by recovery (46%), preparedness actions (28%) and health (23%)(WFP, 2020).

data via a phone survey two to three months after the intervention limits the granularity with which we can disentangle the timing of behaviours, our results suggest that anticipatory cash bolstered existing coping strategies, rather than expanding the choice set of strategies.

Just over half (53%) of control households took action prior to the flood to reduce its impacts, with a control mean of one action per household. Treated households did not take significantly more actions to prepare for flooding (see Figure 3), nor did they pursue different strategies, with the exception of evacuation. We acknowledge that our pre-specified measure of preventative actions is blunt. Exploring further, we observe that treated households were three percentage points more likely to evacuate both household members (p=0.032; control mean of 29%) and livestock (p=0.026; control mean of 17%), after controlling for covariates and union fixed effects. At the margin, we find that treated households were four percentage points more likely (p=0.051) to take some action in preparation for the flood peak.[24]

Although little difference in preventative actions was recorded, we find that the cash transfer was effective in mitigating asset loss during the shock and in the two months after. The cash transfer decreased asset loss and damage by 0.078 standard deviations (p=0.027) relative to the control group. The sub-indices of livestock, household items and crop loss are themselves only marginally significance at conventional levels, but in combination, they produce an asset loss index that is statistically significant in the treatment group at the 5% level. We find that treated households were 2.3 percentage points less likely (p=0.097) to lose small livestock after flooding and 3.2 percentage points less likely (p=0.082) to lose any poultry during flooding, controlling for covariates and union fixed effects.[25] Lower mortality rates of small livestock and poultry (important productive assets) may explain why households benefited from higher food consumption three months after the intervention, especially on the protein-specific sub-index. Our findings are consistent with other literature showing that evacuation in advance of flooding can substantially reduce damages elsewhere by enabling households to remove possessions, evacuate poultry and farm animals, move agricultural equipment, seeds and partially harvest crops (Carsell *et al.*, 2004; Subbiah *et al.*, 2008).

Households have high liquidity needs during and in the months following the flood shock. Almost three in four control households borrowed money between the flood and survey, bor-

[24]The treatment effects for evacuation and taking any action are reported in Appendix Table A6.

[25]The results for livestock mortality are reported in Appendix Table A6. Treated households own 17.2% more (p=0.001) 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. We do not identify this difference in livestock ownership as a result of receiving cash, since we cannot control for baseline livestock assets. However, higher livestock ownership in the treatment group at baseline would only reinforce the significance of lower livestock mortality that we find among recipients of the cash transfer. Unsurprisingly, we find no differences for larger livestock (cows, calves, and buffalo), which are owned by far fewer households.

rowing an average amount of BDT 8,928 (approximately \$105). Notably, almost a third of the sample borrowed more than BDT 10,000 (approximately \$118), double the amount of the cash transfer. However, conditional on borrowing, those receiving the cash transfer borrowed less and more cheaply than control households. The pre-specified costly borrowing index was 0.1 standard deviations lower (p=0.005) on average for treated households relative to the control group. This result remains highly significant correcting for multiple hypothesis testing and across robustness checks (see Section 7). On the extensive margin, treated households were also 2.4 percentage points less likely (p=0.082) to borrow relative to a control mean of 70% (Appendix Table A6).[26] Conditional on borrowing, treated households borrowed lower amounts at cheaper interest rates. We find they borrowed BDT 565 (approximately \$7) less on average in the two months after the onset of the flood if they borrowed at all. Treated borrowers accessed credit at an interest rate that was 0.57 percentage points lower than the control mean of 4.83% per month, and were 10% more likely (p=0.008) to borrow at a zero interest rate (from informal sources), reducing the burden of future indebtedness. At the same time, treated households also reported borrowing food or receiving help from others on 14% fewer days (p<0.001) in the week before the survey relative to control households.

There is no evidence that the cash transfer crowds out remittances on average. Treated households were 12.5% more likely to report receiving remittances on average (a one percentage point increase relative to a control mean of 8%), although this result is not statistically significant with a p-value of 0.155. The absence of crowding-out effects assuages fears that the cash transfer could replace informal support networks (Cox, 1987; Cox and Jakubson, 1995; R. T. Jensen, 20043; Juarez, 2009).[27]

Thus far, the results point to modest increases in food consumption and well-being, and lower levels of asset loss and costly borrowing. We also explore whether households receiving the cash transfer appear to be recovering faster. We find that treated households reported a 0.077 standard deviation increase (p=0.012) in the pre-specified earning potential index, relative to the control households. Treated households are 3.9 percentage points more likely (p=0.001) to have a household member working for remuneration (see Appendix Table A6), and the average hours worked for remuneration per adult were 6.6% higher (p=0.050), equivalent to an additional 36 minutes per week. The cash transfer appears to be playing a small but not insignificant role in post-flood recovery, helping to restore livelihoods and the

[26] The WFP Monitoring and Evaluation report found that loans were largely obtained to meet food needs during the crisis period (40% of households), providing a direct link between the role of the cash transfer, reduction in food insecurity and costly borrowing (WFP, 2020).

[27] For ultra-poor households in our sample, zero-interest borrowing from informal networks seems to be more important than remittances to cope with the shock, whereas fewer than one in 10 households reported receiving remittances. We found that treated households had greater access to these informal loans.

capacity to cope with future shocks.

We cannot determine precisely what proportion of the impact of the transfer arises from the injection of liquidity at a critical juncture compared to the early warning (information) aspect of the transfers. However, evidence strongly points to the primary importance of liquidity. First, households receiving cash were no more likely to report receiving early warning. Second, control households that did evacuate (they had warning but not extra liquidity from cash transfer) were not unambiguously better off than those that did not. Third, control households that bought food to prepare (having liquidity to do so) had significantly higher welfare than those that did not. Lastly, the fact that food is the most common expenditure (91% of households) and the type of pre-emptive actions were mostly similar across treated and control households points to liquidity as the dominant factor in households' ability to cope.

Overall, the treatment effects of the cash transfer are statistically significant, but small in magnitude. The size of these effects is unsurprising, considering that the amount of the one-off cash transfer was very modest, approximately two weeks' worth of food expenditure during floods lasting more than one month. The level of borrowing sheds light on the liquidity needs of households during this period. Instead, what should be striking is that we do measure significant effects across such a wide range of outcomes three months after the intervention, highlighting the effectiveness of a timely transfer.

6 Heterogeneity analysis

6.1 Timing of cash transfers

Although we are unable to directly compare anticipatory action to a traditional response, the welfare benefits we report were all recorded before the arrival of a traditional response. Without cash, comparable ultra-poor households experienced lower food consumption (both adults and children), lower wellbeing, higher asset loss, higher borrowing, and less employment in the period before a traditional response. Some of these deficits, even when temporary, are known to have permanent scarring effects – for example, child undernutrition Victora2021 – suggesting that there are significant advantages from acting early. The small cash transfer may set households on a different trajectory by easing liquidity constraints and permitting better decisions at a critical juncture.

To further investigate the importance of speed in implementation, we explore whether small changes in the timing of the cash transfer relative to flooding matter for household welfare. Qualitative evidence suggests that the cash may have been received too late on average for households to take an alternative set of actions in preparation of the flood, but instead strengthened the use of existing strategies (WFP, 2020; Adams *et al.*, 2021).[28] To examine this, we calculate the number of days between receiving a transfer and peak local flooding in 630 mauzas (approximately village level and surrounding land) based on high resolution satellite data. We then investigate the relationship between cash transfer timing and welfare effects using linear regression and non-parametric methods to estimate the marginal effect of receiving cash one day earlier.[29] Figure 4 shows the number of households receiving cash by the number of days between transfer and local flood peak, and the average flood extent corresponding to this timing (negative days indicate receiving cash after the local flood peak). Most households received the cash before the local flood peak. However, dangerous flood levels persisted for several weeks (see also Figure 1), so the flood peak date should not be considered a discrete event marking flood shock impacts. Rather, the flood peak provides a useful reference for measuring the timing of the cash transfer relative to flood dynamics over a large geographic region.

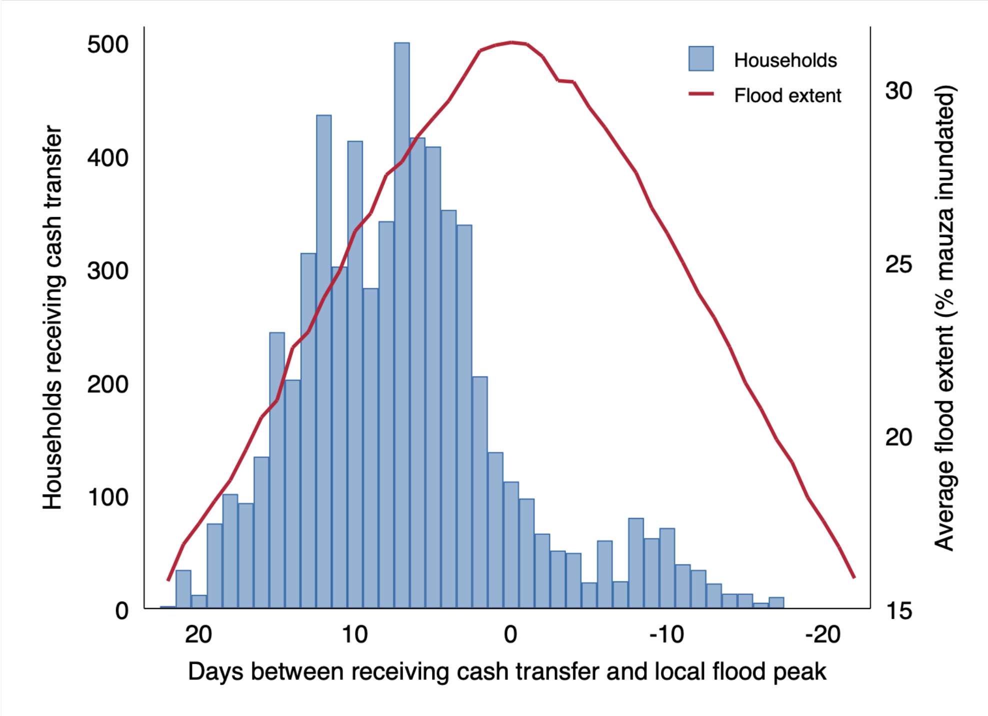
We find evidence to suggest speed is welfare improving, even when considering a cash transfer that is already much earlier than the typical response. Table 4 reports the average effect of receiving cash centred on the flood peak date and the marginal effect of receiving the cash one day earlier, controlling for mauza fixed effects and the date of the survey. First, we observe that the average treatment effects centred on the flood peak date are broadly consistent with our main results in Section 5. When we remove the effect of small changes in timing, the estimated coefficients on the asset loss index and the earning potential index lose significance, but we now estimate a marginally more significant effect on remittances relative to the control group.

Second, we observe that an earlier cash transfer has a stronger effect on adult food consumption. Receiving cash one day earlier increases the adult food consumption index by 0.005 standard deviation ($p<0.05$) when measured 10 to 12 weeks after the intervention.

[28]In particular, households reported that an earlier cash transfer would have been used to stock up on long-life food, drinking water, fuel and medicine; reinforce the roof or walls; take household items to higher ground; and purchase livestock (Adams *et al.*, 2021).

[29]In our pre-analysis plan, we specified that we would analyse the effect of receiving a cash transfer one day earlier using only the transfer dates. We now take advantage of the additional granularity offered by the satellite data and control for the date of the survey, as many control households were surveyed a few days after the treated households. We also pre-specified an alternate model comparing households that received the cash on 14, 15 and 16 July to households that received the transfer on 30 July due to administrative delays. The reason for splitting the sample into these two groups, was that the centrally located Bahadurabad gauging station recorded a flood peak on 17 July. While results are comparable, we are concerned that households that received the cash transfer on 30 July may differ in terms of unobservable characteristics, as some of these households were given the opportunity to reactivate or set up new bKash accounts. Results comparing households receiving cash on 15-16 July to those receiving on 30 July are reported in Appendix Table A7.

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



Notes: The number of households receiving the transfer is shown on the left-hand axis for each day relative to the date of local flood peak (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.

Linear extrapolation would suggest that the average effect of cash dissipates entirely if the cash transfer is not received within 12 days of the local flood peak. This 'back-of-the-envelope' calculation implies that potential welfare gains from emergency cash transfers are forfeited by delaying assistance. There are no other significant welfare improvements from receiving cash a day earlier at the time of the survey. Among mechanistic outcomes, households receiving the cash transfer one day earlier also had a 0.009 standard deviation higher costly borrowing index (p<0.01).[30] This result could partly be explained by the timing of the later cash transfers relative to an important religious fesitval: cash received after peak flooding on 30 July may have substituted borrowing during Eid al-Adha 2020 commencing on 31 July, when purchasing gifts and sacrificing livestock are a norm, whereas cash received earlier had already been spent. Overall, these results suggest that an earlier cash transfer may improve welfare, but a greater variation in timing is required in order to

[30] Among secondary outcomes, households receiving an earlier transfer are more likely to evacuate, but less likely to replant lost crops (Appendix Table A8).

disentangle these effects.

Table 4: Treatment effect of receiving anticipatory cash transfer one day earlier

	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Transfer	0.128***	0.090**	0.114**	0.064	-0.064	-0.121***	0.081*	0.044
	(0.038)	(0.037)	(0.050)	(0.041)	(0.039)	(0.043)	(0.043)	(0.039)
Transfer × days before flood peak	0.001	0.005**	-0.002	0.003	0.003	0.009***	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mauza fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7023	8353	8342	8349	8352	5605	8352	8348
R^2	0.10	0.16	0.17	0.19	0.20	0.17	0.11	0.18

Notes: The standardised mean treatment effect, centred on the date of the flood peak, is shown in the first row. The second row shows the marginal treatment effect of receiving the transfer a day earlier relative to the flood peak. In addition to the covariates used in the main analysis, we also control for interview date. Mauza fixed effects are included. Standard errors are clustered at mauza level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

To address the concern that variation in the timing of floods across space might be confounded with other spatial differences, we show equivalent results when instrumenting on whether households received cash on the exogenously-determined transfer dates (14, 15, or 16 July) using union fixed effects (see Appendix Table A9). However, these results are not robust to dropping households receiving cash on 30 July, which are driving most of the variation observed above (see Appendix Table A10). It is unclear whether results can be attributed fully to timing or may be due to the fact that households receiving cash on 30 July were slightly different, given their less frequent use of mobile money accounts.

We further investigate the relationship between the timing of the cash transfer and welfare effects using non-parametric methods. As illustrated by Figure 5, we estimate the treatment effect for households receiving cash within a 3-day window and use a standard non-parametric method to trace the treatment effect across the flood timeline, where day 0 indicates the flood peak. We restrict our analysis to a 21-day window (15 days prior to the flood peak and 5 days after the flood peak) and show the number of households receiving cash on each day for reference. In addition to controlling for fixed household characteristics, mobile wallet use and union effects as in the main specification, we also control for interview date, land type, flood magnitude and flood duration.

We find further evidence that a faster response relative to the evolution of the flood is welfare improving. The non-parametric analysis suggests that the anticipatory cash transfer is most effective in boosting child and adult food consumption when received 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. Results are robust to varying the bandwidth and a range of alternative methods.[31]

6.2 Exposure to flooding

We explore whether the impacts of the cash transfers are different for households living on different types of land. As noted above, land type captures at least three important dimensions of vulnerability in flood affected areas: (1) exposure to flooding; (2) socio-economic status; and (3) market isolation. Overall, we find the strongest treatment effects for households living on land that is more exposed to the flood and more vulnerable, both in terms of socio-economic status and isolation. In contrast, the effects of cash for more protected households are smaller on almost all outcomes of interest.

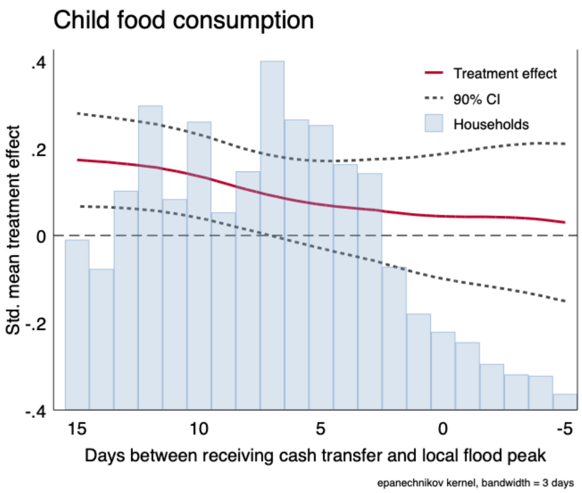
Flood intensity data estimated from satellite imagery confirms that flood waters covered a greater number of mauzas categorised as unprotected or char and for a longer duration than was the case for protected mauzas. Survey data that shows households on protected mainland have a lower dependency ratio, are smaller in size, and are less likely to live in vulnerable housing structures made from raw materials. The distinction between char households and those on unprotected mainland is not as clear on these observable characteristics. Table A11 compares the mean value of time invariant characteristics, flood intensity measures and treatment status across land types.

Table 5 presents regression results; here, we interact treatment status by land type and control for land type dummies. Households on unprotected land form the base category. Across many primary outcomes, the base results are consistent with those presented in Section 5.1: unprotected households receiving transfers had higher child and adult food consumption, higher life satisfaction, and lower asset loss, relative to their control group. However, there was no impact for these households on costly borrowing, remittances or earning potential.

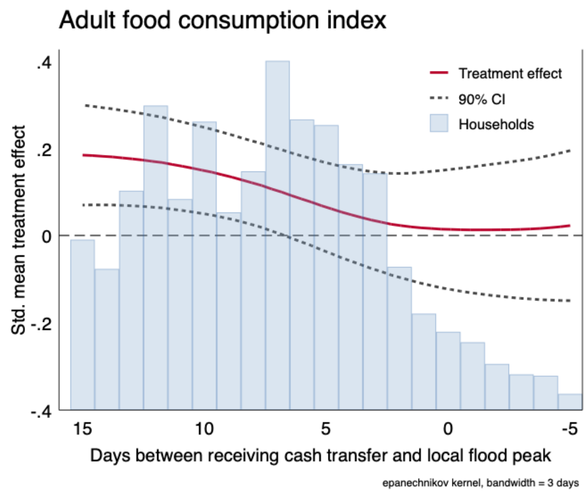
We then compare the treatment effects for char versus unprotected households. Relative to unprotected households, char households that received the transfer have significantly lower adult food consumption and life satisfaction, to the extent that there is no treatment effect of the transfer on these outcomes for char households (p=0.041 and p=0.020 respectively). We cannot rule out important effects of cash on adult food security or welfare during the flood, but that any impacts on these dimensions of welfare did not persist 10 to 12 weeks

[31] Results are robust to restricting the sample to: (i) similar transfer dates (14-16 July) by excluding 30 July transfers; (ii) more comparable flood experiences by excluding households on protected land, i.e. behind an embankment; and (iii) more similar mobile money users by excluding new subscribers.

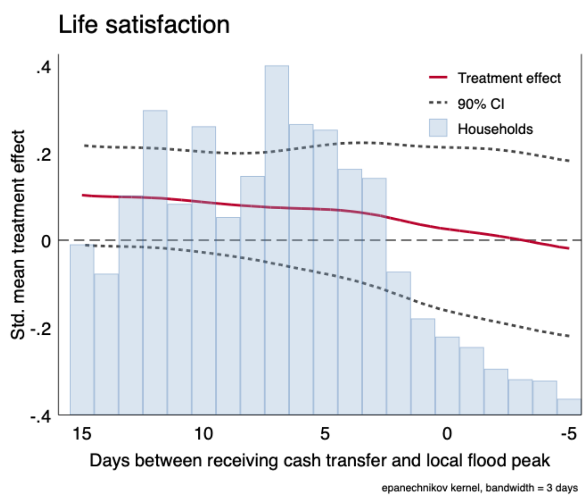
Figure 5: Non-parametric analysis on the timing of the cash transfer



(a) Child food consumption



(b) Adult food consumption



(c) Life satisfaction

Notes: The figure illustrates the non-parametric analysis across our three primary outcomes of interest: child food consumption, adult food consumption and life satisfaction. We employ an Epanechnikov kernel and a bandwidth size of three days. We control for fixed household characteristics, mobile wallet use and union effects as in the main specification. We also control for interview date, and mauza-level land type, flood magnitude and flood duration. Standard errors are clustered at union level. 90% confidence intervals are shown.

Table 5: Treatment effect of receiving anticipatory cash transfer 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) Costly borrowing index	(7) Remit- tances	(8) Earning potential index
Transfer	0.092*	0.154**	0.214***	0.078	-0.152**	-0.014	0.053	0.054
	(0.048)	(0.067)	(0.067)	(0.065)	(0.068)	(0.047)	(0.045)	(0.054)
Char	-0.038	-0.036	0.084	-0.018	-0.039	0.072	-0.020	-0.121
	(0.083)	(0.071)	(0.085)	(0.112)	(0.089)	(0.066)	(0.083)	(0.077)
Transfer × char	-0.018	-0.155**	-0.223**	-0.004	0.088	-0.081	0.032	0.059
	(0.074)	(0.075)	(0.094)	(0.093)	(0.092)	(0.062)	(0.066)	(0.074)
Protected	0.024	-0.031	0.050	0.018	-0.169*	0.189***	0.117	0.016
	(0.085)	(0.083)	(0.084)	(0.112)	(0.087)	(0.063)	(0.096)	(0.077)
Transfer × protected	-0.046	-0.065	-0.034	-0.067	0.129	-0.175**	-0.060	0.006
	(0.063)	(0.094)	(0.108)	(0.110)	(0.079)	(0.078)	(0.070)	(0.080)
Treat effect: Char	0.074	-0.002	-0.009	0.073	-0.064	-0.094**	0.085*	0.113**
Treat effect: Protected	0.045	0.089	0.180**	0.011	-0.023	-0.189***	-0.007	0.060
p-value: Char=Prot.	0.671	0.268	0.018	0.539	0.549	0.197	0.167	0.439
F-test p-value:								
Control $\Delta = 0$	0.729	0.864	0.601	0.949	0.107	0.011	0.233	0.234
Treatment effect $\Delta = 0$	0.754	0.107	0.012	0.794	0.264	0.082	0.380	0.634
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7563	8951	8941	8947	8950	6061	8950	8945
R^2	0.04	0.09	0.10	0.09	0.13	0.09	0.04	0.11

Notes: The baseline is for households on unprotected land. The standardised mean treatment effect of receiving the transfer interacted with land type are shown, with standard errors clustered at union level in parentheses. The second panel shows the treatment effect of receiving cash on for char households and protected households (relative to their corresponding control household), and p-values from a test of equivalence. The third panel shows p-values from an F-test of equivalent outcomes over the three land types for control households and treatment households. 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

after flooding. We cannot distinguish between impacts on char households and unprotected households for other primary outcomes. Compared to the control group, the cash transfer significantly decreases the amount and the cost of borrowing for char households, increases the probability of receiving remittances, and improves earning potential (p=0.036, p=0.069 and p=0.020 respectively). Char households are 13% more likely to have a household member working for remuneration (p<0.001) if they received the transfer relative to a control household on char land. The treatment effect of cash on earning potential is significantly higher for char households than for protected or unprotected households (p=0.028; p=0.022). In the control group, char households are significantly less likely to work for a wage than control households on other land types, suggestive of lower market integration and barriers to labour force participation on char land that the transfer may help overcome (results reported in Appendix Table A12).

The effect of cash on protected households is smaller on almost all outcomes relative to unprotected households, but none of these differences is statistically significant, with the exception of lower costly borrowing. This suggests the cash transfer had fewer welfare impacts on less poor and less exposed households, with implications for targeting limited resources.

7 Robustness to model specification and sample choice

Our results are robust to mobile money use and a wide range of alternative alternative model specifications. In Section 7.1, we check for robustness to different types of mobile money users, given that this plays a role in how our control group is defined. In Section 7.2, we check for robustness to alternative use of fixed effects, sub-samples and winsorising. Lastly, in Section 7.3, we correct for differential non-response.

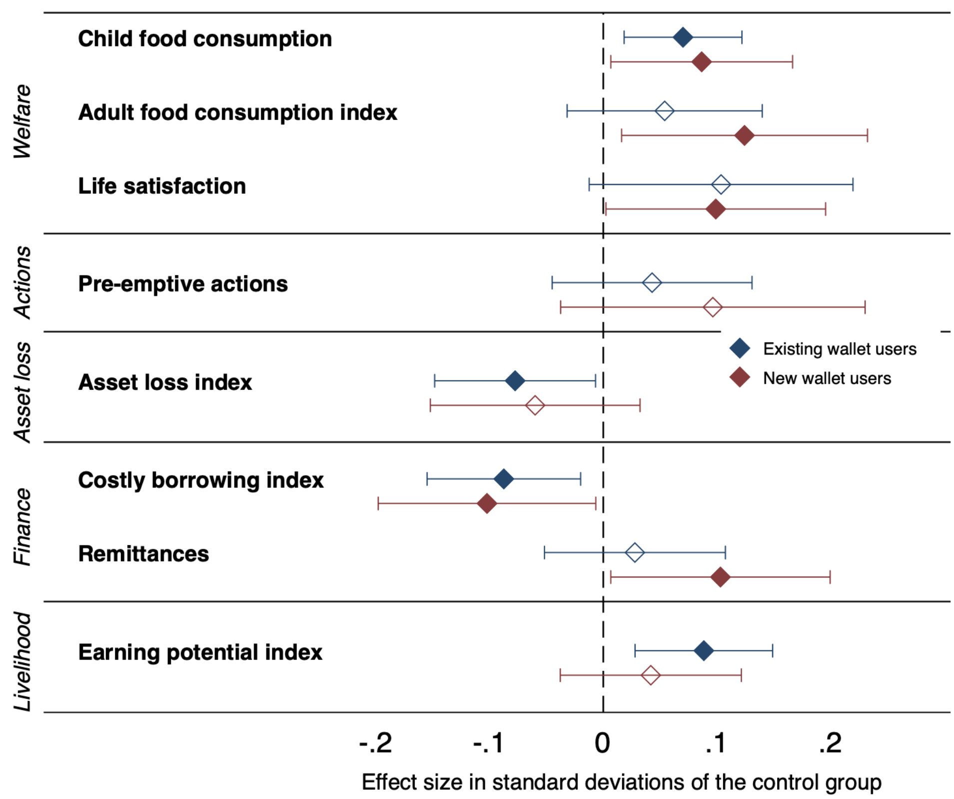
7.1 Robustness to mobile money use

The cash transfer was delivered via mobile money accounts. Although no differences in average mobile money usage are observed between control and treatment households, it is important to ensure that our results are robust to any unobserved or nonlinear differences in mobile money usage between treatment and control households. Therefore, we check whether our results are robust to mobile money use by splitting the sample into households that have previously used a mobile wallet and households with a new account (38% of the sample).[32]

[32] We also check robustness by splitting the sample into three groups: (1) frequent wallet users; (2) infrequent wallet users; and (3) new wallet users. Results are reported in Appendix Table A13. The existing

There is a slightly higher proportion of treated households in the category that reported recently opening a mobile money account, which is unsurprising given the incentive for treated households to set up new mobile money accounts. Figure 6 shows the standardised treatment effects for each sub-sample; we test for differences in the treatment coefficient across models by using the seemingly unrelated regression test (see Appendix Table A14).

Figure 6: Standardised treatment effects on sub-samples by wallet-use



Notes: Markers indicate the standardised mean treatment effect of receiving the cash transfer on pre-specified outcomes in two independent models for existing wallet users (navy) and new wallet users (maroon) respectively. 90% confidence intervals are shown. Solid markers indicate significance at the 10% level. 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.

Overall, our results remain robust to splitting the sample by mobile money use. Statistical power is lower when our sample is split, and some of the results are no longer significant.

wallet user sub-sample is split at the median frequency of wallet use, so frequent wallet users are defined as using mobile money two months prior, and infrequent wallet users as using mobile money more than two months prior. Treated households were asked when they had last used their mobile money accounts to conduct a financial transaction prior to receiving the WFP transfer, whereas control households were asked to report when they last used their mobile money accounts prior to the survey.

Treatment effects for new wallet users remain significant for four of the six outcome variables that were significant for the full sample. For existing wallet users, four results also remain significant. We find no significant difference in treatment effect between these two groups on any outcome. In addition, we find a positive effect on remittances among new wallet users (p=0.079).[33]

We also test the robustness of results to excluding households that signed up to bKash or reactivated an old account after they were contacted by WFP. Most of these households received their transfers on July 30, so this is done by excluding all households that received their transfer on that date. The results are presented in Appendix Table A15. Treatment effects are significant at the 10% level for seven of the eight primary outcomes, including all six that were statistically significant for the full sample.

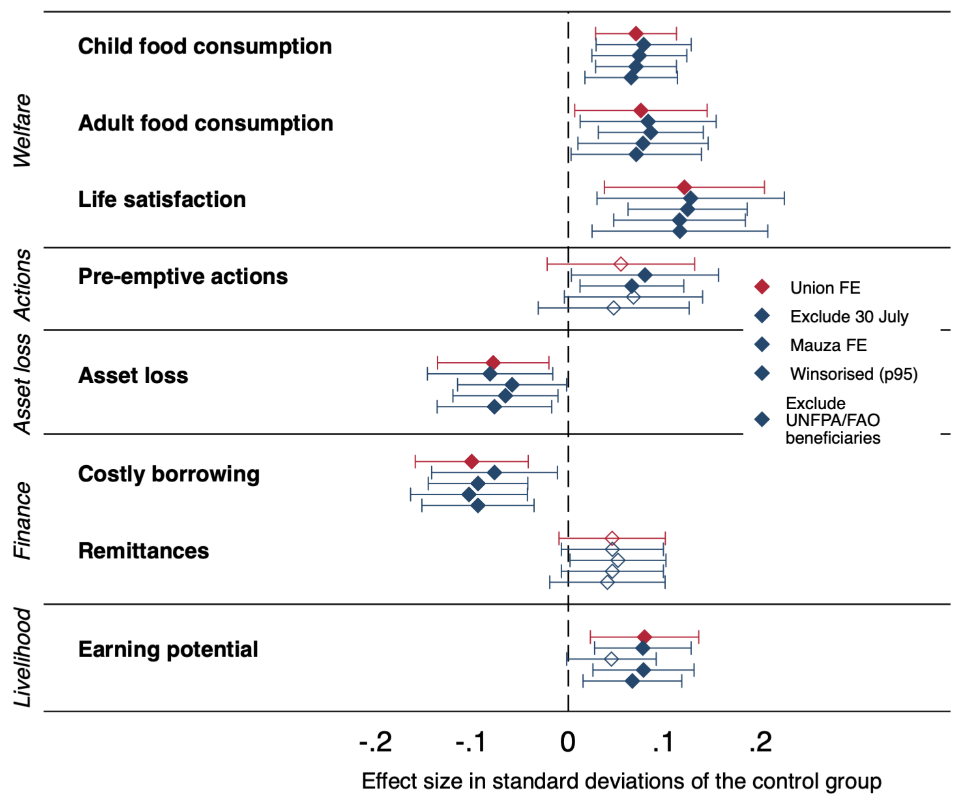
7.2 Robustness to alternative model specifications

We check for robustness to a range of alternative model specifications. Figure 7 illustrates standardised treatment effects using 90% confidence intervals across five different model specifications, with further details provided in Appendix Table A15. Our preferred model specification using union fixed effects and controlling for covariates is shown as the first set of standardised treatment effects for all outcomes. Our results are robust to excluding households receiving a later transfer on July 30 (specification 2), using more conservative fixed effects (specification 3), winsorising outcomes at the 95th percentile (specification 4) and excluding the sub-sample of households that reported receiving assistance from UNFPA and FAO during the same flood period (specification 5). Our results are also robust to excluding covariates, controlling for the survey date, and controlling for various measures of flood intensity estimated at the mauza level from satellite imagery (see Appendix Tables A16, A17 and A18).[34] The latter suggests that the geographic fixed effect in our main specification sufficiently controls for the variation in flood shock across space.

[33] When we split the sample three ways, the effect on remittances is negative for frequent wallet users, this is suggestive of crowding-out effects among this sub-sample, and significantly different from both infrequent and new wallet user groups. See Table A13.

[34] Only the asset loss index loses significance when controlling for survey date.

Figure 7: 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 alternatives (blue), with 90% confidence intervals shown. Solid markers indicate significance at the 10% level. 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. Standard errors are clustered at union level.

7.3 Correcting for differential non-response rates

In this section, we show that our results are unlikely to be driven by differential non-response rates. We identify three types of differential non-response comparing treated to control households: (1) differential non-response conditional on the call being received by the beneficiary (0.6%); (2) differential non-response conditional on the call being received by anyone, beneficiary or not (4.3%); (3) differential non-response conditional on a call made by the survey firm to a phone number (24%).

We have reason to believe that the beneficiary verification data used in targeting households may not have been fully accurate and we sampled control households that were less likely to be contacted in the first instance. Fifty enumerators worked tirelessly to contact roughly 40,000 households in the space of five days using their own SIM cards due to the unexpected much earlier onset of the flood. The exclusion categories used in the verification process were not always correctly recorded, given that nearly all control households reported access to a bKash mobile money account at the time of the survey (contrary to the verification data). Hence, it is plausible that a greater proportion of households were excluded because they were not be contacted at the time of verification. In light of this, we consider the relevant non-response rate to be the second category outlined above (cases in which a phone call was successfully received by someone, beneficiary or otherwise).

We also rule out one explanation for why the third category of non-response might cause us to over-estimate the treatment effects. We consider the case that control households were less likely to respond to the phone surveys, because lack of cash support caused them to experience higher asset loss, including mobile phones, or be more likely to migrate out of mobile coverage. In this case, we would conclude that our treatment effects are likely to be lower bounds as a result.

We correct for all three types of differential non-response using Lee Bound (Lee, 2009). In Table 6, we show the lower bound (from trimming households with outcomes roughly above the 95th percentile) and upper bound (from trimming treated households with outcomes below the 5th percentile) in this order. Correcting for the second type of differential non-response, the effects on child food consumption, asset loss, and costly borrowing are robust and remain highly significant for the most part. The result on child food consumption is robust to correcting even for the most conservative type of differential non-response.

Table 6: Lee bounds to correct for differential non-response

	Standardised mean treatment effect [N]	Lee bounds for non-response – by definition of contacted households: Call received by beneficiary Δ n-r = 0.6% [N]	Call received Δ n-r = 4.3% [N]	Call made Δ n-r = 24% [N]
Child food consumption	0.070*** [7563]	0.070***/0.070*** [7563/7563]	0.070***/0.070*** [7563/7563]	0.070***/0.502*** [7563/6568]
Adult food consumption index	0.075* [8951]	0.053/0.082** [8909/8924]	-0.030/0.141*** [8670/8678]	-0.376***/0.395*** [7394/7484]
Life satisfaction	0.121** [8941]	0.121**/0.121** [8941/8941]	-0.015/0.121** [8702/8941]	-0.334***/0.121** [7470/8941]
Pre-emptive actions	0.055 [8947]	0.045/0.055 [8920/8947]	0.013/0.055 [8837/8947]	-0.154***/0.055 [8362/8947]
Asset loss index	-0.078** [8950]	-0.100***/-0.078** [8916/8950]	-0.190***/-0.078** [8678/8950]	-0.456***/0.191*** [7470/7340]
Costly borrowing index	-0.100*** [6061]	-0.117***/-0.094*** [6041/6035]	-0.201***/-0.059* [5899/5878]	-0.462***/0.119*** [5095/5155]
Remittances	0.046 [8950]	0.046/0.046 [8950/8950]	0.046/0.046 [8950/8950]	-0.286***/0.046 [8325/8950]
Earning potential index	0.077** [8945]	0.052*/0.077** [8903/8945]	-0.030/0.077** [8675/8945]	-0.324***/0.445*** [7358/7463]

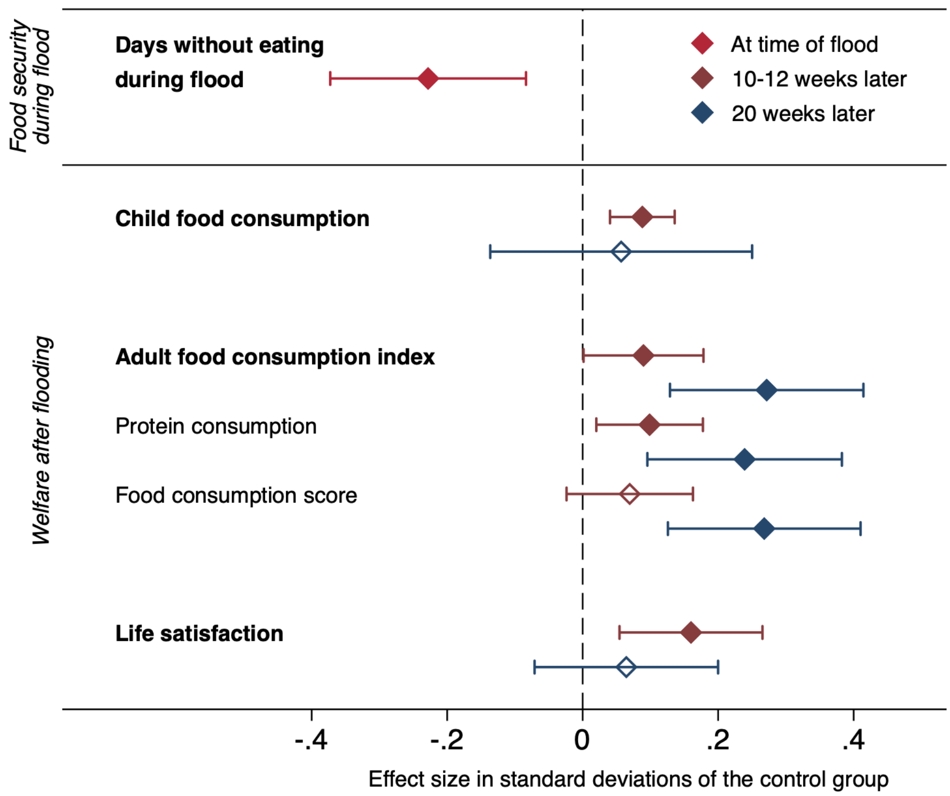
Notes: Lee (2009) bounds (lower/upper) for estimated treatment effects are shown for different definitions of contacted households and corresponding differential non-response rates. Higher non-response was recorded in the control group in all cases. Reasons considered as non-response under alternative definitions: (a) call received by participant – partial interview, refused to interview; (b) call received – partial interview, refused to interview, rescheduled – call received by other person who was staying far from the home during call, participant absent (visiting relatives), not interviewed for other reasons; (c) call made – as for call received and phone switched off, did not answer call. The *unreachable* control category and incorrect phone number cases are excluded from all three non-response groups. 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

8 Results five months post-intervention

We explore the effects of the cash transfer on household welfare in the subset of unions covered by the second round of phone surveys five months after the intervention, noting that this survey has less power and coverage relative to our first round of data collection. We employ the same empirical strategy as outlined in Section 4. For comparability, we also present results from the first round using only the panel of unions covered in both rounds of data collection.

Figure 8 illustrates the standardised treatment effects on our three welfare outcomes for the two rounds of data (results also reported in Appendix Table A19). Focusing first on the treatment effects from the first round of data collected 10-12 weeks after the intervention, we see that the welfare effects of the cash transfer mirror those noted in Section 5.1, if not even marginally stronger in magnitude. Our panel of unions are thus not too dissimilar to the full sample of unions considered in the earlier analysis.

Figure 8: Welfare effects over time within panel unions



Notes: Markers indicate the standardised mean treatment effect of receiving the cash transfer on welfare outcomes during the flood (red), 10-12 weeks later (maroon), and 20 weeks later (blue), with 90% confidence intervals shown. The sample is restricted to unions included in both surveys, and upazilas (the administrative unit above a union) with at least 10 treatment and control households. Solid markers indicate significance at the 10% level. 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.

Five months after the intervention, we find that child food consumption has improved for both treated and control households, relative to the earlier round of data collection. However, as illustrated in Figure 9, control households have converged to treated households, such that the difference is no longer statistically significant; 86% of control households reported that children had consumed at least three meals in the day prior to the survey in December 2020 compared to only 80% in late September. This convergence is reassuring and perhaps unsurprising, given that the child food consumption measure is blunt and captures an extreme coping strategy. Similarly, life satisfaction improves for both treated and control households, with some convergence for control households. We cannot reject the null hypothesis that the cash transfer has had no effect on life satisfaction five months after the intervention relative to the control group. Table 7 reports the p-values and sharpened q-values for the standardised treatment effects on the welfare outcomes and the percentage change relative to the control mean. We do not observe significant treatment effects on any other outcomes.

Table 7: Standardised mean treatment effect on welfare outcomes within panel unions (second round only)

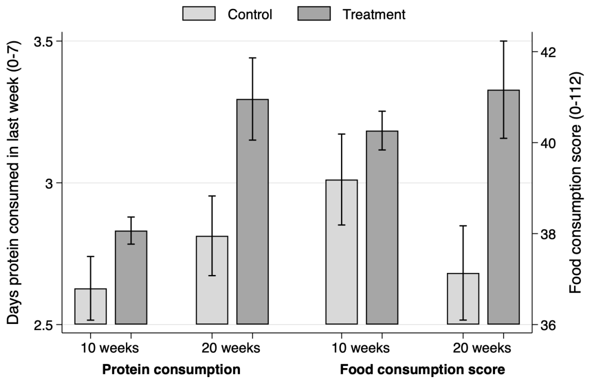
	Std. treatment effect	Control mean	Δ	% Δ	p-value	q-value	N
Day without eating during flood (0/1)	-0.222**	0.28	-0.10	-35.7%	0.030	0.047	1284
Children consumed three meals (0/1)	0.057	0.87	0.02	+2.3%	0.625	0.455	1057
Adult food consumption index	0.272***				0.002	0.010	1284
Days protein products consumed (0–7)	0.239***	2.97	0.48	+16.2%	0.007		1284
Food consumption score (0–112)	0.268***	38.22	4.03	+10.5%	0.003		1284
Life satisfaction (0–10)	0.066	2.41	0.13	+5.4%	0.421	0.391	1283

Notes: The first column shows the standardised mean treatment effect for welfare outcomes measured in the second survey (for households in panel unions). The second column shows the control mean, followed by the non-standardised treatment effect (Δ) and percentage change relative to the control mean (% Δ). p-values are reported on all outcomes and sub-indices, with standard errors clustered at union level. False discovery rate q-values for four 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 and land type. Union fixed effects are included. * p<0.10, ** p<0.05, *** p<0.01

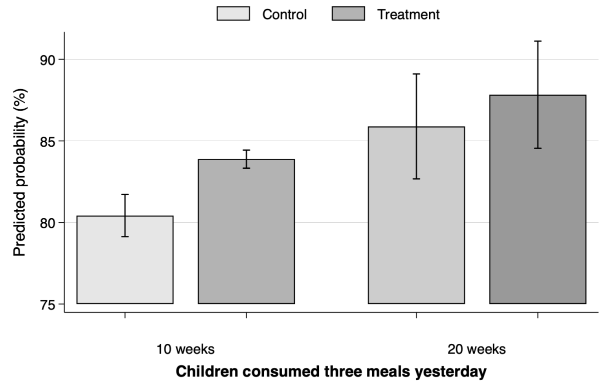
In contrast, we observe a strong positive treatment effect on adult food consumption of 0.272 standard deviations relative to the control group (p=0.002). This effect is statistically significant at the 1% level after correcting for the false discovery rate (Table 7). As shown in Figure 9, improvements in adult food consumption are driven by a strong increase in the number of days in which expensive proteins are consumed (+16%, p=0.007) and in general nutrient in-take or FCS (+11%, p=0.003) in the week prior to the survey. Within the FCS, there is an increase in almost all forms of nutrients consumed by treated households relative to control households. Whereas protein and general nutrient intake increased over time for treated households, protein consumption increased by a lesser extent for control households, while general nutrient intake significantly decreased over time. We test whether control households substitute away from adult food consumption to protect child food consumption over time but reject this hypothesis.[35]

[35]Child and adult food consumption are complementary in panel control households (93% of control households in the second survey were included in the first survey). The decrease in the adult FCS is significant

Figure 9: Child and adult food consumption 10 and 20 weeks after the intervention



(a) Changes in adult food consumption



(b) Changes in child food consumption

Notes: Panel (a) shows the predicted protein consumption and FCS for adults. Panel (b) shows the predicted probability that children consumed three meals yesterday. Both were estimated using the main specification. Samples vary from 10 weeks to 20 weeks. 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. 90% confidence intervals are shown.

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 welfare impact of a one-off anticipatory cash transfer provided to households affected by extreme floods in Bangladesh.

A cash transfer provided seven days on average before local peak flooding allowed households to spend more on food in anticipation of the flood. This had an immediate and large impact on food consumption. Households with the cash transfer were 36% less likely to go a day without eating compared to households that did not receive the transfer. The transfers also encouraged households to take action to reduce the impact of the flood. Households that received the cash transfer were 12% more likely to evacuate household members and 17% more likely to evacuate their livestock. As a result, transfer-receiving households were 8% less likely to lose small livestock and 5% less likely to lose poultry during and after the flooding.

The effects of the transfer were still present three months later. Children in households that received the transfers were 4% more likely to have consumed three meals in the day prior to the survey. Adults in the treatment households had higher food security; for example, the number of days that meat, fish or eggs was consumed was higher by 7%. Stated wellbeing

for control households without children (p=0.078), but not for the majority (77%) of these control households with children (see Appendix Table A20).

was 12.5% higher for transfer-receiving households (using Cantril's ladder of life satisfaction measure). Those receiving the transfer borrowed less (they were 3% less likely to borrow and borrowed 6% less when they did) and did so more cheaply (monthly interest rates were 12% lower). They were more likely to be working (they were 6% more likely to work and hours of work were 7% higher). The effect sizes after three months are small, but are surprisingly broad, impacting both current and future welfare.

There are five important take-aways for crisis response from these results. First, one-off cash transfers provided to households in a natural disaster make a difference to food security and stated wellbeing. They also reduce behaviours that have a cost on future wellbeing (such as reducing child consumption and costly borrowing) and help households recover quicker, pointing to long-run welfare benefits.

Second, early action in a crisis is essential. Failing to act early has real welfare costs. All the welfare impacts estimated in this analysis were measured before the typical arrival date of humanitarian assistance. During the floods of 2019, households started to receive support 100 days after the flood. These results show that providing support early – in this case, even in advance of the flood – can alleviate losses that are known to have scarring effects, such as lower child consumption and costly borrowing. As a result, there are significant advantages from early action, even if later support provides some potential for recovery.

Third, speed matters, even when acting early. A small one-off cash transfer is more effective if received earlier in shock response. Households that received a cash transfer one day earlier during the floods had a small, but statistically significant difference in adult food consumption. This highlights the importance of investing in preparing and targeting well in advance of disasters, such that the intervention can scale immediately with the activation of triggers, and the value of using earlier (10-day rather than five-day) triggers to release funding. It also highlights the need to innovate on protocols for immediately enrolling potential beneficiaries in mobile money platforms (as showcased in Aiken et al. (2021) in Togo).

Fourth, more work is needed to determine the ideal size and frequency of the transfers, as a small one-off transfer appears not to be enough. The transfers provided were equivalent to two weeks' worth of support, but the crisis lasted much longer and the needs of households were much larger than this. Unsurprisingly then, the size of the impacts we estimate are small. It is possible that larger and longer lasting impacts will require larger or repeated transfers. This is something that should be explored in future work.

Fifth, there seems to be some potential in utilising geographic variables in targeting crisis response. We find that the cash transfer was more beneficial for households located in vulnerable lands and in areas where households are on average poorer. While we do not know what exactly is driving the differential results across space, the fact that higher impacts are seen in areas that could be identified as more vulnerable highlights the potential for using more geographic variables to target crisis response. The results highlight that this is as an important avenue for innovation and testing.

There are also some clear lessons from this work on evaluating humanitarian interventions. This paper is an unusual large-scale evaluation of the impact of humanitarian cash transfers provided in response to a sudden onset climate disaster. The unprecedented scale-up of cash transfers during the Covid-19 crisis will likely provide other valuable learning opportunities.

Much more learning on emergency response is needed. 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 requires 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. We hope that this work motivates a stronger commitment by documenting the welfare benefits resulting from the hard work of humanitarian partners and identifying clear lessons on the timing and targeting of support.

References

Abay, K., Berhane, G., Hoddinott, J., and Tafere, K. (2020), *COVID-19 and Food Security in Ethiopia: Do Social Protection Programs Protect?*, Policy Research Working Paper 9475, Washington, DC: World Bank.

Adams, T., Sridhar, P., and Kaur, J. (2021), *Collective Anticipatory Humanitarian Action for Monsoon Flooding - Bangladesh*, 60 Decibels.

Adhvaryu, A., Nyshadham, A., Molina, T., and Tamayo, J. (2018), *Helping children catch up: Early life shocks and the progresa experiment*, NBER Working Paper 24848, National Bureau of Economic Research.

Aguayo, V. M., Sharma, A., and Subedi, G. R. (2015), 'Delivering essential nutrition services for children after the Nepal earthquake', *The Lancet Global Health* 3(11), pp. e665–e666.

Aker, J. C. (2013), *Cash or Coupons? Testing the Impacts of Cash versus Vouchers in the Democratic Republic of Congo*, CGD Working Paper 320, Washington, D.C.: Centre for Global Development.

Aker, J. C., Boumnijel, R., McClelland, A., and Tierney, N. (2016), 'Payment mechanisms and antipoverty programs: Evidence from a mobile money cash transfer experiment in Niger', *Economic Development and Cultural Change* 65(1), pp. 1–37.

Anderson, M. L. (2008), 'Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects', *Journal of the American Statistical Association* 103(484), pp. 1481–1495.

Andrews, C., Hsiao, A., and Ralston, L. (2018), 'Social Safety Nets Promote Poverty Reduction, Increase Resilience, and Expand Opportunities', in: K. Beegle, A. Coudouel, and E. Monsalve (eds.), *Realizing the Full Potential of Social Safety Nets in Africa*, Washington, DC: World Bank, chap. 2, pp. 87–138.

Arndt, C., Davies, R., Gabriel, S., Harris, L., Makrelov, K., Robinson, S., Levy, S., Simbanegavi, W., Seventer, D. van, and Anderson, L. (2020), 'Covid-19 lockdowns, income distribution, and food security: An analysis for South Africa', *Global Food Security* 26, p. 100410.

Asfaw, S., Carraro, A., Davis, B., Handa, S., and Seidenfeld, D. (2017), 'Cash transfer programmes, weather shocks and household welfare: evidence from a randomised experiment in Zambia', *Journal of Development Effectiveness* 9(4), pp. 419–442.

Banks, S. and Dinges, D. F. (2007), 'Behavioral and physiological consequences of sleep restriction', *Journal of Clinical Sleep Medicine* 3(5), pp. 519–528.

Battistin, F. (2016), *Lebanon Cash Consortium (LCC) impact evaluation of the multipurpose cash assistance programme*, Save the Children, International Rescue Committee, ACTED, CARE, Solidarités and World Vision International.

Benjamini, Y., Krieger, A. M., and Yekutieli, D. (2006), 'Adaptive linear step-up procedures that control the false discovery rate', *Biometrika* 93(3), pp. 491–507.

Bessone, P., Rao, G., Schilbach, F., Schofield, H., and Toma, M. (2020), *The Economic Consequences of Increasing Sleep Among the Urban Poor*, NBER Working Paper 26746, National Bureau of Economic Research.

Bottan, N., Hoffmann, B., and Vera-Cossio, D. A. (2021), 'Stepping up during a crisis: The unintended effects of a noncontributory pension program during the Covid-19 pandemic', *Journal of Development Economics* 150, p. 102635.

Carsell, K. M., Pingel, N. D., and Ford, D. T. (2004), 'Quantifying the Benefit of a Flood Warning System', *Natural Hazards Review* 5(3), pp. 131–140.

Castro, L. S., Poyares, D., Leger, D., Bittencourt, L., and Tufik, S. (2013), 'Objective prevalence of insomnia in the São Paulo, Brazil epidemiologic sleep study', *Annals of Neurology* 74(4), pp. 537–546.

Chen, Y. and Zhou, L. A. (2007), 'The long-term health and economic consequences of the 1959-1961 famine in China', *Journal of Health Economics* 26(4), pp. 659–681.

Corporation, I. F. (2016), *IFC Inclusive Business Case Study: bKash*, International Finance Corporation.

Cox, D. (1987), 'Motives for Private Income Transfers', *Journal of Political Economy* 95(3), pp. 508–546.

Cox, D. and Jakubson, G. (1995), 'The connection between public transfers and private interfamily transfers', *Journal of Public Economics* 57(1), pp. 129–167.

Cunha, J. M. (2014), 'Testing paternalism: Cash versus in-kind transfers', *American Economic Journal: Applied Economics* 6(2), pp. 195–230.

Cunha, J. M., De Giorgi, G., and Jayachandran, S. (2019), 'The Price Effects of Cash Versus In-Kind Transfers', *Review of Economic Studies* 86(1), pp. 240–281.

De Mel, S., McKenzie, D., and Woodruff, C. (2012), 'Enterprise Recovery Following Natural Disasters', *Economic Journal* 122(559), pp. 64–91.

Deaton, A. (2008), 'Income, health, and well-being around the world: Evidence from the Gallup World Poll', *Journal of Economic Perspectives* 22(2), pp. 53–72.

Del Carpio, X. V. and Macours, K. (2010), *Leveling the intra-household playing field: Compensation and specialization in child labor allocation*, vol. 31, World Bank, pp. 259–295.

Dercon, S. and Porter, C. (2014), 'Live aid revisited: Long-term impacts of the 1984 Ethiopian famine on children', *Journal of the European Economic Association* 12(4), pp. 927–948.

Dinkelman, T. (2017), 'Long-run Health Repercussions of Drought Shocks: Evidence from South African Homelands', *Economic Journal* 127(604), pp. 1906–1939.

Doocy, S. and Tappis, H. (2017), 'Cash-based approaches in humanitarian emergencies: a systematic review', *Campbell Systematic Reviews* 13(1), pp. 1–200.

Gentilini, U., Almenfi, M., Orton, I., and Dale, P. (2020), *Social Protection and Jobs Responses to COVID-19: A Real-Time Review of Country Measures (September 18, 2020)*, Washington, DC: World Bank.

Gros, C., Bailey, M., Schwager, S., Hassan, A., Zingg, R., Uddin, M. M., Shahjahan, M., Islam, H., Lux, S., Jaime, C., and Coughlan de Perez, E. (2019), 'Household-level effects of providing forecast-based cash in anticipation of extreme weather events: Quasi-experimental evidence from humanitarian interventions in the 2017 floods in Bangladesh', *International Journal of Disaster Risk Reduction* 41, p. 101275.

Gros, C., Easton-Calabria, E., Bailey, M., Dagys, K., Perez, E. C. de, Sharavnyambuu, M., and Kruczkiewicz, A. (2020), 'The effectiveness of forecast-based humanitarian assistance in anticipation of extreme winters: Evidence from an intervention for vulnerable herders in Mongolia', *Disasters*.

Guiteras, R., Jina, A., and Mushfiq Mobarak, A. (2015), 'Satellites, self-reports, and submersion: Exposure to Floods in Bangladesh', *American Economic Review* 105(5), pp. 232–236.

Haushofer, J. and Shapiro, J. (2016), 'The short-term impact of unconditional cash transfers to the poor: Experimental evidence from Kenya', *Quarterly Journal of Economics* 131(4), pp. 1973–2042.

Haushofer, J. and Shapiro, J. (2018), *The Long-Term Impact of Unconditional Cash Transfers: Experimental Evidence from Kenya*, Nairobi, Kenya: Busara Center for Behavioral Economics.

Hidrobo, M., Hoddinott, J., Peterman, A., Margolies, A., and Moreira, V. (2014), 'Cash, food, or vouchers? Evidence from a randomized experiment in northern Ecuador', *Journal of Development Economics* 107, pp. 144–156.

Hou, X. (2010), 'Can drought increase total calorie availability? The impact of drought on food consumption and the mitigating effects of a conditional cash transfer program', *Economic Development and Cultural Change* 58(4), pp. 713–737.

Imbens, G. W. and Rubin, D. B. (2015), *Causal inference: For statistics, social, and biomedical sciences an introduction*, Cambridge University Press.

Ivaschenko, O., Doyle, J., Kim, J., Sibley, J., and Majoka, Z. (2020), 'Does "Manna from Heaven" help? The role of cash transfers in disaster recovery—lessons from Fiji after Tropical Cyclone Winston', *Disasters* 44(3), pp. 455–476.

Janvry, A. de, Finan, F., Sadoulet, E., and Vakis, R. (2006), 'Can conditional cash transfer programs serve as safety nets in keeping children at school and from working when exposed to shocks?', *Journal of Development Economics* 79(2), pp. 349–373.

Jensen, N. D., Barrett, C. B., and Mude, A. G. (2017), 'Cash transfers and index insurance: A comparative impact analysis from northern Kenya', *Journal of Development Economics* 129, pp. 14–28.

Jensen, R. T. (20043), 'Do private transfers "displace" the benefits of public transfers? Evidence from South Africa', *Journal of Public Economics* 88(1-2), pp. 89–112.

Juarez, L. (2009), 'Crowding out of private support to the elderly: Evidence from a demogrant in Mexico', *Journal of Public Economics* 93(3-4), pp. 454–463.

Kling, J. R., Liebman, J. B., and Katz, L. F. (2007), 'Experimental analysis of neighborhood effects', *Econometrica* 75(1), pp. 83–119.

Knippenberg, E. and Hoddinott, J. (2017), *Shocks, social protection, and resilience: Evidence from Ethiopia*, ESSP Working Paper 109, International Food Policy Research Institute (IFPRI).

Kreibich, H., Thieken, A. H., Petrow, T., Müller, M., and Merz, B. (2005), 'Flood loss reduction of private households due to building precautionary measures – Lessons learned from the Elbe flood in August 2002', *Natural Hazards and Earth System Science* 5(1), pp. 117–126.

Lee, D. S. (2009), 'Training, wages, and sample selection: Estimating sharp bounds on treatment effects', *Review of Economic Studies* 76(3), pp. 1071–1102.

Lehmann, C. and Masterson, D. T. R. (2014), *Emergency economies: The impact of cash assistance in Lebanon. An impact evaluation of the 2013–2014 winter cash assistance program for Syrian Refugees in Lebanon*, tech. rep., New York: International Rescue Committee.

Leroy, J. L., Gadsden, P., Rodríguez-Ramírez, S., and De Cossío, T. G. (2010), 'Cash and in-kind transfers in poor rural communities in Mexico increase household fruit, vegetable, and micronutrient consumption but also lead to excess energy consumption', *Journal of Nutrition* 140(3), pp. 612–617.

Lim, J. and Dinges, D. F. (2010), 'A Meta-Analysis of the Impact of Short-Term Sleep Deprivation on Cognitive Variables', *Psychological Bulletin* 136(3), pp. 375–389.

Maccini, S. and Yang, D. (2009), 'Under the weather: Health, schooling, and economic consequences of early-life rainfall', *American Economic Review* 99(3), pp. 1006–1026.

Mackinnon, J. G. and Webb, M. D. (2017), 'Wild Bootstrap Inference for Wildly Different Cluster Sizes', *Journal of Applied Econometrics* 32(2), pp. 233–254.

Macours, K., Premand, P., and Vakis, R. (2012), *Transfers, diversification and household risk strategies: experimental evidence with lessons for climate change adaptation*, Policy Research Working Paper 6053, Washington, DC: World Bank.

Mansur, A., Doyle, J., and Ivaschenko, O. (2017), *Social Protection and Humanitarian Assistance Nexus for Disaster Response*, Social Protection and Labor Discussion Paper 1701, Washington, DC: World Bank.

Premand, P. and Stoeffler, Q. (2020), *Do Cash Transfers Foster Resilience? Evidence from Rural Niger*, Policy Research Working Paper 9473, Washington, DC: World Bank.

Puri, J., Aladysheva, A., Iversen, V., Ghorpade, Y., and Brück, T. (2017), 'Can rigorous impact evaluations improve humanitarian assistance?', *Journal of Development Effectiveness* 9(4), pp. 519–542.

Schokman, A., Bin, Y. S., Simonelli, G., Pye, J., Morris, R., Sumathipala, A., Siribaddana, S. H., Hotopf, M., Rijsdijk, F., Jayaweera, K., and Glozier, N. (2018), 'Agreement between subjective and objective measures of sleep duration in a low-middle income country setting', *Sleep Health* 4(6), pp. 543–550.

Schwab, B. (2019), 'Comparing the Productive Effects of Cash and Food Transfers in a Crisis Setting: Evidence from a Randomised Experiment in Yemen', *Journal of Development Studies* 55(sup1), pp. 29–54.

Singha, M., Dong, J., Sarmah, S., You, N., Zhou, Y., Zhang, G., Doughty, R., and Xiao, X. (2020), 'Identifying floods and flood-affected paddy rice fields in Bangladesh based on Sentinel-1 imagery and Google Earth Engine', *ISPRS Journal of Photogrammetry and Remote Sensing* 166, pp. 278–293.

Skoufias, E., Unar, M., and Gonzalez de Cossio, T. (2013), 'The poverty impacts of cash and in-kind transfers: experimental evidence from rural Mexico', *Journal of Development Effectiveness* 5(4), pp. 401–429.

Subbiah, A., Bildan, L., and Narasimhan, R. (2008), *Background Paper on Assessment of the Economics of Early Warning Systems for Disaster Risk Reduction*, Washington, DC: World Bank, Global Facility for Disaster Reduction, and Recovery.

Thieken, A. H., Kreibich, H., Müller, M., and Merz, B. (2007), 'Coping with floods: Preparedness, response and recovery of flood-affected residents in Germany in 2002', *Hydrological Sciences Journal* 52(5), pp. 1016–1037.

Tranchant, J. P., Gelli, A., Bliznashka, L., Diallo, A. S., Sacko, M., Assima, A., Siegel, E. H., Aurino, E., and Masset, E. (2019), 'The impact of food assistance on food insecure populations during conflict: Evidence from a quasi-experiment in Mali', *World Development* 119, pp. 185–202.

Uddin, K., Matin, M. A., and Meyer, F. J. (2019), 'Operational flood mapping using multitemporal Sentinel-1 SAR images: A case study from Bangladesh', *Remote Sensing* 11(13), p. 1581.

United Nations Resident Coordinator Office (2020), *HCTT Monsoon Flood Humanitarian Response Plan: Monitoring Dashboard (5 September 2020)*, tech. rep., Dhaka: United Nations Resident Coordinator Office, Humanitarian Coordination Task Team.

Victora, C. G., Christian, P., Vidaletti, L. P., Gatica-Domínguez, G., Menon, P., and Black, R. E. (2021), 'Revisiting maternal and child undernutrition in low-income and middle-income countries: variable progress towards an unfinished agenda', *The Lancet*.

Webb, P. (2015), 'How strong is our evidence for effective management of wasting? A review of systematic and other reviews', *Food and Nutrition Bulletin* 36, S65–S71.

Webb, P., Boyd, E., Pee, S. de, Lenters, L., Bloem, M., and Schultink, W. (2014), 'Nutrition in emergencies: Do we know what works?', *Food Policy* 49(P1), pp. 33–40.

Weingärtner, L., Pforr, T., and Wilkinson, E. (2020), *The evidence base on anticipatory action*, World Food Programme.

WFP (2020), *Anticipatory Action Bangladesh: Outcome Survey Report*, World Food Programme.

World Bank (2018), *The State of Social Safety Nets 2018*, Washington, DC: World Bank.

World Bank (2019), *Bangladesh Poverty Assessment: Facing Old and New Frontiers in Poverty Reduction*, Washington, DC: World Bank.

Appendix figures and tables

Table A1: Balance across treatment groups by transfer date

	14 July mean	15 July mean	16 July mean	30 July mean	Joint orth. p-value	14=15=16 July p-value
Individual characteristics						
Age	38.04	39.72	39.59	38.46	0.004	0.003
Female respondent	0.97	0.97	0.97	0.96	0.269	0.141
Household head	0.22	0.20	0.22	0.21	0.704	0.456
Completed primary school	0.34	0.26	0.25	0.30	0.000	0.000
Household characteristics						
Household size	4.73	4.74	4.74	4.72	0.060	0.047
Dependency ratio	0.76	0.74	0.75	0.77	0.064	0.047
Raw material house	0.25	0.28	0.29	0.29	0.751	0.953
Distance to large water body (m)	1419.68	1060.98	1081.00	1165.52	0.565	0.374
Protected mainland	0.38	0.27	0.28	0.26	0.392	0.615
Unprotected mainland	0.26	0.30	0.27	0.29	0.476	0.454
Char land	0.36	0.43	0.45	0.45	0.916	0.806
Anticipatory action						
Received WFP cash transfer	1.00	1.00	1.00	1.00	.	.
Received dignity kit from UNFPA	0.17	0.12	0.11	0.11	0.327	0.174
Received feed or storage from FAO	0.08	0.07	0.05	0.08	0.390	0.348
Technology						
Used digital wallet in last six months	0.48	0.48	0.49	0.38	0.023	0.237
Own mobile	0.79	0.82	0.80	0.79	0.635	0.453
Uses someone else's mobile	0.19	0.16	0.19	0.20	0.310	0.185
Uses mobile at least once a week	0.96	0.96	0.97	0.97	0.679	0.406
Observations	3312	1218	1085	670		

Notes: The tests for equivalence across means report the p-value controlling for union fixed effects and clustering standard errors at union level as in our main specification.

Table A2: Balance in sample from second round of surveys

	Control mean	Treatment mean	Δ	Norm. Diff.	p-value
Individual characteristics					
Age	37.00	37.89	0.88	0.07	0.665
Female respondent	0.99	0.99	0.00	0.03	0.965
Household head	0.22	0.26	0.04	0.09	0.948
Female head	0.25	0.27	0.02	0.05	0.422
Completed primary school	0.36	0.36	-0.01	-0.01	0.804
Household characteristics					
Household size	4.47	4.53	0.05	0.03	0.558
Dependency ratio	0.77	0.78	0.01	0.02	0.919
Raw material house	0.25	0.24	-0.01	-0.03	0.820
Household asset categories (June)	6.02	5.90	-0.12	-0.06	0.491
Protected	0.54	0.54	0.01	0.01	0.819
Unprotected	0.26	0.28	0.02	0.05	0.355
Char	0.20	0.17	-0.03	-0.08	0.155
Anticipatory action					
Received WFP cash transfer in July	0.00	1.00	1.00	.	.
Received WFP cash transfer in October	0.00	0.00	0.00	.	.
Technology					
Used digital wallet in last six months	0.61	0.49	-0.12	-0.25	0.030
Own mobile	0.86	0.87	0.00	0.00	0.860
Uses someone else's mobile	0.13	0.13	-0.00	-0.01	0.959
Uses mobile at least once a week	0.99	0.99	-0.00	-0.00	0.218
Observations	659	632			

Notes: Δ reports the treatment mean minus the control mean. Norm. Diff. reports the normalised difference between the treatment and control group means, following Imbens and Rubin (2015). The last column reports the p-value from ordinary least squares regressions of each variable on the treatment dummy to test equivalence of means, controlling for union fixed effects and clustering standard errors at union level as in our main specification.

Table A3: 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 A4: 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)
	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?

Variable construction for outcome measures (cont.)

Outcome	Construction	Question(s)
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?

Table A5: False Discovery Rate q-values and wild bootstrap inference

	Standardised mean treatment effect	p-value	q-value	Wild bootstrap p-value	N
Child food consumption	0.070***	0.006	0.027	0.007	7563
Adult food consumption index	0.075*	0.072	0.046	0.084	8951
Life satisfaction	0.121**	0.018	0.028	0.016	8941
Pre-emptive actions	0.055	0.240	0.099	0.266	8947
Asset loss index	-0.078**	0.027	0.033	0.033	8950
Costly borrowing index	-0.100***	0.005	0.027	0.010	6061
Remittances	0.046	0.155	0.072	0.162	8950
Earning potential index	0.077**	0.012	0.027	0.021	8945

False discovery rate q-values for eight hypotheses are calculated 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. Standard errors clustered at union level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A6: Treatment effect on secondary outcomes

	(1) Early warning (0/1)	(2) Any action to prepare (0/1)	(3) Evacuated household (0/1)	(4) Evacuated livestock (0/1)	(5) Lost small livestock (0/1)	(6) Lost poultry (0/1)	(7) Borrowed money (0/1)	(8) Worked for wage (0/1)
Transfer	0.009	0.040*	0.034**	0.030**	-0.023*	-0.032*	-0.024*	0.039***
	(0.019)	(0.020)	(0.014)	(0.013)	(0.014)	(0.018)	(0.013)	(0.012)
Control mean	0.61	0.53	0.29	0.17	0.30	0.60	0.70	0.68
% Δ	1.4%	7.4%	11.7%	17.3%	-7.6%	-5.4%	-3.4%	5.8%
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	8951	8947	8947	8947	8952	8950	8946	8950
R^2	0.07	0.10	0.10	0.07	0.06	0.17	0.06	0.06

Notes: 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. * p<0.10, ** p<0.05, *** p<0.01

Table A7: Treatment effect on main outcomes for 14,15,16 July transfers vs 30 July transfers

	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Transfer on 14–16 July	0.144***	0.126***	0.101*	0.094*	-0.051	-0.050	0.065*	0.050
	(0.033)	(0.038)	(0.057)	(0.054)	(0.036)	(0.045)	(0.035)	(0.037)
Transfer on 30 July	0.126**	0.033	0.116*	0.001	-0.122***	-0.205***	0.076	0.099**
	(0.049)	(0.049)	(0.067)	(0.061)	(0.046)	(0.053)	(0.067)	(0.049)
p-value: Before = After	0.686	0.019	0.756	0.083	0.089	0.001	0.853	0.247
q-value: Before	0.001	0.007	0.106	0.106	0.162	0.162	0.106	0.162
q-value: After	0.032	0.252	0.106	0.328	0.029	0.002	0.162	0.091
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7217	8552	8542	8548	8551	5785	8551	8547
R^2	0.04	0.10	0.10	0.11	0.13	0.10	0.04	0.12

Notes: The standardised mean treatment effect of receiving cash before the flood peak (14, 15, 16 July) and after the flood peak (30 July) is shown in the main panel, with standard errors clustered at union level in parentheses. The second panel shows p-values from a test of equivalence comparing before and after flood transfer coefficients. The false discovery rate q-values for 16 hypotheses are calculated over the main outcomes for before and after flood transfers, 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, land type and wallet use category. We also control for survey date. Union fixed effects are included. * p<0.10, ** p<0.05, *** p<0.01

Table A8: Marginal effect of an earlier cash transfer on secondary outcomes

	(1) Early warning (0/1)	(2) Any action to prepare (0/1)	(3) Evacuated household (0/1)	(4) Evacuated livestock (0/1)	(5) Lost small livestock (0/1)	(6) Lost poultry (0/1)	(7) Borrowed money (0/1)	(8) Worked for wage (0/1)	(9) Replanted lost crops (0/1)
Transfer	0.013	0.039*	0.004	0.009	-0.046***	-0.012	-0.004	0.012	-0.030
	(0.021)	(0.021)	(0.020)	(0.015)	(0.016)	(0.020)	(0.017)	(0.019)	(0.029)
Transfer × days before flood peak	0.001	0.001	0.003**	0.002	0.002*	0.001	-0.001	0.001	-0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Control mean	0.61	0.53	0.29	0.17	0.30	0.60	0.70	0.68	0.24
% Δ transfer	2.1%	7.3%	1.3%	5.4%	-15.1%	-1.9%	-0.6%	1.8%	-12.7%
% Δ/day vs transfer	0.2%	0.2%	1.1%	0.9%	0.9%	0.2%	-0.2%	0.1%	-2.1%
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mauza fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	8354	8349	8349	8349	8354	8352	8348	8353	3405
R^2	0.15	0.18	0.18	0.15	0.12	0.24	0.13	0.14	0.18

Notes: Covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. We also control for survey date. Union fixed effects are included. Standard errors are clustered at union level. * p<0.10, ** p<0.05, *** p<0.01

Table A9: Marginal effect of an earlier cash transfer - IV

	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Transfer	0.128***	0.077**	0.064	0.036	-0.074**	-0.125***	0.052	0.067*
	(0.035)	(0.039)	(0.050)	(0.051)	(0.035)	(0.044)	(0.042)	(0.037)
Transfer × days before flood peak	-0.000	0.006**	-0.001	0.006*	0.004	0.010***	0.001	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Weak-id test: F-stat	2686	2515	2515	2518	2520	1853	2516	2532
Endog. test: p-value	0.253	0.113	0.250	0.144	0.062	0.003	0.740	0.270
Over-id test: p-value	0.180	0.993	0.139	0.341	0.063	0.137	0.531	0.369
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
IV	✓	✓	✓	✓	✓	✓	✓	✓
N	7117	8442	8432	8438	8441	5710	8441	8437
R^2	0.01	0.03	0.01	0.01	0.03	0.03	0.01	0.07

Excluded instruments for the number of days before the flood peak include dummy variables for receiving the transfer on the 14, 15, and 16 of July, respectively (30 July is redundant). Estimated using the two-step efficient generalised method of moments.

Weak-id test reports the Kleibergen-Paap F-statistic for weak identification (the Stock-Yogo weak ID critical value for 10% maximal IV size=19.93). Endog. test reports the p-value from an Anderson-Rubin test for the significance of endogenous regressor (H0: days before flood peak=0 and orthogonality conditions are valid). Over-id test reports the p-value from a Hansen J overidentification test of all instruments (H0: the instruments are valid).

Table A10: Marginal effect of an earlier cash transfer - excluding 30 July

	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Transfer	0.088	0.036	0.125	0.133*	-0.019	-0.116	0.035	-0.000
	(0.066)	(0.069)	(0.095)	(0.072)	(0.068)	(0.073)	(0.069)	(0.064)
Transfer × days before flood peak	0.006	0.010	-0.002	-0.006	-0.003	0.009	0.003	0.004
	(0.007)	(0.007)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mauza fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	6469	7695	7685	7691	7694	5142	7694	7692
R^2	0.11	0.15	0.18	0.20	0.21	0.17	0.11	0.18

Notes: Covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. We also control for survey date. Union fixed effects are included. Standard errors are clustered at union level. * p<0.10, ** p<0.05, *** p<0.01

Table A11: Summary statistics by land type

	Unprotected mean	Char mean	Protected mean	Joint orth. p-value
Individual characteristics				
Age	38.10	39.57	37.51	0.751
Female respondent	0.98	0.93	1.00	0.881
Household head	0.22	0.22	0.18	0.531
Completed primary school	0.34	0.26	0.34	0.289
Household characteristics				
Household size	4.72	4.85	4.58	0.428
Dependency ratio	0.76	0.77	0.73	0.828
Raw material house	0.29	0.29	0.24	0.145
Distance to large water body (m)	1397.50	458.19	2025.82	0.000
Satellite measure of flood intensity				
Maximum flood extent (% mauza)	0.35	0.31	0.28	0.347
Flood duration (days above 50% max. extent)	47.91	54.37	38.11	0.022
Anticipatory action				
Received WFP cash transfer	0.73	0.78	0.68	0.019
Received dignity kit from UNFPA	0.09	0.12	0.15	0.629
Received feed or storage from FAO	0.04	0.08	0.07	0.004
Technology				
Used digital wallet in last six months	0.51	0.47	0.47	0.471
Own mobile	0.80	0.79	0.83	0.230
Uses someone else's mobile	0.18	0.19	0.16	0.399
Uses mobile at least once a week	0.96	0.97	0.97	0.692
Observations	2405	3345	3204	

Notes: The tests for equivalence across means report the p-value controlling for union fixed effects and clustering standard errors at union level as in our main specification.

Table A12: Treatment effect on secondary outcomes by land type

	(1) Early warning (0/1)	(2) Any action to prepare (0/1)	(3) Evacuated household (0/1)	(4) Evacuated livestock (0/1)	(5) Lost small livestock (0/1)	(6) Lost poultry (0/1)	(7) Borrowed money (0/1)	(8) Worked for wage (0/1)
Transfer	0.017	0.048	0.031	0.045**	-0.042*	-0.006	-0.021	0.014
	(0.026)	(0.031)	(0.020)	(0.020)	(0.025)	(0.030)	(0.026)	(0.021)
Char	0.012	-0.026	-0.037	0.003	-0.035	0.003	0.000	-0.081**
	(0.046)	(0.042)	(0.037)	(0.030)	(0.032)	(0.038)	(0.033)	(0.036)
Transfer × char	-0.009	0.022	0.010	-0.016	0.035	-0.027	0.030	0.069**
	(0.037)	(0.041)	(0.030)	(0.028)	(0.035)	(0.034)	(0.030)	(0.030)
Protected	0.023	0.028	-0.046	0.039	-0.039	0.057	0.065	-0.015
	(0.041)	(0.052)	(0.041)	(0.029)	(0.033)	(0.044)	(0.042)	(0.040)
Transfer × protected	-0.015	-0.050	-0.003	-0.028	0.019	-0.050	-0.044	-0.000
	(0.042)	(0.049)	(0.033)	(0.025)	(0.032)	(0.041)	(0.033)	(0.033)
Control mean: Unprotected	0.59	0.50	0.26	0.12	0.30	0.55	0.68	0.67
Treat effect: Unprotected	0.017	0.048	0.031	0.045**	-0.042*	-0.006	-0.021	0.014
% Δ: Unprotected	2.9%	9.5%	11.7%	36.1%	-14.1%	-1.1%	-3.0%	2.0%
p-value: Unprotected	0.523	0.131	0.122	0.029	0.098	0.843	0.430	0.524
Control mean: Char	0.65	0.53	0.27	0.15	0.28	0.50	0.69	0.65
Treat effect: Char	0.007	0.070**	0.041*	0.029	-0.007	-0.033*	0.009	0.083***
% Δ: Char	1.1%	13.2%	15.3%	19.4%	-2.5%	-6.7%	1.3%	12.7%
p-value: Char	0.796	0.017	0.090	0.144	0.754	0.095	0.622	0.000
Control mean: Protected	0.60	0.56	0.32	0.22	0.32	0.69	0.73	0.70
Treat effect: Protected	0.002	-0.002	0.028	0.016	-0.023	-0.056*	-0.064***	0.014
% Δ: Protected	0.3%	-0.4%	8.8%	7.4%	-7.1%	-8.1%	-8.8%	2.0%
p-value: Protected	0.953	0.954	0.275	0.344	0.337	0.064	0.001	0.561
p-value: Unprot. = Char.	0.797	0.597	0.733	0.573	0.321	0.428	0.317	0.022
p-value: Unprot. = Prot.	0.721	0.311	0.931	0.251	0.553	0.224	0.186	1.000
p-value: Char = Prot.	0.888	0.146	0.694	0.576	0.637	0.428	0.006	0.028
F-test p-value:								
Control Δ = 0	0.836	0.604	0.439	0.247	0.445	0.237	0.222	0.076
Treatment effect Δ = 0	0.935	0.344	0.910	0.504	0.604	0.472	0.021	0.030
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	8951	8947	8947	8947	8952	8950	8946	8950
R^2	0.07	0.10	0.10	0.07	0.06	0.17	0.06	0.07

Notes: 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. * p<0.10, ** p<0.05, *** p<0.01

Table A13: Standardised treatment effects on sub-samples by wallet-use

	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Existing wallet users								
Transfer	0.070**	0.054	0.104	0.043	-0.078*	-0.088**	0.028	0.089**
	(0.031)	(0.052)	(0.070)	(0.053)	(0.043)	(0.041)	(0.048)	(0.037)
N	4606	5439	5435	5437	5438	3664	5439	5436
R^2	0.05	0.10	0.13	0.11	0.14	0.11	0.05	0.12
New wallet users								
Transfer	0.087*	0.124*	0.099*	0.096	-0.060	-0.102*	0.103*	0.042
	(0.048)	(0.065)	(0.058)	(0.081)	(0.056)	(0.058)	(0.058)	(0.048)
N	2842	3384	3377	3382	3384	2300	3383	3381
R^2	0.07	0.11	0.12	0.15	0.15	0.11	0.06	0.14
p-value: Exist.=New	0.774	0.378	0.959	0.568	0.799	0.835	0.337	0.418
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Results are reported for two separate models corresponding to different mobile money users. Coefficients are interpreted as the standardised mean treatment effect relative to control households in the same wallet use category, with standard errors clustered at union level shown in parentheses. *p*-values from a seemingly unrelated regression test of equivalent coefficients from pairwise comparisons of the models are reported in the second last panel. Covariates for all models 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 A14: Standardised treatment effects on sub-samples by wallet-use

	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Frequent wallet users								
Transfer	0.059	-0.013	0.097	0.014	-0.089	-0.127**	-0.106*	0.130***
	(0.051)	(0.059)	(0.068)	(0.065)	(0.058)	(0.061)	(0.061)	(0.047)
N	2298	2678	2676	2678	2677	1834	2678	2675
R^2	0.07	0.10	0.15	0.19	0.19	0.14	0.09	0.16
Infrequent wallet users								
Transfer	0.117**	0.100	0.091	0.049	-0.066	-0.035	0.140**	0.078
	(0.050)	(0.069)	(0.088)	(0.065)	(0.057)	(0.059)	(0.056)	(0.062)
N	2306	2758	2756	2756	2758	1820	2758	2758
R^2	0.09	0.15	0.15	0.11	0.13	0.13	0.07	0.13
New wallet users								
Transfer	0.087*	0.124*	0.099*	0.096	-0.060	-0.102*	0.103*	0.042
	(0.048)	(0.065)	(0.058)	(0.081)	(0.056)	(0.058)	(0.058)	(0.048)
N	2842	3384	3377	3382	3384	2300	3383	3381
R^2	0.07	0.11	0.12	0.15	0.15	0.11	0.06	0.14
p-value: Freq.=Infreq.	0.450	0.138	0.930	0.607	0.771	0.273	0.000	0.539
p-value: Freq.=New	0.693	0.139	0.983	0.465	0.747	0.782	0.012	0.203
p-value: Infreq.=New	0.657	0.776	0.941	0.597	0.930	0.397	0.658	0.628
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Results are reported for three separate models corresponding to different mobile money users. Coefficients are interpreted as the standardised mean treatment effect relative to control households in the same wallet use category, with standard errors clustered at union level shown in parentheses. p-values from a seemingly unrelated regression test of equivalent coefficients from pairwise comparisons of the models are reported in the second last panel. Covariates for all models 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 A15: 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) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Main								
Transfer	0.070***	0.075*	0.121**	0.055	-0.078**	-0.100***	0.046	0.077**
	(0.025)	(0.042)	(0.050)	(0.046)	(0.035)	(0.035)	(0.032)	(0.030)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7563	8951	8941	8947	8950	6061	8950	8945
R^2	0.04	0.09	0.09	0.09	0.13	0.09	0.04	0.11
Exclude 30 July								
Transfer	0.078***	0.083*	0.127**	0.080*	-0.081**	-0.077*	0.046	0.079**
	(0.030)	(0.043)	(0.059)	(0.046)	(0.039)	(0.039)	(0.033)	(0.034)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	6755	7998	7990	7994	7997	5398	7997	7995
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.11
Mauza fixed effects								
Transfer	0.074**	0.086***	0.124***	0.066**	-0.058*	-0.094***	0.052*	0.045
	(0.030)	(0.033)	(0.038)	(0.033)	(0.034)	(0.031)	(0.030)	(0.028)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mauza fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7471	8862	8852	8858	8861	5954	8861	8856
R^2	0.10	0.14	0.16	0.18	0.20	0.17	0.10	0.17
Winsorised at p95								
Transfer	0.070***	0.078*	0.116***	0.068	-0.065**	-0.103***	0.046	0.078**
	(0.025)	(0.041)	(0.041)	(0.043)	(0.033)	(0.037)	(0.032)	(0.032)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7563	8951	8941	8947	8950	6061	8950	8945
R^2	0.04	0.09	0.10	0.10	0.13	0.09	0.04	0.10
Excl. UNFPA/FAO								
Transfer	0.065**	0.071*	0.116**	0.047	-0.077**	-0.094***	0.041	0.067**
	(0.029)	(0.041)	(0.055)	(0.047)	(0.036)	(0.035)	(0.036)	(0.031)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	6366	7549	7539	7546	7548	5120	7549	7545
R^2	0.04	0.09	0.10	0.09	0.12	0.09	0.04	0.12

Notes: 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. * p<0.10, ** p<0.05, *** p<0.01

Table A16: Standardised mean treatment effects - welfare

	Child food consumption		Adult food consumption index		Life satisfaction	
	(1)	(2)	(3)	(4)	(5)	(6)
Transfer	0.053**	0.070***	0.070	0.075*	0.119**	0.121**
	(0.025)	(0.025)	(0.045)	(0.042)	(0.049)	(0.050)
Controls		✓		✓		✓
Union fixed effects	✓	✓	✓	✓	✓	✓
N	7565	7563	8953	8951	8943	8941
R^2	0.03	0.04	0.06	0.09	0.09	0.09

Notes: 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. * p<0.10, ** p<0.05, *** p<0.01

Table A17: Standardised mean treatment effects - livelihood, finance and actions

	Pre-emptive actions		Asset loss index		Costly borrowing index		Remittances		Earning potential index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Transfer	0.061	0.055	-0.073**	-0.078**	-0.109***	-0.100***	0.047	0.046	0.071**	0.077**
	(0.047)	(0.046)	(0.035)	(0.035)	(0.039)	(0.035)	(0.033)	(0.032)	(0.031)	(0.030)
Controls		✓		✓		✓		✓		✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	8949	8947	8952	8950	6062	6061	8952	8950	8947	8945
R^2	0.09	0.09	0.10	0.13	0.07	0.09	0.03	0.04	0.05	0.11

Notes: 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. * p<0.10, ** p<0.05, *** p<0.01

Table A18: Robustness to controlling for satellite measures of flood intensity at mauza level

	(1) Child food consumption	(2) Adult food consumption index	(3) Life satis-faction	(4) Pre-emptive actions	(5) Asset loss index	(6) Costly borrowing index	(7) Remit-tances	(8) Earning potential index
Main								
Transfer	0.070***	0.075*	0.121**	0.055	-0.078**	-0.100***	0.046	0.077**
	(0.025)	(0.042)	(0.050)	(0.046)	(0.035)	(0.035)	(0.032)	(0.030)
R^2	0.04	0.09	0.09	0.09	0.13	0.09	0.04	0.11
Survey date dummies								
Transfer	0.112***	0.114***	0.089*	0.056	-0.049	-0.093**	0.046	0.064**
	(0.029)	(0.040)	(0.046)	(0.052)	(0.032)	(0.040)	(0.034)	(0.029)
R^2	0.04	0.09	0.10	0.10	0.13	0.10	0.04	0.11
Maximum flood extent								
Transfer	0.073***	0.075*	0.123**	0.054	-0.078**	-0.103***	0.046	0.079**
	(0.025)	(0.042)	(0.051)	(0.046)	(0.035)	(0.035)	(0.033)	(0.031)
Flood extent	0.207**	-0.085	-0.064	-0.043	0.191	0.133	-0.106	-0.108
	(0.104)	(0.134)	(0.181)	(0.182)	(0.161)	(0.121)	(0.128)	(0.144)
R^2	0.04	0.09	0.09	0.09	0.13	0.09	0.04	0.11
Flood duration								
Transfer	0.071***	0.076*	0.123**	0.055	-0.080**	-0.105***	0.048	0.080***
	(0.025)	(0.042)	(0.050)	(0.046)	(0.035)	(0.035)	(0.032)	(0.030)
Flood duration	-0.003	-0.001	-0.001	-0.000	0.001	-0.001	0.002	0.000
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
R^2	0.04	0.09	0.09	0.09	0.13	0.09	0.04	0.11
Flood extent and duration								
Transfer	0.074***	0.075*	0.123**	0.054	-0.078**	-0.103***	0.046	0.079**
	(0.025)	(0.042)	(0.051)	(0.046)	(0.035)	(0.035)	(0.032)	(0.031)
Flood extent	0.318***	-0.067	-0.052	-0.044	0.199	0.195	-0.176	-0.131
	(0.112)	(0.143)	(0.192)	(0.180)	(0.171)	(0.125)	(0.138)	(0.155)
Flood duration	-0.004**	-0.001	-0.000	0.000	-0.000	-0.002	0.003	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
R^2	0.04	0.09	0.09	0.09	0.13	0.09	0.04	0.11
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	7563	8951	8941	8947	8950	6061	8950	8945

Notes: 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. * p<0.10, ** p<0.05, *** p<0.01

Table A19: Welfare effects over time - panel unions

	(1) Days without food in flood	(2) Child food consumption	(3) Adult food consumption index	(4) Adult protein consumption	(5) Adult food consumption score	(6) Life satisfaction
Round 2 (20 weeks)						
Transfer	-0.228**	0.057	0.272***	0.239***	0.268***	0.064
	(0.087)	(0.116)	(0.086)	(0.086)	(0.085)	(0.081)
N	1284	1057	1284	1284	1284	1283
R^2	0.12	0.09	0.14	0.12	0.15	0.13
Round 1 (10-12 weeks)						
Transfer		0.088***	0.090*	0.099**	0.069	0.160**
		(0.029)	(0.053)	(0.047)	(0.056)	(0.063)
N		5289	6258	6258	6258	6250
R^2		0.03	0.09	0.06	0.10	0.08
Controls	✓	✓	✓	✓	✓	✓
Union fixed effects	✓	✓	✓	✓	✓	✓

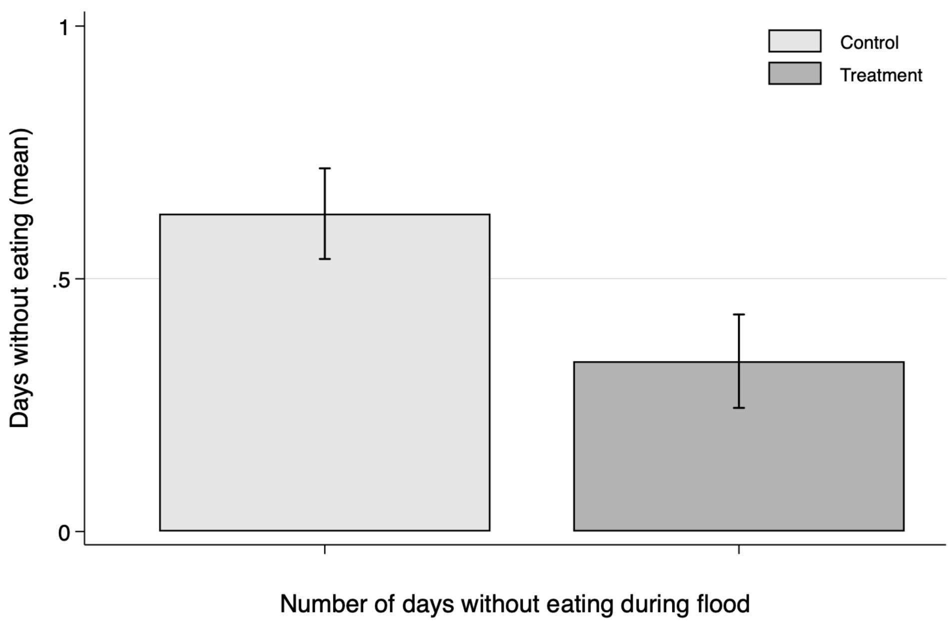
Notes: Covariates include age, gender, education level, household size, dependency ratio, house structure, UNFPA/FAO recipient status and land type. Union fixed effects are included. Standard errors are clustered at union level. * p<0.10, ** p<0.05, *** p<0.01

Table A20: Test for substitution of adult and child food consumption over time in panel control households

	Δ children meals yesterday			Δ children ate three or more meals		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ adult meals yesterday	0.333***			0.268***		
	(0.037)			(0.040)		
Δ adult food consumption score		0.006***			0.003**	
		(0.002)			(0.001)	
Δ adult protein consumption			0.033***			0.014
			(0.011)			(0.009)
N	469	469	469	469	469	469
R^2	0.08	0.02	0.01	0.14	0.01	0.01

Notes: First difference model for panel control households that report child food consumption 10 weeks after flooding and 20 weeks after flooding. 93% of 659 control households in the second survey were also contacted in the first survey. 469 of these panel households (77%) report child food consumption. Positive significant coefficients indicate that changes in child and adult food consumption are complementary. Standard errors are clustered at union level. Note first differencing removes time-invariant confounders at household level. * p<0.10, ** p<0.05, *** p<0.01

Figure A1: Food security during the flood



Notes: 90% confidence intervals are shown. 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.