Aid at the Margins: Timing, Scale, and Targeting in Refugee Camps



Vittorio Bruni
Green Templeton College
School of Anthropology and Museum Ethnography
Centre on Migration, Policy and Society
University of Oxford

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Supervisor and co-supervisor: Carlos Vargas-Silva; Isabel Ruiz

Abstract

Humanitarian assistance is a central feature of economic life in refugee camps. Yet, since 2023, shifts in donor commitments have sharply reduced aid budgets, leading to substantial changes in aid design, including delivery timing, transfer size, modality, and allocation rules. The effects of these changes are both practically relevant and theoretically ambiguous.

This thesis studies how variation in the timing, scale, modality, and allocation of humanitarian aid affects welfare, credit, prices, and social relations in refugee camp economies. Focusing on Kakuma refugee camp and the adjacent Kalobeyei settlement in Kenya, the analysis combines high-frequency household panel data, weekly market price data, and extensive qualitative interviews collected between 2022 and 2025.

The first analytical chapter examines the effects of aid distribution and delays in aid delivery. Exploiting high-frequency surveys with random interview timing, it shows that refugee households smooth consumption under regular aid cycles but experience sharp declines in food consumption and food security when transfers are delayed. Informal credit through local retailers partially mitigates these shocks, but at high cost, with prices for credit-based purchases around 17 percent higher.

The second chapter studies the short-run consequences of a large, unanticipated 20% cut in humanitarian assistance. Exploiting exogenous variation of interview dates around the cut-off, it shows that the aid reduction leads to substantial declines in calorie intake, dietary diversity, food security, and expenditures, alongside deteriorations in mental well-being and shifts in risk and time preferences. The shock also triggers a contraction in informal credit and food prices.

The final chapter examines the shift from universal aid provision to a vulnerability-based targeting approach. Drawing on qualitative evidence, it shows that targeting in a context of widespread poverty and aid dependency is associated with extensive exclusion errors, disruption of informal credit and mutual support systems, erosion of trust in humanitarian institutions, and contributes to insecurity, distress-driven migration, and economic contraction. In these contexts, the social and administrative costs of vulnerability targeting outweigh its perceived efficiency gains.

Overall, the findings suggest that different design choices along any of the margins studied here: timing, scale, modality, or targeting, have far-reaching economic consequences, and they can amplify welfare losses by simultaneously reducing income and dismantling key coping mechanisms. The thesis contributes to theories of intertemporal choice, general equilibrium, and informal credit, and highlights the importance of predictability and universality in highly aid-dependent settings.

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Chapter 1

Introduction

In a context of rising forced displacement and tightening humanitarian budgets, refugee camps have become enduring features of the global landscape. Originally conceived as temporary responses to crisis, many camps have evolved into long-term settlements in which millions of people attempt to live a life in normalcy under severe economic, environmental, and legal constraints (UNHCR 2004; UNHCR, The UN Refugee Agency 2024). Beyond the immediate hardships of displacement, camps are also sites of active economic organization, encompassing formal and informal markets, credit relationships, and social institutions (Betts et al. 2024). Understanding how these camp economies function, particularly in a period of shrinking humanitarian resources, is therefore not only an academic concern, but a pressing policy challenge.

By 2024, more than 300 million people worldwide required humanitarian assistance, yet less than half of global funding requirements were met (Humanitarian Action 2024). In 2025, the withdrawal of the United States from humanitarian commitments reduced global humanitarian financing by approximately 43 percent (Pearson 2025). Refugees living in camps, around 23 million globally, are among the most exposed to these funding shocks (UNHCR 2025). Camp economies are typically remote, poorly integrated with national markets, and heavily dependent on regular aid transfers that sustain consumption, informal credit, and trade (Werker 2007; Betts et al. 2024). In these contexts, when humanitarian assistance is delayed, reduced, or reallocated, the consequences extend beyond individual welfare to affect markets and social relations.

Research in migration and refugee studies has long emphasized that refugee camps function as distinct economic systems in which humanitarian assistance shapes markets and social relations (Harrell-Bond 1986; Betts et al. 2014, 2017). By contrast, the development economics literature has largely analysed humanitarian assistance and cash transfers as discrete welfare interventions, typically focusing on household-level outcomes and examining changes in transfer size, timing, or modality separately (Bastagli et al. 2019). Moreover, most empirical evidence in this literature concerns the expansion of support, rather than unexpected disruptions or withdrawals. As a result, we lack a coherent understanding of how different design features of humanitarian assistance interact, and how unexpected shocks in aid distributions affect welfare and markets. This thesis addresses this gap by integrating insights from both literatures and asking: How do changes in the timing, scale, modality, and allocation rules of humanitarian assistance impact household welfare, informal credit, prices, and social relations in refugee camp economies?

To address these questions, I study Kakuma refugee camp and the adjacent Kalobeyei settlement in Kenya, one of the largest refugee complexes in the world. At the start of data collection in September 2022, the combined population of these camps exceeded 250,000 refugees (Pape and Beltramo 2021; UNHCR 2021), and humanitarian assistance accounted for approximately 68 percent of refugees' monthly income. Food assistance was primarily provided by the World Food Programme (WFP) and amounted to roughly $17 per person per month. Aid delivery varied by location, with refugees receiving assistance either as unrestricted cash transfers or as a combination of restricted cash transfers and in-kind food rations.

During the study period, both restricted and unrestricted cash transfers, intended to be delivered monthly, were subject to recurrent delays, with delay lengths ranging from a few days to more than three weeks. These features provide the basis for the first analytical chapter, which examines how the aid cycle and unexpected delays affect household consumption, informal credit, and prices.

In July 2023, following funding shortfalls, WFP implemented a sudden and uniform 20% reduction in food assistance, lowering the monthly transfer value to around $13 per person. The second analytical chapter exploits this negative income shock to study the welfare, behavioural, credit, and market consequences of this large, unanticipated aid cut.

Finally, in August 2025, amid further budget pressures, WFP replaced the long-standing universal distribution system with a vulnerability-based targeting regime. Refugees were classified into four categories based on assessed vulnerability, with the majority placed in the least vulnerable categories and receiving no assistance. The third analytical chapter analyses this institutional shift to assess how changes in allocation rules reshape coping strategies, informal insurance, and social relations.

To study these questions, the thesis employs a mixed-methods research design combining high-frequency quantitative household data, longitudinal price data, and qualitative interviews collected in Kakuma and Kalobeyei between 2022 and 2025.

The core quantitative dataset consists of a high-frequency panel of 622 South Sudanese refugee households interviewed ten times over a twelve-month period and comprising 6,220 interviews in total. Interviews were conducted within randomly assigned three-day windows which varied across survey waves. This design feature generated quasi-experimental variation between interview dates and aid distribution, delays, and reductions.

These data are paired with weekly price surveys covering 70 food and beverage items across markets in all camps and settlements, enabling the analysis of market-level and general-equilibrium responses.

Quantitative findings are triangulated with over 270 qualitative interviews with refugees, shopkeepers, wholesalers, and humanitarian personnel, as well as a high-frequency food security journaling exercise. Together, this design allows the thesis to identify household- and market-level effects of changes along the margins of aid design, including its timing, size, modality and allocation rules.

The thesis makes four contributions to the literature. First, it bridges migration and refugee studies with development economics by treating humanitarian assistance not as a discrete welfare transfer, but as a systemic feature of refugee camp economies that structures behaviour, markets, and institutions (Harrell-Bond 1986; Jacobsen 2005; Werker 2007; Betts et al. 2014).

Second, it contributes to theories of intertemporal choice under extreme vulnerability [1] by showing how the timing and scale of regular income transfers interact with liquidity and credit constraints, speaking to the Permanent Income Hypothesis, buffer-stock models, and behavioural approaches to consumption smoothing (Friedman 1957; Hall 1978; Deaton 1991; Carroll 1997).

Third, it extends the general-equilibrium literature on cash and in-kind transfers by analysing how delays and sudden reductions in humanitarian assistance operate as negative demand shocks[2], and how price adjustments attenuate but do not neutralize welfare losses (Angelucci and De Giorgi 2009; Cunha et al. 2019; Egger et al. 2022). It also shows that informal credit in camp economies is endogenously constructed around aid modality, size, and predictability, implying that aid cuts dismantle key coping mechanisms rather than merely reducing household income (Townsend 1994; Fafchamps and Lund 2003; Angelucci 2015).

Fourth, the thesis contributes to the literature on universal versus targeted approaches in aid delivery. Building on and extending work on the economic, social, and administrative costs of targeting (Devereux et al. 2015; Devereux 2016; Grosh et al. 2022; MacAuslan and Riemenschneider 2011), it shows that in contexts of widespread poverty and high aid dependence, fine-grained targeting can generate welfare losses that outweigh its intended efficiency gains.

[1] e.g. no freedom of movement, no right to work, living in displacement, fragile livelihood, and credit constraints.

[2] Reductions and delays in aid can be understood as negative demand shocks because their immediate effect is to reduce beneficiaries' purchasing power and consumption expenditure. In settings where assistance constitutes a large share of household income, delays or cuts translate mechanically into a contraction in aggregate demand.

The thesis proceeds as follows. After outlining the study context, data, methods, and theoretical contributions, three analytical chapters examine distinct dimensions of humanitarian aid design. The first chapter analyses the household- and market-level effects of the aid cycle and unexpected delivery delays. The second chapter studies the welfare and general-equilibrium consequences of a large, unanticipated aid cut. The third chapter examines the effects of shifting from universal to vulnerability-based targeting. The thesis concludes with a synthesis of findings, followed by policy implications, caveats and limitations, and directions for future research.

1.1 Context

Since 1990, Kenya has become a major refugee-hosting country (see Figure 1). Under a strict encampment policy, most refugees have been concentrated in two large camp complexes: Dadaab, near the Somali border, and Kakuma, close to the borders with Uganda and South Sudan. Although the distribution of refugees across sites and urban areas has fluctuated over time, Kakuma has consistently remained one of the country's principal refugee hubs. As of January 2026, approximately 300,000 of the roughly 600,000 refugees in Kenya were residing in Kakuma and the adjacent Kalobeyei settlement (UNHCR 2025a).

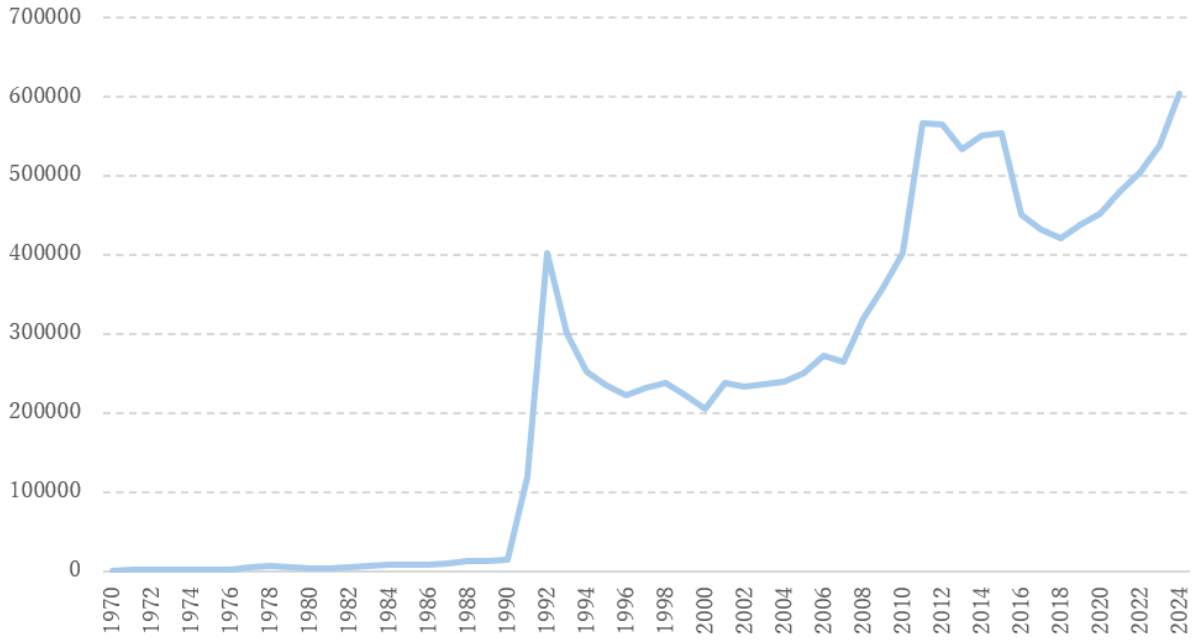
This thesis focuses on Kakuma refugee camp and the adjacent Kalobeyei settlement in Turkana County, northwestern Kenya. The two sites are located in a remote, arid region of northern Kenya characterized by weak infrastructure and limited economic integration with the rest of the country (Jansen 2018).

Kakuma refugee camp was established there in 1992 following displacement linked to civil war in Sudan, and its population has grown steadily over the subsequent decades. After several camp extensions, in 2016, the Government of Kenya and UNHCR opened the Kalobeyei settlement, a few kilometres west of Kakuma (MacPherson and Sterck 2021). [3] Unlike earlier extensions of Kakuma, Kalobeyei was established under a dis-

[3]Due to their close proximity, shared environment, and administration by the same authorities, Kakuma and Kalobeyei are often treated as a single, unified camp.

tinct policy framework intended to promote self-reliance and local development, moving beyond a strict encampment model. It was conceived as an integrated settlement under the Kalobeyei Integrated Socio-Economic Development Programme (KISEDP), with the explicit objective of fostering economic interaction and shared service provision between refugees and host communities in Turkana County (Fellesson 2023; UNHCR, The UN Refugee Agency 2023). In practice, two features most clearly distinguished Kalobeyei from Kakuma: housing and aid modality. Refugees were supported in constructing permanent brick dwellings, and assistance was delivered primarily through cash transfers rather than in-kind food rations.

Figure 1: Number of refugees in Kenya by year (1970 - 2024)

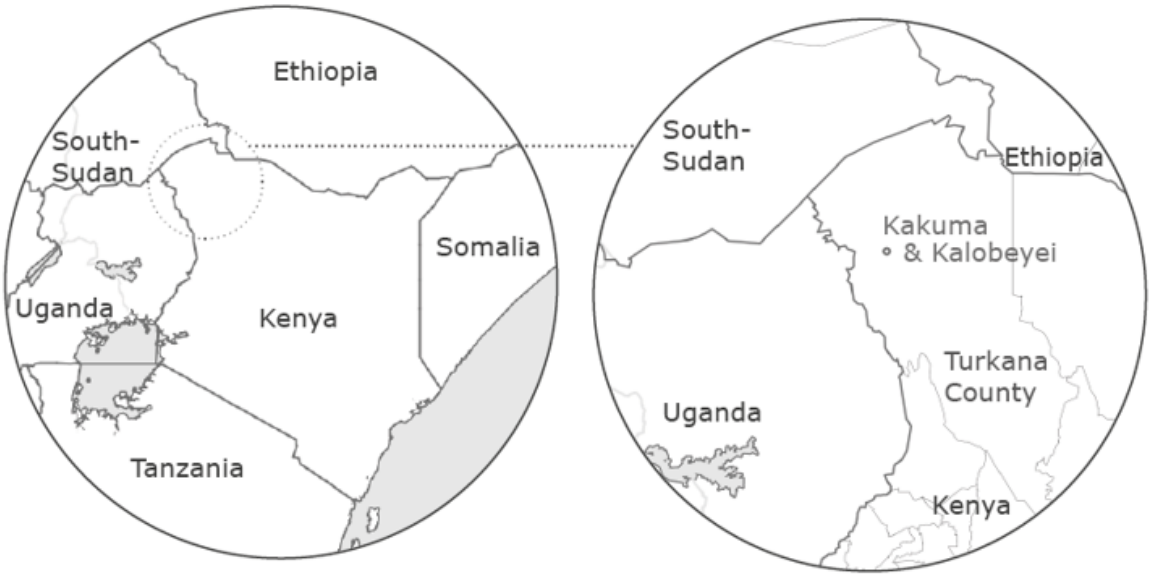


Data source: World Bank 2026

By 2022, the combined population of Kakuma and Kalobeyei exceeded 250,000 refugees, making it the third-largest refugee camp in the world and accounting for approximately 40% of Kenya's refugee population (Pape and Beltramo 2021; UNHCR 2021). In 2022, the largest share of refugees originated from South Sudan (around 50%), followed by refugees from the Democratic Republic of the Congo (DRC) (16%) and Somalia (10%).[4]

[4]Unless otherwise stated, descriptive statistics in this section are based on data collected as part of this DPhil project.

Figure 2: Map of Kenya



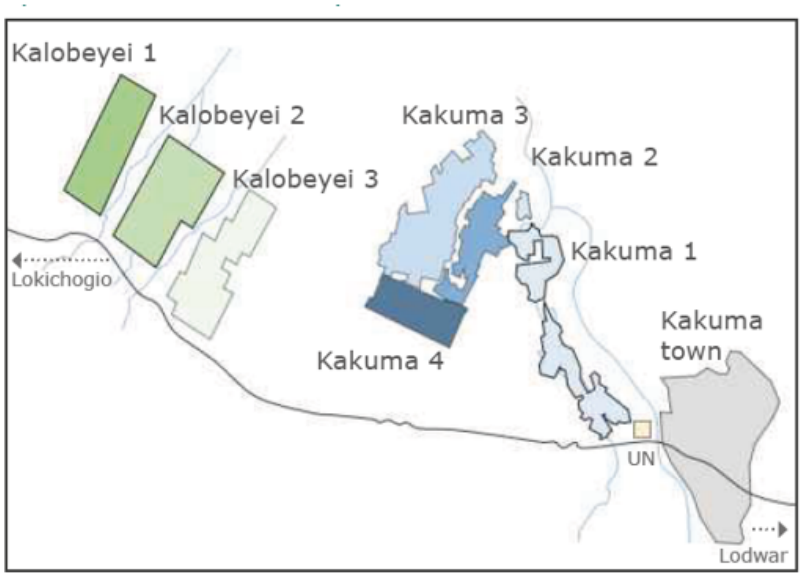
Economic conditions in Kakuma and Kalobeyei are shaped by limited employment opportunities, heavy reliance on humanitarian assistance, and high levels of household debt (Bruni et al. 2024). Kenya's encampment policy restricts refugees' freedom of movement and access to formal employment, resulting in very low employment rates and widespread informality. Only around 15% of adults report working for an income, and these activities are typically casual, low-paid, and unstable. Wages vary by nationality and type of work, but average monthly earnings are below $50. Economic opportunities are further constrained by the harsh, arid climate and limited access to water, which restrict agricultural activity: only about 8% of households engage in farming.

As a result, food insecurity is widespread, with approximately 83% of households classified as food insecure, and only 31% meeting the recommended daily caloric intake of 2100 kcal per person. Humanitarian assistance represents around 68% of refugees' monthly income. In this context, informal debt is a common consumption smoothing strategy: nearly 90% of refugee households are informally indebted to a food retailer. When households face income shortfalls or sudden shocks, they use the aid cards on which humanitarian assistance is delivered as a guarantee to obtain food on credit. While this arrangement allows households to smooth consumption in the short term, it comes at a

cost, as food purchased on credit carries implicit interest rates of around 17%.

While Kakuma and Kalobeyei are jointly administered by the Kenyan government and UNHCR, the World Food Programme (WFP) is the primary provider of humanitarian aid in the form of food assistance (Betts et al. 2020). At the beginning of the data collection period, refugees received food aid through three main modalities, determined by whether they were registered in the original Kakuma camp or in the newer Kalobeyei settlement. Households registered in Kakuma received a combination of restricted cash transfers of approximately $7 per person per month and in-kind food rations amounting to about 9 kg per person per month. By contrast, refugees registered in Kalobeyei received unrestricted cash transfers of approximately $17 per person per month. In both locations, food assistance was intended to cover roughly 80% of minimum caloric requirements.

Figure 3: Map of Kakuma and Kalobeyei



Food assistance in Kakuma and Kalobeyei is delivered according to regular distribution cycles that influence household economic life within the camps. Transfers are intended to be provided on a monthly basis, with households typically planning consumption, borrowing, and expenditures around expected disbursement dates. Both restricted and unrestricted cash transfers are delivered electronically through SIM cards or bank cards, depending on the modality, while in-kind food rations are distributed the first week

of each month at three distribution centers within the camp (Siu et al. 2023; Delius and Sterck 2024).

Cash transfers in both Kakuma and Kalobeyei are subject to frequent delays. Throughout the study period, disbursements were delayed every month. For the unrestricted cash programme, delays averaged 12 days, with a range of 5 to 22 days. Delays were even longer for the restricted cash programme, averaging 16 days and ranging from 5 to 23 days.

Beyond delays, humanitarian assistance in Kakuma and Kalobeyei has been characterized by substantial volatility in its overall level. Over the past decade, funding shortfalls and changing donor priorities have led to repeated adjustments in ration sizes and transfer values. During the period covered by this thesis, these dynamics culminated in a reduction in assistance: in July 2023, food aid to refugees in both Kakuma and Kalobeyei was reduced by approximately 20%. This reduction applied uniformly across beneficiaries.

Even after the July 2023 aid cut, food assistance in Kakuma and Kalobeyei was distributed on a universal basis, with all registered refugees receiving the same levels of support. Yet, in response to ongoing funding constraints, this approach changed in 2025, when humanitarian agencies introduced a vulnerability-based targeting system in Kakuma and Kalobeyei. Under this new regime, refugees were classified into four categories based on assessed vulnerability. Categories one and two, deemed as the most vulnerable, received $8 and $4 dollars' worth of assistance respectively. No support at all was given to categories 3 and 4, comprising a total of 135,500 refugees. This shift marked a fundamental change in the allocation rules governing humanitarian assistance in the camp, replacing the long-standing universal approach with a differentiated system.

These features make Kakuma and Kalobeyei a well-suited setting for examining the economic and social consequences of humanitarian aid design. The central role of humanitarian assistance in shaping economic life makes the context particularly relevant to study how changes in aid design translate into changes in household resources and market

conditions. In addition, variation in aid modalities across space and over time, combined with recurrent delays, cuts, and institutional reforms, allows different dimensions of aid provision to be examined within a single, coherent setting. This makes it possible to analyze humanitarian assistance not as a static transfer, but as a feature of camp economies shaping welfare, markets, and social relations. Importantly, these characteristics are not unique to Kakuma, but are shared by many large refugee camps where livelihoods are constrained and humanitarian assistance plays a central economic role.

1.2 Methods

This section provides an overview of the data and methodological approach used in this thesis. More detailed discussions of methods, identification strategies, and limitations are provided in each analytical chapter, while a concise overview can be found in Table 1.

The analysis in this thesis draws on a mixed-methods research design combining high-frequency quantitative household data, longitudinal price data, and in-depth qualitative interviews collected between September 2022 and October 2025 as part of the Vulnerability, Shocks, and Livelihoods project implemented by the Refugee Studies Centre at the University of Oxford.[5] The use of multiple data sources allows the thesis to examine how humanitarian aid affects welfare, credit, and markets in Kakuma refugee camp from complementary perspectives. By combining high-frequency quantitative data with qualitative evidence, the analysis triangulates findings across methods and provides deeper insight into the mechanisms linking aid design to economic and social outcomes.

The main analysis of aid impacts on refugee households is based on a representative sample of 622 South Sudanese refugee households. Respondents were interviewed ten

[5]I was hired as a research assistant for this project to support the team before, during, and after data collection. Before data collection I engaged actively in the conceptualization and refinement of research ideas, contributed to the formulation of key hypotheses and designed the questionnaire. During data collection, I spent 4.5 months in Kakuma refugee camp and managed a 35-person enumerator team. In this capacity, I executed the research plan by selecting, training, and managing enumerators, coordinating fieldwork, managing the research budget, and conducting data quality checks. After the completion of fieldwork, I contributed to the analysis by cleaning the dataset, creating new variables for analysis, and implementing statistical analysis. Finally, I contributed to the writing of the three chapters contained in this document including drafting, editing and reviewing all sections.

Table 1: Overview of data collection activities

Data collection activity	Period	Length (months)	Number of observations
Preliminary qualitative phase	September 2022	1	70
Household survey	September 2022 - October 2023	12	6220
Price survey	September 2022 - October 2023	12	16770
Second qualitative phase	October 2023 - November 2023	2	134
Final qualitative phase on targeting	August 2025 - October 2025	3	70

times between October 2022 and September 2023, yielding 6,220 household interviews in total. Each survey wave lasted exactly four weeks. For each survey wave, interview dates were randomly assigned within a three-day window. Throughout the year of data collection, survey breaks were introduced to create further variation in interview timing. This design generates quasi-experimental variation between household interviews and aid delivery, delays, and cuts. This design allows us to study intra-month and inter-month changes in consumption, food security, debt, and selected behavioural outcomes, as well as responses to unanticipated shocks such as aid delays or reductions. Interviews were conducted with the household member primarily responsible for food preparation, allowing for detailed and consistent measurement of consumption and food security indicators.

The household survey collected multidimensional information on consumption, food security, expenditure, and income sources. Additional modules captured data on assets, and debt. At the individual level, the survey included measures of time preferences, risk attitudes, cognitive outcomes, and mental well-being, allowing the analysis to explore how changes to aid interact with behavioral and psychological dimensions of well-being.

Second, the thesis incorporates longitudinal price data collected in parallel with the household survey, between September 2022 and October 2023. Weekly prices for 70 food and beverage items were recorded across markets in the four camps of Kakuma and the

three villages of Kalobeyei. These data are used to examine market-level responses to changes in humanitarian assistance and to identify general-equilibrium effects related to aid volatility.

Finally, the analysis draws on in-depth qualitative interviews with refugees, shopkeepers, wholesalers, and humanitarian staff collected in three phases. The first phase, conducted in September 2022, consisted of 70 in-depth interviews and informed the design of the quantitative survey instruments. The second phase, carried out in October–November 2023, included 134 semi-structured interviews and was used to contextualize and deepen the interpretation of quantitative results. A final phase of qualitative data collection between August and October 2025 comprised more than 70 semi-structured interviews with refugees, and forms the basis of the third analytical chapter on vulnerability-based targeting.

1.3 Contribution to the literature

A rich body of work in migration and refugee studies has documented how camps function as distinct economic systems in which aid plays a central role (Harrell-Bond 1986; Jacobsen 2005; Werker 2007; Betts et al. 2014, 2017). However, in the development economics literature, humanitarian assistance and related cash transfer programs are most often analysed as discrete welfare transfers. Despite a few notable exceptions (e.g. Cunha et al. 2019; Egger et al. 2022), this research tends to focus on aid impacts on consumption, food security, and poverty outcomes rather than on broader systemic effects on households, markets, and informal institutions (Bastagli et al. 2019). The aim of this thesis is to bridge the gap between the two disciplines by treating aid design as a systemic feature of refugee camp economies and linking it to canonical theories of household consumption, behaviour, and market dynamics.

Specifically, I decompose humanitarian assistance into four interrelated design dimensions: timing (predictability and delays), scale (transfer size and cuts), modality (cash, restricted cash, and in-kind), and allocation rules (universal versus vulnerability-based

targeting); and examine how variation along each margin propagates through household consumption, informal credit, and local prices within a single aid-dependent economy. In doing so, the thesis speaks directly to core debates in development economics: tests the Permanent Income Hypothesis and buffer-stock models of consumption smoothing (Friedman 1957; Hall 1978; Deaton 1991; Carroll 1997); behavioural models of intertemporal choice under scarcity (Strotz 1956; Laibson 1997; Gabaix and Laibson 2017); general-equilibrium effects (Angelucci and De Giorgi 2009; Cunha et al. 2019; Egger et al. 2022); and the endogenous formation of informal credit and insurance arrangements (Townsend 1994; Fafchamps and Lund 2003).

Taken together, these contributions advance a more integrated conceptualization of aid-dependent economies in which each design choice of humanitarian assistance has significant theoretical and economic implications.

A central contribution to the literature of the thesis concerns how the timing and size of income transfers interact with intertemporal choice under extreme vulnerability. Canonical models of consumption smoothing, most notably the Permanent Income Hypothesis (PIH), predict that households allocate consumption based on expected lifetime income, implying that temporary fluctuations in income should have limited consumption effects when income levels are unchanged (Friedman 1957; Hall 1978). At the other extreme, behavioural models featuring present bias, temptation, or limited attention predict limited smoothing even in the absence of shocks, with households behaving in a hand-to-mouth manner in the most extreme models (Strotz 1956; Laibson 1997; Gul and Pesendorfer 2001; Banerjee and Mullainathan 2010; Gabaix and Laibson 2017; Kaplan and Violante 2022; Gelman 2022). Buffer-stock models emphasize the role of liquidity, savings, and credit constraints in shaping partial smoothing responses to income shocks (Deaton 1991; Carroll 1997).

This thesis advances these debates by exploiting humanitarian aid as a setting in which income is regular but often unpredictably delayed or reduced. This allows a clean separation between changes in income levels and changes in income timing. By jointly

analysing scheduled aid cycles, unexpected delays, and sudden cuts, the thesis shows that predictability is a determinant of consumption smoothing in aid-dependent economies. When transfers arrive as expected households smooth consumption over the aid cycle, consistent with PIH predictions. However, when aid is delayed (or reduced), consumption smoothing breaks down sharply despite unchanged long-run income expectations, in line with models on liquidity and credit constraints. These findings suggest that consumption smoothing in camp economies is shaped by the interaction between predictability, transfer size, and access to credit. Aid timing and scale therefore operate as key consumption smoothing elements, with timing shocks generating welfare losses comparable in magnitude to reductions in transfer size.

A second contribution of the thesis concerns the general-equilibrium effects of humanitarian aid in refugee camps, and how these effects differ across changes in aid modality, timing, and scale. A growing body of work has shown that large cash and in-kind transfers generate substantial spillovers (Angelucci and De Giorgi 2009; Cunha et al. 2019; Egger et al. 2022; Filmer et al. 2023; Delius and Sterck 2024). However, nearly all existing evidence studies the expansion of support, treating transfers as positive demand shocks. By contrast, little is known on how markets respond when assistance is delayed or reduced. In this regard, only a few empirical studies examine the reduction of support, and in all instances, they consider a predictable programme ending and not an unexpected cut (Aizer et al. 2016; Banerjee et al. 2021; Altındağ and O'Connell 2023).

This thesis extends the general-equilibrium literature on cash transfers by studying an unexpected reduction in support. In this case, aid cuts operate as a negative demand shock, not only confined to households' budgets, but transmitting through markets via prices and credit. The thesis shows that market adjustments play an important but limited role in buffering welfare losses. Price declines following cuts partly attenuate the impact of reduced purchasing power, but they do not neutralize the resulting declines in consumption, nutrition, or well-being. Conceptually, this implies that general-equilibrium adjustments in aid-dependent economies mediate shocks through prices and market activity, but remain insufficient to offset the welfare consequences of sudden reductions. More

broadly, these findings highlight that humanitarian assistance should be analysed as an economy-wide intervention in this context, where both the timing and scale of transfers shape equilibrium outcomes in ways that micro-level evaluations miss.

A third contribution of the thesis concerns the role of aid in shaping informal credit in camp economies. A growing body of literature is studying how beneficiaries cope with shocks in humanitarian economies (Dercon 2002) and the links between cash transfers and credit (Angelucci 2015; Gazeaud et al. 2023; Torkelson 2020). Other theoretical work modelled how gifts, informal transfers and networks shape consumption smoothing when formal insurance is absent (Townsend 1994; Fafchamps and Lund 2003; Bloch et al. 2008). Evidence from low-income settings indicates that these informal responses are partly effective consumption smoothing strategies in response to expected shocks (rainy season, lean season, end of programmes), but break down when shocks are unexpected (e.g., Rosenzweig and Binswanger 1993; Townsend 1994; Udry 1994; Dercon and Krishnan 2000; Fafchamps and Lund 2003; Kazianga and Udry 2006). These studies typically examine anticipated and localized shocks, and typically treat informal credit as an exogenous coping mechanism.

This thesis advances the literature by showing that informal credit arrangements in humanitarian settings are endogenously constructed around aid modality, size and predictability. In refugee camps where assistance is delivered as cash or restricted cash, aid cards and expected future transfers are used to secure credit, shaping both access to credit and its size. When aid is reduced the collateral value of future transfers declines and informal lending contracts accordingly. As a result, aid cuts simultaneously reduce household resources and erode one of the principal mechanisms households use to smooth consumption. This suggests that informal credit in camp economies is not an exogenous consumption smoothing mechanism, but is itself sustained or disrupted by the scale, modality, and predictability of humanitarian assistance.

A final contribution of the thesis relates to the literature studying the trade-offs between universal and targeted approaches in aid delivery. A large literature in social

protection and humanitarian aid shows that no targeting approach fits in all contexts, and that the relative performance of universal, categorical, geographic, and vulnerability-based systems depends on administrative capacity, poverty, targeting design, and behavioural responses (Devereux 2016; Devereux et al. 2015; Grosh et al. 2022; Mkandawire 2007). In much of this literature, targeting is framed as a problem of information and efficiency: how to minimize inclusion errors while containing administrative costs. This thesis reframes the issue by treating targeting as an intervention which can reshape social relations, and coping strategies.

The thesis shows that when poverty is widespread and livelihoods are fragile, fine-grained vulnerability targeting may do more than reallocate resources to households in need. Targeting alters behavioural incentives (Baird et al. 2018), undermines existing informal networks, and generates social and psychological costs that are not captured by standard efficiency metrics (MacAuslan and Riemenschneider 2011; Slater and Mphale 2008). These costs are compounded by substantial administrative and monitoring costs, particularly in settings where household poverty and structure fluctuate and information must be continuously updated (Coady and Skoufias 2004; Kidd and Athias 2019). In addition, targeting creates exclusion errors which, in case of extreme poverty, might directly threaten the survival of those excluded. Under such conditions, targeting can weaken both the protective and efficiency objectives it is intended to serve.

Taken together, the contributions of this thesis expand the understanding of humanitarian aid in refugee camp economies. Across timing, scale, modality, and allocation rules, the central insight is that aid design choices have economy-wide implications, rather than marginal adjustments to an otherwise stable economic environment. In contexts of high aid dependence, limited market integration, and fragile livelihoods, humanitarian assistance impact households, credit, and markets simultaneously. Changes along one design margin, such as delaying transfers, reducing their size, altering their modality, or introducing targeting, therefore propagate through multiple channels. By integrating insights from consumption theory, general-equilibrium analysis, and the study of informal institutions, this thesis advances a more coherent understanding of camp economies and clarifies

why and how aid design shapes economic lives in refugee camps.

1.4 Thesis structure

This integrated thesis comprises three analytical chapters, each structured as a standalone article intended for separate publication. Collectively, they examine how humanitarian assistance shapes welfare, markets, and social dynamics in refugee camp settings. Chapter 1 studies the household- and market-level effects of humanitarian aid distribution and delays in aid delivery. Chapter 2 examines the welfare and general-equilibrium impacts of reductions in humanitarian assistance. Chapter 3 analyzes the consequences of shifting from universal to vulnerability-based targeting in aid provision. Together, the chapters study distinct but closely related dimensions of humanitarian aid, its timing, scale, modality, and allocation rules. While each chapter addresses a specific research question, they are unified by a common focus on humanitarian aid as a core economic institution rather than a simple welfare injection.

Chapter 2 and Chapter 3 are co-authored with Professor Olivier Sterck, while Chapter 4 is co-authored with Madison Bakewell and Professor Olivier Sterck. Notwithstanding these collaborations, all errors and omissions in this thesis remain my own.

Chapter 2 examines the household- and market-level effects of humanitarian aid cycles and unanticipated delays in aid delivery. Aid delays, which were prevalent in Kenya and elsewhere before this research began, continue to affect refugee communities globally and have only recently received scholarly attention. Using high-frequency panel data and quasi-experimental variation in interview timing relative to aid distribution, the chapter studies whether refugee households are able to smooth consumption over regular aid cycles and when aid is delayed. The findings show that households can smooth consumption between scheduled disbursements, but that this capacity breaks down during delay periods. When aid is delayed, caloric intake, dietary diversity, food security and mental wellbeing decline, while indebtedness increases. These effects are accompanied by differentiated price responses, with aid-card prices falling during delays due to reduced

demand, while cash prices rise as households substitute toward cash-based transactions.

Chapter 3 investigates the household and market effects of a large, unanticipated reduction in humanitarian assistance. Amid widespread funding shortfalls affecting humanitarian programs globally, refugees in Kakuma and Kalobeyei experienced a uniform 20% aid cut in July 2023 during the data-collection period. Exploiting randomized interview timing around the policy change together with weekly price data, the chapter studies the short- and medium-term impacts of this shock on household welfare, credit, and markets. The findings show that the aid cut generated large and immediate declines in food security, food stocks, nutritional intake, and total expenditure, alongside deteriorations in subjective well-being and sleep quality. The shock also altered time preferences and risk attitudes, indicating broader behavioral responses to economic stress. At the market level, food prices declined following the cut, reflecting a contraction in local demand and partially attenuating, but not offsetting, the welfare losses experienced by households.

Chapter 4 analyzes the shift from universal to vulnerability-based aid targeting implemented in conjunction with aid cuts in Kakuma and Kalobeyei in 2025. As humanitarian budgets contract, aid agencies are replacing uniform assistance with a differentiated system that allocates support based on assessed vulnerability. Drawing on qualitative evidence from semi-structured interviews conducted around the policy change and in the months that followed, the chapter examines how this reform affected household welfare, coping strategies, markets, and social cohesion. The findings indicate that, in a context of widespread poverty and high aid dependence, vulnerability-based targeting can generate exclusion errors and contribute to the breakdown of informal credit and mutual support mechanisms that had previously helped households smooth shocks. The reform was also associated with rising food insecurity, deteriorating mental health, increased insecurity and violence, and distress-driven migration, with market responses likely amplifying these effects through business closures and economic contraction. Overall, the chapter shows that in highly aid-dependent camp economies, shifts in allocation rules can undermine the efficiency, protection, and ethical objectives of targeting relative to more universal approaches.

Across chapters, the central finding is that in contexts of widespread poverty and high aid dependence, even marginal changes in aid design generate large and far-reaching consequences for both households and local economies.

This thesis continues with one section per analytical chapter, followed by a conclusion section. This is further divided into main findings, policy implications, limitations, and future research.

Chapter 2

The Welfare Impacts and Market of Effects of Delays in Humanitarian Assistance

Delays in aid delivery are common, yet their impacts on households and markets are theoretically ambiguous and empirically understudied. Models with financial constraints or present bias predict sharp consumption declines, while the Permanent Income Hypothesis predicts consumption smoothing. We test these predictions using high-frequency data and random interview timing in a large refugee camp in Kenya. Households smooth consumption under regular aid cycles, but delays reduce food consumption and food security. Informal credit through shops mitigates short-term impacts, but entails higher prices (+17%). Prices also respond to the timing of aid. Results support credit constraint models.

Introduction

Income delays are a pervasive and understudied friction that may distort household consumption, behavior, and market dynamics. Previous research has examined how income shocks affect consumption and other outcomes e.g., following cash transfers (Gentilini 2024), weather shocks (Dell et al. 2014), or seasonal fluctuations (Fink et al. 2020). Less is known about the impact of unexpected shifts in the timing of income, holding its level constant. We study this question in the context of humanitarian aid, a vital but often delayed income source for millions.

In 2023, 339 million people required humanitarian assistance, a fourfold increase since 2015 (Development Initiatives 2023). However, only 68% received aid due to insufficient donor contributions. Aid delivery is also frequently delayed due to conflict, logistical barriers, and bureaucracy. In war-affected regions such as Gaza, DR Congo, Ethiopia, and Sudan, blockades, looting, and insecurity cause severe disruptions. Even outside conflict zones, delays are common due to administrative and logistical constraints. From a policy perspective, understanding the impacts of humanitarian aid delays is critical, as they affect millions of highly vulnerable individuals and are likely to become more frequent amid ongoing budget cuts by major donors, including the US, UK, and several European countries.

From an academic perspective, delays in humanitarian aid offer an opportunity to test competing models of intertemporal choice. Classical models such as the Permanent Income Hypothesis (PIH) predict that households smooth consumption by allocating resources based on expected lifetime income, implying minimal impact of temporary fluctuations such as aid delays (Friedman 1957; Hall 1978). In contrast, behavioral models with high discounting, temptation, or limited attention predict limited consumption smoothing even in the absence of shocks (Strotz 1956; Laibson 1997; Gul and Pesendorfer 2001; Banerjee and Mullainathan 2010; Gabaix and Laibson 2017), with the most extreme case corresponding to hand-to-mouth behavior where households consume resources immediately as they arrive (Kaplan and Violante 2022; Gelman 2022). Between these extremes

lie models with credit or savings constraints, where consumption smoothing during shocks is partial and depends on access to credit or precautionary reserves (e.g., Deaton 1991; Carroll 1997).

We test these theories by assessing the impact of humanitarian aid delays in one of the world's largest refugee camps.[1] Kenya hosted about 600,000 refugees in October 2022, when data collection began. The vast majority of these refugees (86%) resided in the Kakuma and Dadaab refugee camps, the two largest refugee camps in Africa. All camp-based refugees in Kenya received food assistance from the World Food Programme (WFP) in the form of monthly cash or in-kind transfers, valued at approximately USD 17 per refugee per month at the start of the study.[2] Most refugees fully depend on these transfers to survive. However, cash transfers were delayed by an average of 11 days during the study period because of bureaucratic happenstance, with significant variation across months (s.d. = 7, min. = 0, max. = 23).

We study consumption smoothing in the presence of transfer delays by exploiting high-frequency data collected in Kakuma over the course of one year, between October 2022 and September 2023.[3] Our identification strategy leverages three unique features of the data. First, ten waves of panel data were collected for a representative sample of 622 South Sudanese households (6,220 interviews in total), enabling us to track socioeconomic variables over time while controlling for household fixed effects. Second, aid transfers were frequently delayed because of bureaucratic procedures that are independent of refugee and market outcomes. Third, interview dates were randomly assigned, creating experimental variation in whether and to what extent households were impacted by delays at the time of their interviews. Our study exploits this experimental variation to study the impact of delays and, more broadly, how the monthly aid cycle affects consumption smoothing and the socioeconomic outcomes of recipients.

[1] The impact of aid cuts is studied in Chapter 3.

[2] According to our survey data, food aid represented around 68% of per capita monthly income during our study.

[3] Data collection was not conducted in Dadaab due to security concerns. Our research covers the Kakuma site, which comprises the four Kakuma camps and the three villages of the Kalobeyei settlement.

During the data collection year, we also gathered longitudinal data on the prices of 70 food and beverage items in different markets in the camp. This high-frequency dataset allows us to study the market effects of aid delays and, more broadly, how prices fluctuate throughout the monthly aid cycle. The quantitative data are supplemented by over 250 qualitative interviews conducted with refugees, wholesalers, food retailers, and humanitarian workers, both before and after the data collection period.

We highlight three key findings from our study. First, we identify large negative impacts of aid delays on refugees' consumption and food security. Delays reduce caloric intake, dietary diversity, food security, and the value of consumption, contradicting the predictions of the PIH. Our regression model predicts that daily calorie intake falls by about 152 calories (8%) following an average delay of 11 days, and by about 317 calories (16%) after the longest observed delay of 23 days. At the same time, we find that refugees are generally able to smooth consumption during a "normal" aid cycle, when transfers are received on time, contrary to the predictions of behavioral and hand-to-mouth models, which typically imply overconsumption upon receipt.

Second, we find strong heterogeneity in the effects of aid delays based on households' place of residence, providing suggestive evidence that credit and liquidity constraints drive our results. Kakuma comprises two distinct sites—the Kakuma camp and the Kalobeyei settlement—each with its own modalities of aid delivery. In Kalobeyei, humanitarian aid is provided as cash transfers via ATM or SIM cards. Refugee households use these cards—and the promise of future cash transfers—as collateral to secure food on credit when aid is delayed. As a result, they can better smooth consumption during delays. This comes at a steep cost, however, as prices for purchases on credit are 17% higher on average. By contrast, in the Kakuma camp, aid is delivered as a mix of in-kind transfers (about 60% of transfer value) and cash transfers (about 40%), limiting households' ability to use cash transfers as collateral to access credit. Facing both liquidity and credit constraints, households in Kakuma struggle to smooth consumption during aid delays.

Finally, we document general-equilibrium effects in local markets. The prices of staple

goods purchased with aid cards fall when transfers are delayed, reflecting weaker demand, while the prices of goods purchased with hard cash rise, as cash becomes the only available means of payment. These findings highlight how market dynamics in humanitarian settings are impacted by the timing of aid.

This chapter makes several contributions to the literature. First, it provides causal evidence on the effects of delays in regular income payments, highlighting the welfare costs of frictions in transfer timing. It adds to research on how the timing (Parker et al. 2013; Brune et al. 2021) and frequency (Aguila et al. 2017; Casaburi and Macchiavello 2019; Brune and Kerwin 2019) of payments shape household outcomes.[4] Whereas these studies exploit randomized variation in payment schedules that are subsequently predictable, we examine unexpected, exogenous disruptions to the timing of payments, which generate temporary liquidity constraints without reducing the overall transfer amount. It complements studies that exploit bureaucratic failures in cash transfer delivery to assess program impacts (e.g., Gilligan et al. 2013, Bazzi et al. 2015).[5] We show that unexpected shocks to transfer timing have large negative effects on refugee households, suggesting that the predictability of aid may be as important for welfare as the amount transferred.

Second, we contribute to the literature on consumption smoothing and intra-cycle consumption dynamics by testing competing models of intertemporal choice in a context of extreme vulnerability, where these theories are arguably most relevant. An important literature examines how households smooth consumption and expenditures between regular payments. In high-income settings, studies document payday-driven expenditure spikes, consistent with models of hyperbolic discounting and present bias (e.g., Stephens 2003, 2006; Shapiro 2005; Mastrobuoni and Weinberg 2009; Hastings and Washington 2010; Gelman et al. 2014; Goldin et al. 2022).[6] In low-income contexts, evidence remains more limited. Aubery and Buisson (2024) find that refugees in Chad do not fully smooth

[4] A burgeoning literature also examines how impacts persist or fade over time (Blattman et al. 2020; Balboni et al. 2022; Altındağ and O'Connell 2023).

[5] Gilligan et al. (2013) study delays in cash transfers to demobilized combatants in Burundi, while Bazzi et al. (2015) examine delays in the rollout of a social transfer program in Indonesia. Both focus on temporary programs and disruptions during early implementation. In contrast, our study examines delays in the delivery of regular and ongoing payments.

[6] Carvalho et al. (2016) also find evidence of present bias.

consumption across the aid cycle, though their data do not capture behavior immediately before or after disbursements, while Angelucci et al. (2024) find evidence consistent with smoothing among PROSPERA beneficiaries in Mexico. This literature faces several empirical challenges. First, existing studies cannot usually distinguish between smoothing over regular, anticipated income cycles and responses to unexpected shocks in timing. Second, transfer dates typically coincide with other predictable payments, confounding causal inference (Gelman et al. 2014; Goldin et al. 2022). Third, most studies focus on expenditures, even though smoothing theories make predictions about consumption (Aguiar and Hurst 2005; Angelucci et al. 2024). Our study addresses these challenges by combining high-frequency panel data with detailed consumption measures (rather than expenditure proxies), randomly assigned interview dates (as in Stephens 2003), and quasi-random variation in aid delays, capturing both the regular aid cycle and unexpected timing shocks. We find that households smooth consumption under normal aid timing, contradicting behavioral and hand-to-mouth models, but struggle to do so when aid is delayed, in contradiction to the PIH. These results highlight the central role of liquidity, credit, and savings constraints in shaping responses to shocks in transfer timing.

Third, our study advances the literature on seasonality, poverty dynamics, and food insecurity. A large literature studies seasonality and consumption smoothing in agrarian economies, where predictable fluctuations in income and prices generate recurrent cycles of food insecurity and borrowing (e.g., Paxson 1993; Devereux et al. 2012; Kaminski et al. 2016), with households only partially adapting (e.g., Basu and Wong 2015; Fink et al. 2020). A related strand of work analyzes the dynamics of poverty (Christiaensen and Shorrocks 2012; Addison et al. 2009) and food insecurity (Barrett 2010), with recent evidence highlighting the importance of short-term (monthly) variations (Merfeld and Morduch 2022). Our data allow us to go a step further by analyzing high-frequency fluctuations in food consumption, food insecurity, and other outcomes within the monthly aid cycle. Unlike the seasonality literature, which focuses on anticipated patterns, we isolate the impact of unexpected and quasi-random shifts in the timing of income, holding its level constant.

Fourth, our findings contribute to the growing literature on the link between cash transfers and credit. Angelucci (2015) and Gazeaud et al. (2023) examine theoretically and empirically how future cash transfers can be used as collateral to finance migration. On the supply side, Torkelson (2020) shows how microfinance institutions in South Africa targeted the recipients of government-guaranteed cash transfers to offer credit, using transfers as a collateral to secure credit. Our results demonstrate that refugees leverage cash transfers as collateral to cope with idiosyncratic shocks. However, this strategy comes at a high cost, as indebted households must accept higher prices from shopkeepers, who also act as their credit providers. More generally, our study contributes to the literature on shocks and coping strategies (Dercon 2002) by examining how households cope with liquidity shocks within a humanitarian economy.

Finally, our study contributes to the growing literature on the market effects of cash transfers, which documents spillover effects on businesses (Egger et al. 2022; Delius and Sterck 2024) and prices (D'Aoust et al. 2018; Cunha et al. 2019; Filmer et al. 2023). It also complements the literature on the short-term effects of food stamps on US prices (Hastings and Washington 2010; Goldin et al. 2022). While most of these studies focus on the effects of changes in the amount or coverage of transfers, we show how variation in the timing of cash delivery, without changes in value, affects local prices. More generally, our study offers insights into how market dynamics evolve within the monthly aid cycle in a humanitarian economy.

The chapter is structured as follows. Section 2.1 provides background on humanitarian assistance and the living conditions of refugees in Kakuma and Kalobeyei. Section 2.2 introduces the data sources and survey design. Section 2.3 presents the analysis of household-level data, while Section 2.4 examines price dynamics. Section 2.5 discusses the welfare implications of our findings, and Section 2.6 concludes.

2.1 Context

Kakuma town is located in Turkana County, an arid area in north-western Kenya bordering Uganda, South Sudan, and Ethiopia. The Kakuma camp was established there in 1992 due to the arrival of South Sudanese refugees escaping the civil war (Jansen 2018). Over the years, recurrent violence and climatic shocks in the region have driven tens of thousands of additional refugees to the camp. To address this growing influx, UNHCR together with the Kenyan Government have expanded the camp multiple times and, in 2016, they opened the Kalobeyei settlement a few kilometers away from Kakuma (MacPherson and Sterck 2021).

The Kakuma camp and Kalobeyei settlement are often regarded as a single large camp due to their close proximity, shared environment, and joint administration by the same UN and government offices. By the end of our fieldwork, in late October 2023, the combined population of Kakuma camp and Kalobeyei settlement reached 270,000 people, making the camp the third largest in the world and the second in Africa (UNHCR 2023). Almost one in two refugee households in Kakuma camp and Kalobeyei settlement were from South Sudan (49%). The camp also hosted a significant number of Somali (16%) and Congolese (10%) households. In October 2022, 81 percent of the refugee population lived in the Kakuma camp, while 19 percent resided in the Kalobeyei settlement.

The refugee population across both sites faces significant socioeconomic challenges, including a high dependency ratio, low employment rates, and a heavy reliance on humanitarian aid (Bruni et al. 2024). Households are typically large, averaging 7.3 members, with children accounting for approximately 60% of the population. Kenya's encampment policy restricts refugee freedom of movement and access to formal employment. Only 40% of households in Kakuma and Kalobeyei have at least one adult earning an income, and the few adults who work earn less than USD 50 per month on average. The majority of households depend on humanitarian assistance—food assistance in particular—to survive. Food assistance represents approximately 68% of total income per capita in Kakuma and Kalobeyei.

In both sites, the World Food Programme (WFP) oversees the distribution of food assistance, aiming to provide 2100 calories per person per day. However, funding shortfalls often result in reduced food assistance. Between October 2022 and June 2023, food assistance provided about 80% of the target level of 2100 calories per person per day. From July 2023 onward, humanitarian assistance was further reduced by approximately 20%. The impact of this aid cut is examined in Chapter 3.

The modalities of food distribution vary between sites.[7] During our study, households registered in Kakuma received a mix of in-kind food assistance (∼60% of total value) and cash transfers (∼40% of total value). A typical in-kind food basket included 6.51 kilograms of grains (such as maize, sorghum, wheat, or rice), 1.86 kilograms of pulses (beans or split peas), and 1 liter of oil. In-kind distributions in Kakuma consistently occurred during the first week of each month. In addition, households registered in Kakuma received monthly cash transfers that are delivered via SIM cards and are restricted. The money can only be used to purchase food items from WFP-contracted shops (Siu et al. 2023; Delius and Sterck 2024).[8]

Households registered in Kalobeyei received cash transfers only. For most households, transfers are delivered via bank cards and are unrestricted, allowing households to spend the money in any shop and on any items. The system also enables cash withdrawals for purchases with hard cash. A smaller subset of households, mainly those without adults, received restricted cash transfers on SIM cards.[9]

Cash transfers in Kakuma and Kalobeyei were consistently delayed during our study, often by several days (Figure 3). According to WFP's Standard Operating Procedures (SoPs), restricted cash transfers in Kakuma were scheduled to be distributed on the

[7]Due to mobility between the sites after registration, a number of households receive assistance modalities that do not align with their current place of residence. In Kakuma, 83% of households received a combination of in-kind food rations and restricted cash transfers, while 17% received cash transfers only. In Kalobeyei, 94% of households received cash transfers exclusively, while the remaining 6% also received in-kind food rations.

[8]The cash transfer amount in Kakuma was KES 850 (USD 7.1) per person per month from October 2022 to June 2023, and KES 450 (USD 3.8) per person per month from July 2023.

[9]In October 2022, the cash transfer amount in Kalobeyei was KES 2,000 (USD 17) per refugee per month. By July 2023, this amount was reduced to KES 1,600 (USD 13) per refugee per month.

23rd of each month. However, delays occurred in ten out of the twelve months, with an average delay of five days and a maximum delay of 15 days (s.d. = 4.2). In Kalobeyei, cash transfers were supposed to be distributed on the 11th of each month as per the SoPs. Despite this, delays were recorded every month for both cash transfer programs. For the unrestricted cash program, the average delay was 12 days (min. = 5, max. = 22, s.d. = 5.3). For the program of restricted transfers, the average delay was even longer, at 16 days (min. = 5, max. = 23, s.d. = 4.3).

The primary reason for delays in cash transfers is that bureaucratic procedures have multiple components, each of which could be delayed in every given month, making delays unpredictable for households. Each month, four steps involving various UN agencies and NGOs must be completed before transfers are delivered. The first step, referred to as "proof of life", involves the UNHCR and WFP sub-offices in Kakuma, along with their partners managing food distribution. They verify whether beneficiaries are still alive and residing in either the Kakuma refugee camp or the Kalobeyei settlement, requiring the fingerprints of at least one household member. The proof of life is conducted during in-kind distribution in Kakuma and during the last week of each month in Kalobeyei. The main output of this step is the generation of a distribution list containing all beneficiaries, which is then submitted to the WFP office in Nairobi. According to SoPs, this first step is supposed to take between 1 and 4 days; however, during our study, it took an average of 9 days. The second step involves cross-checking the submitted distribution list, creating a payment list, and submitting it to WFP's Programme team. This task, carried out by the WFP Supply Chain team in Nairobi, is expected to take one working day. On average, it took 1.3 days during the study period. In the third step, WFP's Programme team reviews the payment list, releases the down payment, and finalizes a new list to submit to the finance team. According to SoPs, this step should also take one day, but it averaged 2.7 days during our study. Finally, the fourth step requires the finance team and business partners at Equity Bank and Safaricom to disburse funds to beneficiaries, a process that should take three days according to SoPs. However, the average time for this step extended to 6.2 days. According to SoPs, in the worst-case scenario, all four

steps combined should take no more than 9 days. Yet, during our data collection year, the process lasted nearly 20 days on average (19.6), a timeline consistent with previous years.

Importantly, the delays we study are neither unique to Kakuma nor limited to the period of our data collection. Aid delays were already widespread before our research began and continue to affect refugee populations in Kenya and beyond. Across Kenya, more than half a million refugees are affected by delayed assistance. Globally, aid delays have been widely documented in both refugee and non-refugee settings (Lentz et al. 2013; Nikulkov et al. 2016).

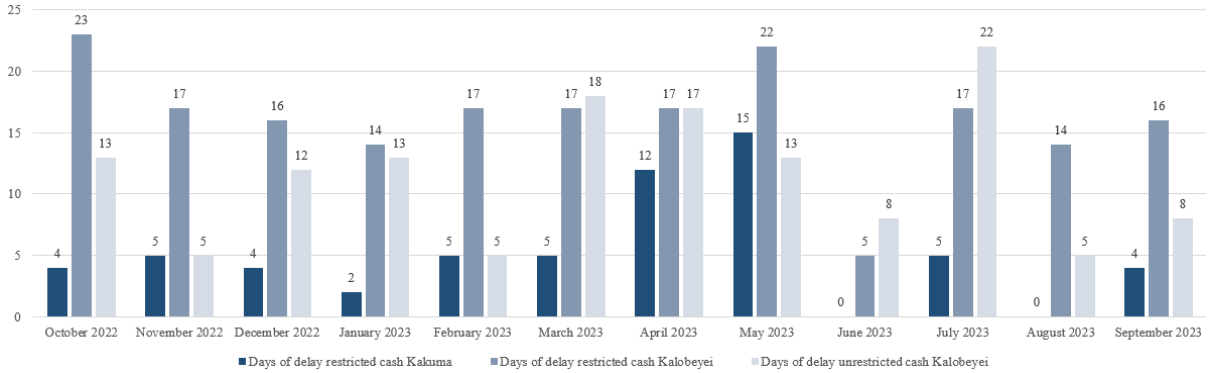


Figure 3: Cash transfer delays in Kakuma and Kalobeyei

Data source: WFP distribution dates. Delays in cash transfers occurred in every month of the study period, but their average length varied across locations and programmes. In Kakuma, the average delay was five days and the maximum was 15 days, whereas in Kalobeyei the average delay was 12 days for unrestricted cash and 16 days for restricted cash, with maximum delays of 22 and 23 days, respectively.

2.2 Data

Between September 2022 and November 2023, we conducted three complementary data collection activities: (1) a high-frequency household survey, (2) a high-frequency price survey, and (3) in-depth qualitative research.

First, we aimed to collect high-frequency data from a representative sample of 622 South Sudanese households.[10] Our objective was to collect 10 waves of data for each household, totaling 6220 interviews. Data collection started on the 6th of October 2022

[10]In the first wave, we also collected data on representative samples of Congolese and Somali households (see Bruni et al. 2024 for details).

and finished on the 27th of September 2023. To ensure the highest data quality on consumption and food security, interviews were conducted with the household member responsible for meal preparation. The data were collected by trained refugee enumerators on survey tablets.

Each wave lasted exactly four weeks and was divided into eight three-day intervals, with Sundays excluded from the survey protocol. Before the first wave, households were randomly assigned to one of these intervals, which determined their interview schedule for all subsequent waves. We employed two strategies to maximize variation in survey days for each household across waves. First, each wave spanned 28 days, while a typical month averages 30.4 days. Second, throughout the data collection period, we scheduled four two-week breaks and one one-week break. These two factors introduced variation in interview timing. For example, households assigned to the first three-day window were interviewed around October 6-8 (Wave 1), November 17-19 (Wave 2), December 15-17, January 26-28, March 2-4, April 13-15, May 11-13, June 22-24, July 20-22, and August 31 to September 1 (Wave 10). Our analysis leverages this exogenous variation in survey days for each household across waves. Overall, 91% of the interviews were conducted within the assigned three-day windows, while 9% fell outside of them. In the analysis, we address this non-compliance using instrumental variable regression (see Section 2.3.1 for details).

Attrition was minimal in this study, with only 34 out of 622 households (5.4%) either refusing to participate or relocating from Kakuma during the data collection period. Attrition is not correlated with the random assignment of interview dates (p-value = 0.52). When households dropped out, they were replaced by their nearest neighbor, who was assigned a new identifier and is treated as a distinct household in the analysis. In total, our full sample includes 656 households, of which 588 completed all 10 waves, 34 attrited during the one-year survey period, and 34 are replacements.

In parallel with the household survey, we conducted a high-frequency price survey of food, beverages, and non-food items in both Kakuma refugee camp and Kalobeyei

settlement. Data collection started on the 27th of October 2022 and finished on the 27th of September 2023. Price data were collected weekly from shopkeepers in the main market areas of the four *camps* of Kakuma and the three *villages* of Kalobeyei. The survey covered 70 different types of food items.[11] A full assessment took a week to complete. The data are used to analyze the market effects of aid distribution and aid delays, and more generally, to study how prices evolve throughout the monthly aid cycle.

Finally, we also conducted more than 250 in-depth interviews before and after the quantitative surveys, which offer insights into the broader economic challenges faced by refugee communities and their coping strategies. The qualitative work was organized in two phases. In September 2022, we conducted 70 semi-structured interviews with refugees and shop owners to explore economic activities, resources, consumption patterns, and coping strategies. These interviews, along with focus group discussions with our enumerators, informed the development of the quantitative surveys. In October and November 2023, we followed up with 134 one-off interviews with refugees, shopkeepers, wholesalers, and key informants. In the same period, we also conducted a food security journaling component with six South Sudanese households from Kakuma and Kalobeyei, interviewing them two to three times per week over a month to understand how they manage food supplies. We use the qualitative data to triangulate results, examine perceptions, and explore mechanisms of impact.

2.3 Analysis of Household Data

2.3.1 Identification Strategy

The analysis of high-frequency household data focuses on two key questions: (1) Can households effectively smooth their consumption and other outcomes throughout the aid cycle? (2) Do delays in cash transfer delivery affect households' ability to smooth their

[11] Different quantities of the same product are considered separately. For instance, maize is typically sold per kg or in bags of 45kg. These are considered as separate goods in the analysis.

consumption and other outcomes?[12] To address these questions, we estimate the following regression equation:[13]

$$Y_{i,t} = \beta_0 + \beta_1 Days_{i,t} + \beta_2 Cash\ delay_{i,t} + \delta^T X_{i,t} + \gamma HH_i + \epsilon_{i,t}, \tag{2.1}$$

where $Y_{i,t}$ represents the outcome of interest for household i at time t.

The variable $Days_{i,t}$ denotes the number of days since the last transfer was received, either cash or in-kind. It is set to zero on the distribution day and increases by one unit each subsequent day until the next distribution day. Figure 5 shows the distribution of this variable by location.[14] The coefficient β_1 measures households' ability to smooth consumption and other outcomes between transfers.[15]

The variable $Cash\ delay_{i,t}$ measures the number of days by which a cash transfer is overdue. It takes the value zero before the expected delivery date, equals one on the first day after the due date if the transfer has not yet arrived, and increases by one for each additional day until the transfer is delivered.[16] The coefficient β_2 captures whether the outcome variables are specifically impacted during delays.

The coefficients β_1 of the smoothing variable and β_2 of the delay variable should be interpreted jointly, as illustrated in Figure 4. Mechanically, there is indeed a strong correlation between these variables. As aid delays increase, the number of days since the last transfer should also rise. In our data, the correlation coefficient between $Days_{i,t}$ and

[12]Because all households in our sample are sometimes affected by aid delays, we cannot compare counterfactual worlds with and without delays. Instead, using household fixed effects, we study how consumption smoothing is affected when aid is delayed.

[13]We did not register a pre-analysis plan (PAP) because data on delays, our key source of identifying variation, only became available after data collection was completed.

[14]Food consumption indicators and the measures of sleep quality and moods relate to the day preceding the survey. For these outcomes, $Days_{i,t}$ is set to zero if distribution occurred the day before the survey. In our main specification, we exclude the few observations recorded on transfer days, as it is unclear whether they belong to the delay period. The results remain similar when these observations are included (Appendix Table A.10).

[15]For simplicity, this specification implicitly assumes that consumption smoothing is similar after cash and in-kind transfers. We test this assumption and find supporting evidence in Appendix Table A.11.

[16]As a robustness check, we also test a specification where $Cash\ delay_{i,t}$ is a binary variable, finding similar results (Appendix Table A.14).

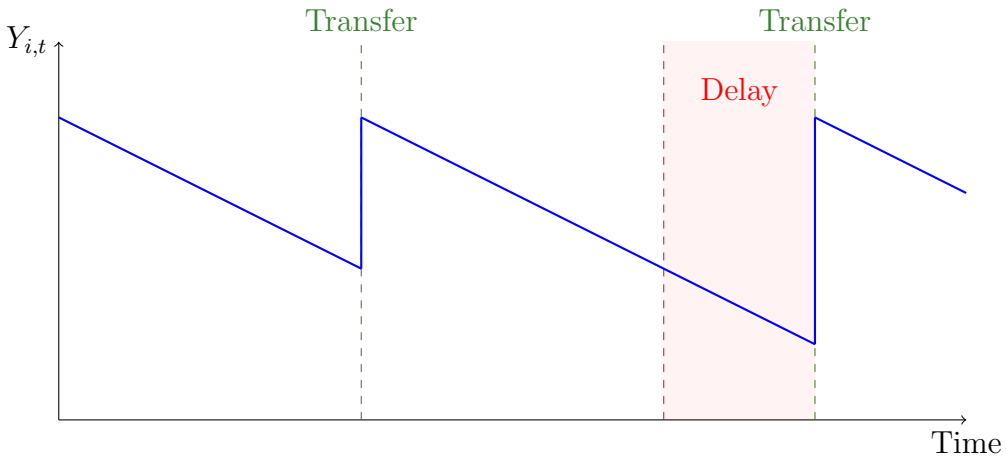
Cash delay$_{i,t}$ is 0.57. As such high correlation between explanatory variables of interest could create a problem of multicollinearity, we perform robustness checks by re-estimating regressions where smoothing and delay variables are included separately. To avoid omitted variable bias, the results of these regressions should only be interpreted if the coefficients of the excluded variables are close to zero and insignificant in the full specification.

Figure 4 illustrates key scenarios for an outcome variable such as consumption, which is expected to decrease over the aid cycle and in response to delays. If $\beta_1 < 0$ but $\beta_2 = 0$, households are unable to smooth consumption over the aid cycle, but aid delays do not have a distinct impact. Importantly, delays still matter indirectly, as each additional day without aid further reduces consumption. If $\beta_1 = 0$ but $\beta_2 < 0$, households are able to smooth consumption over the aid cycle, except when aid is delayed, which leads to a progressive drop in consumption. If $\beta_1 < 0$ and $\beta_2 < 0$, households face two challenges: an inability to smooth consumption over the aid cycle and an additional decline in consumption due to aid delays. In the most optimistic scenario, consistent with the Permanent Income Hypothesis, $\beta_1 = \beta_2 = 0$, indicating that households fully smooth consumption and are unaffected by delays (not shown in the figure).

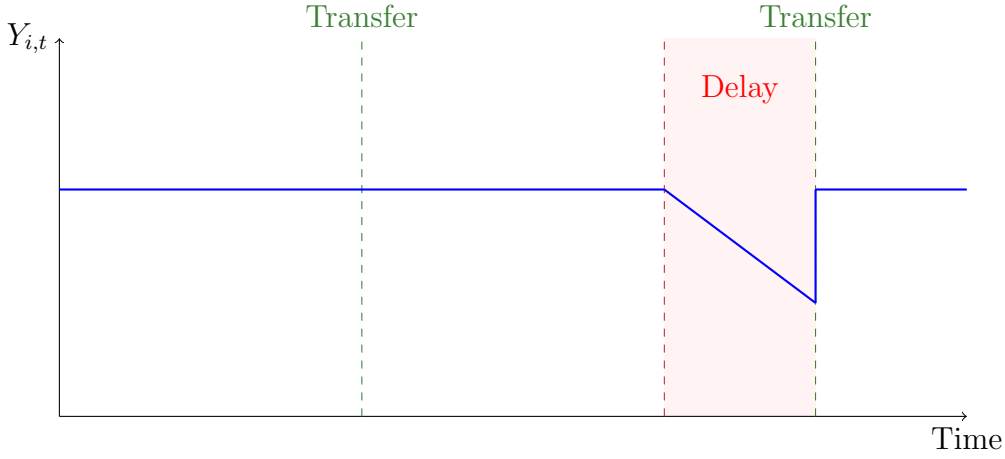
The two variables of interest depend on two sources of variation: aid distribution dates and survey dates. We exploit the randomization of survey dates and quasi-random variation in distribution dates to estimate regression coefficients.

Aid distribution dates are plausibly exogenous to the outcomes of interest. A total of 36 different cash transfers were organized by WFP and its partners during the one-year study period.[17] Importantly, the timing of cash distributions was not tailored to individual households and did not depend on the socioeconomic conditions in the camp. As detailed in Section 2.1, cash transfer dates are set in WFP's standard operating procedures but subject to bureaucratic contingencies. This makes the occurrence of delays plausibly exogenous to household-level outcomes. In Appendix Table A.1, we also

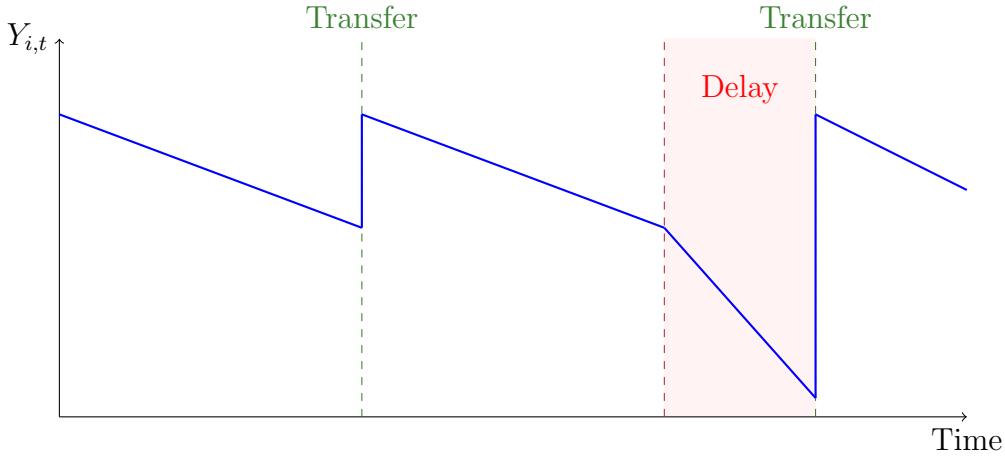
[17]There are three cash transfer modalities (restricted cash transfers in Kakuma, unrestricted cash transfers in Kalobeyei, and restricted cash transfers in Kalobeyei) and 12 transfers per year per modality.



(a) $\beta_1 < 0$, $\beta_2 = 0$. Consumption decreases linearly over the aid cycle.



(b) $\beta_1 = 0$, $\beta_2 < 0$. Decline when aid is delayed.



(c) $\beta_1 < 0$, $\beta_2 < 0$. Consumption decreases linearly over the aid cycle, with additional decline when aid is delayed.

Figure 4: Smoothing and delays for different values of β_1 and β_2.

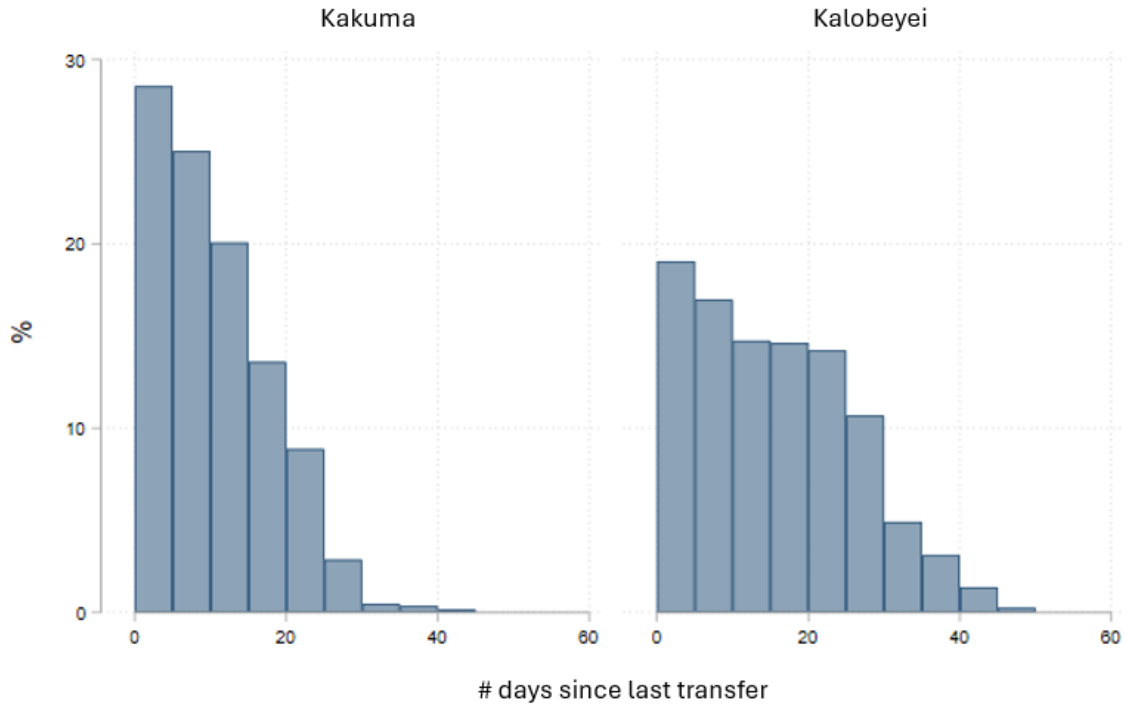


Figure 5: Distribution of the number of days since the last transfer

provide evidence that delays are uncorrelated with the time of the year, weekends or holiday periods, weather data, or the KES-USD exchange rate.

Randomly assigned interview dates are also exogenous. To be sure, actual interview dates may be endogenous. For instance, the poorest households might be more likely to postpone interviews during delays if they feel too weak or stressed to participate. Conversely, they might prioritize participation to receive the study compensation. To address this potential endogeneity concern, we exploit the random assignment of interview dates. As detailed in Section 2.2, each survey wave was organized over four weeks, divided into eight blocks of three consecutive days, with Sundays excluded. Households were randomly assigned to one of these 3-day blocks at the start of the fieldwork. Appendix Table A.2 demonstrates that treatment assignment is orthogonal to 21 baseline variables, with an F-test of joint orthogonality yielding a p-value of 0.96 using randomization inference (Kerwin et al. 2024). This confirms that the randomization process successfully generated exogenous variation. Consequently, the assigned interview day—defined as the midpoint of the 3-day block—is an exogenous variable which we use to construct exoge-

nous instruments for our two variables of interest, denoted $Days^{IV}_{i,t}$ and $Cash\ delay^{IV}_{i,t}$.[18] We use these variables as instruments to estimate Equation (2.1). The compliance rate with random assignment was very high, with 91% of interviews conducted within the randomly assigned 3-day windows. As a result, the first-stage regressions are strong, with Kleibergen-Paap F-statistics exceeding 100 in most specifications (Lee et al. 2022). Results are consistent when using the reduced form, as shown in Appendix Table A.15.

Benchmark regressions include a vector $X_{i,t}$ of control variables that are expected to predict the outcomes.[19] All regressions also include household fixed effects. While the inclusion of control variables and fixed effects is not necessary to obtain unbiased coefficients, they are added to the regressions to improve precision. As shown in Appendix Tables A.7 and A.8, the results remain virtually unchanged when controls or fixed effects are omitted.

We cluster standard errors at two levels. First, the household level, which is the unit of observation repeatedly surveyed over time and the level at which serial correlation in the error term is most likely to arise. Second, the survey date by camp of registration, because households registered in the same camp and interviewed the same day will have the same treatment status. Conclusions are similar if we consider only one level of clustering or use robust standard errors (Appendix Tables A.16 and A.17).

The main analysis focuses on 22 outcome variables, organized into four categories (see Section 2.3.2). To address concerns about multiple testing, we report sharpened q-values in the main analysis to control for the false discovery rate (Benjamini et al. 2006;

[18]For instance, while $Days_{i,t}$ measures the number of days between the *actual* interview date and the most recent aid distribution, $Days^{IV}_{i,t}$ measures the number of days between the *randomly-assigned* interview date and the most recent aid distribution. Similarly, $Cash\ delay^{IV}_{i,t}$ measures the number of days by which the cash transfer was overdue on the randomly-assigned interview date.

[19]The control variables include household size, the number of adults, the number of children in school (to account for potential effects of free school meals on consumption measures), the number of jobs per capita, the value of the harvest during the four weeks preceding the survey, a dummy variable indicating whether the household experienced a shock in the four weeks preceding the survey, a dummy variable for whether behavioral games were conducted at the end of the survey, the number of individuals registered on the household's ration cards, the per capita value of in-kind assistance received during the most recent distribution, and binary and running variables identifying observations before and after the 3rd of July 2023 (a significant aid cut occurred on that day, see Bruni and Sterck (2025) for details). See Table A.3 for a detailed description of these variables.

Anderson 2008). All p-values of the regression coefficients shown in the main analysis of household data are considered together when estimating q-values.

We examine heterogeneous treatment effects based on the camp where households resided and the type of aid they received at baseline. In Kakuma, most households receive humanitarian assistance twice per month—once in-kind and once in cash. By contrast, households in Kalobeyei typically receive cash transfers once per month. Indebtedness is significantly higher in Kalobeyei, as households use the SIM or ATM cards on which they receive aid as collateral to access credit. Since cash assistance amounts are higher in Kalobeyei, the value of the collateral is also greater, enabling shopkeepers to grant higher levels of debt. Consequently, we re-estimate Equation (2.1) separately for households in Kakuma and Kalobeyei, as well as for those receiving in-kind rations versus cash transfers only at baseline.

We conduct multiple tests to assess the robustness and consistency of benchmark results. Specifically, we re-estimate Equation (2.1) without control variables, without fixed effects, excluding both attrited households and replacement households, including transfer days, and considering smoothing and delay variables separately. Additionally, we test extended specifications that account for potential differences in consumption smoothing following cash and in-kind transfers, and for non-linearities in consumption smoothing and in the impact of delays. We consider an alternative definition of the delay variable, as a dummy. We also estimate reduced-form regressions. We compare standard errors obtained with different levels of clustering, and with heteroskedasticity-robust standard errors. The results across these various specifications are largely consistent and are presented in the Appendix. Instances where results are less robust are highlighted and discussed in Section 2.3.5.

2.3.2 Outcome Variables

Humanitarian assistance in refugee camps is typically distributed on a monthly cycle, and our objective is to capture significant changes occurring within this short time frame.

To this end, our questionnaire was specifically designed to track daily fluctuations in consumption patterns, coping strategies, and well-being. The effects of aid delays are expected to be short-lived, with households experiencing particular hardship for a few days during the delay but generally returning to their baseline conditions once aid is delivered. Consequently, our analysis focuses on variables with a recall period of no more than one day.

In the main analysis, we consider 22 outcome variables, which are classified in four categories. The construction of these variables is described in detail in Appendix Table A.4, with descriptive statistics provided in Appendix Table A.5.

1. Food consumption: We collected detailed data on food consumption during the day prior to the survey, including food eaten at home, away from home, and school meals, which we use to construct six outcome variables. To measure food consumption, we use daily calories per capita, the value of food consumption per capita, and a binary variable equal to one if the household ate two or more meals and zero otherwise. To measure dietary diversity, we use FAO's Household Dietary Diversity Score (HDDS) (Kennedy et al. 2011). The score counts the number of types of food that a household ate the day before the survey, out of 12 different categories. Building on the work of Kraay et al. (2024) and Sterck (2024), we also consider the number of days that households need to consume 2100 calories per capita—WFP's daily target. This latter indicator is a distribution-sensitive measure of caloric intake, giving more weight to households with low consumption levels. Finally, we use FAO's Household Food Insecurity Access Scale (HFIAS) with a one-day recall period to measure food insecurity (Coates et al. 2007b).[20]

2. Stocks, debts, and assets: Common strategies to cope with shocks include using food stocks, taking up debt, and using savings or reselling assets. Our questionnaire included questions on food stocks related to maize, sorghum, rice, wheat, pasta, and

[20]In the first survey wave, the recall period for the HFIAS was 28 days; consequently, this wave is excluded from analyses involving this indicator.

oil. We aggregate the weight of these stocks in kilograms (kg) and their value in USD. Previous research highlights that debt is a particularly prevalent coping strategy in the Kakuma camp and Kalobeyei settlement (Bruni et al. 2024; Siu et al. 2023). The primary sources of debt are food retailers, who often provide food on credit but typically hold the household's SIM or ATM card used for receiving food assistance as collateral. In our analysis, we examine both the per capita value of debt provided by food retailers and the total per capita value of debt, including from other sources. Using savings and reselling assets are also common strategies in case of shocks. We measure the value of assets and savings by aggregating the value of 24 types of physical assets (e.g., televisions, mobile phones, tables, chairs, cooking pots), animals (mainly chickens, ducks, and doves), and self-reported savings. The value of these assets is taken from the high-frequency price survey data.

3. Mental well-being, time and risk preferences: We analyze six variables to investigate the effects of aid distribution and delays on mental well-being, time preferences, and risk preferences.[21] Mental well-being is assessed through self-reported measures of life satisfaction and sleep quality, both rated on a Likert scale from 1 to 5. Additionally, we compute a "subjective wellbeing score" based on nine questions from the World Health Organization's Study on global AGEing and adult health (WHO-SAGE). These questions inquire whether participants experienced various moods on the day prior to the survey, such as enjoying activities or feeling worried, rushed, or calm (Kowal et al. 2012). Responses are binary and aggregated to create a score ranging from 0 (poor mood) to 9 (excellent mood) (Taquet et al. 2020).

We measure time and risk preferences using incentivized games. Time preferences are captured by estimating respondents' Required Rate of Return (RRR) through five trade-off questions, where participants choose between receiving 500 Kenyan Shillings (KES) in two months or a smaller amount in one month (Cohen et al. 2020; Andreoni and Sprenger

[21]This module, as per the rest of the survey, was administered to the household member responsible for meal preparation.

2012). The RRR measures preference for the present, and is proportional to the discount rate under specific assumptions. Additionally, we examine a binary choice between receiving KES 200 immediately or KES 300 in one month.[22] To measure risk preferences, participants responded to three questions involving a choice between a guaranteed payment of KES 200 or a larger amount with a 50% probability of payout (Dave et al. 2010). Based on their responses, we categorize participants into four groups according to their level of risk tolerance.

4. Mathematics, memory, and cognitive abilities: We used five games to assess participants' abilities in mathematics, memory, and cognitive tasks. In each wave, mathematical ability was tested using three arithmetic questions related to common market transactions.[23] The number of correct responses is used as the outcome variable.

The remaining four games were played directly on the tablet, and the outcome variables are the number of correct responses for each game. Memory was assessed using an adaptation of the Simon memory game, following Carvalho et al. (2016). Participants were shown sequences of colored shapes of increasing length and were asked to replicate the sequence from memory. Selective attention and inhibitory control were measured using the Flanker Task (Eriksen and Eriksen 1974). Participants were presented with a series of five arrows pointing either left or right and had to indicate the direction of the central arrow. Sustained attention was assessed using the Psychomotor Vigilance Task (PVT), where participants were shown sequences of letters (H, O, X, or L) on the screen and had to click as quickly as possible when the letter "O" appeared (Dinges and Powell 1985). Finally, the Hearts and Flowers Task (HFT) was used to measure participants' ability to process congruent and incongruent information (Davidson et al. 2006). This game consists of three parts:

[22]For logistical reasons, the incentive payment was based on this specific question. Respondents preferring the immediate payment received it on the spot, while those opting for the larger amount were paid during the next survey wave. Although the questionnaire script did not reveal that this question determined the incentive payment, participants may have inferred this over time as the survey was repeated multiple times. Results are therefore likely more salient with this variable.

[23]For example: "One bag of rice costs KES 2800, and one fish costs KES 250. You buy one bag of rice and three fish. How much do you have to pay?"

1. Congruent Information: Participants were shown an "O" on either the left or right side of the screen and had to click the corresponding button (10 rounds).

2. Incongruent Information: Participants were shown an "X" on either side of the screen and had to click the opposite button (10 rounds).

3. Mixed Condition: Participants responded to both congruent and incongruent stimuli in a sequence (20 rounds).

The PVT and HFT were only introduced in the sixth wave and, as a result, the number of observations and statistical power are reduced.

2.3.3 Results

Benchmark results for consumption outcomes are presented in Table 1. Panel A estimates Equation (2.1), including both smoothing and delay variables in the regressions. Panels B and C explore the effects of smoothing and delay variables separately. These short specifications reduce multicollinearity but introduce a risk of omitted variable bias if excluded variables impact the outcomes. The results in Panels B and C should only be interpreted if the coefficients of the excluded variables are statistically insignificant and close to zero in Panel A.

Overall, regression results indicate that delays have a substantial negative impact on food consumption and food security. In Panel A, the regression coefficients for the smoothing variable are close to zero, much smaller in magnitude than those for the delay variable, and statistically insignificant based on sharpened q-values. This null result suggests that households successfully smooth consumption between aid transfers in the absence of delays. Consequently, the results in Panel C for consumption outcomes can be interpreted with confidence. The coefficients for the delay variables in Panels A and C have similar signs and magnitudes but are estimated with greater precision in Panel C.

When aid is delayed, refugees are forced to reduce consumption. Each additional day of delay reduces calorie intake by about 12 calories per person per day. Although this

effect may appear modest, the consequences for households are substantial because the impact accumulates with each passing day, and average calorie intake is already low in the absence of delays: only 1,962 calories per person per day. To illustrate the magnitude of impacts, we report the predicted effects of both the average delay (11 days) and the longest observed delay (23 days). Our regression model predicts that by the end of the average delay, calorie consumption falls by 152 calories, or 8 percent. During the longest observed delay, the model predicts a decline of 317 calories, or 16 percent.

These results align with the second example in Figure 4, which depicts relatively stable consumption and food security outcomes during the aid cycle, except when aid is delayed. This pattern is inconsistent with both the PIH and behavioral models of intertemporal choice, but consistent with the presence of binding credit and saving constraints.

The effects of delays are consistent across other food consumption indicators. The average delay reduces the average value of daily consumption by approximately USD 0.06 (9%), while the longest observed delay lowers it by about USD 0.13 (20%). The percentage of households eating fewer than two meals per day increases by 3 percentage points with an average delay, and 7 percentage points with the longest delay.

Delays also significantly worsen dietary diversity and food insecurity. Delays increase the average number of days that households need to consume 2100 calories per capita. Our data show that, in the absence of delay, refugee households require 1.6 days on average to reach WFP's objective of having 2100 calories per member per day. This increases by 0.14 days (9%) with an average delay and 0.29 days (19%) with the maximum delay.

The comparison of results across Panels A, B, and C underscores the importance of accounting for disruptions in program delivery when assessing households' ability to smooth consumption. Ignoring aid delays, as in Panel B, suggests that refugees struggle to smooth consumption throughout the aid cycle. However, when aid delays are considered (Panels A and C), we find that refugee households generally manage to smooth consumption effectively, except when transfers are delayed. Theoretical and policy implications are very different, as discussed in the conclusion of this article.

The inability of refugees to smooth consumption when facing aid delays was frequently reported during qualitative interviews. When asked about the main challenge linked to aid, a male refugee from Ethiopia living in Kalobeyei reported: *"The major challenge is the delay of aid money, people are dependent on the cash assistance."* When asked the same question, a female refugee from DRC and living in Kakuma reported: *"the main issue that I can say is the aid delays."* When asked about the most difficult time of the month, a male refugee living in Kalobeyei and of Congolese origin told us: *"This is mainly when our aid money delays. When this happens we face difficulty in getting food. I remember last month for example the money was delayed by many days, up to the extent where the shopkeeper did not want to give us food. It was really hard for me and my family to get food but we thank God we managed to survive."* Perhaps most strikingly, a male Ethiopian refugee living in Kalobeyei told us: *"There were many people, families, who were almost to die because of the delay of cash transfers. They should release the money at the right time. Many children could die if the money delays."*

Table 2 presents the estimated effects of aid delays on food stocks, debt, and assets. For food stocks, the regression coefficients of the smoothing variable are statistically significant both when stocks are expressed in kilos and in USD. By contrast, the coefficients of the delay variable are small and statistically insignificant once the smoothing variable is included (Panel A). These results are consistent with the first example in Figure 4.[24] Each additional day without receiving food assistance reduces food stocks by about 0.5 kg or USD 0.6 per capita. This implies that food stocks fall by roughly 25 percent after two weeks and by 50 percent after four weeks, illustrating the steady depletion of stocks over the aid cycle.

Results show that households rely on debt to cope with delays (Table 2, Columns 3 and 4). The coefficient of the smoothing variable is insignificant and close to zero in Panel A, indicating that debt levels remain relatively stable throughout the monthly aid cycle in the absence of delays. However, when cash assistance is delayed, households

[24]For food stocks, we therefore focus on Panel A, as interpreting the results in Panels B and C would risk omitted variable bias.

Table 1: Impacts of aid distribution and aid delays on food indicators

	Food consumption p.c.			Food insecurity		
	Calories (kcal)	USD	> 2 meals	Dietary diversity	days/ 2100 kcal	HFIAS score
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Smoothing and delay						
# days since last transfer	1.159	0.000368	-0.000471	0.0000653	-0.000107	0.00793
	(2.610)	(0.000903)	(0.00106)	(0.00279)	(0.00440)	(0.00505)
	[0.933]	[0.933]	[0.933]	[1]	[1]	[0.318]
# days of delay	-13.78***	-0.00578***	-0.00291*	-0.0114**	0.0127*	0.0114
	(4.445)	(0.00150)	(0.00171)	(0.00470)	(0.00757)	(0.00849)
	[0.016]	[0.002]	[0.26]	[0.069]	[0.261]	[0.381]
Kleibergen-Paap F-statistic	221.3	221.3	221.3	221.3	218.1	179.1
Panel B: Smoothing only						
# days since last transfer	-3.457*	-0.00157**	-0.00144*	-0.00375*	0.00417	0.0119***
	(1.850)	(0.000679)	(0.000842)	(0.00208)	(0.00292)	(0.00395)
	[0.218]	[0.086]	[0.26]	[0.235]	[0.368]	[0.018]
Kleibergen-Paap F-statistic	502.9	502.9	502.9	502.9	500.9	533.7
Panel C: Delay only						
# days of delay	-12.43***	-0.00535***	-0.00345**	-0.0113***	0.0126***	0.0206***
	(3.082)	(0.00110)	(0.00136)	(0.00342)	(0.00488)	(0.00673)
	[0.001]	[0.001]	[0.057]	[0.009]	[0.055]	[0.016]
Kleibergen-Paap F-statistic	681.7	681.7	681.7	681.7	677.7	566.9
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	6178	6178	6178	6178	6158	5557
Mean if no delay	1962.4	0.675	0.536	4.789	1.560	4.522

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1). Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Standard errors are clustered at the household level, and at the survey date by camp of registration level. Anderson (2008) sharpened q-values are shown in square brackets. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

increase their borrowing from shopkeepers by an average of USD 2.6 per person (7%) after an average delay (11 days) and USD 5.4 per person (15%) after the longest delay. These amounts are substantial relative to the monthly value of food assistance (USD 17 per person at baseline). Food retailers are the main provider of credit in Kakuma and Kalobeyei, with 95% of households in our sample having contracted a debt with their shopkeeper and 93% of total household debt originating from food retailers.

Households do not seem to rely on assets and savings to cope with aid delays. Based on sharpened q-values, regression coefficients of the smoothing and delay variables are statistically insignificant in all specifications.

Qualitative data also indicate that aid delays are a key driver of widespread indebtedness. Many households reported falling into a debt trap during qualitative interviews. When asked why she had gone into debt, a Somali refugee living in Kalobeyei explained: *"The debt I have is only for food, and my debt amounts to almost fifty thousand [KES]. It is because of the delay of aid. That is the major reason for my debt."* Similarly, a female refugee from DR Congo living in Kalobeyei shared: *"When I came in the camp, my aid card was in my hands, but I needed food because of delays and there was no way for the shopkeeper to give me food unless I gave my card as guarantee. Unfortunately, I was not able to repay the debt because I do not have any other source of income."* The link between aid delays and indebtedness was repeatedly emphasized and, when discussing ways to alleviate debt burdens, improving aid delivery was frequently mentioned as a solution. A Somali female refugee living in Kalobeyei told us that to alleviate debts, UN agencies should be: *"Improving on the issue of aid delays. If there is an improvement on the delay, it would address some of the problems. The delay of the money partly contributes to the suffering of refugees."* In an interview, a Congolese male refugee residing in Kalobeyei linked the issue of aid delays not only to food scarcity and indebtedness, but also to limited dietary diversity: *"I eat the same food because the fund is always sent late, when we have already borrowed [...]. For example, last month, the July fund was sent in August, imagine how people survived for the full of July. This delay is making us to have many debts at the shops."* Instances of credit rationing were consistently reported. As

put by a female South Sudanese refugee living in Kakuma: *"Having a lot of debts is not good, sometimes the shopkeeper will refuse to give you more credit because of high debts, even if you are in need."*

Table 2: Impacts of aid distribution and aid delays on debt, assets and savings

	Food stocks		Debt p.c. (USD)		Assets & savings
	(kg)	(USD)	Food retailer	Total	(USD)
	(1)	(2)	(3)	(4)	(5)
Panel A: Smoothing and delay					
# days since last transfer	-0.544***	-0.595***	-0.0217	-0.0371	-0.353
	(0.107)	(0.117)	(0.0532)	(0.0572)	(0.652)
	[0.001]	[0.001]	[0.845]	[0.668]	[0.791]
# days of delay	0.133	0.147	0.235**	0.249**	2.290*
	(0.176)	(0.180)	(0.108)	(0.119)	(1.295)
	[0.611]	[0.594]	[0.105]	[0.119]	[0.23]
Kleibergen-Paap F-statistic	170.4	170.4	170.4	170.4	170.4
Panel B: Smoothing only					
# days since last transfer	-0.506***	-0.553***	0.0456	0.0343	0.303
	(0.0811)	(0.0892)	(0.0414)	(0.0447)	(0.815)
	[0.001]	[0.001]	[0.508]	[0.611]	[0.853]
Kleibergen-Paap F-statistic	200.7	200.7	200.7	200.7	200.7
Panel C: Delay only					
# days of delay	-0.478***	-0.521***	0.211**	0.207**	1.895
	(0.129)	(0.132)	(0.0830)	(0.0914)	(1.605)
	[0.003]	[0.002]	[0.054]	[0.085]	[0.471]
Kleibergen-Paap F-statistic	302.6	302.6	302.6	302.6	302.6
Household fixed effects	Yes	Yes	Yes	Yes	Yes
N	6178	6178	6178	6178	6178
Mean if no delay	28.31	28.50	37.15	40.12	763.0

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1). Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Standard errors are clustered at the household level, and at the survey date by camp of registration level. Anderson (2008) sharpened q-values are shown in square brackets. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tables 3 report the effects of aid delays on sleep quality, mood, happiness, time and risk preferences, and various tests of math, memory, and cognitive abilities. Based on sharpened q-values, most regression coefficients are statistically insignificant. Overall, we find no evidence that these variables are impacted by delays.

Yet, during our qualitative research, refugee respondents frequently described the severe impact of food insecurity and high debt levels on their mental well-being. When asked about the main consequences of food scarcity and indebtedness, one refugee female from South Sudan living in Kalobeyei shared: *"To me, sometimes I lock myself inside, and I don't sleep. Sometimes from 11pm, 12am to 3am I don't sleep. I just keep thinking: what kind of life is this?"* Another female refugee from South Sudan but living in Kalobeyei described the mental toll of hunger: *"Sometimes you think until your mind is running short of things to think. Sometimes you're just silent [...] Sometimes you think to a point when you find KES 50 and you just think of getting local alcohol and drink so that you can sleep without thinking."* One refugee mother from South Sudan living in Kakuma explained the impacts on her children: *"When children sleep hungry, it might bring stress to them [...] They get sick, and the sickness is of hunger because they lack energy in the body and get weak due to hunger."* A male South Sudanese refugee residing in Kalobeyei expressed high level of stress because of debt: *"I can't sleep well at night because I am just thinking of a way to pay this debt. It has been something frustrating and I am working hard because I want to clear all this debt."* For some, the psychological toll of debt was overwhelming. When asked how she felt after accumulating a large amount of debt, another female refugee from South Sudan and living in Kalobeyei responded: *"The months are stressful because I have debt at the shop and I never cleared that. This affects me a lot. When I think about it I feel like finishing myself."*

Several factors may explain the discrepancy between quantitative and qualitative results related to sleep and mental well-being. The null findings from regression analysis could reflect a genuine absence of impact, while qualitative reports may be anecdotal. Alternatively, the lack of quantitative impacts may result from measurement error, leading to attenuation bias (Hausman 2001). A third possibility is that our specification, which focuses on short-term dynamics within the monthly cycle, cannot capture effects that accumulate over time. For instance, aid delays cause debt to build up over months or years, which qualitative evidence suggests may generate stress in the longer term, even if short-term regressions fail to detect it.

The analysis of household data shows that refugees are generally able to smooth consumption between regular transfers, but delays have severe and widespread effects: reducing food consumption, worsening food security, and increasing reliance on debt. While our quantitative analysis cannot identify downstream impacts on mental health and cognition, qualitative evidence indicates that food insecurity and unsustainable debt burdens may translate into heightened stress and hardship.

Table 3: Impacts of aid distribution and aid delays on mental health, time and risk preferences, cognitive abilities, memory and math

	Mental well-being			Time preferences		Risk pref.
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Smoothing and delay						
# days since last transfer	0.00265	0.00737	0.00288	0.000173	0.000210	-0.00103
	(0.00181)	(0.00545)	(0.00181)	(0.00113)	(0.000677)	(0.00116)
	[0.356]	[0.381]	[0.314]	[1]	[0.98]	[0.653]
# days of delay	-0.000118	-0.00800	-0.00516	0.00176	0.000994	0.00184
	(0.00337)	(0.00982)	(0.00370)	(0.00208)	(0.00124)	(0.00228)
	[1]	[0.653]	[0.379]	[0.653]	[0.653]	[0.653]
Kleibergen-Paap F-statistic	221.3	221.3	169.3	171.0	170.4	170.4
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	6178	6178	6173	6122	6178	6178
Mean if no delay	3.023	4.843	2.716	1.839	0.879	1.343

	Cognitive abilities			Memory and math	
	Flanker task score (0-10)	Psychomotor vigilance task score (0-30)	Hearts flowers task score (0-40)	Math test score (0-3)	Memory & test score (0-36)
	(1)	(2)	(3)	(4)	(5)
Panel A: Smoothing and delay					
# days since last transfer	-0.0000683	-0.00338	-0.0391**	0.000178	0.0109
	(0.00333)	(0.00827)	(0.0162)	(0.00197)	(0.0124)
	[1]	[0.933]	[0.069]	[1]	[0.653]
# days of delay	-0.00116	0.0218	-0.0105	-0.00302	-0.00461
	(0.00560)	(0.0148)	(0.0270)	(0.00353)	(0.0234)
	[1]	[0.356]	[0.941]	[0.653]	[1]
Kleibergen-Paap F-statistic	170.4	86.22	86.22	170.4	170.4
Household fixed effects	Yes	Yes	Yes	Yes	Yes
N	6178	3069	3069	6178	6178
Mean if no delay	8.613	28.29	35.24	2.085	19.24

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1). Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Standard errors are clustered at the household level, and at the survey date by camp of registration level. Anderson (2008) sharpened q-values are shown in square brackets. Panel B and C are presented in Annex Table A.6. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.3.4 Heterogeneous Effects

We examine heterogeneous effects across Kakuma and Kalobeyei to better understand the mechanisms behind households' inability to smooth consumption during aid delays. Although the total monthly value of assistance is similar across sites, the modalities differ: Kakuma households receive two smaller transfers per month—one in-kind and one in cash—while Kalobeyei households receive a single monthly cash transfer. These differences likely shape impacts and coping strategies. In both sites, the ATM and SIM cards used for cash transfers serve as collateral to secure credit from food retailers. Because cash transfers are larger in Kalobeyei, this collateral has higher value, giving households greater access to debt to cope with delays.

We replicate our benchmark analysis separately for households registered in Kakuma and Kalobeyei in Appendix Tables A.18 and A.19. We obtain similar results if we instead focus on actual place of residency (Appendix Tables A.20 and A.21) or on households receiving in-kind and cash transfers versus those receiving cash transfers only (Appendix Tables A.22 and A.23).[25]

We emphasize that the results of this analysis should be interpreted with caution as households' place of residence was not randomly assigned. While our knowledge of the context and qualitative evidence suggest the modality of assistance is the main factor driving differences between the two sites, we cannot rule out that other factors may also play a role.

We find important differences in how refugee households registered in the two sites cope with delays. The effects of cash transfer delays on nutrition are observed in both sites, but they are significantly larger in magnitude for households registered in Kakuma, where households receive a mix of in-kind and cash assistance. For daily calorie intake, the estimated regression coefficient of delays is 3.7 times larger in Kakuma than in Kalobeyei. In Kakuma, the regression model predicts a reduction of 347 calories (16%) after 11 days of delay and 725 calories (34%) after 23 days, compared to 93 calories (4%) and

[25]Regressions with interaction terms are shown in Appendix Tables A.24 to A.26.

195 calories (8%) in Kalobeyei.[26] Similar patterns emerge for other nutrition and food security outcomes.

By contrast, impacts on debt are much larger for households registered in Kalobeyei, indicating they rely much more on credit secured with their ATM or SIM cards to cope with aid delays. In Kalobeyei, regressions predict that an 11-day delay increases total debt per capita by USD 3.4 (5%) and a 23-day delay by USD 7.2 (10%). Regression coefficients are much smaller and statistically insignificant in Kakuma. Consistent with these results, average household debt per capita in Kalobeyei is approximately five times higher than in Kakuma.

These findings highlight the central role of credit and liquidity constraints in shaping households' ability to cope with delays. Access to debt linked to cash transfers allows households to buffer shocks. Yet this coping strategy comes at a cost: households that borrow face significantly higher prices when purchasing goods on credit from their primary food retailers. Our price data indicate that goods bought on credit are, on average, 17% more expensive, effectively functioning as an implicit interest rate. Assessing the welfare implications of delays thus requires weighing the consumption-smoothing benefits of credit against its borrowing costs. We examine these trade-offs in Section 2.5, comparing scenarios with and without access to debt.

2.3.5 Robustness Tests

We assess the robustness of our main specification against various modifications in Appendix Tables A.7 to A.17.

Appendix Tables A.7 and A.8 show that results remain consistent when excluding control variables or household fixed effects. Similarly, results are similar after excluding the 34 households that attrited during the study period along with their replacements, or including observations on transfer dates (Appendix Table A.9 and A.10).

[26] Delays were on average shorter in Kakuma during our study period, partly attenuating these differences (Figure 3).

In Appendix Table A.11, we use an extended specification that accounts for potential differences in consumption smoothing following cash and in-kind transfers. Consistent with benchmark results, the coefficients of both smoothing variables are statistically insignificant and close to zero with most outcomes. The main exception is food stocks, which decline with the number of days since aid distribution. This decline is more pronounced for in-kind aid, which is likely due to the fact that households receive in-kind rations in a single installment, whereas cash transfers offer greater flexibility in usage throughout the aid cycle. The coefficients of the delay variables remain consistent with our benchmark results.

To test for potential non-linearities, quadratic terms were added for the smoothing and delay variables (Appendix Tables A.12 and A.13). In both cases, the coefficients of the quadratic terms are usually statistically insignificant, which is why we focus on linear regressions in our main specification.

In Appendix Table A.14, we also consider an alternative definition of delay variables, as a dummy variable equal to one when aid is delayed and zero otherwise. The coefficients of the smoothing variables remain statistically insignificant and close to zero for most outcomes, except for food stocks. Cash delays lead to lower food consumption, higher food insecurity, and increased indebtedness. Reduced-form regressions also yield similar results (Appendix Table A.15).

In our main specification, standard errors are clustered at the household level and the survey date by camp of registration. Conclusions are similar if we consider only one level of clustering or use robust standard errors (Appendix Tables A.16 to A.17).

2.4 Analysis of Price Data

2.4.1 Identification Strategy

We use high-frequency market data to study how food prices fluctuate across the aid cycle and assess whether aid delays impact prices. We estimate a regression model analogous

to the one used in the analysis of household data:

$$Log(P_{jkt}) = \beta_0 + \beta_1 Days_{kt} + \beta_2 Cash\ delay_{kt} + \delta^T X_{jkt} + Product_j + Market_k + \epsilon_{jkt}, \tag{2.2}$$

where P_{jkt} represents the price of product j in market k at time t.

The variable $Days_{kt}$ is the number of days since the last transfer and $Cash\ delay_{kt}$ measures the number of days a cash transfer is overdue.[27] The coefficient β_1 captures how prices fluctuate following aid transfers, while β_2 isolates the additional effect of delays in cash transfers.

As in the analysis of household data, the variables $Days_{kt}$ and $Cash\ delay_{kt}$ are highly correlated (coefficient of correlation = 0.70), raising concerns about multicollinearity. To address this, we also estimate specifications with each variable included separately, noting that the results of these short specifications should only be interpreted if the coefficient of the omitted variable is close to zero and statistically insignificant in the full specification.

Explanatory variables of interest depend on the distribution and survey dates. The variation in distribution dates is assumed to be quasi-random, determined by the WFP and its partners independently of market dynamics. Price data were collected weekly across various markets, and all regressions include market fixed effects to account for unobservable market characteristics that remain constant over time.[28] Additionally, product fixed effects are included to control for differences in average price levels between products. Prices are expressed in logarithms to interpret regression coefficients in percentage terms.[29] Benchmark regressions also incorporate a vector $X_{i,t}$ of control variables, which

[27] In Kakuma, $Days_{kt}$ is defined as the number of days since the most recent transfer, cash or in-kind, whichever occurred more recently. In Kalobeyei, it is the number of days since the last cash transfer. Because cash and in-kind transfers could plausibly have different effects on prices (Cunha et al. 2019), we also estimate a specification that distinguishes between the two modalities, but find no significant differences (Appendix Tables A.35 and A.36).

[28] Market fixed effects are defined at the level of the four Kakuma camps and the three Kalobeyei villages.

[29] The logarithmic transformation reduces the importance of outliers at the top of the distribution and

capture the content of in-kind rations and the market impacts of an aid cut that occurred during the data collection period (see Bruni and Sterck 2025).[30] Results are robust if we exclude market fixed effects or control variables (Appendix Tables A.27 to A.30).

Standard errors are two-way clustered. The first dimension is the product–market level, which is the unit of observation repeatedly surveyed over time. The second is survey date by camp, since all prices observed in the same camp on the same day have the same treatment status. Results are robust to using only one clustering dimension or heteroskedasticity-robust standard errors (Appendix Tables A.33 and A.34).

To address concerns regarding multiple testing, we also include sharpened q-values, which are calculated using all p-values from the price analysis (Benjamini et al. 2006; Anderson 2008).

We assess the robustness and consistency of benchmark results with multiple robustness tests. Specifically, we re-estimate Equation (2.2) without control variables (Appendix Tables A.27 and A.28), and without fixed effects (Appendix Tables A.29 and A.30). We compare standard errors obtained with only one-way clustering (Appendix Tables A.31 and A.32), and with heteroskedasticity robust standard errors (Appendix Tables A.33 and A.34). Additionally, we test a specification accounting for potential differences in prices following cash and in-kind transfers (Appendix Tables A.35 and A.36). We also consider specifications where the outcome variable is the price divided by the mean or median price of each product (Appendix Tables A.37 to A.40). The results of our main specification remain virtually unchanged across these robustness tests.

2.4.2 Outcome Variables

We analyze two different measures of prices.

magnifies differences at its bottom. We obtain similar results if we instead express prices in percentage of the mean or median for each product (Appendix Tables A.37 to A.40).

[30]To capture dynamics specific to in-kind distributions, the vector of controls includes a binary indicator for the staple cereal provided in the most recent distribution. For example, this variable equals 1 for rice between October 3, 2022, and November 6, 2022, when rice was the main staple distributed. We also control for the number of days since that cereal was distributed. In addition, the vector of controls includes binary and continuous variables identifying observations before and after the aid cut of July 3, 2023.

1. Cash prices: Cash prices refer to the amounts refugees pay when purchasing goods using banknotes and coins. We collected weekly data on the prices of 70 commonly traded food and beverage items in the different markets of Kakuma and Kalobeyei. This includes staples such as maize, rice, and oil, as well as other items like tomatoes or milk. For the analysis, we examine the prices of all goods collectively and the prices of staple foods separately.[31]

2. Aid-card prices: Previous research shows that the law of one price does not consistently hold in Kakuma and Kalobeyei, even within individual shops (Siu et al. 2023; Delius and Sterck 2024). Customers paying with SIM cards linked to restricted cash transfers are often charged significantly higher prices for the same goods. Refugees receiving unrestricted cash transfers also often face higher prices if they pay using their ATM aid cards. These customers usually have outstanding debts to their shopkeepers and are charged inflated prices as an implicit interest rate. Shopkeepers justify these higher prices as necessary to cover transaction costs and default risks.

The practice of overcharging customers who pay with SIM or ATM cards is explicitly prohibited by the WFP. As a result, our price survey initially focused on cash prices only. Starting from June 2023, we also asked shopkeepers to report aid-card prices.[32] A simple regression with market-week fixed effects shows that aid-card prices are approximately 15% higher than cash prices.[33] The difference is 14% in Kakuma and 17% in Kalobeyei. The slightly larger markup in Kalobeyei is likely driven by higher levels of indebtedness in that area. While we use this data in the analysis, we note that misreporting is possible and only three months of data were collected. Results with this variable should therefore be interpreted with caution.

[31]Staples include maize (kg or bag), maize flour (kg or bag), sorghum (kg or bag), sorghum flour (kg or bag), wheat flour (kg or bag), corn-soya blend, rice (kg or bag), pasta (500g), oil (1L, 3L, or 5L), sugar (kg), beans (kg), lentils (kg), cowpeas (kg), and potatoes (kg).

[32]In some markets, our enumerator recorded these prices as aid-card prices while in others, she recorded them as debt prices. For a few places and weeks, we have both sets of prices and find that differences between both sets of prices are small, less than 2%. We group these prices together in the analysis and include a dummy identifying differences in how these prices were labeled.

[33]Such markup is consistent with previous evidence from Siu et al. (2023) and Delius and Sterck (2024), who find price differences ranging from 16% to 33% based on household expenditure data.

We analyze price data for Kakuma and Kalobeyei both jointly and separately. The impact of cash transfer delays may differ in Kalobeyei, where transfer amounts are larger but debt is more commonly used as a coping mechanism for shocks. However, given the close proximity of the two sites, we anticipate differences to be relatively small.

2.4.3 Results

Table 4 reports the results for cash prices. Panel A includes both variables of interest in the regression. Based on sharpened q-values, none of the coefficients are statistically significant. This null result could reflect multicollinearity between the two variables (correlation coefficient = 0.7). A different pattern emerges in Panels B and C, where the variables are introduced separately. Panel B shows that cash prices rise with the number of days since the last transfer, while Panel C shows that prices are higher when aid is delayed. These effects are concentrated in staple foods, with no meaningful differences between Kakuma and Kalobeyei. The results in Panels B and C reflect the same underlying dynamic: when aid is delayed, the number of days since the last transfer is higher, and cash prices tend to rise. The estimates in Panel C imply that an average 11-day delay raises staple prices by about 3%, while a maximal 23-day delay raises them by about 6%.

Table 5 reports the results for aid-card prices. Delays in cash transfers tend to reduce these prices, particularly for staple foods. Panel C implies that staple prices fall by about 5% following an average 11-day delay, and by about 11% following a 23-day delay. The estimated coefficients are larger in Kakuma, where assistance is provided as a mix of in-kind and cash. A plausible explanation is that market effects are more pronounced in Kakuma, where demand fluctuates more with delays because households are less likely to use debt to cope with the shock.

The contrasting effects of delays on cash and aid-card prices can be explained by the substitutability between hard cash and cash-transfer money. Immediately after cash-transfer distribution, households rely mainly on the cash transfers they just received, which keeps aid-card prices high and cash prices low. As the distribution date recedes,

households increasingly use hard cash to fill consumption gaps. Just before the expected delivery of cash transfers, shops typically replenish stocks in anticipation of higher demand. However, if cash transfers are delayed, supply is high as stocks are full but demand with cash-transfer money is low as transfers are yet to be delivered. This causes aid-card prices to fall. At the same time, households use hard cash to bridge the gap, driving cash prices up.

These dynamics illustrate how aid transfers shape market outcomes. On average, aid-card prices are about 15% higher than cash prices, yet both fluctuate over the aid cycle. Immediately after cash transfers are delivered, aid-card prices peak as households use cash-transfer money to buy food. As the cycle progresses, and especially when transfers are delayed, aid-card prices fall below their average level, while cash prices rise as households rely more on hard cash until transfers are delivered.

Table 4: Impacts of aid distribution and aid delays on cash prices (log)

	Dependent variable: log prices (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods	Staple	All goods	Staple	All goods	Staple
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Smoothing and delay						
# days since last transfer	0.000450	0.000914*	0.000168	0.00117	0.000416	0.000497
	(0.000312)	(0.000517)	(0.000459)	(0.000721)	(0.000626)	(0.000728)
	[0.24]	[0.162]	[0.561]	[0.189]	[0.426]	[0.426]
# days of delay	0.00109	0.00125	-0.000235	0.00211*	0.00169	0.00174
	(0.000661)	(0.00116)	(0.00141)	(0.00116)	(0.00114)	(0.00178)
	[0.189]	[0.3]	[0.657]	[0.158]	[0.235]	[0.333]
Panel B: Smoothing only						
# days since last transfer	0.000748***	0.00125***	0.000149	0.00134*	0.00106***	0.00115**
	(0.000267)	(0.000395)	(0.000478)	(0.000734)	(0.000324)	(0.000432)
	[0.026]	[0.015]	[0.566]	[0.158]	[0.015]	[0.038]
Panel C: Delay only						
# days of delay	0.00177***	0.00262***	-0.000130	0.00282*	0.00242***	0.00262**
	(0.000557)	(0.000926)	(0.00135)	(0.00164)	(0.000505)	(0.00105)
	[0.015]	[0.026]	[0.678]	[0.178]	[0.001]	[0.046]
Market and Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16760	5458	9841	3189	6919	2269

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of cash prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Impacts of aid distribution and aid delays on aid-card prices (log)

	Dependent variable: log prices (aid card)					
	Both sites		Kakuma		Kalobeyei	
	All goods	Staple	All goods	Staple	All goods	Staple
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Smoothing and delay						
# days since last transfer	0.000151	-0.000121	-0.000232	0.000829	0.000244	-0.000847
	(0.000480)	(0.00107)	(0.000842)	(0.00177)	(0.000263)	(0.00109)
	[0.566]	[0.678]	[0.58]	[0.549]	[0.37]	[0.396]
# days of delay	-0.00138	-0.00450**	-0.00612**	-0.0196***	-0.000557	-0.00178
	(0.00106)	(0.00201)	(0.00230)	(0.00512)	(0.000474)	(0.00177)
	[0.273]	[0.079]	[0.046]	[0.015]	[0.3]	[0.333]
Panel B: Smoothing only						
# days since last transfer	-0.000214	-0.00130	-0.00101	-0.00172	0.0000643	-0.00142*
	(0.000325)	(0.000806)	(0.000821)	(0.00199)	(0.000175)	(0.000731)
	[0.426]	[0.189]	[0.3]	[0.382]	[0.561]	[0.158]
Panel C: Delay only						
# days of delay	-0.00110	-0.00473***	-0.00686***	-0.0169***	-0.000122	-0.00331**
	(0.000676)	(0.00144)	(0.00187)	(0.00351)	(0.000327)	(0.00115)
	[0.189]	[0.015]	[0.015]	[0.004]	[0.561]	[0.044]
Market and Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6949	2996	4084	1768	2865	1228

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of aid-card prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.5 Welfare Impacts

Our analysis shows that aid delays significantly affect households, with notable differences between sites. In Kakuma, where assistance is provided as a mix of cash and in-kind, refugees respond to delays by temporarily reducing consumption. In Kalobeyei, where aid consists only of cash, households rely on debt to cope with delays and, as a result, the impact on consumption is smaller.

In this section, we propose back-of-the-envelope calculations to illustrate the welfare costs of delays. This exercise requires making different assumptions as we do not observe a group of refugees who never face delays and should be interpreted as illustrative rather than as a precise estimate of welfare losses. All refugees are affected by delays and our identification strategy can only capture how consumption and other indicators fluctuate within households as a function of aid distribution dates and delays.

We construct counterfactual scenarios that are based on regression results, separately for Kakuma and Kalobeyei. Our analysis of welfare impacts focuses on the value of food consumption as our data does not allow us to measure short-term variation in other types of expenditure (we estimate that food expenditures represent about 83% of total expenditures). We use an Atkinson Social Welfare Function with a degree of inequality aversion equal to 2, in line with Sterck (2024). We express daily consumption per capita in 2017 PPP$ and bottom-code the data at PPP$ 0.25 per day to prevent very low values from dominating our estimates (Kraay et al. 2024).

In Kakuma, we construct a counterfactual scenario without delays in three steps. First, we re-estimate Equation (2.1) with daily consumption in 2017 PPP$. Second, we "cancel" the effect of cash transfer delays by adding the predicted effect of delays ($\beta_2 Cash\ delay_{i,t}$) to the value of daily consumption. Finally, we rescale the resulting variable down so that each household's average in the counterfactual scenario equals its observed average daily consumption.

Results are summarized in Table 6. On average, refugees require 1.09 days to consume

just PPP$ 1 when delays occur, compared to 1.07 days in the absence of delays. Atkinson's Equally Distributed Equivalent Income (EDEI)—which measures the level of per capita income that, if equally distributed across all individuals, would generate the same level of social welfare as the observed unequal distribution—is PPP$ 0.92 per person per day with delays and PPP$ 0.93 without delays. Delays in Kakuma imply a welfare loss of approximately 1.5%.

We estimate the monetary value of this loss for the 400,000 refugees in Kenya who relied on a combination of in-kind and cash transfers in October 2022, when our research began.[34] We estimate that the cost of delays is equivalent to PPP$ 2.1 million per year, or USD 1.1 million at market exchange rates.

Table 6: Measured and counterfactual welfare indicators

	Kakuma		Kalobeyei		
	Delays	No delays	Delays	No delays	
				Debt	No debt
	(1)	(2)	(3)	(4)	(5)
Average poverty, in days/PPP$	1.09	1.07	1.12	1.10	0.95
EDEI, in PPP$/day	0.92	0.93	0.89	0.91	1.05
Welfare impact, in %		1.5		1.75	15
Welfare impact, in million PPP$		2.1		0.28	2.5
Welfare impact, in million USD		1.1		0.15	1.4

Notes: Counterfactual scenarios are described in the text. Average poverty is average number of days needed to consume $1 (Sterck 2024). Atkinson's Equally Distributed Equivalent Income (EDEI) is the inverse of average poverty; it is the level of per capita income that—if equally distributed across all individuals—would generate the same level of social welfare as the observed unequal distribution. The Social Welfare Function (SWF) is an Atkinson SWF with a degree of inequality aversion of 2. The estimates of welfare losses compare the EDEI in different scenarios. We use UNHCR population data to estimate welfare losses for the entire camp populations.

In Kalobeyei, households cope with delays primarily through debt, with two main implications. First, direct impacts on consumption are smaller. Using the same counterfactual approach as in Kakuma, we estimate a regression coefficient less than half as large. However, because delays were typically longer in Kalobeyei during the study period, the overall welfare impacts are similar. On average, refugees require 1.12 days to obtain PPP$

[34]This includes refugees in Kakuma and Dadaab camps.

1 when delays occur, compared to 1.10 days in the absence of delays. Atkinson's EDEI is PPP$ 0.89 per person per day with delays and PPP$ 0.91 without delays. Delays imply a welfare loss of about 1.8%.

Second, reliance on debt exposes households to higher prices. Indebted households face an implicit interest rate through higher markups on goods purchased on credit. Using price data, we estimate that aid-card prices are 17% higher than cash prices in Kalobeyei, consistent with previous estimates by Delius and Sterck (2024) and MacPherson and Sterck (2021). Nearly all households in our sample (97%) reported borrowing from their main food retailer and are therefore likely to face these higher prices. Since about 89% of household income in Kalobeyei comes from cash transfers, this mechanism has a large impact.

To incorporate this into the counterfactual, we assume that debt is primarily caused by delays and that, in the absence of delays, households would not be indebted and would therefore avoid these implicit interest costs. We adjust the counterfactual measure of consumption for indebted households upward, assuming that without delays, the portion of consumption financed by cash transfers would be 17% cheaper. The welfare effects on businesses are assumed negligible relative to those on households. As shown by Delius and Sterck (2024), only a limited number of food retailers operate in Kalobeyei, benefiting from significant market power and high revenues. Consequently, welfare effects on these few businesses are expected to be of secondary importance.[35]

When accounting for debt, we estimate that refugees would need 0.95 days on average to consume PPP$ 1 in the absence of delays, compared to 1.12 days with delays. Atkinson's EDEI is PPP$ 1.05 in the counterfactual scenario without delays and debt. The welfare loss due to delays and debt in Kalobeyei is 15%, which is substantially larger than in Kakuma. This shows that debt may not be an optimal coping strategy in the

[35]During qualitative interviews, businesses in Kalobeyei reported struggling with extreme levels of indebtedness. Many rely on credit from wholesalers to purchase stock when aid is delayed, while customers frequently buy goods on credit (see also Bruni et al. 2024). As a result, reducing indebtedness is expected to have mixed welfare effects on businesses—potentially lowering profits in the short term but enhancing long-term financial sustainability.

long run, even if it helps smooth consumption in the short run.

The monetary value of the welfare loss for the 47,000 refugees living in Kalobeyei as of October 2022 amounts to roughly PPP$ 2.7 million per year, or USD 1.4 million at market exchange rates.

2.6 Conclusion

More than 300 million people worldwide require humanitarian assistance (Humanitarian Action 2023). However, growing funding shortfalls, logistical obstacles, and security threats often force essential programs to be cut, scaled back, or delayed. News and images from Gaza, DR Congo, Ethiopia, Sudan, and other crises suggest these disruptions are likely to have dramatic consequences.

The chapter assessed the welfare and market impacts of delays in humanitarian assistance in Kenya, where refugees living in camps receive a combination of in-kind and cash transfers every month. Cash transfers were almost always delayed during our one-year high-frequency survey, sometimes by several weeks. To study the impact of delays, we exploited high-frequency household data, combined with random variation in interview dates and quasi-random variation in aid delays. We also used high-frequency price data to document how prices vary within the monthly aid cycle in response to the timing of transfers.

Our setting, where the value of aid is predictable but its timing is uncertain, allows us to test competing models of intertemporal choice: from classical models such as the PIH, which predict smooth consumption, to behavioral models and models with liquidity, savings, or credit constraints, which predict limited smoothing and, in extreme cases, hand-to-mouth behavior.

We summarize our findings around two fundamental questions. First, can refugee households effectively smooth consumption across the regular aid cycle? Second, do

delays in aid delivery affect their ability to do so? The PIH predicts a yes to both: consumption should track expected lifetime income, not temporary fluctuations. In contrast, behavioral models with high discounting, temptation, or limited attention predict a no to both: households should overspend transfers and fail to plan ahead, leading to cyclical consumption even when aid is predictable. Our results support a third possibility: yes to the first and no to the second.

Indeed, we find that households are generally able to smooth consumption over the course of a regular aid cycle. However, when transfers are delayed, we observe sharp declines in food consumption and food security, along with increased indebtedness. These results highlight the importance of incorporating program disruptions into impact evaluations. An analysis that would ignore delays would misleadingly suggest that households struggle to smooth consumption, pointing to present bias, temptation, or poor planning as the problem. In reality, when aid is delivered on time, refugees do smooth consumption well. The problem lies in unpredictable shocks, not behavioral frictions. This distinction has direct implications for policy and program design, discussed below.

Our analysis identifies credit constraints as a key mechanism underlying households' inability to smooth consumption during aid delays. Households with better access to credit—particularly in Kalobeyei where future cash transfers are widely used as collateral—are able to smooth consumption when aid is delayed, consistent with the predictions of the PIH. By contrast, tighter credit constraints—as in Kakuma where only a portion of aid is delivered in cash—limit households' ability to cope with delays. While debt helps refugees smooth consumption during delays, it comes at a significant welfare cost, as purchasing goods on credit increases prices by approximately 17%, reflecting implicit interest rates.

Our results show that addressing delays is of primary importance. First, because delays have significant impacts on household welfare. Second, because aid delays—and more generally disruptions in humanitarian assistance—are widespread, affecting the poorest and most vulnerable segments of the world's populations. Third, because addressing aid

delays may be a low-hanging fruit in a context of reduced aid budgets.

We propose different policy measures to address aid delays. First, since multiple humanitarian agencies and teams within these organizations are involved in aid distribution, strengthening coordination mechanisms is crucial. This is not merely a management issue. Given the significant funding constraints these organizations face, donors must ensure they are adequately resourced and incentivized to deliver aid efficiently and effectively. Second, humanitarian agencies should commit to disbursing a portion of cash transfers on time, even if beneficiary lists are not fully finalized, to prevent disruptions in household consumption. Most refugee crises are protracted, with households remaining in camps for several years. Consequently, the proof-of-life verification process—one of the primary causes of delays—rarely results in dramatic changes to distribution lists. Third, staggering aid transfers within refugee camps could help reduce collective financial strain and enable traditional coping mechanisms through support networks to function more effectively. Finally, addressing over-indebtedness and its consequences should be a priority to prevent long-term financial vulnerabilities, especially in Kalobeyei. Research to identify effective approaches to prevent and reduce indebtedness is much needed (Garz et al. 2021; Karlan et al. 2019). These policy implications are expected to be not only relevant for the nearly 600,000 refugees in Kenya but also for the 300 million people worldwide who depend on humanitarian assistance.

2.7 Appendix

2.7.1 Balance Tests and Variables

Table A.1: Balance table for delays

	Delay dummy			
	(1)	(2)	(3)	(4)
Date	0.104	0.124	0.123	0.203
	(0.120)	(0.122)	(0.129)	(0.151)
Date sq.	-0.00000225	-0.00000269	-0.00000266	-0.00000447
	(0.00000259)	(0.00000264)	(0.00000280)	(0.00000328)
Weekend dummy	0.0387	0.0367	0.0297	0.0240
	(0.0593)	(0.0596)	(0.0601)	(0.0822)
Holiday dummy	0.0807	0.0923	0.0850	0.147*
	(0.0734)	(0.0757)	(0.0780)	(0.0811)
Precipitation		0.00335	0.0290	0.0307
		(0.00638)	(0.0227)	(0.0261)
Highest temperature		-0.00769	-0.00684	0.0182
		(0.0134)	(0.0167)	(0.0217)
Lowest temperature		0.0124	0.0138	0.00373
		(0.00959)	(0.0170)	(0.0210)
Precipitation (sq. dev.)			-0.000745	-0.000842
			(0.000632)	(0.000712)
Highest temperature (sq. dev.)			-0.00000807	0.00247
			(0.00281)	(0.00318)
Lowest temperature (sq. dev.)			0.000122	-0.0000355
			(0.000888)	(0.00107)
Exchange rate				0.0459*
				(0.0263)
Exchange rate, growth				2.763
				(4.241)
Constant	-1201.4	-1436.2	-1416.9	-2312.3
	(1384.2)	(1408.5)	(1492.8)	(1735.2)
Observations	366	366	366	308
R^2	0.006	0.013	0.017	0.036
Fri	0.688	0.700	0.801	0.552

Notes: The p-values for the F-test are calculated using randomization inference, following the recommendation of Kerwin et al. (2024). The dependent variable is a dummy variable equal to one if restricted or unrestricted cash transfers were delayed and zero otherwise. Weather variables are taken from Climate Prediction Center of the U.S. National Oceanic and Atmospheric Administration. Exchange rate data is taken from Google Finance. Regressions consider days between the 1st of October 2022 and the 1st of October 2023.

Table A.2: Balance tests for randomly assigned interview date

	Dependent variable: Randomly-assigned interview date (1)
Household size	0.0194
	(0.0419)
Nr of adults in hh	-0.0365
	(0.0596)
Child headed household	1.278
	(1.073)
Respondent is the household head	0.296
	(0.249)
Respondent gender	0.0617
	(0.293)
Respondent age	-0.000564
	(0.0110)
Respondent education	0.00930
	(0.0289)
Respondent speaks Dinka	-0.0615
	(0.456)
Respondent speaks Juba Arabic	-0.315
	(0.318)
Respondent speaks Nuer	-0.271
	(0.446)
registration date	-0.00832
	(0.0235)
household has electricity	0.355
	(0.285)
Kakuma 1	-0.413
	(0.466)
Kakuma 2	-0.886
	(0.616)
Kakuma 3	-0.395
	(0.479)
Kalobeyei 1	-0.776
	(0.750)
Kalobeyei 2	-0.833
	(0.752)
Kalobeyei 3	-0.599
	(0.756)
Number of ration cards	0.0762
	(0.0994)
Ration card total size	0.00148
	(0.0360)
Bamba chakula beneficiary	0.471
	(0.661)
Observations	622
R^2	0.019
F-test p-value (OLS, randomization inference)	0.955
Chi^2-test p-value (mlogit, randomization inference)	0.643

Notes: The p-values for the F-test and Chi^2-test of joint orthogonality are calculated using randomization inference, following the recommendation of Kerwin et al. (2024). The dependent variable represents the 3-day intervals randomly assigned to households at baseline, which determine target interview dates across all survey waves.

Table A.3: List of control variables used in the analysis of household data

Variable	Variable description
Children in school	Count variable representing the number of children aged 6 or older who attended school on the day of the interview.
Shock dummy	Binary variable which takes the value of one if the household faced a shock (theft, ceremony, having to pay bribes, natural hazards, or other shocks) in the four weeks preceding the interview and zero otherwise.
Jobs per capita	Number of jobs per household member (only adult employment).
Harvest value	Value of the food harvested during the four weeks preceding the interview, estimated using prices collected at baseline, and expressed in Kenyan Shillings.
Random order behavioral module	Each questionnaire had a behavioral module, which was randomly assigned to appear at the beginning or end of the questionnaire. This variable is a binary variable taking the value of one if the behavioral module appeared at the end of the questionnaire and zero otherwise.
Household size	Count variable indicating the number of people living in the household.
Adults	Count variable indicating the number of adults living in the household.
Size of ration cards	Count variable indicating the total number of individuals listed on the household ration cards.
In-kind basket value	Per capita value of last in-kind basket, estimated using prices collected at baseline (equal to zero if the household does not receive in-kind aid).
Ration cut, dummy	Dummy variable equal to one for interviews done after the 3rd of July 2023, when a ration cut of about 20% was implemented.
Ration cut, running variable	Running variable identifying interview dates, equal to zero on the 3rd of July 2023.
Ration cut, interaction	Interaction variable between the ration cut dummy and the running variable.

Table A.4: List of outcome variables used for the regressions with household data

Variable	Variable construction
Food consumption per capita: calories, value, and >2 meals	The food consumption module collected data on all food and beverages consumed, whether prepared at home or eaten outside. To estimate *calorie intake per capita* (kcal), we converted each food item to kilograms using the 2012 FAO density database (version 2.0) and multiplied it by its calorie content per kilogram. Total household calorie intake was then aggregated and adjusted for household size. Similarly, to estimate the per capita value of food consumption (USD), we converted the quantity of each item to kilograms, applied unit prices estimated at baseline, aggregated the total value, and divided by household size. Using the same data, we also constructed a binary variable taking the value of one if the household had two or more meals the day before the interview and zero otherwise.
Dietary diversity	We used data from the food consumption module to construct the Household Dietary Diversity Score (HDDS) for the day preceding the survey, following the methodology and food group classifications of (Kennedy et al. 2011).
Days/2100 kcal	Following Sterck (2024), we construct a distribution-sensitive measure of calorie intake by inverting per capita calorie intake and multiplying the result by 2100, yielding the number of days required to reach 2100 kcal.
HFIAS score	The Household Food Insecurity Access Scale (HFIAS) score measures household food insecurity. We constructed this variable following the 2007 FAO HFIAS methodology but adapted it to a one-day recall period (Coates et al. 2007b). From wave 2, the questionnaire asked whether the household experienced nine specific symptoms of food insecurity on the day before the survey, ranging from concerns about food scarcity to going an entire day and night without eating. The score reflects the number of symptoms experienced, ranging from 0 to 9.
Food stocks (kg and USD)	For seven staple goods—maize, sorghum, wheat flour, rice, pasta, and oil—our questionnaire asked households to report their stock quantities in kilograms. We then aggregated these weights to determine the total food stock in kilograms. Using baseline unit prices, we also estimated the total value of food stocks.
Debt per capita (USD)	The questionnaire asked households whether they owed debt to one or more food retailers and, if so, to report the amount in Kenyan Shillings. Additionally, households reported debts to family and friends (in goods or money), formal debts to banks, microfinance institutions, or NGOs, and debts to informal savings groups (such as merry-go-rounds). We express debt per capita and distinguish between debt from food retailers and total debt from all sources.
Value of assets and savings (KES)	This variable represents the total value of a household's physical assets, livestock, and savings at the time of the interview, expressed in KES, both at the household level and per capita. The questionnaire collected data on ownership of 24 asset types (e.g., televisions, mobile phones, tables, chairs, cooking pots) and 10 types of animals (primarily chickens, ducks, and doves). Savings amounts were self-reported. Asset values were calculated by multiplying the quantity owned by baseline market prices from our price survey.
Sleep quality Likert (1-5)	Sleep quality is assessed using answers to the following question, taken from the World Health Organization's Study on global AGEing and adult health (WHO-SAGE): "Please rate the quality of your sleep last night. Was it very good, good, moderate, poor or very poor?" Answers are coded from 1 "very poor" to 5 "very good".
Good mood score (0-9)	We compute a "mood score" based on nine questions from the WHO-SAGE study. These questions inquire whether participants experienced various moods on the day prior to the survey, such as enjoying activities or feeling worried, rushed, or calm (Kowal et al. 2012). Responses are binary and aggregated to create a score ranging from 0 (poor mood) to 9 (excellent mood) (Taquet et al. 2020).
Happiness Likert (1-5)	Happiness is assessed using answers to the following question, taken from World Values Survey: "All things considered, how satisfied are you with your life as a whole today?" Answers are coded from 1 "very unhappy" to 5 "very happy".
Required Rate of Return	The questionnaire included five trade-off questions in which participants chose between receiving 500 Kenyan Shillings (KES) in two months or a smaller amount in one month. We calculate the Required Rate of Return (RRR) in two steps (Cohen et al. 2020). First, we determine the indifference point—the amount xx at which the respondent switches their choice. Second, we estimate the RRR using the formula: $RRR = log(500/x)$.
Payment now (dummy)	The questionnaire included a binary choice question: receive KES 200 immediately or KES 300 in one month. For logistical reasons, the incentive payment was based on this specific question. Respondents who chose the immediate payment received it on the spot, while those who opted for the delayed payment received it during the next survey wave. We treat responses as a binary variable.
Risk loving score (1-4)	To assess risk preferences, participants answered three questions, each offering a choice between a guaranteed KES 200 and a larger amount (KES 300, 400, or 500) with a 50% probability of payout (Dave et al. 2010). Based on their choices, we classify participants into four risk tolerance groups.
Math test score (0-3)	In each wave, mathematical ability was tested using three arithmetic questions related to common market transactions. One example of question was: "One bag of rice costs KES 2800, and one fish costs KES 250. You buy one bag of rice and three fish. How much do you have to pay?" Due to a coding error in one survey wave, six questions were included instead of three. For consistency, we divided the number of correct answers by 2 in that wave to ensure the variable ranged between 0 and 3. The number of correct responses is used as the outcome variable.
Memory test score (0-36)	Memory was assessed using an adaptation of the Simon memory game, following Carvalho et al. (2016). Participants were shown sequences of colored shape (red O,blue \$, yellow +, or green Δ) of increasing length. They were then asked to replicate the sequence from memory. Each respondent first completed four practice trials with sequences of one to four symbols, followed by eight test trials with sequences of one to eight symbols. The outcome variable is the number of correct responses.
Flanker task score (0-10)	Selective attention and inhibitory control were assessed using the Flanker Task (Eriksen and Eriksen 1974). Participants were shown sequences of five arrows pointing either left or right and had to identify the direction of the central arrow. Each respondent completed 10 practice trials followed by 10 test trials, which were used to construct the outcome variable. The outcome is the number of correct responses.
Psychomotor vigilance task score (0-30)	"Sustained attention was measured using the Psychomotor Vigilance Task (PVT) (Dinges and Powell 1985). Participants were shown sequences of letters (H, O, X, or L) on the tablet screen and instructed to click as quickly as possible when the letter 'O' appeared. Each respondent first completed a practice trial with 20 symbols, followed by a test trial with 30 symbols. This task was introduced in the sixth wave of the panel survey.
Hearts & flowers task score (0-40)	The Hearts and Flowers Task (HFT) was used to assess participants' ability to process congruent and incongruent information (Davidson et al. 2006). The task consists of three parts: (1) Congruent Condition: Participants saw an "O" on either the left or right side of the screen and had to click the corresponding button (10 rounds). (2) Incongruent Condition: Participants saw an "X" on either side of the screen and had to click the opposite button (10 rounds). (3) Mixed Condition: Participants responded to both congruent and incongruent stimuli in a sequence (20 rounds). The outcome variable is the number of correct responses. This task was introduced in the sixth wave of the panel survey.

Table A.5: Descriptive statistics

Variable	Observations	Mean	Std. dev.	Median
Calories (kcal)	6220	1905.9	1072.5	1647.5
Food consumption (USD)	6220	0.665	0.382	0.598
2 meals or more	6220	0.559	0.497	1
Dietary Diversity (HDDS)	6220	4.758	1.337	5
Days to get 2100 kcal	6200	1.586	1.540	1.272
Food insecurity (HFIAS)	5598	4.341	2.524	5
Food stocks (kg)	6220	24.59	29.94	14
Food stocks (USD)	6220	23.96	30.41	13.50
Debt per capita, food retailer (USD)	6220	44.20	46.24	31.29
Debt per capita, total (USD)	6220	46.98	47.59	36.17
Assets, household level (USD)	6220	755.5	639.9	588.5
Sleep quality (Likert 1-5)	6220	3.095	1.071	3
Good mood score (0-9)	6220	5.152	3.589	6
Happiness (Likert 1-5)	6215	2.766	1.016	3
Required Rate of Return	6164	1.814	0.725	2.303
Payment now (dummy)	6220	0.877	0.329	1
Risk loving score (1-4)	6220	1.400	0.990	1
Math test score (0-3)	6220	2.010	1.184	3
Memory test score (0-36)	6220	19.12	6.595	19
Flanker task score (0-10)	6220	8.598	1.810	9
PVT score (0-30)	3104	28.33	3.076	30
HFT score (0-40)	3104	34.97	5.902	37
# days since last transfer	6220	13.03	9.705	12
Delayed cash transfer (dummy)	6220	0.389	0.487	0
Household size	6220	9.329	5.472	8
Nr of adults in hh	6220	3.516	2.680	3
registration date	6220	16.05	4.590	16
Where is the household living?	6220	0.585	0.493	1
Number of ration cards	6220	1.953	1.414	2
Bamba chakula beneficiary	6220	0.588	0.492	1
Observations	6220			

2.7.2 Analysis of Household Data - Additional Tables

Table A.6: Impacts of aid distribution and aid delays on mental health, time and risk preferences, cognitive abilities, memory and math (Panel B and C)

	Mental well-being			Time preferences		Risk pref.
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Smoothing only						
# days since last transfer	0.00261*	0.00469	0.00141	0.000677	0.000495	-0.000502
	(0.00144)	(0.00412)	(0.00148)	(0.00101)	(0.000599)	(0.00101)
	[0.235]	[0.546]	[0.614]	[0.71]	[0.653]	[0.929]
Kleibergen-Paap F-statistic	502.9	502.9	199.3	201.2	200.7	200.7
Panel C: Delay only						
Delayed cash transfer	0.00297	0.000579	-0.00193	0.00195	0.00123	0.000683
	(0.00272)	(0.00750)	(0.00305)	(0.00187)	(0.00111)	(0.00199)
	[0.546]	[1]	[0.729]	[0.576]	[0.546]	[0.951]
Kleibergen-Paap F-statistic	681.7	681.7	301.3	299.1	302.6	302.6
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	6178	6178	6173	6122	6178	6178
Mean if no delay	3.023	4.843	2.716	1.839	0.879	1.343

	Cognitive abilities			Memory and math	
	Flanker task score (0-10)	Psychomotor vigilance task score (0-30)	Hearts flowers task score (0-40)	Math test score (0-3)	Memory & test score (0-36)
	(1)	(2)	(3)	(4)	(5)
Panel B: Smoothing only					
# days since last transfer	-0.000401	0.00211	-0.0418***	-0.000687	0.00953
	(0.00288)	(0.00694)	(0.0155)	(0.00160)	(0.0100)
	[1]	[0.98]	[0.045]	[0.933]	[0.614]
Kleibergen-Paap F-statistic	200.7	90.57	90.57	200.7	200.7
Panel C: Delay only					
# days of delay	-0.00124	0.0187	-0.0462*	-0.00282	0.00756
	(0.00494)	(0.0124)	(0.0265)	(0.00287)	(0.0187)
	[1]	[0.343]	[0.254]	[0.614]	[0.933]
Kleibergen-Paap F-statistic	302.6	180.1	180.1	302.6	302.6
Household fixed effects	Yes	Yes	Yes	Yes	Yes
N	6178	3069	3069	6178	6178
Mean if no delay	8.613	28.29	35.24	2.085	19.24

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1). Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Standard errors are clustered at the household level, and at the survey date by camp of registration level. Anderson (2008) sharpened q-values are shown in square brackets. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Main specification with no control variables

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	2.104	0.000398	-0.000627	-0.000393	-0.000637	0.00786	-0.510***	-0.576***	-0.0290	-0.0430	-0.468
	(2.612)	(0.000906)	(0.00105)	(0.00274)	(0.00434)	(0.00505)	(0.105)	(0.114)	(0.0515)	(0.0558)	(0.656)
# days of delay	-14.73***	-0.00569***	-0.00267	-0.0109**	0.0136*	0.0117	0.0811	0.109	0.271***	0.286**	2.644**
	(4.499)	(0.00150)	(0.00173)	(0.00463)	(0.00768)	(0.00847)	(0.172)	(0.176)	(0.104)	(0.115)	(1.306)
Panel B: Smoothing only											
# days since last transfer	-2.754	-0.00148**	-0.00151*	-0.00398*	0.00385	0.0119***	-0.487***	-0.545***	0.0473	0.0377	0.277
	(1.861)	(0.000692)	(0.000849)	(0.00206)	(0.00285)	(0.00397)	(0.0799)	(0.0887)	(0.0417)	(0.0451)	(0.789)
Panel C: Delay only											
# days of delay	-12.28***	-0.00523***	-0.00341**	-0.0113***	0.0128***	0.0209***	-0.490***	-0.536***	0.238***	0.238***	2.120
	(3.129)	(0.00112)	(0.00141)	(0.00342)	(0.00493)	(0.00676)	(0.127)	(0.130)	(0.0825)	(0.0915)	(1.562)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00211	0.00644	0.00240	0.000238	0.000391	-0.000914	0.000119	0.0116	0.000448	-0.00331	-0.0377**
	(0.00175)	(0.00531)	(0.00179)	(0.00115)	(0.000673)	(0.00118)	(0.00191)	(0.0124)	(0.00333)	(0.00816)	(0.0159)
# days of delay	0.000412	-0.00698	-0.00452	0.00160	0.000749	0.00173	-0.00309	-0.00717	-0.00200	0.0227	-0.0104
	(0.00337)	(0.00980)	(0.00372)	(0.00207)	(0.00123)	(0.00227)	(0.00348)	(0.0236)	(0.00561)	(0.0148)	(0.0268)
Panel B: Smoothing only											
# days since last transfer	0.00225	0.00413	0.00113	0.000691	0.000602	-0.000427	-0.000751	0.00961	-0.000117	0.00246	-0.0403***
	(0.00141)	(0.00407)	(0.00147)	(0.00102)	(0.000603)	(0.00103)	(0.00157)	(0.0100)	(0.00289)	(0.00689)	(0.0153)
Panel B: Delay only											
# days of delay	0.00288	0.000543	-0.00183	0.00187	0.00119	0.000706	-0.00296	0.00586	-0.00150	0.0196	-0.0456*
	(0.00271)	(0.00759)	(0.00309)	(0.00187)	(0.00112)	(0.00197)	(0.00286)	(0.0190)	(0.00495)	(0.0124)	(0.0264)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) without control variables. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects. Variables that depend on theactual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Main specification without fixed effects

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	5.912**	0.000686	-0.00297**	-0.000493	-0.00143	0.0344***	-0.340***	-0.333***	-0.233**	-0.244**	1.244
	(2.528)	(0.000889)	(0.00117)	(0.00284)	(0.00412)	(0.00597)	(0.0935)	(0.100)	(0.0957)	(0.0976)	(0.940)
# days of delay	-23.50***	-0.00581***	0.00254	-0.0119**	0.0145**	-0.0486***	-0.266*	-0.371**	0.841***	0.828***	-2.575
	(4.277)	(0.00148)	(0.00211)	(0.00504)	(0.00703)	(0.0115)	(0.156)	(0.158)	(0.202)	(0.208)	(1.853)
Panel B: Smoothing only											
# days since last transfer	-1.551	-0.00116*	-0.00216**	-0.00427**	0.00320	0.0185***	-0.410***	-0.430***	-0.0123	-0.0259	0.567
	(1.819)	(0.000676)	(0.000922)	(0.00215)	(0.00279)	(0.00455)	(0.0749)	(0.0804)	(0.0715)	(0.0730)	(0.813)
Panel C: Delay only											
# days of delay	-17.35***	-0.00509***	-0.000543	-0.0124***	0.0130***	-0.0125	-0.601***	-0.698***	0.612***	0.589***	-1.353
	(2.958)	(0.00109)	(0.00165)	(0.00377)	(0.00466)	(0.00903)	(0.125)	(0.127)	(0.147)	(0.154)	(1.606)
N	6188	6188	6188	6188	6168	5566	6188	6188	6188	6188	6188

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	-0.00474**	-0.0246***	-0.00192	0.00219	0.00108	-0.00993***	0.00571**	0.0332**	0.001000	-0.00840	-0.0470***
	(0.00238)	(0.00809)	(0.00216)	(0.00165)	(0.000703)	(0.00247)	(0.00269)	(0.0143)	(0.00338)	(0.00869)	(0.0167)
# days of delay	0.0133***	0.0605***	0.00397	-0.00447	-0.000175	0.0206***	-0.0143***	-0.0449	0.00105	0.0181	-0.00591
	(0.00431)	(0.0157)	(0.00430)	(0.00313)	(0.00135)	(0.00536)	(0.00500)	(0.0279)	(0.00606)	(0.0116)	(0.0247)
Panel B: Smoothing only											
# days since last transfer	-0.000522	-0.00541	-0.000876	0.00102	0.00103*	-0.00451***	0.00196	0.0214**	0.00128	-0.00354	-0.0486***
	(0.00165)	(0.00547)	(0.00174)	(0.00130)	(0.000583)	(0.00159)	(0.00211)	(0.0104)	(0.00287)	(0.00771)	(0.0164)
Panel C: Delay only											
# days of delay	0.00834***	0.0349***	0.00209	-0.00233	0.000883	0.0109***	-0.00867**	-0.0123	0.00203	0.0105	-0.0487*
	(0.00297)	(0.0107)	(0.00346)	(0.00246)	(0.00112)	(0.00372)	(0.00387)	(0.0205)	(0.00513)	(0.0103)	(0.0261)
N	6188	6188	6183	6132	6188	6188	6188	6188	6188	3072	3072

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) without fixed effects. Outcome variables are defined in Appendix Table A.4. All regressions control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Main specification with no replacement or attrited households

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Food stocks (kg)	Food stocks (USD)	Debt p.c. (USD) Food retailer	Debt p.c. (USD) Total	Assets HH (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Smoothing and delay											
# days since last transfer	1.090	0.000327	-0.000438	-0.00144	0.000449	0.00860	-0.567***	-0.618***	-0.0100	-0.0304	-0.342
	(2.674)	(0.000921)	(0.00113)	(0.00283)	(0.00440)	(0.00523)	(0.109)	(0.118)	(0.0557)	(0.0594)	(0.685)
# days of delay	-13.96***	-0.00589***	-0.00277	-0.00847*	0.0123	0.0113	0.170	0.178	0.236**	0.236**	2.432*
	(4.579)	(0.00155)	(0.00178)	(0.00475)	(0.00784)	(0.00883)	(0.178)	(0.181)	(0.113)	(0.120)	(1.356)
Panel B: Smoothing only											
# days since last transfer	-3.566*	-0.00164**	-0.00136	-0.00427**	0.00458	0.0125***	-0.518***	-0.567***	0.0573	0.0369	0.351
	(1.896)	(0.000684)	(0.000891)	(0.00210)	(0.00290)	(0.00405)	(0.0833)	(0.0914)	(0.0429)	(0.0459)	(0.855)
Panel C: Delay only											
# days of delay	-12.68***	-0.00551***	-0.00328**	-0.0102***	0.0129**	0.0213***	-0.465***	-0.514***	0.225***	0.202**	2.049
	(3.191)	(0.00112)	(0.00142)	(0.00346)	(0.00508)	(0.00693)	(0.132)	(0.135)	(0.0859)	(0.0912)	(1.687)
N	5849	5849	5849	5849	5830	5261	5850	5850	5850	5850	5850

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00231	0.00920*	0.00339*	0.0000268	0.000125	-0.000707	-0.0000173	0.00856	0.000730	-0.00390	-0.0406**
	(0.00184)	(0.00558)	(0.00187)	(0.00116)	(0.000705)	(0.00117)	(0.00202)	(0.0127)	(0.00345)	(0.00851)	(0.0162)
# days of delay	-0.000422	-0.0127	-0.00679*	0.00138	0.00116	0.00212	-0.00324	-0.00737	-0.00137	0.0206	-0.00306
	(0.00347)	(0.00998)	(0.00382)	(0.00213)	(0.00129)	(0.00234)	(0.00366)	(0.0243)	(0.00605)	(0.0151)	(0.0283)
Panel B: Smoothing only											
# days since last transfer	0.00217	0.00497	0.00146	0.000421	0.000456	-0.000104	-0.000939	0.00646	0.000341	0.00133	-0.0414***
	(0.00147)	(0.00413)	(0.00152)	(0.00102)	(0.000616)	(0.00103)	(0.00167)	(0.0101)	(0.00300)	(0.00711)	(0.0153)
Panel C: Delay only											
# days of delay	0.00228	-0.00195	-0.00300	0.00141	0.00130	0.00133	-0.00326	0.00222	-0.000550	0.0171	-0.0403
	(0.00279)	(0.00746)	(0.00312)	(0.00189)	(0.00114)	(0.00205)	(0.00303)	(0.0193)	(0.00534)	(0.0125)	(0.0271)
N	5849	5849	5845	5798	5850	5850	5850	5850	5850	2904	2904

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with no replacement or attrited households. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Main specification without excluding transfer days

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	1.054	0.000337	-0.000433	-0.0000985	0.000468	0.00859*	-0.532***	-0.580***	-0.0183	-0.0346	-0.418
	(2.591)	(0.000897)	(0.00105)	(0.00279)	(0.00437)	(0.00509)	(0.107)	(0.116)	(0.0530)	(0.0569)	(0.652)
# days of delay	-13.34***	-0.00567***	-0.00275	-0.0111**	0.0121	0.0102	0.127	0.142	0.235**	0.246**	2.271*
	(4.390)	(0.00148)	(0.00171)	(0.00464)	(0.00751)	(0.00845)	(0.176)	(0.179)	(0.107)	(0.117)	(1.282)
Panel B: Smoothing only											
# days since last transfer	-3.423*	-0.00157**	-0.00136	-0.00382*	0.00454	0.0121***	-0.495***	-0.539***	0.0493	0.0364	0.237
	(1.837)	(0.000672)	(0.000841)	(0.00206)	(0.00290)	(0.00392)	(0.0805)	(0.0885)	(0.0411)	(0.0442)	(0.813)
Panel C: Delay only											
# days of delay	-12.11***	-0.00528***	-0.00325**	-0.0112***	0.0126***	0.0202***	-0.468***	-0.507***	0.214***	0.208**	1.803
	(3.045)	(0.00108)	(0.00137)	(0.00334)	(0.00484)	(0.00659)	(0.128)	(0.130)	(0.0816)	(0.0896)	(1.593)
N	6210	6210	6210	6210	6190	5589	6210	6210	6210	6210	6210

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5) (12)	Good mood score (0-9) (13)	Happiness Likert (1-5) (14)	Required Rate of Return (15)	Payment now (dummy) (16)	Risk loving score (1-4) (17)	Math test score (0-3) (18)	Memory test score(0-36) (19)	Flanker task score(0-10) (20)	Psychomotor vigilance task score (0-30) (21)	Hearts & flower task score (0-40) (22)
Panel A: Smoothing and delay											
# days since last transfer	0.00273	0.00718	0.00286	0.000231	0.000241	-0.00101	0.000188	0.0119	-0.000143	-0.00331	-0.0372**
	(0.00180)	(0.00546)	(0.00180)	(0.00113)	(0.000677)	(0.00116)	(0.00197)	(0.0125)	(0.00333)	(0.00824)	(0.0160)
# days of delay	-0.000192	-0.00742	-0.00464	0.00165	0.000875	0.00182	-0.00304	-0.00281	-0.000836	0.0260*	-0.00679
	(0.00334)	(0.00976)	(0.00370)	(0.00206)	(0.00123)	(0.00225)	(0.00351)	(0.0234)	(0.00552)	(0.0149)	(0.0265)
Panel B: Smoothing only											
# days since last transfer	0.00267*	0.00468	0.00152	0.000706	0.000494	-0.000488	-0.000687	0.0111	-0.000384	0.00344	-0.0389**
	(0.00144)	(0.00413)	(0.00148)	(0.00100)	(0.000597)	(0.00101)	(0.00160)	(0.0102)	(0.00287)	(0.00699)	(0.0153)
Panel C: Delay only											
# days of delay	0.00299	0.000925	-0.00145	0.00191	0.00115	0.000682	-0.00283	0.0105	-0.000997	0.0229*	-0.0409
	(0.00269)	(0.00745)	(0.00306)	(0.00184)	(0.00110)	(0.00196)	(0.00285)	(0.0189)	(0.00485)	(0.0125)	(0.0258)
N	6210	6210	6205	6154	6210	6210	6210	6210	6210	3101	3101

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) without excluding transfer days. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Main specification with two smoothing variables

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last cash transfer	-0.191	0.000358	-0.000448	0.000749	0.00215	0.00210	-0.401***	-0.429***	-0.0343	-0.0503	-0.259
	(2.568)	(0.000936)	(0.00112)	(0.00286)	(0.00445)	(0.00482)	(0.0980)	(0.104)	(0.0583)	(0.0625)	(0.712)
# days since last in-kind transfer	5.547	0.000330	-0.000246	-0.00259	-0.00730	0.0233***	-0.959***	-1.086***	0.0188	-0.00341	-0.731
	(4.248)	(0.00126)	(0.00141)	(0.00398)	(0.00648)	(0.00838)	(0.179)	(0.200)	(0.0583)	(0.0644)	(0.787)
# days of delay	-13.06***	-0.00577***	-0.00296*	-0.0117**	0.0115	0.0150*	0.0380	0.0379	0.243**	0.259**	2.240*
	(4.426)	(0.00151)	(0.00172)	(0.00471)	(0.00757)	(0.00833)	(0.169)	(0.171)	(0.110)	(0.121)	(1.266)
Panel B: Smoothing only											
# days since last cash transfer	-4.741***	-0.00165**	-0.00148*	-0.00332	0.00617**	0.00749*	-0.390***	-0.418***	0.0402	0.0290	0.428
	(1.821)	(0.000711)	(0.000889)	(0.00213)	(0.00299)	(0.00382)	(0.0729)	(0.0783)	(0.0455)	(0.0486)	(0.887)
# days since last in-kind transfer	2.041	-0.00122	-0.00104	-0.00573	-0.00421	0.0275***	-0.951***	-1.079***	0.0657	0.0465	-0.299
	(3.886)	(0.00114)	(0.00131)	(0.00365)	(0.00571)	(0.00788)	(0.171)	(0.192)	(0.0522)	(0.0585)	(0.816)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5) (12)	Good mood score (0-9) (13)	Happiness Likert (1-5) (14)	Required Rate of Return (15)	Payment now (dummy) (16)	Risk loving score (1-4) (17)	Math test score (0-3) (18)	Memory test score(0-36) (19)	Flanker task score(0-10) (20)	Psychomotor vigilance task score (0-30) (21)	Hearts & flower task score (0-40) (22)
Panel A: Smoothing and delay											
# days since last cash transfer	0.00236	0.00852	0.00263	0.000826	0.000318	-0.00110	-0.000820	0.0234*	0.000969	-0.00453	-0.0379**
	(0.00186)	(0.00552)	(0.00185)	(0.00119)	(0.000750)	(0.00126)	(0.00226)	(0.0128)	(0.00351)	(0.00817)	(0.0164)
# days since last in-kind transfer	0.00339	0.00164	0.00381	-0.00253	0.0000183	-0.000482	0.00194	-0.0265	-0.00221	-0.00183	-0.0384
	(0.00265)	(0.00820)	(0.00288)	(0.00158)	(0.000796)	(0.00154)	(0.00258)	(0.0176)	(0.00492)	(0.0125)	(0.0235)
# days of delay	0.0000624	-0.00834	-0.00502	0.00141	0.000907	0.00185	-0.00222	-0.0130	-0.00196	0.0227	-0.0114
	(0.00342)	(0.00983)	(0.00373)	(0.00208)	(0.00127)	(0.00231)	(0.00362)	(0.0230)	(0.00557)	(0.0149)	(0.0272)
Panel B: Smoothing only											
# days since last cash transfer	0.00238	0.00561	0.00109	0.00126	0.000596	-0.000533	-0.00150	0.0194*	0.000367	0.00158	-0.0410***
	(0.00145)	(0.00420)	(0.00150)	(0.00106)	(0.000647)	(0.00109)	(0.00181)	(0.0105)	(0.00307)	(0.00680)	(0.0156)
# days since last in-kind transfer	0.00341	-0.000594	0.00284	-0.00225	0.000193	-0.000125	0.00152	-0.0290*	-0.00259	0.00140	-0.0400*
	(0.00253)	(0.00748)	(0.00275)	(0.00153)	(0.000788)	(0.00149)	(0.00251)	(0.0168)	(0.00468)	(0.0120)	(0.0234)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with two smoothing variables, one for cash transfers and one for in-kind transfers. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Main specification with quadratic smoothing

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	3.328	-0.0000168	0.00312	-0.00188	0.00124	0.0143	-1.019***	-1.180***	-0.0112	-0.0978	-1.510
	(5.291)	(0.00191)	(0.00212)	(0.00613)	(0.00778)	(0.0121)	(0.230)	(0.254)	(0.115)	(0.122)	(1.651)
# days since last transfer squared	-0.0765	0.0000136	-0.000127*	0.0000685	-0.0000477	-0.000234	0.0167***	0.0206***	-0.000369	0.00214	0.0407
	(0.147)	(0.0000565)	(0.0000703)	(0.000191)	(0.000238)	(0.000362)	(0.00572)	(0.00619)	(0.00404)	(0.00421)	(0.0693)
# days of delay	-12.66***	-0.00598***	-0.00105	-0.0124**	0.0134	0.0150	-0.119	-0.164	0.240*	0.217	1.676
	(4.789)	(0.00161)	(0.00214)	(0.00560)	(0.00893)	(0.00992)	(0.194)	(0.195)	(0.134)	(0.146)	(1.049)
Panel B: Smoothing only											
# days since last transfer	6.089	0.00129	0.00335*	0.000827	-0.00166	0.0104	-0.986***	-1.135***	-0.0777	-0.158	-1.973
	(5.365)	(0.00192)	(0.00203)	(0.00599)	(0.00779)	(0.0118)	(0.232)	(0.255)	(0.110)	(0.116)	(1.632)
# days since last transfer squared	-0.287**	-0.0000859	-0.000144**	-0.000138	0.000175	0.0000463	0.0147***	0.0178***	0.00377	0.00587*	0.0696
	(0.137)	(0.0000525)	(0.0000561)	(0.000161)	(0.000198)	(0.000314)	(0.00532)	(0.00583)	(0.00325)	(0.00335)	(0.0672)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00270	0.00837	0.00202	-0.00115	-0.000566	-0.00291	-0.0136***	-0.0255	-0.0164**	-0.0171	-0.0460
	(0.00451)	(0.0136)	(0.00433)	(0.00273)	(0.00148)	(0.00308)	(0.00442)	(0.0285)	(0.00725)	(0.0172)	(0.0429)
# days since last transfer squared	-0.00000173	-0.0000351	0.0000305	0.0000466	0.0000273	0.0000662	0.000486***	0.00128	0.000576**	0.000492	0.000246
	(0.000137)	(0.000432)	(0.000131)	(0.0000884)	(0.0000470)	(0.000104)	(0.000134)	(0.000867)	(0.000223)	(0.000525)	(0.00138)
# days of delay	-0.0000931	-0.00749	-0.00562	0.00106	0.000582	0.000837	-0.0104**	-0.0239	-0.00985	0.0142	-0.0143
	(0.00376)	(0.0111)	(0.00409)	(0.00259)	(0.00159)	(0.00288)	(0.00416)	(0.0263)	(0.00606)	(0.0180)	(0.0356)
Panel B: Smoothing only											
# days since last transfer	0.00272	0.0100	0.00357	-0.00144	-0.000727	-0.00314	-0.0108**	-0.0189	-0.0137*	-0.0238	-0.0392
	(0.00442)	(0.0134)	(0.00429)	(0.00253)	(0.00135)	(0.00283)	(0.00425)	(0.0279)	(0.00712)	(0.0168)	(0.0369)
# days since last transfer squared	-0.00000328	-0.000160	-0.0000662	0.0000648	0.0000374	0.0000806	0.000308***	0.000869	0.000407**	0.000818*	-0.0000821
	(0.000122)	(0.000381)	(0.000120)	(0.0000706)	(0.0000352)	(0.0000830)	(0.000113)	(0.000765)	(0.000201)	(0.000437)	(0.00104)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with quadratic smoothing. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Main specification with quadratic delays

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	0.543	0.000149	-0.000479	-0.00102	0.000866	0.00873*	-0.535***	-0.583***	-0.0437	-0.0494	0.146
	(2.729)	(0.000950)	(0.00109)	(0.00300)	(0.00430)	(0.00515)	(0.115)	(0.124)	(0.0553)	(0.0592)	(0.944)
# days of delay	-4.763	-0.00258	-0.00279	0.00454	-0.00143	-0.00123	0.00997	-0.00593	0.513**	0.405	-4.018
	(11.08)	(0.00411)	(0.00414)	(0.0129)	(0.0180)	(0.0221)	(0.428)	(0.437)	(0.240)	(0.261)	(4.167)
# days of delay squared	-0.572	-0.000203	-0.00000708	-0.00101	0.000898	0.000814	0.00781	0.00973	-0.0177	-0.00997	0.402
	(0.574)	(0.000231)	(0.000248)	(0.000677)	(0.00107)	(0.00117)	(0.0205)	(0.0208)	(0.0145)	(0.0156)	(0.306)
Panel B: Delay only											
# days of delay	-3.432	-0.00222	-0.00397	0.00203	0.000701	0.0197	-1.325***	-1.462***	0.404*	0.282	-3.654
	(9.277)	(0.00347)	(0.00381)	(0.0104)	(0.0170)	(0.0204)	(0.340)	(0.355)	(0.207)	(0.226)	(2.387)
# days of delay squared	-0.620	-0.000216	0.0000354	-0.000920	0.000821	0.0000676	0.0593***	0.0659***	-0.0135	-0.00521	0.388*
	(0.552)	(0.000219)	(0.000246)	(0.000620)	(0.00111)	(0.00116)	(0.0189)	(0.0197)	(0.0141)	(0.0152)	(0.233)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5) (12)	Good mood score (0-9) (13)	Happiness Likert (1-5) (14)	Required Rate of Return (15)	Payment now (dummy) (16)	Risk loving score (1-4) (17)	Math test score (0-3) (18)	Memory test score(0-36) (19)	Flanker task score(0-10) (20)	Psychomotor vigilance task score (0-30) (21)	Hearts & flower task score (0-40) (22)
Panel A: Smoothing and delay											
# days since last transfer	0.00255	0.00593	0.00193	-0.00106	0.0000330	-0.000140	0.00174	0.00915	0.00311	0.00103	-0.0358**
	(0.00194)	(0.00594)	(0.00189)	(0.00127)	(0.000731)	(0.00127)	(0.00215)	(0.0141)	(0.00345)	(0.00884)	(0.0177)
# days of delay	0.00136	0.0131	0.00694	0.0174***	0.00323	-0.00940	-0.0228**	0.0170	-0.0414***	-0.0308	-0.0497
	(0.00919)	(0.0280)	(0.00924)	(0.00607)	(0.00326)	(0.00663)	(0.00998)	(0.0646)	(0.0149)	(0.0349)	(0.0883)
# days of delay squared	-0.0000939	-0.00134	-0.000771	-0.000999***	-0.000143	0.000717*	0.00126**	-0.00138	0.00257***	0.00329*	0.00246
	(0.000528)	(0.00163)	(0.000533)	(0.000357)	(0.000187)	(0.000429)	(0.000593)	(0.00381)	(0.000845)	(0.00174)	(0.00529)
Panel B: Delay only											
# days of delay	0.00761	0.0277	0.0117	0.0148***	0.00332	-0.00975*	-0.0185**	0.0398	-0.0336**	-0.0284	-0.135*
	(0.00766)	(0.0225)	(0.00802)	(0.00486)	(0.00279)	(0.00557)	(0.00827)	(0.0493)	(0.0134)	(0.0294)	(0.0799)
# days of delay squared	-0.000320	-0.00187	-0.000957*	-0.000897***	-0.000146	0.000730*	0.00109**	-0.00226	0.00227***	0.00319*	0.00599
	(0.000490)	(0.00148)	(0.000504)	(0.000314)	(0.000173)	(0.000395)	(0.000543)	(0.00337)	(0.000816)	(0.00164)	(0.00505)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with quadratic delays. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Main specification with binary delays

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and binary delay											
# days since last transfer	-0.733	-0.000327	-0.0000581	-0.000989	0.000990	0.00833*	-0.430***	-0.469***	-0.0477	-0.0551	0.528
	(2.586)	(0.000913)	(0.00115)	(0.00294)	(0.00409)	(0.00480)	(0.107)	(0.116)	(0.0570)	(0.0611)	(1.064)
Aid delay (binary)	-76.38	-0.0348**	-0.0389**	-0.0774	0.0891	0.0974	-2.231	-2.474	2.741**	2.626**	-6.594
	(50.01)	(0.0163)	(0.0176)	(0.0537)	(0.0762)	(0.0876)	(1.905)	(1.967)	(1.063)	(1.163)	(13.08)
Panel B: Binary delay only											
Aid delay (binary)	-85.84**	-0.0390***	-0.0396***	-0.0902**	0.102*	0.206***	-7.536***	-8.257***	2.153***	1.947**	-0.0867
	(35.87)	(0.0121)	(0.0130)	(0.0378)	(0.0543)	(0.0729)	(1.430)	(1.507)	(0.779)	(0.859)	(10.08)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and binary delay											
# days since last transfer	0.00248	0.00468	0.000871	-0.00120	-0.000174	-0.000447	0.00282	0.00407	0.00307	-0.00171	-0.0419**
	(0.00203)	(0.00630)	(0.00196)	(0.00138)	(0.000783)	(0.00145)	(0.00225)	(0.0138)	(0.00369)	(0.00934)	(0.0184)
Aid delay (binary)	0.00371	0.000440	0.0158	0.0553**	0.0197	-0.00163	-0.103***	0.160	-0.102	0.117	0.00513
	(0.0393)	(0.118)	(0.0403)	(0.0255)	(0.0145)	(0.0281)	(0.0398)	(0.240)	(0.0643)	(0.178)	(0.320)
Panel B: Binary delay only											
Aid delay (binary)	0.0357	0.0608	0.0265	0.0404**	0.0175	-0.00714	-0.0683**	0.211	-0.0641	0.0975	-0.464*
	(0.0280)	(0.0771)	(0.0305)	(0.0183)	(0.0111)	(0.0198)	(0.0281)	(0.171)	(0.0501)	(0.133)	(0.271)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) by using binary delay variables. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Main specification with reduced form

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Food stocks (kg)	Food stocks (USD)	Debt p.c. (USD) Food retailer	Debt p.c. (USD) Total	Assets HH (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Smoothing and delay											
# days since last transfer	1.113	0.000366	-0.000381	0.000153	-0.000206	0.00689	-0.438***	-0.479***	-0.0245	-0.0373	-0.352
	(2.258)	(0.000782)	(0.000913)	(0.00240)	(0.00381)	(0.00444)	(0.0916)	(0.0994)	(0.0451)	(0.0483)	(0.511)
# days of delay	-12.78***	-0.00535***	-0.00273*	-0.0107**	0.0119*	0.0110	0.0627	0.0704	0.206**	0.217**	1.995*
	(3.923)	(0.00133)	(0.00152)	(0.00415)	(0.00671)	(0.00759)	(0.150)	(0.153)	(0.0929)	(0.101)	(1.157)
Panel B: Smoothing only											
# days since last transfer	-3.121*	-0.00141**	-0.00129*	-0.00339*	0.00376	0.0106***	-0.419***	-0.458***	0.0378	0.0284	0.253
	(1.610)	(0.000593)	(0.000732)	(0.00181)	(0.00254)	(0.00349)	(0.0680)	(0.0748)	(0.0345)	(0.0371)	(0.673)
Panel C: Delay only											
# days of delay	-11.49***	-0.00492***	-0.00317***	-0.0105***	0.0117***	0.0190***	-0.407***	-0.443***	0.179**	0.177**	1.618
	(2.743)	(0.000995)	(0.00122)	(0.00307)	(0.00437)	(0.00601)	(0.109)	(0.111)	(0.0713)	(0.0781)	(1.362)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00228	0.00641	0.00247	0.0000820	0.000137	-0.000879	0.000239	0.00881	-0.0000119	-0.00340	-0.0291**
	(0.00156)	(0.00473)	(0.00152)	(0.000935)	(0.000560)	(0.000964)	(0.00163)	(0.0103)	(0.00274)	(0.00659)	(0.0124)
# days of delay	0.00000678	-0.00679	-0.00428	0.00157	0.000903	0.00153	-0.00264	-0.00289	-0.00102	0.0185	-0.0144
	(0.00301)	(0.00877)	(0.00319)	(0.00179)	(0.00107)	(0.00196)	(0.00300)	(0.0201)	(0.00480)	(0.0123)	(0.0228)
Panel B: Smoothing only											
# days since last transfer	0.00228*	0.00416	0.00117	0.000558	0.000411	-0.000417	-0.000560	0.00794	-0.000321	0.00167	-0.0330***
	(0.00126)	(0.00360)	(0.00123)	(0.000837)	(0.000497)	(0.000838)	(0.00133)	(0.00826)	(0.00238)	(0.00548)	(0.0122)
Panel C: Delay only											
# days of delay	0.00265	0.000647	-0.00164	0.00166	0.00105	0.000583	-0.00238	0.00656	-0.00103	0.0157	-0.0389*
	(0.00245)	(0.00675)	(0.00260)	(0.00159)	(0.000943)	(0.00170)	(0.00244)	(0.0160)	(0.00421)	(0.0103)	(0.0223)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Reduced form regressions estimating Equation (2.1). Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Main specification with household-level clustering

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Food stocks (kg)	Food stocks (USD)	Debt p.c. (USD) Food retailer	Debt p.c. (USD) Total	Assets HH (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Smoothing and delay											
# days since last transfer	1.159	0.000368	-0.000471	0.0000653	-0.000107	0.00793*	-0.544***	-0.595***	-0.0217	-0.0371	-0.353
	(2.091)	(0.000718)	(0.000916)	(0.00253)	(0.00318)	(0.00465)	(0.0574)	(0.0592)	(0.0492)	(0.0520)	(0.629)
# days of delay	-13.78***	-0.00578***	-0.00291*	-0.0114**	0.0127**	0.0114	0.133	0.147	0.235**	0.249**	2.290*
	(3.806)	(0.00128)	(0.00158)	(0.00453)	(0.00593)	(0.00838)	(0.101)	(0.0941)	(0.0943)	(0.103)	(1.236)
Panel B: Smoothing only											
# days since last transfer	-3.457**	-0.00157***	-0.00144**	-0.00375**	0.00417*	0.0119***	-0.506***	-0.553***	0.0456	0.0343	0.303
	(1.560)	(0.000566)	(0.000727)	(0.00186)	(0.00234)	(0.00348)	(0.0475)	(0.0496)	(0.0388)	(0.0417)	(0.781)
Panel C: Delay only											
# days of delay	-12.43***	-0.00535***	-0.00345***	-0.0113***	0.0126***	0.0206***	-0.478***	-0.521***	0.211***	0.207**	1.895
	(2.817)	(0.00100)	(0.00125)	(0.00331)	(0.00436)	(0.00628)	(0.0813)	(0.0763)	(0.0736)	(0.0821)	(1.539)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00265	0.00737	0.00288*	0.000173	0.000210	-0.00103	0.000178	0.0109	-0.0000683	-0.00338	-0.0391**
	(0.00168)	(0.00523)	(0.00164)	(0.00106)	(0.000591)	(0.00118)	(0.00175)	(0.0116)	(0.00315)	(0.00835)	(0.0162)
# days of delay	-0.000118	-0.00800	-0.00516	0.00176	0.000994	0.00184	-0.00302	-0.00461	-0.00116	0.0218	-0.0105
	(0.00316)	(0.00909)	(0.00327)	(0.00196)	(0.00108)	(0.00217)	(0.00332)	(0.0207)	(0.00568)	(0.0136)	(0.0261)
Panel B: Smoothing only											
# days since last transfer	0.00261*	0.00469	0.00141	0.000677	0.000495	-0.000502	-0.000687	0.00953	-0.000401	0.00211	-0.0418***
	(0.00140)	(0.00410)	(0.00136)	(0.000897)	(0.000524)	(0.00102)	(0.00141)	(0.00962)	(0.00263)	(0.00734)	(0.0148)
Panel C: Delay only											
# days of delay	0.00297	0.000579	-0.00193	0.00195	0.00123	0.000683	-0.00282	0.00756	-0.00124	0.0187	-0.0462*
	(0.00263)	(0.00714)	(0.00273)	(0.00166)	(0.000968)	(0.00189)	(0.00267)	(0.0172)	(0.00475)	(0.0119)	(0.0243)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with cluster standard errors by household (reported in parentheses). Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Main specification with robust standard errors

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Food stocks (kg)	Food stocks (USD)	Debt p.c. (USD) Food retailer	Debt p.c. (USD) Total	Assets HH (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Smoothing and delay											
# days since last transfer	1.159	0.000368	-0.000471	0.0000653	-0.000107	0.00793*	-0.544***	-0.595***	-0.0217	-0.0371	-0.353
	(2.085)	(0.000709)	(0.000883)	(0.00253)	(0.00320)	(0.00443)	(0.0557)	(0.0554)	(0.0501)	(0.0532)	(0.644)
# days of delay	-13.78***	-0.00578***	-0.00291*	-0.0114**	0.0127**	0.0114	0.133	0.147	0.235**	0.249**	2.290*
	(3.551)	(0.00122)	(0.00157)	(0.00449)	(0.00587)	(0.00813)	(0.105)	(0.0998)	(0.0915)	(0.0977)	(1.221)
Panel B: Smoothing only											
# days since last transfer	-3.457**	-0.00157***	-0.00144**	-0.00375**	0.00417*	0.0119***	-0.506***	-0.553***	0.0456	0.0343	0.303
	(1.532)	(0.000547)	(0.000697)	(0.00190)	(0.00232)	(0.00336)	(0.0408)	(0.0405)	(0.0429)	(0.0451)	(0.767)
Panel C: Delay only											
# days of delay	-12.43***	-0.00535***	-0.00345***	-0.0113***	0.0126***	0.0206***	-0.478***	-0.521***	0.211***	0.207**	1.895
	(2.574)	(0.000938)	(0.00124)	(0.00336)	(0.00425)	(0.00617)	(0.0748)	(0.0693)	(0.0787)	(0.0831)	(1.486)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00265	0.00737	0.00288	0.000173	0.000210	-0.00103	0.000178	0.0109	-0.0000683	-0.00338	-0.0391**
	(0.00181)	(0.00558)	(0.00179)	(0.00112)	(0.000590)	(0.00125)	(0.00182)	(0.0115)	(0.00328)	(0.00795)	(0.0164)
# days of delay	-0.000118	-0.00800	-0.00516	0.00176	0.000994	0.00184	-0.00302	-0.00461	-0.00116	0.0218	-0.0105
	(0.00332)	(0.00962)	(0.00334)	(0.00206)	(0.00107)	(0.00225)	(0.00343)	(0.0207)	(0.00604)	(0.0134)	(0.0259)
Panel B: Smoothing only											
# days since last transfer	0.00261*	0.00469	0.00141	0.000677	0.000495	-0.000502	-0.000687	0.00953	-0.000401	0.00211	-0.0418***
	(0.00145)	(0.00433)	(0.00147)	(0.000935)	(0.000499)	(0.00104)	(0.00148)	(0.00956)	(0.00267)	(0.00714)	(0.0149)
Panel C: Delay only											
# days of delay	0.00297	0.000579	-0.00193	0.00195	0.00123	0.000683	-0.00282	0.00756	-0.00124	0.0187	-0.0462*
	(0.00266)	(0.00747)	(0.00274)	(0.00172)	(0.000911)	(0.00187)	(0.00279)	(0.0172)	(0.00490)	(0.0121)	(0.0241)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with heteroskedasticity-robust standard errors (reported in parentheses). Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Main specification with households registered in Kakuma

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	-0.345	-0.00213	-0.00305*	-0.00600	0.00429	0.0456***	-1.480***	-1.646***	-0.0471	-0.0751	-0.0469
	(5.632)	(0.00155)	(0.00156)	(0.00479)	(0.00747)	(0.00966)	(0.240)	(0.263)	(0.0335)	(0.0465)	(0.807)
# days of delay	-31.17**	-0.00890***	-0.00274	-0.0274**	0.0306**	0.0230	-0.350	-0.324	0.0891	-0.0408	-0.768
	(13.03)	(0.00342)	(0.00326)	(0.0119)	(0.0132)	(0.0188)	(0.635)	(0.662)	(0.0728)	(0.0968)	(1.727)
Panel B: Smoothing only											
# days since last transfer	-6.287	-0.00382***	-0.00358**	-0.0112***	0.0102	0.0500***	-1.526***	-1.689***	-0.0353	-0.0805*	-0.148
	(4.729)	(0.00122)	(0.00140)	(0.00401)	(0.00636)	(0.00801)	(0.196)	(0.219)	(0.0329)	(0.0454)	(0.734)
Panel C: Delay only											
# days of delay	-31.54***	-0.0112***	-0.00601*	-0.0338***	0.0353***	0.0687***	-1.814***	-1.952***	0.0426	-0.115	-0.814
	(11.04)	(0.00274)	(0.00311)	(0.0100)	(0.0111)	(0.0195)	(0.650)	(0.697)	(0.0736)	(0.0985)	(1.536)
N	2565	2565	2565	2565	2555	2309	2563	2563	2563	2563	2563

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5) (12)	Good mood score (0-9) (13)	Happiness Likert (1-5) (14)	Required Rate of Return (15)	Payment now (dummy) (16)	Risk loving score (1-4) (17)	Math test score (0-3) (18)	Memory test score(0-36) (19)	Flanker task score(0-10) (20)	Psychomotor vigilance task score (0-30) (21)	Hearts & flower task score (0-40) (22)
Panel A: Smoothing and delay											
# days since last transfer	0.00785**	0.0117	0.00732**	-0.00299*	-0.000193	0.00109	-0.00519*	-0.00755	-0.00534	-0.00589	-0.0336
	(0.00326)	(0.0106)	(0.00287)	(0.00167)	(0.000703)	(0.00150)	(0.00306)	(0.0221)	(0.00523)	(0.0131)	(0.0208)
# days of delay	-0.00734	-0.0324	-0.00349	0.00643**	-0.00245	0.00750***	-0.0106	0.0700	-0.00167	0.0201	0.0474
	(0.00687)	(0.0233)	(0.00673)	(0.00322)	(0.00174)	(0.00283)	(0.00775)	(0.0487)	(0.00976)	(0.0277)	(0.0463)
Panel B: Smoothing only											
# days since last transfer	0.00645**	0.00552	0.00686**	-0.00213	-0.000517	0.00207	-0.00658**	0.00168	-0.00556	-0.00410	-0.0294
	(0.00271)	(0.00900)	(0.00264)	(0.00158)	(0.000709)	(0.00140)	(0.00303)	(0.0207)	(0.00484)	(0.0125)	(0.0196)
Panel C: Delay only											
# days of delay	0.00106	-0.0199	0.00375	0.00345	-0.00265	0.00857***	-0.0157**	0.0625	-0.00696	0.0152	0.0194
	(0.00554)	(0.0193)	(0.00639)	(0.00297)	(0.00173)	(0.00256)	(0.00773)	(0.0439)	(0.00894)	(0.0266)	(0.0435)
N	2565	2565	2563	2555	2563	2563	2563	2563	2563	1269	1269

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) for households registered in Kakuma only. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.19: Main specification with households registered in Kalobeyei

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	1.807	0.00143	0.000813	0.00338	-0.00274	-0.00840*	-0.00731	0.00570	0.0130	-0.00271	-0.583
	(2.592)	(0.00105)	(0.00138)	(0.00327)	(0.00578)	(0.00467)	(0.0546)	(0.0469)	(0.0822)	(0.0859)	(0.958)
# days of delay	-10.59***	-0.00571***	-0.00358*	-0.0101**	0.0111	0.0197**	-0.156	-0.171**	0.265*	0.313**	2.913**
	(4.056)	(0.00162)	(0.00192)	(0.00506)	(0.00939)	(0.00822)	(0.101)	(0.0817)	(0.144)	(0.154)	(1.408)
Panel B: Smoothing only											
# days since last transfer	-2.363	-0.000816	-0.000596	-0.000616	0.00165	-0.000266	-0.0628	-0.0551	0.108*	0.109*	0.455
	(1.924)	(0.000809)	(0.00103)	(0.00231)	(0.00334)	(0.00396)	(0.0433)	(0.0371)	(0.0552)	(0.0583)	(1.212)
Panel C: Delay only											
# days of delay	-8.485***	-0.00404***	-0.00263*	-0.00620*	0.00794	0.00975	-0.164**	-0.164**	0.280***	0.310***	2.259
	(2.966)	(0.00122)	(0.00143)	(0.00350)	(0.00520)	(0.00679)	(0.0798)	(0.0648)	(0.0952)	(0.103)	(2.002)
N	3613	3613	3613	3613	3603	3248	3615	3615	3615	3615	3615

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5) (12)	Good mood score (0-9) (13)	Happiness Likert (1-5) (14)	Required Rate of Return (15)	Payment now (dummy) (16)	Risk loving score (1-4) (17)	Math test score (0-3) (18)	Memory test score(0-36) (19)	Flanker task score(0-10) (20)	Psychomotor vigilance task score (0-30) (21)	Hearts & flower task score (0-40) (22)
Panel A: Smoothing and delay											
# days since last transfer	-0.000875	0.00594	-0.000149	0.00192	-0.0000176	-0.00225	0.00350	0.0163	0.00140	-0.00679	-0.0428
	(0.00213)	(0.00593)	(0.00231)	(0.00152)	(0.000993)	(0.00169)	(0.00245)	(0.0148)	(0.00452)	(0.0120)	(0.0263)
# days of delay	0.00390	-0.000776	-0.00334	-0.000563	0.00197	0.00203	-0.00423	-0.0243	-0.00191	0.0283	-0.0266
	(0.00374)	(0.0102)	(0.00418)	(0.00259)	(0.00158)	(0.00291)	(0.00399)	(0.0259)	(0.00678)	(0.0181)	(0.0330)
Panel B: Smoothing only											
# days since last transfer	0.000660	0.00564	-0.00134	0.00172	0.000685	-0.00153	0.00199	0.00764	0.000716	0.00357	-0.0525**
	(0.00170)	(0.00431)	(0.00179)	(0.00125)	(0.000812)	(0.00139)	(0.00181)	(0.0120)	(0.00364)	(0.00957)	(0.0250)
Panel C: Delay only											
# days of delay	0.00288	0.00615	-0.00350	0.00158	0.00195	-0.000490	-0.000304	-0.00600	-0.000344	0.0229	-0.0608*
	(0.00296)	(0.00749)	(0.00325)	(0.00214)	(0.00131)	(0.00239)	(0.00292)	(0.0207)	(0.00554)	(0.0148)	(0.0322)
N	3613	3613	3610	3567	3615	3615	3615	3615	3615	1800	1800

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) for households registered in Kalobeyei only. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.20: Main specification with households living in Kakuma

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and Delay											
# days since last transfer	-0.132	-0.00183	-0.00273*	-0.00509	0.00392	0.0436***	-1.454***	-1.628***	-0.0621*	-0.0871*	-0.0620
	(5.676)	(0.00155)	(0.00155)	(0.00471)	(0.00739)	(0.00956)	(0.242)	(0.263)	(0.0319)	(0.0459)	(0.803)
# days of delay	-30.59**	-0.00789**	-0.00188	-0.0244**	0.0270**	0.0125	-0.327	-0.252	0.130**	0.0144	-0.668
	(13.45)	(0.00344)	(0.00350)	(0.0121)	(0.0130)	(0.0193)	(0.656)	(0.679)	(0.0621)	(0.0908)	(1.783)
Panel B: Smoothing only											
# days since last transfer	-6.004	-0.00335***	-0.00309**	-0.00978**	0.00913	0.0460***	-1.499***	-1.663***	-0.0443	-0.0852*	-0.154
	(4.673)	(0.00121)	(0.00138)	(0.00383)	(0.00617)	(0.00794)	(0.193)	(0.215)	(0.0318)	(0.0443)	(0.717)
Panel C: Delay only											
# days of delay	-30.74***	-0.00995***	-0.00495	-0.0302***	0.0314***	0.0589***	-1.878***	-1.988***	0.0633	-0.0785	-0.735
	(11.09)	(0.00273)	(0.00330)	(0.00991)	(0.0106)	(0.0199)	(0.658)	(0.703)	(0.0664)	(0.0925)	(1.545)
N	2563	2563	2563	2563	2554	2307	2562	2562	2562	2562	2562

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and Delay											
# days since last transfer	0.00694**	0.00762	0.00642**	-0.00231	-0.0000401	0.000748	-0.00482	-0.0128	-0.00446	-0.00212	-0.0336
	(0.00319)	(0.0103)	(0.00289)	(0.00167)	(0.000714)	(0.00145)	(0.00301)	(0.0220)	(0.00530)	(0.0123)	(0.0203)
# days of delay	-0.00679	-0.0235	-0.00268	0.00539	-0.00186	0.00695**	-0.00991	0.0769	0.00423	0.000887	0.0300
	(0.00702)	(0.0223)	(0.00684)	(0.00331)	(0.00181)	(0.00290)	(0.00788)	(0.0511)	(0.00962)	(0.0246)	(0.0460)
Panel B: Smoothing only											
# days since last transfer	0.00564**	0.00312	0.00605**	-0.00157	-0.000295	0.00170	-0.00618**	-0.00220	-0.00388	-0.00204	-0.0307
	(0.00262)	(0.00877)	(0.00275)	(0.00157)	(0.000715)	(0.00133)	(0.00299)	(0.0203)	(0.00498)	(0.0121)	(0.0192)
Panel C: Delay only											
# days of delay	0.00103	-0.0149	0.00416	0.00290	-0.00190	0.00775***	-0.0151*	0.0633	-0.000525	-0.00110	-0.00153
	(0.00565)	(0.0186)	(0.00676)	(0.00308)	(0.00181)	(0.00256)	(0.00788)	(0.0454)	(0.00921)	(0.0251)	(0.0433)
N	2563	2563	2561	2554	2562	2562	2562	2562	2562	1268	1268

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) for households residing in Kakuma only. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.21: Main specification with households living in Kalobeyei

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	1.899	0.00135	0.000813	0.00312	-0.00288	-0.00896*	-0.0140	0.000645	0.0212	0.00649	-0.474
	(2.582)	(0.00104)	(0.00137)	(0.00327)	(0.00577)	(0.00464)	(0.0541)	(0.0462)	(0.0817)	(0.0851)	(0.966)
# days of delay	-10.87***	-0.00581***	-0.00376**	-0.0103**	0.0120	0.0220***	-0.140	-0.162**	0.252*	0.295*	2.799**
	(4.019)	(0.00161)	(0.00186)	(0.00505)	(0.00934)	(0.00804)	(0.100)	(0.0813)	(0.142)	(0.151)	(1.417)
Panel B: Smoothing only											
# days since last transfer	-2.376	-0.000935	-0.000667	-0.000923	0.00183	0.000120	-0.0635	-0.0568	0.110**	0.111*	0.517
	(1.939)	(0.000809)	(0.00103)	(0.00233)	(0.00336)	(0.00394)	(0.0431)	(0.0367)	(0.0557)	(0.0586)	(1.212)
Panel C: Delay only											
# days of delay	-8.676***	-0.00425***	-0.00282**	-0.00668*	0.00862	0.0115*	-0.155*	-0.162**	0.275***	0.302***	2.274
	(2.979)	(0.00122)	(0.00140)	(0.00351)	(0.00523)	(0.00664)	(0.0797)	(0.0647)	(0.0953)	(0.103)	(1.995)
N	3613	3613	3613	3613	3602	3250	3614	3614	3614	3614	3614

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	-0.000706	0.00766	0.000126	0.00156	0.0000717	-0.00218	0.00328	0.0171	0.00140	-0.0102	-0.0452*
	(0.00217)	(0.00608)	(0.00230)	(0.00154)	(0.000985)	(0.00169)	(0.00244)	(0.0148)	(0.00446)	(0.0127)	(0.0263)
# days of delay	0.00373	-0.00374	-0.00364	0.0000478	0.00175	0.00212	-0.00405	-0.0230	-0.00253	0.0356*	-0.0188
	(0.00373)	(0.0103)	(0.00415)	(0.00258)	(0.00154)	(0.00285)	(0.00394)	(0.0256)	(0.00683)	(0.0186)	(0.0329)
Panel B: Smoothing only											
# days since last transfer	0.000761	0.00619	-0.00116	0.00158	0.000691	-0.00142	0.00184	0.00892	0.000508	0.00249	-0.0519**
	(0.00172)	(0.00427)	(0.00176)	(0.00127)	(0.000816)	(0.00140)	(0.00182)	(0.0120)	(0.00359)	(0.0100)	(0.0250)
Panel C: Delay only											
# days of delay	0.00291	0.00511	-0.00351	0.00177	0.00183	-0.000288	-0.000422	-0.00411	-0.000977	0.0277*	-0.0541*
	(0.00295)	(0.00734)	(0.00320)	(0.00214)	(0.00130)	(0.00237)	(0.00291)	(0.0206)	(0.00557)	(0.0150)	(0.0321)
N	3613	3613	3610	3566	3614	3614	3614	3614	3614	1801	1801

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) for households residing in Kalobeyei only. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.22: Main specification with households receiving both cash and in-kind

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	-1.466	-0.000843	-0.00132	-0.00481	0.00220	0.0305***	-1.103***	-1.225***	-0.0924	-0.126	-0.584
	(4.293)	(0.00122)	(0.00138)	(0.00422)	(0.00580)	(0.00811)	(0.185)	(0.201)	(0.0756)	(0.0811)	(0.703)
# days of delay	-16.25**	-0.00558**	-0.00616**	-0.0171**	0.0194**	0.0165	0.127	0.124	0.171	0.140	0.868
	(7.739)	(0.00217)	(0.00272)	(0.00800)	(0.00922)	(0.0137)	(0.341)	(0.352)	(0.155)	(0.168)	(1.382)
Panel B: Smoothing only											
# days since last transfer	-5.992*	-0.00240***	-0.00304***	-0.00957***	0.00765*	0.0351***	-1.077***	-1.199***	-0.0563	-0.0961	-0.401
Panel B: Delay only											
# days of delay	-17.74***	-0.00643***	-0.00751***	-0.0220***	0.0217***	0.0459***	-0.918***	-1.037***	0.0836	0.0208	0.315
	(5.745)	(0.00155)	(0.00229)	(0.00642)	(0.00714)	(0.0125)	(0.296)	(0.311)	(0.127)	(0.142)	(1.101)
N	3504	3504	3504	3504	3491	3155	3501	3501	3501	3501	3501

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00790***	0.0156*	0.00832***	-0.00170	-0.000282	0.000759	-0.00190	-0.00646	-0.00175	-0.00524	-0.0187
	(0.00274)	(0.00868)	(0.00252)	(0.00152)	(0.000756)	(0.00151)	(0.00265)	(0.0178)	(0.00468)	(0.0113)	(0.0213)
# days of delay	-0.00751	-0.0212	-0.00667	0.00463*	-0.000276	0.00563*	-0.00848*	0.00624	-0.00363	0.0148	0.00278
	(0.00515)	(0.0154)	(0.00500)	(0.00255)	(0.00173)	(0.00300)	(0.00511)	(0.0334)	(0.00796)	(0.0214)	(0.0400)
Panel B: Smoothing only											
# days since last transfer	0.00581**	0.00969	0.00691***	-0.000718	-0.000340	0.00195	-0.00369	-0.00515	-0.00252	-0.00276	-0.0182
	(0.00240)	(0.00705)	(0.00227)	(0.00147)	(0.000713)	(0.00143)	(0.00252)	(0.0157)	(0.00396)	(0.0108)	(0.0199)
Panel C: Delay only											
# days of delay	0.000549	-0.00533	0.00121	0.00302	-0.000543	0.00635**	-0.0103**	0.000114	-0.00529	0.0106	-0.0121
	(0.00450)	(0.0123)	(0.00453)	(0.00256)	(0.00162)	(0.00287)	(0.00493)	(0.0294)	(0.00651)	(0.0207)	(0.0377)
N	3504	3504	3498	3482	3501	3501	3501	3501	3501	1739	1739

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) for households receiving both cash and in-kind only. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.23: Main specification with households receiving cash transfers only

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	3.077	0.00155	0.000144	0.00409	-0.00114	-0.00771	0.00484	0.0205	0.0368	0.0332	-0.0447
	(2.787)	(0.00114)	(0.00154)	(0.00383)	(0.00598)	(0.00524)	(0.0661)	(0.0544)	(0.0789)	(0.0828)	(1.053)
# days of delay	-12.77***	-0.00620***	-0.000720	-0.00908	0.00889	0.0152	-0.192	-0.180*	0.259*	0.297**	2.804
	(4.466)	(0.00188)	(0.00225)	(0.00592)	(0.0115)	(0.00978)	(0.124)	(0.1000)	(0.136)	(0.149)	(1.823)
Panel B: Smoothing only											
# days since last transfer	-1.735	-0.000785	-0.000127	0.000664	0.00222	-0.00168	-0.0613	-0.0416	0.126**	0.135**	0.921
	(2.207)	(0.000948)	(0.00109)	(0.00272)	(0.00323)	(0.00433)	(0.0485)	(0.0399)	(0.0628)	(0.0660)	(1.467)
Panel C: Delay only											
# days of delay	-9.015**	-0.00430***	-0.000543	-0.00409	0.00750	0.00551	-0.186**	-0.156**	0.303***	0.336***	2.751
	(3.565)	(0.00155)	(0.00156)	(0.00413)	(0.00649)	(0.00789)	(0.0915)	(0.0734)	(0.108)	(0.117)	(2.654)
N	2674	2674	2674	2674	2667	2402	2677	2677	2677	2677	2677

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	-0.00249	0.00192	-0.00254	0.00190	0.000259	-0.00199	0.00229	0.0189	-0.000658	-0.000331	-0.0535
	(0.00217)	(0.00654)	(0.00224)	(0.00163)	(0.00110)	(0.00182)	(0.00284)	(0.0178)	(0.00450)	(0.0141)	(0.0354)
# days of delay	0.00725*	0.00427	-0.000683	-0.00137	0.00195	-0.000167	-0.000302	-0.0154	0.000982	0.0315	-0.00830
	(0.00391)	(0.0116)	(0.00463)	(0.00294)	(0.00171)	(0.00330)	(0.00495)	(0.0315)	(0.00781)	(0.0192)	(0.0356)
Panel B: Smoothing only											
# days since last transfer	0.000245	0.00353	-0.00278	0.00143	0.000929	-0.00205	0.00218	0.0136	-0.000319	0.00980	-0.0562*
	(0.00181)	(0.00486)	(0.00192)	(0.00128)	(0.000897)	(0.00146)	(0.00206)	(0.0138)	(0.00389)	(0.0111)	(0.0327)
Panel C: Delay only											
# days of delay	0.00421	0.00662	-0.00369	0.000892	0.00225	-0.00253	0.00242	0.00705	0.000201	0.0313**	-0.0509
	(0.00324)	(0.00866)	(0.00389)	(0.00233)	(0.00143)	(0.00265)	(0.00358)	(0.0244)	(0.00684)	(0.0151)	(0.0356)
N	2674	2674	2675	2640	2677	2677	2677	2677	2677	1330	1330

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) for households receiving cash transfers only. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.24: Main specification with interaction term by place of registration

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	1.575	-0.00174	-0.00284	-0.00449	0.00288	0.0437***	-1.483***	-1.660***	-0.0584	-0.0848	0.0437
	(5.867)	(0.00164)	(0.00174)	(0.00522)	(0.00768)	(0.00989)	(0.238)	(0.261)	(0.0482)	(0.0555)	(0.785)
# days of delay	-37.96***	-0.0122***	-0.00949*	-0.0350**	0.0421**	0.0255	-0.322	-0.355	-0.277***	-0.376***	1.420
	(14.50)	(0.00404)	(0.00512)	(0.0148)	(0.0175)	(0.0222)	(0.624)	(0.646)	(0.104)	(0.119)	(1.633)
# days since last transfer X Kalobeyei	-1.070	0.00297	0.00335	0.00623	-0.00383	-0.0532***	1.419***	1.609***	0.0413	0.0548	-0.630
	(6.508)	(0.00203)	(0.00248)	(0.00657)	(0.00986)	(0.0117)	(0.248)	(0.270)	(0.0923)	(0.100)	(1.249)
# days of delay X Kalobeyei	30.12**	0.00729	0.00742	0.0279*	-0.0354*	-0.00470	0.223	0.236	0.630***	0.769***	1.243
	(15.27)	(0.00462)	(0.00585)	(0.0167)	(0.0200)	(0.0246)	(0.634)	(0.654)	(0.169)	(0.187)	(2.221)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.0107***	0.0128	0.00750**	-0.00291	-0.0000886	0.000652	-0.00522	-0.00563	-0.00509	-0.00425	-0.0311
	(0.00373)	(0.0118)	(0.00312)	(0.00180)	(0.000744)	(0.00150)	(0.00319)	(0.0217)	(0.00523)	(0.0133)	(0.0211)
# days of delay	-0.00998	-0.0347	-0.00191	0.00835***	-0.00195	0.00635***	-0.0131*	0.0934*	-0.00357	0.0105	0.0675
	(0.00880)	(0.0277)	(0.00678)	(0.00316)	(0.00164)	(0.00237)	(0.00770)	(0.0481)	(0.00945)	(0.0264)	(0.0457)
# days since last transfer X Kalobeyei	-0.0121***	-0.00849	-0.00696*	0.00490**	0.000370	-0.00243	0.00794**	0.0280	0.00760	0.00123	-0.0121
	(0.00451)	(0.0139)	(0.00391)	(0.00237)	(0.00123)	(0.00232)	(0.00402)	(0.0265)	(0.00704)	(0.0175)	(0.0336)
# days of delay X Kalobeyei	0.0148	0.0349	-0.00238	-0.00944**	0.00359	-0.00505	0.0107	-0.129**	0.00117	0.0151	-0.103*
	(0.00995)	(0.0310)	(0.00805)	(0.00395)	(0.00222)	(0.00374)	(0.00859)	(0.0543)	(0.0114)	(0.0316)	(0.0551)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with interaction term by place of registration. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.25: Main specification with interaction term by camp

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	1.716	-0.00131	-0.00266	-0.00312	0.00281	0.0417***	-1.443***	-1.624***	-0.0711	-0.0949*	0.0278
	(5.879)	(0.00165)	(0.00173)	(0.00516)	(0.00763)	(0.00976)	(0.240)	(0.261)	(0.0469)	(0.0549)	(0.782)
# days of delay	-37.83**	-0.0119***	-0.00881*	-0.0328**	0.0381**	0.0167	-0.306	-0.300	-0.254**	-0.341***	1.563
	(14.90)	(0.00415)	(0.00530)	(0.0152)	(0.0175)	(0.0230)	(0.645)	(0.664)	(0.107)	(0.123)	(1.683)
# days since last transfer X Kalobeyei	-1.153	0.00238	0.00313	0.00437	-0.00394	-0.0506***	1.364***	1.562***	0.0657	0.0765	-0.596
	(6.459)	(0.00202)	(0.00244)	(0.00645)	(0.00969)	(0.0114)	(0.248)	(0.269)	(0.0906)	(0.0981)	(1.241)
# days of delay X Kalobeyei	29.77*	0.00700	0.00660	0.0253	-0.0303	0.00540	0.225	0.189	0.590***	0.713***	1.040
	(15.62)	(0.00468)	(0.00598)	(0.0171)	(0.0199)	(0.0252)	(0.652)	(0.670)	(0.171)	(0.188)	(2.277)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score(0-36)	Flanker task score(0-10)	Psychomotor vigilance task score (0-30)	Hearts & flower task score (0-40)
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Panel A: Smoothing and delay											
# days since last transfer	0.00978***	0.00893	0.00661**	-0.00251	-0.000148	0.000803	-0.00448	-0.00575	-0.00384	-0.000900	-0.0306
	(0.00368)	(0.0116)	(0.00315)	(0.00180)	(0.000757)	(0.00153)	(0.00313)	(0.0218)	(0.00531)	(0.0122)	(0.0207)
# days of delay	-0.00932	-0.0271	-0.00130	0.00681**	-0.00145	0.00557**	-0.0126	0.0955*	-0.000225	-0.00870	0.0531
	(0.00902)	(0.0277)	(0.00684)	(0.00322)	(0.00173)	(0.00249)	(0.00781)	(0.0504)	(0.00928)	(0.0234)	(0.0449)
# days since last transfer X Kalobeyei	-0.0106**	-0.00255	-0.00562	0.00420*	0.000498	-0.00272	0.00692*	0.0274	0.00577	-0.00477	-0.0144
	(0.00448)	(0.0138)	(0.00391)	(0.00240)	(0.00126)	(0.00242)	(0.00399)	(0.0266)	(0.00703)	(0.0171)	(0.0330)
# days of delay X Kalobeyei	0.0135	0.0240	-0.00348	-0.00724*	0.00292	-0.00400	0.0102	-0.130**	-0.00251	0.0418	-0.0818
	(0.0102)	(0.0310)	(0.00808)	(0.00398)	(0.00224)	(0.00374)	(0.00865)	(0.0563)	(0.0113)	(0.0293)	(0.0537)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with interaction term by camp of residence. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.26: Main specification with interaction term by type of aid received

	Table 1: Food Indicators						Table 2: Debt, assets and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Food stocks (kg) (7)	Food stocks (USD) (8)	Debt p.c. (USD) Food retailer (9)	Debt p.c. (USD) Total (10)	Assets HH (USD) (11)
Panel A: Smoothing and delay											
# days since last transfer	-0.503	-0.000261	-0.00103	-0.00302	0.000522	0.0275***	-1.099***	-1.212***	-0.0899	-0.125	-0.605
	(4.305)	(0.00127)	(0.00143)	(0.00430)	(0.00600)	(0.00817)	(0.182)	(0.199)	(0.0781)	(0.0829)	(0.692)
# days of delay	-19.65**	-0.00781***	-0.00868***	-0.0218**	0.0253**	0.0184	0.111	0.0775	0.0695	0.0527	1.730
	(7.862)	(0.00226)	(0.00317)	(0.00853)	(0.0103)	(0.0143)	(0.332)	(0.342)	(0.162)	(0.177)	(1.378)
# days since last transfer X cash recipient	2.570	0.000986	0.000591	0.00480	-0.000284	-0.0349***	1.034***	1.146***	0.113	0.147	0.421
	(4.981)	(0.00167)	(0.00215)	(0.00611)	(0.00768)	(0.0101)	(0.193)	(0.205)	(0.110)	(0.115)	(1.211)
# days of delay X cash recipient	9.631	0.00330	0.00984**	0.0171	-0.0216	-0.00400	-0.223	-0.170	0.257	0.302	0.861
	(8.825)	(0.00296)	(0.00430)	(0.0112)	(0.0150)	(0.0176)	(0.353)	(0.357)	(0.206)	(0.227)	(2.342)
N	6178	6178	6178	6178	6158	5557	6178	6178	6178	6178	6178

	Table 3: Mental health, time and risk preferences						Table 4: Memory and cognitive abilities				
	Sleep quality Likert (1-5) (1)	Good mood score (0-9) (2)	Happiness Likert (1-5) (3)	Required Rate of Return (4)	Payment now (dummy) (5)	Risk loving score (1-4) (6)	Math test score (0-3) (7)	Memory test score(0-36) (8)	Flanker task score(0-10) (9)	Psychomotor vigilance task score (0-30) (10)	Hearts & flower task score (0-40) (11)
Panel A: Smoothing and delay											
# days since last transfer	0.0101***	0.0199**	0.00879***	-0.00150	-0.000379	0.000751	-0.00195	-0.00736	-0.00189	-0.00477	-0.0189
	(0.00303)	(0.00924)	(0.00265)	(0.00155)	(0.000766)	(0.00149)	(0.00274)	(0.0177)	(0.00473)	(0.0114)	(0.0217)
# days of delay	-0.00985*	-0.0288*	-0.00697	0.00532**	0.000212	0.00513*	-0.0106**	0.0189	-0.00470	0.0124	0.0112
	(0.00570)	(0.0168)	(0.00500)	(0.00251)	(0.00165)	(0.00290)	(0.00522)	(0.0331)	(0.00803)	(0.0210)	(0.0386)
# days since last transfer X Kalobeyei	-0.0140***	-0.0239**	-0.0112***	0.00343	0.00103	-0.00303	0.00331	0.0361	0.00308	0.00246	-0.0430
	(0.00416)	(0.0121)	(0.00335)	(0.00216)	(0.00127)	(0.00224)	(0.00395)	(0.0244)	(0.00618)	(0.0170)	(0.0394)
# days of delay X Kalobeyei	0.0195***	0.0404*	0.00593	-0.00701**	0.00109	-0.00492	0.0122	-0.0497	0.00532	0.0173	-0.0301
	(0.00749)	(0.0217)	(0.00685)	(0.00356)	(0.00216)	(0.00423)	(0.00742)	(0.0444)	(0.0116)	(0.0265)	(0.0511)
N	6178	6178	6173	6122	6178	6178	6178	6178	6178	3069	3069

Notes: Instrumental Variable (IV) regressions estimating Equation (2.1) with interaction term by type of aid received. Outcome variables are defined in Appendix Table A.4. All regressions include household fixed effects and control variables, as specified in Appendix Table A.3. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Cluster standard errors by date of the interview and household are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.7.3 Analysis of Price Data - Additional Tables

Table A.27: Impacts of aid distribution and aid delays on cash prices (log), no control variables

	Dependent variable: log prices (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000317	0.000720	0.000949	0.00196**	-0.000447	-0.000305
	(0.000444)	(0.000597)	(0.000624)	(0.000834)	(0.000710)	(0.000938)
# days of delay	0.00246**	0.00309**	0.00263	0.00554***	0.00356**	0.00370*
	(0.000950)	(0.00146)	(0.00163)	(0.00113)	(0.00146)	(0.00219)
Panel B: Smoothing only						
# days since last transfer	0.00101***	0.00159***	0.00125**	0.00258**	0.000881**	0.00107**
	(0.000315)	(0.000485)	(0.000564)	(0.000990)	(0.000383)	(0.000456)
Panel C: Delay only						
# days of delay	0.00294***	0.00417***	0.00341***	0.00712***	0.00278***	0.00317***
	(0.000628)	(0.00116)	(0.000963)	(0.00210)	(0.000778)	(0.00114)
Observations	16760	5458	9841	3189	6919	2269
R^2	0.987	0.988	0.987	0.988	0.987	0.989

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of cash prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects. Control variables are excluded from regressions. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.28: Impacts of aid distribution and aid delays on aid-card prices (log), no control variables

	Dependent variable: log prices (Bamba Chakula)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000521	0.000454	0.000974	0.00282	0.000400	-0.000465
	(0.000836)	(0.00145)	(0.00207)	(0.00303)	(0.000449)	(0.00123)
# days of delay	-0.00147	-0.00532*	-0.00568	-0.0214*	-0.000857	-0.00273
	(0.00170)	(0.00283)	(0.00666)	(0.0102)	(0.000756)	(0.00195)
Panel B: Smoothing only						
# days since last transfer	0.000120	-0.00100	0.000235	0.00000393	0.000112	-0.00138
	(0.000459)	(0.000949)	(0.00138)	(0.00249)	(0.000297)	(0.000879)
Panel C: Delay only						
# days of delay	-0.000449	-0.00443***	-0.00254	-0.0122***	-0.000125	-0.00359**
	(0.000633)	(0.00148)	(0.00212)	(0.00336)	(0.000518)	(0.00138)
Observations	6949	2996	4084	1768	2865	1228
R^2	0.989	0.988	0.989	0.987	0.989	0.990

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of aid-card prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects. Control variables are excluded from regressions. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.29: Impacts of aid distribution and aid delays on cash prices (log), without market fixed effects

	Dependent variable: log prices (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000367	0.000895*	0.000233	0.00117	0.000407	0.000482
	(0.000280)	(0.000517)	(0.000444)	(0.000730)	(0.000615)	(0.000719)
# days of delay	0.000987	0.00124	-0.000303	0.00210*	0.00170	0.00175
	(0.000662)	(0.00116)	(0.00147)	(0.00117)	(0.00113)	(0.00175)
Panel B: Smoothing only						
# days since last transfer	0.000645***	0.00124***	0.000209	0.00134*	0.00105***	0.00114**
	(0.000215)	(0.000381)	(0.000465)	(0.000738)	(0.000321)	(0.000435)
Panel C: Delay only						
# days of delay	0.00161***	0.00275***	-0.000158	0.00281*	0.00241***	0.00260**
	(0.000478)	(0.000889)	(0.00136)	(0.00164)	(0.000508)	(0.00105)
Observations	16760	5458	9841	3189	6919	2269
R^2	0.987	0.989	0.987	0.988	0.987	0.989

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of cash prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.30: Impacts of aid distribution and aid delays on aid-card prices (log), without market fixed effects

	Dependent variable: log prices (Bamba Chakula)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000533	0.000906	-0.000172	0.000932	0.000242	-0.000819
	(0.000643)	(0.00140)	(0.000855)	(0.00230)	(0.000262)	(0.00116)
# days of delay	-0.000913	-0.00341	-0.00646**	-0.0199**	-0.000580	-0.00191
	(0.00131)	(0.00273)	(0.00253)	(0.00846)	(0.000486)	(0.00175)
Panel B: Smoothing only						
# days since last transfer	0.000284	-0.0000178	-0.000988	-0.00166	0.0000557	-0.00143*
	(0.000391)	(0.000909)	(0.000926)	(0.00248)	(0.000169)	(0.000773)
Panel C: Delay only						
# days of delay	0.000168	-0.00154	-0.00701***	-0.0169***	-0.000147	-0.00338**
	(0.000646)	(0.00159)	(0.00167)	(0.00420)	(0.000317)	(0.00118)
Observations	6949	2996	4084	1768	2865	1228
R^2	0.989	0.988	0.990	0.989	0.990	0.990

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of aid-card prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.31: Impacts of aid distribution and aid delays on cash prices (log), only product–market level clustering

	Dependent variable: log prices (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000450**	0.000914***	0.000168	0.00117***	0.000416	0.000497
	(0.000212)	(0.000293)	(0.000254)	(0.000384)	(0.000373)	(0.000438)
# days of delay	0.00109**	0.00125**	-0.000235	0.00211**	0.00169**	0.00174**
	(0.000438)	(0.000572)	(0.000723)	(0.000891)	(0.000688)	(0.000848)
Panel B: Smoothing only						
# days since last transfer	0.000748***	0.00125***	0.000149	0.00134***	0.00106***	0.00115***
	(0.000166)	(0.000228)	(0.000249)	(0.000379)	(0.000217)	(0.000249)
Panel C: Delay only						
# days of delay	0.00177***	0.00262***	-0.000130	0.00282***	0.00242***	0.00262***
	(0.000344)	(0.000448)	(0.000709)	(0.000886)	(0.000399)	(0.000479)
Observations	16760	5458	9841	3189	6919	2269
R^2	0.987	0.989	0.987	0.988	0.987	0.989

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of cash prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are clustered by product and market. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.32: Impacts of aid distribution and aid delays on aid-card prices (log), only product–market level clustering

	Dependent variable: log prices (Bamba Chakula)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000151	-0.000121	-0.000232	0.000829	0.000244	-0.000847
	(0.000359)	(0.000452)	(0.000556)	(0.000764)	(0.000485)	(0.000596)
# days of delay	-0.00138	-0.00450***	-0.00612*	-0.0196***	-0.000557	-0.00178
	(0.00102)	(0.00119)	(0.00318)	(0.00427)	(0.00115)	(0.00116)
Panel B: Smoothing only						
# days since last transfer	-0.000214	-0.00130***	-0.00101**	-0.00172**	0.0000643	-0.00142***
	(0.000270)	(0.000384)	(0.000500)	(0.000669)	(0.000327)	(0.000471)
Panel C: Delay only						
# days of delay	-0.00110	-0.00473***	-0.00686**	-0.0169***	-0.000122	-0.00331***
	(0.000764)	(0.00102)	(0.00278)	(0.00367)	(0.000776)	(0.000987)
Observations	6949	2996	4084	1768	2865	1228
R^2	0.989	0.989	0.990	0.989	0.990	0.990

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of aid-card prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are clustered by product and market. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.33: Impacts of aid distribution and aid delays on cash prices (log), robust standard errors

	Dependent variable: log prices (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000450*	0.000914***	0.000168	0.00117***	0.000416	0.000497
	(0.000233)	(0.000317)	(0.000310)	(0.000440)	(0.000411)	(0.000563)
# days of delay	0.00109**	0.00125*	-0.000235	0.00211**	0.00169**	0.00174
	(0.000520)	(0.000677)	(0.000809)	(0.00107)	(0.000848)	(0.00113)
Panel B: Smoothing only						
# days since last transfer	0.000748***	0.00125***	0.000149	0.00134***	0.00106***	0.00115***
	(0.000181)	(0.000242)	(0.000304)	(0.000437)	(0.000232)	(0.000304)
Panel C: Delay only						
# days of delay	0.00177***	0.00262***	-0.000130	0.00282***	0.00242***	0.00262***
	(0.000405)	(0.000518)	(0.000793)	(0.00108)	(0.000478)	(0.000604)
Observations	16760	5458	9841	3189	6919	2269
R^2	0.987	0.989	0.987	0.988	0.987	0.989

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of cash prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.34: Impacts of aid distribution and aid delays on aid-card prices (log), robust standard errors

	Dependent variable: log prices (Bamba Chakula)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000151	-0.000121	-0.000232	0.000829	0.000244	-0.000847
	(0.000353)	(0.000443)	(0.000566)	(0.000748)	(0.000461)	(0.000570)
# days of delay	-0.00138	-0.00450***	-0.00612**	-0.0196***	-0.000557	-0.00178
	(0.000981)	(0.00115)	(0.00302)	(0.00386)	(0.00115)	(0.00133)
Panel B: Smoothing only						
# days since last transfer	-0.000214	-0.00130***	-0.00101**	-0.00172***	0.0000643	-0.00142***
	(0.000257)	(0.000315)	(0.000457)	(0.000607)	(0.000316)	(0.000378)
Panel C: Delay only						
# days of delay	-0.00110	-0.00473***	-0.00686***	-0.0169***	-0.000122	-0.00331***
	(0.000713)	(0.000816)	(0.00242)	(0.00306)	(0.000781)	(0.000883)
Observations	6949	2996	4084	1768	2865	1228
R^2	0.989	0.989	0.990	0.989	0.990	0.990

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of aid-card prices. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.35: Impacts of aid distribution and aid delays on cash prices (log), with two smoothing variables

	Dependent variable: log prices (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last cash transfer	0.000312	0.000321	0.000262	0.000152	0.000416	0.000497
	(0.000286)	(0.000550)	(0.000309)	(0.000674)	(0.000626)	(0.000728)
# days since last in-kind transfer	0.0000785	0.000861	0.0000296	0.000679		
	(0.000348)	(0.000541)	(0.000318)	(0.000556)		
# days of delay	0.00119	0.00200	-0.000691	0.00240	0.00169	0.00174
	(0.000776)	(0.00139)	(0.00151)	(0.00186)	(0.00114)	(0.00178)
Observations	16760	5458	9841	3189	6919	2269
R^2	0.987	0.989	0.987	0.988	0.987	0.989
Panel B: Smoothing only						
# days since last cash transfer	0.000602***	0.000807**	0.000170	0.000471	0.00106***	0.00115**
	(0.000210)	(0.000363)	(0.000265)	(0.000533)	(0.000324)	(0.000432)
# days since last in-kind transfer	0.000154	0.000976*	0.00000188	0.000776		
	(0.000346)	(0.000533)	(0.000333)	(0.000559)		
Observations	16760	5458	9841	3189	6919	2269
R^2	0.987	0.989	0.987	0.988	0.987	0.989

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of cash prices, and including two smoothing variables differentiating between restricted and unrestricted cash transfers. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.36: Impacts of aid distribution and aid delays on aid-card prices (log), with two smoothing variables

	Dependent variable: log prices (Bamba Chakula)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last cash transfer	0.0000148	-0.000174	-0.000177	0.000663	0.000244	-0.000847
	(0.000375)	(0.000796)	(0.000492)	(0.00116)	(0.000263)	(0.00109)
# days since last in-kind transfer	-0.000493	-0.00117	-0.000633	-0.00108		
	(0.000603)	(0.00122)	(0.000631)	(0.00143)		
# days of delay	-0.00100	-0.00410**	-0.00472*	-0.0175**	-0.000557	-0.00178
	(0.000895)	(0.00179)	(0.00234)	(0.00641)	(0.000474)	(0.00177)
Observations	6949	2996	4084	1768	2865	1228
R^2	0.989	0.989	0.990	0.989	0.990	0.990
Panel B: Smoothing only						
# days since last cash transfer	-0.000173	-0.000960	-0.000531	-0.000708	0.0000643	-0.00142*
	(0.000274)	(0.000673)	(0.000481)	(0.00111)	(0.000175)	(0.000731)
# days since last in-kind transfer	-0.000570	-0.00149	-0.000884	-0.00204		
	(0.000599)	(0.00119)	(0.000570)	(0.00125)		
Observations	6949	2996	4084	1768	2865	1228
R^2	0.989	0.989	0.990	0.989	0.990	0.990

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the logarithm of aid-card prices, and including two smoothing variables differentiating between restricted and unrestricted cash transfers. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.37: Impacts of aid distribution and aid delays on cash prices, normalized by the mean price of each product

	Dependent variable: price normalized by mean price (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000283	0.000874*	0.000218	0.00144**	-0.0000417	0.000191
	(0.000343)	(0.000499)	(0.000481)	(0.000688)	(0.000765)	(0.000714)
# days of delay	0.00124*	0.00108	-0.000136	0.00215*	0.00232*	0.00194
	(0.000667)	(0.00113)	(0.00139)	(0.00110)	(0.00131)	(0.00175)
Panel B: Smoothing only						
# days since last transfer	0.000623**	0.00117***	0.000207	0.00161**	0.000836**	0.000922**
	(0.000285)	(0.000389)	(0.000494)	(0.000705)	(0.000392)	(0.000425)
Panel C: Delay only						
# days of delay	0.00166***	0.00239**	4.46e-08	0.00302*	0.00225***	0.00228**
	(0.000557)	(0.000922)	(0.00129)	(0.00173)	(0.000542)	(0.00103)
Observations	16760	5458	9841	3189	6919	2269
R^2	0.083	0.097	0.095	0.101	0.083	0.109

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the cash price divided by the mean price of each product. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.38: Impacts of aid distribution and aid delays on aid-card prices, normalized by the mean price of each product

	Dependent variable: price normalized by mean price (Bamba Chakula)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	-0.000113	-0.000289	-0.000356	0.000673	-0.000313	-0.00122
	(0.000527)	(0.00116)	(0.000885)	(0.00189)	(0.000436)	(0.00121)
# days of delay	-0.000731	-0.00448**	-0.00433	-0.0194***	0.000351	-0.00145
	(0.00121)	(0.00217)	(0.00268)	(0.00574)	(0.000929)	(0.00206)
Panel B: Smoothing only						
# days since last transfer	-0.000305	-0.00147*	-0.000904	-0.00185	-0.000200	-0.00169*
	(0.000313)	(0.000861)	(0.000804)	(0.00204)	(0.000214)	(0.000789)
Panel C: Delay only						
# days of delay	-0.000947	-0.00504***	-0.00546**	-0.0172***	-0.000208	-0.00365**
	(0.000727)	(0.00154)	(0.00234)	(0.00392)	(0.000491)	(0.00132)
Observations	6949	2996	4084	1768	2865	1228
R^2	0.213	0.207	0.201	0.232	0.280	0.209

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the aid-card price divided by the mean price of each product. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.39: Impacts of aid distribution and aid delays on cash prices, normalized by the median price of each product

	Dependent variable: price normalized by median price (cash)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	0.000252	0.000875*	0.000201	0.00145**	-0.0000519	0.000200
	(0.000358)	(0.000503)	(0.000498)	(0.000691)	(0.000809)	(0.000718)
# days of delay	0.00124*	0.00113	-0.0000119	0.00227**	0.00226*	0.00197
	(0.000668)	(0.00115)	(0.00139)	(0.00112)	(0.00134)	(0.00176)
Panel B: Smoothing only						
# days since last transfer	0.000593**	0.00118***	0.000200	0.00163**	0.000802*	0.000942**
	(0.000291)	(0.000391)	(0.000506)	(0.000710)	(0.000411)	(0.000428)
Panel C: Delay only						
# days of delay	0.00162***	0.00245***	0.000113	0.00315*	0.00217***	0.00232**
	(0.000543)	(0.000928)	(0.00129)	(0.00172)	(0.000533)	(0.00104)
Observations	16760	5458	9841	3189	6919	2269
R^2	0.186	0.152	0.186	0.151	0.202	0.170

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the cash price divided by the median price of each product. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.40: Impacts of aid distribution and aid delays on aid-card prices, normalized by the median price of each product

	Dependent variable: price normalized by median price (Bamba Chakula)					
	Both sites		Kakuma		Kalobeyei	
	All goods (1)	Staple (2)	All goods (3)	Staple (4)	All goods (5)	Staple (6)
Panel A: Smoothing and delay						
# days since last transfer	-0.000331	-0.000258	-0.000266	0.000717	-0.000794	-0.00122
	(0.000649)	(0.00117)	(0.000983)	(0.00192)	(0.000731)	(0.00122)
# days of delay	0.000227	-0.00459**	-0.00377	-0.0197***	0.00172	-0.00149
	(0.00166)	(0.00221)	(0.00307)	(0.00583)	(0.00166)	(0.00206)
Panel B: Smoothing only						
# days since last transfer	-0.000271	-0.00147	-0.000743	-0.00185	-0.000240	-0.00170*
	(0.000335)	(0.000871)	(0.000915)	(0.00207)	(0.000381)	(0.000795)
Panel C: Delay only						
# days of delay	-0.000407	-0.00509***	-0.00461	-0.0174***	0.000303	-0.00368**
	(0.000907)	(0.00156)	(0.00318)	(0.00400)	(0.000791)	(0.00133)
Observations	6949	2996	4084	1768	2865	1228
R^2	0.455	0.275	0.430	0.301	0.516	0.281

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (2.2), where the outcome variable is the aid-card price divided by the median price of each product. Columns (1) and (2) include both Kakuma and Kalobeyei markets, while Columns (3) and (4) focus on Kakuma markets, and Columns (5) and (6) focus on Kalobeyei markets. Regressions in Columns (1), (3), and (5) include all goods, whereas Columns (2), (4), and (6) consider staple goods only. All regressions include product and market fixed effects, and control variables that account for the ration cut implemented on July 3, 2023. Standard errors are two-way clustered, at the product–market and survey-date-by-camp levels. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 3

The Welfare and Market Impacts of Cuts in Humanitarian Assistance

We study the short-term welfare and market effects of a 20% cut in humanitarian assistance in one of the world's largest refugee camps, in Kenya. Using high-frequency household data with randomized survey timing and weekly price data, we find sharp declines in calorie intake (-7%), dietary diversity (-7%), food security (-12%), and expenditures (-21%). We find evidence of a credit crunch and deflation, mirroring dynamics observed during financial crises in high-income countries. These findings show that aid cuts affect recipients not only directly but also indirectly through general-equilibrium effects.

Introduction

In 2024, over 300 million people were in need of humanitarian assistance, yet only two-thirds received any support and less than half of funding needs were met (Humanitarian Action 2024). Despite these gaps, in 2025, the United States announced that it would scale back its contributions, which accounted for 43% of global humanitarian funding (Pearson 2025). Rather than offsetting this policy, other major donors are also reducing their aid budgets (Loy 2025). With growing funding gaps, aid cuts and disruptions have become a recurring feature of humanitarian operations. Yet we know almost nothing about their economic effects, despite their scale and potential severity.

From a theoretical perspective, the welfare consequences of sudden aid cuts are ambiguous. Their effects depend, first, on whether aid cuts are perceived as temporary or permanent, with the latter likely generating stronger responses. Second, households may respond by adjusting consumption patterns through reductions in quantity or quality, but may also partially smooth consumption to the extent that they can rely on credit, savings, or informal insurance (Deaton 1991; Townsend 1994; Carroll 1997). In contexts of high aid dependency, large-scale aid cuts can also trigger crisis-like general-equilibrium effects akin to episodes of demand contraction, deflation, and credit crunch observed following economic crises in high-income countries (Bernanke 2023). Despite their policy relevance, however, such dynamics remain poorly documented empirically. For ethical and practical reasons, the experimental literature on aid transfers has largely focused on the expansion of support rather than its unexpected withdrawal.

We study this question in Kakuma, Kenya, one of the world's largest refugee camps. Globally, more than 117 million people are forcibly displaced (UNHCR 2025), including over 23 million living in camps and settlements that, like Kakuma, function as semi-isolated local economies with limited integration into national and global markets (Betts et al. 2024).[1] In July 2023, the World Food Programme reduced monthly transfers by 20%, following a drop in US funding.

[1] Africa hosts the largest number of forcibly displaced people (42 million), of whom about 62% live in camps.

We evaluate the impact of this shock using high-frequency panel data from 622 refugee households, each surveyed ten times over a twelve-month period, for a total of 6,220 interviews. We exploit experimental variation in interview dates and compare outcomes immediately before and after the aid cut while controlling for household fixed effects. The aid cut was plausibly exogenous, triggered by global funding constraints rather than local conditions and implemented uniformly across households, ruling out selective exposure. We find no evidence of contemporaneous discontinuities in macro- or micro-economic conditions, supporting the interpretation that the observed changes around July 2023 are attributable to the aid cut itself.

In parallel with the household survey, we conducted a high-frequency price survey, tracking weekly prices for 70 food and beverage items across multiple locations in the camp. We use these data to assess general-equilibrium effects on prices. To contextualize and triangulate our quantitative findings, we also analyze more than 250 qualitative interviews with refugees, shopkeepers, humanitarian workers, and other key informants conducted before and after the cut.

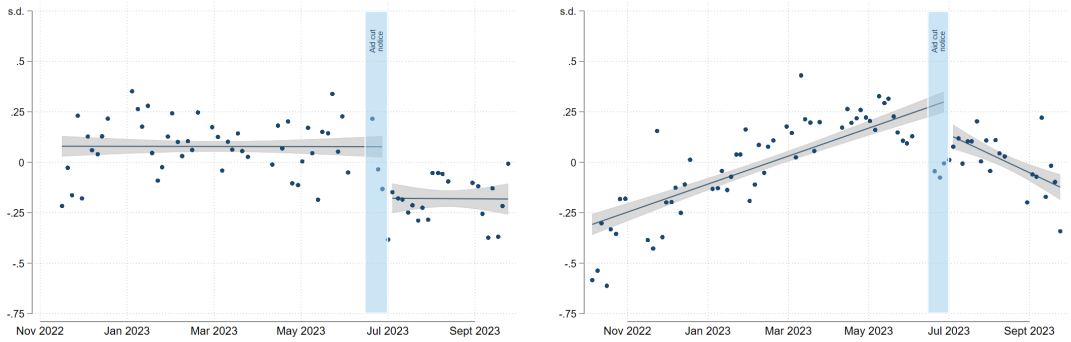
Key results are previewed in Figure 6, which plots standardized household outcomes around the aid cut, and in Figure 7, which shows corresponding price dynamics. We highlight three empirical findings. First, the aid cut had wide-ranging impacts at the household level, affecting consumption, food security, mental well-being, and behavioral outcomes. Following the cut, we observe declines in caloric intake (-7%), dietary diversity (-7%), and household expenditures (-21%), while food insecurity rose. We also document a deterioration in self-reported well-being and sleep quality, along with shifts in behavioral preferences: respondents became more future-oriented and more willing to take risks. Our estimates suggest that the welfare costs of the aid cut far exceed the corresponding fiscal savings for donors, by several orders of magnitude.

Second, we find evidence of a credit crunch following the aid cut, with a significant reduction in household borrowing and widespread reports of tightened lending conditions among shopkeepers. In this context, over 90% of refugees are indebted to food retailers,

and use aid cards on which they receive cash transfers as collateral to secure debt. Shopkeepers extend credit to their customers in anticipation of future cash transfers. When aid was reduced, the value of the collateral fell, leading shopkeepers to reduce lending limits or stop lending altogether. Average borrowing from food retailers declined by approximately 10% immediately after the cut and continued to fall in subsequent months. Households with higher levels of debt before the aid cut were particularly affected. As access to credit shrank, households increasingly relied on asset liquidation to cope with the shock.

Third, we find that prices declined significantly following the aid cut (-4%) and continued to decrease subsequently (-2% per month), illustrating how a negative demand shock propagates in a semi-closed and spatially segmented market. The decline was especially pronounced for staple goods. This price reduction partly mitigated the negative impact of the aid cut on households.

This chapter makes several contributions to the literature. First, it contributes to the literature on the effects of cash and in-kind transfers on household welfare (Gentilini 2024), by rigorously testing the short-run impacts of cuts to humanitarian assistance. The welfare consequences of such covariate shocks are theoretically ambiguous, depending on expectations, available coping mechanisms, and market responses, which makes credible empirical evidence essential. While extensive research has examined how the size, timing, and conditionality of transfers affect outcomes across a range of contexts, most of this evidence comes from the expansion of support, rather than its withdrawal. As a result, we know little about the effects of large-scale aid cuts which, for practical and ethical reasons, are more challenging to study. In Denmark, Dustmann et al. (2024) find that cuts to refugee social assistance increase employment rates in the short run, but increase poverty and crime. A few studies examine the long-term effects of cash transfers after they phase out (Aizer et al. 2016; Banerjee et al. 2021; Altındağ and O'Connell 2023), yet these papers study predictable program endings, not unexpected reductions in regular support. In Chapter 2, we study the impact of delays in humanitarian assistance using



(a) Consumption, food security

(b) Debt

(c) Stocks & assets

(d) Sleep, mood, subj. well-being

(e) Preference for the present

(f) Risk preferences

Figure 6: Impact of aid cut on standardized indices

Data source: survey data. Outcomes of interest are described in Table A.44. To produce these figures, they are grouped into summary indices using Principal Component Analysis. For each summary index, residuals from regressions on fixed effects are extracted, standardized, and used in the analysis. The blue dots show the mean of the residuals for each randomly assigned survey date. The line shows linear fits before and after the aid cut (July 3 2023). The blue-shaded area marks the period during which the aid cut was progressively announced in the camp, from June 16 when market coordinators were notified, until July 3, when in-kind distribution began.

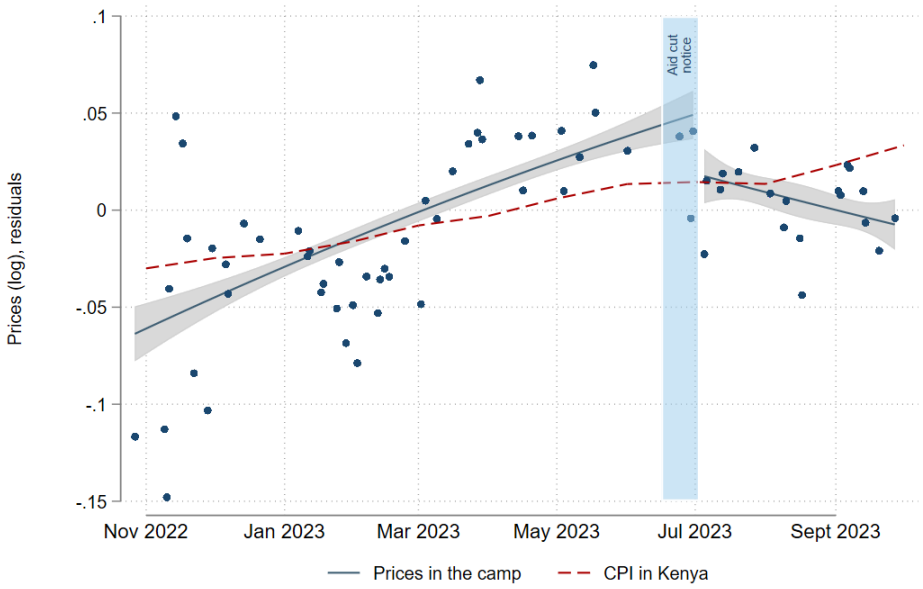


Figure 7: Impact of aid cut on prices

Data sources: Price data for Kakuma and Kalobeyei come from our market survey, while Consumer Price Index (CPI) data for Kenya are from the Kenya National Bureau of Statistics. To isolate price changes unrelated to market or product characteristics, we regress prices on product and market fixed effects and use the residuals. Blue dots represent average residual prices for each survey date. The blue line with a grey confidence interval shows a quadratic fit of prices before and after the cut. The dashed red line shows the national CPI trend over the same period. The blue-shaded area marks the period during which the aid cut was progressively announced in the camp, from June 16 when market coordinators were notified, until July 3, when in-kind distribution began.

the same high-frequency panel data. Conceptually, aid delays and aid cuts represent two different policy-relevant failures: aid delays disrupt liquidity while preserving long-run income, whereas aid cuts lower expected resources. We add to this literature by documenting the short-term consequences of a large-scale aid cut, which causes sharp and sustained declines in food consumption, food security, and total expenditure, as well as deterioration in sleep quality and psychological well-being. These findings complement recent simulation-based projections of the global health impacts of current USAID cuts (Cavalcanti et al. 2025; Kenny and Sandefur 2025).[2]

Second, this chapter speaks to the theoretical literature on intertemporal choice and consumption smoothing, which offers no clear predictions about households' responses to sudden income losses. While the Permanent Income Hypothesis (PIH) predicts that households smooth consumption by allocating resources based on expected lifetime in-

[2]It also complements macro-level analyses of foreign aid shocks (see e.g., Nielsen et al. 2011 on conflict) as well as studies analyzing unexpected program disruptions, including a disrupted loan program (Budjan et al. 2026) and delays in the roll-out of cash transfer programs (Gilligan et al. 2013; Bazzi et al. 2015).

come (Friedman 1957; Hall 1978), models with credit or savings constraints instead predict only partial smoothing, depending on access to credit or precautionary reserves (e.g., Deaton 1991; Carroll 1997). Behavioral models with present bias, temptation, or limited attention further predict limited consumption smoothing even in the absence of shocks (Strotz 1956; Laibson 1997; Gul and Pesendorfer 2001; Banerjee and Mullainathan 2010; Gabaix and Laibson 2017), with the most extreme case corresponding to hand-to-mouth behavior, in which households consume resources immediately upon receipt (Kaplan and Violante 2022; Gelman 2022). Consistent with buffer-stock models (Deaton 1991; Carroll 1997), we find that refugee households deplete assets to cope with the aid cut and the collapse of informal credit. Yet this is not sufficient to smooth consumption, highlighting the fragility of informal coping mechanisms in humanitarian economies.

Third, our work adds to the growing literature on cognitive economics, which shows that stress, hunger, and mental load can impair cognitive function, decision-making, and performance (Mani et al. 2013; Kaur et al. 2025). We find that respondents become more future-oriented and more willing to take risks following the aid cuts. These results challenge the canonical assumption of stable preferences (Deaton 1991; Morduch 1995) and instead support dynamic behavioral models, in which economic volatility reshapes not only consumption but also the underlying parameters of decision-making (Chetty and Szeidl 2007; Mullainathan and Shafir 2013; Meles et al. 2025).

Fourth, we contribute to the literature on credit and shocks. A large body of theoretical work has modeled informal transfers and credit networks as mechanisms for smoothing consumption in the absence of formal insurance (Townsend 1994; Fafchamps and Lund 2003; Bloch et al. 2008). Quasi-experimental evidence from low-income settings shows that informal credit and risk pooling within social networks are partly effective in response to idiosyncratic shocks, but tend to break down during covariate shocks (e.g., Rosenzweig and Binswanger 1993; Townsend 1994; Udry 1994; Dercon and Krishnan 2000; Fafchamps and Lund 2003; Kazianga and Udry 2006). These studies typically examine localized, temporary, or anticipated shocks, whereas aid cuts are large-scale, policy-driven, and often sudden. We show that in refugee economies, where shopkeepers treat aid cards and future

humanitarian transfers as collateral, aid cuts not only reduce household consumption, but also lead to a contraction of the informal credit system. Following the aid cut, lenders reassess repayment expectations, restrict credit, and enforce repayment, triggering a shift from debt-based smoothing to asset depletion.

Finally, we contribute to the literature on general equilibrium effects of demand shocks, and in particular, cash transfer programs. There has been considerable interest in understanding how fiscal stimulus and cash infusions affect prices and broader economic dynamics, dating back to early macroeconomic theory (Keynes 1936). This literature recently expanded to low- and middle-income settings, where researchers have studied the general equilibrium and price effects of large-scale cash and in-kind transfer programs (Angelucci and De Giorgi 2009; Cunha et al. 2019; Egger et al. 2022; Filmer et al. 2023; Delius and Sterck 2024). Empirical evidence on the price effects of contractionary shocks, such as aid cuts, remains limited. This chapter addresses this gap by studying the impacts of a large cut in humanitarian aid within a semi-closed and spatially segmented market. We show that the aid reduction triggered a sharp decline in food prices, partly mitigating negative impacts on households.

The chapter proceeds as follows. Section 3.1 describes the context in which refugees live in Kenya. Section 3.2 introduces the data and the main outcomes of interest while Section 3.3 presents the identification strategy, variables, and results of the household analysis. Section 3.4 illustrates the specification, variables and results of the price analysis. Section 3.5 discusses the welfare implications of our findings, and Section 3.6 concludes.

3.1 Context

This section first describes the context and the organization of food assistance in Kakuma. It then details the July 2023 aid cut and the process through which information about the cut spread within the camp. Finally, it places this episode in the broader context of

aid volatility in Kenya and in other humanitarian settings.

Kakuma context. The Kakuma refugee camp lies in Turkana County, a dry and arid region in northwestern Kenya, bordering Uganda, South Sudan, and Ethiopia. The camp was originally established in 1992 to provide refuge for Sudanese fleeing civil war (Betts et al. 2018). Over the past thirty years, the camp has grown significantly as a result of recurring violence and climate-related crises in the region, making it the third-largest refugee camp worldwide and the second-largest in Africa (UNHCR 2023). To accommodate the increasing number of refugees, the Kenyan government and UNHCR have expanded the camp several times and, in 2016, set up the Kalobeyei settlement a short distance from Kakuma (Betts et al. 2020).

The Kakuma camp and the Kalobeyei settlement are often considered as one large camp, given their proximity, their shared environment, and the fact that they are administered by the same authority. By late July 2023, when the aid cut took place, the combined population of Kakuma and Kalobeyei had reached 279,000. Refugee households in Kakuma and Kalobeyei primarily originate from South Sudan (49%), with Somali (16%) and Congolese (10%) households also forming a significant share. In October 2022, 81% of refugees were residing in Kakuma, while 19% lived in Kalobeyei.

The refugee population in Kakuma refugee camp and Kalobeyei settlement is heavily reliant on humanitarian aid, lacks employment opportunities, and faces high poverty levels. According to our data, 77% of South Sudanese households are fully dependent on humanitarian aid and have no additional income from work or remittances. Aid covers as much as 91% and 95% of monthly income for South Sudanese in Kakuma camp and Kalobeyei settlement respectively. Employment opportunities are extremely scarce, with only 9% of adult South Sudanese in Kakuma and 8% in Kalobeyei working for an income. The limited number of employment opportunities and high dependency on humanitarian aid create challenging living conditions. Our data show 95% of South Sudanese households in Kakuma and 86% in Kalobeyei are food insecure.[3]

[3]We use the Household Food Insecurity Access Prevalence index (HFIAS) (Coates et al. 2007a)

Food assistance and transfer modalities. The World Food Program (WFP) is the primary provider of aid to refugees in Kakuma and Kalobeyei. The type of assistance a household receives depends on the camp where they were initially registered.

In Kakuma, refugees are supported through a combination of cash transfers and in-kind food rations. The cash transfers are disbursed monthly via preregistered SIM cards. They are restricted, allowing refugees to use the funds exclusively at WFP-contracted shops and solely for purchasing food items (Siu et al. 2023; Delius and Sterck 2024).[4] During the year of data collection, monthly in-kind rations typically included 6.51 kilograms of grains (such as maize, sorghum, wheat, or rice), 1.86 kilograms of pulses (beans or split peas), and 1 liter of oil, providing between 1,000 and 1,300 calories per person per day.

In Kalobeyei, the WFP provides food assistance to refugees exclusively through cash transfers. These transfers are issued on a bank card and are unrestricted, allowing beneficiaries to purchase any goods at any store that accepts card payments. This system also allows beneficiaries to withdraw cash directly, enabling them to make purchases using physical currency.[5]

The value of food assistance provided by WFP is based on their valuation of a Minimum Expenditure Basket (MEB), which is the minimum cost required to meet a household's basic needs (WFP and UNHCR 2022). Between October 2022 and June 2023, humanitarian assistance from the WFP covered only 80% of the MEB because of budget constraints.

The July 2023 aid cut and its dissemination. In July 2023, this coverage was reduced by approximately 20% following further funding shortfalls. In Kakuma, the quantity of in-kind food rations remained unchanged, but the value of restricted cash transfers

[4]Our qualitative work suggests refugees are able to informally withdraw cash from their SIM card by paying a 10 percent withdrawal fee to the shopkeeper.

[5]Previous research indicates that unrestricted cash transfers compared to restricted transfers increase subjective well-being and non-food expenditure, without affecting food consumption (Siu et al. 2023).

decreased from KES 850 (USD 7.08) to KES 450 (USD 3.75) per person per month. In Kalobeyei—where aid is delivered entirely in cash—the value of monthly unrestricted transfers declined from KES 2,000 (USD 17) to KES 1,600 (USD 13).

The news of the July-2023 aid cut was gradually disseminated in Kakuma through a combination of official communication and informal channels. WFP works closely with food retailers on cash transfer programs and value chain development, maintaining regular contact via a WhatsApp group with market coordinators, who serve as focal points for different areas of the camp. The first official notification in this group occurred on June 16, 2023. Our qualitative research shows the information then circulated progressively, including through WhatsApp networks. It was first shared in the Whatsapp group of community leaders on June 20, with a leader complaining on June 28 about not being informed earlier. The information was first shared in a youth job-opportunities group by June 29, and finally in the group we had with enumerators on July 2 (this is when we learned about the aid cut). Finally, the entitlement sheets describing the content of food assistance were posted on public noticeboards when in-kind food distribution began on July 3.

No information was provided on how long the cut would last or whether further reductions might follow. In its June 16 message, WFP announced the implementation of the cut *"from July onward, until further notice."* The level and composition of food assistance depend on donor contributions, and WFP could not know whether its financial situation would subsequently improve or deteriorate. In practice, however, when aid cuts or increases are implemented, they typically persist for several months or years.

Aid volatility in Kenya and beyond. The July-2023 aid cut occurred within a broader pattern of volatility in humanitarian aid provision in Kenya. In 2018, refugees received assistance equivalent to 86% of the MEB. This share declined progressively, to 70% in 2019, 60% in 2020, and 50% in 2022. In late 2022, thanks to a record contribution from the United States and increased support from other donors, the World Food Programme was able to raise rations to 80% between September 2022 and June 2023 (WFP Kenya

2023). In July 2023, however, rations were again reduced to about 60% of the MEB, as the exceptional level of U.S. funding was not sustained. Further cuts occurred in March 2024 and April 2025, well after the end of our data collection. Our study focuses on the July 2023 aid cut.

These fluctuations are not unique to Kenya. They reflect a broader pattern of declining donor commitments to long-term humanitarian support, even as global needs rise. In 2023, 339 million people required humanitarian assistance, but donors provided less than half of the $56.7 billion required to meet identified needs (Humanitarian Action 2023). Although funding requirements have tripled and the number of people in need has quadrupled since 2015, donor contributions have not kept pace. As a consequence, only 68 percent of the population in need received humanitarian assistance in 2023, and organizations have been compelled to scale down, delay, or suspend critical programs in most contexts. Funding volatility of this kind has made aid cuts and disruptions a recurrent feature of humanitarian operations.

3.2 Data

From September 2022 to November 2023, we implemented three interrelated data collection activities: (1) a high-frequency quantitative household survey; (2) a longitudinal price monitoring survey; and (3) a series of in-depth qualitative interviews.

The high-frequency household survey was administered to a representative sample of 622 South Sudanese households.[6] Each household was interviewed ten times over a 12-month period, yielding a total of 6,220 interviews. To enhance the accuracy of data on consumption and food security, the respondent selected was the household member responsible for meal preparation. All interviews were conducted by trained refugee enumerators using electronic tablets for data entry.[7]

[6]The sample was stratified by ethnicity, and the enumerator team was organized accordingly to ensure that respondents could be interviewed in their native language. The questionnaire was translated into Nuer, Dinka, and Juba Arabic. In the initial wave, representative samples of Congolese and Somali households were also surveyed; see Bruni et al. 2024 for details.

[7]The field team comprised 13 enumerators, three team leaders overseeing data collection and quality

Each of the ten waves lasted four weeks. To ensure substantial variation in interview timing across households and calendar time, waves were intentionally staggered rather than aligned to fixed calendar months. Specifically, each wave followed a 28-day cycle, slightly shorter than the average calendar month, and four two-week breaks and one one-week break were introduced between selected waves. This design ensured that interview dates drifted across the calendar over successive waves, generating wide dispersion in survey timing both across households and within households over time. Within each wave, households were randomly assigned to a three-day interview window. Our identification strategy exploits this randomized interview timing to estimate the short-term effects of the July 2023 aid cut on household outcomes.

Attrition in the study was minimal. Of the 622 households initially enrolled, 34 (5.4%) either declined to continue participation or relocated from Kakuma during the 12-month data collection period. When a household exited the study, it was replaced by the nearest neighboring household, which was assigned a new identifier and is treated as a distinct observation in the analysis. The resulting sample comprises 656 households in total: 588 completed all 10 survey waves, 34 exited the study, and 34 entered as replacements. Results are robust even when excluding attrited and replacement households from the analysis (Appendix Table A.47). Attrition was uncorrelated with the random assignment of interview dates ($p = 0.52$), mitigating concerns about selection bias.

Concurrently with the high-frequency household survey, we conducted a longitudinal price survey covering 70 food and beverage items, as well as selected non-food items, in both Kakuma and Kalobeyei. A full price assessment was conducted over the course of one week, with four such rounds completed per household survey wave. Each price survey produced a detailed record of item-level prices across the four *camps* in Kakuma and the three *villages* in Kalobeyei. For this assessment, enumerators visited multiple retail shops in each location to gather available price information. Since not all items were sold in every shop due to product availability and seasonality, prices were collected opportunistically until the item list was as complete as possible for each location. These

control, and two field coordinators.

longitudinal price data were used to assess the market-level effects of the aid cut.

In parallel with the quantitative data collection, we conducted in-depth qualitative interviews to inform our quantitative survey, contextualize our results, and explore refugees' lived experiences, in both Kakuma and Kalobeyei. Prior to launching the survey, we carried out 70 semi-structured interviews with refugees and food retailers, which informed the design of the quantitative instruments. A second wave of qualitative work was conducted in October–November 2023, comprising 134 follow-up interviews with refugees, shopkeepers, wholesalers, and other key informants. We also implemented a food security journaling exercise with six South Sudanese households, interviewed two to three times per week over a one-month period. Qualitative data are used to triangulate findings, study perceptions, and explore mechanisms of impact.

3.3 Analysis of Household Data

3.3.1 Identification Strategy

We exploit high-frequency panel data and the randomized timing of interview dates, combined with household fixed effects, to compare outcomes immediately before and after the aid cut.[8] The aid cut occurred at the beginning of the eighth survey wave, giving us about nine months of observations before the cut and three months afterwards. Our identification strategy captures both the immediate effect of the cut and the evolution of outcomes during the three subsequent months. It therefore focuses on short-term effects.

As described in Section 3.1, the aid cut was first announced to market coordinators on June 16, and the information gradually spread across the camp until July 3, when in-kind food distribution began and the entire population was expected to be informed. We use July 3 as the cutoff date in our main analysis, while noting that results are generally robust, and in some cases more pronounced, when using earlier dates (Appendix Tables A.55 and A.56).

[8]We did not register a pre-analysis plan (PAP) because the aid cut was unexpected at the start of data collection and remained unknown to us until it was implemented.

The randomization of interview dates ensures that, in expectation, households surveyed immediately before and after the cutoff are similar in the absence of the cut or other time-varying covariate shocks. The inclusion of household fixed effects allows us to control for all time-invariant household characteristics and focus on within-household changes.

We consider the aid cut to be plausibly exogenous for two main reasons. First, the decision was driven by funding shortfalls faced by the WFP, rather than by local conditions or household characteristics. Second, the aid cut affected all refugee households in Kakuma and Kalobeyei simultaneously and uniformly. Qualitative evidence shows the aid cut came as a surprise to residents. These features rule out selective exposure and limit concerns about reverse causality or omitted variable bias.

The key identifying assumption of this design is that, absent the aid cut, household outcomes would have evolved smoothly over time around the cutoff. The primary threat to identification is therefore the presence of other covariate shocks occurring around the same time, which could violate this continuity assumption.[9] We address this concern in two complementary ways. First, in the next section, we directly assess the smooth-trends assumption by examining a wide range of potential time-varying confounders at both the macro and local level around July 2023. We find no evidence that outcomes are confounded by contemporaneous shocks or discontinuities at the cutoff, supporting the interpretation that observed changes are driven by the aid cut rather than by coincident events.

Second, drawing on the regression discontinuity literature, we implement two complementary estimation strategies. Our benchmark specification is a global parametric approach, which exploits the full panel and incorporates detailed control variables and fixed effects to absorb the aid cycle and other temporal fluctuations. As a robustness check, we also estimate a local linear specification, restricting the sample to a narrow window around the cutoff. By focusing on a shorter time span, this latter approach re-

[9]Randomized interview timing mitigates concerns related to idiosyncratic shocks.

duces the likelihood that other covariate shocks confound our estimates, but at the cost of using fewer observations.

In benchmark regressions, we estimate the short-term impact of the aid cut on household well-being using the following specification:

$$\begin{aligned} Y_{it} =& \beta_0 + \beta_1 Aid\ cut_t + \beta_2 Time_t + \beta_3 Aid\ cut_t \times Time_t \\ &+ \delta^T X_{it} + HH_i + DoM_t + DoW_t + \epsilon_{it}, \end{aligned} \tag{3.1}$$

where Y_{it} represents the outcome of interest for household i interviewed at time t. The variable $Aid\ cut_t$ is a binary variable equal to one after the aid cut, and zero otherwise.[10] The coefficient β_1 captures the immediate level shift in the outcome following the cut. The variable $Time_t$ counts the number of days since the aid cut. It is set to zero on the day of the cut and takes negative value before the cut and positive values afterwards. Its coefficient, β_2, is the slope of the pre-cut time trend. The coefficient β_3 of the interaction variable $Aid\ cut_t \times Time_t$ captures the change in the slope of the time trend after the cut.

Interview dates were randomly assigned, ensuring that observations before and after the cut are, in expectation, similar in the absence of the aid cut or other covariate shock occurring around the same time. The results of an omnibus F-test with randomization inference Kerwin et al. (2024) confirm that the random assignment of survey dates is orthogonal to baseline characteristics (p-value = 0.96, see Appendix Table A.40). We face limited non-compliance with randomization: approximately 9% of interviews were done outside their assigned three-day window. We use an instrumental variable strategy to address potential endogeneity arising from this non-compliance. We use the assigned (rather than actual) interview date to construct instruments for each variable of interest

[10]Food consumption indicators, as well as sleep quality and mood measures, refer to the day prior to the survey. For these outcomes, $Aid\ cut_t$ is set to zero if the cut occurred one day before the interview. In our main specification, we exclude the small number of observations conducted on the day of the cut. Results are robust to including these observations. Expenditure is measured using a 28-day recall period; when analyzing this variable, we exclude observations from interviews conducted within 28 days after the cut, as the recall window spans both pre- and post-cut periods. Results remain similar when these observations are included in the analysis.

that depends on the interview date.[11] First-stage results are strong, with Kleibergen-Paap F-statistics exceeding 76 across all benchmark regressions (Lee et al. 2022).

Benchmark regressions include a vector of time-varying control variables X_{it} and several sets of fixed effects to address potential confounders. These controls account for time-varying fluctuations unrelated to the aid cut and help separate the effect of the aid cut from regular dynamics, for instance linked to the aid cycle or seasonality.[12]

Benchmark regressions also include three sets of fixed effects. Household fixed effects (HH_i) control for time-invariant household characteristics. Day-of-month fixed effects (DoM_t) capture variation across calendar days within each month, accounting for intra-month trends. Finally, Day-of-week fixed effects (DoW_t) control for systematic patterns across weekdays, such as schooling schedules or routines associated with regular religious days.

Standard errors are clustered at the household level, which is the unit of observation repeatedly surveyed over time and the level at which serial correlation in the error term is most likely to arise. As our analysis focuses on 23 outcome variables, we calculate sharpened q-values to control the false discovery rate (FDR) and address concerns about multiple testing (Anderson 2008; Benjamini et al. 2006). All the p-values of regression coefficients in the main analysis of household data are considered together when estimating q-values.

In Section 3.3.5, we explore heterogeneous treatment effects by camp of registration

[11] For instance, the IV for $Aid\ cut_t$ is a binary variable equal to one if the assigned interview date is after the aid cut, and zero otherwise. The assigned interview date is defined as the midpoint of the randomly-assigned three-day block.

[12] To account for the aid distribution cycle and delays, we follow Bruni and Sterck (2025) and include two controls: (1) the number of days between the interview and the most recent cash or in-kind distribution, (2) the number of days by which cash transfers are overdue as compared to the expected transfer date. We further control for other income sources and household-level shocks. This includes the value of recent harvests, the number of jobs per capita, and a binary variable for whether the household experienced any negative shock in the four weeks preceding the survey. In addition, we include the number of adults in the household and the number of children currently attending school. The latter helps account for changes in food consumption driven by access to free school meals. To capture variation in aid entitlements, we also control for household size, the number of individuals registered on the household's ration cards, and the value of the last in-kind basket in Kakuma. We also control for a dummy for whether behavioral games were conducted at the end of the survey. A detailed list and description of control variables is available in Appendix Table A.43.

or residence, by the modality of aid, and by pre-cut household economic conditions.

In Section 3.3.6, we conduct multiple tests to assess the robustness of our main specification, with results shown in the appendix. First, we estimate a local linear specification, which focuses on the three survey waves around the cut (Table A.54). Then, we re-estimate Equation 3.1 without control variables (Table A.45), without fixed effects (Table A.46), by removing attrited or replacement households (Table A.47), and without removing observations on and around the cut-off date (Table A.48). We also test a reduced form specification (Table A.50), and a set of regressions in which we include observations from wave 1 to the food indicators (Table A.53). In all instances, results remain robust. To explore possible anticipation effects, we run a robustness test in which we use June 16th as a cut-off date, which is when market coordinators were first notified of the aid cut (Table A.55). We also run a regression with the cut-off date being June 24th, as this is the mid-point between the first announcement of the aid cut and its implementation on July 3rd (Table A.56). We also present the results of different two different placebo tests (Tables A.57 and A.58). Cases in which robustness tests diverge from our main specification are discussed in detail in Section 3.3.6.

3.3.2 Time-varying confounders around the aid cut

This section assesses whether time-varying factors other than the aid cut exhibit discontinuities around July 2023 that could plausibly confound our estimates. We examine potential confounders in three domains: national macroeconomic and political conditions, other income and aid-related shocks within the camp, and household-level shocks and composition.

Macroeconomic and political conditions. We first assess whether broader macroeconomic or political developments in Kenya exhibit discontinuities around the cutoff date. Using national data, we find no evidence of a discontinuity in the consumer price index (Figure 7) or in the exchange rate around early July 2023 (Table A.41). Since 2023 was marked by episodes of political tension in Kenya, we also examine ACLED data on

protests, violence, and fatalities (ACLED 2026), and find no discontinuity in the number of events or fatalities at the cutoff, either for Kenya as a whole or within Turkana County (Table A.41).

Other local income and aid-related shocks. We next consider whether other income sources or aid programs could have experienced contemporaneous disruptions. Humanitarian assistance is by far the dominant source of income in our sample, accounting for over 90 percent of monthly income for South Sudanese households in both Kakuma and Kalobeyei. We find no evidence that delays in food assistance transfers were more frequent before or after the July 2023 cut (Table A.41). Other UNHCR cash-based interventions, such as support for energy or hygiene products, were not active around the time of the aid cut; only one such transfer occurred during our survey period, in March 2023.

Labor income represents the second most important income source for households, raising the possibility that funding shortfalls could have simultaneously affected incentive work through NGOs and international organizations. We find no evidence of a discontinuous change in employment or incentive work around the cutoff (Table A.42).

Agricultural production accounts for less than one percent of household income. We observe significant changes in rainfall and temperature around July 2023 (Table A.41), which reflect seasonal patterns (Figure A.1). The "long rains" season usually occurs between April and July, while the "short rains" season occurs between October and November. We also observe a decline in the share of households reporting agricultural activity. However, we find no significant change in the value of food harvested. Controlling for harvest values in our benchmark specification, or excluding households with regular farming activity, yields very similar results (Tables A.45 and A.49).

Other shocks and household composition. We find no discontinuities in the incidence of other self-reported shocks, including theft (the most frequently reported shock), health events, climate-related shocks, ceremonial expenditures, or other unexpected expenses

(Table A.42). In addition, household size and composition evolve smoothly around the cutoff, ruling out compositional changes as a driver of the results (Table A.42).

Overall, we find no evidence of other discontinuities around July 2023, suggesting our estimates are likely driven by the aid cut itself rather than by coincident shocks.

3.3.3 Outcome Variables

We developed a survey instrument tailored to monitor short-term changes in consumption, assets, debt, psychological well-being, time and risk preferences, and cognitive functioning. Most outcomes are measured either on the day of the interview or using a one-day recall period, allowing us to identify the immediate impacts of the aid cut and assess short-term dynamics. The only exception is expenditure, which is a comparatively more volatile outcome, and is measured with a 28-day recall period.

Before describing the outcomes, we briefly discuss the scope for strategic misreporting in response to the aid cut and explain why it is unlikely to drive our results. While concerns about measurement error in self-reported outcomes are common in survey research (e.g., Beegle et al. 2012), their relevance here is limited. The survey instrument is held constant throughout the study period, so comparisons before and after the aid cut are not confounded by changes in measurement methodology. Moreover, not all outcomes rely on simple self-reports: several key measures are based on incentivized tasks, which are unlikely to be affected by strategic reporting. Some of our main findings also move in the opposite direction of what strategic underreporting would predict—for example, we document a decline in household debt following the aid cut, whereas households attempting to influence assistance would have incentives to overstate financial distress. Importantly, the survey spans ten waves, with the aid cut occurring only in the eighth wave; by that point, respondents were well aware that the research team had no influence over the timing or level of assistance. More generally, most households in this context were already severely food insecure prior to the aid cut and routinely exposed to frequent idiosyncratic shocks, with food assistance covering only about 80 percent of the minimum expenditure basket.

This implies that any incentive to misreport, if present, would have existed throughout the survey period rather than emerging discontinuously after the aid cut.

Our main analysis focuses on a set of 23 outcome variables, grouped into four categories. Detailed descriptions of the construction of each variable are provided in Appendix Table A.44.

1. Food consumption and expenditure: We collected comprehensive data on households' food consumption and meal preparation for the day preceding the survey, and use this data to construct six outcome variables.[13] Food consumption is captured using (i) daily caloric intake per capita, (ii) the monetary value of food consumed per capita, and (iii) a binary indicator equal to one if the household consumed two or more meals the previous day, and zero otherwise. Dietary diversity is measured using the FAO's Household Dietary Diversity Score (HDDS) (Kennedy et al. 2011), which counts the number of food groups consumed out of 12 possible categories. We also consider two measures of food insecurity. Building on Kraay et al. (2024) and Sterck (2024), we measure the number of days households need to meet the WFP-recommended daily intake of 2100 calories per capita. This variable is a distribution-sensitive measure of caloric intake, giving more weight to households with low consumption levels. We also use the FAO's Household Food Insecurity Access Scale (HFIAS) with a one-day recall period to assess the frequency and severity of disruptions in access to adequate food (Coates et al. 2007a).[14] Finally, we use a measure of per capita expenditure by aggregating spending across sixteen categories of expenditure over the four weeks preceding the interview.[15]

[13]The first survey wave included a slightly shorter consumption module, with fewer items and response options. This likely contributes to the systematically lower consumption levels recorded in that wave. As a result, including wave one may artificially induce a positive time trend in consumption, potentially biasing our estimate of the aid cut's impact. In our benchmark analysis, we therefore exclude wave one from the analysis of food consumption. Results remain similar—but more pronounced—when wave one is included (Appendix Table A.53).

[14]The HFIAS recall period was 28 days in the first survey wave; therefore, this wave is excluded from analyses involving this measure.

[15]These categories included: education, healthcare, shocks, food consumed at home and outside, soap and hygiene, transportation, entertainment, clothing, airtime, holidays, charcoal and firewood, electricity, house repairs, new equipment, community contributions, and other expenses.

2. Stocks, debts, and assets: Households often rely on a range of coping strategies in response to shocks, including re-selling or utilizing food stocks, increasing debt levels, or liquidating assets. To capture data on food stocks, enumerators recorded the quantities of key staple foods (maize, sorghum, rice, wheat, pasta, and oil) held by each household during the interview. In the analysis, we use both the total stock weight (in kilograms) and its estimated monetary value in USD. Previous research in Kakuma and Kalobeyei has highlighted the central role of debt as a coping mechanism, with food retailers acting as the primary lenders (Siu et al. 2023; Bruni and Sterck 2025). We measure both the value of household debt provided by food retailers, as well as the value of household debt from all sources. Liquidating assets is another commonly used strategy in response to shocks. We estimate the value of household assets by aggregating the reported value of 24 categories of physical assets (e.g., televisions, mobile phones, tables, chairs, cooking pots), animals (mainly chickens, ducks, and doves), and self-reported savings.

3. Mental well-being, time preferences, and risk attitudes: We assess impacts on mental well-being using self-reported measures of happiness and sleep quality, both rated on a Likert scale from 1 to 5. We also compute a "subjective wellbeing score" based on nine questions from the World Health Organization's Study on Global AGEing and Adult Health (WHO-SAGE). These questions examine whether participants experienced various moods on the day prior to the survey, such as enjoying activities or feeling worried, rushed, or calm (Kowal et al. 2012). Responses are binary and aggregated to create a score ranging from 0 (poor mood) to 9 (excellent mood) (Taquet et al. 2020).

We assess impacts on time and risk preferences using incentivized games. Time preferences are measured through a series of five trade-off questions designed to estimate respondents' Required Rate of Return (RRR). In each question, participants chose between receiving KES 500 in two months or a smaller amount in one month (Cohen et al. 2020; Andreoni et al. 2015; Andreoni and Sprenger 2012). The RRR is a measure of preference for the present and corresponds to the implied discount rate under standard assumptions. We also consider a binary variable indicating whether participants preferred KES

200 immediately or KES 300 after one month.[16] Risk preferences were elicited through three binary-choice questions, each involving a trade-off between a guaranteed payment of KES 200 and a riskier option with a 50% chance of receiving a larger amount (Dave et al. 2010). Based on their responses, we classify participants into four categories of risk tolerance, with higher values indicating greater risk-loving behavior and lower values reflecting higher risk aversion.

4. Mathematics, memory and cognitive abilities: Shocks that impact nutrition and food insecurity can have downstream effects on cognitive function, especially if the shock is sustained (Mani et al. 2013). To measure this, we used five games to evaluate participants' skills in mathematics, memory, and cognitive tasks. For each game, performance is measured by the number of correct responses. Except for the math questions, all games were played directly on the tablets by participants. In each survey wave, mathematical skills were evaluated using three arithmetic problems based on common market transactions.[17] Memory was assessed using an adapted version of the Simon memory game, based on Carvalho et al. (2016). Participants were presented with sequences of colored shapes that grew in length, and they were asked to reproduce the sequences from memory. Selective attention and inhibitory control were measured through the Flanker Task (Eriksen and Eriksen 1974), in which participants viewed a series of five arrows and indicated the direction of the central arrow while ignoring the surrounding distractors. Sustained attention was assessed using the Psychomotor Vigilance Task (PVT), where participants had to respond as quickly as possible when the letter "O" appeared in a sequence of letters displayed on the screen (Dinges and Powell 1985). Finally, participants' ability to process congruent and incongruent information was assessed using the Hearts and Flowers Task (HFT) (Davidson et al. 2006). The first part of the game was on congruent information,

[16]For logistical reasons, incentive payments were tied to this question. Immediate payments were disbursed on the spot, while delayed payments were made during the next survey wave. Although this was not explicitly stated in the script, participants may have inferred this over time, potentially increasing the relevance of this question.

[17]For instance: "A bag of rice costs KES 2800, and one fish costs KES 250. If you buy one bag of rice and three fish, how much will you pay?" In one wave, a coding error resulted in six questions being asked instead of three. To maintain consistency, the number of correct answers in this wave was halved to ensure the variable ranges from 0 to 3.

in which an "O" was shown on either the left or right side of the screen and participants had to click the corresponding button. The second part was on incongruent information, and an "X" appeared on either side of the screen and respondents had to click the opposite button. The last task on mixed conditions showed a sequence of both congruent and incongruent stimuli, to which respondents had to reply accordingly.

The PVT and HFT were introduced starting in the sixth wave, resulting in a smaller sample size and lower statistical power for these outcomes.

3.3.4 Results

Table 8 reports the estimated effects of the aid cut on nutrition, food security, and household expenditure. Daily caloric intake per capita dropped by 136 kcal—a 7% reduction relative to the pre-cut mean. The value of food consumption fell by USD 0.07 per day (10%), and the share of households consuming two or more meals per day declined by 7 percentage points. These patterns indicate that households were unable to smooth consumption in response to the shock. In addition to eating less, they altered the composition of their diets: dietary diversity declined by 7%, suggesting substitution toward less varied foods. Food insecurity also worsened. Before the cut, households needed an average of 1.5 days to consume 2100 kcal; this rose by 0.18 days (12%) after the cut.

Household expenditure per capita fell by USD 3.2 per month, which corresponds to 21% of the pre-cut mean. We note that total expenditures declined by roughly the same proportion as the aid cut itself—around 20%—while caloric intake and the value of food consumption fell by "only" about 7 to 10%. This suggests that households reallocated their spending in order to protect food consumption to the largest possible extent, absorbing the shock primarily through reductions in non-food expenditures.

We find no evidence of changes in time trends for these outcomes. Of the seven variables examined, only two interaction terms are statistically significant, and their directions are inconsistent. This pattern supports the interpretation shown in Figure 6a: the aid cut caused a discrete and significant drop in consumption, but levels remained

stable both before and after the cut. Overall, the results indicate a substantial and persistent decline in household welfare following the aid reduction.

Impacts on nutrition and expenditure were also reported in qualitative interviews.[18] For instance, a Somali food retailer in Kakuma reported: *"After the aid reduction, the lives of refugees became hard. That was the money sustaining them. [...] Things are insufficient and hunger is visible."* Hunger was often reported in relation to the aid cut: *"Because everyone depends on 'Bamba Chakula' [cash transfers], hunger has stricken after the aid cut"* explained another South Sudanese refugee in Kakuma. When asked if aid was enough after the aid cut, a Somali female refugee told us: *"No, it is not enough. Now aid can buy you three kilograms of rice. [...] Before the aid cut, you could use it to buy wheat flour, rice, spaghetti, and the other small things that you needed."* When asked how, after the aid cut, she could afford soap, washing powder, and female sanitary products, the same respondent reported: *"I can't afford that, and it's not only me but my fellow refugees as well. Only few can afford it."* A Somali shopkeeper in Kakuma also reported impacts on the diversity of diets: *"Bamba Chakula [the cash transfer program] is not enough for people now. Before it was good but, since they reduced the amount of aid, you can buy nothing with it. People are crying. Previously people used aid to buy all sorts of things but now, they only buy maize and beans. They no longer buy things like sugar or flour. Money is not enough."* Another South Sudanese refugee in Kakuma told us: *"We used to eat two meals a day. But now, my family and I are forced to eat one meal a day. At times we only eat in the morning until the next day. [...] Back then, before the year 2012, I used to carry my food ration on a motorbike because it was very heavy, around twenty kilograms [...] But now, the three kilograms and one liter of oil, I can carry it by using my bare hands and I can take it home by foot."* These accounts corroborate our quantitative findings, highlighting increased hunger and food insecurity, and reduced dietary diversity because of reduced purchasing power following the aid cut.

Table 9 shows the estimated effects on common coping strategies. The results show

[18]The majority of quotes on the impacts of the aid cut are drawn from interviews with refugee shopkeepers. Our semi-structured interview guide for shopkeepers included a specific module on the aid cut.

Table 8: Aid cut impacts on food security

	Food consumption p.c.			Food insecurity			Expenses p.c.
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals per day	Dietary Diversity	days/ 2100 kcal	HFIAS score	(USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Kakuma and Kalobeyei							
Aid cut	-136.4**	-0.0684***	-0.0748***	-0.325***	0.181**	0.586***	-3.234***
	(61.45)	(0.0177)	(0.0263)	(0.0688)	(0.0755)	(0.123)	(0.730)
	[.036]	[.001]	[.009]	[.001]	[.025]	[.001]	[.001]
Time (in months)	-10.93	-0.00207	-0.00878***	0.0222**	-0.00118	-0.0555***	0.0777
	(8.311)	(0.00275)	(0.00333)	(0.00965)	(0.0126)	(0.0162)	(0.0633)
	[.172]	[.348]	[.014]	[.031]	[.563]	[.002]	[.198]
Aid cut x Time	-8.811	0.0138	0.0289**	-0.0725**	-0.0109	-0.0420	0.152
	(29.70)	(0.00930)	(0.0131)	(0.0363)	(0.0466)	(0.0624)	(0.310)
	[.507]	[.136]	[.037]	[.054]	[.538]	[.365]	[.416]
N	5557	5557	5557	5557	5539	5557	5526
Kleibergen-Paap F-statistic	692.7	692.7	692.7	692.7	683.8	692.7	424.5
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05
Panel B: Kakuma							
Aid cut	-86.33	-0.0600**	-0.0987**	-0.249**	0.276**	0.640***	-3.547***
	(114.4)	(0.0276)	(0.0393)	(0.108)	(0.129)	(0.200)	(0.999)
	[.348]	[.041]	[.02]	[.031]	[.043]	[.004]	[.002]
Time (in months)	-29.72*	-0.00148	-0.0126**	0.0343**	0.00200	-0.0650**	-0.0538
	(17.05)	(0.00441)	(0.00524)	(0.0167)	(0.0204)	(0.0276)	(0.102)
	[.087]	[.495]	[.026]	[.05]	[.563]	[.028]	[.402]
Aid cut x Time	-6.046	0.0240	0.0593***	-0.0503	-0.0650	-0.0245	0.442
	(55.13)	(0.0171)	(0.0189)	(0.0550)	(0.0803)	(0.113)	(0.385)
	[.563]	[.149]	[.005]	[.306]	[.343]	[.539]	[.217]
N	2307	2307	2307	2307	2298	2307	2289
Kleibergen-Paap F-statistic	91.34	91.34	91.34	91.34	90.10	91.34	178.2
Baseline mean	2240.5	0.681	0.383	4.880	1.415	5.297	11.67
Panel C: Kalobeyei							
Aid cut	-190.8***	-0.0831***	-0.0779**	-0.387***	0.131	0.645***	-3.765***
	(67.19)	(0.0236)	(0.0368)	(0.0903)	(0.0908)	(0.157)	(1.025)
	[.009]	[.002]	[.044]	[.001]	[.141]	[.001]	[.001]
Time (in months)	-0.299	-0.00266	-0.00327	0.0115	0.000693	-0.0572***	0.223***
	(8.411)	(0.00358)	(0.00444)	(0.0121)	(0.0165)	(0.0206)	(0.0846)
	[.566]	[.348]	[.348]	[.294]	[.566]	[.01]	[.015]
Aid cut x Time	5.653	0.00597	0.0110	-0.0684	0.00690	-0.0241	0.0987
	(35.23)	(0.0120)	(0.0182)	(0.0484)	(0.0563)	(0.0756)	(0.473)
	[.546]	[.416]	[.373]	[.149]	[.559]	[.499]	[.541]
N	3250	3250	3250	3250	3241	3250	3234
Kleibergen-Paap F-statistic	118.5	118.5	118.5	118.5	117.6	118.5	193.4
Baseline mean	1859.5	0.704	0.711	4.830	1.519	3.616	17.45

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Anderson (2008) sharpened q-values are shown in square brackets. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

declining food stocks, a contraction in informal borrowing, and a drop in household assets following the aid cut, with these negative effects intensifying over time.

Despite rising needs, households faced tighter credit constraints following the aid reduction. Average household debt to food retailers declined by USD 33—about 10% of the pre-shock average—and continued to fall by USD 24 per month thereafter. At first glance, this may appear counterintuitive, as standard microeconomic theory predicts that households would demand more credit to smooth consumption following the income shock (Friedman 1957; Hall 1978). However, this interpretation overlooks supply-side adjustments in the informal credit market. In Kakuma and Kalobeyei, credit is typically secured using aid cards as collateral, and the amount of credit that shopkeepers are willing to extend is closely linked to the size of monthly cash transfers. When transfers are reduced, the collateral loses value, prompting shopkeepers to tighten credit limits, much like the credit contractions observed during financial crises (Jensen and Johannesen 2017; Bernanke 2023).

Given tighter credit constraints, households appear to turn to asset liquidation to cope with the shock. The average value of household assets fell by USD 37—which is about 5% of the pre-cut average—and continued to fall by USD 14 per month thereafter. Average food stocks dropped by about 1.9 kg following the cut but this effect is not statistically significant (q-value = 0.2). Stocks subsequently declined by about 1.3 kg per month.

These findings point to a scenario similar to the one reported in Figures 6b and 6c, where food stocks, debt, and assets all declined immediately after the aid cut, and kept diminishing in subsequent months. Households responded to the shock not through borrowing—which contracted sharply—but by reducing consumption and food stocks and by liquidating assets, consistent with buffer-stock saving models (Deaton 1991; Carroll 1997). This shift from reversible coping strategies (such as borrowing) to less reversible ones (like asset liquidation) may leave households increasingly vulnerable to future shocks.

The widespread contraction in informal credit was also consistently reported in our qualitative interviews. Across accounts, food retailers described a mix of enforced debt

recovery and credit rationing following the aid cut. A Burundian shop owner in Kakuma told us that after the aid cut: *"Food retailers stopped giving out food on credit to fix existing debt. For example, the cash aid of this month will be divided into two parts: one part will be used to pay the debt taken last month and the remaining amount will be given to the customer in the form of food."* Another Burundian shopkeeper in Kakuma reported: *"After the aid cut, we decided to reduce the quantity of food we give on credit or refuse to give out food on credit totally."* Most shopkeepers we talked to changed the way they give credit after the aid cut happened: *"I have systematically tried to minimize the amount of credit I advance to customers"*, reported a male Ethiopian food retailer in Kakuma. In simple terms, a Somali food retailer explained the link between credit and the aid reduction: *"When we give out credit we have a limit; since the aid is reduced, the credit is also reduced."* Credit rationing and enforced repayment were also reported by customers, as one Somali refugee put it: *"After the aid cut, it became very hard for me to repay my debts to the shopkeeper. If I go to the food retailer and try to get additional credit, he would not give me any."* These accounts illustrate how the aid cut disrupted one of the few coping mechanisms available to refugees.

Table 10 shows that the aid cut affected respondents' psychological and behavioral outcomes. Results show no immediate decline in self-reported sleep quality or happiness, but rather a gradual deterioration following the aid cut. We also find shifts in measured time and risk preferences: following the cut, respondents became more likely to prefer larger delayed rewards over smaller immediate ones and more willing to choose larger but uncertain rewards. The mechanisms underlying these changes are difficult to identify with our quantitative data, and our qualitative interviews do not provide direct evidence on the channels that drive these responses. At the same time, the literature offers mixed evidence on how economic shocks affect risk preferences: while many studies find that hardship leads to greater risk aversion, others document increased risk-taking in response to adverse events (Voors et al. 2012; Hanaoka et al. 2018). We therefore interpret these results as evidence that measured preferences changed following the shock, but we lack data to support any particular behavioral mechanism.

Table 9: Aid cut impacts on debt and assets

	Food stocks		HH Debt (USD)		HH Assets
	(kg)	(USD)	Food retailer	Total	(USD)
	(1)	(2)	(3)	(4)	(5)
Panel A: Kakuma and Kalobeyei					
Aid cut	-1.857	0.242	-32.52***	-31.52***	-37.42***
	(1.546)	(1.638)	(8.812)	(9.664)	(14.40)
	[.203]	[.546]	[.001]	[.003]	[.016]
Time (in months)	0.525***	0.691***	11.30***	11.53***	26.99***
	(0.170)	(0.165)	(1.324)	(1.434)	(2.097)
	[.005]	[.001]	[.001]	[.001]	[.001]
Aid cut x Time	-1.305**	-0.808	-24.34***	-25.89***	-14.40*
	(0.655)	(0.713)	(3.626)	(4.053)	(7.449)
	[.055]	[.221]	[.001]	[.001]	[.062]
N	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	521.9	521.9	521.9	521.9	521.9
Baseline mean	24.68	22.99	341.1	365.0	701.7
Panel B: Kakuma					
Aid cut	-4.803*	-0.883	-23.90***	-26.09**	-45.52**
	(2.879)	(3.027)	(7.067)	(11.35)	(20.23)
	[.099]	[.507]	[.002]	[.031]	[.035]
Time (in months)	1.162***	1.477***	4.012***	5.525***	34.90***
	(0.338)	(0.332)	(1.054)	(1.408)	(3.533)
	[.002]	[.001]	[.001]	[.001]	[.001]
Aid cut x Time	-3.301***	-2.009	-8.949***	-15.74***	-45.76***
	(1.145)	(1.319)	(3.095)	(5.414)	(8.415)
	[.008]	[.127]	[.008]	[.008]	[.001]
N	2532	2532	2532	2532	2532
Kleibergen-Paap F-statistic	154.0	154.0	154.0	154.0	154.0
Baseline mean	33.37	33.52	111.4	144.4	949.8
Panel C: Kalobeyei					
Aid cut	-4.383***	-3.854***	-44.06***	-43.37***	-35.14
	(1.338)	(1.190)	(14.71)	(15.23)	(25.36)
	[.003]	[.003]	[.006]	[.009]	[.15]
Time (in months)	0.560***	0.639***	16.29***	15.77***	21.22***
	(0.156)	(0.129)	(2.152)	(2.281)	(2.570)
	[.002]	[.001]	[.001]	[.001]	[.001]
Aid cut x Time	-0.285	-0.471	-32.51***	-30.10***	9.024
	(0.712)	(0.637)	(5.735)	(5.931)	(11.75)
	[.453]	[.348]	[.001]	[.001]	[.347]
N	3578	3578	3578	3578	3578
Kleibergen-Paap F-statistic	278.6	278.6	278.6	278.6	278.6
Baseline mean	18.49	15.49	504.7	522.1	525.0

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Anderson (2008) sharpened q-values are shown in square brackets. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Aid cut impacts on mood, time and risk preferences

	Mental wellbeing			Time preferences		Risk pref.
	Sleep quality Likert (1-5) (1)	Good mood score (0-9) (2)	Happiness Likert (1-5) (3)	Required Rate of Return (4)	Payment now (dummy) (5)	Risk loving score (1-4) (6)
Panel A: Kakuma and Kalobeyei						
Aid cut	-0.0588	0.00960	0.0154	-0.202***	-0.0480***	0.141***
	(0.0494)	(0.145)	(0.0469)	(0.0284)	(0.0134)	(0.0258)
	[.205]	[.566]	[.496]	[.001]	[.002]	[.001]
Time (in months)	0.0376***	0.139***	0.0263***	0.0440***	0.0234***	-0.0533***
	(0.00664)	(0.0205)	(0.00627)	(0.00437)	(0.00247)	(0.00543)
	[.001]	[.001]	[.001]	[.001]	[.001]	[.001]
Aid cut x Time	-0.0437*	-0.125*	-0.0729***	0.0236*	-0.00475	0.0306***
	(0.0238)	(0.0751)	(0.0211)	(0.0127)	(0.00611)	(0.0106)
	[.074]	[.099]	[.002]	[.07]	[.344]	[.008]
N	6112	6112	6107	6067	6112	6112
Kleibergen-Paap F-statistic	834.7	834.7	517.2	513.8	521.9	521.9
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438
Panel B: Kakuma						
Aid cut	-0.162**	0.215	-0.0487	-0.156***	-0.0731***	0.164***
	(0.0767)	(0.263)	(0.0763)	(0.0434)	(0.0182)	(0.0369)
	[.045]	[.341]	[.368]	[.002]	[.001]	[.001]
Time (in months)	0.0720***	0.189***	0.0573***	0.0543***	0.0216***	-0.0515***
	(0.0103)	(0.0368)	(0.00992)	(0.00665)	(0.00338)	(0.00812)
	[.001]	[.001]	[.001]	[.001]	[.001]	[.001]
Aid cut x Time	-0.00896	-0.133	-0.00207	0.00303	-0.0120	0.0337**
	(0.0361)	(0.139)	(0.0333)	(0.0182)	(0.00825)	(0.0139)
	[.532]	[.288]	[.566]	[.546]	[.139]	[.025]
N	2533	2533	2531	2528	2532	2532
Kleibergen-Paap F-statistic	100.5	100.5	153.7	152.9	154.0	154.0
Baseline mean	2.740	3.519	2.535	1.985	0.916	1.134
Panel C: Kalobeyei						
Aid cut	0.0526	-0.137	0.106*	-0.228***	-0.0337	0.144***
	(0.0673)	(0.172)	(0.0621)	(0.0405)	(0.0209)	(0.0395)
	[.344]	[.343]	[.093]	[.001]	[.107]	[.001]
Time (in months)	0.00771	0.105***	-0.000769	0.0354***	0.0257***	-0.0558***
	(0.00860)	(0.0234)	(0.00806)	(0.00592)	(0.00349)	(0.00760)
	[.313]	[.001]	[.563]	[.001]	[.001]	[.001]
Aid cut x Time	-0.0673**	-0.150*	-0.126***	0.0330*	-0.00153	0.0235
	(0.0318)	(0.0860)	(0.0275)	(0.0177)	(0.00926)	(0.0168)
	[.044]	[.087]	[.001]	[.07]	[.546]	[.149]
N	3577	3577	3574	3537	3578	3578
Kleibergen-Paap F-statistic	160.6	160.6	276.8	274.7	278.6	278.6
Baseline mean	3.281	6.013	2.897	1.643	0.813	1.655

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Anderson (2008) sharpened q-values are shown in square brackets. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11 presents the estimated effects of the aid cut on cognitive performance. We find no statistically significant impacts on any of the memory or cognition tests. The regression coefficient of the math score is positive and statistically significant, suggesting respondents answered more questions correctly after the aid cut. This counterintuitive result likely reflects two factors. First, mathematical performance improved over time, possibly due to repetition of similar questions across waves. Second, the math questions in wave 7—the survey wave immediately preceding the cut—were significantly more difficult, partly explaining the result. To assess mathematical skills, four sets of three math questions were used across survey waves. Waves 1 and 4 had one set; waves 2, 5, and 9 had another; waves 3, 7, and 10 a third; and waves 6 and 8 a fourth. Including fixed effects for these question sets in the regression reduces the estimated impact of the aid cut by about half and increases its p-value to 0.09. The fixed effect coefficients confirm that the third set of questions—used in wave 7—was the most difficult: the associated coefficient is negative and significant at the 1% level, indicating that respondents answered 0.3 fewer questions correctly compared to the first (easiest) set.

3.3.5 Heterogeneous Effects

We first examine heterogeneity in treatment effects by site, and then turn to heterogeneity with respect to pre-cut household economic conditions, focusing on consumption, debt, and asset holdings.

Refugee households in Kakuma and Kalobeyei receive different aid packages and may therefore respond differently to the aid cut. Although the overall value of monthly assistance was broadly similar across the two sites prior to the cut, and the size of the cut was comparable, the modalities of delivery differ. In Kakuma, households receive two staggered transfers each month: one in-kind and one in restricted cash. In contrast, households in Kalobeyei receive a single monthly transfer in the form of unrestricted cash.

These differences influence access to informal credit, as the aid cards used for cash transfers serve as collateral for borrowing. The larger and more flexible cash transfers in

Table 11: Aid cut impacts on cognitive abilities

	Math test score (0-3) (1)	Memory test score(0-36) (2)	Flanker task score(0-10) (3)	Psychomotor vigilance task score (0-30) (4)	Hearts & flower task score (0-40) (5)
Panel A: Kakuma and Kalobeyei					
Aid cut	0.191***	0.221	0.109	-0.0415	0.0225
	(0.0435)	(0.310)	(0.251)	(0.0846)	(0.529)
	[.001]	[.355]	[.44]	[.416]	[.566]
Time (in months)	0.00450	0.220***	-0.00591	0.0930***	0.342
	(0.00661)	(0.0413)	(0.117)	(0.0107)	(0.238)
	[.365]	[.001]	[.566]	[.001]	[.143]
Aid cut x Time	0.0313	0.106	0.0855	-0.0317	0.277
	(0.0212)	(0.161)	(0.137)	(0.0402)	(0.252)
	[.136]	[.366]	[.368]	[.344]	[.231]
N	6112	6112	3069	6112	3069
Kleibergen-Paap F-statistic	521.9	521.9	195.9	521.9	195.9
Baseline mean	1.935	18.66	28.28	8.471	34.38
Panel B: Kakuma					
Aid cut	0.144**	0.0384	-0.181	-0.106	0.466
	(0.0622)	(0.538)	(0.408)	(0.140)	(0.717)
	[.031]	[.566]	[.44]	[.348]	[.368]
Time (in months)	-0.0102	0.419***	0.108	0.0733***	0.101
	(0.0105)	(0.0740)	(0.190)	(0.0188)	(0.328)
	[.287]	[.001]	[.379]	[.001]	[.503]
Aid cut x Time	0.0960***	-0.0525	0.0178	0.0377	0.290
	(0.0315)	(0.271)	(0.242)	(0.0631)	(0.352)
	[.006]	[.545]	[.566]	[.373]	[.34]
N	2532	2532	1268	2532	1268
Kleibergen-Paap F-statistic	154.0	154.0	95.06	154.0	95.06
Baseline mean	2.343	19.85	28.39	8.639	35.39
Panel C: Kalobeyei					
Aid cut	0.167***	0.492	-0.0197	-0.0254	-0.188
	(0.0643)	(0.402)	(0.366)	(0.112)	(0.857)
	[.016]	[.199]	[.566]	[.539]	[.539]
Time (in months)	0.0215**	0.0798	0.0725	0.110***	0.497
	(0.00900)	(0.0485)	(0.157)	(0.0140)	(0.358)
	[.026]	[.102]	[.433]	[.001]	[.15]
Aid cut x Time	-0.0121	0.139	-0.0498	-0.0852	0.247
	(0.0295)	(0.205)	(0.174)	(0.0548)	(0.361)
	[.448]	[.365]	[.508]	[.119]	[.365]
N	3578	3578	1801	3578	1801
Kleibergen-Paap F-statistic	278.6	278.6	76.56	278.6	76.56
Baseline mean	1.644	17.81	28.20	8.351	33.66

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Anderson (2008) sharpened q-values are shown in square brackets. Note that different math tests of varying difficulty were used in different waves. When adding math test fixed effects, the value of the coefficient on the variable "Math test score" reduces by about half and its p-value increases to 0.09. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Kalobeyei facilitate broader participation in informal credit markets. Given the collapse of the credit market described in the previous section, the effects of the aid cut may therefore be more pronounced in Kalobeyei.

To explore this, we examine heterogeneous treatment effects by current place of residence, by place of registration, and by type of assistance (Appendix Tables A.59 to A.61).[19] Because households were not randomly assigned to sites, the results should be interpreted cautiously. Qualitative and contextual evidence suggest that differences largely reflect the modality of assistance, though other factors may also be at play.

We find no evidence of significant heterogeneity in the immediate effects of the aid cut. Across both sites, we observe reductions in caloric intake, food expenditures, meal frequency, dietary diversity, and expenditures, alongside a rise in food insecurity. In both sites, we also observe a contraction of the informal credit market, as well as some evidence of asset liquidation as a coping strategy. Over time, however, we find suggestive evidence that the contraction in informal credit became more pronounced in Kalobeyei, which likely reflects the greater importance of the credit market in Kalobeyei. For several nutrition-related outcomes, the deterioration in household welfare also appears to become more severe in Kalobeyei. One possible explanation is that households in Kakuma were better able to cope by liquidating assets, as predicted by buffer-stock saving models (Deaton 1991; Carroll 1997). This explanation is supported both by their comparatively higher pre-cut asset holdings and by evidence that asset values declined more rapidly in Kakuma following the cut.

We complement this analysis by exploring heterogeneity with respect to pre-cut household economic conditions, focusing on consumption, debt, and asset holdings. Because we do not have exogenous variation in these pre-cut characteristics, the resulting estimates should be interpreted as exploratory rather than causal.

[19]Due to mobility between the sites after registration, a number of households receive assistance modalities that do not align with their current place of residence. In Kakuma, 83% of households received a combination of in-kind food rations and restricted cash transfers, while 17% received cash transfers only. In Kalobeyei, 94% of households received cash transfers exclusively, while the remaining 6% also received in-kind food rations.

We examine heterogeneity with respect to consumption, using a standardized consumption index constructed via principal component analysis from pre-cut values (waves 1–7) of seven outcomes related to consumption, food insecurity, and expenditures. Results reported in Table A.62 show that reductions in consumption following the aid cut are significantly larger among households with higher pre-cut consumption levels. For example, the estimates imply a decline of 134 calories per person per day for households at the mean of the pre-cut consumption index, compared with a decline of 318 calories per person per day for households one standard deviation above the mean. In contrast, households with below-average pre-cut consumption experience no statistically significant decline in consumption. Households with higher levels of consumption before the cut also exhibit larger reductions in asset holdings following the cut, suggesting that they respond to the shock through a combination of reduced consumption and asset liquidation.

We next examine heterogeneity by pre-cut levels of debt and asset holdings (Tables A.63 and A.64). Households with above-average pre-cut debt experience larger declines in consumption, as well as sharper reductions in outstanding debt and food stocks. This pattern is consistent with qualitative evidence indicating that households heavily reliant on informal credit prior to the cut became credit constrained once the value of aid-based collateral declined, forcing them to reduce both borrowing and consumption.

Finally, we find that households with higher-than-average pre-cut asset holdings exhibit substantially larger declines in assets following the aid cut, consistent with the use of assets as a coping mechanism. These households are better able to maintain food stocks despite the shock. By contrast, households with below-average asset holdings experience significant reductions in food stocks, suggesting more limited capacity to smooth consumption through asset liquidation. These results suggest that households' coping strategies depend on their pre-cut economic conditions.

3.3.6 Robustness Tests

Our benchmark specification uses all observations from the data, controlling for other sources of time-varying fluctuations using covariates and different sets of fixed effects. This approach explicitly recognizes that the aid cut did not occur in a void and tries to model the aid-cycle, seasonality, and other monthly and weekly patterns that could bias impact estimates. Instead of controlling for these dynamics, an alternative, and conceptually opposite approach is to rely on a simple local linear model and to limit the influence of confounders by exploiting the randomized interview timing and household fixed effects, and by restricting the sample to a narrow window around the aid cut.

We test this local linear regression discontinuity approach with household fixed effects in Table A.54, estimating the following equation:

$$Y_{it} = \beta_0 + \beta_1 Aid\ cut_t + HH_i + \epsilon_{it}, \tag{3.2}$$

where we restrict the sample to the survey wave of the aid cut and the waves immediately before and after. This represents the narrowest possible bandwidth compatible with household fixed effects. A limitation of this approach is that it focuses on very short-term impacts only and has reduced statistical power due to the smaller sample size. More importantly, this specification likely underestimates the true impacts, as it does not control for the concurrent distribution of in-kind assistance in Kakuma beginning on July 3.[20]

Despite these limitations, we find relatively consistent results for key outcomes, including food consumption, food insecurity, expenditures, and time preferences. Estimates remain statistically significant at conventional levels, though point estimates are sometimes smaller. This variability is expected, as this specification relies on roughly one-third

[20]The local linear approach is also more susceptible to anticipation effects, as households and businesses learned about the aid cut before July 3, and observations from this announcement period constitute a larger share of the sample in this specification.

as many observations as the benchmark model. We also detect a contraction in the informal credit market. In contrast, effects on assets, sleep quality, subjective well-being, and risk-taking are not statistically significant. For these outcomes, our benchmark regressions show that effects intensify over time. Because the local linear approach relies on a narrow time window, it may fail to capture dynamic impacts that emerge more gradually.

Table 12: Local Regression Discontinuity Design

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-152.2**	-0.0474***	-0.0567**	-0.338***	0.187**	0.474***	-1.723***
	(59.61)	(0.0175)	(0.0257)	(0.0692)	(0.0853)	(0.117)	(0.454)
N	1832	1832	1832	1832	1826	1832	1832
Kleibergen-Paap F-statistic	18292.3	18292.3	18292.3	18292.3	18297.3	18292.3	24801.5
Baseline mean	1969.8	0.667	0.524	4.854	1.539	4.301	14.77

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-4.826***	-2.604*	-15.45**	-13.07*	1.662
	(1.467)	(1.473)	(6.726)	(7.478)	(9.918)
N	1832	1832	1832	1832	1832
Kleibergen-Paap F-statistic	24801.5	24801.5	24801.5	24801.5	24801.5
Baseline mean	22.61	21.37	364.2	385.7	788.1

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.0184	0.117	-0.00689	-0.0505**	-0.00649	0.00128
	(0.0477)	(0.140)	(0.0487)	(0.0238)	(0.0132)	(0.0221)
N	1832	1832	1832	1831	1832	1832
Kleibergen-Paap F-statistic	18292.3	18292.3	24801.5	24666.2	24801.5	24801.5
Baseline mean	3.184	5.492	2.850	1.877	0.931	1.298

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	0.382***	1.117***	0.255*	0.136*	0.763**
	(0.0539)	(0.313)	(0.140)	(0.0806)	(0.338)
N	1832	1832	1826	1832	1826
Kleibergen-Paap F-statistic	24801.5	24801.5	24735.0	24801.5	24735.0
Baseline mean	1.900	19.38	28.30	8.869	34.80

Notes: Instrumental Variable (IV) regressions with waves 7, 8, and 9 only. Outcome variables are defined in Appendix Table A.44. All regressions include household fixed effects. The aid cut variable is instrumented by a binary variable identifying whether the randomly assigned interview date is after the aid cut (=1) or not (=0). One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We also assess the robustness of benchmark regressions to various specification changes. In Tables A.45 and A.46, we drop control variables and fixed effects. In the specifications without control variables results remain stable, while in the estimation without fixed effects coefficients are less precisely estimated but not statistically different from our preferred specification.

In Table A.47, we re-estimate the model excluding the 34 households that attrited during the one-year survey as well as their replacements. Estimates are similar both in terms of magnitude and statistical significance, suggesting that the minimal attrition observed in the study did not significantly affect the results. Table A.48 presents estimates from the main specification without excluding observations on the date of the aid cut. Results are similar. Table A.50 shows results are similar with reduced-form regressions, using OLS regressions with explanatory variables constructed based on randomized interview dates. Table A.52 shows significance levels are similar with robust standard errors. Appendix A.53 reports estimates for the food-related outcomes, without excluding the first survey wave. Our baseline specification excludes wave one data for these outcomes, due to concerns about under-reporting in the initial survey round and the subsequent expansion of the food module in wave two. Results remain robust and, if anything, slightly stronger.

Tables A.55 and A.56 report estimates from our main specification using alternative cut-off dates of June 16 and June 24, respectively. The aid cut was first communicated informally to a limited number of market coordinators on June 16, with information gradually spreading until in-kind distribution started on July 3.[21] We therefore test whether our results are sensitive to anticipation effects associated with earlier information leaks. In both cases, the estimated coefficients remain virtually unchanged in magnitude and significance, confirming that results are robust to alternative timing assumptions.

To assess whether our main results reflect pre-existing trends or methodological issues rather than the aid cut itself, we conduct placebo tests using February 6 and March

[21] June 24 is approximately the mid-point between June 16 and July 3.

6, 2023, as alternative placebo cut-off dates (Tables A.57 and A.58). These dates were selected based on three criteria: (1) they fall on the first Monday of the month, mirroring the timing of the July 3 cut, which coincided with the start of in-kind distribution; (2) they are surrounded by at least three months of data on both sides, ensuring sufficient observations to detect placebo effects; and (3) they do not overlap with breaks in data collection, ruling out January and April 2023. We exclude all observations after July 3 to avoid contamination from the actual aid cut.

Across both placebo dates, we observe a small number of statistically significant coefficients, some in the direction of our main results, others not. These effects are generally inconsistent in sign and significance across tables. Effect sizes and significance levels are generally smaller than in our main results (4 out of 42 coefficients are significant at the 1% level in placebo tests versus 11 out of 23 in our benchmark results). Yet, more than 10% of coefficients are significant at the 10% level, inviting caution in the interpretation of small effect sizes.

3.4 Analysis of Price Data

3.4.1 Identification strategy

We use high-frequency price data to examine how food prices responded to the aid cut. We estimate the following regression equation:

$$\begin{aligned} log(P_{jkt}) =& \beta_0 + \beta_1 Aid\ cut_t + \beta_2 Time_t + \beta_3 Aid\ cut_t \times Time_t \\ &+ \delta^T X_{jkt} + Product_j + Market_k + \epsilon_{jkt}, \end{aligned} \tag{3.3}$$

where $\log(P_{jkt})$ denotes the logarithm of the price of product j in market k at time t. The variable $Aid\ cut_t$ is a binary indicator equal to one if the price observation was collected after the aid cut, and zero otherwise. We consider July 3 as the cutoff in

the main analysis, showing that results are robust (and often more salient) with earlier dates. The variable $Time_t$ captures the number of months between the price observation and the aid cut; it takes negative values for pre-cut observations. The interaction term $Aid\ cut_t \times Time_t$ equals zero before the aid cut and reflects the number of months since the cut afterward. The interpretation of the coefficients β_1, β_2, and β_3 aligns with the household-level analysis in Section 3.3.

Our preferred specification includes a vector of control variables X_{jkt} to control for price variations due to the monthly aid cycle.[22] We also include product fixed effects to control for differences in average price levels between products, and market fixed effects to account for unobservable market characteristics. Market fixed effects are defined at the level of the four Kakuma camps and the three Kalobeyei villages.

We report cluster-robust standard errors at the product–market level, which is the level of observation repeatedly surveyed over time. We also report sharpened q-values calculated using all p-values from the price analysis to address concerns regarding multiple testing (Anderson 2008; Benjamini et al. 2006).

We estimate Equation (3.3) both jointly for Kakuma and Kalobeyei and separately for each site. Despite their geographic proximity, price effects may differ across the two settings due to high transportation costs and differences in aid delivery modalities. To assess whether differences are statistically significant, we extend Equation (3.3) by including interaction terms with a dummy variable identifying observations in Kakuma (Table A.75).

We conduct multiple robustness checks to assess the sensitivity of our main specification. We re-estimate Equation 3.3 without control variables (Table A.65), without market fixed effects (Table A.66), and with heteroskedasticity-robust standard errors (Table A.67), and all yield almost identical results to our main specification. We also change our

[22]Control variables include: the number of days since the last cash and in-kind transfers; a numeric variable measuring the number of days by which a cash transfer is overdue; a binary variable equal to one for the cereal distributed in the most recent in-kind distribution; and, for this cereal, a count of the days since its distribution. These variables are described in Bruni and Sterck (2025), which examines the impact of aid delays on refugee welfare and markets.

outcome variable from logarithmic price, to prices normalized by the mean and median (Tables A.68 and A.69). We estimate a simpler local linear specification, which focuses on the three survey waves around the cut (Table A.70). Again, results remain virtually unchanged. We run a test to study potential heterogeneity by camp, and find no evidence of it (Table A.75). We show two robustness tests in which we used June 16th and June 24th as cutoff dates. June 16th is the date in which the aid cut was first announced, while June 24th is the mid-point between the aid-cut announcement and its implementation. In both cases, results are practically unchanged from our main specification (Tables A.71 and A.72). We then run two placebo tests setting the cut-off date on the first Monday of February and March 2023 (Tables A.73 and A.74). In both cases, we observe prices increasing following the placebo aid-cut, reflecting broader inflation trends at the country level.

3.4.2 Outcome variables

We use weekly data on the prices of 70 commonly traded food and beverage items. We also consider the price of staple and non-staple foods separately.[23] In the analysis, all prices are expressed in logarithms to interpret coefficients in percentage terms.

Previous research shows that the law of one price does not hold in Kakuma and Kalobeyei, as prices vary depending on whether goods are purchased with cash or aid money (Siu et al. 2023; Delius and Sterck 2024; Bruni and Sterck 2025). Our benchmark analysis focuses exclusively on cash prices, for which we collected data throughout the study period. Cash prices are also less susceptible to misreporting by shopkeepers, given that the common practice of overcharging clients who pay with aid money is prohibited by WFP regulations.[24]

[23]Staples include maize (kg or bag), maize flour (kg or bag), sorghum (kg or bag), sorghum flour (kg or bag), wheat flour (kg or bag), corn-soy blend, rice (kg or bag), pasta (500g), oil (1L, 3L, or 5L), sugar (kg), beans (kg), lentils (kg), cowpeas (kg), and potatoes (kg).

[24]We began collecting data on prices paid with aid money or debt shortly before the aid cut. Figure A.2 provides an overview of their evolution during the data collection period. In line with our benchmark results, the trends suggest that prices for goods purchased with aid money or debt also declined significantly following the aid cut.

3.4.3 Results

The reduction in aid had significant effects on food prices in both Kakuma and Kalobeyei (Table 13). The sudden loss of purchasing power following the aid cut led to an immediate drop in food prices, followed by a continued decline in subsequent months. In the short term, the aid cut led to a 3.7% decline in food prices. This negative effect intensified over time, with prices continuing to fall at an average rate of 2% per month.

Figure 7 provides visual evidence of these dynamics.[25] To rule out the possibility that these price movements reflect broader trends in Kenya or globally, the figure also plots the evolution of Kenya's Consumer Price Index (CPI) over the same period. The data show prices in Kenya rose steadily throughout the study period—by 6.5% over one year—confirming that the observed price trends in Kakuma and Kalobeyei reflect local market dynamics in response to the aid cut rather than broader national or global trends.

We find no statistically significant differences in price effects between sites. The aid cut affected both staple and non-staple foods, but the impact was stronger for staple foods, which make up the bulk of household purchases. The average price decline was 5.3% for staples, compared to 2.9% for non-staple foods.

This important market response reflects the specific structure of Kakuma's economy. It is a relatively closed system with significant market imperfections, where traders cannot easily sell outside the refugee camp or adjust prices based on national or regional markets (Delius and Sterck 2024). As a result, local prices are primarily shaped by internal supply and demand, making them sensitive to localized income shocks.

While our finding that prices continued to decline over time is consistent with dynamic pricing models, which predict slow adjustment due to the gradual dissemination of information (Mankiw and Reis 2002), we do not believe that information frictions are

[25]The figure also provides suggestive evidence that prices may have begun to decline even before July 3rd, which likely reflects the fact that market coordinators were informed about the aid cut in advance, on June 16. WFP has close ties with local shopkeepers, who meet regularly to discuss pricing within the camps (Delius and Sterck 2024). They are typically notified in advance about the contents of upcoming food rations to allow them to adjust their supply accordingly. Results are robust when considering earlier cutoff dates (Tables A.71 and A.72).

the primary driver of the continued decline in prices. Instead, our findings suggest that purchasing power continued to deteriorate as households exhausted assets and savings and as access to credit diminished. Traders likely revised their expectations downward over time in response to persistently weak demand.

Our results are robust to several specification changes, including regressions without control variables (Table A.65) or market fixed effects (Table A.66), with robust standard errors (Table A.67), or with alternative transformations of the outcome (Tables A.68 and A.69). Results are also similar with a simplified local linear model using three waves of data (Table A.70).

We also consider earlier cutoff dates reflecting the fact that information about the cut started circulating from June 16 (Tables A.71 and A.72). In all cases, estimates remain consistent. We also conduct placebo tests using the same placebo cutoff dates as in the household analysis (Tables A.73 and A.74). Coefficients are positive and statistically significant in all regressions, likely reflecting general price increases during the first nine months of the study. If anything, this suggests our estimates of price effects are conservative.

Table 13: Impacts of aid cut on cash prices (log), cluster standard errors (product and market)

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0366***	-0.0525***	-0.0290***	-0.0362***	-0.0506**	-0.0295*	-0.0426***	-0.0497**	-0.0392**
	(0.00874)	(0.0148)	(0.0108)	(0.0119)	(0.0193)	(0.0151)	(0.0142)	(0.0241)	(0.0178)
	[.001]	[.001]	[.005]	[.003]	[.005]	[.013]	[.003]	[.011]	[.01]
Time (in months)	0.0132***	0.0148***	0.0124***	0.0143***	0.0144***	0.0142***	0.0130***	0.0139***	0.0126***
	(0.00171)	(0.00210)	(0.00235)	(0.00224)	(0.00258)	(0.00310)	(0.00298)	(0.00348)	(0.00415)
	[.001]	[.001]	[.001]	[.001]	[.002]	[.002]	[.005]	[.005]	[.02]
Aid cut x Time	-0.0199***	-0.0240***	-0.0180***	-0.0246***	-0.0246***	-0.0247***	-0.0150**	-0.0219***	-0.0117
	(0.00404)	(0.00558)	(0.00536)	(0.00564)	(0.00777)	(0.00749)	(0.00583)	(0.00805)	(0.00773)
	[.001]	[.001]	[.001]	[.001]	[.001]	[.001]	[.001]	[.001]	[.003]
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.989	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.5 Welfare impacts

By cutting aid by 20 percent, donors saved approximately USD 26 million per year considering all camp-based refugees in Kenya. In this section, we present back-of-the-envelope calculations to estimate the monetary value of the resulting welfare losses, which we then compare to the donors' savings.

We use two variables to estimate welfare impacts. First, we examine the per capita value of food consumption, which is measured with a one-day recall period and yields highly precise regression coefficients. However, food accounts for only about 83% of total expenditures. As a robustness check, we also consider total expenditures per capita, a more comprehensive measure but based on a 28-day recall period.[26]

We use an Atkinson Social Welfare Function with a degree of inequality aversion equal to 2, in line with Sterck (2024). We express daily consumption and expenditure per

[26]In Kakuma, we add the value of the last in-kind ration to total expenditures to ensure the resulting variable also captures consumption from in-kind aid.

capita in 2017 PPP$ and bottom-code the values at $0.25 per day to prevent very low values from dominating our estimates (Kraay et al. 2024). As described by Sterck (2024), we re-estimate Equation (3.1) with the inverse transformation of the consumption and expenditure variables to estimate per capita welfare impacts.

Before the aid cut, households in Kakuma needed an average of 2.3 days to consume the equivalent of PPP$ 2.15—the World Bank's international extreme poverty line. This reflects exceptionally high levels of poverty, exceeding even the average in South Sudan, the world's second poorest country (Sterck 2024). After the cut, this figure rose to 2.6 days. In other words, refugees required more than two and a half days to consume what the World Bank considers the minimum daily amount needed to survive.

We estimate the aggregate impact of the aid cut by multiplying the impact estimate by the total number of refugees living in Kenyan camps at the time.[27] Then, we use the income distribution of the US, as provided by Mahler et al. (2022), to estimate the percentage reduction in US incomes that would produce an equivalent welfare loss.

We find that the impact of aid cuts on consumption is equivalent—in welfare terms—to a 0.6% reduction in all incomes across the US. While this may appear small in relative terms, it corresponds to an annual loss of about USD 55 billion to the US economy.[28] In other words, donors' annual savings of USD 26 million from the aid cut generate a welfare loss comparable to applying a USD 55 billion income cut proportionally across US incomes.

We obtain even more extreme results when considering total expenditures. Before the aid cut, households in Kakuma required an average of 3.5 days to spend the equivalent of USD 2.15.[29] The cut increased this by nearly one full day. Scaled to all refugees in

[27]According to UNHCR data, there were 279,178 refugees and asylum-seekers in Kakuma and Kalobeyei, and 363,607 in Dadaab.

[28]This calculation uses a total income figure of approximately USD 10 trillion, based on the US average income per capita in the World Bank PIP data multiplied by population size. This is lower than official GDP estimates (around USD 25 trillion) and therefore yields conservative estimates of welfare losses in Kakuma.

[29]On average, our measure of daily expenditure is lower than reported consumption. This may reflect measurement error due to the longer recall period, but it could also indicate that many refugees acquire

Kenyan camps, the welfare loss is equivalent to a 1.9% drop in all US incomes, amounting to roughly USD 189 billion annually.

While these figures may appear extreme, they illustrate the vast scale of the welfare impact relative to the modest USD 26 million in donor savings. They highlight how a major shock to one of the world's poorest populations can translate into a disproportionately large welfare loss.

3.6 Conclusion

As donor fatigue deepens and major funders such as USAID scale back operations, essential humanitarian programs are being slashed or withdrawn across the globe. Despite the scale of these disruptions, empirical evidence on the household- and market-level impacts of aid cuts remains limited. Theoretically, models of intertemporal consumption smoothing offer contrasting predictions (Friedman 1957; Hall 1978), depending on liquidity constraints and access to credit and insurance markets (Jappelli and Pagano 1989; Deaton 1991; Townsend 1994), as well as on general-equilibrium effects (Angelucci and De Giorgi 2009; Cunha et al. 2019; Egger et al. 2022; Filmer et al. 2023; Delius and Sterck 2024). Credible empirical evidence is therefore essential.

This chapter studied the short-term effects of a large-scale aid cut implemented in July 2023 in Kenya. We leverage a unique high-frequency panel dataset collected from 622 households across ten survey waves, together with market-level price data and data from more than 250 qualitative interviews. Our identification strategy exploits the plausibly-exogenous nature of the aid cut and high-frequency panel data with randomized timing of interview dates to assess both the immediate and short-run dynamic impacts on consumption, credit, behavior, and market prices.

We find that the aid cut led to substantial reductions in household consumption, food security, and psychological well-being in the short run. Caloric intake declined by 7%,

food in-kind—often on credit from shopkeepers—and these transactions may not have been properly recorded as expenditures.

dietary diversity by a similar margin, and household expenditures by 21%, while food insecurity rose. We also observe declines in self-reported sleep quality and happiness.

The aid cut also triggered important general equilibrium effects on the local economy. The informal credit system collapsed because it relied on aid cards as collateral, and their value dropped sharply after the aid cut. Average borrowing from food retailers dropped by 10% immediately after the cut and continued to decline thereafter. This forced households to turn to other coping strategies, including reducing consumption, dietary diversity, and expenditures, but also liquidating assets.

At the same time, food prices fell by 4% in the immediate aftermath of the cut and continued to decline by roughly 2% per month in subsequent months, reflecting a sustained contraction in demand and partially mitigating the impact on households. The decline was especially pronounced for staple goods.

From a policy perspective, our findings highlight the human cost of large-scale aid cuts, and the importance of donor commitment to humanitarian aid. In many ways, the current wave of aid cuts can be seen as a moral failure. The 20% cut in humanitarian aid saved donors an estimated USD 26 million per year, but generated welfare losses for refugees equivalent to a 0.6% to 1.9% reduction in all US incomes—the equivalent of USD 55 to 189 billion annually. These estimates highlight how modest fiscal savings for donors can translate into disproportionately large welfare losses when affecting extremely poor populations.

While our analysis focused on refugees in Kenya, the mechanisms and implications extend well beyond this context. In 2024, more than 300 million people were in need of humanitarian assistance, and aid cuts have become a recurrent feature of humanitarian operations worldwide. The cut we study reduced assistance from about 80% to about 60% of a full ration, but subsequent funding shortfalls from major donors such as the United States have led to even deeper reductions, with some populations entirely excluded from aid (Bakewell et al. 2025). Our estimates should therefore be interpreted as lower-bound effects of current aid cuts.

3.7 Appendix

3.7.1 Balance Tests and Variables

Table A.40: Balance tests with pre-determined variables

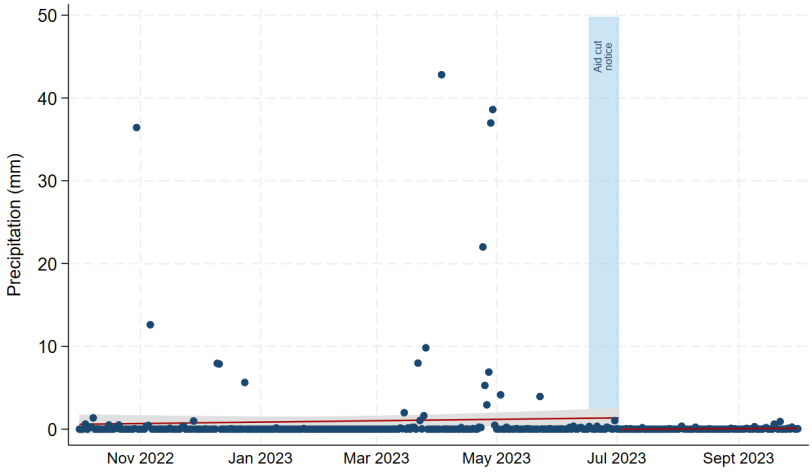
	Dependent variable: Randomly-assigned interview date (1)
Household size	0.0194
	(0.0419)
Nr of adults in hh	-0.0365
	(0.0596)
Child headed household	1.278
	(1.073)
Respondent is the household head	0.296
	(0.249)
Respondent gender	0.0617
	(0.293)
Respondent age	-0.000564
	(0.0110)
Respondent education	0.00930
	(0.0289)
Respondent speaks Dinka	-0.0615
	(0.456)
Respondent speaks Juba Arabic	-0.315
	(0.318)
Respondent speaks Nuer	-0.271
	(0.446)
registration date	-0.00832
	(0.0235)
household has electricity	0.355
	(0.285)
Kakuma 1	-0.413
	(0.466)
Kakuma 2	-0.886
	(0.616)
Kakuma 3	-0.395
	(0.479)
Kalobeyei 1	-0.776
	(0.750)
Kalobeyei 2	-0.833
	(0.752)
Kalobeyei 3	-0.599
	(0.756)
Number of ration cards	0.0762
	(0.0994)
Ration card total size	0.00148
	(0.0360)
Bamba chakula beneficiary	0.471
	(0.661)
Observations	622
R^2	0.019
F-test p-value (OLS, randomization inference)	0.955
Chi^2-test p-value (mlogit, randomization inference)	0.643

Notes: The p-values for the F-test and Chi^2-test of joint orthogonality are calculated using randomization inference, following the recommendation of Kerwin et al. (2024). The dependent variable represents the 3-day intervals randomly assigned to households at baseline, which determine target interview dates across all survey waves.

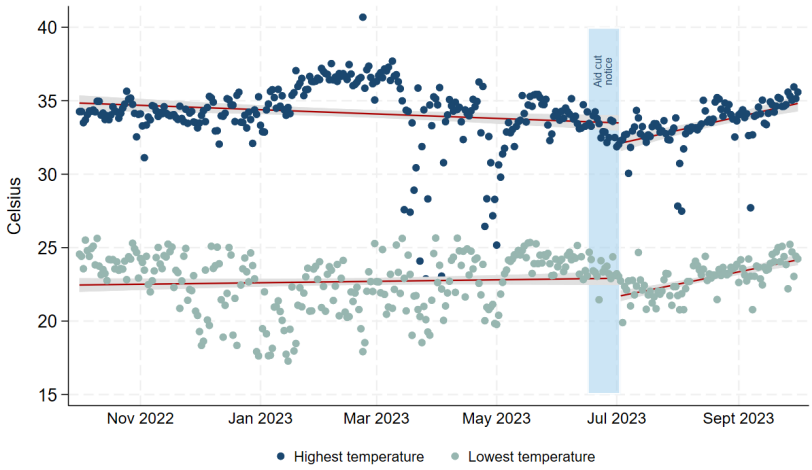
Table A.41: Time-Varying Confounders around the Aid Cut - Macro Variables

	Exchange rate		Violence (ACLED)				Cash transfer	Precipitations	Temperature (Celsius)	
	KES/USD	growth	Kenya		Turkana		delay	(mm)	Max	Min
			# events	# fatalities	# events	# fatalities	(dummy)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After ration cut (announcement)	0.384	-0.000135	2.431	1.870	-0.0763	-0.0359	-0.0128	-1.376**	-1.377***	-1.194***
	(0.287)	(0.00221)	(2.591)	(1.816)	(0.0945)	(0.207)	(0.120)	(0.661)	(0.352)	(0.275)
Time (in months)	0.0820***	0.00000470	0.0106***	0.00359*	0.000169	0.000404	0.000203	0.00281	-0.00492***	0.00167
	(0.00147)	(0.00000641)	(0.00272)	(0.00199)	(0.000282)	(0.000310)	(0.000378)	(0.00362)	(0.00112)	(0.00128)
Aid cut x Time	0.00247	-0.0000122	-0.0662*	-0.0294	0.000214	0.000313	-0.00109	-0.00155	0.0349***	0.0255***
	(0.00433)	(0.0000410)	(0.0400)	(0.0259)	(0.00148)	(0.00310)	(0.00206)	(0.00367)	(0.00430)	(0.00393)
Constant	139.0***	0.000956	6.906***	2.950***	0.213***	0.212***	0.624***	1.366**	33.49***	22.91***
	(0.211)	(0.00118)	(0.561)	(0.354)	(0.0519)	(0.0657)	(0.0597)	(0.661)	(0.256)	(0.186)
Observations	336	308	365	365	365	365	366	366	366	363
R^2	0.966	0.002	0.044	0.022	0.002	0.002	0.002	0.011	0.082	0.048

Notes: Regressions consider days between the 1st of October 2022 and the 1st of October 2023. Exchange rate data come from Google Finance. Violence data extracted from ACLED (2026). Data on aid delays come from Bruni and Sterck (2025). Weather variables are taken from Climate Prediction Center of the U.S. National Oceanic and Atmospheric Administration. Robust standard errors in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



(a) Precipitation



(b) Precipitation

Figure A.1: Discontinuity in weather variables

Data source: Weather variables are taken from Climate Prediction Center of the U.S. National Oceanic and Atmospheric Administration.

Table A.42: Time-Varying Confounders around the Aid Cut - Household Survey

	Employment				Farming		Household composition		
	Any work		Incentive work		dummy	value (KES)	# total	# adults	# children
	# job	job/adult	# job	job/adult					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Aid cut	0.0261	-0.00412	-0.0130	-0.00289	-0.0571**	-24.76	0.0797	0.0650	0.0147
	(0.0291)	(0.00563)	(0.0162)	(0.00247)	(0.0270)	(21.55)	(0.0739)	(0.0540)	(0.0393)
Time (in months)	-0.00738***	-0.000712	0.000880	0.000142	0.0101***	2.088	-0.0283*	-0.0223**	-0.00595
	(0.00275)	(0.000441)	(0.00158)	(0.000284)	(0.00209)	(1.515)	(0.0152)	(0.0107)	(0.00900)
Aid cut x Time	0.0103	0.00482*	0.0121*	0.00187**	-0.00574	8.026	0.0132	0.0363	-0.0231
	(0.0123)	(0.00251)	(0.00718)	(0.000951)	(0.0109)	(11.29)	(0.0328)	(0.0239)	(0.0169)
N	5526	5526	5526	5526	5526	5526	6112	6112	6112
Kleibergen-Paap F-statistic	428.9	428.9	428.9	428.9	428.9	428.9	530.3	530.3	530.3
Baseline mean	0.218	0.0323	0.120	0.0170	0.0883	25.66	9.362	5.823	3.539

	Ceremonial expenditure (dummy)	Theft (dummy)	Climate shock (dummy)	Police payment (dummy)	Other shock (dummy)	Illness or injury in HH #	Illness or injury in HH # per capita	Expenses due to shocks KES	Expenses due to shocks KES per capita
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.00484	0.0293	-0.0192	0.0130	0.00911	-0.106	-0.00331	225.2	18.62
	(0.0158)	(0.0315)	(0.0131)	(0.0143)	(0.0148)	(0.203)	(0.0151)	(297.9)	(38.50)
Time (in months)	-0.00305**	-0.0123***	-0.00172	-0.00225***	-0.00481***	-0.0648***	-0.00757***	-114.3***	-14.88***
	(0.00122)	(0.00223)	(0.00114)	(0.000824)	(0.00115)	(0.00702)	(0.000845)	(29.45)	(3.439)
Aid cut x Time	0.00606	0.00409	0.00101	-0.00106	0.00524	0.0821	0.00451	-18.55	5.541
	(0.00746)	(0.0136)	(0.00484)	(0.00650)	(0.00660)	(0.0724)	(0.00623)	(133.8)	(15.38)
N	5526	5526	5518	5526	5526	5526	5526	5526	5526
Kleibergen-Paap F-statistic	428.9	428.9	429.3	428.9	428.9	428.9	428.9	428.9	428.9
Baseline mean	0.0355	0.114	0.0329	0.0157	0.0255	0.863	0.105	1047.8	126.7

Notes:Instrumental Variable (IV) regressions estimating Equation (3.1). Employment, farming, and shock variables measured with a 28-day recall period. All regressions include household, day-of-month, and day-of-week fixed effects. Control variables are excluded from regressions. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.43: List of control variables used in the analysis of household data

Variable	Variable description
Children in school	Count variable representing the number of children aged 6 or older who attended school on the day of the interview.
Shock dummy	Binary variable which takes the value of one if the household faced a shock (theft, ceremony, having to pay bribes, natural hazards, or other shocks) in the four weeks preceding the interview and zero otherwise.
Jobs per capita	Number of jobs per household member (only adult employment).
Harvest value	Value of the food harvested during the four weeks preceding the interview, estimated using prices collected at baseline, and expressed in Kenyan Shillings.
Random order behavioral module	Each questionnaire had a behavioral module, which was randomly assigned to appear at the beginning or end of the questionnaire. This variable is a binary variable taking the value of one if the behavioral module appeared at the end of the questionnaire and zero otherwise.
Household size	Count variable indicating the number of people living in the household.
Adults	Count variable indicating the number of adults living in the household.
Size of ration cards	Count variable indicating the total number of individuals listed on the household ration cards.
In-kind basket value	Per capita value of last in-kind basket, estimated using prices collected at baseline (equal to zero if the household does not receive in-kind aid).
Days since transfer	Numeric variable set to zero if distribution occurred the day before the survey, and increases by 1 for each subsequent day.
Cash delay	Numeric variable measuring the number of days by which a cash transfer is overdue. It takes the value zero before the expected delivery date, equals one on the first day after the due date if the transfer has not yet arrived, and increases by one for each additional day until the transfer is delivered.

Table A.44: List of outcome variables used for the regressions on household effects

Variable	Variable construction
Food consumption per capita: calories, value, and >2 meals	The food consumption module collected data on all food and beverages consumed, whether prepared at home or eaten outside. To estimate *calorie intake per capita* (kcal), we converted each food item to kilograms using the 2012 FAO density database (version 2.0) and multiplied it by its calorie content per kilogram. Total household calorie intake was then aggregated and adjusted for household size. Similarly, to estimate the per capita value of food consumption (USD), we converted the quantity of each item to kilograms, applied unit prices estimated at baseline, aggregated the total value, and divided by household size. Using the same data, we also constructed a binary variable taking the value of one if the household had two or more meals the day before the interview and zero otherwise.
Dietary diversity	We used data from the food consumption module to construct the Household Dietary Diversity Score (HDDS) for the day preceding the survey, following the methodology and food group classifications of (Kennedy et al. 2011).
Days/2100 kcal	Following Sterck (2024), we construct a distribution-sensitive measure of calorie intake by inverting per capita calorie intake and multiplying the result by 2100, yielding the number of days required to reach 2100 kcal.
HFIAS score	The Household Food Insecurity Access Scale (HFIAS) score measures household food insecurity. We constructed this variable following the 2007 FAO HFIAS methodology but adapted it to a one-day recall period (Coates et al. 2007a). From wave 2, the questionnaire asked whether the household experienced nine specific symptoms of food insecurity on the day before the survey, ranging from concerns about food scarcity to going an entire day and night without eating. The score reflects the number of symptoms experienced, ranging from 0 to 9.
Expenditure p.c.	The expenditure module collected information on how much household spent on sixteen different expenditure categories during the four weeks preceding the interview. These categories included: education, healthcare, shocks, food consumed at home and outside, soap and hygiene, transportation, entertainment, clothing, airtime, holidays, charcoal and firewood, electricity, house repairs, new equipment, community contributions and other expenses. Total household expenditure results from the aggregation of these sixteen expenditure categories. It is expressed per capita.
Food stocks (kg and USD)	For seven staple goods—maize, sorghum, wheat flour, rice, pasta, and oil—our questionnaire asked households to report their stock quantities in kilograms. We then aggregated these weights to determine the total food stock in kilograms. Using baseline unit prices, we also estimated the total value of food stocks.
Household debt (USD)	The questionnaire asked households whether they owed debt to one or more food retailers and, if so, to report the amount in Kenyan Shillings. Additionally, households reported debts to family and friends (in goods or money), formal debts to banks, microfinance institutions, or NGOs, and debts to informal savings groups (such as merry-go-rounds). We measure debt at the household level and distinguish between debt from food retailers and total debt from all sources.
Value of assets and savings (KES)	This variable represents the total value of a household's physical assets, livestock, and savings at the time of the interview, expressed in KES, both at the household level and per capita. The questionnaire collected data on ownership of 24 asset types (e.g., televisions, mobile phones, tables, chairs, cooking pots) and 10 types of animals (primarily chickens, ducks, and doves). Savings amounts were self-reported. Asset values were calculated by multiplying the quantity owned by baseline market prices from our price survey.
Sleep quality Likert (1-5)	Sleep quality is assessed using answers to the following question, taken from the World Health Organization's Study on global AGEing and adult health (WHO-SAGE): "Please rate the quality of your sleep last night. Was it very good, good, moderate, poor or very poor?" Answers are coded from 1 "very poor" to 5 "very good".
Good mood score (0-9)	We compute a "mood score" based on nine questions from the WHO-SAGE study. These questions inquire whether participants experienced various moods on the day prior to the survey, such as enjoying activities or feeling worried, rushed, or calm (Kowal et al. 2012). Responses are binary and aggregated to create a score ranging from 0 (poor mood) to 9 (excellent mood) (Taquet et al. 2020).
Happiness Likert (1-5)	Happiness is assessed using answers to the following question, taken from World Values Survey: "All things considered, how satisfied are you with your life as a whole today?" Answers are coded from 1 "very unhappy" to 5 "very happy".
Required Rate of Return	The questionnaire included five trade-off questions in which participants chose between receiving 500 Kenyan Shillings (KES) in two months or a smaller amount in one month. We calculate the Required Rate of Return (RRR) in two steps (Cohen et al. 2020). First, we determine the indifference point—the amount xx at which the respondent switches their choice. Second, we estimate the RRR using the formula: $RRR = log(500/x)$.
Payment now (dummy)	The questionnaire included a binary choice question: receive KES 200 immediately or KES 300 in one month. For logistical reasons, the incentive payment was based on this specific question. Respondents who chose the immediate payment received it on the spot, while those who opted for the delayed payment received it during the next survey wave. We treat responses as a binary variable.
Risk loving score (1-4)	To assess risk preferences, participants answered three questions, each offering a choice between a guaranteed KES 200 and a larger amount (KES 300, 400, or 500) with a 50% probability of payout (Dave et al. 2010). Based on their choices, we classify participants into four risk tolerance groups.
Math test score (0-3)	In each wave, mathematical ability was tested using three arithmetic questions related to common market transactions. One example of question was: "One bag of rice costs KES 2800, and one fish costs KES 250. You buy one bag of rice and three fish. How much do you have to pay?" Due to a coding error in one survey wave, six questions were included instead of three. For consistency, we divided the number of correct answers by 2 in that wave to ensure the variable ranged between 0 and 3. The number of correct responses is used as the outcome variable.
Memory test score (0-36)	Memory was assessed using an adaptation of the Simon memory game, following Carvalho et al. (2016). Participants were shown sequences of colored shape (red O,blue \$, yellow +, or green Δ) of increasing length. They were then asked to replicate the sequence from memory. Each respondent first completed four practice trials with sequences of one to four symbols, followed by eight test trials with sequences of one to eight symbols. The outcome variable is the number of correct responses.
Flanker task score (0-10)	Selective attention and inhibitory control were assessed using the Flanker Task (Eriksen and Eriksen 1974). Participants were shown sequences of five arrows pointing either left or right and had to identify the direction of the central arrow. Each respondent completed 10 practice trials followed by 10 test trials, which were used to construct the outcome variable. The outcome is the number of correct responses.
Psychomotor vigilance task score (0-30)	"Sustained attention was measured using the Psychomotor Vigilance Task (PVT) (Dinges and Powell 1985). Participants were shown sequences of letters (H, O, X, or L) on the tablet screen and instructed to click as quickly as possible when the letter 'O' appeared. Each respondent first completed a practice trial with 20 symbols, followed by a test trial with 30 symbols. This task was introduced in the sixth wave of the panel survey.
Hearts & flowers task score (0-40)	The Hearts and Flowers Task (HFT) was used to assess participants' ability to process congruent and incongruent information (Davidson et al. 2006). The task consists of three parts: (1) Congruent Condition: Participants saw an "O" on either the left or right side of the screen and had to click the corresponding button (10 rounds). (2) Incongruent Condition: Participants saw an "X" on either side of the screen and had to click the opposite button (10 rounds). (3) Mixed Condition: Participants responded to both congruent and incongruent stimuli in a sequence (20 rounds). The outcome variable is the number of correct responses. This task was introduced in the sixth wave of the panel survey.

3.7.2 Analysis of Household Data - Additional Tables

Table A.45: Main specification with no control variables

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-139.5**	-0.0693***	-0.0760***	-0.319***	0.183**	0.588***	-3.495***
	(61.93)	(0.0182)	(0.0262)	(0.0689)	(0.0760)	(0.123)	(0.796)
Time (in months)	-8.638	-0.00193	-0.00969***	0.0229**	-0.00547	-0.0594***	-0.0234
	(8.304)	(0.00276)	(0.00327)	(0.00928)	(0.0123)	(0.0162)	(0.0683)
Aid cut x Time	-9.540	0.0171*	0.0311**	-0.0670*	-0.00493	-0.0409	0.395
	(29.58)	(0.00922)	(0.0130)	(0.0362)	(0.0478)	(0.0624)	(0.329)
N	5557	5557	5557	5557	5539	5557	5526
Kleibergen-Paap F-statistic	718.7	718.7	718.7	718.7	709.3	718.7	428.9
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-2.435	-0.0552	-30.34***	-29.44***	-34.63**
	(1.490)	(1.591)	(8.720)	(9.682)	(14.05)
Time (in months)	0.471***	0.622***	11.35***	11.41***	27.48***
	(0.166)	(0.155)	(1.304)	(1.406)	(2.103)
Aid cut x Time	-1.232*	-0.625	-24.13***	-25.99***	-17.02**
	(0.652)	(0.715)	(3.619)	(4.064)	(7.294)
N	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	530.4	530.4	530.4	530.4	530.4
Baseline mean	24.68	22.99	341.1	365.0	701.7

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.0510	0.00781	0.0224	-0.203***	-0.0493***	0.140***
	(0.0490)	(0.144)	(0.0464)	(0.0286)	(0.0133)	(0.0258)
Time (in months)	0.0406***	0.143***	0.0279***	0.0439***	0.0231***	-0.0532***
	(0.00659)	(0.0202)	(0.00616)	(0.00434)	(0.00244)	(0.00532)
Aid cut x Time	-0.0429*	-0.117	-0.0725***	0.0278**	-0.00564	0.0283***
	(0.0237)	(0.0750)	(0.0208)	(0.0126)	(0.00604)	(0.0106)
N	6112	6112	6107	6067	6112	6112
Kleibergen-Paap F-statistic	889.9	889.9	525.8	522.9	530.4	530.4
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	0.192***	0.202	0.109	-0.0540	-0.0733
	(0.0435)	(0.310)	(0.252)	(0.0843)	(0.529)
Time (in months)	0.00565	0.225***	0.0123	0.0939***	0.439*
	(0.00659)	(0.0409)	(0.116)	(0.0107)	(0.235)
Aid cut x Time	0.0296	0.117	0.0510	-0.0359	0.157
	(0.0210)	(0.159)	(0.137)	(0.0398)	(0.245)
N	6112	6112	3069	6112	3069
Kleibergen-Paap F-statistic	530.4	530.4	200.1	530.4	200.1
Baseline mean	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects. Control variables are excluded from regressions. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.46: Main specification with no fixed effects

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Aid cut	-104.2*	-0.0440**	-0.0491*	-0.229***	0.0974	0.270**	-2.763***	3.109*	5.850***	-21.25**	-19.93**	-51.24***
	(59.28)	(0.0173)	(0.0252)	(0.0711)	(0.0840)	(0.134)	(0.640)	(1.717)	(1.844)	(8.904)	(10.02)	(13.70)
Time (in months)	-23.12***	-0.00605**	-0.00984***	0.0170*	0.0111	-0.0321**	0.0827	0.306**	0.426***	10.68***	11.26***	28.83***
	(7.908)	(0.00263)	(0.00321)	(0.00947)	(0.0127)	(0.0162)	(0.0612)	(0.155)	(0.149)	(1.346)	(1.443)	(2.133)
Aid cut x Time	-1.044	0.0144	0.0190	-0.0765**	-0.00977	-0.00388	-0.0968	-2.822***	-2.493***	-20.78***	-23.34***	-11.49*
	(28.52)	(0.00890)	(0.0129)	(0.0370)	(0.0438)	(0.0642)	(0.301)	(0.698)	(0.790)	(4.031)	(4.491)	(6.853)
N	5566	5566	5566	5566	5548	5566	5602	6188	6188	6188	6188	6188
Kleibergen-Paap F-statistic	928.8	928.8	928.8	928.8	915.7	928.8	590.9	884.6	884.6	884.6	884.6	884.6
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05	24.68	22.99	341.1	365.0	701.7

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0237	0.104	0.0182	-0.168***	-0.0571***	0.154***	0.178***	0.223	0.0240	-0.0726	-0.631
	(0.0485)	(0.149)	(0.0451)	(0.0288)	(0.0131)	(0.0298)	(0.0450)	(0.302)	(0.215)	(0.0773)	(0.417)
Time (in months)	0.0342***	0.124***	0.0226***	0.0431***	0.0229***	-0.0589***	0.00484	0.211***	0.0456	0.104***	0.631***
	(0.00630)	(0.0201)	(0.00611)	(0.00436)	(0.00235)	(0.00549)	(0.00648)	(0.0406)	(0.0992)	(0.0106)	(0.187)
Aid cut x Time	-0.0414*	-0.131*	-0.0691***	0.0196	-0.000657	0.0234*	0.0432**	0.123	-0.0459	-0.0588	-0.0319
	(0.0238)	(0.0749)	(0.0209)	(0.0123)	(0.00591)	(0.0121)	(0.0216)	(0.158)	(0.124)	(0.0373)	(0.226)
N	6188	6188	6183	6132	6188	6188	6188	6188	3072	6188	3072
Kleibergen-Paap F-statistic	1271.8	1271.8	878.7	866.1	884.6	884.6	884.6	884.6	320.5	884.6	320.5
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include control variables, as specified in Appendix Table A.43, and no fixed effects. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.47: Main specification with no attrited or replacement households

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Aid cut	-136.4**	-0.0684***	-0.0748***	-0.325***	0.181**	0.586***	-3.234***	-1.857	0.242	-32.52***	-31.52***	-37.42***
	(61.45)	(0.0177)	(0.0263)	(0.0688)	(0.0755)	(0.123)	(0.730)	(1.546)	(1.638)	(8.812)	(9.664)	(14.40)
Time (in months)	-10.93	-0.00207	-0.00878***	0.0222**	-0.00118	-0.0555***	0.0777	0.525***	0.691***	11.30***	11.53***	26.99***
	(8.311)	(0.00275)	(0.00333)	(0.00965)	(0.0126)	(0.0162)	(0.0633)	(0.170)	(0.165)	(1.324)	(1.434)	(2.097)
Aid cut x Time	-8.811	0.0138	0.0289**	-0.0725**	-0.0109	-0.0420	0.152	-1.305**	-0.808	-24.34***	-25.89***	-14.40*
	(29.70)	(0.00930)	(0.0131)	(0.0363)	(0.0466)	(0.0624)	(0.310)	(0.655)	(0.713)	(3.626)	(4.053)	(7.449)
N	5557	5557	5557	5557	5539	5557	5526	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	692.7	692.7	692.7	692.7	683.8	692.7	424.5	521.9	521.9	521.9	521.9	521.9
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05	24.68	22.99	341.1	365.0	701.7

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0588	0.00960	0.0154	-0.202***	-0.0480***	0.141***	0.191***	0.221	0.109	-0.0415	0.0225
	(0.0494)	(0.145)	(0.0469)	(0.0284)	(0.0134)	(0.0258)	(0.0435)	(0.310)	(0.251)	(0.0846)	(0.529)
Time (in months)	0.0376***	0.139***	0.0263***	0.0440***	0.0234***	-0.0533***	0.00450	0.220***	-0.00591	0.0930***	0.342
	(0.00664)	(0.0205)	(0.00627)	(0.00437)	(0.00247)	(0.00543)	(0.00661)	(0.0413)	(0.117)	(0.0107)	(0.238)
Aid cut x Time	-0.0437*	-0.125*	-0.0729***	0.0236*	-0.00475	0.0306***	0.0313	0.106	0.0855	-0.0317	0.277
	(0.0238)	(0.0751)	(0.0211)	(0.0127)	(0.00611)	(0.0106)	(0.0212)	(0.161)	(0.137)	(0.0402)	(0.252)
N	6112	6112	6107	6067	6112	6112	6112	6112	3069	6112	3069
Kleibergen-Paap F-statistic	834.7	834.7	517.2	513.8	521.9	521.9	521.9	521.9	195.9	521.9	195.9
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1) after excluding attrited and replacement households. Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.48: Main specification without excluding observations on or around the aid cut

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Aid cut	-139.0**	-0.0693***	-0.0807***	-0.330***	0.201**	0.600***	-1.472***	-2.664*	-0.740	-34.07***	-33.32***	-35.85**
	(60.35)	(0.0173)	(0.0256)	(0.0679)	(0.0841)	(0.122)	(0.420)	(1.520)	(1.602)	(8.684)	(9.566)	(14.59)
Time (in months)	-10.65	-0.00201	-0.00872***	0.0224**	-0.00170	-0.0563***	0.0814	0.522***	0.687***	11.29***	11.51***	27.01***
	(8.293)	(0.00274)	(0.00333)	(0.00966)	(0.0126)	(0.0163)	(0.0621)	(0.170)	(0.165)	(1.324)	(1.434)	(2.097)
Aid cut x Time	-8.539	0.0139	0.0318**	-0.0715**	-0.0213	-0.0462	-0.677***	-0.891	-0.303	-23.48***	-24.88***	-15.29**
	(28.88)	(0.00910)	(0.0126)	(0.0352)	(0.0498)	(0.0606)	(0.204)	(0.647)	(0.709)	(3.552)	(3.972)	(7.513)
N	5589	5589	5589	5589	5571	5589	6144	6144	6144	6144	6144	6144
Kleibergen-Paap F-statistic	706.1	706.1	706.1	706.1	696.8	706.1	528.6	528.6	528.6	528.6	528.6	528.6
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05	24.68	22.99	341.1	365.0	701.7

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0565	-0.00209	0.00660	-0.208***	-0.0477***	0.147***	0.180***	0.157	0.106	-0.0340	-0.0307
	(0.0488)	(0.142)	(0.0466)	(0.0283)	(0.0134)	(0.0256)	(0.0425)	(0.306)	(0.253)	(0.0852)	(0.528)
Time (in months)	0.0373***	0.139***	0.0262***	0.0440***	0.0234***	-0.0533***	0.00443	0.219***	-0.00546	0.0929***	0.349
	(0.00665)	(0.0205)	(0.00627)	(0.00437)	(0.00247)	(0.00543)	(0.00660)	(0.0412)	(0.117)	(0.0107)	(0.238)
Aid cut x Time	-0.0431*	-0.119	-0.0678***	0.0269**	-0.00507	0.0274***	0.0372*	0.146	0.0916	-0.0352	0.297
	(0.0233)	(0.0732)	(0.0208)	(0.0126)	(0.00591)	(0.0104)	(0.0206)	(0.159)	(0.137)	(0.0400)	(0.251)
N	6144	6144	6139	6099	6144	6144	6144	6144	3101	6144	3101
Kleibergen-Paap F-statistic	845.9	845.9	523.7	520.1	528.6	528.6	528.6	528.6	192.1	528.6	192.1
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Observations falling on the aid-cut date are included in this regression. For expenditures, observations related to the four weeks following the cut are included. Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.49: Main specification with no household regularly involved in agriculture

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Aid cut	-125.2*	-0.0619***	-0.0783***	-0.311***	0.190**	0.608***	-3.146***	-2.069	0.173	-30.57***	-29.13***	-39.62***
	(64.50)	(0.0186)	(0.0272)	(0.0717)	(0.0804)	(0.129)	(0.761)	(1.615)	(1.719)	(9.158)	(10.09)	(14.93)
Time (in months)	-11.88	-0.00278	-0.00850**	0.0239**	-0.00208	-0.0564***	0.0508	0.530***	0.709***	10.79***	10.97***	27.05***
	(8.775)	(0.00285)	(0.00350)	(0.00997)	(0.0135)	(0.0171)	(0.0653)	(0.178)	(0.173)	(1.341)	(1.462)	(2.194)
Aid cut x Time	-3.669	0.0159*	0.0327**	-0.0717*	-0.0162	-0.0558	0.143	-1.301*	-0.790	-23.42***	-24.87***	-15.15*
	(31.00)	(0.00965)	(0.0135)	(0.0373)	(0.0497)	(0.0652)	(0.318)	(0.681)	(0.746)	(3.773)	(4.215)	(7.884)
N	5172	5172	5172	5172	5154	5172	5146	5693	5693	5693	5693	5693
Kleibergen-Paap F-statistic	648.7	648.7	648.7	648.7	639.9	648.7	400.8	475.0	475.0	475.0	475.0	475.0
Baseline mean	2027.7	0.693	0.561	4.841	1.479	4.351	14.82	25.01	23.42	328.9	353.0	708.7

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0693	-0.0246	0.0184	-0.217***	-0.0576***	0.140***	0.190***	0.191	0.124	-0.0701	0.0636
	(0.0514)	(0.150)	(0.0491)	(0.0297)	(0.0135)	(0.0257)	(0.0449)	(0.325)	(0.266)	(0.0880)	(0.554)
Time (in months)	0.0407***	0.146***	0.0286***	0.0440***	0.0252***	-0.0546***	0.00333	0.225***	-0.0262	0.0977***	0.200
	(0.00693)	(0.0215)	(0.00653)	(0.00456)	(0.00251)	(0.00560)	(0.00683)	(0.0429)	(0.123)	(0.0113)	(0.255)
Aid cut x Time	-0.0497**	-0.113	-0.0787***	0.0268**	-0.00677	0.0335***	0.0344	0.128	0.0920	-0.0242	0.453*
	(0.0247)	(0.0781)	(0.0221)	(0.0132)	(0.00624)	(0.0107)	(0.0221)	(0.166)	(0.145)	(0.0423)	(0.271)
N	5691	5691	5688	5656	5693	5693	5693	5693	2857	5693	2857
Kleibergen-Paap F-statistic	770.7	770.7	470.6	470.0	475.0	475.0	475.0	475.0	177.7	475.0	177.7
Baseline mean	3.049	4.906	2.751	1.789	0.864	1.436	1.950	18.65	28.28	8.475	34.44

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1) after excluding households regularly involved in agriculture (defined as reporting agriculture involvement in more than 3 waves). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.50: Main specification with reduced form

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-129.9**	-0.0649***	-0.0708***	-0.308***	0.171**	0.552***	-3.151***
	(57.80)	(0.0166)	(0.0248)	(0.0648)	(0.0714)	(0.116)	(0.681)
Time (in months)	-10.89	-0.00202	-0.00877***	0.0224**	-0.00114	-0.0557***	0.0779
	(8.344)	(0.00276)	(0.00334)	(0.00968)	(0.0127)	(0.0162)	(0.0629)
Aid cut x Time	-11.74	0.0119	0.0268**	-0.0816**	-0.00609	-0.0237	0.113
	(28.34)	(0.00893)	(0.0124)	(0.0347)	(0.0451)	(0.0595)	(0.290)
N	5557	5557	5557	5557	5539	5557	5526
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-2.791*	-0.800	-32.78***	-32.10***	-36.98***
	(1.533)	(1.620)	(8.498)	(9.356)	(13.53)
Time (in months)	0.595***	0.769***	11.38***	11.63***	27.08***
	(0.170)	(0.165)	(1.319)	(1.430)	(2.095)
Aid cut x Time	-1.374**	-0.882	-24.64***	-26.14***	-14.97**
	(0.651)	(0.709)	(3.533)	(3.960)	(7.209)
N	6112	6112	6112	6112	6112
Baseline mean	24.68	22.99	341.1	365.0	701.7

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.0530	0.0126	0.0190	-0.199***	-0.0479***	0.139***
	(0.0462)	(0.136)	(0.0458)	(0.0278)	(0.0131)	(0.0251)
Time (in months)	0.0374***	0.138***	0.0259***	0.0441***	0.0235***	-0.0532***
	(0.00665)	(0.0205)	(0.00626)	(0.00435)	(0.00246)	(0.00541)
Aid cut x Time	-0.0464**	-0.126*	-0.0718***	0.0218*	-0.00533	0.0308***
	(0.0229)	(0.0724)	(0.0207)	(0.0124)	(0.00600)	(0.0104)
N	6112	6112	6107	6067	6112	6112
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	0.190***	0.242	0.147	-0.0378	-0.123
	(0.0426)	(0.303)	(0.232)	(0.0829)	(0.488)
Time (in months)	0.00439	0.220***	-0.0307	0.0928***	0.392*
	(0.00656)	(0.0412)	(0.110)	(0.0107)	(0.223)
Aid cut x Time	0.0324	0.101	0.103	-0.0318	0.241
	(0.0209)	(0.159)	(0.135)	(0.0394)	(0.245)
N	6112	6112	3069	6112	3069
Baseline mean	1.935	18.66	28.28	8.471	34.38

Notes: Reduced-form regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are replaced by their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.51: Main specification with OLS

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Aid cut	-89.61	-0.0491***	-0.0686***	-0.246***	0.118*	0.509***	-3.221***	-1.972	0.0306	-33.05***	-32.11***	-35.47***
	(56.78)	(0.0167)	(0.0242)	(0.0644)	(0.0706)	(0.113)	(0.700)	(1.528)	(1.614)	(8.571)	(9.495)	(12.87)
Time (in months)	-14.69*	-0.00334	-0.00954***	0.0180*	0.00402	-0.0503***	0.0822	0.530***	0.694***	11.32***	11.58***	27.14***
	(8.204)	(0.00272)	(0.00322)	(0.00940)	(0.0124)	(0.0157)	(0.0628)	(0.169)	(0.163)	(1.323)	(1.431)	(2.098)
Aid cut x Time	-17.60	0.00882	0.0277**	-0.0985***	0.00437	-0.0222	0.130	-1.335**	-0.782	-24.31***	-25.92***	-15.84**
	(29.26)	(0.00921)	(0.0128)	(0.0358)	(0.0466)	(0.0605)	(0.298)	(0.647)	(0.704)	(3.496)	(3.931)	(7.168)
N	5557	5557	5557	5557	5539	5557	5526	6112	6112	6112	6112	6112
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05	24.68	22.99	341.1	365.0	701.7

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0463	-0.0128	0.0187	-0.197***	-0.0470***	0.135***	0.201***	0.265	0.171	-0.0299	0.0347
	(0.0467)	(0.138)	(0.0462)	(0.0279)	(0.0132)	(0.0249)	(0.0433)	(0.304)	(0.233)	(0.0821)	(0.490)
Time (in months)	0.0370***	0.140***	0.0253***	0.0440***	0.0234***	-0.0530***	0.00380	0.217***	-0.0372	0.0909***	0.344
	(0.00655)	(0.0203)	(0.00627)	(0.00435)	(0.00246)	(0.00539)	(0.00656)	(0.0412)	(0.113)	(0.0107)	(0.225)
Aid cut x Time	-0.0483**	-0.125*	-0.0700***	0.0216*	-0.00528	0.0312***	0.0306	0.0932	0.113	-0.0274	0.310
	(0.0235)	(0.0737)	(0.0208)	(0.0122)	(0.00594)	(0.0100)	(0.0210)	(0.159)	(0.136)	(0.0395)	(0.248)
N	6112	6112	6107	6067	6112	6112	6112	6112	3069	6112	3069
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: OLS regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables are constructed using the actual interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.52: Main specification with robust standard errors

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	HH expenses (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-136.4**	-0.0684***	-0.0748***	-0.325***	0.181**	0.586***	-3.234***
	(58.45)	(0.0185)	(0.0252)	(0.0691)	(0.0801)	(0.117)	(0.777)
Time (in months)	-10.93	-0.00207	-0.00878***	0.0222**	-0.00118	-0.0555***	0.0777
	(7.795)	(0.00262)	(0.00327)	(0.00907)	(0.0128)	(0.0148)	(0.0582)
Aid cut x Time	-8.811	0.0138	0.0289**	-0.0725**	-0.0109	-0.0420	0.152
	(29.43)	(0.00932)	(0.0126)	(0.0359)	(0.0452)	(0.0586)	(0.359)
N	5557	5557	5557	5557	5539	5557	5526
Kleibergen-Paap F-statistic	895.6	895.6	895.6	895.6	882.9	895.6	484.5
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-1.857	0.242	-32.52***	-31.52***	-37.42**
	(1.375)	(1.390)	(8.753)	(9.796)	(15.03)
Time (in months)	0.525***	0.691***	11.30***	11.53***	26.99***
	(0.168)	(0.159)	(1.120)	(1.236)	(1.680)
Aid cut x Time	-1.305**	-0.808	-24.34***	-25.89***	-14.40*
	(0.646)	(0.676)	(4.036)	(4.696)	(7.900)
N	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	627.4	627.4	627.4	627.4	627.4
Baseline mean	24.68	22.99	341.1	365.0	701.7

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.0588	0.00960	0.0154	-0.202***	-0.0480***	0.141***
	(0.0504)	(0.155)	(0.0478)	(0.0305)	(0.0143)	(0.0268)
Time (in months)	0.0376***	0.139***	0.0263***	0.0440***	0.0234***	-0.0533***
	(0.00581)	(0.0177)	(0.00555)	(0.00387)	(0.00207)	(0.00460)
Aid cut x Time	-0.0437*	-0.125	-0.0729***	0.0236	-0.00475	0.0306**
	(0.0251)	(0.0790)	(0.0246)	(0.0151)	(0.00685)	(0.0124)
N	6112	6112	6107	6067	6112	6112
Kleibergen-Paap F-statistic	1032.6	1032.6	625.4	616.6	627.4	627.4
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score(0-36)	Psychomotor vigilance task score(0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	0.191***	0.221	0.109	-0.0415	0.0225
	(0.0462)	(0.332)	(0.261)	(0.0858)	(0.533)
Time (in months)	0.00450	0.220***	-0.00591	0.0930***	0.342
	(0.00561)	(0.0351)	(0.125)	(0.0104)	(0.243)
Aid cut x Time	0.0313	0.106	0.0855	-0.0317	0.277
	(0.0234)	(0.173)	(0.143)	(0.0428)	(0.263)
N	6112	6112	3069	6112	3069
Kleibergen-Paap F-statistic	627.4	627.4	230.8	627.4	230.8
Baseline mean	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1). Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.53: Table 8, including observations from wave 1

	Calories p.c.	Food expenses p.c.	> 2 meals	Dietary Diversity	days/ 2100 kal	HFIAS
	(1)	(2)	(3)	(4)	(5)	(6)
Aid cut	-273.3***	-0.0986***	-0.0719***	-0.368***	0.329***	0.586***
	(60.60)	(0.0176)	(0.0259)	(0.0670)	(0.0735)	(0.123)
Time (in months)	40.00***	0.0103***	-0.0103***	0.0388***	-0.0610***	-0.0555***
	(6.383)	(0.00223)	(0.00280)	(0.00776)	(0.0106)	(0.0162)
Aid cut x Time	-52.36*	0.00134	0.0276**	-0.0832**	0.0494	-0.0420
	(29.13)	(0.00916)	(0.0128)	(0.0357)	(0.0461)	(0.0624)
N	6112	6112	6112	6112	6092	5557
Kleibergen-Paap F-statistic	834.7	834.7	834.7	834.7	823.5	692.7
Baseline mean	1933.8	0.672	0.582	4.808	1.584	4.315

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1) for food and expenses indicators only. As compared to the main specification, this specification includes observations from wave 1. Outcome variables are defined in Appendix Table A.44. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.54: Local Regression Discontinuity Design

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-152.2**	-0.0474***	-0.0567**	-0.338***	0.187**	0.474***	-1.723***
	(59.61)	(0.0175)	(0.0257)	(0.0692)	(0.0853)	(0.117)	(0.454)
N	1832	1832	1832	1832	1826	1832	1832
Kleibergen-Paap F-statistic	18292.3	18292.3	18292.3	18292.3	18297.3	18292.3	24801.5
Baseline mean	1969.8	0.667	0.524	4.854	1.539	4.301	14.77

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-4.826***	-2.604*	-15.45**	-13.07*	1.662
	(1.467)	(1.473)	(6.726)	(7.478)	(9.918)
N	1832	1832	1832	1832	1832
Kleibergen-Paap F-statistic	24801.5	24801.5	24801.5	24801.5	24801.5
Baseline mean	22.61	21.37	364.2	385.7	788.1

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.0184	0.117	-0.00689	-0.0505**	-0.00649	0.00128
	(0.0477)	(0.140)	(0.0487)	(0.0238)	(0.0132)	(0.0221)
N	1832	1832	1832	1831	1832	1832
Kleibergen-Paap F-statistic	18292.3	18292.3	24801.5	24666.2	24801.5	24801.5
Baseline mean	3.184	5.492	2.850	1.877	0.931	1.298

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	0.382***	1.117***	0.255*	0.136*	0.763**
	(0.0539)	(0.313)	(0.140)	(0.0806)	(0.338)
N	1832	1832	1826	1832	1826
Kleibergen-Paap F-statistic	24801.5	24801.5	24735.0	24801.5	24735.0
Baseline mean	1.900	19.38	28.30	8.869	34.80

Notes: Instrumental Variable (IV) regressions with waves 7, 8, and 9 only. Outcome variables are defined in Appendix Table A.44. All regressions include household fixed effects. The aid cut variable is instrumented by a binary variable identifying whether the randomly assigned interview date is after the aid cut (=1) or not (=0). One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.55: Main specification with cutoff date on June 16th

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	HH expenses (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-52.79	-0.0391**	-0.0500**	-0.207***	0.199**	0.549***	-0.280
	(60.16)	(0.0185)	(0.0245)	(0.0677)	(0.0935)	(0.117)	(0.494)
Time (in months)	-11.37	-0.00191	-0.00870**	0.0298***	-0.0112	-0.0784***	0.0705
	(9.163)	(0.00313)	(0.00371)	(0.0101)	(0.0121)	(0.0177)	(0.0681)
Aid cut x Time	-38.59	-0.00106	0.0131	-0.121***	0.00945	0.0374	-0.944***
	(25.87)	(0.00823)	(0.0107)	(0.0315)	(0.0486)	(0.0518)	(0.201)
N	5557	5557	5557	5557	5539	5557	5526
Kleibergen-Paap F-statistic	837.0	837.0	837.0	837.0	825.8	837.0	464.1
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-2.364	-0.640	-35.24***	-35.88***	-36.57***
	(1.591)	(1.661)	(8.447)	(9.453)	(12.41)
Time (in months)	0.652***	0.767***	13.12***	13.47***	28.39***
	(0.195)	(0.188)	(1.382)	(1.502)	(2.144)
Aid cut x Time	-1.178**	-0.505	-23.04***	-23.91***	-15.38**
	(0.584)	(0.626)	(3.353)	(3.715)	(6.775)
N	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	662.1	662.1	662.1	662.1	662.1
Baseline mean	24.68	22.99	341.1	365.0	701.7

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(1)	(2)	(3)	(4)	(5)	(6)
Aid cut	-0.0177	0.0750	0.0601	-0.180***	-0.0388***	0.0947***
	(0.0492)	(0.150)	(0.0482)	(0.0278)	(0.0121)	(0.0233)
Time (in months)	0.0380***	0.137***	0.0251***	0.0478***	0.0243***	-0.0553***
	(0.00705)	(0.0219)	(0.00667)	(0.00463)	(0.00268)	(0.00581)
Aid cut x Time	-0.0511**	-0.118*	-0.0712***	-0.000644	-0.00963*	0.0474***
	(0.0210)	(0.0658)	(0.0186)	(0.0115)	(0.00571)	(0.00980)
N	6112	6112	6107	6067	6112	6112
Kleibergen-Paap F-statistic	1044.0	1044.0	656.9	651.4	662.1	662.1
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score(0-36)	Psychomotor vigilance task score(0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(7)	(8)	(9)	(10)	(11)
Aid cut	0.252***	0.341	-0.0387	0.283	-1.215**
	(0.0448)	(0.300)	(0.0827)	(0.255)	(0.531)
Time (in months)	-0.00581	0.203***	0.0948***	-0.156	1.012***
	(0.00722)	(0.0429)	(0.0118)	(0.180)	(0.341)
Aid cut x Time	0.0293	0.0840	-0.0308	0.224	-0.309
	(0.0196)	(0.141)	(0.0346)	(0.188)	(0.345)
N	6112	6112	6112	3069	3069
Kleibergen-Paap F-statistic	662.1	662.1	662.1	274.5	274.5
Baseline mean	1.935	18.66	8.471	28.28	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1) and re-estimating independent variables with 16th of June 2023 as a cut-off date, corresponding to the date in which the aid cut was first announced to market coordinators in the camp. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.56: Main specification with cutoff date on June 24th

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	HH expenses (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-110.7*	-0.0584***	-0.0470*	-0.277***	0.277***	0.607***	-1.046***
	(57.70)	(0.0175)	(0.0246)	(0.0667)	(0.0914)	(0.116)	(0.398)
Time (in months)	-8.617	-0.00103	-0.00933***	0.0295***	-0.0140	-0.0744***	0.0927
	(8.736)	(0.00294)	(0.00361)	(0.00972)	(0.0116)	(0.0173)	(0.0646)
Aid cut x Time	-25.09	0.00487	0.0149	-0.104***	-0.0158	0.00562	-0.809***
	(26.91)	(0.00863)	(0.0113)	(0.0334)	(0.0515)	(0.0541)	(0.186)
N	5557	5557	5557	5557	5539	5557	6112
Kleibergen-Paap F-statistic	650.3	650.3	650.3	650.3	641.6	650.3	536.3
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-2.599	-0.807	-36.49***	-36.43***	-33.10**
	(1.629)	(1.709)	(8.529)	(9.347)	(13.54)
Time (in months)	0.628***	0.763***	12.56***	12.84***	27.67***
	(0.189)	(0.183)	(1.386)	(1.494)	(2.127)
Aid cut x Time	-1.134*	-0.478	-23.34***	-24.44***	-16.41**
	(0.627)	(0.683)	(3.400)	(3.810)	(6.971)
N	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	536.3	536.3	536.3	536.3	536.3
Baseline mean	24.68	22.99	341.1	365.0	701.7

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(1)	(2)	(3)	(4)	(5)	(6)
Aid cut	-0.0196	0.146	0.0397	-0.198***	-0.0352***	0.121***
	(0.0471)	(0.147)	(0.0483)	(0.0278)	(0.0125)	(0.0245)
Time (in months)	0.0371***	0.132***	0.0256***	0.0475***	0.0236***	-0.0553***
	(0.00695)	(0.0215)	(0.00659)	(0.00457)	(0.00263)	(0.00569)
Aid cut x Time	-0.0535**	-0.147**	-0.0719***	0.00865	-0.0103*	0.0410***
	(0.0215)	(0.0680)	(0.0194)	(0.0117)	(0.00566)	(0.0102)
N	6112	6112	6107	6067	6112	6112
Kleibergen-Paap F-statistic	809.8	809.8	531.8	527.9	536.3	536.3
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score(0-36)	Psychomotor vigilance task score(0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(7)	(8)	(9)	(10)	(11)
Aid cut	0.234***	0.333	-0.0297	0.108	-1.314**
	(0.0443)	(0.304)	(0.0823)	(0.276)	(0.567)
Time (in months)	-0.00246	0.207***	0.0935***	-0.0309	0.954***
	(0.00703)	(0.0428)	(0.0115)	(0.157)	(0.301)
Aid cut x Time	0.0298	0.0831	-0.0342	0.117	-0.209
	(0.0201)	(0.147)	(0.0358)	(0.165)	(0.304)
N	6112	6112	6112	3069	3069
Kleibergen-Paap F-statistic	536.3	536.3	536.3	241.1	241.1
Baseline mean	1.935	18.66	8.471	28.28	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1) and re-estimating independent variables with 24th of June 2023 as a cut-off date, corresponding to the mid-point between the first announcement of the cut and its implementation date. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.57: Placebo test with cutoff date equal to 6 February 2023

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-108.3	-0.0635**	-0.0541*	0.00933	-0.0521	-0.164	-0.588
	(78.76)	(0.0253)	(0.0312)	(0.0840)	(0.0914)	(0.148)	(0.560)
Time (in months)	64.65*	0.0319***	0.0144	0.0571	-0.0340	-0.105	0.375*
	(34.45)	(0.0118)	(0.0144)	(0.0390)	(0.0478)	(0.0650)	(0.195)
Aid cut x Time	-81.95**	-0.0348***	-0.0205	-0.0624	0.0614	0.134*	-0.332
	(36.52)	(0.0125)	(0.0154)	(0.0418)	(0.0532)	(0.0709)	(0.221)
N	4075	4075	4075	4075	4062	4075	4630
Kleibergen-Paap F-statistic	568.0	568.0	568.0	568.0	558.6	568.0	476.7
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-3.160*	-3.366**	21.20***	24.33***	3.684
	(1.695)	(1.561)	(8.197)	(9.022)	(14.90)
Time (in months)	2.289***	2.014***	17.06***	18.13***	46.04***
	(0.511)	(0.462)	(3.115)	(3.512)	(5.323)
Aid cut x Time	-2.181***	-1.318**	-17.81***	-19.71***	-35.34***
	(0.576)	(0.525)	(4.119)	(4.726)	(7.434)
N	4630	4630	4630	4630	4630
Kleibergen-Paap F-statistic	476.7	476.7	476.7	476.7	476.7
Baseline mean	24.68	22.99	341.1	365.0	701.7

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.0550	0.139	0.0524	0.0108	-0.0389**	0.0807**
	(0.0640)	(0.192)	(0.0563)	(0.0347)	(0.0164)	(0.0375)
Time (in months)	0.0448**	0.0241	-0.0190	0.0717***	0.0455***	-0.118***
	(0.0202)	(0.0610)	(0.0187)	(0.0126)	(0.00719)	(0.0162)
Aid cut x Time	0.00127	0.161**	0.0617***	-0.0549***	-0.0257***	0.0885***
	(0.0232)	(0.0676)	(0.0213)	(0.0148)	(0.00824)	(0.0173)
N	4630	4630	4625	4587	4630	4630
Kleibergen-Paap F-statistic	737.1	737.1	471.5	471.0	476.7	476.7
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	0.0995*	-0.118		-0.139	
	(0.0517)	(0.339)		(0.109)	
Time (in months)	-0.0125	0.305***		0.137***	
	(0.0170)	(0.112)		(0.0360)	
Aid cut x Time	0.00739	-0.111		-0.0359	
	(0.0210)	(0.147)		(0.0421)	
N	4630	4630		4630	
Kleibergen-Paap F-statistic	476.7	476.7		476.7	
Baseline mean	1.935	18.66		8.471	

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1) and re-estimating independent variables with 6 of February 2023 as a placebo cut-off date. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.58: Placebo test with cutoff date equal to 6 March 2023

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-166.1**	-0.0201	-0.0359	-0.111	0.0602	0.0279	-1.454**
	(75.56)	(0.0272)	(0.0322)	(0.0902)	(0.0850)	(0.142)	(0.589)
Time (in months)	40.80*	0.00948	0.000125	0.0666**	-0.0529	-0.120***	0.374**
	(23.82)	(0.00846)	(0.0101)	(0.0280)	(0.0326)	(0.0448)	(0.159)
Aid cut x Time	-37.49	-0.0165	-0.00345	-0.0496	0.0794	0.128**	-0.0862
	(31.89)	(0.0107)	(0.0138)	(0.0379)	(0.0483)	(0.0647)	(0.226)
N	4003	4003	4003	4003	3990	4003	4558
Kleibergen-Paap F-statistic	627.5	627.5	627.5	627.5	618.7	627.5	443.4
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.05

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-2.768*	-1.388	17.45	20.63*	-26.27**
	(1.443)	(1.330)	(10.74)	(11.22)	(13.11)
Time (in months)	1.664***	1.352***	16.98***	18.44***	45.31***
	(0.357)	(0.334)	(2.516)	(2.930)	(4.111)
Aid cut x Time	-1.618**	-1.047*	-22.70***	-26.78***	-35.18***
	(0.646)	(0.608)	(5.081)	(5.645)	(8.364)
N	4558	4558	4558	4558	4558
Kleibergen-Paap F-statistic	443.4	443.4	443.4	443.4	443.4
Baseline mean	24.68	22.99	341.1	365.0	701.7

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	-0.0213	0.124	0.0704	-0.0124	-0.0941***	0.104***
	(0.0648)	(0.197)	(0.0570)	(0.0334)	(0.0172)	(0.0336)
Time (in months)	0.0327**	0.0596	-0.00955	0.0659***	0.0460***	-0.101***
	(0.0151)	(0.0457)	(0.0146)	(0.0101)	(0.00578)	(0.0129)
Aid cut x Time	0.0204	0.143*	0.0573***	-0.0486***	-0.0143*	0.0711***
	(0.0247)	(0.0748)	(0.0220)	(0.0147)	(0.00837)	(0.0164)
N	4558	4558	4553	4515	4558	4558
Kleibergen-Paap F-statistic	769.5	769.5	439.2	438.0	443.4	443.4
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0970*	-0.451		-0.149	
	(0.0530)	(0.364)		(0.103)	
Time (in months)	0.0214	0.298***		0.119***	
	(0.0137)	(0.0883)		(0.0274)	
Aid cut x Time	0.00539	0.0282		0.00115	
	(0.0234)	(0.146)		(0.0420)	
N	4558	4558		4558	
Kleibergen-Paap F-statistic	443.4	443.4		443.4	
Baseline mean	1.935	18.66		8.471	

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1) and re-estimating independent variables with 6 of March 2023 as a placebo cut-off date. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.59: Heterogeneous treatment effects with camp dummy

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Aid cut	-182.8***	-0.0819***	-0.0829**	-0.416***	0.147	0.699***	-3.865***	-4.925***	-4.684***	-37.88***	-35.74**	-29.06
	(66.15)	(0.0231)	(0.0352)	(0.0903)	(0.0990)	(0.156)	(0.932)	(1.425)	(1.354)	(14.15)	(14.64)	(21.53)
Time (in months)	5.462	-0.00152	-0.00229	0.0212*	-0.00993	-0.0626***	0.244***	0.594***	0.648***	16.70***	16.30***	21.97***
	(8.306)	(0.00353)	(0.00424)	(0.0119)	(0.0166)	(0.0205)	(0.0843)	(0.168)	(0.151)	(2.134)	(2.249)	(2.405)
Aid cut x Time	-18.23	0.00132	0.00771	-0.0781	0.0307	-0.0334	0.0288	0.00470	-0.0165	-34.23***	-32.41***	3.270
	(34.41)	(0.0117)	(0.0178)	(0.0475)	(0.0531)	(0.0733)	(0.423)	(0.752)	(0.687)	(5.219)	(5.416)	(10.37)
Aid cut x Kakuma	99.86	0.0336	0.0163	0.222	0.0872	-0.272	1.635	7.177**	11.83***	10.35	7.563	-19.00
	(125.6)	(0.0344)	(0.0489)	(0.141)	(0.162)	(0.244)	(1.136)	(3.238)	(3.501)	(15.57)	(17.97)	(28.77)
Time (in months) x Kakuma	-39.25**	-0.00147	-0.0154**	0.00133	0.0203	0.0181	-0.397***	-0.181	0.0342	-12.73***	-11.20***	12.07***
	(16.97)	(0.00530)	(0.00622)	(0.0190)	(0.0236)	(0.0316)	(0.123)	(0.341)	(0.319)	(2.419)	(2.653)	(4.443)
Aid cut x Time x Kakuma	22.72	0.0327*	0.0526**	0.0227	-0.100	-0.0296	0.261	-3.065**	-1.586	24.88***	16.32**	-44.84***
	(59.60)	(0.0178)	(0.0247)	(0.0709)	(0.0922)	(0.126)	(0.514)	(1.276)	(1.480)	(5.952)	(7.403)	(11.57)
N	5557	5557	5557	5557	5539	5557	5526	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	388.0	388.0	388.0	388.0	383.3	388.0	268.6	313.9	313.9	313.9	313.9	313.9
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.03	24.62	22.93	340.9	364.9	702.6

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	0.00311	-0.220	0.0716	-0.208***	-0.0344*	0.114***	0.197***	0.404	0.0687	-0.0489	-0.506
	(0.0646)	(0.166)	(0.0594)	(0.0384)	(0.0198)	(0.0363)	(0.0615)	(0.389)	(0.324)	(0.105)	(0.735)
Time (in months)	0.00940	0.107***	0.00161	0.0342***	0.0260***	-0.0544***	0.0214**	0.0660	0.0463	0.114***	0.621**
	(0.00830)	(0.0231)	(0.00775)	(0.00578)	(0.00339)	(0.00717)	(0.00865)	(0.0468)	(0.140)	(0.0136)	(0.309)
Aid cut x Time	-0.0552*	-0.134	-0.117***	0.0290*	-0.00402	0.0267*	-0.0181	0.165	-0.0333	-0.0815	0.231
	(0.0315)	(0.0851)	(0.0273)	(0.0167)	(0.00842)	(0.0148)	(0.0292)	(0.201)	(0.165)	(0.0526)	(0.336)
Aid cut x Kakuma	-0.126	0.588**	-0.111	0.0219	-0.0351	0.0682	-0.0205	-0.336	0.0940	0.00950	1.166
	(0.0944)	(0.291)	(0.0920)	(0.0561)	(0.0253)	(0.0508)	(0.0851)	(0.645)	(0.454)	(0.160)	(0.832)
Time (in months) x Kakuma	0.0657***	0.0679*	0.0569***	0.0220**	-0.00575	0.00200	-0.0398***	0.359***	-0.126	-0.0492**	-0.629*
	(0.0123)	(0.0401)	(0.0118)	(0.00858)	(0.00450)	(0.0102)	(0.0128)	(0.0861)	(0.201)	(0.0210)	(0.352)
Aid cut x Time x Kakuma	0.0279	0.0428	0.110***	-0.0107	-0.00327	0.0122	0.123***	-0.147	0.309	0.124*	0.0631
	(0.0470)	(0.154)	(0.0417)	(0.0233)	(0.0113)	(0.0192)	(0.0410)	(0.316)	(0.257)	(0.0738)	(0.446)
N	6112	6112	6107	6067	6112	6112	6112	6112	3069	6112	3069
Kleibergen-Paap F-statistic	487.0	487.0	310.7	309.3	313.9	313.9	313.9	313.9	92.55	313.9	92.55
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1), with explanatory variables of interest interacted with a dummy identifying the camp of current residence (Kalobeyei = 0, Kakuma = 1). All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.60: Heterogeneous treatment effects with dummy identifying households' registration site

Table 1: Food and expenses

	Calories p.c. (kcal)	Food expenses p.c. (USD)	> 2 meals	Dietary Diversity	days/ 2100 kcal	HFIAS score	Expenses p.c. (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid cut	-166.6**	-0.0782***	-0.0741**	-0.394***	0.113	0.636***	-3.904***
	(65.80)	(0.0231)	(0.0351)	(0.0896)	(0.0978)	(0.156)	(0.930)
Time (in months)	6.385	-0.000920	-0.00307	0.0208*	-0.00905	-0.0570***	0.234***
	(8.317)	(0.00351)	(0.00422)	(0.0118)	(0.0166)	(0.0207)	(0.0860)
Aid cut x Time	-27.64	-0.00235	0.00395	-0.0890*	0.0451	-0.0185	0.0569
	(33.36)	(0.0114)	(0.0177)	(0.0470)	(0.0527)	(0.0734)	(0.425)
Aid cut x Kakuma	61.04	0.0241	-0.00412	0.169	0.166	-0.121	1.730
	(126.0)	(0.0348)	(0.0493)	(0.141)	(0.163)	(0.246)	(1.137)
Time (in months) x Kakuma	-41.33**	-0.00280	-0.0135**	0.00268	0.0180	0.00405	-0.378***
	(17.12)	(0.00536)	(0.00629)	(0.0189)	(0.0237)	(0.0318)	(0.125)
Aid cut x Time x Kakuma	44.87	0.0413**	0.0616**	0.0476	-0.135	-0.0629	0.202
	(60.03)	(0.0181)	(0.0248)	(0.0716)	(0.0928)	(0.127)	(0.515)
N	5557	5557	5557	5557	5539	5557	5526
Kleibergen-Paap F-statistic	387.9	387.9	387.9	387.9	383.5	387.9	268.4
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.03

Table 2: Food Stock, debt and savings

	Food Stock (kg)	Food Stock (USD)	HH Debt (USD) Food retailer	HH Debt (USD) All sources	Assets & savings
	(8)	(9)	(10)	(11)	(12)
Aid cut	-4.859***	-4.739***	-38.36***	-36.51**	-23.22
	(1.438)	(1.373)	(14.17)	(14.65)	(22.04)
Time (in months)	0.605***	0.682***	16.48***	16.14***	21.94***
	(0.173)	(0.156)	(2.156)	(2.273)	(2.395)
Aid cut x Time	0.00650	-0.0360	-34.21***	-32.41***	1.079
	(0.755)	(0.691)	(5.217)	(5.419)	(10.39)
Aid cut x Kakuma	7.011**	11.91***	11.93	9.817	-32.89
	(3.251)	(3.517)	(15.60)	(17.97)	(28.56)
Time (in months) x Kakuma	-0.205	-0.0393	-12.37***	-10.95***	12.28***
	(0.345)	(0.322)	(2.455)	(2.683)	(4.490)
Aid cut x Time x Kakuma	-3.089**	-1.582	25.06***	16.58**	-39.79***
	(1.285)	(1.492)	(5.937)	(7.403)	(11.51)
N	6112	6112	6112	6112	6112
Kleibergen-Paap F-statistic	314.9	314.9	314.9	314.9	314.9
Baseline mean	24.62	22.93	340.9	364.9	702.6

Table 3: Mood, time, and risk preferences

	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)
	(13)	(14)	(15)	(16)	(17)	(18)
Aid cut	0.00870	-0.198	0.0793	-0.207***	-0.0291	0.117***
	(0.0656)	(0.166)	(0.0594)	(0.0393)	(0.0196)	(0.0367)
Time (in months)	0.0114	0.110***	0.00285	0.0325***	0.0253***	-0.0547***
	(0.00836)	(0.0235)	(0.00782)	(0.00586)	(0.00336)	(0.00719)
Aid cut x Time	-0.0619*	-0.139	-0.122***	0.0327*	-0.00403	0.0270*
	(0.0323)	(0.0849)	(0.0272)	(0.0169)	(0.00838)	(0.0147)
Aid cut x Kakuma	-0.143	0.529*	-0.132	0.0191	-0.0471*	0.0610
	(0.0955)	(0.293)	(0.0922)	(0.0559)	(0.0255)	(0.0510)
Time (in months) x Kakuma	0.0617***	0.0636	0.0545***	0.0262***	-0.00405	0.00274
	(0.0125)	(0.0406)	(0.0120)	(0.00859)	(0.00456)	(0.0102)
Aid cut x Time x Kakuma	0.0437	0.0478	0.120***	-0.0205	-0.00328	0.0109
	(0.0469)	(0.154)	(0.0417)	(0.0232)	(0.0113)	(0.0190)
N	6112	6112	6107	6067	6112	6112
Kleibergen-Paap F-statistic	485.7	485.7	311.7	310.3	314.9	314.9
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438

Table 4: Cognitive abilities

	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(19)	(20)	(21)	(22)	(23)
Aid cut	0.198***	0.472	0.114	0.00361	-0.633
	(0.0615)	(0.386)	(0.344)	(0.105)	(0.729)
Time (in months)	0.0215**	0.0558	0.0377	0.108***	0.664**
	(0.00866)	(0.0468)	(0.150)	(0.0134)	(0.305)
Aid cut x Time	-0.0225	0.194	-0.0290	-0.0740	0.205
	(0.0289)	(0.200)	(0.170)	(0.0520)	(0.328)
Aid cut x Kakuma	-0.0225	-0.509	0.00792	-0.113	1.421*
	(0.0854)	(0.629)	(0.447)	(0.161)	(0.845)
Time (in months) x Kakuma	-0.0404***	0.388***	-0.112	-0.0343	-0.717**
	(0.0129)	(0.0839)	(0.202)	(0.0212)	(0.363)
Aid cut x Time x Kakuma	0.134***	-0.233	0.297	0.103	0.129
	(0.0409)	(0.316)	(0.262)	(0.0736)	(0.458)
N	6112	6112	3069	6112	3069
Kleibergen-Paap F-statistic	314.9	314.9	91.82	314.9	91.82
Baseline mean	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1), with explanatory variables of interest interacted with a dummy identifying households' registration site (Kalobeyei = 0, Kakuma = 1). All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.61: Heterogeneous treatment effects with dummy identifying households receiving in-kind rations vs. cash transfers only

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Expenses p.c. (USD) (7)	Food Stock (kg) (8)	Food Stock (USD) (9)	HH Debt (USD) Food retailer (10)	HH Debt (USD) All sources (11)	Assets & savings (12)
Aid cut	-217.9***	-0.0966***	-0.0698*	-0.299***	0.0855	0.497***	-4.158***	-3.915**	-3.962**	-21.49	-19.85	-47.10
	(78.32)	(0.0273)	(0.0403)	(0.102)	(0.124)	(0.180)	(0.998)	(1.676)	(1.564)	(16.40)	(16.84)	(29.75)
Time (in months)	3.708	-0.00247	-0.00361	0.0172	0.000681	-0.0368	0.226**	0.622***	0.695***	13.79***	13.03***	20.16***
	(9.406)	(0.00408)	(0.00497)	(0.0136)	(0.0180)	(0.0227)	(0.0996)	(0.202)	(0.179)	(2.666)	(2.801)	(2.744)
Aid cut x Time	-19.10	0.000116	-0.00476	-0.153***	0.0493	0.0180	0.197	-0.611	-0.675	-36.92***	-35.02***	9.836
	(39.09)	(0.0131)	(0.0206)	(0.0536)	(0.0648)	(0.0800)	(0.507)	(0.868)	(0.773)	(6.333)	(6.513)	(13.38)
Aid cut x In-kind	117.7	0.0489	0.00918	0.0382	0.174	0.0225	1.495	6.010*	10.42***	-3.065	-5.067	0.359
	(128.2)	(0.0383)	(0.0521)	(0.151)	(0.176)	(0.258)	(1.228)	(3.274)	(3.437)	(17.96)	(19.83)	(35.63)
Time (in months) x In-kind	-40.24**	-0.00432	-0.0154**	-0.000853	0.00712	-0.0194	-0.287**	-0.227	0.0161	-9.510***	-7.801**	11.69**
	(17.09)	(0.00572)	(0.00667)	(0.0197)	(0.0243)	(0.0327)	(0.132)	(0.374)	(0.340)	(2.978)	(3.205)	(4.822)
Aid cut x Time x In-kind	21.54	0.0222	0.0593**	0.139*	-0.130	-0.0212	-0.0407	-2.566*	-1.760	27.66***	19.57**	-39.38***
	(60.45)	(0.0212)	(0.0269)	(0.0748)	(0.0923)	(0.133)	(0.576)	(1.395)	(1.503)	(6.829)	(8.161)	(14.37)
N	4845	4845	4845	4845	4831	4845	4822	5330	5330	5330	5330	5330
Kleibergen-Paap F-statistic	341.0	341.0	341.0	341.0	339.8	341.0	254.7	297.8	297.8	297.8	297.8	297.8
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.03	24.62	22.93	340.9	364.9	702.6

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5) (13)	Good mood score (0-9) (14)	Happiness Likert (1-5) (15)	Required Rate of Return (16)	Payment now (dummy) (17)	Risk loving score (1-4) (18)	Math test score (0-3) (19)	Memory test score (0-36) (20)	Flanker task score (0-10) (21)	Psychomotor vigilance task score (0-30) (22)	Hearts & flower task score (0-40) (23)
Aid cut	-0.0336	-0.154	0.0324	-0.191***	-0.0280	0.130***	0.155**	0.616	-0.282	0.0867	-1.180
	(0.0748)	(0.197)	(0.0673)	(0.0457)	(0.0242)	(0.0448)	(0.0720)	(0.452)	(0.373)	(0.123)	(0.891)
Time (in months)	0.00790	0.105***	0.00908	0.0308***	0.0238***	-0.0554***	0.0303***	0.0897*	0.234	0.119***	0.612*
	(0.00928)	(0.0268)	(0.00910)	(0.00679)	(0.00395)	(0.00845)	(0.0100)	(0.0528)	(0.161)	(0.0157)	(0.346)
Aid cut x Time	-0.0520	-0.213**	-0.136***	0.0306	0.00118	0.0206	-0.0353	0.227	-0.224	-0.133**	0.537
	(0.0344)	(0.0935)	(0.0296)	(0.0194)	(0.00998)	(0.0178)	(0.0348)	(0.242)	(0.191)	(0.0606)	(0.372)
Aid cut x In-kind	-0.0683	0.426	-0.0734	0.0208	-0.0525*	0.0257	0.0659	-0.540	0.0272	-0.240	1.865*
	(0.103)	(0.309)	(0.0948)	(0.0586)	(0.0295)	(0.0565)	(0.0924)	(0.655)	(0.450)	(0.171)	(0.950)
Time (in months) x In-kind	0.0594***	0.0659	0.0419***	0.0238**	0.000827	0.00728	-0.0501***	0.327***	-0.0453	-0.0509**	-0.599
	(0.0131)	(0.0418)	(0.0126)	(0.00922)	(0.00508)	(0.0110)	(0.0139)	(0.0857)	(0.207)	(0.0223)	(0.377)
Aid cut x Time x In-kind	0.0175	0.0203	0.115***	-0.0351	-0.0147	0.00485	0.137***	-0.242	0.0725	0.188**	-0.376
	(0.0490)	(0.155)	(0.0429)	(0.0249)	(0.0125)	(0.0213)	(0.0460)	(0.342)	(0.293)	(0.0821)	(0.475)
N	5331	5331	5328	5294	5330	5330	5330	5330	2672	5330	2672
Kleibergen-Paap F-statistic	430.7	430.7	294.0	292.6	297.8	297.8	297.8	297.8	75.04	297.8	75.04
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1), with explanatory variables of interest interacted with a dummy identifying households receiving in-kind transfers versus cash transfers only (cash only = 0, in-kind + cash = 1). Households reporting different aid packages during the study period are excluded. All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.62: Heterogeneous treatment effects with summary index of consumption (standardized)

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Expenses p.c. (USD) (7)	Food Stock (kg) (8)	Food Stock (USD) (9)	HH Debt (USD) Food retailer (10)	HH Debt (USD) All sources (11)	Assets & savings (12)
Aid cut	-133.9**	-0.0676***	-0.0738***	-0.319***	0.176**	0.584***	-3.263***	-1.867	0.247	-32.42***	-31.46***	-37.27***
	(60.95)	(0.0174)	(0.0263)	(0.0682)	(0.0753)	(0.123)	(0.729)	(1.531)	(1.626)	(8.799)	(9.655)	(14.28)
Time (in months)	-11.44	-0.00228	-0.00886***	0.0217**	-0.000482	-0.0544***	0.0775	0.515***	0.679***	11.30***	11.54***	26.93***
	(8.304)	(0.00275)	(0.00333)	(0.00961)	(0.0130)	(0.0162)	(0.0631)	(0.171)	(0.165)	(1.328)	(1.438)	(2.100)
Aid cut x Time	-7.018	0.0146	0.0290**	-0.0725**	-0.0126	-0.0477	0.172	-1.242*	-0.743	-24.53***	-26.09***	-14.22*
	(29.56)	(0.00926)	(0.0131)	(0.0362)	(0.0472)	(0.0626)	(0.310)	(0.652)	(0.711)	(3.609)	(4.031)	(7.452)
Aid cut x Consumption PCA index (sd)	-184.2***	-0.0763***	-0.0266	-0.190**	0.237**	0.388***	-0.126	-5.331***	-5.421***	1.178	2.303	-26.36**
	(56.79)	(0.0177)	(0.0230)	(0.0758)	(0.113)	(0.124)	(0.650)	(1.437)	(1.541)	(7.730)	(8.285)	(11.99)
Time (in months) x Consumption PCA index (sd)	-6.776	-0.000557	-0.00387	-0.0184*	0.00694	-0.000376	0.00448	0.329**	0.235*	1.388	1.601	0.564
	(7.717)	(0.00280)	(0.00276)	(0.00988)	(0.0213)	(0.0148)	(0.0639)	(0.135)	(0.129)	(1.688)	(1.769)	(2.089)
Aid cut x Time x Consumption PCA index (sd)	41.73	0.0102	0.00468	0.0412	-0.0412	-0.0656	-0.137	0.944	0.786	-0.720	-0.833	6.256
	(28.95)	(0.00951)	(0.0120)	(0.0418)	(0.0588)	(0.0632)	(0.286)	(0.635)	(0.660)	(3.278)	(3.590)	(5.199)
N	5553	5553	5553	5553	5535	5553	5522	6108	6108	6108	6108	6108
Kleibergen-Paap F-statistic	428.4	428.4	428.4	428.4	422.4	428.4	265.5	325.3	325.3	325.3	325.3	325.3
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.03	24.62	22.93	340.9	364.9	702.6

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0593	0.00441	0.0155	-0.202***	-0.0483***	0.141***	0.190***	0.219	0.143	-0.0411	-0.00389
	(0.0493)	(0.145)	(0.0468)	(0.0283)	(0.0134)	(0.0258)	(0.0435)	(0.310)	(0.255)	(0.0848)	(0.531)
Time (in months)	0.0376***	0.139***	0.0262***	0.0439***	0.0235***	-0.0533***	0.00449	0.219***	-0.0202	0.0928***	0.354
	(0.00664)	(0.0205)	(0.00628)	(0.00437)	(0.00246)	(0.00542)	(0.00661)	(0.0413)	(0.118)	(0.0107)	(0.238)
Aid cut x Time	-0.0430*	-0.122	-0.0721***	0.0240*	-0.00492	0.0308***	0.0319	0.112	0.0997	-0.0307	0.267
	(0.0238)	(0.0751)	(0.0211)	(0.0126)	(0.00611)	(0.0106)	(0.0212)	(0.161)	(0.138)	(0.0402)	(0.251)
Aid cut x Consumption PCA index (sd)	0.0317	0.138	0.00923	-0.0466*	0.0226*	-0.00267	-0.0283	-0.439	-0.125	-0.144*	0.155
	(0.0478)	(0.138)	(0.0407)	(0.0281)	(0.0122)	(0.0262)	(0.0463)	(0.280)	(0.227)	(0.0857)	(0.404)
Time (in months) x Consumption PCA index (sd)	-0.00347	-0.0226	0.000392	-0.00617	-0.00151	0.00780*	0.00173	-0.0179	-0.145	-0.0104	0.0595
	(0.00593)	(0.0176)	(0.00593)	(0.00396)	(0.00230)	(0.00459)	(0.00636)	(0.0408)	(0.101)	(0.0101)	(0.163)
Aid cut x Time x Consumption PCA index (sd)	-0.0236	-0.00313	-0.0375**	0.0149	0.000682	-0.0246**	0.0205	0.224	0.174	0.0859**	-0.109
	(0.0232)	(0.0651)	(0.0187)	(0.0124)	(0.00467)	(0.0110)	(0.0225)	(0.145)	(0.117)	(0.0403)	(0.209)
N	6108	6108	6103	6063	6108	6108	6108	6108	3065	6108	3065
Kleibergen-Paap F-statistic	514.9	514.9	322.4	320.3	325.3	325.3	325.3	325.3	122.2	325.3	122.2
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1), with explanatory variables of interest interacted with a summary index of consumption (standardized with mean = 0 and s.d.=1). The summary index of consumption is constructed using Principal Component Analysis with the outcomes that relate to consumption, food insecurity, and expenditures in waves 1 to 7, before the aid cut (Table A.44). All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.63: Heterogeneous treatment effects with total household debt (standardized)

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Expenses p.c. (USD) (7)	Food Stock (kg) (8)	Food Stock (USD) (9)	HH Debt (USD) Food retailer (10)	HH Debt (USD) All sources (11)	Assets & savings (12)
Aid cut	-139.4**	-0.0688***	-0.0758***	-0.324***	0.182**	0.591***	-3.227***	-1.925	0.203	-32.15***	-31.21***	-38.03***
	(60.96)	(0.0176)	(0.0263)	(0.0687)	(0.0753)	(0.123)	(0.732)	(1.540)	(1.632)	(8.923)	(9.679)	(14.44)
Time (in months)	-11.15	-0.00215	-0.00879***	0.0219**	-0.00121	-0.0549***	0.0773	0.521***	0.682***	11.22***	11.43***	27.00***
	(8.297)	(0.00275)	(0.00332)	(0.00965)	(0.0127)	(0.0162)	(0.0633)	(0.171)	(0.165)	(1.271)	(1.384)	(2.104)
Aid cut x Time	-6.280	0.0149	0.0294**	-0.0705*	-0.0122	-0.0480	0.158	-1.259*	-0.748	-23.67***	-25.11***	-14.22*
	(29.69)	(0.00932)	(0.0131)	(0.0362)	(0.0467)	(0.0623)	(0.310)	(0.653)	(0.711)	(3.560)	(4.016)	(7.435)
Aid cut x Total HH Debt (sd)	-128.9***	-0.0373**	-0.0508**	-0.144**	0.0369	0.168	-0.348	-3.178**	-4.011***	-27.78*	-39.01**	-12.06
	(48.13)	(0.0150)	(0.0241)	(0.0656)	(0.0596)	(0.121)	(0.513)	(1.331)	(1.386)	(14.25)	(15.16)	(11.84)
Time (in months) x Total HH Debt (sd)	9.927	0.00122	0.00797***	0.00401	-0.00911	0.000855	0.0102	-0.0479	-0.0670	8.287***	8.585***	3.255
	(7.007)	(0.00244)	(0.00301)	(0.00952)	(0.00902)	(0.0166)	(0.0560)	(0.145)	(0.131)	(2.910)	(2.957)	(2.519)
Aid cut x Time x Total HH Debt (sd)	20.85	-0.00766	0.00344	-0.0307	0.0317	-0.0433	-0.0139	1.201*	0.706	-20.26***	-18.50***	6.748
	(23.28)	(0.00758)	(0.0120)	(0.0348)	(0.0359)	(0.0585)	(0.232)	(0.617)	(0.622)	(4.798)	(5.158)	(7.790)
N	5553	5553	5553	5553	5535	5553	5522	6108	6108	6108	6108	6108
Kleibergen-Paap F-statistic	433.6	433.6	433.6	433.6	428.4	433.6	270.1	328.9	328.9	328.9	328.9	328.9
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.03	24.62	22.93	340.9	364.9	702.6

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0577	0.00357	0.0174	-0.203***	-0.0482***	0.142***	0.190***	0.229	0.110	-0.0429	-0.0517
	(0.0493)	(0.145)	(0.0470)	(0.0284)	(0.0134)	(0.0257)	(0.0436)	(0.309)	(0.251)	(0.0847)	(0.542)
Time (in months)	0.0375***	0.138***	0.0262***	0.0439***	0.0235***	-0.0534***	0.00439	0.220***	-0.00486	0.0930***	0.375
	(0.00662)	(0.0204)	(0.00623)	(0.00438)	(0.00247)	(0.00544)	(0.00656)	(0.0410)	(0.117)	(0.0108)	(0.240)
Aid cut x Time	-0.0432*	-0.117	-0.0727***	0.0240*	-0.00494	0.0306***	0.0337	0.101	0.0879	-0.0304	0.243
	(0.0239)	(0.0752)	(0.0211)	(0.0127)	(0.00612)	(0.0106)	(0.0212)	(0.162)	(0.137)	(0.0402)	(0.251)
Aid cut x Total HH Debt (sd)	0.0204	-0.232*	0.0241	-0.0409	0.0119	-0.0211	-0.0625	0.367	-0.137	-0.00683	-0.781**
	(0.0457)	(0.131)	(0.0438)	(0.0281)	(0.0155)	(0.0263)	(0.0422)	(0.284)	(0.194)	(0.0716)	(0.397)
Time (in months) x Total HH Debt (sd)	-0.0110*	0.0216	-0.0131**	-0.00162	-0.000748	-0.00287	0.0205***	-0.137***	0.145*	0.0136	0.468***
	(0.00579)	(0.0181)	(0.00586)	(0.00433)	(0.00249)	(0.00538)	(0.00602)	(0.0374)	(0.0857)	(0.0103)	(0.156)
Aid cut x Time x Total HH Debt (sd)	-0.0286	-0.0648	-0.0307	0.0140	0.00448	0.00333	-0.0357*	-0.0117	-0.186	-0.0144	-0.0538
	(0.0229)	(0.0652)	(0.0215)	(0.0121)	(0.00591)	(0.00912)	(0.0204)	(0.166)	(0.119)	(0.0405)	(0.198)
N	6108	6108	6103	6063	6108	6108	6108	6108	3065	6108	3065
Kleibergen-Paap F-statistic	526.2	526.2	325.9	323.4	328.9	328.9	328.9	328.9	118.3	328.9	118.3
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1), with explanatory variables of interest interacted with the mean value of total household debt in waves 1 to 7, before the aid cut (standardized with mean = 0 and s.d.=1). All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.64: Heterogeneous treatment effects with the value of household assets (standardized)

	Table 1: Food and expenses							Table 2: Food Stock, debt and savings				
	Calories p.c. (kcal) (1)	Food expenses p.c. (USD) (2)	> 2 meals (3)	Dietary Diversity (4)	days/ 2100 kcal (5)	HFIAS score (6)	Expenses p.c. (USD) (7)	Food Stock (kg) (8)	Food Stock (USD) (9)	HH Debt (USD) Food retailer (10)	HH Debt (USD) All sources (11)	Assets & savings (12)
Aid cut	-137.6**	-0.0690***	-0.0749***	-0.325***	0.182**	0.590***	-3.269***	-1.985	0.128	-32.51***	-31.46***	-35.12**
	(61.25)	(0.0178)	(0.0265)	(0.0690)	(0.0757)	(0.123)	(0.729)	(1.508)	(1.591)	(8.904)	(9.725)	(13.71)
Time (in months)	-11.14	-0.00209	-0.00884***	0.0222**	-0.00115	-0.0556***	0.0787	0.521***	0.679***	11.31***	11.57***	26.99***
	(8.332)	(0.00276)	(0.00332)	(0.00965)	(0.0127)	(0.0162)	(0.0614)	(0.170)	(0.164)	(1.324)	(1.433)	(2.026)
Aid cut x Time	-8.919	0.0140	0.0287**	-0.0734**	-0.0108	-0.0452	0.177	-1.205*	-0.684	-24.49***	-26.15***	-15.67**
	(29.73)	(0.00932)	(0.0131)	(0.0364)	(0.0468)	(0.0625)	(0.311)	(0.641)	(0.696)	(3.673)	(4.095)	(7.397)
Aid cut x Assets & Savings (sd)	-53.94	-0.00406	-0.00593	0.0134	0.0306	0.0207	1.306*	4.025**	5.826***	0.990	-6.443	-72.72***
	(60.06)	(0.0172)	(0.0227)	(0.0635)	(0.0792)	(0.127)	(0.702)	(1.822)	(2.002)	(7.171)	(9.017)	(15.32)
Time (in months) x Assets & Savings (sd)	-13.52*	-0.00347	-0.00364	-0.00898	0.00950	-0.00705	-0.356***	-0.0263	0.135	-1.601	-1.271	15.45***
	(7.697)	(0.00312)	(0.00289)	(0.00991)	(0.0126)	(0.0173)	(0.0698)	(0.270)	(0.225)	(1.325)	(1.418)	(3.593)
Aid cut x Time x Assets & Savings (sd)	17.05	0.01000	0.00221	0.0485	-0.0292	-0.00529	0.280	-1.636*	-1.052	0.155	-3.222	-23.55***
	(26.84)	(0.00839)	(0.0113)	(0.0339)	(0.0463)	(0.0619)	(0.316)	(0.880)	(1.072)	(2.781)	(3.379)	(7.160)
N	5553	5553	5553	5553	5535	5553	5522	6108	6108	6108	6108	6108
Kleibergen-Paap F-statistic	418.1	418.1	418.1	418.1	413.6	418.1	262.0	318.2	318.2	318.2	318.2	318.2
Baseline mean	2017.9	0.694	0.575	4.851	1.476	4.315	15.03	24.62	22.93	340.9	364.9	702.6

	Table 3: Mood, time, and risk preferences						Table 4: Cognitive abilities				
	Sleep quality Likert (1-5)	Good mood score (0-9)	Happiness Likert (1-5)	Required Rate of Return	Payment now (dummy)	Risk loving score (1-4)	Math test score (0-3)	Memory test score (0-36)	Psychomotor vigilance task score (0-30)	Flanker task score (0-10)	Hearts & flower task score (0-40)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Aid cut	-0.0561	0.00366	0.0197	-0.202***	-0.0491***	0.143***	0.189***	0.245	0.0886	-0.0430	0.0239
	(0.0495)	(0.144)	(0.0466)	(0.0285)	(0.0134)	(0.0260)	(0.0436)	(0.310)	(0.250)	(0.0849)	(0.535)
Time (in months)	0.0374***	0.138***	0.0262***	0.0438***	0.0235***	-0.0534***	0.00470	0.218***	-0.0000443	0.0932***	0.336
	(0.00664)	(0.0205)	(0.00626)	(0.00437)	(0.00244)	(0.00542)	(0.00661)	(0.0403)	(0.116)	(0.0107)	(0.237)
Aid cut x Time	-0.0439*	-0.119	-0.0748***	0.0245*	-0.00449	0.0298***	0.0317	0.104	0.0876	-0.0316	0.286
	(0.0240)	(0.0749)	(0.0210)	(0.0126)	(0.00613)	(0.0107)	(0.0213)	(0.162)	(0.138)	(0.0404)	(0.250)
Aid cut x Assets & Savings (sd)	-0.0310	0.170	-0.130***	0.0373	0.00950	-0.0361*	-0.0159	-0.263	0.161	-0.0347	0.522
	(0.0448)	(0.163)	(0.0475)	(0.0257)	(0.0114)	(0.0202)	(0.0471)	(0.276)	(0.190)	(0.0680)	(0.343)
Time (in months) x Assets & Savings (sd)	0.0182***	0.00436	0.0187***	0.00460	-0.00817***	0.0104**	-0.0128**	0.247***	-0.0795	-0.0163*	-0.331**
	(0.00702)	(0.0208)	(0.00689)	(0.00445)	(0.00183)	(0.00444)	(0.00591)	(0.0390)	(0.0957)	(0.00918)	(0.158)
Aid cut x Time x Assets & Savings (sd)	0.00369	0.0304	0.0366	-0.0213**	0.00508	-0.00229	0.0173	-0.259**	0.135	0.0308	0.0542
	(0.0208)	(0.0750)	(0.0232)	(0.00956)	(0.00402)	(0.00999)	(0.0190)	(0.130)	(0.125)	(0.0333)	(0.196)
N	6108	6108	6103	6063	6108	6108	6108	6108	3065	6108	3065
Kleibergen-Paap F-statistic	505.1	505.1	315.5	313.6	318.2	318.2	318.2	318.2	109.9	318.2	109.9
Baseline mean	3.056	4.976	2.746	1.787	0.855	1.438	1.935	18.66	28.28	8.471	34.38

Notes: Instrumental Variable (IV) regressions estimating Equation (3.1), with explanatory variables of interest interacted with the value of household assets in waves 1 to 7, before the aid cut (standardized with mean = 0 and s.d.=1). All regressions include household, day-of-month, and day-of-week fixed effects, and control variables, as specified in Appendix Table A.43. Variables that depend on the actual interview dates are instrumented using their counterparts constructed based on the randomly-assigned interview dates. One-way clustered standard errors at the household level in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.7.3 Analysis of Price Data - Additional Figure and Tables

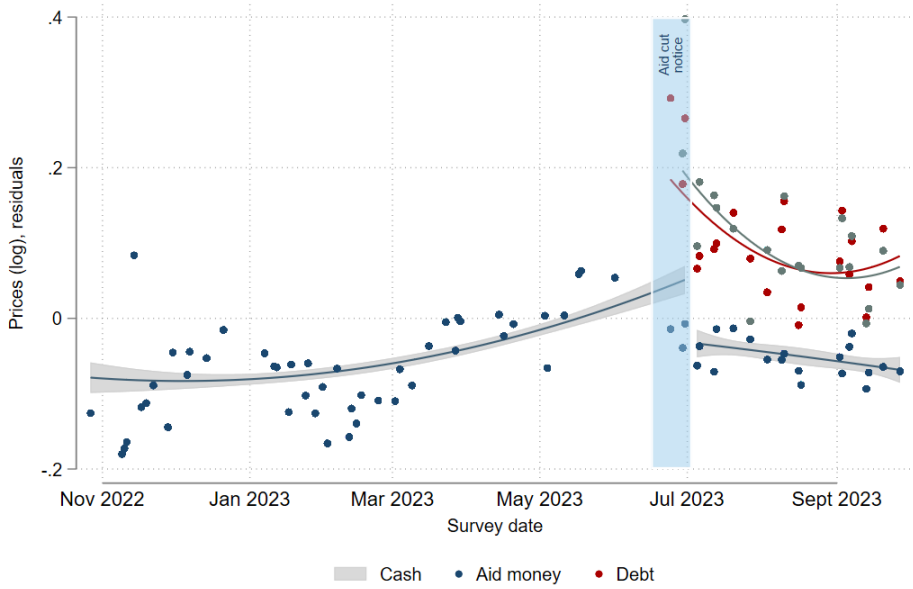


Figure A.2: Impact of the aid cut on prices

Data source: price survey data. Staple goods only. Data collection on prices with aid money or debt started in June 2023 and lasted for 3 months. To isolate price changes unrelated to market or product characteristics, we regress prices on product and market fixed effects and use the residuals. Blue dots represent average residual prices for each survey dates. The blue line with a grey confidence interval shows a quadratic fit of prices before and after the cut. The green and red quadratic fits show the evolution of aid card and debt prices respectively. The blue-shaded area marks the period during which the aid cut was progressively announced in the camp, from June 16 when market coordinators were notified, until July 3, when in-kind distribution began.

Table A.65: Impacts of aid cut on cash prices (log), without control variables

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0368***	-0.0514***	-0.0297***	-0.0362***	-0.0583***	-0.0255*	-0.0374***	-0.0422*	-0.0351**
	(0.00870)	(0.0149)	(0.0108)	(0.0115)	(0.0193)	(0.0143)	(0.0135)	(0.0235)	(0.0167)
Time (in months)	0.428***	0.498***	0.395***	0.445***	0.543***	0.397***	0.405***	0.434***	0.390***
	(0.0521)	(0.0646)	(0.0715)	(0.0639)	(0.0818)	(0.0868)	(0.0882)	(0.105)	(0.123)
Aid cut x Time	-0.704***	-0.892***	-0.614***	-0.764***	-0.924***	-0.689***	-0.621***	-0.847***	-0.513**
	(0.122)	(0.173)	(0.161)	(0.166)	(0.234)	(0.219)	(0.180)	(0.259)	(0.237)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.989	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. Control variables are excluded from regressions. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.66: Impacts of aid cut on cash prices (log), without market fixed effects

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0360***	-0.0521***	-0.0283***	-0.0356***	-0.0503**	-0.0288*	-0.0421***	-0.0492**	-0.0388**
	(0.00873)	(0.0149)	(0.0108)	(0.0119)	(0.0193)	(0.0151)	(0.0142)	(0.0242)	(0.0176)
Time (in months)	0.396***	0.448***	0.370***	0.423***	0.438***	0.416***	0.394***	0.419***	0.381***
	(0.0519)	(0.0638)	(0.0709)	(0.0675)	(0.0782)	(0.0934)	(0.0903)	(0.106)	(0.126)
Aid cut x Time	-0.607***	-0.728***	-0.549***	-0.734***	-0.744***	-0.729***	-0.454**	-0.660***	-0.356
	(0.123)	(0.170)	(0.163)	(0.171)	(0.236)	(0.227)	(0.177)	(0.244)	(0.234)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.989	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product fixed effects, and control variables that account for aid distribution. Market fixed effects are excluded from regressions. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.67: Impacts of the aid cut on cash prices with robust standard errors

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0366***	-0.0525***	-0.0290***	-0.0362***	-0.0506***	-0.0295**	-0.0426***	-0.0497***	-0.0392***
	(0.00689)	(0.00954)	(0.00910)	(0.00921)	(0.0134)	(0.0120)	(0.0109)	(0.0144)	(0.0146)
Time (in months)	0.401***	0.449***	0.377***	0.434***	0.439***	0.432***	0.396***	0.422***	0.383***
	(0.0327)	(0.0438)	(0.0437)	(0.0447)	(0.0639)	(0.0588)	(0.0541)	(0.0670)	(0.0742)
Aid cut x Time	-0.606***	-0.731***	-0.547***	-0.749***	-0.747***	-0.752***	-0.457***	-0.666***	-0.357*
	(0.0973)	(0.131)	(0.130)	(0.131)	(0.180)	(0.173)	(0.149)	(0.190)	(0.201)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.989	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables that account for aid distribution. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.68: Impacts of aid cut on cash prices, normalized by the mean price of each product

	Dependent variable: price/mean (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0290***	-0.0455***	-0.0212*	-0.0274**	-0.0388**	-0.0221	-0.0345**	-0.0460*	-0.0289
	(0.00884)	(0.0140)	(0.0113)	(0.0118)	(0.0176)	(0.0153)	(0.0149)	(0.0234)	(0.0191)
Time (in months)	0.326***	0.394***	0.293***	0.352***	0.355***	0.352***	0.312***	0.380***	0.277**
	(0.0503)	(0.0580)	(0.0696)	(0.0651)	(0.0696)	(0.0914)	(0.0897)	(0.0989)	(0.126)
Aid cut x Time	-0.573***	-0.704***	-0.511***	-0.700***	-0.683***	-0.710***	-0.429**	-0.658***	-0.318
	(0.117)	(0.151)	(0.157)	(0.166)	(0.211)	(0.224)	(0.166)	(0.219)	(0.222)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.083	0.097	0.073	0.095	0.101	0.088	0.083	0.109	0.072

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the cash price divided by the mean price of each product. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables that account for aid distribution.Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.69: Impacts of aid cut on cash prices, normalized by the median price of each product

	Dependent variable: price/median(cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0273***	-0.0480***	-0.0173	-0.0241**	-0.0405**	-0.0164	-0.0346**	-0.0496**	-0.0273
	(0.00914)	(0.0146)	(0.0116)	(0.0121)	(0.0184)	(0.0156)	(0.0154)	(0.0244)	(0.0198)
Time (in months)	0.329***	0.402***	0.293***	0.347***	0.359***	0.342***	0.323***	0.393***	0.288**
	(0.0516)	(0.0591)	(0.0715)	(0.0659)	(0.0705)	(0.0925)	(0.0932)	(0.101)	(0.131)
Aid cut x Time	-0.576***	-0.717***	-0.510***	-0.693***	-0.696***	-0.694***	-0.441**	-0.669***	-0.332
	(0.121)	(0.155)	(0.163)	(0.171)	(0.216)	(0.231)	(0.172)	(0.225)	(0.230)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.186	0.152	0.187	0.186	0.151	0.187	0.202	0.170	0.204

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the cash price divided by the median price of each product. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables that account for aid distribution. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.70: Price effects using a local Regression Discontinuity Design

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0261***	-0.0677***	-0.00643	-0.0228**	-0.0719***	0.000142	-0.0307***	-0.0625***	-0.0154
	(0.00727)	(0.0135)	(0.00839)	(0.00972)	(0.0169)	(0.0115)	(0.0111)	(0.0224)	(0.0124)
Observations	5122	1646	3476	2926	932	1994	2196	714	1482
R^2	0.988	0.990	0.977	0.988	0.990	0.979	0.988	0.990	0.976

Notes: Ordinary Least Squares (OLS) regressions the logarithm of cash prices on the aid cut dummy, with only waves 7, 8, and 9. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables that account for aid distribution. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.71: Impacts of aid cut on cash prices (log), with cutoff date June 16th 2023

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0287***	-0.0383**	-0.0241*	-0.0210	-0.0383*	-0.0127	-0.0444***	-0.0335	-0.0496**
	(0.00988)	(0.0164)	(0.0124)	(0.0128)	(0.0230)	(0.0155)	(0.0167)	(0.0246)	(0.0219)
Time (in months)	0.0146***	0.0163***	0.0138***	0.0152***	0.0162***	0.0148***	0.0155***	0.0150***	0.0157***
	(0.00182)	(0.00222)	(0.00249)	(0.00246)	(0.00277)	(0.00343)	(0.00313)	(0.00372)	(0.00436)
Aid cut x Time	-0.0228***	-0.0287***	-0.0200***	-0.0280***	-0.0291***	-0.0275***	-0.0178***	-0.0267***	-0.0135*
	(0.00387)	(0.00491)	(0.00523)	(0.00534)	(0.00686)	(0.00721)	(0.00584)	(0.00706)	(0.00797)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.989	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables. The cutoff date for this placebo test is the 16th of June 2023, corresponding to the date in which the aid cut was first announced to market coordinators in the camp. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.72: Impacts of aid cut on cash prices (log), with cutoff date June 24th 2023

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	-0.0347***	-0.0458***	-0.0294**	-0.0283**	-0.0459**	-0.0200	-0.0490***	-0.0405*	-0.0532**
	(0.00967)	(0.0160)	(0.0121)	(0.0127)	(0.0224)	(0.0156)	(0.0164)	(0.0240)	(0.0216)
Time (in months)	0.0146***	0.0163***	0.0138***	0.0152***	0.0162***	0.0148***	0.0155***	0.0150***	0.0157***
	(0.00182)	(0.00222)	(0.00249)	(0.00246)	(0.00277)	(0.00343)	(0.00313)	(0.00372)	(0.00436)
Aid cut x Time	-0.0228***	-0.0287***	-0.0200***	-0.0280***	-0.0291***	-0.0275***	-0.0178***	-0.0267***	-0.0135*
	(0.00387)	(0.00491)	(0.00523)	(0.00534)	(0.00686)	(0.00721)	(0.00584)	(0.00706)	(0.00797)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.989	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables. The cutoff date for this placebo test is the 24th of June 2023, corresponding to the mid-point between the first announcement of the cut and its implementation date. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.73: Impacts of aid cut on cash prices (log), placebo test with cutoff date February 6th 2023

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)	All food (7)	Staple food (8)	Non-staple food (9)
Aid cut	0.0362***	0.00702	0.0495***	0.0349***	0.0169	0.0429***	0.0394***	-0.00778	0.0612***
	(0.00839)	(0.0144)	(0.0103)	(0.0112)	(0.0212)	(0.0130)	(0.0126)	(0.0161)	(0.0168)
Time (in months)	0.00182	0.000993	0.00304	0.00556	0.000192	0.00894	-0.00526	-0.000608	-0.00649
	(0.00549)	(0.00839)	(0.00722)	(0.00747)	(0.0126)	(0.00942)	(0.00807)	(0.00909)	(0.0116)
Aid cut x Time	-0.00142	0.00157	-0.00369	-0.00586	0.000680	-0.00980	0.00647	0.00506	0.00613
	(0.00583)	(0.00870)	(0.00772)	(0.00797)	(0.0130)	(0.0102)	(0.00846)	(0.00964)	(0.0121)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.988	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables. The cutoff date for this placebo test is February 6th 2023. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.74: Impacts of aid cut on cash prices (log), placebo test with cutoff date March 6th 2023

	Dependent variable: log prices (cash)								
	Both sites			Kakuma			Kalobeyei		
	All food	Staple food	Non-staple food	All food	Staple food	Non-staple food	All food	Staple food	Non-staple food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Aid cut	0.0537***	0.0906***	0.0354***	0.0499***	0.0856***	0.0322***	0.0587***	0.0968***	0.0394***
	(0.00657)	(0.00961)	(0.00830)	(0.00836)	(0.0128)	(0.0104)	(0.0106)	(0.0145)	(0.0135)
Time (in months)	0.00473	-0.0119***	0.0135***	0.00704	-0.00967	0.0157***	0.00106	-0.0174***	0.0109
	(0.00321)	(0.00422)	(0.00436)	(0.00438)	(0.00598)	(0.00587)	(0.00482)	(0.00574)	(0.00671)
Aid cut x Time	-0.00959**	0.00503	-0.0174***	-0.0121**	0.00229	-0.0197***	-0.00564	0.0109	-0.0146*
	(0.00383)	(0.00493)	(0.00525)	(0.00522)	(0.00703)	(0.00708)	(0.00567)	(0.00664)	(0.00797)
Observations	16760	5458	11302	9841	3189	6652	6919	2269	4650
R^2	0.987	0.989	0.975	0.987	0.988	0.976	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3), where the outcome variable is the logarithm of cash prices. Column (1), Column (2) and Column (3) include both Kakuma and Kalobeyei markets; Column (4), Column (5) and Column (6) focus on Kakuma markets; and Column (7), Column (8), and Column (9) focus on Kalobeyei markets. Regressions in Columns (1), (4), and (7) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables. The cutoff date for this placebo test is March 6th 2023. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.75: Heterogeneous treatment effects on prices, with interaction terms with camp dummy (Kalobeyei=0, Kakuma=1)

	Dependent variable: log prices (cash)					
	All food (1)	Staple food (2)	Non-staple food (3)	All food (4)	Staple food (5)	Non-staple food (6)
Aid cut	-0.0366***	-0.0525***	-0.0290***	-0.0365***	-0.0505***	-0.0299**
	(0.00874)	(0.0148)	(0.0108)	(0.0119)	(0.0193)	(0.0151)
Time (in months)	0.401***	0.449***	0.377***	0.437***	0.441***	0.435***
	(0.0521)	(0.0640)	(0.0713)	(0.0674)	(0.0777)	(0.0933)
Aid cut x Time	-0.606***	-0.731***	-0.547***	-0.750***	-0.749***	-0.753***
	(0.123)	(0.170)	(0.163)	(0.171)	(0.235)	(0.227)
Aid cut x Kakuma				-0.00559	0.000933	-0.00901
				(0.0184)	(0.0308)	(0.0231)
Time x Kakuma				-0.0441	-0.0216	-0.0551
				(0.111)	(0.129)	(0.154)
Aid cut x Time x Kakuma				0.295	0.0840	0.399
				(0.245)	(0.337)	(0.325)
Observations	16760	5458	11302	16760	5458	11302
R^2	0.987	0.989	0.975	0.987	0.989	0.975

Notes: Ordinary Least Squares (OLS) regressions estimating Equation (3.3) with interaction terms with a dummy variable identifying prices collected in Kakuma. The outcome variable is the logarithm of cash prices. Regressions in Columns (1) and (4) include all foods; Columns (2), (5), and (8) consider staple food only; and Columns (3), (6), and (9) include all food items aside from staple. All regressions include product and market fixed effects, and control variables that account for aid distribution. Standard errors are clustered at the product–market level and reported in parentheses. Statistical significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 4

Rationing Humanitarian Aid: Welfare, Markets, and Social Costs of Differentiated Assistance

Humanitarian agencies increasingly rely on vulnerability-based targeting to ration shrinking aid budgets, yet little is known about its effects in settings where poverty is widespread and budgets are being cut. This chapter studies the rollout of differentiated food assistance alongside aid cuts in Kakuma and Kalobeyei refugee camps in Kenya. Drawing on 70 qualitative interviews with refugees, shopkeepers, and humanitarian staff conducted around the policy change, we examine how the move away from universal aid coupled with a general aid reduction affected household welfare, coping strategies, markets, and social cohesion. Respondent interviews suggest that differentiated assistance led to extensive exclusion errors, heightened food insecurity, and the breakdown of informal credit and mutual support systems that previously helped households smooth shocks. The reform also eroded trust in humanitarian institutions, increased insecurity, worsened mental health outcomes, and contributed to distress-driven migration. Food retailers reported business closures, lack of customers, and goods rotting on shelves suggesting a contraction of the camp economy. The findings suggest that in contexts of near-universal deprivation and volatile livelihoods, differentiated assistance may undermine both efficiency and protection objectives. More universal approaches may offer greater welfare and social stability under severe resource constraints.

Introduction

After the largest recorded drop in international humanitarian assistance in 2024, and further reductions in 2025, donors and agencies have had to rethink how assistance is delivered (ALNAP 2025). In refugee camps, this has involved both reductions in overall aid levels and a shift away from universal provision, where all beneficiaries receive the same level of support, towards vulnerability-based systems that differentiate assistance across households (UNHCR and World Food Programme 2024). Targeting is typically justified on efficiency grounds, understood as maximising the poverty-reducing impact of a fixed programme budget by directing a larger share of transfers to poorer households (Atkinson 1995; Coady and Skoufias 2004). In practice, however, these allocation changes often occur alongside contractions in total resources, so that targeting and aid cuts are jointly implemented rather than separately. This logic rests on several assumptions: vulnerability can be measured accurately and cheaply, household circumstances are stable, and reallocating transfers does not substantially alter behaviour or generate wider social and market spillovers. In refugee camps characterised by near-universal poverty, volatile livelihoods, incomplete markets, and high dependence on aid, these assumptions may be especially fragile. In such settings, redistributing assistance across similarly poor households, while overall support is declining, may generate limited redistributive gains while altering risk-sharing arrangements, informal credit, and local market activity. Yet despite the rapid expansion of vulnerability-based systems in refugee camps, we lack a clear theoretical and empirical understanding of how such reforms, combining targeting and reductions in aid, compare to universal provision in these contexts (Gassmann et al. 2025).

A large literature in social protection evaluates the relative performance of alternative targeting mechanisms, including categorical, geographic, self-targeting, and vulnerability-based approaches (Mkandawire 2007; Grosh et al. 2022; Devereux 2016; Devereux et al. 2015). Most of this work studies programmes that are targeted by design, while only a limited number of papers compare universal and targeted provision directly (Gentilini et al.

2019). By contrast, many refugee assistance systems historically operated on a largely universal basis (Altındağ and O'Connell 2023), and only recently have begun transitioning towards targeted approaches, often under conditions of shrinking budgets. In addition, most of the available evidence comes from development or high-income contexts, rather than refugee camps, which are characterised by distinct legal and economic constraints (Gassmann et al. 2025). As a result, there is limited evidence on what happens when a refugee operation moves from universal provision to household-level targeting in a context of declining assistance. It is similarly unclear how such reforms are experienced by those living through them, and how they reshape social relations and economic interactions within camp economies.

We address these gaps by examining the welfare and social consequences of the joint introduction of targeting and aid cuts in Kakuma refugee camp and Kalobeyei settlement in Kenya. Until August 2025, food assistance in Kakuma and Kalobeyei was universal. All refugees received the same level of support, regardless of household characteristics, which was equivalent to USD 8 per person per month. After the reform, refugees were classified into four vulnerability categories and assistance levels varied accordingly. Category 1 households received USD 8 per person per month, and Category 2 households USD 4, equal to 40% and 20% of the minimum food basket respectively. Refugees in Categories 3 and 4, about 44 percent of the population, were cut off from food assistance entirely. Following protests and operational disruptions, the system was revised in October 2025. Rations for Categories 1 and 2 increased slightly, and limited assistance was reintroduced for Category 3 households. The move to differentiated assistance coincided with a broader decline in aid. The policy change combined greater selectivity with lower aggregate support.

Our analysis draws on 70 semi-structured interviews with refugee households, shopkeepers, and humanitarian staff conducted in two waves: around the rollout of differentiated assistance in August 2025 and again two months later, in October 2025. This design allows us to document both immediate reactions to the reform and short-term adjustments in its aftermath. A qualitative approach is particularly well suited to the

objectives of this chapter. Quantitative studies are valuable for measuring targeting accuracy and average changes in observable outcomes such as consumption or labour supply (Bastagli et al. 2016), but they are less well equipped to capture how reforms are experienced, interpreted, contested, and mediated through informal credit, mutual support, insecurity, and social norms (MacAuslan and Riemenschneider 2011). These dimensions are especially relevant in this context because the effects of targeting under aid cuts in settings of near-universal poverty may operate not only through direct and measurable losses, but also through changes in the local institutions and social arrangements that previously helped households smooth shocks. Attending to these experiences may therefore help uncover social and economic costs that are less easily captured through standard quantitative approaches (Devereux et al. 2015).

Our findings point to three interrelated patterns. First, the combination of reduced aid and differentiated assistance is associated with a deterioration in welfare, reflected in worsening food insecurity, physical health, and psychological well-being. Respondents frequently described meal skipping, reduced dietary diversity, and persistent hunger, alongside stress, anxiety, and suicidal thoughts. Beyond these direct effects, households reported a shift in coping strategies. Pre-existing mechanisms, such as informal credit and mutual support, became harder to access, and were often replaced by more costly and less reliable alternatives, such as reselling house parts or eating unsafe foods. In this sense, the reform appears not only to have reduced consumption, but also to have increased the cost of smoothing it, as deteriorating physical and mental health compounded the reliance on more costly coping strategies.

Second, respondents' accounts picture an economy-wide contraction affecting markets, credit, and labour opportunities. Shopkeepers reported sharp declines in demand, unsold goods, and rising unpaid debts, in some cases leading to business closures. At the same time, refugees reported increasing difficulty finding casual work or employment with NGOs. In a setting where assistance underpins consumption, trade, retail, and informal credit, the combination of aid cuts with targeting appears to have weaken the capacity of the local economy to absorb the shock, further limiting households' ability to smooth

consumption through market-based or credit-based mechanisms.

Third, respondents describe a range of social and institutional consequences associated with the reform. These include protests and boycotts during the rollout and non-compliance with programme rules. Shopkeepers and refugees alike reported increased insecurity, earlier closing hours, spending on informal protection, looting, and curfews. Perceived misclassification was widely reported, with respondents describing both the material consequences of exclusion and the administrative burden of appeals processes. At the same time, respondents reported increased out-migration, often involving households or individuals with greater resources and social capital. These accounts point to social spillovers that extend beyond direct welfare effects, generating additional costs associated with non-compliance, administrative processes and exclusion errors, insecurity, and the erosion of physical and social capital through selective out-migration.

This chapter makes several contributions to the literature. First, it contributes to longstanding debates on the relative merits of alternative targeting systems in social protection and humanitarian policy (Hanna and Olken 2018; Gentilini et al. 2019). While a large body of work highlights the potential efficiency gains of targeting by reducing inclusion errors and increasing the marginal utility of each dollar spent (Coady and Skoufias 2004; Grosh et al. 2022), the findings of this research suggest that this logic becomes less straightforward in settings characterised by near-universal poverty, aid cuts, and strong dependence on aid. In such contexts, the welfare implications of targeting appear to depend on a broader set of considerations. First, the relative importance of inclusion and exclusion errors may differ from standard settings. When most households are clustered near subsistence, the welfare costs of exclusion may be substantial, while the gains from excluding relatively better-off households may be limited (Devereux et al. 2015; Handa and Davis 2006). Second, targeting does not only reallocate resources but also entails administrative costs of its own. Identifying, verifying, and updating beneficiary status requires data collection systems and appeals mechanisms that may be resource-intensive, particularly in environments where household conditions are unstable and difficult to observe (Coady and Skoufias 2004; Kidd and Athias 2019). Third, targeting may be associ-

ated with behavioural and social responses that affect programme functioning. Existing work has highlighted how targeting can shape perceptions of fairness, social cohesion, and incentives, potentially influencing labour participation, informal support, and community relations (MacAuslan and Riemenschneider 2011; Slater and Mphale 2008). The qualitative evidence presented in this chapter is consistent with these concerns, as respondents reported boycotts, insecurity, positively selected out-migration, and changes in informal coping strategies. These findings suggest that the efficiency comparison between targeted and universal systems extends beyond potential gains from reduced inclusion errors and higher marginal returns to transfers. It should also account for administrative costs, the welfare implications of exclusion errors, and the behavioural and social responses that targeting may generate.

Second, the chapter contributes to the limited literature on reductions in humanitarian assistance. Much existing work focuses on planned programme phase-outs or transitions toward self-reliance, often under conditions of gradual adjustment (Aizer et al. 2016; Banerjee et al. 2021; Altındağ and O'Connell 2023). By contrast, we examine a relatively abrupt contraction of food assistance in a refugee camp context, combined with a change in how remaining resources are allocated. Consistent with prior evidence (see Chapter 3), respondents reported worsening food insecurity and reduced borrowing capacity following declines in aid. Beyond these direct effects, the findings point to a broader set of market responses, including declines in aggregate demand, tighter access to credit, and reduced economic activity. These patterns suggest that reallocating assistance in a context of declining aggregate support may not be fully absorbed by local markets, and may instead be associated with further contractions in economic activity.

Third, the chapter contributes to a growing literature that conceptualises humanitarian assistance not only as household income support but also as a component of local economic systems. In camp settings where formal labour markets are constrained, transfers can underpin trade, credit, risk-sharing arrangements and social relations (Harrell-Bond 1986; Jacobsen 2005; Werker 2007; Betts et al. 2014). The qualitative evidence presented here is consistent with this perspective. Respondents described how changes in assistance

were associated with shifts in market activity, liquidity, and informal financial arrangements, suggesting that transfers play a role in sustaining local economic interactions beyond recipient households. This perspective highlights that the effects of targeting and aid reductions may extend beyond direct beneficiaries. In particular, where transfers support aggregate demand and informal credit, withdrawing or reallocating assistance may be associated with broader changes in market functioning. These findings are in line with work emphasising the general equilibrium effects of transfers in constrained environments (Egger et al. 2022; Cunha et al. 2019), and underscore the importance of considering economy-wide dynamics when assessing alternative allocation systems.

The remainder of the chapter is structured as follows. Section 1 describes the study context. Section 2 outlines the methodological approach. Section 3 presents the results, and Section 4 concludes.

1 Context

Kakuma refugee camp is located in Turkana County in northern Kenya, near the borders with South Sudan and Uganda. Established in 1992 following large-scale displacement from Sudan, Somalia, Ethiopia, and the Democratic Republic of the Congo (Jansen 2018), the camp has evolved into one of the largest protracted refugee settlements globally. In 2016, the Kalobeyei Settlement was created approximately 3.5 kilometres from Kakuma to accommodate continued inflows and to pilot a more development-oriented model of refugee assistance (Betts et al. 2020). Together, Kakuma and Kalobeyei host approximately 309,000 refugees (UNHCR 2025) and are often administratively and operationally treated as a single site.

Kenya's encampment policy has historically restricted refugees' freedom of movement and access to formal labour markets. As a result, livelihood opportunities remain limited and dependence on humanitarian assistance is widespread. Survey evidence indicates that only around 24 percent of refugees report any form of employment, most commonly as incentive workers for NGOs operating within the camp (MacPherson and Sterck 2019).

The majority of households therefore rely heavily on monthly food and cash transfers as a primary source of subsistence.

Historically, core assistance in refugee camp settings has often been delivered on a relatively uniform basis within a given site, even where narrower programmes targeted specific groups such as malnourished children or pregnant and lactating women. Kakuma broadly followed this model. Since its creation in the 1990s onward, registered refugees received blanket food assistance, initially through in-kind distributions of cereals, pulses, oil, salt, and corn-soya blend, with ration levels adjusted periodically in response to operational and funding conditions (WFP 1998, 2005, 2007).

The main change before 2025 concerned how assistance was delivered rather than to whom it was delivered. From the mid-2010s, food assistance increasingly shifted from in-kind provision towards cash-based modalities. In 2015, WFP introduced *Bamba Chakula*, a restricted mobile cash transfer redeemable at approved traders in Kakuma (Betts et al. 2018; MacPherson and Sterck 2019). In Kalobeyei, assistance was delivered in cash from the outset, and from 2019 restrictions on purchases were further relaxed in line with self-reliance objectives (Delius and Sterck 2024; Siu et al. 2023). Despite these changes, core food assistance continued to be distributed on a broadly uniform basis. This distinction is important for the chapter: before 2025, modality changed, but the underlying allocation rule remained comparatively stable.

More recently, some refugee operations have moved towards vulnerability-based or "differentiated" assistance, often under conditions of growing budget pressure. Joint WFP–UNHCR guidance on needs-based targeting was formalised in 2016, but operational shifts towards household differentiation became more visible across a number of refugee settings in the early 2020s (WFP and UNHCR 2017). Pilot or operational shifts to vulnerability-based food assistance preceded Kenya's 2025 reforms in a few settings: notably Rwanda (mid-2021) and parts of Uganda (from 2023), where WFP–UNHCR transitioned from blanket to needs-based provision (World Food Programme 2021, 2023). Kakuma and Kalobeyei are therefore not an isolated case, but part of a wider policy move

towards greater selectivity in refugee assistance under fiscal constraint.

In Kakuma and Kalobeyei, however, this shift took place in a setting characterised by encampment policy, widespread poverty, volatile livelihoods, and continued dependence on humanitarian support. That background is important for interpreting the reform. In settings where transfers also underpin informal credit and local demand, this redistribution may carry social and market consequences in addition to its direct welfare effects.

The relevant policy change analyzed in this Chapter occurred in August 2025. Until July 2025, all refugees in Kakuma and Kalobeyei received the equivalent of roughly USD 8 per person per month in food assistance. Following a household-level assessment conducted in May 2025, humanitarian agencies, in coordination with the Government of Kenya, introduced a vulnerability-based differentiated assistance system. Households were assigned to one of four categories (see Table 14). Approximately 65,000 refugees were classified as Category 1 and continued to receive about 40 percent of a full food ration, roughly equivalent to USD 8 per person per month at the time of implementation. Around 107,000 refugees in Category 2 received about 20 percent of a full ration, roughly USD 4 per person per month. These two categories were intended to capture households seen as lacking stable income and facing structural vulnerabilities such as disability, single parenthood, or high dependency ratios. The largest group, around 121,500 refugees, was assigned to Category 3 and initially excluded from food assistance, while a further 14,000 refugees in Category 4 were also excluded. In total, around 44 percent of the refugee population lost access to food assistance at the point of rollout.

Table 14: Differentiated assistance categories at rollout in August 2025

Category	Refugees	Refugees	Aid	Aid
	#	%	% of full food basket	USD per person/month
Category 1	65,000	21.1%	40%	8
Category 2	107,000	34.8%	20%	4
Category 3	121,500	39.5%	0%	0
Category 4	14,000	4.6%	0%	0
Total	307,500	100%		

Following implementation challenges and protests, the system was revised in October

2025. Rations for Categories 1 and 2 were increased to 55 and 35 percent of a full ration, respectively, and limited assistance equivalent to 20 percent of a full ration was reintroduced for Category 3 households, although some unverified Category 3 refugees remained excluded at the time of writing. Assistance continued to be delivered as in-kind rations in Kakuma and primarily as cash transfers in Kalobeyei, with transfer values varying by category.

Two features of this reform are central to the analysis that follows. First, it did not merely introduce a new classification system; it also coincided with a reduction in aggregate aid to the camp. The reform therefore combined a fall in the overall volume of assistance with a shift from broadly uniform provision to more selective household-level allocation. Second, the change was implemented across a setting in which assistance had long been woven into everyday subsistence and exchange. This makes it difficult to interpret later outcomes as reflecting "targeting" alone or "aid cuts" alone. Many of the consequences documented in this chapter are likely to reflect the interaction between the two.

2 Methodology

This study adopts a qualitative research design based on semi-structured interviews to examine how the shift from universal to vulnerability-based targeting was experienced by refugee households in Kakuma and Kalobeyei. The objective is to understand how refugees experience and perceive the differentiated assistance programme, its impacts, and consequences. We also sought to document the reported challenges the programme generates at the individual, household, and community levels, such as adjustments in employment, coping strategies, access to credit, and interactions with community members and institutions.

To this end, we conducted 70 semi-structured interviews with refugees (N=52), shopkeepers (N=16), and staff from international organizations (N=2). Interviews were carried out in both Kakuma refugee camp and Kalobeyei settlement to ensure broad ge-

ographical coverage. The sample size was largely determined in advance based on operational constraints. Within each household, we interviewed the person primarily responsible for food preparation. Interviews typically lasted between 40 minutes and one hour.

Respondents were selected through purposive sampling with minimal stratification by differentiated assistance category, gender, and by respondent type (household vs. shopkeeper) (Palinkas et al. 2015). Shopkeepers were further stratified between official Bamba Chakula retailers and other shops [1]. Out of the 68 refugees interviewed, 39 were female and 29 male. Among the 16 shopkeepers, 7 were vetted Bamba Chakula shops, 5 were non–Bamba Chakula shops, and 4 were business owners who had closed their shops due to differentiated assistance. We learned the category of 41 of the 52 households interviewed: 9 were in category 1, 7 in category 2, 15 in category 3, and 10 in category 4. Across all respondents, the majority were originally from South Sudan (N=40), with additional participants from Burundi (N=6), Sudan (N=6), Ethiopia (N=5), Somalia (N=4), Uganda (N=3), Rwanda (N=2), and Eritrea (N=2).

The household interview guide aimed to capture detailed narratives on key domains: personal and household background, experiences with aid cuts, coping responses, understanding of and reactions to the differentiated assistance categories, interactions with appeals or verification processes, changes in access to credit and market conditions, trust in humanitarian institutions, and expectations about future solutions. Interviews with shopkeepers documented changes in sales, profits, debts, security measures, credit arrangements, and relationships with both customers and suppliers. Interviews with shopkeepers who had closed their businesses due to differentiated assistance provided insights into business distress and shop closures.

Twelve interviews were conducted in English over the phone by the research team, while the remaining fifty-eight were carried out in person by a team of refugee enumerators. Prior to data collection, all enumerators received one-on-one training on qualitative

[1] Official Bamba Chakula retailers are shops vetted and contracted by the World Food Programme (WFP) to accept restricted cash transfers as payment.

interviewing techniques, research ethics, and the study's interview guides, followed by regular on-the-job supervision throughout fieldwork. In-person interviews were conducted in local languages, audio-recorded, and transcribed into English by the enumerator team. The research team reviewed each transcript within 24 hours of submission to improve translation accuracy and address any inconsistencies. The final corpus consists of approximately 250,000 words and over 400 pages of transcribed material. All participants provided informed consent, and the study received ethical approval from the University of Oxford's Research Ethics Committee.

Data collection was conducted in two waves, from the 4th to the 10th of August 2025 (N=38), and from the 20th to the 29th of October 2025 (N=32). The August round was planned to coincide with the rollout of the differentiated assistance programme; however, due to protests and insecurity, WFP and UNHCR postponed implementation to the second half of August, so the first wave effectively captured household and market conditions just prior to the implementation of the new targeting approach. The second wave, in October 2025, enabled us to examine the perceived impacts of the programme two months after its roll-out.

We analysed the transcripts using a thematic analysis approach (Braun and Clarke 2021). After familiarising ourselves with the transcripts, we developed an initial deductive codebook reflecting the study's guiding questions. We then expanded this codebook deductively with emerging concepts observed in the transcripts. The codebook was refined through multiple rounds of collaborative discussion as additional themes surfaced during coding. All interviews were coded using a shared codebook, with interviews double-checked by researchers to identify inconsistencies, and clarify interpretation. Thematic patterns were compared across respondent types and data-collection waves to identify convergent and divergent narratives.

We recognise that our institutional affiliations, national backgrounds, and socioeconomic positions shape how respondents might have perceived us and how we interpret their accounts. To mitigate these concerns, interviews were conducted by a team of

refugee enumerators who lived in Kakuma refugee camp and shared key aspects of respondents' lived experiences. Working closely with these enumerators also grounded our interpretation of the data and helped challenge analytical frames that might otherwise have reflected external, rather than local, perspectives.

The findings are presented in five parts, following the sequence through which respondents described the reform and its consequences. We begin with the immediate shock of rollout, including communication failures, opposition to categorisation, and early disruption of distribution. We then examine household welfare, focusing first on food insecurity, then on the coping strategies households used as support declined, and finally on mental health. The third part turns to economic transmission, documenting how reduced and uneven assistance affected market demand and trader activity within the camp economy. We then analyse categorisation as a distinct component of the reform, focusing on perceived exclusion, opacity, and its implications for trust. The final section considers broader social consequences, including insecurity and migration. This structure is intended to distinguish between the initial implementation shock, its material and economic effects, and the wider social responses that followed.

3 Results

3.1 Rollout and opposition

The rollout of differentiated assistance in Kakuma in 2025 was marked by communication problems, opposition to categorisation, and early disruptions to distribution. In early July, WFP, UNHCR, and DRS sent SMS messages to refugees to inform households of their assigned category, with notification expected to be completed by 18 July, ahead of the planned August rollout. In practice, however, many refugees struggled to access this information. To check their category, some first had to retrieve their Bamba Chakula SIM cards from shopkeepers, who were holding them as collateral for outstanding debt. Respondents reported that this created delays and confusion, as shopkeepers often had large numbers of SIM cards and could not easily identify the correct one. As one Somali

man explained, *"The shopkeeper has so many [Bamba Chakula SIM cards] and doesn't know which is which. If people want to check their category number, they can't... A man who is handling 3000 cards, when will he get the chance to check it for you?"* Others reported that they had their card but never received the SMS. These difficulties led to long queues at helpdesks and repeated attempts to verify household status. A South Sudanese woman allocated to Category 2 described the process as follows, *"It took me waiting in long lines to find my category. I searched for it for two days; sometimes you reach the help desk and they tell you to go back and come the next day. On the third day, I was told I was not in the system. I suffered for many days, but at last I got [my category] when they finally found my name in the system."*

Some respondents also described deliberate refusal to engage with the categorisation process in an attempt to boycott the reform. Rather than attempting to verify their status, they chose not to report to the distribution centre or helpdesk, objecting to a system that assigned different levels of assistance across refugee households. For these respondents, refusal was one way of resisting a change they regarded as unfair. As one South Sudanese respondent explained, *"Once people realized that differentiated assistance will harm them, no one reported to the distribution centre and the exercise was boycotted... people are dying of hunger."* Across interviews, respondents often contrasted the new system with the previous arrangement of equal distribution, arguing that all refugees should be treated equally.

Opposition to the reform escalated further in late July. On 21 July 2025, after category assignment had been completed and dissatisfaction with the new system had spread through the camp, refugees gathered outside UN and WFP offices to protest. Several respondents referred to placards carrying the slogan *"No to differentiated assistance."* One Burundian woman assigned to Category 2 recalled: *"Ever since differentiated assistance was introduced, people have never lived in peace... people went to the street objecting to differentiated assistance."* The initial demonstration later turned violent. Protesters entered one of WFP's food distribution centres and set a tent on fire. Police intervened and opened fire. At least two people were reported killed. Some respondents also described

looting in the market during the unrest. One shopkeeper explained, *"[The protest] compelled me to close my business for days... shops were looted that time of the protest but mine was saved."* Others noted that insecurity around the protest temporarily disrupted other services in the camp, including schools. A Burundian mother of four recalled, *"I remember, one week ago [during the protest], the schools were closed without students finalizing their exams."*

These events also affected the early operation of the new system. Distribution was delayed in early August because of insecurity and resumed only later in the month. Even after distribution restarted, some respondents reported that groups of young men gathered near distribution centres, discouraging or threatening those who remained eligible for assistance. This left some households unwilling to collect food. A South Sudanese woman allocated to Category 1 explained, *"Although I have Categories 1 and 2, I'm not able to go to the ration distribution because people in the community are threatening us if we go."* Another respondent linked this fear to her own household vulnerability: *"If I go, who will protect us at night, I am just alone, I don't have anyone... I am poor, if the food is brought home, who will be with me at home to protect me, for I don't have a husband... Let the food be given to everyone, even if it means just a cup."*

These accounts indicate that the rollout of differentiated assistance was contested from an early stage. Communication failures made it difficult for some households to learn their category assignment, while opposition to unequal distribution contributed to boycott, protest, business closures, looting, and temporary disruption of distribution. These disruptions can be understood as additional social costs of the new system, beyond the direct reduction and reallocation of assistance.

3.2 Household welfare

Food insecurity

The most immediate material consequence described by respondents was a deterioration in food security. Across interviews, households reported eating less, relying on cheaper

foods, and going for longer periods without food than before. These accounts were not limited to households excluded from assistance. Respondents in all four categories, including those who continued to receive some support, described difficulty meeting basic food needs. The evidence therefore suggests a broad worsening in food security following the reform, with targeting shaping how these losses were distributed across households.

Food insecurity was reported across all assistance categories. Households in Category 1, despite remaining eligible for the highest level of support, did not describe themselves as food secure. As one South Sudanese woman in Category 1 put it, *"See my house? it's empty. See my kitchen? It's empty. [...] I still have the same problems that I had before, this category doesn't help me at all. [...] I still can't afford to even feed my children decent meals."* Another respondent in the same category similarly stated, *"I am receiving food but still it's not enough."* Comparable accounts came from Categories 2, 3, and 4. A Category 2 respondent explained, *"The food provided to Category 2 is not enough to live on. It is far too little."* A Category 3 respondent described exclusion from assistance in more direct terms, *"There is no food, no assistance, and nothing to depend on. Life has become a daily struggle. My children go hungry, and I feel helpless because I have no way to provide for them."* Even among households classified as more self-reliant, food insecurity remained severe. A Category 4 South Sudanese mother told us, *"We are now surviving only by begging from neighbors. Sometimes they give us a little cooked food, but it is not enough and we cannot depend on that every day."*

A common response to food shortages was a reduction in meal frequency. Respondents across categories described skipping meals or reducing portion sizes as part of everyday survival rather than as a short-lived adjustment. As one Category 3 South Sudanese woman explained, *"before we use to eat twice a day but now we eat once a day."* A Category 4 Ethiopian man similarly reported, *"Previously, the support I received would cover for my needs, but now with no support, I am just surviving by skipping meals. Over the last 5 months, if I ate lunch, I skipped supper."* Even among Category 1 households, meal reduction was common. A Ugandan woman explained, *"the amount of food we use to consume reduced, and when there are challenges you reduce and even eat once in a*

day."

Respondents also described a decline in dietary diversity. Many reported shifting towards cheaper staple foods and being unable to purchase complementary items needed for a minimally adequate diet. An Ethiopian respondent in Category 4 explained, *"This made me change my normal diet and meal frequency. My family don't get a balanced diet plus sometimes they skip meals."* He continued, *"Previously, when I received food ration, which was often rice and oil, I could hustled to make sure I added more sweet and other food to the diet."* Several respondents noted that even visiting the market had become difficult because they lacked the cash needed to make purchases. As one Category 3 respondent put it, *"I don't have money to go to the market, it's been 3 months now, I don't go to the market. Where do I get money?"*

Some respondents also reported visible physical effects of prolonged food deprivation. A Category 2 South Sudanese woman noted that *"The children lost weight because there is not enough food."* A 58-year-old Ethiopian man in Category 1 described substantial weight loss, *"The physical impact of differentiated assistance is that I lost 21 Kg, from 74 Kg to 53 Kg due to the lack of food. This happen to me not because I am sick but because of the lack of food and diet."*

These qualitative accounts are corroborated by food consumption surveys conducted at the end of each qualitative interview. Following the approach in the previous analytical Chapters, participants were asked to recall all food consumed on the day prior to the interview, allowing us to compute daily caloric intake and dietary diversity.[2] Given the small and non-probabilistic sample (N = 37), these results should be interpreted with caution. In July 2023, following earlier aid cuts, average caloric intake in the camp was 1,723 kcal per person per day. After the introduction of differentiated assistance, this figure dropped to 994 kcal. Dietary diversity, measured using the Household Dietary Diversity Score (HDDS), declined from 4.5 in July 2023 to 3.16 after differentiated assistance.

[2]For the dietary diversity score we used the Household Dietary Diversity Score as in Kennedy et al. 2011. The score counts the types of food that a household ate the day before the survey, out of 12 different categories, with higher scores indicating more dietary diversity.

Coping strategies

As food insecurity intensified, households described a narrowing of the strategies available to manage shortfalls. Several mechanisms that had previously helped smooth consumption appear to have weakened, while some respondents reported turning to more precarious alternatives. The evidence suggests not only that households were eating less, but also that the informal arrangements that had previously helped them cope had become harder to rely on.

One important change concerned access to informal credit. Prior to differentiated assistance, many households obtained food from retailers on credit, often by leaving their aid card as collateral. Survey evidence from 2022 suggests that this practice was widespread: 87% of refugees were indebted to a food retailer, and among these, the share using their aid card as a guarantee was 71% in Kakuma and 95% in Kalobeyei (Bruni et al. 2024). Following the reform, however, many respondents said that shopkeepers had stopped accepting aid cards because they no longer trusted that money would be deposited onto them regularly. As one South Sudanese woman explained, *"The shopkeepers know very well that the Bamba card does not actually hold money any more, so they refuse to let anyone borrow against it."* This tightening was reported even by some households in Categories 1 and 2. As a Sudanese woman put it, *"It's not allowed [to get food on credit] because they're not sure whether we're going to receive food, even if we're in Categories 1 or 2."* Shopkeepers described similar concerns. One trader explained, *"Nowadays, I have decided to stop giving food on credit because the book is full and no money is coming."*

A second change concerned mutual support within families, neighbourhoods, churches, and community-based organisations. Previous to the reform food sharing and informal assistance as part of everyday life, especially in periods of difficulty. After the reduction and reallocation of aid, however, many said that such support had become harder to sustain because need had become more widespread. A South Sudanese refugee explained, *"I do have some debt, and for a long time, I believed that the church might be able to assist me in repaying it. Unfortunately, that hope is no longer there because the church leaders have explained that there is no money available."* Others made similar points: *"Even if*

you go to someone, you can't get help because they're suffering in the same way you are suffering." Several interviewees in Categories 1 and 2 also reported pressure to share their already limited transfers with neighbours and relatives who had been excluded from assistance. As one woman in Category 2 explained, *"The little food we receive cannot be kept for ourselves alone. We have to share it with those in category 3 and category 4, who were not given anything at all."* This suggests that the reform did not simply reduce resources; it also altered the direction of informal support. Rather than relatively better-off households supporting poorer ones, some of the most vulnerable respondents described being expected to share already inadequate assistance with others around them.

As usual coping options narrowed, refugees appeared to have turned to riskier and costlier strategies. This included selling roofing iron sheets to buy food or repay debt, and in some cases foraging unsafe food. One refugee explained, *"This is how we survive: you remove 1 iron sheet from the house, you go and sell it, then try as much as not to starve your children."* Another described feeding children improvised soup made from dried leaves collected near farms when no other food was available. A few accounts referred to transactional sex as a way of repaying debt or accessing food when other options had been exhausted. *"For us women when things are hard, we give our body in exchange, without knowing whether the man is sick or not. And that is even better than being taken to prison. I prefer to sleep with him in exchange of credit"*, said a female South Sudanese respondent. These accounts should not be treated as representative of all households, but they suggest that as established coping mechanisms weakened, some respondents felt pushed towards increasingly insecure alternatives.

These findings suggest that the reform affected household welfare through more than one route. It was associated not only with lower food consumption, but also with a weakening of the informal mechanisms through which households had previously managed shortfalls. Once coping strategies become more expensive and riskier, the cost of maintaining consumption levels increases.

Mental health

The crisis is taking a severe toll on refugees' mental health. Even before the recent aid cuts, mental health outcomes among refugees in Kakuma were poor (Pozuelo et al. 2023).[3] Our findings suggest that since the August reform, the situation in Kakuma has deteriorated even further.

A recurring theme is the mutually reinforcing relationship between hunger, physical illness, and psychological distress. One mother explained: *"Yes, it affects me both physically and mentally, because thinking about how my children can survive has made me more stressed. Even now, I have developed a swollen throat because of too much thinking, and sometimes I lose my voice. All of these are because of stress."* Another respondent connected food scarcity, somatic symptoms, and cognitive decline: *"I am very weak because even when you look at us now, we are suffering. So, if this is the life of the camp, then we don't have a decent life to live. This has affected our blood. Nobody looks healthy and fit when they have no support. So I am very weak, sick, and hungry. We are suffering. [...] It started with my brain. My memory is now partially disoriented, and I barely remember simple things. For instance, to have a bath, I often forget to bath, not because of the scarcity of water, but because my mind is weak. And my fractured hip has become worse. I cannot walk far because I have no strength."*

Parents repeatedly linked their distress to the inability to feed and protect their children, describing shame, sleeplessness, and a feeling of moral failure. One father said: *"I'm trying to raise a family and this is affecting me a lot. For example, if the food is over. [...] At the end of the 10th day of the month, this is when I get trauma, because the food is over. There are kids at home crying. [...] When they cry, they complain to me because they only know that I am the provider. This affects me a lot physically, psychologically, and also mentally."* Food insecurity and indebtedness is also associated with shame. *"Yes, I feel very bad for being burden to my family"* an Eritrean old man in

[3]In East Africa more generally, Pozuelo et al. (2023) find that 31 percent of refugees in East Africa exhibited moderate or high depressive symptoms, compared to only 10 percent in the host population, and that nearly two-thirds show signs of functional impairment.

Category 4 told us, adding *"Whenever I ask for support, that makes me feel real bad for not being on my own or financially independent. At times when I ask money from my brothers, I feel quite stressed for thinking that they might not have the money or not."* A mother captured the shame associated with unpayable debts: *"How can I take another debt, yet I have not paid the previous debt? I am ashamed."*

Children's mental health is also deteriorating. Young children have difficulties understanding why food is lacking, which in turn adds further stress to parents. *"Psychologically, I am not ok because the children do not understand the situation. She can't understand that we need to eat only once, or even sleep hungry sometimes."* Caregivers reported demoralization, school absenteeism, and pervasive hopelessness among adolescents. *"Our children who are meant to envision big things a head of their lives"* a father told us, adding that *"Now due to the hardship and aid cuts, the have become hopeless and miserable. This also seem to shatter their future. They are giving up hope. We really have observed this as they become quite demoralized to wake up early and go to school. [...] This all make me understand that we are in a very critical and horrifying situation."*

References to death and dying appear frequently in interviews. A 41-year-old South Sudanese woman told us: *"I am waiting [to die]. One day you will come and find me dead in my house [...] If you will not see me tomorrow or some other day, if you don't see my face, you will come to my room and knock; if no one responds, you will open the door, and close the door back; if you find me down, it's okay."* Fatalism was widespread in conversations, with many reporting it could be soon their turn: *"People are dying of hunger, others are dying due to malnutrition at the hospital and, I do not know, maybe it is me tomorrow."*

Across interviews, suicidal ideation and reports of suicide attempts surfaced with disturbing regularity. Virtually every respondent mentioned an increase in suicide attempts or deaths in their community. *"Death is right next door," explained one shopkeeper, "because if people can't eat, what do you expect to happen? So, better death than hunger."* Some parents even described considering suicide in the hope that their death

might prompt authorities to reclassify their children as vulnerable and restore their access to aid. A 32-year-old South Sudanese mother told us: *"Category 3 has brought hunger to my family already. How am I going to take care of my children? I am going to pick a rope and hang myself to death. [...] It is better if I die, even if my children will be on their own, because I cannot watch them suffer."*

3.3 Economic contraction

Respondents described the reform not only as a reduction in household resources, but also as a shock to the wider camp economy. In Kakuma and Kalobeyei, humanitarian assistance functions as an important source of purchasing power, and many interviewees linked the reduction and uneven redistribution of aid to weaker demand in local markets. Shopkeepers repeatedly described fewer customers, slower sales, and growing difficulty sustaining their businesses. As one trader explained, *"My business solely relied on Bamba Chakula. About 90 percent of the products I sold were bought using Bamba Chakula. [...] Now, no one has any money. People are not working. We have no customers."*

Shopkeepers consistently described stagnating demand in a context of widespread poverty and lack of income generating opportunities. Several reported unsold goods, food rotting on shelves, and increasing difficulty restocking. Some also linked weaker sales to rising unpaid debts, as customers who had previously relied on aid-financed purchases were no longer able to repay what they owed. As one shopkeeper explained, *"I used to give people food on credit because I knew they would pay me when the Bamba Chakula came. But now, people are cut off. They have no money and cannot pay. I have lost everything."* Many business owners described moral and financial dilemmas: continue selling on credit to hungry neighbours, or close their shops. As one shopkeeper told us, *"People with problems come to the shop for assistance and you end up sympathizing and helping them. Here in the community, everyone knows me and expects me to help them. My shop is slowly dying. I am planning to close."*

These accounts point to the character of Kakuma's aid economy. Rather than op-

erating independently of aid, local markets appear to depend heavily on transfers as a source of purchasing power and demand. When assistance contracts, the effects appear to spread beyond individual recipient households, extending to traders, service providers, and others whose livelihoods depend on the circulation of aid within the camp.

These findings raise questions about the resilience of market-based humanitarian models in remote camp settings. Over the last decade, cash and voucher assistance has often been presented as a way of supporting local markets while expanding household choice. The accounts from Kakuma suggest that where market activity depends heavily on the continued flow of aid, such systems may also be highly exposed to funding shocks.

3.4 (Mis)-categorization

Across interviews, respondents often described the reform not only as a reduction in assistance, but also as a process of classification that many found difficult to understand and to accept. Complaints about category assignment were common, particularly among households placed in Categories 3 and 4 who had lost access to assistance altogether. These accounts do not allow us to measure the overall extent of misclassification, but they do show that many respondents regarded the categorisation process as opaque and inconsistent with their own understanding of household need.

Several interviewees who considered themselves highly vulnerable described being placed in categories that left them with little or no support. A recurring concern was that households with children, no stable income, or limited support networks had nonetheless been classified as less vulnerable. As Louise, a single mother placed in Category 3, explained, *"Without a doubt, I believe I have been placed in the wrong category. I have young children, I am a single mother with no support from anyone, I am jobless, and none of my children are working."* Others focused less on the category itself than on the process through which it was assigned. Sarah, a market vendor who lost her income after being forced to close her stall, described her experience as follows: *"This decision was made without my consent or consultation. [...] Even when I went to complain, they didn't*

listen to me; they just sent me away."

Respondents also frequently questioned whether fine distinctions between households were workable in a setting where most people considered need to be widespread. Some argued that the differences captured by the system did not reflect lived realities in the camp. A Burundian respondent, for example, stated, *"There is no refugee who is rich; all of us are poor and vulnerable. Every refugee deserves to be helped without exception."*

A related theme concerned fairness. Across interviews, many respondents contrasted the new system with the earlier norm of equal distribution and described categorisation as a departure from that principle. As one South Sudanese mother put it, *"Let everyone get the same, even if it's just a spoonful."* Another respondent explained the same concern in relational terms: *"How can you eat and yet your neighbor is not eating anything? [...] Even when the food is little, it has to be distributed equally to everyone so that at least everyone will be happy."* These views suggest that for many interviewees, the reform was judged not only in terms of the amount of aid received, but also in terms of whether the distribution rule itself was seen as socially acceptable.

The categorisation process also appears to have affected trust in humanitarian agencies. Several respondents described the reform as sudden, insufficiently explained, and introduced with limited consultation. Elizabeth, who had been assigned to Category 3, explained, *"I no longer trust the United Nations, because the way they introduced these changes made me feel as if the UN had abandoned us. For a long time, I believed the UN was here to protect us, to stand with us, and to listen to our voices whenever we were in need. But these sudden changes were introduced without explanation or preparation for the people who were going to be most affected. Everything felt different. I cried many times because of this."* This kind of account suggests that categorisation was experienced not only as an administrative procedure, but also as a change in the relationship between refugees and the institutions responsible for assistance.

The main reason why donors and international organizations promote differentiated assistance is the perceived inefficiency of equal distribution. If all refugees receive the

same support, some individuals who could potentially manage without aid continue to benefit, generating inclusion errors. From a this perspective, such errors represent a waste of scarce resources that could be redirected toward those deemed most in need.

Yet in contexts such as Kakuma, where nearly all refugees live in extreme poverty, the welfare cost of inclusion errors appears minimal. Our data and qualitative evidence show that even the relatively better-off refugees remain poor by any objective standard. According to our 2022 data, 77 percent of South Sudanese households, comprising half the camp population, relied exclusively on humanitarian aid, and aid represented around 90 percent of household income. These conditions translate into very high rates of food insecurity. With the recent rounds of aid cuts, these figures have most likely significantly worsened. At the same time, strong local norms of reciprocity ensure that refugees with slightly more resources are expected, and often socially obliged, to share with neighbours and relatives who have less. Equal aid distribution therefore coexists with informal transfers that help to smooth inequalities within the camp.

By contrast, differentiated assistance introduces a new set of costs. The first are the costs of exclusion errors. When aid is the primary source of livelihood, classification mistakes no longer mean that a household receives slightly more than others—they mean that a family receives nothing at all. Such errors determine whether a household eats or goes hungry.

An additional type of cost relates to the implementation and maintenance of differentiated assistance. Classifying refugees accurately in such contexts is difficult. First, because nearly everyone is poor. In such an environment, virtually everyone qualifies as vulnerable, and as a result fine-grained distinctions are perceived as arbitrary and harmful by beneficiaries.

Second, refugees' resources and living conditions are unstable. Jobs, when available, are often casual and temporary; business income fluctuates; remittances are sporadic. Targeting accuracy depends on reliable, up-to-date information about household circum-

stances, yet in Kakuma livelihoods are fluid and precarious. This volatility undermines the reliability of any static vulnerability assessment.

Third, the administrative data needed to support precise targeting is lacking. Unlike in high-income settings, where governments can track income and employment in near-real time, humanitarian agencies in Kakuma operate with limited and outdated information. Accurate targeting requires continuous data collection, verification, and a functioning appeals process—activities that are costly and divert resources away from the assistance itself. Maintaining such a system shifts limited funds from helping people to measuring poverty. In addition to efficiency issues, this raises ethical questions about whether shrinking budgets should be spent refining a classification system, or ensuring a basic minimum for all.

The costs of exclusion errors and classification efforts are interdependent: the more resources are invested in data collection and appeals mechanisms, the fewer exclusion errors there will be. The crucial question is whether these exclusion and classification costs exceed the potential savings from reducing inclusion errors. While our data do not allow for this type of cost-benefit analysis, in settings like Kakuma, where poverty is nearly universal, the efficiency gains from excluding a minority are likely to be negligible, while both the administrative costs of implementation and the human costs of exclusion errors are likely to be high.

3.5 Social consequences

Insecurity

One of the most immediate social consequences described by respondents was a worsening sense of insecurity. After the initial protests surrounding rollout, many interviewees reported that fear and tension remained part of daily life. Shopkeepers, single mothers, and people with disabilities were among those who described feeling especially exposed. Across these accounts, insecurity was often linked to a combination of hunger, rising desperation, and resentment between those who continued to receive some assistance and

those who had been excluded.

Virtually every shopkeeper interviewed expressed a concern about theft. A Somali shopkeeper explained, *"There has been no safety since the aid cut. We are living in fear."* With minimal support from the police, they faced persistent anxiety over break-ins. A Burundian business owner highlighted the absence of police support: *"At night, some people came and stole items from my shop. I saw them, but I didn't want to risk my life by confronting them. There were more than 5 with pangas and other weapons. Instead of putting my life in danger, I let them take whatever they wanted and go. I reported to the police, and they said that they will follow up, but I have not received feedback to date."* Some shopkeepers reported adopting community strategies to compensate for limited formal protection. As one shopkeeper explained: *"The block leaders, community members and the youth made arrangements to protect the block. They make rounds at day and night. They recruited some people to protect the community."* Others rely on ad-hoc measures to protect their business, including hiring boys to sleep in their shop overnight. Yet, these informal protection measures are becoming untenable with rising insecurity: *"There was a time when someone cut the side of the shop using a machete, now the boys won't sleep in there."*

At the time of research, police have introduced curfews across the camp. Still, many shopkeepers reported that they were already closing early due to fear of theft. As one shopkeeper explained: *"The security is not good. When it's around 2pm I have to close my shop because there are men who roam around. Also, at night we have to close our shop early because of fear."* Another reiterated that incidents once confined to the night now occur in daylight, connecting insecurity with children dropping out of school due to hunger: *"Initially, they would steal from you when it was night, but now they steal from you during the daytime. Security has become a concern. So many people are hungry, for example, the unaccompanied children who do not have people to help them. Some of them are youth who are in school. They decide to drop out of school and start stealing from people to get food."*

The deterioration of security extended beyond the targeting of shops. Many refugees reported that the categorisation system deepened social divisions and caused tension between those who continued to receive food assistance and those who had been cut off. For instance, a Sudanese refugee explained, *"Although [my household has been allocated to] Categories 1 and 2, I'm not able to go to the distribution centre because people in the community are threatening us... we're just staying at home, unable to collect rations. People fear going for rations because of the threats... they might meet someone and be harmed because those in Categories 3 and 4 aren't being given rations."* Another refugee in Category 1 similarly described the risk of walking home with rations, *"Who will guard me, if I go to receive my rations, will the police bring me back to my home? [shakes head] I will just stay at home."*

A gendered pattern of insecurity emerged throughout interviews. Similar to responses from shopkeepers, accounts of theft generally identified perpetrators as male youths, who were reported to steal phones and food. *"If you walk on the road, someone might beat you and take anything you have, like a phone... You don't have peace of mind because people are hungry. It's men. Most of them are size 1... they don't have any support to depend on. Most have been placed in Categories 3 and 4."* In Kakuma, 81% of single-person households are male, with most aged 18-28 and largely perceived to be self-reliant according to WFP's differentiated assistance categories (Guyatt 2016). In contrast, respondents in Categories 1 and 2 were predominantly single mothers. These women spoke of fear not only while collecting rations but also at night, when the absence of a man in the household left them feeling particularly at risk. A single mother of two told us, *"If I do go [to collection rations], who will protect us, I am just alone. I don't have anyone, I am a survivor, no one is with me at night. Who will guard me? I am in Category 1, I am poor, but if the food is brought home, who will protect me, I don't have a husband."* Another woman explained how her mother was attacked while collecting food for the household, *"Recently, things became even worse she was attacked while trying to get food, and now her leg has been dislocated.*"

Similar concerns about protection were raised by respondents with disabilities. People

with disabilities fall under Category 1. One Somali man who was paralyzed after being shot in the leg described his fear, *"Now the problem I have is that the UN has cut aid, but I am in that group of people who were given number one. But the question is, how will you fetch it? People are scared for their lives. They told me they will cut my throat. When the policeman is there at the time of fetching in the center, you have safety, but when you take it and walk out there are problems. Even men who are stronger than me are afraid, I stand no chance as a handicapped man with a wheelchair. The two kilos I will receive will be snatched away from me. I am terrified. Isn't it a problem if I get killed for food?"* He continued, *"They are carrying pangas (machete) in their back. Now see me, because of fear, I have my knife here in my belt. The routes are not safe anymore. They can throw me out my wheelchair so at least carrying a knife might protect me,"*

The respondents shared a belief that security could not be restored while assistance remained unevenly distributed. The fear that many experienced was a direct result of the tension and mistrust caused by the introduction of differentiated assistance. As one respondent put it, *"If the food is given to all the people, there will not be any challenges, but if everybody receives even a small amount, we will have peace."* Another added, *"We never have peace unless food is available...even if just a spoonful is distributed to all, we will know that everyone has [the same]."*

This deterioration in security added further costs to the reform beyond its direct effects on assistance levels. For shopkeepers, theft, looting, and fear of attack meant shorter opening hours, additional spending on informal protection, and in some cases damage or loss of stock. For households, fear and insecurity made it harder to collect food, move safely through the camp, or rely on local markets. These accounts suggest that the reform was associated not only with lower support, but also with a more insecure environment that raised the costs of everyday survival and economic activity.

Migration

A second social consequence described by respondents was growing pressure to leave. Many refugees are returning to their country of origin, while others are relocating to

neighboring refugee hosting countries, or to other areas within Kenya. Although available data on departures from Kakuma remain limited, IOM reports that, since mid-June, approximately 14,000 individuals have returned to South Sudan through the Nadapal border crossing alone, with most departures happening after August. Among those surveyed, the majority (77%) cited food insecurity linked to ration cuts in Kenya's refugee camps as their primary reason for returning to South Sudan (IOM 2025).

Food insecurity was reported as the main reason to leave Kakuma also in our interviews, as reported by a South Sudanese refugee, *"Footing up to South Sudan is better than starving and dying of hunger in the refugee camp."* Another refugee who at the time of the interview had already returned to her country of origin reported, *"We were placed in category 3, I had no chance of bringing enough food to the table, especially for my children, so I made the decision of coming to South Sudan."* Quite simply, *"Refugees were left in a bad situation, those who got good categories stayed without any worry however those who found themselves in category 3 and 4 left for South Sudan, Uganda and other countries because they had no food to give their children."* Additional reasons for leaving included the presence of agriculture, rainfall, jobs, and healthcare elsewhere. For instance, a female South Sudanese reefugee told us, *"I believe it's time for me to go back home [...] because back home I have a land to cultivate."* Similarly, when exploring possible migration destinations, another respondent reported, *"according to me, in Kitale will be fine because people will be able to do farming. Here in Kakuma is hard because how are you going to get rain?"* Finally, a 29-years-old refugee, explained why his mother, whom we interviewed in 2023, left the camp, *"the reason why my mother left the camp is because of her health condition. She became very sick, and we tried all the UN health facilities. We explored every possible option here, but nothing worked, and her health kept deteriorating."*

Regardless of destination and reasons for migration, available migration options entail substantial risks. Return to South Sudan exposes individuals to ongoing conflict and insecurity; movement within Kenya without proper documentation leaves refugees vulnerable to arrest and harassment; and relocation to other refugee-hosting states, many of which

are also implementing aid reductions, offers few guarantees of support. The risks of these journeys are well known to refugees who, in part, seem to prefer taking these odds over remaining in the camp. A south Sudanese mother explained, *"I always think of packing my properties with my children and walk back to my country if I manage, and if I do not manage it is better to die along the road while going to my country over taking a rope and hanging myself here."*

Respondents' accounts suggest that patterns of departure and immobility observed in Kakuma align with the aspiration–capability framework (De Haas 2021). In this framework, an individual's migration decision is shaped by the interaction between the aspiration to migrate and the capability to act on those aspirations. While poorer individuals may exhibit strong desires to move, particularly when local conditions are harsh, they often lack the financial, social, or informational resources required to do so. This produces forms of "involuntary immobility," in which the poorest are either unable to migrate at all or can only undertake short-distance and highly risky forms of movement (De Haas 2021; Carling 2002). As put by a Somali business owner, *"People are moving out, people are relocating. Only the poor people who have no shillings have remained in the camp."* A thirty-eight-year-old female refugee from Burundi further explained this concept, *"You plan something when you have money. [...] I don't have anyone to help me. So, I am not planning anything. I am waiting for what God will do for us. If it's to die, I am ready."*

By contrast, relatively better-off households may have lower or more heterogeneous aspirations to migrate, yet possess greater capabilities that enable mobility. As a result, migration tends to be positively selective: those who move tend to be better-off, better educated, or more entrepreneurial than those who remain (De Haas 2021; Carling 2002). Several business owners reported that their customers, described as less dependent on aid, had already left: *"So many people, like a lot of people used to come here. [...] Those people have relocated when the economy went down and the USA cut down their aid. My customers have decreased. My situation is now hard."* Another business owner reported the intention to relocate himself, *"I feel like migrating away from Kakuma to my home country to start my business there because if I continue to stay I will end up closing my*

business with items in it."

Positively selected out-migration was also described as an intra-household phenomenon: *"Some parents have decided to run away and leave their children here [. . .] One of them is my neighbour, and I have decided to take his last born to stay with my kids."* Another shopkeeper told us, *"Some people wrapped their things, and they were ready to travel. And some are rushing selling items to move out [...] They only leave behind maybe one person to take care of the house and the card in case there is a ration."* Another interviewee noted: *"I'm a family of size 4 but currently I'm all alone around here because the rest of the people moved back home."*

Even among those able to leave, migration was described as highly uncertain and often dangerous. For instance, a South Sudanese mother told us that *"The biggest problem I am facing now is with my own son. He is planning to risk his life by taking this long, hard, and dangerous journey on foot to South Sudan. As a parent, this worries me deeply because I know how unsafe and unpredictable such a journey can be. Many young people who attempt this path either never make it to their destination or arrive in worse conditions than they left. [...] Some die along the way due to hunger, thirst, or attacks, while others fall into the hands of smugglers or armed groups. The thought that my son is willing to expose himself to this danger because of the challenges caused by DA makes me feel helpless and broken."*

In Kakuma, the young, educated, and economically active individuals, those most capable of migrating, are also the ones most affected by cuts in assistance, further reinforcing the positive selection of migrants described above. Over time, this dynamic could constrain opportunities for self-reliance, as the remaining population may be disproportionately composed of the poor, elderly, and most vulnerable. The departure of individuals with skills, networks, or income-generating ability can weaken local economic activity and reduce the availability of support within social networks. In this sense, the consequences of the reform may extend beyond the households that leave, affecting those who remain through a gradual erosion of human and social capital.

4 Conclusion

With humanitarian budgets shrinking globally, aid agencies are increasingly asked to "do more with less." In response, assistance programmes are being redesigned to prioritise efficiency, often through more selective targeting. In refugee assistance, this has involved a shift away from universal provision toward vulnerability-based systems that differentiate support across households. While such approaches are now increasingly adopted, their broader implications in refugee camp settings remain ambiguous (Gassmann et al. 2025). In addition, the largely quantitative focus of current evidence, while generating important insights, struggles to unpack the perspectives, uncertainties, and lived experiences of those subject to these policies (MacAuslan and Riemenschneider 2011). As a result, the complex social consequences through which these reforms reshape coping strategies, community relations, and local economies tend to be overlooked and only partially understood.

To address this gap, we examine the combination of aid cuts with the transition from universal to vulnerability-based food assistance in Kakuma refugee camp and Kalobeyei settlement in Kenya. We base our analysis on qualitative interviews conducted around the rollout of differentiated assistance and aid cuts in August 2025 and again two months later. This design allowed us to capture both immediate reactions and short-term adjustments to the policy change. We organise our findings around a set of interrelated consequences that point to widespread food insecurity, mental health deterioration, and a broader set of costs than those typically considered in the design of targeting systems. These costs include disruptions to implementation through boycott and non-compliance, the breakdown of consumption smoothing mechanisms, the adoption of more costly and less reversible coping strategies, high prevalence of perceived exclusion errors, increased insecurity affecting both households and traders, and the selective out-migration of skilled individuals.

First, hunger emerged as the most immediate and pervasive consequence of the reform. Across categories and locations, respondents described skipping meals, reducing portion sizes, and relying on less diverse and cheaper foods. Food insecurity was not con-

fined to households excluded from assistance, but was also reported by those receiving support. Hunger was not only described as an outcome in itself, but also as a central theme underpinning other effects. It shaped how respondents experienced insecurity and migration decisions, and contributed to deteriorating physical and mental wellbeing.

Second, respondents described how the reform disrupted the coping mechanisms that they had long relied on to smooth consumption. Refugees and food retailers alike reported that informal credit, which had previously allowed households to bridge shortfalls, tightened sharply as aid cards lost their collateral value and trust in future transfers weakened. At the same time, interviewees reported that informal mutual support networks became less effective as poverty became widespread. In some cases, redistribution pressures reversed direction, with already vulnerable households in Categories 1 and 2 describing having to share their limited transfers with excluded neighbors. As these mechanisms weakened, households reported turning to more costly and less reversible coping strategies, including selling parts of their shelters, and in some cases relying on precarious or unsafe means of securing food. These changes suggest that the reform not only reduced consumption, but also increased the cost of maintaining it.

Third, respondents consistently reported a deterioration in mental wellbeing. Hunger, debt, and uncertainty were closely linked to experiences of stress, shame, and feelings of hopelessness. Parents described distress associated with being unable to provide for their children, while caregivers reported changes in children's behaviour, including demoralisation and reduced school attendance. Accounts of suicidal ideation and suicide attempts were widespread across interviews, suggesting that the consequences of the reform extended beyond material deprivation to include significant psychological costs.

Fourth, respondents' accounts point to a contraction of the local economy. As large segments of the population lost access to assistance, traders reported declining demand, unsold goods, and growing difficulty sustaining their businesses. Some described closing shops or reducing operations, while refugees reported fewer opportunities for casual work and income generation. These accounts seem to indicate that the reform has impacted

not only households but the broader camp economy.

Fifth, the rollout of the reform and the introduction of categorisation generated additional administrative costs, boycotts, and widespread perceived exclusion errors. Refugees reported that communication failures, delays in category assignment, and limited access to appeals contributed to confusion and dissatisfaction at the point of implementation. Some respondents described boycotting the system or refusing to engage with it, while protests and insecurity temporarily disrupted distribution. Beyond these immediate effects, categorisation was generally experienced as opaque and, often, inconsistent with respondents' own understanding of need. In this context of extreme poverty, exclusion errors carried particularly high costs as households cut off from assistance frequently lacked alternative means of subsistence. These dynamics suggest that the costs of targeting extended beyond administrative costs to include exclusion errors, non-compliance, and contestation, possibly leading to reduced effectiveness.

Finally, the reform was associated with a set of social consequences. Respondents described rising insecurity, including theft, violence, and fear of moving within the camp. Shopkeepers reported closing earlier, investing in informal protection, or facing losses from theft and looting. At the same time, many interviewees described growing pressure to leave the camp. Migration was often framed as a response to food insecurity. The accounts of the respondents suggest that this movement was positively selected within as well as between households, with younger, more educated, or more economically active individuals more likely to leave. Over time, such patterns may weaken local economic activity and informal support networks by reducing the stock of human and social capital within the camp.

These findings suggest that the performance of vulnerability-based targeting depends on the broader resource environment. In settings of widespread poverty and declining aid, the costs of exclusion, administrative frictions, and social spillovers may outweigh the gains from reallocating resources across households. Under such conditions, targeting may amplify, rather than attenuate, the effects of aid reductions. By contrast, its benefits seem

more likely to materialize during periods of resource expansion and economic stability.

Chapter 5

Conclusions

1 Main findings

Humanitarian assistance is under severe strain. In 2024, more than 300 million individuals required humanitarian support, yet only around two-thirds received assistance, and less than half of funding requirements were met (Humanitarian Action 2024). This gap widened further in 2025 following a substantial reduction in contributions from the United States, which had previously accounted for approximately 43% of global humanitarian financing (Pearson 2025). Other major donors similarly scaled back their commitments, with early evidence pointing to significant adverse effects on vulnerable populations across the globe (Loy 2025).

Displaced populations have been disproportionately affected, especially the roughly 9 million individuals residing in refugee camps worldwide (UNHCR 2025b). Camps are typically located in remote areas characterized by harsh environmental conditions, weak infrastructure, and limited integration with surrounding markets, constraining opportunities for self-reliance. In such settings, livelihoods, coping strategies, and informal risk-sharing arrangements are tightly linked to humanitarian assistance. As a result, aid is not a simple food voucher that some households receive, but the backbone of the economy.

This thesis adopts a mixed-method, longitudinal approach to study how humanitar-

ian aid influences camp economies, focusing on Kakuma refugee camp in Kenya, the third-largest refugee camp worldwide. It combines quantitative, longitudinal data on households with high-frequency price data to examine how aid affects household welfare, prices, and credit markets. Results and interpretations are further supported by qualitative interviews with refugees, shopkeepers, and humanitarian personnel.

The first analytical chapter examines the welfare and market effects of aid transfers and their delays, in a context where the nominal value of assistance is known in advance, but the timing of disbursement is uncertain. At the household level, this context allows for a comparison of alternative models of intertemporal choice, ranging from the PIH, which implies consumption smoothing (Friedman 1957; Hall 1978), to behavioral models incorporating various constraints, which predict incomplete smoothing (Strotz 1956; Laibson 1997; Deaton 1991; Carroll 1997; Gul and Pesendorfer 2001; Banerjee and Mullainathan 2010; Gabaix and Laibson 2017). The analysis focuses on two core questions: whether refugee households smooth consumption across regular transfer cycles, and whether unanticipated delays undermine this capacity.

The empirical findings do not fully align with either theoretical model. Consistent with the PIH, households' smooth consumption when transfers arrive as scheduled. However, this capacity breaks down when aid is delayed. In such periods, food consumption and food security decline sharply, while indebtedness increases. These results suggest that predictable aid supports consumption smoothing, and that temporary disruptions in transfer timing generate substantial welfare losses.

The evidence indicates that limited access to credit is the primary constraint preventing households from smoothing consumption during delays. Households with greater borrowing capacity can better smooth consumption when aid is delayed. By contrast, credit-constrained households show sharp consumption declines in response to delays. While borrowing attenuates these shocks, it does so at a cost: transactions conducted on credit involve 17% higher prices, on average.

This chapter also shows that aid transfers and their timing shape price dynamics.

First, it illustrates that following the disbursement of cash transfers, aid-card prices rise sharply, as households purchase goods with this currency, and cash prices drop. As the cycle advances, and particularly when disbursements are delayed, this pattern reverses: aid-card prices decrease, whereas cash prices increase as households substitute toward transactions financed with cash. These divergent price movements are consistent with partial substitutability between cash-transfer and cash.

The second analytical chapter investigates the short-run consequences of a 20% reduction in humanitarian assistance implemented in Kenya in July 2023. The chapter contributes to the literature on models of intertemporal consumption smoothing (Friedman 1957; Hall 1978), credit and informal insurance (Jappelli and Pagano 1989; Deaton 1991; Townsend 1994), and general equilibrium responses to aggregate demand shocks (Angelucci and De Giorgi 2009; Cunha et al. 2019; Egger et al. 2022; Filmer et al. 2023; Delius and Sterck 2024). Broadly, this chapter aims to answer the following research question: What happens to households and markets when aid is reduced?

The findings illustrate that the contraction in aid generated large and immediate welfare effects. In the short run, household consumption, caloric intake, and dietary diversity declined sharply, with calories falling by approximately 7% and expenditures by around 20%, alongside an overall increase in food insecurity. The shock also affected non-material dimensions of wellbeing: respondents reported deteriorations in sleep quality and subjective well-being, as well as shifts in risk tolerance and time preferences.

The analysis indicates that the aid cut triggered a contraction in informal credit markets, undermining one of refugees' most important coping mechanisms. Informal borrowing arrangements were often secured by using aid cards and future aid disbursement as a guarantee. As aid levels declined, the value of this collateral fell accordingly, leading lenders to ration credit. On average, household debt dropped by approximately 10% immediately after the cut and continued to decline thereafter. In response to both the reduction in aid and the collapse of informal credit, households resorted to selling assets. However, asset sales were insufficient to smooth consumption and are typically viewed as

a less reversible and more risky coping strategy than taking on debt.

The analysis of food prices illustrates that the aid reduction induced general equilibrium effects. Food prices fell by about 4% in the immediate aftermath of the cut and continued to decline in subsequent months, consistent with a contraction in local demand, and partially attenuating the welfare losses.

The final analytical chapter examines the welfare effects of moving from universal to vulnerability-based aid distribution in a context of aid cuts. As humanitarian budgets have tightened, humanitarian agencies increasingly rely on targeting to allocate limited resources. The core argument is straightforward: when all refugees receive the same assistance, some receive aid even if they could plausibly do without, reducing the resources available to those most in need. In August 2025, this shift was implemented in Kakuma refugee camp, where the World Food Programme, UNHCR, and the Government of Kenya replaced universal assistance with a targeted system. Drawing on qualitative interviews conducted around the time of the policy change and two months later, the chapter asks a simple but consequential question: what happens to refugees when aid provision moves from universal to targeted in a period of aid reductions? By doing so, the chapter contributes to the literature studying the different economic, social, and administrative costs linked to different universal or targeting strategies (Devereux 2016; Devereux et al. 2015; Samson 2021; MacAuslan and Riemenschneider 2011; Hoddinott 2007).

Despite the perceived efficiency gains of targeted aid, the findings suggest that in settings of widespread poverty such as Kakuma, the hidden costs of targeting outweigh its ethical and practical advantages. While targeting may reduce inclusion errors, it also generates exclusion errors, leaving some of the most vulnerable refugees without assistance. In this context, such errors are not merely inefficiencies but can have direct consequences for survival. Moreover, targeted systems entail substantial implementation and maintenance costs: from an appeal system to manage exclusion errors, to data collection and analysis costs. Targeting effectiveness depends on timely and accurate information about household economic conditions. In Kakuma's largely informal economy, where household

composition fluctuates over time, and employment is unstable, the precision of static vulnerability assessments is limited. This raises a fundamental ethical question: whether limited humanitarian resources are better spent sustaining a fragile classification system or ensuring a basic level of assistance for all.

In addition to the administrative costs and exclusion errors, the analysis shows that the shift to targeting was accompanied by substantial changes in Kakuma's economic and social life. Respondents described that longstanding coping strategies deteriorated. In particular, access to informal credit collapsed as aid cards, previously used as collateral, lost their value. Informal and progressive redistribution arrangements, through which relatively better-off households supported those in greatest need, also broke down. These were replaced by more regressive dynamics, whereby households still receiving assistance, who are often the most vulnerable, reported having to share limited resources with those no longer eligible. Respondents reported resorting to risky coping strategies, selling housing materials and, in some cases, engaging in transactional sex. The transition was also associated with higher levels of violence and insecurity, worsening food insecurity, distress-driven migration, and deteriorating mental health, with many respondents reporting suicidal thoughts. These effects were further amplified by market responses, as shopkeepers described unsold goods, lack of custumers and business closures. Overall, the findings indicate that in this setting, where aid is a key pillar of the local economy, vulnerability-based targeting can undermine both efficiency and protection objectives, whereas more universal approaches are more likely to support welfare and social stability.

The three chapters show that humanitarian aid in camp settings should not be understood as a simple welfare injection, but as a central feature of the camp economy. Aid shapes household welfare and coping strategies and generates general-equilibrium effects on prices and credit markets, ultimately influencing how camp economies function. Predictable transfers support consumption smoothing, whereas delayed transfers lead to sharp declines in food security. Sudden aid reductions weaken food security, reduce expenditure and access to credit, alter risk-taking and time preferences, and worsen mental health outcomes, while also generating general-equilibrium effects. Finally, vulnerability-

based targeting is associated with fractures in informal insurance and mutual support mechanisms, increases in distress-driven migration, loss of trust in humanitarian agencies, and is linked to broader economic contraction and business closures.

2 Policy implications

The findings of this thesis yield clear implications for the design of humanitarian assistance in camp settings, particularly those characterized by widespread poverty, informality, and high dependence on aid. The discussion that follows is primarily intended for humanitarian agencies and relief actors involved in aid delivery. Rather than proposing universal policy solutions, this section highlights design implications that follow directly from the empirical results and are likely to be most relevant in contexts such as Kakuma and similar refugee camps.

A first implication concerns the timing and predictability of aid disbursement. The results in Chapter 1 show that delays in aid delivery generate substantial welfare losses, even when the nominal value of assistance is known in advance. Reducing delays therefore represents a margin along which welfare gains may be achieved without increasing transfer amounts. The evidence suggests that delays are often driven by coordination frictions across and within agencies, pointing to the importance of strengthening coordination mechanisms to ensure timely delivery. In addition, the findings indicate that maintaining a guaranteed minimum level of assistance disbursed on time, even when beneficiary lists are not fully finalized, could substantially mitigate welfare losses. The proof-of-life verification process that contributes to delays rarely results in large revisions to beneficiary lists, suggesting that partial or provisional disbursement may be welfare-improving. Finally, staggering the timing of transfers across households may help reduce synchronized liquidity constraints and allow informal support and risk-sharing networks to operate more effectively.

Beyond timing, the scale of humanitarian assistance has systemic consequences in camp economies. The results in Chapter 2 show that aid reductions operate as negative

aggregate demand shocks, with effects extending beyond direct beneficiaries to prices, credit markets, and local economic activity. Back-of-the-envelope calculations illustrate the asymmetry between fiscal savings for donors and welfare losses for recipients: the 20% reduction in humanitarian assistance in Kakuma generated estimated savings of approximately $26 million per year, while the implied welfare losses, when scaled to the USA context for illustration, would amount to tens of billions of dollars annually. This comparison highlights how relatively modest budgetary savings for donors can translate into disproportionately large welfare losses for extremely poor populations. The findings suggest that incorporating welfare-based metrics into cost–benefit assessments may improve decision-making around aid cuts. In addition, implementing reductions gradually, by staggering cuts across time or locations rather than applying them simultaneously, may help ease market pressures, allow households and local businesses to adjust more smoothly, and preserve space for informal support mechanisms. Ultimately, the manner in which aid is reduced matters as much as the magnitude. The careful design of aid reductions can significantly mitigate the negative effects of the cut.

The effectiveness of humanitarian aid depends not only on its level and timing, but also on how aid modalities interact with beneficiaries' behaviour and existing informal institutions. In practice, aid modalities both shape and are shaped by informal economic responses, suggesting that agencies would benefit from explicitly accounting for these dynamics in program design. The evidence in this thesis shows that cash-based transfers are embedded within a large and active informal credit system, in which aid cards and expected future transfers serve as collateral for borrowing. In practice, a substantial share of transactions in the camp economy occurs on credit rather than through cash-on-hand, with shopkeepers operating simultaneously as retailers, lenders, and providers of informal insurance. In this context, informal credit functions as a de facto medium of exchange for many households. When expected aid declines or becomes less predictable, the resulting loss in collateral value can trigger a contraction in informal lending, amplifying financial risk and accelerating distress. Humanitarian aid programs that focus narrowly on liquidity or cash transfers therefore risk missing one of the primary channels through

which aid affects welfare and local markets. These findings suggest that agencies designing, scaling, or reducing cash transfer programs would benefit from monitoring informal lending practices and anticipating how changes in transfer timing and volume interact with repayment cycles, retailer liquidity, and household access to key coping strategies.

A final implication concerns aid allocation rules, and in particular the use of vulnerability-based targeting relative to universal approaches. The findings highlight that allocation regimes are not purely technical instruments, but socially embedded interventions whose legitimacy shapes their effectiveness. As a result, even when targeting appears efficient on paper, it is unlikely to be effective, and may be counterproductive, when it is strongly contested by beneficiaries. In Kakuma, the shift to targeting was not only contested in qualitative interviews, but also met with mass protests, the destruction of World Food Programme warehouses, and subsequent military intervention. These events indicate a severe breakdown in the perceived legitimacy of the allocation system, undermining its effectiveness even prior to full implementation. More broadly, the evidence suggests that targeting performs poorly in settings characterized by widespread poverty, high aid dependence, and strong informal redistribution and insurance mechanisms. In such contexts, exclusion errors are not marginal, and the administrative and social costs of (miss-)classification can outweigh efficiency gains. These findings challenge the conventional policy rationale for introducing targeting during periods of aid contraction. In highly aid-dependent camp economies, contractions are precisely when targeting is likely most destabilizing, as it compounds welfare losses, credit contraction, business closures, distress-driven mobility, and deteriorating mental health, conditions under which opportunities for self-reliance further decline. By contrast, targeting may be more defensible and preferable when implemented during periods of aid expansion and under conditions of relative economic growth.

3 Limitations

The findings of this thesis are subject to various limitations. These limits differ across chapters, reflecting the distinct data sources, research questions, and identification strategies employed. Chapter-specific limitations are discussed within each analytical chapter, while broader, cross-cutting limitations are summarized below.

The first limitation concerns external validity. In this respect, Kakuma and Kalobeyei represent an extreme case along both economic and legal dimensions: most households rely on humanitarian assistance as their primary source of income, employment opportunities are scarce, and refugees face restrictions on mobility and access to formal work. The camp's remote location and weak infrastructure further limit integration with surrounding markets, resulting in an economy that is largely informal and aid-centered. These features constrain the direct generalizability of the results to other environments, or urban refugee contexts, where access to labor markets, savings, and formal financial institutions is greater. At the same time, they provide strong analytical leverage to study aid-dependent camp economies, making the findings most directly relevant to refugee camps, an environment that remains home to over 9 million refugees globally (UNHCR 2025b).

A second cross-cutting limitation concerns the extent to which observed behaviors and market responses are endogenous to the humanitarian aid regime itself. In Kakuma and Kalobeyei, households, retailers, and informal institutions have adapted over time to the presence, modality, and predictability of aid, shaping patterns of credit, consumption smoothing, and risk sharing. As a result, the economic responses documented in this thesis reflect not only reactions to specific shocks, such as delays, cuts, or targeting, but also the cumulative effects of prolonged exposure to humanitarian assistance. This path dependence implies that similar policy changes may generate different responses in contexts with shorter aid histories. The findings should therefore be interpreted as conditional on an economy in which humanitarian assistance has long functioned as a central organizing feature.

A third limitation of this work relates to its time horizon. A central design feature of the thesis is the use of high-frequency data to study short- to medium-term dynamics. For instance, it examines how food security, expenditure, debt, and prices evolve within and across monthly aid cycles over the course of a year. While this approach provides substantially richer temporal variation than cross-sectional analyses or yearly panel data, it does not capture longer-term adjustment processes. Households and markets may adapt over longer horizons; however, in settings where consumption smoothing is limited and vulnerability is acute, short-run welfare losses and general-equilibrium disruptions are themselves economically and socially consequential. The results therefore speak most directly to the short- to medium-term consequences of aid design choices, which are often the most policy-relevant in humanitarian crisis contexts.

Finally, the thesis does not seek to explain why specific aid design choices, such as cuts, delays, or targeting, were adopted in the first place. Decisions around humanitarian funding and allocation are shaped by donor politics, geopolitical shocks, and institutional constraints that lie beyond the scope of this analysis. The focus here is on the consequences of these decisions once implemented, rather than on their political determinants. Understanding how donor incentives and global governance structures shape aid volatility and targeting choices remains an important area for future research, but is analytically distinct from the welfare, market, and social effects studied in this thesis.

4 Future research

The findings of this thesis point to several directions for future research. By documenting how humanitarian assistance shapes welfare, markets, and social dynamics in refugee camps, the analysis raises new questions about the scope, mechanisms, and design of aid beyond the specific contexts studied here. The directions outlined below build on the core insights of the thesis and aim to extend them to new populations, alternative institutional environments, different research methods, and unresolved policy trade-offs.

First, future research should investigate spillover effects of reducing aid on host popu-

lations. Refugee camps are often closely linked to surrounding host communities through labor markets, trade, and informal credit relationships. Changes in humanitarian assistance may therefore affect not only refugees but also host households, particularly in regions where refugees constitute a large share of the population. Understanding these spillovers is important both for assessing the full welfare consequences of aid cuts and for informing host–refugee relations and political support for humanitarian programs.

Second, extending the analysis beyond camp settings is key. While this thesis focuses on refugee camps, the majority of refugees worldwide reside in urban contexts. Urban refugees typically face different labor market opportunities and credit constraints and have access to different types and levels of aid, which may substantially alter how they experience aid delays, cuts, and changes in allocation rules. Future work should examine whether changes in aid to refugees generate comparable welfare losses, credit contractions, and market effects in urban settings, or whether greater economic integration mitigates these impacts.

Third, additional comparative evidence is needed to assess the generality of the mechanisms documented in this thesis. The role of informal credit, the use of aid cards as collateral, and the interaction between aid and credit has been very well documented in Kakuma refugee camp, but only partially explored in other contexts. Yet, these practices may vary across refugee and IDP camps with different institutional arrangements, aid modalities, and degrees of market integration. Comparative studies across camps and countries would help distinguish which features of humanitarian economies are context-specific and which are more general. In turn, this would clarify the conditions under which informal credit systems emerge, evolve, and persist.

Fourth, the central role of informal debt in refugee economies highlighted in this thesis calls for experimental and quasi-experimental evaluations of policies aimed at reducing over-indebtedness. Potential interventions include improvements in transfer predictability, alternative payment modalities, debt relief or restructuring mechanisms, and partnerships with formal or informal financial actors. Rigorous evaluation of such policies

would provide much-needed evidence on how humanitarian programs can mitigate the long-term costs of informal debt without undermining this important coping strategy.

Fifth, future research should seek to quantify the social, economic, and administrative costs of vulnerability-based targeting relative to more universal approaches. While this thesis documents these costs qualitatively, there remains limited causal evidence on the magnitude of exclusion errors and of the administrative and social costs associated with different targeting schemes. Quantitative studies that estimate these trade-offs would contribute to a more complete and policy-relevant assessment of vulnerability-based targeting and provide credible parameters to justify or reject its implementation.

Finally, an important complementary research agenda concerns the political economy of humanitarian aid volatility. This thesis focuses on the consequences of aid delays, cuts, and targeting decisions once implemented, rather than on their determinants. Future work could examine how and why donors, organizations, and governments decide to reduce or reallocate humanitarian assistance. As the evidence in this thesis shows, reductions in humanitarian aid generate relatively small fiscal savings for donors while imposing large welfare losses on beneficiaries. Such research would help clarify the political and institutional reasons why aid cuts occur and could ultimately support more consistent, long-term commitments or legal frameworks that reduce the likelihood of sudden and reactive aid reductions, such as those observed in recent years.

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