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Cash and Cohesion in Crisis: On the Impacts of Anticipatory Cash Transfers in IDP Camps in South Sudan during Floods

Dorothee Weiffen[1,2*], Ghassan Baliki[1,2], Jan Elfes[1,3] and Tilman Brück[1,2,4]

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Highlights

- Households in protracted displacement use one-off anticipatory cash transfer at the onset of a severe flood to upgrade their shelters.
- Improved shelters significantly mitigated the severe negative impacts of floods.
- Substantial impacts on household welfare remain absent.
- The transfer enhanced social cohesion by reducing theft, conflict, and within-camp displacement in IDP camps.
- Robust shelter conditions are an essential driver of social cohesion.

Abstract
Households living in humanitarian settings face extreme vulnerability to adverse shocks. Cash transfers can reduce this vulnerability and enhance household welfare. However, the potential of anticipatory cash transfers - delivered before the adverse impacts of a shock unfold - in safeguarding welfare and fostering social stability remains underexplored. To address this knowledge gap, we study the effects of a one-off anticipatory cash transfer provided to internally displaced households in Bentiu, South Sudan, at the onset of severe floods. We examine the short- and medium-term impacts of this cash transfer using a quasi-experimental design that

[1] ISDC – International Security and Development Center, Berlin, Germany
[2] Zero Hunger Lab, Thaer-Institute of Agricultural and Horticultural Sciences, Humboldt-University of Berlin, Germany
[3] School of Information and Communication Studies, University College Dublin, Ireland
[4] Leibniz Institute of Vegetable and Ornamental Crops (IGZ), Großbeeren, Germany

*Corresponding author: Dorothee Weiffen, ISDC - International Security and Development Center, Gerichtstraße 49, 13347 Berlin, Germany. Email: weiffen@isdc.org

closely resembles a random treatment assignment. We utilize three waves of panel data: shortly before and shortly after the intervention (during the flood onset) and six months after the intervention (after the floods have receded). Despite unsatisfied basic needs, the findings reveal preferences for strategic investment over short-term consumption: The intervention did not improve food security or mental health and immediate health gains dissipated post-flood. Instead, households prioritized investments in shelter reinforcement, which reduced severe flood impacts by 13%. Shelter investment unexpectedly contributed to community cohesion, reducing displacement (24%), theft (18%), and conflict (24%). Key mechanisms are lockable doors, shelters that withstand the water, and generally less flood stress, highlighting the critical role of housing in fostering security and social stability in crisis settings.

Keywords
cash transfer, risk, humanitarian crisis, forced displacement, natural disasters, community cohesion, anticipatory action

JEL Classifications
O12, Q54, I32 , D91

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1 Introduction

Adverse shocks shape community cohesion, which can have both positive and negative effects (Fiedler, 2023; Langlotz et al., 2025; Vesco et al., 2025). On one hand, extreme events like wars and climatic shocks can foster prosocial behaviour, unity and collective action within a community (Bauer et al., 2016; De Juan & Hänze, 2021; Ferguson & Leroch, 2023). On the other hand, shocks can also exacerbate inequality (Aggarwal, 2021; Paglialunga et al., 2022) and resource competition (Koubi, 2019; Maystadt et al., 2014) which, in turn, can escalate social tensions and lead to violent conflict (Burke et al., 2024; De Juan & Hänze, 2021; van Weezel, 2020). One-fifth of the global population is expected to experience a climate-related shock at least once in their lifetime while being extremely vulnerable in terms of their income, education and access to services (Hill et al., 2024). Displaced communities are particularly susceptible to the adverse impacts of a climatic disaster due to high economic vulnerability and limited capacity to prepare for, cope with and recover from the impacts of a climatic shock (Betts et al., 2024). Given the proximity of neighbours and limited resources in refugee and Internally Displaced Persons (IDP) camps, we expect climatic shocks to significantly impact the social structures within these communities.

Cash transfers are an effective tool to reduce vulnerability and strengthen the welfare of households in displacement and humanitarian settings (Altındağ & O'Connell, 2023; Brück & d'Errico, 2019; Egger et al., 2022; Gupta et al., 2024; Hagen-Zanker et al., 2018; Haushofer & Shapiro, 2016; Hızıroğlu Aygün et al., 2024; Kurdi, 2021; MacPherson & Sterck, 2021; Premand & Stoeffler, 2022; Salti et al., 2022). Cash transfers can support vulnerable societies in recovering from the adverse impact of climatic shocks (Ivaschenko et al., 2020; Matata et al., 2023; Petrova & Rosvold, 2024) and can stabilise consumption during episodes of climatic volatility (Asfaw et al., 2017; Hou, 2010). Furthermore, cash transfers are effective in reducing the negative impacts of other shocks such as violent conflict (Brück et al., 2019; Ecker et al., 2024) or health pandemics (Gupta et al., 2024; Lawson-McDowall et al., 2021). Anticipatory cash transfers, which we define as payments provided before the impacts of a forecasted climatic shock unfold, can also play an important role in improving the well-being of households. Emerging evidence using both experimental and quasi-experimental designs shows that anticipatory cash transfers can safeguard household welfare (Balana et al., 2023; Dunsch et al., 2025; Gros et al., 2019; Pople et al., 2021, 2024) and support business resilience against climatic shocks when combined with in-kind assistance (Gros et al., 2022). However, the positive impacts of anticipatory cash transfers may dissipate as disasters intensify (Gros et al., 2019) or, in some cases, fail to materialise entirely (Mogge et al., 2024). Notably, no study has yet rigorously examined the impact of anticipatory cash transfers in a context that is already in severe crisis, such as a forced displacement setting. Displaced households face distinct institutional and economic constraints compared to other populations (Betts et al., 2024) that may lead to systematically different spending and investment decisions of cash compared to other low-income settings. While one-off transfers are often invested rather than consumed due to the windfall effect of receiving relatively large amounts of money at once (Haushofer & Shapiro, 2016; Sulaiman, 2018), risk and poverty, typical for displacement and other crisis settings, are known to shift preferences towards short-term consumption (Callen et al., 2014; Haushofer & Fehr, 2014). As a result, in such settings, anticipatory cash transfers may be used to address immediate needs, rather than to prepare for the upcoming adverse shock as intended.

Additionally, the evidence on the impacts of (anticipatory) cash transfers on social and community cohesion remains limited, particularly in settings characterised by extreme uncertainty, such as displacement camps (Jeong & Trako, 2022). On the one hand, cash provided to displaced households can improve the relations with other community members as well as with the host communities (Abu Hamad et al., 2025; Lehmann & Masterson, 2014; Valli et al., 2019); on the other hand, the transfers can also increase tensions between displaced and the host communities (Tamim et al., 2025), influencing community dynamics in complex ways. In non-displacement contexts, cash transfers are shown to build trust (Petrova & Rosvold, 2024) and foster cooperation (Attanasio et al., 2015), while simultaneously leading to heightened levels of jealousy, mistrust and even violence against beneficiaries (Della Guardia et al., 2022; Myers et al., 2024; Pavanello et al., 2016; Premand & Rohner, 2024). Despite the limited evidence available, the societal impacts of anticipatory cash transfers in crisis or forced displacement settings remain poorly understood and contested.

To address these knowledge gaps, we investigate the impacts of anticipatory cash transfers on welfare and social outcomes. More specifically, we examine (i) how households allocate windfall income; (ii) if an anticipatory cash transfer mitigates the negative impacts of adverse shocks; (iii) what welfare impacts occur both in the short-term (during shock) and the medium-term (after shock); and (iv) what unintended effects on social cohesion occur. To answer these research questions, we investigate a one-off anticipatory cash transfer provided at the onset of severe floods in an IDP camp in post-war South Sudan, assessing outcomes before and after the disaster. The anticipatory cash transfer was worth 65 USD per household, representing about two weeks' average household expenditures. The International Organisation for Migration (IOM), a United Nations agency, provided assistance at the onset of expected but unprecedented floods and recommended using the transfer for shelter investments ('labelled cash transfer'). Given budget limitations, only some vulnerable households received the cash, permitting us to compare beneficiary (or treatment) and non-beneficiary (or control) households. To conduct a rigorous assessment, we use an innovative quasi-experimental design, where we selected an almost equal tier of households in terms of vulnerability to the control group.

We have four main findings. (i) The majority of households receiving the anticipatory cash transfer allocated it to improve their shelter conditions, strengthening their resilience against flooding, rather than purchasing food or other consumables. (ii) By improving shelter conditions, the anticipatory cash transfer reduced severe negative impacts of the floods by 13% (-10.5pp, 90% CI $[-16.9, -4.1]$, $p < 0.01$). (iii) We find mixed results on the direct welfare impacts: On the one hand, we observe an immediate improvement in the health outcome (though not after six months), probably as better shelter protected against ill health during flooding. On the other hand, we do not observe other significant impacts on food security or mental health, neither directly after the transfer through immediate consumption nor in the recovery phase of the floods through returns from shock protection. (iv) Interestingly, we find impacts beyond shock mitigation as the anticipatory cash transfer decreased the experience of interpersonal conflict within the community (-24%, -11.2pp, 90% CI $[-18.6, -3.7]$, $p < 0.05$), theft (-18%, -8.2pp, 90% CI $[-14.7, -1.6]$, $p < 0.05$) and the likelihood of households being internally displaced within the camps (-24%, -5.9pp, 90% CI $[-11.5, -0.3]$, $p < 0.1$). These impacts are significant in the post-flood period, highlighting how simple shelter upgrades, such as lockable doors or reinforced walls, can protect the social stability of a highly vulnerable community.

Our study contributes to the emerging literature on anticipatory action by adding evidence on the impacts of anticipatory cash transfers and providing the first study on the outcome of social cohesion and in a severe crisis setting, here protracted forced displacement. We identify three policy implications: First, households prioritise shelter reinforcement over immediate satisfaction of basic needs, reflecting the strong desire for protection. Second, in contexts with limited market access, in-kind assistance may be more effective than cash transfers as organisations can better overcome access challenges than the beneficiaries themselves, contributing to the ongoing debate on cash versus in-kind assistance in humanitarian interventions. Third, a critical, yet overlooked, benefit of anticipatory cash transfers is their ability to strengthen community cohesion. Improved shelter conditions can enhance security and stability, reducing conflict with other community members, theft and displacement within the community. This unintended positive impact is a novel contribution to the literature on anticipatory action.

The remainder of this paper is organised as follows: In Section 2, we describe the case study, the intervention and the theoretical framework. In Section 3, we outline our empirical approach. In Section 4, we present and discuss our descriptive findings, the impact findings during and after the flood and robustness checks. In Section 5, we conclude.

2 Context

2.1 The case of Bentiu, South Sudan

We study households in IDP camps in Bentiu in Unity State, South Sudan (Bentiu Town Sites). The study location is prone to severe floods and extreme heat, recurrent violence and crime as well as food shortages and health epidemics, resulting in a multifaceted humanitarian crisis. According to the 2008 census, the population of Unity State was estimated at 586 thousand people (National Bureau of Statistics, Republic of South Sudan, 2012). The area around Bentiu was severely affected by the civil war in South Sudan after its independence, which caused approximately 70 thousand fatalities in Unity State (Checchi et al., 2018). As of July/August 2022, 440 thousand IDPs were hosted in Unity State, of which 55% were children (UNOCHA, 2022). This is the highest state-level number of IDPs in South Sudan. For 39% of these IDPs, natural disasters are the main reason for displacement, followed by conflict and communal clashes (IOM, 2023). The population in and around Bentiu town comprises approximately 99 thousand persons, while only 10% of the population belongs to the host community (DRC, 2024), leading to extreme population pressure and competition for resources and assistance. The camps are organised in different campsites, of which we study three, namely Bimruor (Site A), Kuermandoke (Site B) and Bieh (Site E). Bimruor and Kuermandoke included nine blocks at the start of our study, while Bieh included 15 blocks.

The area is situated in the lowlands of the White Nile and Bahr-el-Ghazal rivers, which flood recurrently, severely affecting the IDP camps (Zwijnenburg et al., 2023). From 2019 to 2024, flood intensities increased significantly, such that water did not recede from previously dry land even during the dry season. In Unity State, 233 thousand people are estimated to have been affected by the unprecedented floods of 2022/2023 (UNSC, n.d.). The studied campsites are protected by dykes, however, breaches and overflowing are common. Figure A1 in the Appendix presents satellite data indicating that a part of the study area was flooded in January 2023. The water flooded latrines and bathing facilities in these camps, leading to contamination threats. Furthermore, the area was also indirectly affected by the flood in 2022/2023: Main roads and the airport runway were temporarily flooded during this period, cutting the supply chains in and out of the region. From April to July 2023, in the county where Bentiu is located, 70% of the population was projected to be acutely food insecure (IPC Global Partners, 2023). Over 80% of the whole South Sudanese population was estimated to require humanitarian assistance in 2023 (UNOCHA, 2022).

Within the Bentiu Town IDP camps, international organisations provide services, such as food assistance, shelter assistance, water, sanitation and hygiene (WASH) infrastructure and health facilities. Schools, small shops and churches are integrated within the camps. Common income generation methods include selling goods, particularly firewood, which can be collected in the distant bushes outside the campsites or fish caught in the surrounding water. The camps are divided and managed in blocks, with each camp block having a designated leader. A typical shelter within the camps is built out of clay with a plastic sheet as a roof. At the household level, flood protection measures include relocating shelters to elevated spots, elevating the shelters themselves or reinforcing walls to withstand temporary flooding. The IDP camps of Bentiu are uniquely suitable for investigating how households respond to assistance amidst extreme political, economic, institutional and climatic crises. Settings similar to Bentiu remain significantly underrepresented in the existing literature because of the difficulty of conducting rigorous research (Puri et al., 2017) despite the critical need for evidence for informed policy-making, also in light of the escalating climate crisis and increasing forced displacement (IPCC, 2023).

2.2 The intervention

To address our research objectives, we investigate a one-off anticipatory cash transfer equivalent to 65 USD that was distributed to households at the onset of the forecasted severe 2022/2023 flood. This amount covers approximately the basic living expenses of an average household in the camp for two weeks. The main objective of the cash transfer is to allow vulnerable households to prepare for the upcoming flood and to mitigate its negative impacts. The intervention prioritised female-headed households, households with members with disabilities and/or elderly members. IOM identified vulnerable households through a community-based approach: Following the recommendations of corresponding block leaders, IOM selected the most vulnerable households, considering the priority criteria within the community. In total, IOM selected 1,351 households to receive the cash transfer. In the three studied campsites, 15% of all households received the support. The cash transfer was unconditional and households could spend the money as they deemed fit. However, households have been encouraged to invest the cash in improving their shelters to protect them against the anticipated severe floods ('labelled cash transfer'). Given the instability of markets in and around the camps at the timing of the cash transfer, households could spend the money on limited goods and services, which include food, hygiene items or buying shelter materials, such as timber to strengthen the walls and the roofs of the dwelling or tin sheets to be used as doors for the shelter as well as construction and transportation services. The cash transfer was distributed at all three sites between 21 and 29 September 2022, at the onset of the forecasted flood.

2.3 Theoretical framework

The anticipatory cash transfer program is based on a well-defined theoretical framework. The transfer should provide beneficiaries with the autonomy to allocate the money as they deem fit, thereby offering flexibility in addressing their most pressing needs. In settings where formal financial services are absent and inflation is high, saving options are limited, leaving beneficiaries to choose between immediate consumption and investment. Consumption spending immediately impacts household welfare by increasing the accessibility of goods and services, thereby satisfying acute basic needs. The literature demonstrates that cash transfers contribute to meeting these needs in humanitarian settings (Jeong & Trako, 2022; van Daalen et al., 2022). Hence, direct consumption would immediately benefit the recipients' well-being.

On the contrary, one-off transfers allow for larger investments, which typically result in welfare improvements in the longer run (Haushofer & Shapiro, 2016; Sulaiman, 2018). However, in the specific context of Bentiu, investment opportunities are highly constrained due to remoteness, interrupted value chains and a general shortage of goods. Further, the anticipated flood posed a significant risk of destruction to any investments. Given these limitations and the provided recommendation, the most plausible option is investment in shelters. Apart from purchasing key inputs to upgrade shelters, the specific target group of women, the elderly and disabled persons particularly benefits from purchasing services to construct the shelter and procure the inputs. Robust shelters protect the household from water and are less likely to collapse, even though they get flooded. A dry shelter can better protect health by reducing the risk of waterborne diseases and other threats from the water, such as petroleum contamination and snakes. Likewise, a safe shelter can contribute to better mental health by offering protection and providing a stable living environment, reducing the risk of stress and anxiety. Additionally, a shelter provides the possibility to store food securely and prevent it from contamination, which would directly affect food security. The literature from non-emergency contexts confirms that robust shelters have substantial positive effects on several key welfare outcomes, including food security, health and mental health (Cattaneo et al., 2009; Galiani et al., 2017; Verschuur et al., 2020), underscoring expected welfare gains beyond the acute flood. The project design did not entail consequences for social cohesion. However, upgraded shelters may reduce the need for relocation. Lockable doors reduce the incidence of theft and prevent unauthorised access to the shelter, thereby potentially reducing conflict. Sharing the support with other community members might contribute to better relationships within the community (Attanasio et al., 2015). On the flip side, the transfer may also increase jealousy and tensions within the community (Della Guardia et al., 2022; Pavanello et al., 2016; Roelen, 2020) and increase theft (Cameron & Shah, 2014).

3 Empirical approach

We use a quasi-experimental design with three waves of household panel data ('Baseline', 'Follow-up 1' and 'Follow-up 2') from treatment and control households to address our research questions, namely we examine (i) how households allocate windfall income; (ii) if an anticipatory cash transfer mitigates the negative impacts of adverse shocks; (iii) what welfare impacts occur both in the short-term (during shock) and the medium-term (after shock); and (iv) what unintended effects on social cohesion occur.

3.1 Variables

To assess these research questions, we tailor the survey modules around the local context, paying special attention to selecting modules that reflect the severity of the context. Only 17% of our respondents are able to read and write. Hence, we work with simplified modules to gather information as meticulously as possible without intimidating or overwhelming the respondents. Below, we describe the outcome and control variables used in the analysis.

To measure shelter conditions, we use three dummy variables: First, if the households report that rain leaks through the roof (water leaks); second, if they report that their shelter is in a 'rather bad' or a 'very bad' condition; third, if the shelter condition is 'very bad' to better assess the lower tail impacts. The enumerators asked the participants to point to their shelters before responding to this question for validation.

For food security, we utilise the domains of the Food Insecurity Experience Scale (FIES), which is particularly adequate for measuring extreme hunger (Cafiero et al., 2018; Saint Ville et al., 2019). FIES consists of eight short yes-or-no questions, referring to each a different experience and insecurity level, ranging from being worried about food to not having access to it. The module captures self-reported behaviours and experiences related to food, particularly regarding resource limitations that hinder their ability to obtain adequate food (FAO, 2017). The questions address a time frame of three weeks before each interview.

To assess the health status, we use a set of simple yes-and-no questions, building on the health modules developed by Stojetz et al. (2022). The module asks if the respondent has experienced the following symptoms in the past two weeks: fever, cough, diarrhoea, stomach pain, chest pain, dizziness and tiredness.

We use the Generalised Anxiety Disorder Module (GAD-7), introduced by Spitzer et al. (2006), to measure mental well-being. The respondents were asked how often they experienced certain types of anxiety symptoms in the past two weeks before each interview. The module includes, for example, trouble falling asleep, worrying too much or being afraid that something awful is happening. Four ordinal responses range from 'not at all' to 'nearly every day'. The module has been implemented and tested in similar contexts in the region (Angelucci et al., 2023; Mughal et al., 2020).

The questions on food security, health and mental health are assessed on the individual level of the respondent. To reduce the dimensionality of each of the food security, health and mental well-being domains, we extract for each module the first component from the principal component analysis to weigh each variable for a joint score (Bertram-Hümmer & Baliki, 2015; Kolenikov & Angeles, 2009). This approach is common in related literature (Amare et al., 2021; Liu et al., 2017). In the case of food security and health, where binary variables represent the domains, we count each 'yes' response as one. Regarding mental health, responses ranged from zero for those who do encounter the issue to three for those who report experiencing it 'nearly every day'. To achieve comprehensive comparability across the scores, we rescaled all three scores to take a value between 0 and 1, where higher values imply that households are worse off. For robustness, we repeated our main analysis using unweighted scores by summing up the domains of each module. Once again, we standardised the total score to create an index ranging from 0 to 1.

To assess flood exposure, we use a variable capturing if households were affected by floods with three answer possibilities: 'No', 'Yes, mildly' and 'Yes, severely'. This variable reflects the respondents' subjective assessment of the flood's adverse impact on their households. The reference period for flood impact is six months for the Baseline and four months for Follow-up 2. This adjustment ensures that we exclude any flood experiences occurring before the transfer and its investment so that we can attribute any variation to the treatment. Therefore, we do not assess the flood impact in Follow-up 1. We prefer the subjective measure over satellite imagery for this setting because the control and treatment households live very close to each other and the resolution is too broad to identify variation at the micro-level (see Figure A1 in the Appendix). Furthermore, the satellite imagery can only capture the exposure at the shelter (or where the interview was conducted), ignoring nuanced adverse impacts, particularly in other places where the households may carry out daily activities or livelihoods (Brück et al., 2016).

For community cohesion, we focus on the dimensions of safety and community integration. We use dummy variables capturing whether households experienced theft or displacement within the camp and whether they were involved in conflict or violence within the community. If they were affected by conflict or theft, we additionally asked if they were mildly or severely impacted. We assess conflict and theft with the same reference period as the floods. For displacement within the camp, we ask for a reference period of three weeks before each wave to capture the acute response to flooding.

Lastly, we assess variables related to demographic and socioeconomic characteristics and assistance. For assistance, we asked if households received any other support packages, namely WASH kits, dignity kits, shelter support, non-food items or cash. We include the latter only before Follow-up 2 to avoid confounding with the studied cash transfer. We consider the types of assistance in the past three weeks before each wave.

We address missing data in time-fixed covariates using information from the same household from other waves. For time-varying categorical variables, we imputed missing values by the most common outcome within the same wave. For continuous variables, we use the wave-specific mean. The largest number of missing values is the age of the household head, with 65 missing values across all three waves. This likely reflects the relatively low importance attributed to precise age in years among older individuals in the camp. Apart from this, the number of missing values for any other variable is limited to a maximum of six observations across all data.

3.2 Data

We collected three waves of panel data. We conducted the first two waves immediately before (August 2022) and immediately after (October 2022) the cash distribution. These interviews took place during the rainy season and at the onset of the anticipated flood. We collected Follow-up 2 six months after the cash distribution and after the flood period in the dry season (March 2023). Notably, even though part of the region was already flooded in August, the most severe episode of the floods occurred between December 2022 and January 2023 (UNSC, n.d.). Hence, with this setup, we can measure the immediate change caused by the cash transfer in a similar season and the medium-term change in the flood recovery phase.

We gathered data from 1,195 households in the first round of data collection (Baseline). 93% of the original survey households were female-headed. Male-headed households systematically differ from female-headed households in terms of several outcome variables (see Table A1 in the Appendix). Acknowledging that our analysis does not represent the entire beneficiary population, we restricted our sample to only female-headed households to improve the precision of the estimated effects. Given the small share of male-headed households in the overall sample, the restriction does not compromise the statistical power of the study. We could follow up with 942 female-headed households in two post-treatment waves. For the main analysis, we only use the balanced panel data, where households were interviewed in all three waves. We test the robustness of our results with the unbalanced panel and including the male-headed households. In total, 15% of the female-headed households were not interviewed in all waves. We do not find differential attrition that might significantly bias the results or affect the validity of our designs. Table A2 in the Appendix shows a detailed breakdown of the drivers of attrition. Households that received any support and households with better shelters at Baseline were less likely to attrit. However, out of the 30 pre-treatment variables we test, only these two are significantly different at the 5% level. In some camp blocks, attrition is substantially higher than in others, as indicated by the R^2 of the regression with and without block-fixed effects (difference of 0.11).

Study participation was voluntary and followed the 'do-no-harm' approach (Anderson & Wallace, 1999). Before conducting the interviews, enumerators informed respondents about the purpose of the study, the use of the data and their rights to refuse to respond fully to the questionnaire or partly to separate modules or questions. The enumerators obtained informed consent verbally and recorded it in the survey tool before the start of the interview. We received verbal approval from local community leaders to conduct the study. We registered the study and trial in the Registry for International Development Impact Evaluations (ID censored for peer review). Notably, the study was registered after data collection was completed and before analysis was conducted. This was due to the need for rapid data collection, also in light of the onset of the floods, which we prioritised over the registration process. The survey data were collected digitally through face-to-face interviews using the same team of enumerators for all rounds. The enumerators conducted interviews in the local language, translating the questionnaire in real-time. We supervised most of the data collection in Bentiu in person and conducted two training sessions on ethical and practical topics with the enumerators. The first session took place before the start of the study and the second refresher training session took place before the last wave of data collection.

3.3 Econometric approach

To causally measure the impact of the cash transfer, accounting for any time-invariant confounders and reducing unobserved heterogeneity, we use linear two-way fixed effects regression models (2FE) as described in Equation (1) below:

$$Y_{it} = \beta_1 \, \textit{treatment}_{it} + \beta_2 \, \textit{wave}_t + \beta_3 \, X_{it} + \alpha_i + u_{it}. \tag{1}$$

where Y_{it} denotes the outcome variable for individual (or household) i in Wave $t \in \{0, 1, 2\}$. The term α_i represents the time-invariant, household-specific error term, which drops out when taking within-household differences. The term u_{it} is the time-varying error term and X_{it} is a vector of time-varying covariates. The coefficient of interest is β_1, which captures the impact of receiving the anticipatory cash transfer, $\textit{treatment}_{it}$, taking the value of 1 for treated households if $t \in \{1, 2\}$ and 0 otherwise.

We analyze the short-term impacts (pre-shock) at $t = 1$ and medium-term (post-shock) impacts at $t = 2$ separately. Therefore, all the main results are based on two waves of panel data. We also test the joint impact by including the two post-treatment waves and accounting for wave-fixed effects, as well as a difference-in-differences estimation as robustness checks. We test the use of a 2FE model against random effects models using the Hausman test and we address heteroskedasticity by applying cluster-robust standard errors, as indicated by the results of the Breusch-Pagan test.

3.4 Study design

To resemble random treatment assignment, we followed the following sampling strategy: First, IOM registered the 'most vulnerable' households for the cash transfer, prioritising women and households with elderly and disabled members for the treatment group in coordination with block leaders. Out of all cash recipient households in the three campsites (1,020 in total), we randomly selected 600 treatment households for our sample after stratifying by the number of beneficiary households in each block. For each block, we provided a list of randomly selected replacement households in case the designated treatment households could not be reached during the pre-treatment data collection. Approximately 22% of the treatment households in our sample were selected from the replacement list starting from the top. Second, before the start of the data collection, we replicated the community-based approach used for the treatment assignment to select the control group. To closely mirror the treatment group, we compiled exhaustive lists based on the selected sample of treated households. We calculated the optimal number of control households and the ratio of female-headed households to be interviewed for each block. Then, in coordination with the block leaders, our enumerators assigned households to the control group that 'would be selected next in case there would be another cash transfer with a similar amount', following the same prioritisation criteria for identifying vulnerable treated households. All households in the treatment and control group met at least one critical vulnerability criterion including at least one female adult (99%) or a person with a disability (73%) or a household head over 50 years of age (22%) at Baseline (not displayed), confirming the compliance to the priority criteria in the control and the treatment group.

By construction, the exhaustive character of our design suggests that treatment households may exhibit higher vulnerability levels. To ensure the validity of the analysis, the difference in vulnerability between treatment and control group must remain minimal. We identify three potential factors that could lead to significant discrepancies in vulnerability between these groups. First, the marginal decline in vulnerability could be very steep, causing the average treatment household to be substantially more vulnerable than the average control household. Second, block leaders, knowing there would be no subsequent cash transfer, may have invested systematically less effort in identifying the most vulnerable households for the control group. Third, leaders may have selected treatment households based on unobserved factors, such as family ties or personal preferences. We demonstrate the robustness of our approach in the next section, providing strong evidence to reject these concerns.

4 Results and discussion

4.1 Descriptive statistics

Table 1, Column (1) displays summary statistics for all relevant covariates and outcomes as mean values with standard deviations in parentheses or as percentages. The first three covariates are proxies for the priority criteria: female-headed households and households with elderly and/or disabled members. All household heads are female at Baseline by design and the ratio of adult female household members among all adults is 61%. On average, the household heads are 40 years old and 15% of the household members in the sample have a disability. Continuing with other key covariates, a household includes, on average, ten members, of which about six are under 18 years of age. The household characteristics show a high degree of vulnerability in the population: Only 17% of the household heads are literate. At Baseline, 39% of the households have a mobile phone and 77% of the households receive any kind of assistance. The most common type of assistance is in-kind food assistance, provided to 63% of the households.

Next, we summarise the outcome variables at Baseline, which consistently showcase the dire situation in the IDP camps of Bentiu. First, 51% of the households reside in shelters in 'very bad' conditions, while only 14% of the respondents live in shelters in 'very good' conditions at

Baseline (see Table 1, Column (1)). Second, 99% of the households report they were affected by the floods six months before Baseline. Of these, 78% report they were severely affected by floods. This underscores the far-reaching impact of the floods and the associated trauma experienced by the people in the IDP camps. Third, Figures A2-A4 in the Appendix break down the welfare indicators by their domains for each wave. Generally, we observe high levels of food insecurity, ill health and mental issues across all waves. Food insecurity increases over the course of the study, with particularly dire results in the post-flood period, while issues related to mental health, especially fear and worry, are highest at the onset of the floods. For health outcomes, no clear trend is observable. The PCA indices in Table 1, Column (1), indicate mean scores of 0.72 and 0.67, for food insecurity and adverse health, respectively. The scores, on average, are close to the maximum levels, underscoring a high concentration at the upper bound. The mean score for mental health is 0.32. Fourth, in terms of social cohesion, 25% of the households were displaced within the camp during the three weeks before Baseline. Moreover, 46% of the respondents report an impact from some kind of interpersonal conflict and 46% report theft, highlighting the insecurity in the camps of Bentiu.

In the next step, we assess the comparability between the groups to demonstrate that our identification strategy ensures that the control group closely resembles the treatment group. Starting with the priority criteria, in the panel dataset, where we only include female-headed households, 76% of the households in the control group and 81% of the households in the treatment group include at least one person with a disability or a household head over the age of 50 years (Not displayed). This emphasises better compliance of the treatment group with the priority criteria, which may reflect the exhaustive selection of the treatment households. Table 1, Columns (2)-(4) iterate OLS regressions with different specifications of Baseline values on treatment assignment, with Column (2) only including the proxies for selection. We control for block-fixed effects to account for cluster-level characteristics. Households with older household heads are more represented in the treatment group ($p < 0.05$). In contrast, the control group shows a slight but statistically insignificant overrepresentation of female household members, while no clear pattern emerges for members with disabilities. Figure A5 in the Appendix depicts the distribution of a vulnerability score derived from a principal component analysis of normalised values for household head age and the ratios of female and disabled household members. The graph highlights the remarkably similar distributions between the control and treatment group. The mean scores are 0.435 for the control and 0.430 for the treatment group. A t-test does not indicate a significant difference in this vulnerability score between the two groups (p=0.638). This underscores that the slight differences detected above balance out in the combined score. We conclude that the overall vulnerability based on the priority criteria of the control group closely mirrors that of the treatment group.

Next, we include a long list of covariates and outcome variables in the regression to test the independent insignificance of the variables, with and without block-fixed effects, as shown in Table 1, Columns (3) and (4), respectively. Overall, we do not observe any substantial differences between the control and treatment group at Baseline. Apart from the age of the household head, no other variable indicates statistically significant differences across both specifications. Other differences at the 10% significance level become insignificant when adding or removing block-fixed effects. Moreover, F-tests indicate no joint significance of the covariates on treatment assignment ($p > 0.47$), while R^2 indicates low predictive power of the models. Table A3 in the Appendix examines differences in block residency by group. Again, an F-test indicates that there is no joint significance of block residency on treatment assignment ($p = 0.88$).

In conclusion, out of 30 variables, only one indicates significant differences between control and treatment households across all specifications. We attribute this to the exhaustive selection of older community members for the cash transfer. Although we cannot entirely rule out unobserved confounding, these results provide strong evidence against the existence of substantial systematic disparities, enabling us to interpret our results causally with confidence.

Table 1: Baseline characteristics and predictors of treatment assignment

	(1) Overall (Mean (SD)/share)	(2) Treatment (Coef (SE))	(3) Treatment (Coef (SE))	(4) Treatment (Coef (SE))
Demographic and socioeconomic characteristics				
Ratio of female adults in HH	60.5%	-0.068 (0.073)	-0.124 (0.086)	-0.135 (0.085)
Age of HHH	39.8 (13.1)	0.003** (0.001)	0.004*** (0.001)	0.003** (0.001)
Ratio of HH members with disabilities	14.9%	0.024 (0.096)	-0.023 (0.125)	0.041 (0.121)
Respondent is HHH	98.3%	–	-0.064 (0.131)	-0.111 (0.129)
HHH is literate	17.0%	–	-0.039 (0.073)	-0.025 (0.071)
Marital status of HHH				
HHH is divorced	0.1%	–	–	–
HHH is married	88.5%	–	0.528 (0.528)	0.450 (0.509)
HHH is widowed	11.4%	–	0.458 (0.529)	0.387 (0.510)
No. HH members	9.6 (4.6)	–	-0.019* (0.011)	-0.015 (0.010)
HH members $<$ 18 years	6.3 (2.9)	–	0.020 (0.015)	0.015 (0.015)
Age oldest child	9.1 (8.8)	–	0.001 (0.002)	0.001 (0.002)
HH owns phone	39.1%	–	-0.016 (0.036)	-0.008 (0.035)
HH owns livestock	12.4%	–	-0.011 (0.077)	-0.029 (0.074)
HH grows vegetables	24.7%	–	0.071 (0.050)	0.048 (0.049)
Assistance (3 weeks)				
WASH	44.7%	–	0.059 (0.052)	0.038 (0.051)
Dignity	22.1%	–	-0.051 (0.054)	-0.048 (0.052)
Food	62.5%	–	-0.007 (0.065)	0.023 (0.062)
Non-food items	35.0%	–	0.030 (0.050)	0.029 (0.048)
Shelter	20.0%	–	-0.056 (0.054)	-0.028 (0.051)
Any support	77.4%	–	-0.059 (0.082)	-0.055 (0.080)
Shelter condition				
Very bad	50.8%	–	–	–
Rather bad	9.6%	–	-0.045 (0.067)	-0.012 (0.064)
Rather good	26.0%	–	-0.088 (0.056)	-0.052 (0.051)
Very good	13.6%	–	-0.050 (0.081)	-0.015 (0.078)
Water leaks	95.3%	–	0.135 (0.082)	0.149* (0.081)
Flood impact				
Floods - Mild impact (6 months)	20.9%	–	-0.262 (0.203)	-0.292 (0.200)
Floods - Severe impact (6 months)	78.3%	–	-0.203 (0.201)	-0.255 (0.198)
Welfare indices				
Food insecurity index	0.722 (0.294)	–	0.081 (0.073)	0.069 (0.072)
Adverse health index	0.668 (0.299)	–	0.024 (0.063)	0.018 (0.062)
Adverse mental health index	0.322 (0.269)	–	-0.040 (0.079)	-0.035 (0.076)
Social cohesion				
Displacement in camp (3 weeks)	24.5%	–	0.099* (0.055)	0.058 (0.053)
Conflict - Mild impact (6 months)	29.2%	–	-0.045 (0.051)	-0.024 (0.049)
Conflict - Severe impact (6 months)	17.0%	–	0.072 (0.065)	0.103* (0.063)
Theft - Mild impact (6 months)	18.9%	–	0.016 (0.060)	0.020 (0.058)
Theft - Severe impact (6 months)	27.1%	–	0.011 (0.051)	-0.003 (0.049)
Observations	942	942	942	942
Block FE	–	Yes	Yes	No
R^2	–	0.030	0.067	0.035
P-value (F-Stat)	–	0.738	0.507	0.471

Notes. Column (1) displays average values of variables. Continuous variables are displayed by means with standard deviations in parentheses. Categorical variables are shown in percentages. Columns (2)-(4) report the coefficients from OLS regressions on treatment assignment with standard errors in parentheses. HH=household, HHH=household head. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Investment decision

Next, we examine investment decisions. Based on post-treatment data from Follow-up 1 (not displayed), 93% of treated households allocated the majority of the anticipatory cash transfer to shelter upgrades, while 7% primarily spent it on health products or food. None of the respondents stated that they spent the cash transfer mainly on other consumable goods or remittances. These findings indicate a strong preference for investing in shelters over immediate consumption, aligning with evidence from refugee settings in non-flood-prone areas (Gupta et al., 2024). This preference for investment over consumption confirms the evidence that one-off transfers are likely to be invested (Haushofer & Shapiro, 2016; Sulaiman, 2018), contrasting myopic spending preferences driven by poverty and risk (De Bruijn & Antonides, 2022; Karim & Noy, 2016). This finding is also novel in the literature on anticipatory cash transfers, which mainly reports consumption smoothing (Balana et al., 2023; Gros et al., 2019; Pople et al., 2021).

Still, in light of the high prevalence of food insecurity, adverse health and, accordingly, adverse mental health (as shown in Figures A2-A4 in the Appendix), the preference for flood protection over satisfaction of immediate basic needs is unexpected. There are three potential explanations for the uniform spending decision in shelters. First, high compliance with the recommendation to allocate funds for shelter upgrades may have driven the shelter investment. Given the absence of a control group with an unlabeled transfer, we cannot rule out that the Hawthorne Effect might have impacted the beneficiaries' responses rather than their actual investment decisions. Second, in highly restricted markets, shelter upgrades may represent one of the few viable spending options, aside from purchasing consumables. The broad provision of food and health assistance suggestively crowded out the spending on food and health goods. Third, the trauma of the preceding flood experience is likely to influence the strong desire for flood protection.

The strong and uniform demand for shelter improvements raises the question of whether the unconditional one-off cash transfer is the right tool to prepare for an expected adverse shock in the context of limited market structures. Direct in-kind shelter assistance may offer a more cost-effective alternative, as it may better overcome the market accessibility constraints present within the camp.

4.3 Intended effects at the onset of the floods (pre-shock)

Next, we focus on the intended impacts of the anticipatory cash transfer at the onset of the floods (pre-shock period) using the Baseline and Follow-up 1 data from the household panel. Table 2 displays immediate treatment effects on shelter conditions and on welfare outcomes. The decision of the treated households to invest the cash in improving their shelters results in a strong and immediate 75%-reduction in the prevalence of water leaks inside shelters (-71.4pp, 90% CI [-75.9, -66.9], $p < 0.01$), an 89%-reduction in reporting 'rather bad' or 'very bad' shelter conditions (-53.7pp, 90% CI [-60.0, -47.4], $p < 0.01$) and a 92% decrease in reporting 'very bad' shelter conditions alone (-46.6pp, 90% CI [-52.8, -40.3], $p < 0.01$). Hence, the investment of the anticipatory cash transfer in shelters effectively and immediately improves shelter conditions at the onset of the flood. These strong causal improvements also confirm compliance with the transfer labelling and diminish the likelihood of a pure Hawthorne Effect in reported shelter investment. The coefficients of the wave-fixed effects also highlight that the control households improved their shelter conditions at the onset of the floods, reinforcing the interpretation of a strong desire for flood preparedness.

In terms of welfare outcomes, the transfer translated into an immediate reduction in adverse health by 7% on the index score (-0.048, 90% CI [-0.095, -0.002], $p < 0.1$), as shown in Column (5) in Table 2. The impact is driven by a reduction in stomach issues by 16% (-8.5pp, 90% CI [-16.5, -1.5], $p < 0.1$, not displayed). We observe that a small share of treated households spent the cash to purchase health-related consumables, which might be driving the weak positive cash effect on the health index. To better disentangle this effect, we run the analysis separately, taking into account how the cash was spent (not displayed). The expenditure on food and health goods or health goods alone does not indicate a significant association with improving health. However, better health outcomes are strongly and significantly attributed to better dwelling conditions (0.166, 90% CI [0.094, 0.237], $p < 0.01$, not displayed). A potential channel may be better hygienic standards in the shelter and protection from flood contamination.

Beyond the positive effect on health, we do not observe any significant immediate impacts of the cash transfer on food insecurity. The null finding is not unexpected, especially since only a few treated households directly purchased food items using this anticipatory cash transfer. Moreover, we do not observe any significant immediate impacts on mental health outcomes following the anticipatory cash transfer, which is surprising given the highly stressful situation at the onset of the floods. We expected treated households to feel less anxious and more prepared to withstand the anticipated flood, particularly a few weeks after receiving cash and improving their shelter condition. Conversely, the cash transfer might have increased the households' awareness of the upcoming flood, offsetting the feeling of preparedness.

The null effect on mental well-being and food security emphasises the limitations of anticipatory cash in improving the welfare of extremely vulnerable households living in challenging settings. These findings contrast existing evidence on the effectiveness of anticipatory cash transfers provided before a disaster in stabilising the well-being of households during a disaster (Dunsch et al., 2025; Pople et al., 2021, 2024) and generally one-off cash transfers in improving welfare in developing settings (Egger et al., 2022; Haushofer & Shapiro, 2016) and refugee settings (Gupta et al., 2024). Given the absence of rigorous studies testing the impact of anticipatory one-off cash transfers in settings that are already in severe crisis, our contrasting findings highlight that the constraints of such interventions may stem from restricted market functionality in a forced displacement setting, which limits access to many essential goods beyond the basic goods supplied by humanitarian organisations.

Table 2: Pre-shock treatment effects of anticipatory cash transfers

	(1) Water leaks	(2) Bad shelter condition	(3) Very bad shelter condition	(4) Adverse food security	(5) Adverse health	(6) Adverse mental health
Baseline mean	0.953	0.604	0.508	0.722	0.668	0.322
(SD)	(0.211)	(0.489)	(0.5)	(0.294)	(0.299)	(0.269)
Impact	-0.714***	-0.537***	-0.466***	-0.005	-0.048*	0.010
	(0.027)	(0.038)	(0.038)	(0.024)	(0.028)	(0.021)
Follow-up 1	-0.050**	-0.052	-0.052	-0.057**	-0.051*	0.077***
	(0.023)	(0.037)	(0.039)	(0.023)	(0.027)	(0.019)
Observations	1884	1884	1884	1884	1884	1884
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.596	0.268	0.231	0.087	0.042	0.092
P-value (F-Stat)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001

Notes. Two-way fixed effects estimations with cluster-robust standard errors in parentheses. Outcome indicators in Columns (4) - (6) are based on weights from principal component analysis and range from 0-1. Control variables include dummy variables if the household received other support packages 3 weeks before each wave. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.4 Intended effects after the floods (post-shock)

Table 3 displays the intended impact of the intervention six months after the cash distribution and after the floods receded (post-shock period) on shelter conditions, welfare outcomes and flood impact mitigation. In terms of shelter conditions, we continue to detect significantly better shelter conditions due to the anticipatory cash transfer. However, the effect sizes diminish considerably over time, with only a 9%-reduction in water leaks (-9.0pp, 90% CI $[-13.9, -4.2]$, $p < 0.01$), a 12%-reduction in 'rather bad' or 'very bad' shelter conditions (-7.2pp, 90% CI $[-14.2, -0.3]$, $p < 0.1$) and a 26%-reduction in 'very bad' shelter conditions (-13.1pp, 90% CI $[-20.5, -5.8]$, $p < 0.01$) compared to Baseline levels. This contrasts sharply with the immediate reductions of 75%, 89% and 92%, respectively. Nevertheless, the intervention supported households in withstanding the severe flood period between Follow-up 1 and Follow-up 2. Specifically, as shown in Columns (7) and (8), the anticipatory cash transfer causally results in a 7% reduction in the overall experience of any negative flood impact (-6.8pp, 90% CI $[-12.1, -1.4]$, $p < 0.05$) and a more significant 13% reduction in the experience of severe adverse flood impacts

(-10.5pp, 90% CI $[-16.9, -4.1]$, $p < 0.01$) emphasizing the protective potential of upgraded shelters. We observe a general trend of reduced flood impact compared to Baseline, as reflected in the wave-fixed effects, likely attributable to improved camp-level protection measures such as dykes and water pumps.

Regarding the welfare outcomes, we do not observe any significant post-shock impacts. Notably, the improvement in health outcomes observed immediately after receiving cash dissipates in the post-flood period. In related literature, the impacts of cash transfers also commonly diminish shortly after regular support is phased out (Altındağ & O'Connell, 2023; Salti et al., 2022) or after a recurrent natural disaster takes place (Gros et al., 2019). The timing, modality and amount of the cash transfers in climatically and politically volatile settings, however, can be pivotal to sustainably improving the economic, nutritional and psychological well-being of vulnerable and poor households (Gupta et al., 2024; Pople et al., 2021). More evidence and research are needed to test these parameters under extreme conditions and uncertainty, identifying how anticipatory cash transfers may help build longer-term resilience.

Table 3: Post-shock treatment effects of anticipatory cash transfers

	(1) Water leaks	(2) Bad shelter condition	(3) Very bad shelter condition	(4) Adverse food security	(5) Adverse health	(6) Adverse mental health	(7) Any flood impact	(8) Severe flood impact
Baseline mean	0.953	0.604	0.508	0.722	0.668	0.322	0.993	0.783
(SD)	(0.211)	(0.489)	(0.5)	(0.294)	(0.299)	(0.269)	(0.086)	(0.412)
Impact	-0.090***	-0.072*	-0.131***	-0.022	-0.015	0.034	-0.068**	-0.105***
	(0.029)	(0.042)	(0.044)	(0.023)	(0.027)	(0.023)	(0.032)	(0.039)
Follow-up 2	-0.123***	0.011	-0.003	0.094***	0.023	0.128***	-0.333***	-0.473***
	(0.022)	(0.032)	(0.031)	(0.018)	(0.019)	(0.017)	(0.024)	(0.029)
Observations	1884	1884	1884	1884	1884	1884	1884	1884
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.203	0.021	0.056	0.122	0.044	0.123	0.375	0.386
P-value (F-Stat)	< 0.001	0.01	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001

Notes. Two-way fixed effects estimations with cluster-robust standard errors in parentheses. Outcome indicators in Columns (4) - (6) are based on weights from principal component analysis and range from 0-1. Control variables include dummy variables if the household received other support packages 3 weeks before each wave. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.5 Unintended effects on community cohesion

Beyond the intended impacts of the anticipatory cash transfer and shelter improvement in reducing the severe negative consequences of the flood, Table 4 shows unintended positive effects in strengthening community stability. Specifically, six months after the cash provision, treatment households are 24% less likely to relocate within the camps in Bentiu (-5.9pp, 90% CI [-11.5, -0.4], $p < 0.1$). We attribute this positive effect to the improved and robust shelter conditions, which better protect against water and are less likely to collapse, particularly because of temporal flooding.

Unexpectedly, the cash transfer also reduced the experience of conflict or violence within the community by 24% (-11.2pp, 90% CI [-18.6, -3.7], $p < 0.05$) and of theft by 18% (-8.2pp, 90% CI [-14.7, -1.6], $p < 0.05$). We attribute these impacts to improved shelter security, as the installation of lockable doors provides displaced households with greater protection against (violent) theft. Still, we observe a general increase in theft over time (12.3pp, 90% CI [7.4, 17.2], $p < 0.01$), suggesting greater overall exposure to crime in the camps. Moreover, households reporting severe flood impacts are more likely to report conflicts (15.1pp, 90% CI [11.3, 18.8], $p < 0.01$, not displayed), suggesting that reduced flood stress may positively influence relationships within the community. In turn, less conflict with other community members may also drive the decrease in displacement within the camp. Another potential mechanism for reduced conflict, which we cannot measure in this study, is the possibility that assistance sharing strengthens relationships within the community.

The declining time trends in displacement and severe conflict (-12.8pp, 90% CI [-16.8, -8.7], $p < 0.01$ and -7.7pp, 90% CI [-11.4, -4.1], $p < 0.01$, respectively) further contradict negative impacts on community tensions. Contrary to notions in the literature suggesting that cash transfers may exacerbate social tensions (Della Guardia et al., 2022; Pavanello et al., 2016) and increase local levels of conflict (Premand & Rohner, 2024), our study presents novel evidence that an anticipatory cash transfer in a humanitarian emergency setting can, in fact, enhance community cohesion and the sense of safety for beneficiaries.

A potential challenge to the robustness of the findings on social cohesion is the possibility that the cash transfer influenced reporting behaviours rather than actual outcomes. After receiving cash, treatment households may have felt a greater sense of being cared for. As a result, they could have been more likely to report improved outcomes regardless of the true values. However, if reporting bias was driving the results, we would expect to see systematic overreporting across the other outcomes as well, including the welfare outcomes, which is not the case. This suggests that the observed improvements in social cohesion likely reflect true changes rather than a reporting bias.

Table 4: Post-shock treatment effects of anticipatory cash transfers on displacement, conflict and theft

	(1) Displacement in camp	(2) Conflict (any)	(3) Severe conflict	(4) Theft (any)	(5) Severe theft
Baseline mean	0.245	0.462	0.17	0.46	0.271
(SD)	(0.43)	(0.499)	(0.376)	(0.5)	(0.445)
Impact	-0.059*	-0.112**	-0.031	-0.082**	-0.014
	(0.034)	(0.045)	(0.028)	(0.040)	(0.039)
Follow-up 2	-0.128***	-0.060*	-0.077***	0.123***	-0.026
	(0.025)	(0.034)	(0.022)	(0.030)	(0.029)
Observations	1884	1884	1884	1884	1884
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.090	0.068	0.086	0.130	0.108
P-value (F-Stat)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001

Notes. Two-way fixed effects estimations with cluster-robust standard errors in parentheses. For displacement within the camp, a time frame of 3 weeks is covered. All other outcomes cover a time frame of 6 months before Baseline and 4 months before Follow-up 2. Follow-up 1 was excluded from this analysis. Control variables include dummy variables if the household received other support packages 3 weeks before each wave. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.6 Robustness

In this section, we demonstrate that the findings remain robust against other model specifications. In our first robustness check, we use a difference-in-difference regression instead of the fixed effects model (Table A4). In contrast to individual fixed effects estimates, difference-in-difference estimates enable the inclusion of time-fixed control variables to account for potential confounding factors. First, we run a difference-in-difference model on our balanced panel (see

Columns (1) and (2) in Table A4). Second, to further examine if systematic attrition may have influenced our results, we refrained from dropping all observations from households that did not respond to all interviews (Columns (3) and (4) in Table A4). This analysis complements the attrition analysis in Section 3. Third, to rule out potential biases through the ex-post exclusion of male-headed households from the sample and to test the validity of our results for the whole beneficiary population, we repeat the analysis with the unbalanced sample, including male-headed households (Columns (5) and (6) in Table A5). Fourth, we jointly test both post-treatment waves, accounting for wave-fixed effects in a difference-in-difference regression to examine overall treatment effects (Column (7) in Table A5). Under all specifications, the main results remain qualitatively similar and do not change the interpretation of our findings. The weak effects on health in the pre-shock period and theft experience in the post-shock period become statistically insignificant. Moreover, we observe a significant negative impact of the anticipatory cash transfer on mental health in the post-shock models. A component analysis indicates an increasing but insignificant impact of the transfer on various anxiety domains. The increase in mental health issues is potentially attributed to more awareness of the upcoming disaster and higher anticipation of its potential severity. All other effects remain unchanged.

To evaluate the potential impact of weighting derived from principal component analysis on our results, we conduct a sensitivity analysis by re-estimating the fixed effects regression on welfare outcome scores, excluding the weights from the analysis and, again, normalising from 0 to 1 (see Table A5 in the Appendix). Consistent with our earlier findings, we do not observe any treatment effect on food security. The slightly negative impact on mental health post-shock from the other robustness test remains. However, the immediate weak impact on health outcomes becomes statistically insignificant. The robustness tests underscore the strong and consistent treatment effect of the anticipatory cash transfer on shelter conditions (as measured by water leaks and the incidence of 'very bad' shelter conditions), both in the pre-shock and post-shock periods. The effect in mitigating severe flood impacts and reducing involvement in conflicts within the community remains strongly significant under all specifications.

5 Conclusion

We use an anticipatory one-off cash transfer provided to vulnerable households at the onset of an unprecedented flood in a forced displacement setting to estimate the pre-shock and post-shock intended and unintended outcomes, including food security, health and mental well-being as well as community cohesion impacts and test if the intervention helped households absorb the adverse impacts of the flood. We use three-wave balanced panel data from internally displaced households living in camps in Bentiu, South Sudan, a region affected by recurrent political and climatic volatility, which is partially isolated from regional and national markets during floods.

Our findings reveal that, on average, beneficiary households invested the cash to improve their shelter and living conditions. Our findings highlight the critical role of household-level capacity in implementing protective measures among vulnerable populations. Importantly, these shelter upgrades proved to be an essential step to resilience against the adverse impacts of the floods. Traditional impacts, such as improvements in food security or mental health, are absent immediately after the cash distribution and when the water had receded. However, six months after receiving one-off cash support, unexpectedly, beneficiaries experience less crime and violent conflict and are less likely to be displaced within the camps. We attribute these findings to the improved security and protection provided by shelter upgrades.

Our work contributes to the broad literature on cash transfers and to the more specific literature on anticipatory action, giving first insights into the outcome of social cohesion and from a setting that is already in severe crisis. With the escalating impacts of climate change, climatic shocks are becoming more frequent, which is particularly dangerous in highly vulnerable humanitarian settings like IDP camps. While the application of anticipatory assistance is rising, its effectiveness has been only vaguely studied to date. Our findings are novel and highly policy-relevant. Our study emphasises the importance of shelters in humanitarian settings, one of the few measures that can be taken in advance to mitigate the severe impacts of a climatic shock at the micro-level. This insight contributes to the ongoing debate on cash versus in-kind assistance, suggesting that in-kind shelter support may be both more cost-effective and more accessible in remote areas compared to cash transfers for individual purchases.

Three limitations in our study must be acknowledged. First, given the extreme levels of food insecurity and low health in the camp, combined with the multitude of assistance services provided to households, our outcome measures may not have been sufficiently sensitive to detect more subtle impacts of the anticipatory cash transfer. Second, by design, treatment and control households resided in the same communities and in the same camp blocks. This spatial proximity likely caused within-community spillovers, such as the informal sharing of resources, which may have attenuated the estimated treatment effects. These limitations, which we cannot address empirically in this study, might have reduced the probability of detecting the impacts of the small cash transfer. Third, the specificity of the anticipatory cash transfer and the unique crisis context restrict the external validity of our findings.

Future research could enhance external validity by systematically analysing underlying mechanisms and treatment effect heterogeneity. Such insights would help clarify how, for whom and under what conditions anticipatory assistance works. Moreover, comparing the effectiveness of anticipatory cash transfers to other modalities of anticipatory assistance would be of considerable policy relevance. While this study focuses on cash, the observed allocation of the transfer to building materials closely mirrors outcomes associated with in-kind shelter support, which might be more cost-effective. Additionally, it remains unclear whether larger cash transfers could enhance the long-term resilience of the households and what benefits stronger community cohesion may have for the lives and livelihoods of displaced people in this crisis region. In addition, future research could examine the role of labelling by comparing the effects of a labelled transfer to an unlabelled alternative, which might help disentangle behavioural responses and mitigate potential Hawthorne Effects. We did not explore the impact of varying the timing of the transfer. Given the anticipatory nature of the intervention, timing may be a critical determinant of its effectiveness and warrants further investigation. Future research could also screen for further (positive or negative) unintended consequences and investigate the longer-term dynamics of anticipatory cash transfers.

This study concludes with a key insight: Households living in extreme uncertainty strive for protection. Anticipatory cash transfers can play a crucial role in mitigating the adverse impacts of disasters while simultaneously enhancing living conditions and fostering social cohesion in crisis settings.

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A Appendix

[Figure A1]

[Figure A2]

[Figure A3]

[Figure A4]

[Figure A5]

[Figure A1]

[Figure A2]

[Figure A1]

[Figure A2]

[Figure A3]

[Figure A4]

[Figure A5]

Tables

Table A1: Differences between male- and female-headed households at baseline

	(1) Overall (Mean (SD)/Share)	(2) Female-headed HH (Coef (SE))
Treatment	49.2%	0.036** (0.016)
Shelter condition		
Very bad	49.8%	
Rather bad	10.5%	-0.099*** (0.029)
Rather good	26.6%	-0.077*** (0.025)
Very good	13.2%	0.045 (0.030)
Water leaks	22.5%	0.105*** (0.036)
Flood impact		
Floods - Mild impact (6 months)	21.2%	0.074 (0.091)
Floods - Severe impact (6 months)	78.0%	0.062 (0.091)
Welfare indices		
Food insecurity index	0.726 (0.292)	-0.011 (0.032)
Adverse health index	0.674 (0.297)	-0.007 (0.028)
Adverse mental health index	0.324 (0.268)	0.040 (0.034)
Social cohesion		
Displacement in camp (3 weeks)	26.1%	-0.095*** (0.024)
Conflict - Mild impact (6 months)	29.3%	0.050** (0.023)
Conflict - Severe impact (6 months)	16.7%	0.056** (0.028)
Theft - Mild impact (6 months)	18.9%	-0.022 (0.024)
Theft - Severe impact (6 months)	28.5%	-0.038* (0.022)
Observations	1011	1011
Block FE	–	Yes
R^2	–	0.107
P-value (F-Stat)	–	<0.001

Notes. The data is restricted to households that responded to all three interviews. Column (1) displays average values of outcome variables. Continuous variables are displayed by means with standard deviations in parentheses. Categorical variables are shown in percentages. Column (2) reports the coefficients from OLS regression on household head gender with standard errors in parentheses. HH=household, HHH=household head. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Differences between attrited and non-attrited households at baseline

	(1) Overall (Mean (SD)/Share)	(2) Attrition (Coef (SE))	(3) Attrition (Coef (SE))
Treatment	49%	0.037* (0.021)	0.021 (0.022)
Demographic and socioeconomic characteristics			
Age of HHH	39.9 (13.2)	0.001 (0.001)	0.001 (0.001)
Ratio of female adults	60.9%	0.019 (0.054)	0.065 (0.056)
Ratio of members with disabilities	14.7%	-0.063 (0.080)	-0.107 (0.082)
Respondent is HHH	98.2%	-0.003 (0.080)	0.015 (0.082)
Literacy of HHH	17.3%	0.024 (0.045)	0.040 (0.046)
Marital status (HHH):			
Divorced	0.1%	–	–
Married	89.1%	0.062 (0.357)	0.141 (0.363)
Widowed	10.8%	0.004 (0.358)	0.080 (0.364)
No. of members	9.5 (4.5)	-0.004 (0.007)	-0.003 (0.007)
Members < 18 years	6.3 (2.9)	0.004 (0.010)	0.002 (0.010)
Age of oldest child	9.0 (8.8)	0.001 (0.002)	-0.000 (0.002)
Owns phone	38.3%	-0.008 (0.023)	-0.008 (0.023)
Owns livestock	13.1%	0.090* (0.047)	0.072 (0.048)
Grows vegetables	26.0%	-0.026 (0.031)	-0.002 (0.031)
Assistance (3 weeks)			
WASH	43.9%	0.016 (0.033)	0.021 (0.034)
Dignity	21.7%	0.011 (0.034)	-0.005 (0.035)
Food	60.6%	0.054 (0.040)	0.035 (0.041)
Non-food items	34.9%	0.048 (0.032)	0.038 (0.032)
Shelter	19.2%	-0.045 (0.034)	-0.048 (0.034)
Any support	75.3%	-0.105** (0.051)	-0.102* (0.052)
Shelter condition			
Very bad	53.0%	–	–
Rather bad	9.3%	-0.041 (0.042)	-0.058 (0.043)
Rather good	24.1%	-0.026 (0.036)	-0.088*** (0.034)
Very good	13.7%	-0.071 (0.052)	-0.124** (0.052)
Water leaks	95.3%	0.015 (0.051)	-0.040 (0.053)
Flood impact			
Floods - Mild impact (6 months)	20.7%	-0.037 (0.129)	-0.015 (0.133)
Floods - Severe impact (6 months)	78.6%	-0.036 (0.128)	0.007 (0.132)
Welfare indices			
Food insecurity index	0.727 (0.299)	0.042 (0.045)	0.045 (0.047)
Adverse health index	0.679 (0.294)	-0.038 (0.040)	-0.038 (0.042)
Adverse mental health index	0.316 (0.264)	-0.044 (0.051)	-0.047 (0.051)
Social cohesion			
Displacement in camp (3 weeks)	25.2%	0.027 (0.035)	0.050 (0.035)
Conflict - Mild impact (6 months)	29.2%	-0.046 (0.032)	-0.020 (0.032)
Conflict - Severe impact (6 months)	15.8%	-0.027 (0.042)	-0.041 (0.042)
Theft - Mild impact (6 months)	19.9%	-0.010 (0.037)	-0.007 (0.038)
Theft - Severe impact (6 months)	27.0%	-0.017 (0.032)	-0.017 (0.032)
Observations	1113	1113	1113
Block FE R^2	–	0.161	0.047
P-value (F-Stat)	–	<0.001	0.020

Notes. Data restricted to female-headed households. Column (1) shows means and standard deviations. Columns (2) and (3) show OLS coefficients on attrition. HH=household, HHH=household head. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Baseline balance across camp blocks

	(1) Overall (Mean (SD)/Share)	(2) Treatment Coef (SE)
Camp A, Block 1	5.5%	–
Camp A, Block 2	5.1%	0.005 (0.101)
Camp A, Block 3	5.1%	-0.120 (0.101)
Camp A, Block 4	5.2%	-0.068 (0.100)
Camp A, Block 5	4.8%	-0.091 (0.102)
Camp A, Block 6	5.5%	-0.038 (0.099)
Camp A, Block 7	4.9%	0.008 (0.102)
Camp A, Block 8	5.5%	-0.058 (0.099)
Camp A, Block 9	4.6%	-0.093 (0.104)
Camp B, Block 1	4.2%	-0.133 (0.106)
Camp B, Block 2	5.2%	-0.088 (0.100)
Camp B, Block 3	4.5%	-0.082 (0.104)
Camp B, Block 4	4.8%	-0.091 (0.102)
Camp B, Block 5	4.2%	-0.058 (0.106)
Camp B, Block 6	6.1%	-0.119 (0.096)
Camp B, Block 7	4.8%	-0.113 (0.102)
Camp B, Block 8	4.0%	-0.163 (0.107)
Camp E, Block 1	1.3%	-0.141 (0.161)
Camp E, Block 2	1.9%	0.220 (0.137)
Camp E, Block 3	1.5%	0.014 (0.151)
Camp E, Block 4	1.4%	-0.096 (0.156)
Camp E, Block 5	1.4%	-0.019 (0.156)
Camp E, Block 6	1.2%	-0.012 (0.167)
Camp E, Block 7	0.8%	-0.183 (0.191)
Camp E, Block 8	0.4%	-0.308 (0.261)
Camp E, Block 9	1.3%	-0.141 (0.161)
Camp E, Block 10	1.1%	-0.258 (0.173)
Camp E, Block 11	1.0%	-0.113 (0.181)
Camp E, Block 12	0.6%	-0.391* (0.217)
Camp E, Block 13	0.8%	-0.308 (0.191)
Camp E, Block 14	0.1%	-0.558 (0.507)
Camp E, Block 15	1.3%	-0.058 (0.161)
Observations	942	943
R^2	–	0.024
P-value (F-Stat)	–	0.88

Notes. Column (1) displays average values of variables in percentage. Column (2) reports the coefficients from OLS regression on treatment with standard errors in parentheses. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Robustness tests: Difference-in-Differences models

	(1) Pre-shock	(2) Post-shock	(3) Pre-shock	(4) Post-shock	(5) Pre-shock	(6) Post-shock	(7) Post-treatment
Water leaks in shelter	-0.714*** (0.026)	-0.083*** (0.028)	-0.725*** (0.025)	-0.069*** (0.026)	-0.718*** (0.024)	-0.048* (0.026)	-0.076*** (0.029)
Very bad or rather bad shelter cond.	-0.546*** (0.039)	-0.067 (0.041)	-0.540*** (0.036)	-0.073* (0.038)	-0.537*** (0.035)	-0.083** (0.037)	-0.064 (0.042)
Very bad shelter cond.	-0.48*** (0.038)	-0.131*** (0.043)	-0.480*** (0.036)	-0.146*** (0.040)	-0.467*** (0.035)	-0.136*** (0.039)	-0.130*** (0.043)
Food insecurity	-0.005 (0.026)	-0.008 (0.024)	-0.021 (0.024)	-0.015 (0.022)	-0.030 (0.023)	-0.022 (0.021)	-0.009 (0.024)
Adverse health	-0.042 (0.028)	-0.018 (0.027)	-0.035 (0.026)	-0.008 (0.025)	-0.041 (0.025)	-0.013 (0.024)	-0.012 (0.027)
Adverse mental health	0.017 (0.024)	0.038* (0.023)	0.011 (0.022)	0.035* (0.021)	-0.001 (0.021)	0.031 (0.020)	0.044* (0.023)
Floods (any)		-0.066** (0.033)		-0.052* (0.031)		-0.064** (0.029)	
Floods (severe)		-0.103*** (0.039)		-0.068* (0.037)		-0.086** (0.035)	
Displacement in camp		-0.046 (0.030)		-0.046* (0.028)		-0.057* (0.030)	
Conflict (any)		-0.102** (0.043)		-0.089** (0.041)		-0.090** (0.039)	
Conflict (severe)		-0.028 (0.029)		-0.025 (0.027)		-0.029 (0.026)	
Theft (any)		-0.057 (0.042)		-0.023 (0.039)		-0.023 (0.037)	
Theft (severe)		0.003 (0.039)		0.020 (0.037)		0.027 (0.036)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Balanced panel	Balanced panel	Imbalanced panel	Imbalanced panel	Imbalanced panel incl. Male HHH	Imbalanced panel incl. Male HHH	Balanced panel

Notes. Difference-in-differences estimates with cluster-robust standard errors in parentheses. Welfare indicators are based on weights from principal component analysis and range from 0-1. For displacement within the camp, a time frame of 3 weeks is covered. Other shock outcomes cover a timeframe of 6 months before Wave 1 and 4 months before Wave 3. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Control variables include dummy variables if the household received other support packages 3 weeks before each wave, the camp block, the age of the household head, the ratio of female and disabled household members, the household size, if they own a phone.

Table A5: Robustness tests: Welfare outcomes without weights

	(1)	(2)	(3)	(4)	(5)	(6)
	Adverse food security		Adverse health		Adverse mental health	
	Pre-shock	Post-shock	Pre-shock	Post-shock	Pre-shock	Post-shock
Baseline mean	0.733		0.703		0.367	
(SD)	(0.268)		(0.277)		(0.243)	
Impact	-0.008	-0.020	-0.036	-0.011	-0.011	0.039*
	(0.022)	(0.021)	(0.026)	(0.025)	(0.018)	(0.021)
Observations	1884	1884	1884	1884	1884	1884
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.091	0.127	0.037	0.041	0.074	0.115
P-value (F-Stat)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001

Notes. Two-way fixed effects estimations with cluster-robust standard errors in parentheses. Outcomes range from 0-1. Control variables include dummy variables if the household received other support packages 3 weeks before each wave. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figures

Figure A1

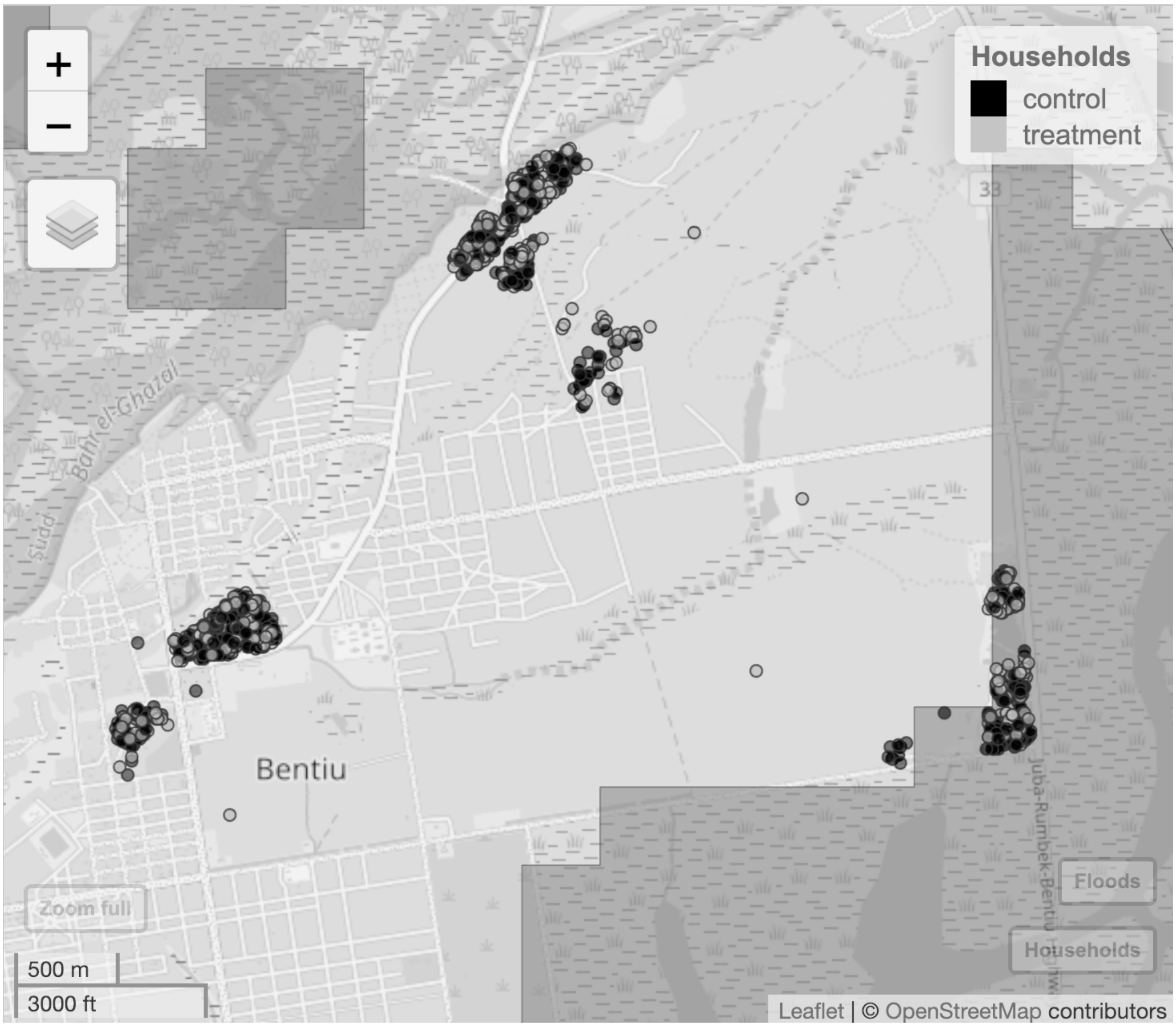


Figure A2

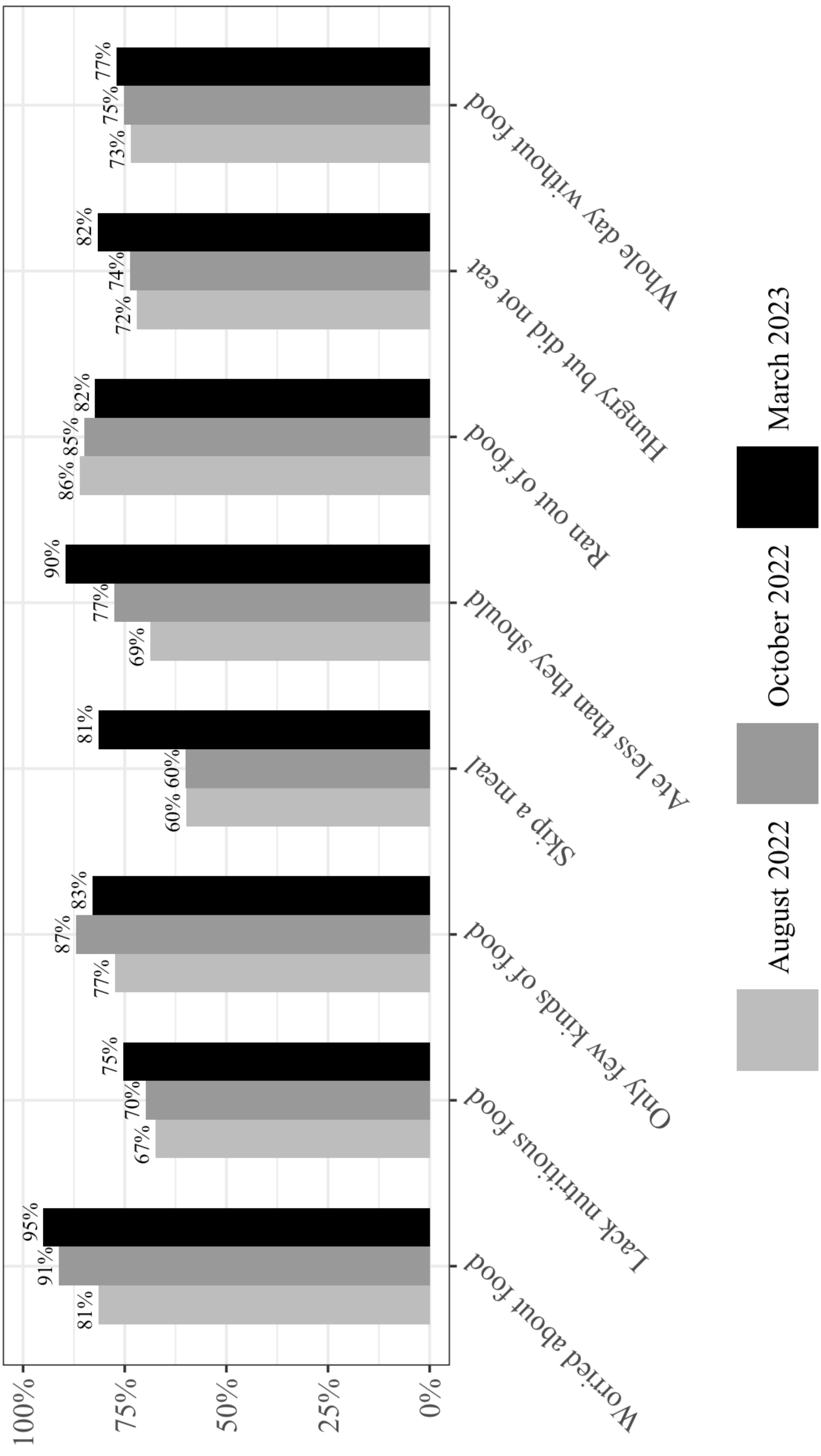

August 2022
October 2022
March 2023
Worried about food
Lack nutritious food
Only few kinds of food
Skip a meal
Ate less than they should
Ran out of food
Hungry but did not eat
Whole day without food
0%
25%
50%
75%
100%
81% 91% 95%
67% 70% 75%
77% 87% 83%
60% 60% 81%
69% 77% 90%
86% 85% 82%
72% 74% 82%
73% 75% 77%

Figure A3

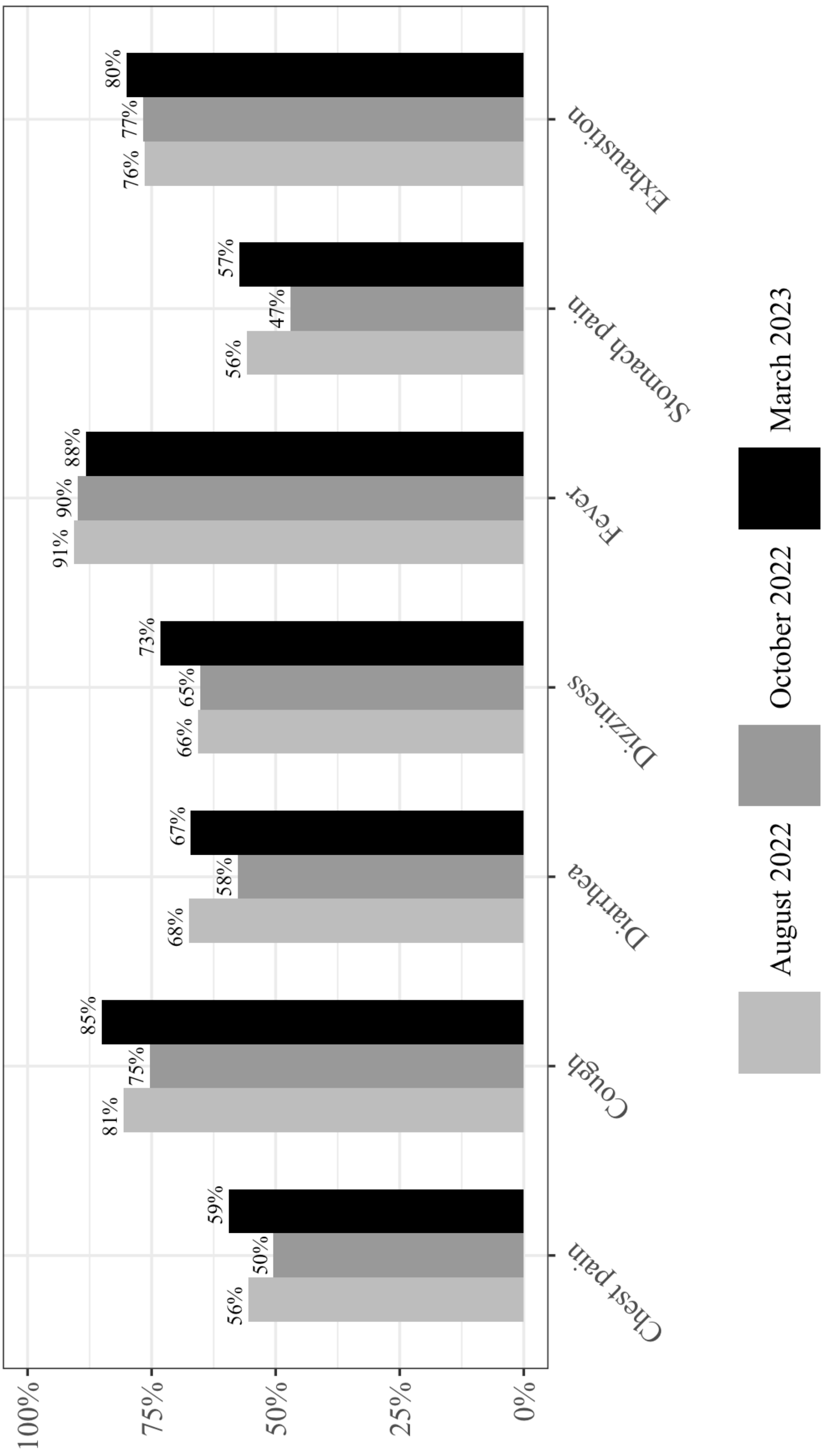

100%
75%
50%
25%
0%
Chest pain
Cough
Diarrhea
Dizziness
Fever
Stomach pain
Exhaustion
56%
50%
59%
81%
75%
85%
68%
58%
67%
66%
65%
73%
91%
90%
88%
56%
47%
57%
76%
77%
80%
August 2022
October 2022
March 2023

Figure A4

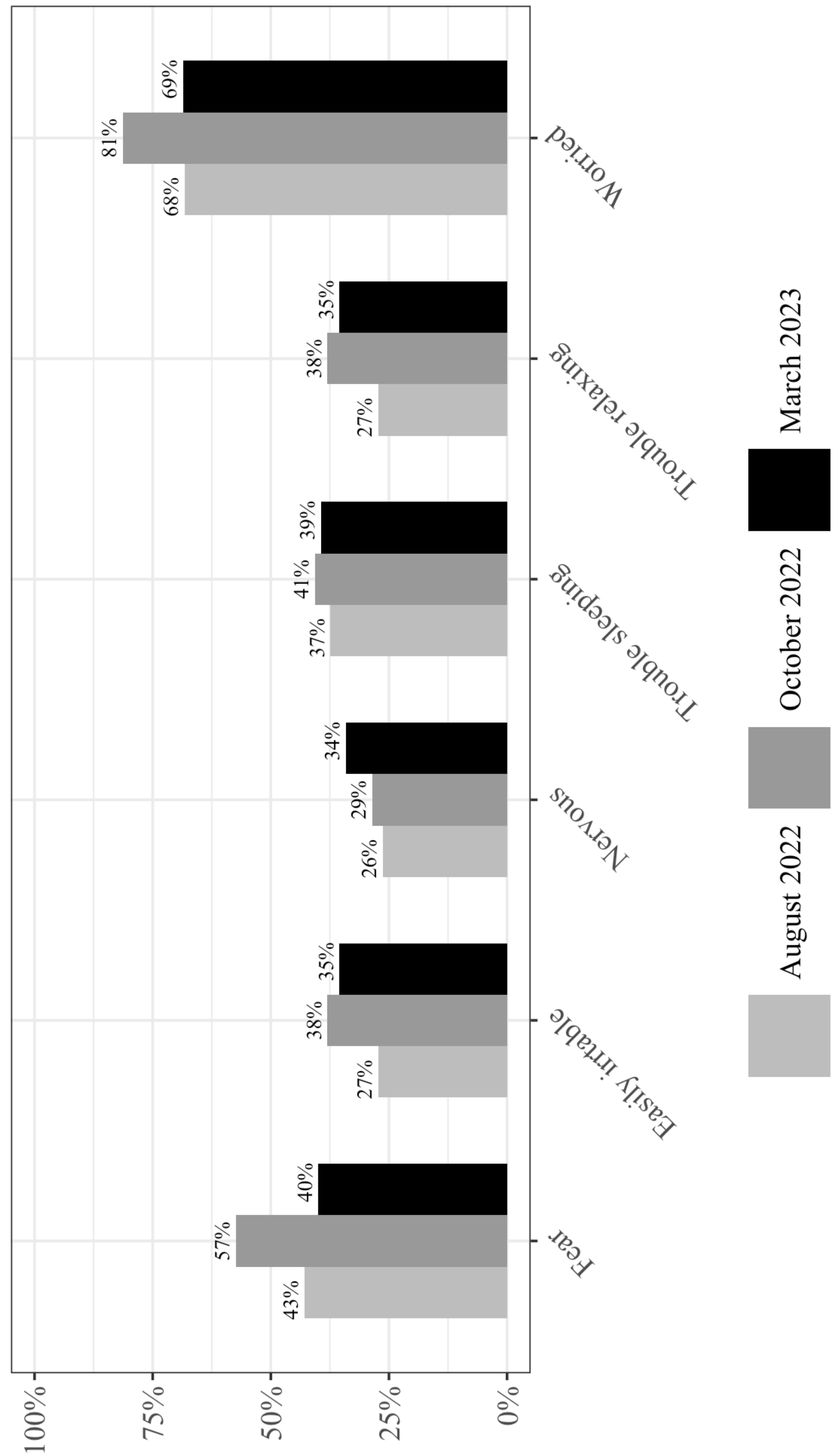
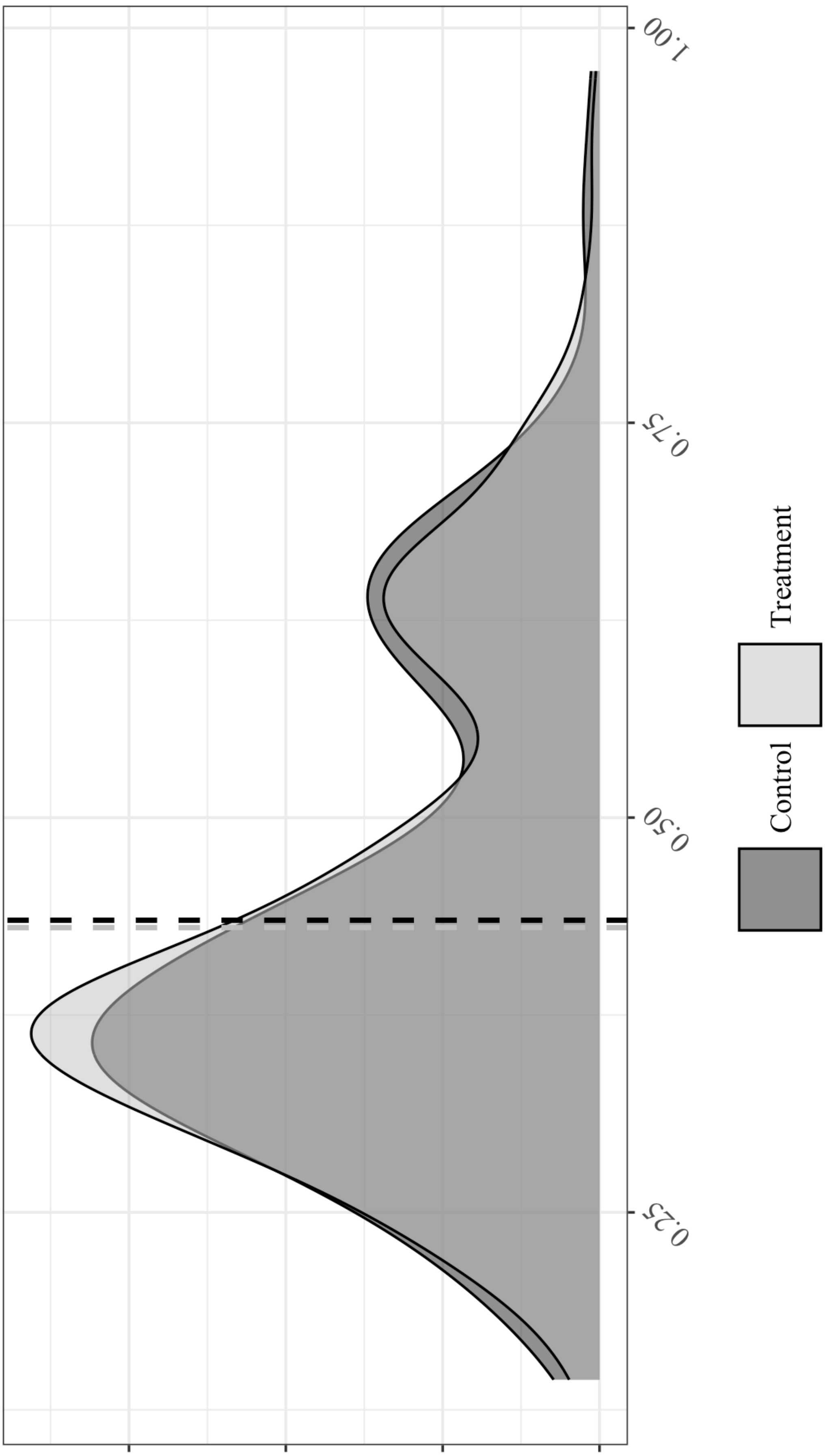

Worried
Trouble relaxing
Trouble sleeping
Nervous
Easily irritable
Fear
68%
81%
69%
27%
38%
35%
37%
41%
39%
26%
29%
34%
27%
38%
35%
43%
57%
40%
100%
75%
50%
25%
0%
August 2022
October 2022
March 2023

Figure A5



Captions

Figure A1

Title. Flood exposure in the campsites using satellite images

Notes. Intersection of sample households and maximum flood water extent between 12.01.2023 and 16.01.2023 (dark grey squares) (United Nations Satellite Center, n.d.).

Figure A2

Title. Food insecurity levels across waves using the FIES domains

Notes. Figure shows the percentages of households indicating that they have experienced the corresponding FIES domain in the past three weeks, disaggregated by wave.

Figure A3

Title. Health levels across waves

Notes. Figure shows the percentages of households indicating that they have experienced the corresponding health issue in the past two weeks, disaggregated by wave.

Figure A4

Title. Mental health issues faced on at least half of the days across waves

Notes. Figure shows the percentages of households indicating that they have experienced the corresponding mental health issue at least half of the days in the past two weeks, disaggregated by wave.

Figure A5

Title. Distribution of vulnerability score based on selection criteria by control and treatment group

Notes. Vulnerability score is based on the normalised age of the household head, the ratio of female household members and the ratio of household members with disabilities at Baseline